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**1946-1958**

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*January 1, 1953 - December 31, 1953*

Lawrence Berkeley Laboratory  
University of California

Release for Announcement in  
Energy Research Abstracts

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Journal  
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XBB 911-140

Peter, David, Stephen, and Lynne Seaborg  
(with Cricket), 1953

### 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<sup>12</sup> 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<sub>3</sub> 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 Bisacetylacetone."

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  $\text{PuF}_3$ ," [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 (Electrical Engineering), "Instrumentation for Energy Determination of High Energy Particles."

Silva, Robert Joseph (Fall 1954-June 1959), Seaborg/Harvey, "Mechanisms of the ( $\alpha$ ,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  $\text{Ra}^{226}$ ,  $\text{Th}^{230}$ ,  $\text{U}^{235}$ , and  $\text{Np}^{237}$ ."

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 Trella (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 Helmholtz/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), Péter 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, summer 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 (1946), 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. Grahman (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-), Paul Lathrop (1949), 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 Piontek, 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  $(n,\gamma)$  and  $(n,\text{fission})$  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 hybridization 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 Corporation (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 [REDACTED] 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 [REDACTED], and soon thereafter (October) Jeanette returned to Southern California where she was married on [REDACTED] (and another marriage on [REDACTED]). Our second son, David Michael, was born on [REDACTED]; 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 [REDACTED]. 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 [REDACTED]. (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 [REDACTED] 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 [REDACTED], 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.

Thursday, January 1, 1953

The Kalksteins (Marv and Edda) and the Hollanders (Jack and Margie) came out to watch the Rose Bowl game with us [neither couple has a television set] and to have supper with our family. Southern California beat Wisconsin by a score of 7 to 0.

Friday, January 2, 1953

I worked on my writing projects, etc. for a while today, then Dan Wilkes and I went out to the Contra Costa Golf Club where we played 18 holes with a couple of other fellows--John McCrossen and Hugh Hockett (DW-82, GTS-100, JM-78, HH-96).

Saturday, January 3, 1953

Again, it was a family day although I read and worked on some of my projects.

Sunday, January 4, 1953

I read for a while and visited with my parents. Later the older kids and I drove my parents to the railroad station to catch the train back to Los Angeles. Before going home, we stopped to visit the Thompsons (230 Elaine Drive, Concord) for I had a few ideas to discuss with Stan about our new element, particularly since I will stop at Argonne on my trip to Washington this coming week.

Monday, January 5, 1953

There were the usual number of administrative matters and phone calls to handle this morning. Clark Kerr called with some questions about athletic matters and my attendance at the NCAA meeting. In particular, he wanted to bring to my attention the letter signed by the Presidents of all the nine PCC schools at a meeting last year in San Francisco and sent to all their staff members:

The presidents of the member institutions of the Pacific Coast Intercollegiate Conference at a meeting held in San Francisco in January, 1952 expressed the opinion that illegal subsidization was the basic problem in American intercollegiate athletics. It was agreed to advise the Conference that it was the judgment of the presidents that the Conference should take whatever steps might be deemed necessary to get the facts concerning alleged unauthorized financial aid to athletes. It was further agreed that if the Conference decided to take such action and if evidence of illegal activity is presented to the president of any member institution involved in such activity, he will take appropriate disciplinary action against any employee of the institution or its associated students who may be involved....

Kerr mentioned that one item to be discussed at the meeting is the television agreement between the Big 10 and the PCC. He is particularly interested in the status of Booster Clubs, in light of last year's Presidents' letter, and said that he believes Stanford has one of the

best; he asked if we see the books of such clubs. He asked me to look into the finances of the football players and determine how work assignments are made, etc. Kerr also wants to make sure that the players are making normal progress toward a degree--Wallace Sterling (President of Stanford) claims that their players do.

Since I am leaving tomorrow morning, I made an effort to talk with a number of people (by phone or in person), including John Rasmussen (about the Swedish trip), Herbert R. Johnston (who has decided not to consider our job offer until next fall), and others.

In the mail that arrived today was a December 31 letter from Bernard Harvey:

Your phone call to me came on the day that the NRX pile died! Since then an atmosphere of crisis has prevailed here, and it was not until last night that it seemed timely for me to discuss personal affairs with Les Cook.

It seems that he, at least, would raise no objection to my leaving as soon as I can obtain U. S. visas. However, there are one or two other points which I would like to have cleared up before I send Mr. Reynolds a formal acceptance of the offer which you are making.

Harvey went on to remind me that he gave no assurance that he intends to become a U. S. citizen and that he cannot exclude the possibility that, after working in Berkeley for several years, he may want to return to Canada or England temporarily or permanently. He said that he hopes he is not raising any new difficulties since we have discussed this verbally, but he feels this should be formalized now. Harvey then wrote, "The extent of the crisis here is shown by the fact that even senior Treasury Civil Servants have been used for cleaning the pile basement with mops and rubber pails of nitric acid."

The senior staff and I had our usual brown-bag luncheon meeting in my office at noon. We talked about the football games, my forthcoming trip to Argonne (and the new element), my trip to Washington, in addition to a few other matters.

#### Tuesday, January 6, 1953

I was picked up at 7 a.m. and driven to the San Francisco airport in time to catch the 8:45 a.m. TWA Flight 36 (a "Constellation") to Chicago. Joe Katz and an Argonne driver met the flight, which arrived at 5:30 p.m., and I was driven to the Argonne Guest House. On this visit I need to talk with Joe Katz about some problems with PPR, Volume 14A, but I also hope to arrange with Manning for the transfer of some of the Argonne Mike material to us. Joe hinted that some of the fellows will have some surprising news for me when I meet with them tomorrow.

#### Wednesday, January 7, 1953

In Chicago. When I saw Winston Manning, he handed me a copy of the report ANL-WMM-1059, dated January 6, 1953, in which Argonne work suggested the assignment of our 6.6 Mev transcalifornium alpha activity

to an isotope of element 99 (and the assignment of a 6.35 Mev alpha activity to element 100). I was taken by surprise, but I told Manning and Fried, who was present, that we have come to the same conclusion regarding the 6.6 Mev alpha activity. We then decided to have a meeting with the rest of the Argonne group. Before the meeting, Manning and I discussed personnel, particularly for Project Whitney; Manning mentioned that Donald F. Peppard would be a good man for an associate director or a group leader. He also suggested John Leroy Maier, who works with Peppard, as a possible graduate student, saying that Maier is a good lab man although not as good as Peter Gray. I told him about Harris Levy as a possible man for Argonne; we also discussed Jim Wallmann, who is employed by du Pont here at Argonne. Although Burris would like to have Wallmann return to Berkeley to work with him, I was told that Argonne has already made him an offer.

The Argonne group (Winston M. Manning, Sherman Fried, Martin H. Studier, Paul R. Fields, H. Diamond, A. Hirsch, J. F. Mech, G. Pyle, John Huizenga, and others) met with me in the conference room of "F" corridor of the chemistry building. I feel I made it clear to them that the UCRL spontaneous fission systematics had misled us in our initial assignment of the 6.6 Mev alpha activity to element 100, saying that our systematics hadn't anticipated the high spontaneous fission activity in the californium fraction, which overlapped into the transcalifornium region in our ion exchange separations; I emphasized that we had recognized that the 6.6 Mev alpha activity is due to an isotope of element 99 within a day or two after we mailed them a copy of the memorandum that assigned this activity to element 100. There was a lot of discussion; Studier pointed out the success he felt he was having with the Kramish spontaneous fission systematics, and Huizenga pointed out his idea, which was new to me, that the spontaneous fission rate for a given Z seems to go through a maximum with increasing A. In a comparison of intensities of activity of the 6.6 Mev alpha activity, it seemed that the Argonne group had something of the order of at least 50 times as much as the Berkeley group. I was also told that the first Argonne elution experiment took place on December 23 (four days after my phone call of December 19 to Manning). I was given the results of Mark Inghram's mass spectrographic analysis of the plutonium and uranium fractions and was told that the ratio of 6.6 to the 6.35 Mev alpha activity was about 5 in their work. They had, at this time, not yet observed any decay in the 6.6 Mev alpha activity, and I told them that our group appears to be observing a decay.

After the meeting I met with Fried and Manning in Manning's office and suggested that there should be some sort of compromise between the laboratories about the matter of discovery, but Manning said that he was not willing to discuss this without further consultation with his group. I had no choice but to accept.

During the day President Sproul telephoned me with some instructions about the NCAA meeting. He told me to inform Jones (Claude E., new UCLA Faculty Athletic Representative) and Freeborn (Stanley B., my predecessor) that he supported the construction of an amendment to give authorization to the 17-member Council of the NCAA to take action between NCAA meetings, excluding suspension or dismissal, in cases of flagrant violations. Sproul said that the PCC presidents have decided that the

NCAA wording covering financial aid to athletes is too loose and that Hugh Willett (of USC) will present an amendment. The presidents support the change for spring football practice (20 sessions) from 30 to 36 days but want to eventually eliminate spring practice altogether. He also said that we should follow our conscience with regard to the television agreement between the Big 10 and the PCC. He said the PCC presidents opposed postseason games (other than the Rose Bowl) for PCC football teams. Sproul offered other advice and mentioned that he is opposed, although not strongly, to all postseason Bowl games.

I telephoned Ghiorso in Berkeley to talk with Ghiorso about the surprising news I heard this morning regarding the Argonne work on transcalifornium elements.

Later I interviewed Donald Peppard and learned that he is making \$10,620 per year, is 40 years old and has three sons. (Peppard is being considered for the position as Associate Director of Chemistry of Project Whitney.) I also talked with John L. Maier.

An Argonne car drove me to the airport for the 6 p.m. Capital Flight 902 (a Constellation) to Washington. I arrived in Washington at 9:25 p.m. and checked in at the Statler Hotel.

Thursday, January 8, 1953

In Washington. I called and then went to see Donald Lane. Lane showed me the brief about the validity of Contract eng-30 that he is about to file with the AEC, and I made a couple of suggestions.

I also telephoned both Doyle Northrup and Spof English to tell them about the latest Berkeley results and made arrangements to meet with them tomorrow.

The NCAA meeting at the Mayflower Hotel was taken up with reports and discussions about television, including various types of subscription television: 1. Telemeter (a \$7 coin box at set), 2. Skiatron (method of key cards sold anywhere or sent through mails), 3. Phonevision (Zenith--by phone or by combination). There were pro and con opinions about controlled television and the NCAA Committee report, which Hall (Yale) and Asa S. Bushnell (Commissioner, Eastern Collegiate Athletic Conference) supported and Edward W. "Moose" Krause (Athletic Director, Notre Dame), a representative of the University of Pennsylvania, and a representative of Du Mont opposed (stating they oppose controlled TV).

I attended a reception in the East Room of the Mayflower Hotel (from 5 p.m. until 7 p.m.), hosted by the Southern Conference, whose President is C. M. Farrington and which includes institutions in the City of Washington (George Washington University) and adjacent areas.

[In Berkeley, the research group met as usual with the following people in attendance: Asaro, Carniglia, Carr, Clark, Cobble, Dauben, Grover, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Levy, Michel, Nervik, Passell, Perlman, Raby, Rasmussen, Slater, and Templeton.

Slater reported that he has been working on the d,p reaction on

palladium; he presented cross sections for a number of energies: 50 Mev = 5.0 mb, 100 Mev = 1.9 mb, 150 Mev = 0.64 mb, and 190 Mev = 0.28 mb. Slater said that these give a straight line plot for the region above 50 Mev with the slope more like the case of bismuth than thorium and uranium. He commented that there may be a closed shell at 64 neutrons that may cause the same effect as in the bismuth case. Iz and Rasmussen talked about the effect of the neutron binding energy on the slope.

Clark talked about his study of the tribromides of the rare earths, which have structures similar to those of aluminum trichloride. He said that he has one powder pattern of yttrium tribromide, in which there is strong evidence that the bromine atoms are in hexagonal close packing, but he will need a better powder pattern to say whether the complete structure is the same. Clark also talked about his method of growing single crystals.

Michel summarized some work he has done on the light cesium isotopes in the time of flight isotope separator:  $Cs^{125}$ , 45 m,  $\beta^+$  2.03 Mev;  $Cs^{126}$  ?;  $Cs^{127}$ , 5.5 h,  $\beta^+$  1.4 Mev;  $Cs^{128}$ , 3.8 m,  $\beta^+$  (not done on  $\beta$ -ray spectrometer);  $Cs^{129}$ , 32.8 h, EC;  $Cs^{130}$ , 30.0 m,  $\beta^+$  1.97 Mev. Michel went over the production of these isotopes, which he and Mathur are producing by bombardment of iodine with helium ions.

Asaro talked about the gamma rays of ionium ( $Th^{230}$ ) and compared his results with those of Joliot-Curie.

Passell reported that he has worked up some old  $Pa^{232}$  data that O'Kelley and Jenkins obtained in 1951. He presented a decay scheme based on this work and on some information he found in the literature.

Perlman presented some results obtained by Biller (who has completed his work for his Ph.D. degree and will go to work for the M. W. Kellogg Co. in New Jersey) in his study of the high energy fission of bismuth. Biller was able to construct a contour map of yields, from which he was able to predict the yields of isotopes that were impossible to measure directly.

\* \* \* \* \*

Helen went to a PTA meeting in the afternoon and then visited her friend Bee during the evening.]

Friday, January 9, 1953

In Washington. There were a number of talks at the NCAA meeting today, such as those by President Cloyd H. Marvin (George Washington University), Carl G. Snavely (the head of the Football Coaches Association), and Illinois Senator Paul Douglas. I also talked with Al Masters (Athletic Director at Stanford University) about the Pat Cannamela incident (in the Berkeley-USC football game of 1951), and he told me that Stanford President Wallace Sterling will back us in action against USC in this connection.

I went to the AFOAT headquarters and had a conference with a group including Doyle Northrup, Wilbert H. Urry, Malcolm Henderson, Spofford G.

English, Daniel R. Miller, Elwood M. Douthett, Don Rock, General William M. Canterbury (who is in charge of the collection of debris from the nuclear weapons tests), George M. Kavanagh, and others. We discussed detection work in general as well as the Berkeley and Argonne work on the 6.6 Mev alpha activity. I successfully arranged with Northrup for more B29 filter papers (Major Russell will deliver from Sacramento). We agreed to look at debris from the 500 kt King shot. I talked with English and others about publishing our work in UCRL secret reports, and I agreed to send English a copy of our memorandum when I return to Berkeley. English also mentioned that we have \$100,000 excess in our budget.

Later I attended a business session of the NCAA. There will be secret balloting on the TV question. We heard reports from vice presidents, committees, etc., including one by Fritz Crisler, (Athletic Director, University of Michigan and Chairman of the Football Rules Committee), who spoke in favor of the free substitution rule in football. Ralph W. Aigler (University of Michigan) reported for the Executive Committee on NCAA office and business procedures--the national office will be moved from Chicago to Kansas City, the 1954 Convention will be held in Cincinnati, and the 1955 Convention in New York (second week in January). A. B. Moore (University of Alabama) reported for the NCAA Council on the actions of various committees. Wilbur Johns (Athletic Director, UCLA) reported for the Extra Events Committee--there will be nine certified postseason bowl games after 1952, the conditions for the bowl games were described, and the report was adopted by unanimous vote.

In the continued discussion about television, Browne (University of Illinois) moved that the Hall report with its resolution on control be adopted, but then Father Joyce (Edmund P.) (Notre Dame) spoke against the report, saying that the division of profits is a manifestation of socialism. Murray (University of Pennsylvania) also spoke against the report. The actual plan for the 1953 season will be submitted by mail, with the requirement of a 2/3 vote for adoption. The secret vote about the adoption of the Hall report was 172 voting for the plan and 13 voting against it (a majority was needed for the vote to carry).

Saturday, January 10, 1953

In Washington. Again I attended the NCAA meeting. Today was a business session and the length of the playing and practice seasons was discussed. Paul J. Blommers (Iowa) spoke for the Committee on Playing and Practice Seasons with the help of a mimeographed handout. Each institution should eliminate extreme practice sessions. The football season should start at a defined time, and the length of spring practice should be restricted. For basketball, the number of games should be limited to a maximum of 26, practice should start the third week in October, the season should start in December; an ideal schedule would be 22-24 games with practice to start November 1. Aigler moved acceptance of the report and that the report be printed with implementation to be worked out later; this motion carried. Willett gave the report of the Membership Committee. There was a discussion of the procedures to be followed in cases of infraction of rules. There are three cases before the NCAA for circularization, and ten more cases pending. The report was accepted.

I left Washington at 1:30 p.m. via United Flight 647 for San Francisco. A lab driver picked me up when the plane arrived at 9:55 p.m. and drove me home to Lafayette. It has been a most full and interesting trip, but I am happy to get home.

[At the lab more B29 filter paper samples arrived from Major Russell at Sacramento, as the result of Doyle Northrup's instructions.]

Sunday, January 11, 1953

The kids were happy to see me this morning, and Pete told me that he went to Cathy Steinbach's birthday party yesterday. I spent considerable time with the children before looking over the mail that Iz brought home Friday for me.

Included in the mail was a thank-you note from Felix Bloch for my congratulatory letter about the 1952 Nobel Prize in Physics.

I read a January 6 letter from Jerry (Jerome J.) Howland, who reported that he was not made a member of the permanent staff at Brookhaven. Jerry said that, although there were a few interesting jobs available in that part of the country, he has not been enthusiastic about them, partly because he has been bothered more than usual this year by hay fever and allergies. He is beginning a terminal vacation, hopes to visit Los Alamos, and has been told by Winston Manning to investigate the possibilities at Livermore. Jerry added that he probably will not reach Berkeley until the week of February 9.

A short letter had arrived from Warren H. Crowell (President, UCLA Alumni Association), thanking me for keeping him posted as to my dates in the Los Angeles area and saying that he hopes to have some definite information about the plans for Charter Week in connection with my receipt of the UCLA Alumnus of the Year Award.

There was also a note from the secretary to Professor A. Wormall thanking me for some reprints Wormall had received from us (Wormall is in Brazil until April).

Another thank-you letter came from Stan McCaffrey for my nomination of Mrs. Lillian M. Gilbreth and Dr. Selman Waksman for the Berkeley Alumnus of the Year Award for 1952.

I also had phone conversations with Stan and Al and learned that they have arranged to receive additional "fallout" material from Louie (Louis B.) Werner (a former graduate student of ours, now at the Naval Radiological Defense Laboratory). This will give us a larger sample with which to work.

Although I had seen the memo in Chicago, I studied "ANL-WMM-1059" by H. Diamond, P. Fields, A. Hirsch, J. Mech, G. Pyle, and M. Studier on "Results on the Transplutonium Fraction from the Mike Debris."

I also read a letter from Jos. W. E. Harrisson (Chairman, Philadelphia Section, ACS) with information about their meeting on

February 19, during which I will receive the John Scott Award.

Monday, January 12, 1953

This morning, in addition to long conversations with Stan, Al, and Gary about the Chicago work, I received and made a number of telephone calls. Spof English telephoned, suggesting I prepare our report about the work on Mike debris and send it to him and Charles Marshall (AEC Declassification Officer). Spof also said that he will be at Argonne on Wednesday and that he will tell them to do the same thing. I immediately sent Spof a copy of our memorandum on these experiments "MB-IP-719." I wrote, "Although there have been advances made since the date of this memorandum, I am sending it in its original form without corrections. In particular, the statement at the top of page 4 where the elution experiments are stated not to differentiate between element 99 and element 100 should be modified because an elution experiment run a day or two later showed unequivocally that the 6.58 Mev alpha particle is due to an isotope of element 99, perhaps  $^{99}_{\alpha}^{253}$  or possibly  $^{99}_{\alpha}^{251}$ . Also the spontaneous fission was shown soon after this memorandum was written to reside entirely in the element 98 fraction, in violation of the most simple spontaneous fission systematics but presumably in agreement with a somewhat more involved overall treatment of spontaneous fission data."

I responded to the December 31 letter from Bernard Harvey and said that his letter is in accord with my memory of our verbal discussions: namely, "that there is a definite possibility that you will become a United States citizen but that this is not a certainty". I told Harvey that, as he requested, I have again called this to the attention of our Security Division and have given them a copy of his letter in order that there can be no misunderstanding. I then wrote that "we are looking forward to seeing you here near the end of March or at such time as you find that you can reasonably make the change".

On January 6 Stan McCaffrey wrote inviting me, along with Dr. Herman A. Spindt, Dr. Ira B. Cross, Dean Mary B. Davidson, Professor Joel Hildebrand, Professor Frank Kidner, and Dean Knowles Ryerson, to serve on the Berkeley campus Faculty Alumni House Committee. Since the first meeting of the Committee was scheduled to be held at lunchtime in the Women's Faculty Club on Thursday, Doral had called McCaffrey's office and explained that I would not receive the invitation until today. Today I sent back the card to say that I will serve and that I will attend Thursday's meeting.

I also signed a letter that Margie composed to Scholtens & Zoon (Academische Boekhandel, Groningen, The Netherlands), who had asked on December 29 for literature on elements 99 and 100. I wrote, "Although there may be a few cases of predicted properties of elements 99 and 100 in the literature, I know of no case in which the synthesis of these elements has been reported."

During the day I also telephoned Greg Engelhard about some athletic matters. We made arrangements to have lunch together on Friday. During the conversation I asked Greg about the procedure for handling my expenses to the recent conferences--the PCC and the NCAA--which will be reimbursed by the Athletic Department of the ASUC.

The senior staff held its regular noon brown-bag luncheon meeting in my office. Much of the discussion was about the Argonne work on elements 99 and 100.

In today's mail was a January 9 letter from Donald Lane, who enclosed a copy of the brief that he filed with the AEC. Lane said that he followed my suggestion and revised the reference to the letter from Dr. Bush, which in effect urged the applicants to accept Capt. Lavender's proposal. Lane wrote that the AEC General Counsel will probably file a reply brief on February 9.

I also received and read an informative letter from Stanley Parton (State Vice-Chairman, Distinguished Service Awards) about the program on January 17 at San Jose for the awarding of the Junior Chamber of Commerce Awards to the five Outstanding Young Men of the State of California for 1952.

Tuesday, January 13, 1953

I spent some of the day on campus, talking with various colleagues.

On the hill I talked with the fellows about the Mike work. I also worked on various writing projects, such as the editorial I am preparing for Reactor Science and Technology.

Wednesday, January 14, 1953

This morning Professor E. T. McBee (Purdue University) telephoned to ask me about a couple of people in whom they are interested. One was Bill (William L.) Jolly from UC. I told McBee that I thought he is considered to be a good man but that I will check with Wendell Latimer. McBee also asked about John Turkevich of Princeton University; in this case, I said that I certainly would hire him, if possible. I then called Latimer and learned that he considers Jolly, who is presently an instructor here, a better man than Alan W. Searcy, a UC Ph.D., who is presently on the staff at Purdue. I also telephoned Jolly and learned that he is interested in academic work but he is somewhat concerned about the draft. Ken Street also mentioned how highly he thinks of Jolly, and so I then telephoned McBee and reported this information. I said that we are interested in having Jolly work with us; however, I assured McBee that it would be ethical for him to make Jolly an offer. McBee added that they may be able to offer Jolly an assistant professorship.

As Greg Engelhard suggested, I sent him an itemized list of my expenses to date, noting that I can legitimately charge the San Francisco to Chicago and return part of my trip to Washington to the AEC.

I recently received a letter from Albert A. Caretto, Jr. (The University of Rochester), describing his background and asking if a postdoctoral fellowship might be available in Berkeley. Today I wrote Caretto, said that we do have a few positions here that are essentially postdoctoral fellowships, but our budget limits us very severely in this respect. Since I do not know yet how we will stand on this matter next fall, I suggested that he have Dr. Edwin Wiig and Dr. Seymour Katcoff send me letters of recommendation. I also mentioned that he might

consider an outside fellowship, such as one from the National Research Council or one from the Atomic Energy Commission.

Also, in this morning's mail was a letter from Winston Manning, who said they would like very much to explore with Gary Higgins the possibility of his joining their staff in nuclear chemistry. I immediately telephoned Manning and told him that Higgins has already joined Ken Street's group on the Whitney Project.

I declined a December 30 invitation from Mr. Robert E. Robinson (Chairman, Committee on Drama, Lectures, and Music, University of California, Santa Barbara) to speak this coming spring; however, I wrote that if I could have a rain check on the invitation, I should like to pay the campus a visit during some future season.

I wrote a note to Dr. Joseph C. Boyce (Associate Laboratory Director, Argonne) to thank him for the letter of recommendation he wrote in behalf of John C. Maier (I gave Boyce's letter to Miss Kittedge in the Chemistry Department office). I also wrote to Maier to remind him that, if he decides to apply for admission, he should send a transcript of his university record and other supporting letters to Dean K. S. Pitzer.

In addition to my usual tour of the labs, I read some of the reports and journals that have arrived recently.

Helen went to the tea for the chemistry wives, which was held this afternoon at the home of Rebecca Cason.

At 6 p.m. I had dinner at Spenger's (1919 4th St., Berkeley) with a number of lab people and Willard F. Libby, presently a member of the GAC.

#### Thursday, January 15, 1953

The following were present at the research group meeting this morning: Asaro, Carniglia, Carr, Clark, Cobble, Cunningham, Feay, Grover, Hoff, Hollander, Jaffe, Kalkstein, Lessler, Levy, Nervik, Passell, Perlman, Raby, Rasmussen, Ruben, Seaborg, and Stephens.

Carniglia gave a rather lengthy talk on their method of heating the crucible in effusion measurements, utilizing respectively an induction heater and a heating coil.

Nervik presented an elution curve (40 cm of 4% cross-linked Dowex-50 resin eluted with lactic acid--pH 3.40) of rare earth elements from a bombardment of tantalum with 340 Mev protons. The elution time to the peak took from 2 hours 56 minutes for lutetium to 16 hours 30 minutes for samarium. There was considerable discussion about the resolution, yields, etc.

Levy talked for the rest of the meeting about neutron and proton binding energies, the Fermi gas model, and the independent particle shell model.

\* \* \* \* \*

At 10:10 a.m. I gave the final Chemistry 123 lecture of the semester, speaking on the heavy elements and atomic energy.

After the lecture I stopped in the Department office and spoke with Dean Pitzer about a couple of potential graduate students--John L. Maier (whom I met on my last visit at Argonne) and William P. Bryan (a UCLA graduate student of Ralph James).

At noon I met in the Conference Room of the Women's Faculty Club with members of the Berkeley campus Faculty Alumni House Committee, whose purpose is to consider how the campus faculty can contribute to the effort of the California alumni to raise funds to build the Alumni House on the Berkeley campus. It was decided that statements on the worthiness of the project, prepared individually, to be used for publicity would be helpful to the campaign.

At 1:15 p.m. I met with Roger Batzel, who discussed some of the problems of the Project Whitney program.

During my rounds of the labs, I found that the fellows--Stan, Gary, and Al--have dissolved the additional B29 filter samples, together with the "fallout" material they received from Louie Werner, and are making an elution run. This should be a significant sample. Al said the pulse analysis should be completed by some time tomorrow.

When I got home, Helen announced that Lynne has the flu, which means that Helen will be busy for a few days as the other kids take their turns with the bug. Hopefully, I will avoid it.

Friday, January 16, 1953

Before going to campus for a 10 a.m. appointment with Eugene Burdick, at which I reported on the NCAA meeting, I spent some time with Stan, Al, and Gary and the data they are obtaining with the new sample of Mike debris.

Greg Engelhard and I had lunch together--we talked about the NCAA meeting and some of the points that Burdick passed on to me this morning from Kerr.

The mail, which I picked up on campus, included an advance invitation from Clint (Clinton W.) Evans (Head Baseball Coach) to attend the annual luncheon of the old baseball group at the YMCA building next to Edwards Field on February 21. Clint explained that this is really a traditional luncheon that is followed by a game between the alumni and the varsity.

Marv Kalkstein recently reported to me that his draft status had been changed to 1-A, and I asked Earl to prepare a memo for George Everson to use as backup material in his request for reclassification. Today I signed the memo, which described Kalkstein's background, research program, his part in the Whitney chemistry program, and emphasized the current small graduating classes from the nation's universities and the present great demand for scientists.

When I checked with the fellows--Al, Stan, and Gary--I learned that

they have positively identified a 7.1 Mev alpha activity as due to an isotope of element 100 with an intensity of about 4% of that of the 6.6 Mev alpha activity at this time.

This evening Helen reported our second case of flu--Pete.

Saturday, January 17, 1953

I worked on my writing for much of the day.

I learned from Al and Stan that the 7.1 Mev alpha activity is decaying but the data are insufficient to establish the half-life with any accuracy.

Later in the day I went down to San Jose with Clark Kerr for the reception (6:00 p.m.) and banquet (7:30 p.m.) of the California State Junior Chamber of Commerce Awards ceremony at the Sainte Claire Hotel (Market and San Carlos Sts.). [The officers of this group are Charles Johnson (State President) and Ray Gray (San Jose Chapter President).] I introduced the judges--Elystus Hayes (attorney), Dr. Clark Kerr, Dr. John T. Wahlquist (President, San Jose State College), James Mussatti (Manager, California Chamber of Commerce), Paul C. Smith (former Editor, San Francisco Chronicle)--to the audience. Each judge presented the Award to one of the five Outstanding Young Men of California for 1952. Bob Mathias was unable to be present because he was named one of the 10 Outstanding Young Men of the Nation and was receiving this honor in Cedar Rapids, Iowa; however, I read the following resumé about Mathias:

Born and reared in Tulare, California, is 21 years of age, unmarried.

He was an outstanding athlete in track and football in Tulare High School and also at Kiski Preparatory School in Pennsylvania. In 1948, at the age of 17, he became the youngest Olympic Decathlon Champion at London, England. He was also awarded, in 1948, the James E. Sullivan Trophy as the amateur athlete who did most to advance the cause of good sportsmanship.

Bob Mathias, successfully defended his A.A.U. Decathlon Title in 1949 and 1950. In the fall of 1951 he began playing football at Stanford University and led them to the Pacific Coast Championship and the Rose Bowl.

In 1952, he successfully defended the Decathlon Title at the Olympic games at Helsinki, breaking his own record.

We feel that Bob Mathias is eminently qualified to be one of the Outstanding Young Men of our state as a result of the world renowned performance in the field of athletics and the inspiration he has been to the youth of our country.

Awards also went to Dr. William Muller (Medicine), Dr. Stanley Skillicorn (Medicine), William S. Mailliard (Congressman), and Warren Dorn (Civic Enterprise). Robert R. Gros (Public Relations Director, Pacific Gas and Electric) was the speaker after the banquet.

Sunday, January 18, 1953

Dave had the flu this morning, and I spent most of the day in my study writing. I also prepared some questions, which will be combined with Iz's, for the Chemistry 123 final.

When I checked with Al and Stan, I was told that they can say only that the 7.1 Mev alpha activity has a half-life between one-half and two days.

Monday, January 19, 1953

Steve was obviously ill this morning, and Helen reported that she felt that she was also getting the "bug."

The first thing this morning was a conference with Al, Stan, and Gary. The half-life of the new activity (element 100) is still between one-half and two days, and we concluded that this activity is being kept alive by a long-lived beta emitter in the element 99 fraction. We have also learned that Los Alamos found a 7.1 Mev alpha activity in a transplutonium actinide fraction before our fellows did; however, they did not chemically identify the activity.

It is fortunate that I have been working on my editorial for the classified journal, Reactor Science and Technology, for I received a teletype from Frank R. Ward (Editor) this morning, who asked if I plan to submit it by February 1; I was able to reply that he can expect the editorial by February 1.

Gordon S. Watkins (Provost, Riverside campus) wrote on January 7 with details about the Charter Day ceremony and an invitation for Helen to accompany me. Today I wrote to inform him that Helen will be unable to accompany me, but that I would like to accept his offer to send a car to the Los Angeles area (my parents' home at 9237 San Antonio Avenue in South Gate) for me.

I learned from Miss Kittredge that Dean Pitzer has accepted William Bryan (of UCLA) as a graduate student, so I wrote the usual letter to Bryan about employment with us at the Radiation Laboratory, the type of research, the clearance, etc. A copy of my letter was sent to both Dean Pitzer and Mr. Everson. To Everson I explained that a salary equivalent to a second-year graduate student is called for because Bryan has a master's degree.

Bill Giauque recently talked with me about again nominating Samuel Jackson Barnett (my UCLA physics professor) for the Nobel Prize in Physics. I wrote to the Nobel Committee in Stockholm, requesting that they use my letter of January 23, 1952 as part of the present nomination. I pointed out that this thus suggests that S. A. Goudsmit and G. E. Uhlenbeck be considered jointly with Barnett for the 1953 Prize.

The senior staff had its regular Monday noon luncheon meeting in my office. Again, much of the conversation was about the Mike work.

I gave Millie Davis my editorial for Reactor Science and Technology

to type.

In my rounds today I met Richard L. Boegner; Boegner has been hired as a replacement for Don Martin in Herman Robinson's electronics group. I also noted that the Health Chemists are beginning to prepare Room 107 (Bldg. 5) for the arrival of a slug from Arco.

Tuesday, January 20, 1953

The Chemistry 123 class had its final examination today.

Chemistry 123

Final Examination  
January 20, 1953

1. (40)

(3) (a) How would you prove whether  $\text{HNO}_3$  oxidizes iodide to iodine or iodate with only tracer quantity of radioactive  $\text{I}^{131}$ ?

(3) (b) Indicate by nuclear reactions a practical means of producing  $\text{Ac}^{227}$  using pile neutrons.

(3) (c) Write reactions and explain how it is possible that  $\text{Np}^{237}$  is produced in a pile operating with natural uranium.

(2) (d) Explain why heavy element transmutations could not be studied with a Cockcroft-Walton type accelerator.

(4) (e) Explain why both  $\text{A}^{40}$  and  $\text{Ca}^{40}$  are stable even though  $\text{A}^{40}$  is several hundred kev heavier than  $\text{Ca}^{40}$ .

(3) (f) The spin of  $\text{N}^{14}$  is 1 unit. Explain how this is compatible with the neutron-proton concept of nuclear structure and not with the older proton-electron model.

(2) (g) Explain why you would expect  $\text{He}^3$  to have a spin  $1/2$  according to the single particle shell structure concept.

(3) (h) State qualitatively why beta decay processes often proceed to excited states of the decay products rather than to the ground state although more energy would be available for the ground state transition.

(3) (i) A certain beta emitter was reported as having a five minute half-life and the end-point of the beta spectrum as 0.5 Mev. Why is this measurement probably in error?

(2) (j) Explain the origin of technetium x-rays in the decay of 6-hr.  $\text{Tc}^{99}$ .

(4) (k) Predict the possible mode or modes of decay for  $\text{Pb}^{202}$ . Could it be beta stable? Explain.

(4) (l) What would be the principal mechanism for the absorption of (1) 150 kev gamma rays in gold and (2) 500 kev gamma rays in aluminum?

(2) (m) To what radioactive family would  $^{85}_{\text{At}}\text{At}^{219}$  belong?  
(2) (n) Why cannot a conventional cyclotron be used to accelerate electrons to 10 Mev?

2. (10)

(5) (a) What weight of RaD ( $\text{Pb}^{210}$ ) is present at equilibrium with one curie of radium ( $\text{Ra}^{226}$ )?  
(5) (b) The age of the elements is estimated as  $3.0 \times 10^9$  years. What was the isotopic composition of potassium at the time of their formation? State any assumptions made.

3. (10)

The capture cross section of  $\text{Au}^{197}$  for thermal neutrons is 100 barns. If a thin gold foil weighing 10 grams is irradiated in a neutron flux of  $10^{12}$  neutrons/cm<sup>2</sup> sec., what will be the saturation activity of 2.7 day  $\text{Au}^{198}$ ?

4. (10)

(8) (a) The proton energy threshold for the reaction  $\text{Cl}^{37}(\text{p}, \text{n})\text{A}^{37}$  is 1.64 Mev. Calculate the electron capture decay energy for  $\text{A}^{37}$ .  
(2) (b) What error is made in the use of 1.64 Mev as the "Q" value for the reaction as shown?

5. (10)

Show in detail why it would be very difficult to observe the alpha branching of  $\text{Ra}^{228}$  ( $\text{MsTh}_1$ ). Construct in rough fashion such graphs as would be necessary to prove your point.

6. (15)

(8) (a) Calculate the specific alpha activity of the thorium fraction from a sample of a pure thorium ore immediately after separation.  
(7) (b) Indicate qualitatively what would happen to the alpha activity of the preparation over a period of about 10 years.

7. (5)

Calculate the potential barrier of bismuth for a carbon ion ( $\text{C}^{12}$  with six units of charge).

Because of the decision made by our committee on the Alumni House last Thursday, I prepared the following statement:

The establishment of an Alumni House on the Berkeley campus strikes me as an excellent idea. Not only will this be a focal point for alumni during their return visits to campus, but it should serve admirably as a place where alumni and professors can renew acquaintanceships. The building is planned in such a way that it can provide needed space for meetings of many types and hence will certainly be an asset to the University as a whole.

Earlier this month I received a dozen Ektachrome transparencies from Earl M. Mielke, who has been taking photomicrographs of crystals, and asked for advice on improving the reliability of his method and the brilliance of his slides and prints. I had one of our amateur photographers look them over and prepare a response offering a few suggestions for me to sign today.

Truman Kohman wrote in December and on January 10 with comments and suggestions for the "Table of Isotopes," including some information on work by Kohman and G. I. Mulholland on  $Nd^{150}$ . Usually, Hollander handles the correspondence connected with the "Table of Isotopes," but I felt the necessity of writing to Kohman personally. In response to his comments about changing the name of the document, I wrote:

...The term "nuclide" was used in the descriptive material but the title "Table of Isotopes" was retained with the implication that this means "Table of the Isotopes of All the Elements." Actually my two coauthors were more opposed to a change of title than I was, my attitude being one of not caring too much one way or another...

During my tour of the labs, I was told some Hanford barrels arrived today.

Chapter Seven in Robert de Roos' "Portrait of a University," which appeared in the "Panorama" section of today's San Francisco Chronicle featured the stories (with pictures) of the six Nobel Prize Winners of the University of California: Glenn T. Seaborg, Edwin M. McMillan, William F. Giauque, Ernest O. Lawrence, Wendell M. Stanley, and John H. Northrup.

Wednesday, January 21, 1953

I received and made the usual phone calls, including one to Spof English about our Mike work, and then proofed and mailed to Dr. Frank R. Ward (Editor, Reactor Science and Technology) my editorial for this classified journal. I began:

Some of the guest written editorials in preceding issues of this journal have made interesting suggestions and raised provocative questions in connection with the progress and program of the Atomic Energy Commission. With such friendly and possibly constructive criticism as a pattern it seems reasonable to the present writer to fulfill his assignment in a similar vein. Having served as a member of the General Advisory Committee for the first three and one-half

years of the A.E.C. era, the writer is in a somewhat unfortunate position from the standpoint of choosing an area for concentration; he has had a unique opportunity to make criticisms and has usually found that they were either acted upon or the reasons for not doing so were clearly developed.

However, one suggestion, dating from the first meeting of the Commission in January 1947 and brought up many times since, has never been accepted and convincing reasons for not doing so have not, in our opinion been forthcoming. This has to do with the recovery of certain by-products, especially some important isotopes of transuranium elements, on a routine, continuous basis at the present and future plutonium production plants. Particular reference is made to  $\text{Np}^{237}$ , formed during the operation of plutonium production reactors, and  $\text{Am}^{241}$ , which is best recovered by extraction from the  $\text{Pu}^{241}$  which is present in the plutonium in the course of the purification process following the decontamination and isolation procedure....

I then went on to describe the uses for these nuclides and what scientifically may be learned from them.

Iz, our reader, and I spent much of the day correcting the Chemistry 123 final examinations and making out the grades for the semester: David L. Allred 59 (final), 327 (points), C (grade); John F. Below 80, 486, A; Heber C. Brill 57, 420, B; Roman I. Bystroff 67, 406, B; Edward S. Clark 72, 464, B; Nancy G. Colby 48, 331, C; William P. Cox 91, 523, A; John W. Crump 86, 503, A; Clarence M. Davenport 75, 486, A; Margaret J. Edson 66, 413, B; Walton P. Ellis 75, 470, B; Belmont S. Evans 67, 452, B; Stanley D. Fair 63, 393, B; Don J. Farqunar 67, 415, B; Watson B. Fearing 60, 317, C; Carl E. Fieber 60, 351, C; Joseph J. Franaszek 60, 391, B; Peter R. Gray 87, 496, A; Edward A. Grens 72, 463, B; James R. Grover 89, 525, A; Joseph F. Gustaferro 69, 413, B; Rex Gygax 71, 430, B; Loren G. Hepler 82, 492, A; Terrence V. Hogan 67, 388, B; Alfred Holston 79, 493, A; Peter O. Jackson 78, 410, B; Paul R. Kromann 85, 464, B; John W. Kury 85, 507, A; Richard M. Lessler 68, 429, B; John R. McCarthy 57, 335, B; Don S. McClelland 70, 452, B; Ralph D. McLaughlin 68, 428, B; Gilbert J. Mains 84, 487, A; Hirdaya B. Mathur 75, 462, B; Stanley A. Meyerhoff 60, 389, B; Mary S. Nakata 48, 283, C; Hawkins Ng 58, 332, C; George Pavlov 40, 246, C; Robert L. Pierce 66, 389, B; Bruce A. Raby 78, 462, A; Everett E. Roberts 90, 521, A; Robert M. Rodden 80, 474, B; George G. Sapp 49, 337, C; Julian J. Schamus 63, 358, C; Rex H. Shudde 85, 518, A; Hugh C. Silcox 28, 209, D; Stanley G. Smith 87, 507, A; Warren G. Smith 80, 492, A; Lawrence C. Snyder 80, 457, B; Frank S. Stephens 92, 542, A; Donald G. Stoffey 63, 349, C; Joseph P. Surls 94, 560, A; Mikio Suzuki 58, 328, C; Krisna Tantranon 74, 386, B; Robert D. Thompson 48, 362, C; Ulrich Toggweiler 30, 217, D; Vernon H. Troutner 62, 413, B; William E. Wallace 83, 491, A; Charles L. Weaver 69, 449, B; Byron G. Weissberg 80, 443, B; Robert A. Wessman 42, 277, C; Lawrence W. Wolf 59, 382, B; David D. Work 79, 486, A; and Harold J. Zabsky 50, 336, A. The total possible number of points was 590.

Thursday, January 22, 1953

The attendance at the research group meeting this morning was smaller

than usual, probably because some of the students are having final examinations: Asaro, Batzel, Cobble, Dauben, Feay, Hoff, Hollander, Hulet, Hyde, Kalkstein, Lessler, Michel, Rasmussen, Seaborg, Slater, Stephens, and Templeton.

Asaro spoke about some work they have done on the second excited states of even-even nuclei. He said that they have observed the alpha spectra of  $\text{Th}^{228}$  in partial equilibrium with its daughters. Asaro noted that Rosenblum found two alpha groups separated by 85 kev, the higher energy group being in 72% abundance. Asaro said they found an 84 kev separation and the same relative abundances. He reported that Rosenblum obtained six conversion lines ( $\beta^-$  spectroscopy) corresponding to an 84.3 kev gamma ray while he (Asaro) found another alpha group at 5.17 Mev with an intensity of 0.2% relative to that of  $\text{Ra}^{224}$ . Rosenblum found an alpha group at 5.19 Mev, which he assigned to  $\text{Ra}^{224}$ ; Asaro reported that they found an alpha group at 5.19 Mev also and tentatively assigned the alpha group at 5.17 Mev to  $\text{Th}^{228}$ . Asaro went on to talk about the energy levels of  $\text{Pb}^{208}$ , which are reached by  $\beta^-$  decay of  $\text{Tl}^{208}$ . He discussed the experimental and theoretical conversion coefficients and proposed a decay scheme. Asaro also reported some of the experimental data for  $\text{Pu}^{240}$ ,  $\text{Th}^{228}$ , and  $\text{Ra}^{224}$  are still tentative.

The other person to report was Hoff, who talked about gamma rays in  $\text{Pu}^{237}$ , which was produced in a proton bombardment of 100 mg of  $\text{Np}^{237}$ . The  $\text{Pu}^{237}$  was separated from  $\text{Np}^{237}$  by ion exchange methods. Hoff said they saw K x-rays of plutonium or neptunium on a scintillation counter and a gamma ray of about 66 kev. The latter was distressing because  $\text{Pu}^{237}$  possibly decays by K capture to  $\text{Np}^{237}$ , whose levels are quite well known from  $\text{Am}^{241}$  alpha decay and do not include a 66 kev transition. Hoff presented the level scheme of  $\text{Np}^{237}$  as determined by alpha decay and said that he plans to try to count coincidences between K x-rays and the 66 kev x-ray; this would prove that the 66 kev gamma ray occurs during K capture. There was some discussion, and I suggested that, if it is not K capture, perhaps there is an isomeric transition. I pointed out that if so, it should be so highly converted that he should not see the gamma ray.

\* \* \* \*

I stopped in and talked with some of my colleagues in the Department before going up to the hill.

On recent trips it has been suggested to me, particularly by Herb (Herbert C.) Brown (Purdue University) that Professor Hermann Irving Schlesinger (Department of Chemistry, The University of Chicago) should be nominated for the Nobel Prize in Chemistry. There seemed to be considerable enthusiasm about this, and I have agreed to submit the nomination, based on material supplied by Brown, Joe Katz, and others; this arrived in this morning's mail, and I immediately began working on the letter.

John Rasmussen and his family will leave shortly for Sweden, and I have had several conversations with him about the trip. I suggested that John "feel Professor Siegbahn out" about the possibility of some sort of an exchange program so that Stan Thompson can spend a sabbatical at the

Nobel Institute.

I made the usual tour of the various labs and talked for some time with Stan and Al about the work on the Mike debris. I learned that Ken Hulet, who works with Stan, passed his prelims today with the comment "fair exam." His committee consisted of F. Campbell Williams (chairman), Iz Perlman, William F. Giauque, William D. Gwinn, William G. Dauben, Roy Overstreet, and Jack M. Peterson.

I attended a dinner meeting of the Athletic Board at the Durant Hotel this evening in my role as Faculty Athletic Representative. This meeting, which is held periodically during the school year is attended by Brutus Hamilton, Greg Engelhard, and alumni and student representatives. We discussed the various items that were covered at the meeting of the NCAA in Washington earlier this month.

Friday, January 23, 1953

Included in my phone calls this morning was one to Donald Peppard (Argonne) to ask if he had made a decision about my job offer. Peppard said that he has written me that he has decided not to accept.

Other phone calls went to Clark Kerr and President Sproul to discuss athletic matters.

I completed and mailed to the Nobel Committee for Chemistry my nomination of Professor Schlesinger for the 1953 Nobel Prize in Chemistry. I began:

I should like to nominate Dr. Hermann Irving Schlesinger, Professor of Chemistry at the University of Chicago, to receive the 1953 Nobel Prize in Chemistry. Professor Schlesinger's contributions to chemistry have been many and varied, but I base this nomination chiefly on his contributions to the chemistry of the boron hydrides, the borohydride salts, and lithium aluminum hydride, and to the application of these compounds in science and industry, particularly in organic technology. My evaluation of his principal contributions in this field are elaborated in the following.

I then went on to detail Professor Schlesinger's work in this field.

In the mail was a note from Dimitri R. Stein (American Representative of the Gmelin Institute of Inorganic Chemistry in New York), who thanked me for my letter of December 23 and the reprint I sent him for the Gmelin Institute. Stein added that he was surprised that a couple of the items he requested were still classified and expressed his appreciation for my suggestions on how to obtain the remaining items.

Clarence E. Larson (head of the Oak Ridge National Laboratory and whom I have known since Manhattan Project days) is visiting the area; I talked with him for a while--he described some work on nuclear reactions with 25 Mev nitrogen nuclei by Alexander Zucker--and I showed him through some of our labs.

Also, Bill Jenkins is passing through town on a business trip and came up to the lab to talk with some of the fellows. Helen and I invited him to spend the night with us, and we had some of our friends over during the evening--the Hydes, the Higgins, Lee Perlman, Herman Robinson, et al.

Saturday, January 24, 1953

Bill Jenkins spent the day with us; the kids were delighted to be with their old pal! Bill left during the evening to continue with his trip.

Sunday, January 25, 1953

Most of my day was spent reading and with the kids.

Monday, January 26, 1953

After a few phone calls, I had long discussions with Stan, Al, and Gary about their plans for the Mike work. They have redissolved the element 99 fraction from their last experiment and are running another elution experiment.

I received the letter Donald F. Peppard mentioned in our recent telephone conversation, in which he said that he has reached the conclusion that, largely because of certain personal involvements, he will not make a move at this time. I routed the letter to Iz and Earl.

The senior staff had its regular noon luncheon meeting in my office, and then I spent most of the afternoon reading.

Tuesday, January 27, 1953

I took care of some administrative matters and phone calls. Robert Daerr (Security) called about an Indian citizen, Mangipudi Kendata Ramaniah, who wants to work with us; he asked for a letter of justification. I told Doral to have Earl handle this.

I talked with Stan, Al, and Gary, who told me that their experiment yesterday showed conclusively that the half-life of the 7.1 Mev alpha emitter of element 100 is about one day. They have concluded that the mass number must be 255.

Joe Katz sent me some more chapters of PPR, Volume 14A to proof, including Chapter 5, "The Chemistry of Protactinium" by Robert E. Elson, which I looked at today. Since Elson is now at Livermore, I also plan to have him check it.

Wednesday, January 28, 1953

I phoned Bob Elson to discuss his chapter 5 ("The Chemistry of Protactinium") of Volume 14A of the PPR.

Thursday, January 29, 1953

Present at the group meeting this morning were Asaro, Batzel, Carniglia, Carr, Cobble, Dauben, Feay, Peter Rygaard Gray (my graduate student, born in [REDACTED], from Michigan College of Mining and Technology), Grover, Hollander, Hulet, Kalkstein, Levy, Michel, Nervik, Passell, Raby, Rasmussen, Seaborg, and Warren G. Smith (a graduate student from San Diego State College who has elected to do his research with us).



Peter Gray



Warren Smith

Cobble continued with his extensive report on the empirical considerations of entropy; today, he discussed the entropies of organic complex ions.

Asaro again talked about the excited states of even-even nuclei. He mentioned that last week he described a plot of neutron number of a nucleus vs the ratio of the separation between the first and second excited states to the separation between the ground and first excited states. This morning he noted that Rosenblum's data on the third alpha group (5.194 Mev) of Ra<sup>224</sup> (parent of Rn<sup>220</sup> with 134 neutrons) seems to be in error, and that he (Asaro) found, in two samples containing varying percentages of Ra<sup>224</sup> and Th<sup>228</sup>, alpha groups of energies of 5.21 and 5.17 Mev with the ratio of the 5.21 Mev group to a known Ra<sup>224</sup> alpha group varying with his samples. This means that the 5.21 Mev group does not come from Ra<sup>224</sup> and must come from Th<sup>228</sup>. There was considerable discussion, and I said that the data seem to support the possibility of a subshell at 136 neutrons, as suggested last week.

\* \* \* \* \*

After the meeting I stopped in the Department of Chemistry office before going up to the hill.

In today's mail was a letter from J. Till NeVille (Vice-chairman, Television Committee, ACS, Southern California Section), describing plans for a special show on March 15 (during the ACS convention) and reminding me that I had told Marshall Odeen (brother of my 1941 girl friend Margaret Odeen) that I would contribute to the program. NeVille asked for a draft of my proposed program.

I received and read a January 27 letter with belated congratulations about the Nobel Prize from Norman Bonner (Cornell University). Bonner made a few comments about Bob Wolke, who has applied for a postdoctoral fellowship here, and then mentioned that, if I run across any good jobs on the West Coast, he would like to hear about them for, although he likes Cornell, he is getting awfully tired of the Eastern climate.

On the hill I telephoned Frank Long at Cornell about a couple of matters, including some ACS business, and to ask for an evaluation of Norman Bonner's work.

In my correspondence I sent a note to Herb Brown (Purdue), thanked him for the information on Dr. Schlesinger, said that I put together a nominating letter, and got it off in plenty of time. I added that I think he has a pretty good chance [for Nobel Prize in Chemistry], but it may take a few years to materialize.

In response to a January 27 letter, I wrote to W. J. Sparks (Vice Chairman, Division of Chemistry and Chemical Technology, National Research Council) and told him that, although the Joint Commission on Radioactivity of the International Council of Scientific Unions is scheduled to meet in Stockholm next summer, my schedule is such that I shall be unable to attend.

I returned Chapter 5, "The Chemistry of Protactinium," of Volume 14A to Joe Katz, saying that my comments are written in red and that Elson looked at it and gave his ok.

Friday, January 30, 1953

As usual there were a number of administrative matters to handle when I got to the lab this morning. I phoned Joe Katz to tell him that some figures are missing in a couple of chapters of Volume 14A, which he recently sent me to proof.

I also talked with Rixford Snyder (Faculty Athletic Representative at Stanford) about some athletic matters.

In a note to Joe Kennedy, I reported that I have had a letter from Norman Bonner who still indicates that he is interested in locating himself in California; since we need men for new projects developing out here, I asked Joe if he still retains a high opinion of him.

Miss Kittredge informed us that Dean Pitzer accepted John L. Maier, the young fellow at Argonne whom I met through Donald Peppard, as a graduate student. Doral prepared the usual letter to Maier offering employment here at the lab, which I signed today.

I also signed a lengthy letter that Earl drafted to George Everson, justifying our offering space for research to Mangipudi Venkata Ramaniah, who wants to do graduate work in radiochemistry at the University of California.

I made the usual rounds of the labs, talked the students, and then did some reading.

Saturday, January 31, 1953

This was mostly a family day, but late in the afternoon I went to a cocktail party at the McCaffreys. Then, during the evening, I attended the Santa Clara-California basketball game with the Brutus Hamiltons and the "Brick" Mullers. The Bears defeated the Santa Clara Broncos by a score of 70 to 64.

Sunday, February 1, 1953

The day was spent with my family and working on the manuscript for my lecture for the Extension Division course at UCLA in March. My lecture is entitled "The Actinide Elements and Nuclear Power."

Monday, February 2, 1953

This was a rather busy day with numerous phone calls and conferences. I phoned Joe Katz about missing figures in one of the chapters of PPR, Volume 14A that I am proofing--he will take care of the matter. I also talked with Hoylande Young at Argonne, again about Volume 14A.

A bit later I met with Ken Street and others, such as Jolly (William L.), Hanson (Donald N.), Wilke (Charles R.), and Niday (James B.) about some of the problems associated with Project Whitney.



Donald Hanson



Charles Wilke

Today's mail brought me a January 28 letter from Geoffrey Wilkinson (Harvard), describing a postdoctoral fellowship (\$4,500) they have available. This year, he wrote, they are particularly interested in getting somebody in nuclear or inorganic chemistry. I told Doral to show the letter to Harris Levy.

Mildred Oncken (Chicago Section of the ACS) wrote, requesting a supporting letter for the nomination of Dr. Hoylande D. Young for the Austin M. Patterson Award for achievement in chemical documentation.

We recently mailed our manuscript for the "Table of Isotopes" to J. W(illiam) Buchta (Editor, Reviews of Modern Physics) for publication in that journal. This consisted of the large typed sheets that Millie Davis prepared with her IBM electric typewriter, along with the negatives we had prepared for our report version. Today I received a letter from Buchta in which he said that they have decided the best way to publish the manuscript would be to make cuts, full-page size, of the tables and references. The original large-size manuscript pages would be used rather than the negatives or prints. Buchta said that this would avoid all proof-reading but it would also mean that changes could not be made in the manuscript. He asked if this plan of procedure would be satisfactory to us. Finally, he asked if our laboratory would underwrite a page charge of \$7.50 to \$10.00 per page.

Much of the noontime senior staff meeting today was taken up with a

discussion about our work on the new elements and Argonne's effort. Neither Stan nor Al is willing to concede codiscovery of element 99. The Argonne group doesn't seem to be making a claim for the discovery of element 100--at least not yet. It was suggested that Winston Manning visit us in Berkeley to talk about the matter, and I offered to phone him about this.

At about 1:30 p.m. I telephoned Manning, ostensibly to ask about Hoylande's nomination for the Patterson Award. Then, on the subject of the new elements, I pointed out that Al and Stan are not yet willing to concede codiscovery and proposed he visit Berkeley to discuss the discovery, suggesting Monday, February 16--this he could not do, but he said that he might be able to come on Friday, February 20. I read to him the first paragraph, page 4, of our memorandum MB-IP-719 (December 19, 1953) in which we stated that our ion exchange chemical separation was not adequate in itself to differentiate between element 99 and element 100 and that we relied on our spontaneous fission systematics to assign tentatively this highly spontaneous fissionable nuclide to an even-even nuclide of element 100 (assumed to be due to the same isotope as that which emitted the 6.6 Mev alpha particles). However, by Monday, December 22 we knew that the spontaneous fission came from an isotope of element 98 (due to the tailing of element 98 into the element 99 position) and the 6.6 Mev alpha activity corresponded to element 99. I then went on to tell him about our positive identification of element 100. In addition, I gave him the dates of our work here--the first elution of element 99 on Friday, December 19, with the pulse analysis on Monday, December 22. The work on element 100 began on Thursday, January 15 and concluded with pulse analysis on Friday, January 16. I suggested that he should telephone Rod Spence and give him their results, if any, on element 100. Winston told me that they cleaned up the chamber and began new experiments the middle of last week (implying they hadn't found anything before then). Finally, he told me that they have lost faith in the 6.35 Mev alpha particle as due to element 100, and I replied that we couldn't find the 6.35 Mev alpha particle by a factor of 100 compared with the 6.6 Mev alpha particle of element 99.

At about 2:15 p.m. I telephoned Rod Spence in Los Alamos. Spence told me that they are getting new material from Eniwetok. He suggested, and I agreed to, an omnibus Ivy Report from Los Alamos with no mention about open publication. I mentioned the possibility of Los Alamos participating in the naming of the element 100 (because they observed the 7 Mev alpha particle although without identification of its atomic number). I suggested that, in connection with the planned Castle shot in the Pacific, we might set up a joint laboratory to work on the recoverable material. I told Spence about our latest work-- $^{99}_{\beta^-} \rightarrow ^{100}_{\alpha}$  (1 day, 7 Mev alpha, 4% of 6.6 Mev alpha). Spence said that he didn't think that Los Alamos should participate in open literature publication, but it was suggested that possibly the discovery is declassifiable.

I talked with Stan and Al, convinced them that we should offer codiscovery of element 99 to Argonne in the interests of good relations, then again telephoned Manning (about 3 p.m.) and said that we will consider joint codiscovery on element 99 in order to preserve our relationship with them. Manning implied that they have no new element

100 6.4 Mev alpha activity with only a little bit left in their original sample. He said they will look over their data and call Wednesday morning to comment further on element 100.

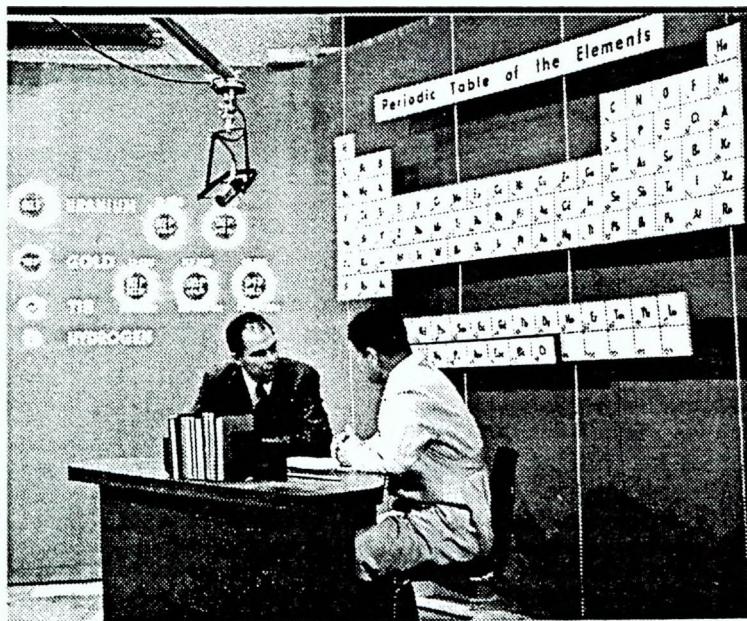
Much of the rest of the afternoon was spent with Al and Stan.

Tuesday, February 3, 1953

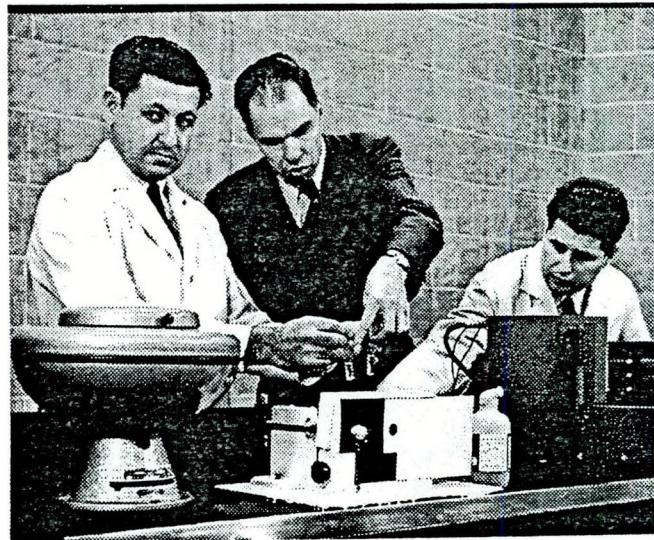
I took care of some administrative matters and talked with Al and Stan.

Reprints of my Nobel Lecture, "The Transuranium Elements: Present Status," have arrived for me. The Swedes did a very nice job reproducing the talk.

Much of the rest of the day was taken up at KRON-TV in San Francisco with preparations for Dr. Earl Herald's "Science in Action" program (produced by Ben Draper) entitled "Discovery of Plutonium" and starring Herald, Al Ghiorso, and me (Jack Hollander helped with the preparations). I first spoke about the periodic table, uranium, and the transuranium elements, and then demonstrated radioactivity by irradiating a silver dollar with neutrons followed by holding it near a Geiger counter; I talked about plutonium and its production and demonstrated nuclear fission by holding a sample of uranium that had not been exposed to a neutron source up to the Geiger counter and then holding an exposed sample up to the counter. Then, I discussed fission, which Hollander demonstrated with a sample of  $U^{235}$  and the same fission counter used in 1941 to demonstrate the fission of plutonium, attached to an oscilloscope. We concluded with a graphic demonstration of fission--a series of mousetraps set with "neutron" corks, into which a "neutron" cork was dropped, releasing two more "neutron" corks, etc., until all the mousetraps were sprung.



Glenn Seaborg and Earl Herald on "Science in Action" February 3, 1953



Al Ghiorso, Glenn Seaborg, and Earl Herald, February 3, 1953

This was broadcast over Channel 4 from 7 to 7:30 p.m. and was a good pattern for the program that J. Till NeVille asked me to provide at the time of the Los Angeles ACS meeting in March. Herald announced that next week's (February 10) program will feature Dr. Paul W. Merrill (Mount Wilson and Palomar Observatories) on "The World's Largest Telescope."

As I was entering our driveway when I arrived home from San Francisco, young Jeff Lincoln, son of our neighbors across the street, ran up to me excitedly, to exclaim that he had just seen me on TV. I asked how he liked the program, and my ego was shattered when he said he didn't know because he had changed the channel to watch another program.

Wednesday, February 4, 1953

Preston M. Harris (Ohio State University) wrote on January 30 about a promising potential graduate student, John Gordon, who is interested in working in the organic-physical field with Melvin Calvin. Today I asked Doral to make a copy of the letter and send it down to Melvin Calvin; I then wrote Harris a note and said that I have called his letter to Dr. Calvin's attention.

I made a few phone calls, including one to Greg Engelhard about some athletic matters. Iz, Jack Hollander, and I discussed the letter I received Monday from J. W. Buchta, and this morning I phoned Buchta and discussed his proposal to make cuts of the tables. I agreed to this and said that we can make correction strips, using the same typewriter, to be pasted on the original manuscript. Buchta suggested that the corrections be sent, along with indications for the location of each strip, to Miss Ruth Bryans (American Institute of Physics) within a week, and I said that we can probably do this within a week; I also agreed that our laboratory will honor a charge of \$8.50 per page.

I spent much of rest of the day proofing my 43-page manuscript, "The Actinide Elements and Nuclear Power," which Millie typed, for my lecture in Louis N. Ridenour's Extension Series, "Modern Physics for the

Engineer." This will be presented on the UCLA campus of March 16.

Then about noon Manning telephoned me to say that they want to submit their report in about 10 days for their fellows now have some evidence for element 100; they want to watch the decay for about 10 days. He implied that they have done no recent elutions, which would mean that their elution date preceded January 15 (our date of discovery of element 100). I told him that the date of their interpretation would also have to enter into the determination of who should be credited with the discovery of element 100. I suggested that he phone Spence with their data since we had, and he said he would have his men do so.

I conferred with Stan and Al and then telephoned Rod Spence about 2:45 p.m. He said that he had not heard from Argonne, but I mentioned that they may call. He said that he would receive their data on element 100 in the role of a neutral third party. I added that he need not inform us of their results. Rod said that the intensity of the 7 Mev alpha activity that they observe is 2-3% of that of the 6.6 Mev alpha activity. I again told him about our interpretation: a long-lived  $^{99}_{\alpha}$  decaying by beta-particle emission to  $^{100}_{\alpha}$ , which decays by the emission of 7 Mev alpha particles with a half-life of about 1 day. (It appears that the Argonne people are finding something entirely different, a long-lived element 100 alpha emitter decaying by the emission of 6.35-6.4 Mev alpha particles.) Rod asked about the 6.1 Mev alpha-emitting californium isotope; I told him that it could be  $Cf^{249}$  or  $Cf^{252}$  or higher if there is a closed shell there. Rod had no other ideas on this.

We received our portion (about 75 pounds) of a few hundred pounds of coral obtained from an atoll near the site of the Mike explosion, which had caught much of the debris. (The coral was divided between Los Alamos, Argonne, and the Radiation Laboratory.)

Thursday, February 5, 1953

Present at this "first of the month" group meeting were Batzel, Carr, Clark, Cobble, Cunningham, Glass, Gunn, Grover, Gray, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Kofstad, Levy, Mathur, McLaughlin, Michel, Nervik, Passell, Perlman, Raby, Ruben, Seaborg, Surls, and Templeton.

Mathur reported that he and Tom Passell ran  $Cs^{125}$  (previously reported) on the  $\beta$ -ray spectrograph and determined its positron energy to be  $2.05 \pm 0.02$  Mev. In recent bombardments of  $CaI_2$  with 150 Mev helium ions they saw 6-minute  $Cs^{123}$ ? (assignment verified by milking xenon daughter), 45-minute  $Cs^{125}$ , and 5.5-hour  $Cs^{127}$ . In another bombardment the reaction was  $I^{127}(p,xn)Xe^{121}$  (40 minutes),  $Xe^{123}$  (1.8 hours),  $Xe^{122}$  (19 hours). Mathur said that, if the iodine daughters are milked within the first few hours, the following activities are seen: 4-minute  $I^{122}$ , 1.8-hour  $I^{121}$ , 13-hour  $I^{123}$ . Only the 4-minute activity is seen if the milking takes place after 24 hours, indicating that  $Xe^{122}$  is the source of the 19-hour activity. Since  $Xe^{123}$  has the 1.8-hour half-life, the 40-minute activity must be due to  $Xe^{121}$ . Mathur reported their work indicates a value of  $\sim 1.7$  Mev for the positron of  $Xe^{123}$ , but they haven't been able to make enough to run on the  $\beta$ -spectrometer. When they ran  $Xe^{122}$ , they saw the 3.12 Mev positrons from  $I^{122}$ .  $Xe^{122}$  is probably

an electron capture isotope. At my suggestion, Mathur summarized their glow discharge method for collecting these rare earths.

Kofstad reported that he has measured some 60 cross sections in the 340 Mev proton spallation of silver with the maximum cross section of 100 mb at silver--the lowest atomic number seen so far is that of chlorine, with the cross section for Cl<sup>38</sup>,  $1.3 \times 10^{-3}$  mb and for Cl<sup>39</sup>,  $4.3 \times 10^{-4}$  mb. There was considerable discussion about these low cross sections, and Kofstad reported cross section values for Mn<sup>56</sup> and Ni<sup>65</sup>, which are also near the minimum side. He also said that he tried to look at positron-emitting cadmium isotopes. The group made several comments and suggestions.

Gray reported some solvent extraction data on the rare earths, which evoked considerable discussion.

Nervik talked about his experiments on the upper rare earth elements produced from tantalum spallation with 340 Mev protons. Work is continuing on lutetium isotopes. A sample of ytterbium was run on Michel's time-of-flight isotope separator with an activity seen at mass number 166, which consisted of a growth corresponding to a half-life of 7.7 hours (Tm<sup>166</sup>) and a decay of 59.5 hours (Yb<sup>166</sup>). On another bombardment the ytterbium fraction was milked for the thulium daughter after the short-lived ytterbium had all decayed. He also looked at the erbium fraction, which was dominated by a 29-hour half-life; no significant activity was found in a collection at mass numbers 165, 163, and 172. They plan to look at holmium isotopes.

\* \* \* \* \*

Before going up to the hill, I stopped in the Department office and talked with a number of colleagues.

On the hill I learned that all the power in Bldg. 5 had been off for ten minutes this morning but no safety problems occurred.

Working on my correspondence, I responded to the January 27 letter from J. Till NeVille and said that I shall be pleased to contribute to the March 15 TV program, "Chemists in Action," not only because of the occasion of the ACS's National Convention, but also because of a conviction that through such presentations as this we may help to arouse in the public more awareness and interest in fundamental scientific endeavor, and perhaps help to stimulate young people toward scientific careers. I then went on to give him a brief outline of the script of "The Discovery of Plutonium," which was produced with my participation by the California Academy of Sciences and broadcast on the TV show "Science in Action" last Tuesday. I suggested a similar show for their March 15 program.

My manuscript, "The Actinide Elements and Nuclear Power," was mailed to Dr. Louis N. Ridenour in Los Angeles. This will be used for my lecture for the UCLA Extension Series, "Modern Physics for the Engineer." I also telephoned China Lake, where I am scheduled to repeat this lecture, about the arrangements.

I also mailed James H. Stack (Managing Editor, ACS News Service) some material in connection with my Philadelphia ACS Section Address, which I explained will be the occasion of my receiving the City of Philadelphia John Scott Award and Medal. I enclosed a copy of my Nobel address and said that the Philadelphia talk will be similar.

In today's mail was a February 3 letter from Seymour Katcoff (Brookhaven), describing the work on fission products that Albert A. Caretto did with him during the past summer. Katcoff said that Caretto was thorough and conscientious and that he believes that both Caretto and the Radiation Laboratory would benefit by his appointment as a postdoctoral fellow.

Friday, February 6, 1953

This morning I replied affirmatively to a February 3 letter from Warren H. Crowell about the date of March 20th for the presentation of the Dickson Award at the time of the Charter Day celebration at UCLA.

I made a number of phone calls and had a number of conferences today, including one with Earl Hyde. Hyde, who made a trip to midwestern and eastern universities during early January to obtain information on hydride compounds for Project Whitney and who went to Los Angeles with Ed Orlemann last week to visit three more groups, prepared a report on the information they learned on this trip, which he gave me, along with some personal observations.

In my rounds of the labs I learned that there was a spill in Room 107 (Building 5), which was determined as coming from the curium-americium chemistry cave. Although there were some hot spots on the floor of the room and on the floor outside the door, the air count was ok.

I had a talk with Stan and Al about the coral we received on Wednesday from Rod Spence. Later in the day (5 p.m.) I reached Spence by phone. He told me that Vic Benson picked up the material and received a roentgen or two of exposure to radiation during the process. Spence also said, in response to my query, that they haven't made any alpha measurements on the material as yet--their gamma-ray measurements agree with ours. He told me that the material came from the next closest island to that of the blast and that Benson scooped up the hot spots from a wet bed about six inches down. Spence said that he feels the first foot of coral is worth working up although the specific activity of the remainder of the material is less than the material we received. I learned that he has not yet heard from Manning.

Saturday, February 7, 1953

I spent some time with the kids, then Dan Wilkes and I went out to Contra Costa Golf Club for nine holes of golf (DW-44, GTS-52).

Helen and I had dinner at the Latimers' home, along with the Joe Hamiltons and the Luis Alvarezes.

Sunday, February 8, 1953

In my free time I read and worked on a couple of writing projects.

Monday, February 9, 1953

This was a busy day. As a result of various conferences, phone calls, and Earl Hyde's recent trips, we have decided to invite Dr. Thomas R. P. Gibb, Jr. of Tufts College in Medford, Massachusetts to visit us to discuss the hydride problem connected with Project Whitney and to possibly offer him a position with that program. I telephoned Gibb this morning and learned that he is willing to come here during the week of February 23, so I promptly wrote a memo to this effect to George Everson, stating that Dr. Gibb is one of the foremost authorities in the country on this subject. I also provided Everson with Gibb's Q clearance number.

Among my calls was one to Greg Engelhard about some athletic matters and one to Mabel Kittredge to check on some graduate students' admittance matters.

In today's mail was a confirmatory letter, dated February 5, from J. W. Buchta about the arrangements for the publication of the "Table of Isotopes" in Reviews of Modern Physics. I routed the letter to Iz and Hollander.

I also received a February 4 letter from Frank R. Ward, stating that it has been decided that the editorial I submitted for publication in the classified journal Reactor Science & Technology is unacceptable because there is long-range detection and weapons information in it. Ward said that they will publish it if I agree to certain deletions; however, they will have to hold it for the June issue since the deadline for the March issue has passed. Ward also said that it has been suggested that the entire editorial might be submitted as an AEC information paper; he also suggested that I talk with him or his associate editor (David W. Lillie), and Spofford G. English (Division of Research) when I next visit Washington.

A letter also arrived from C. E. Larson, who enclosed an abstract of a talk entitled "Nuclear Reactions with 25 Mev Nitrogen Nuclei" by Alexander Zucker, which was presented at the Cambridge meeting of the American Physical Society. (Zucker is doing some pioneer work in heavy ion reactions.) Larson told me about this work when he visited Berkeley last month. In this letter he said that they are presently making modifications on their cyclotron and the investigations will resume in a few more weeks. If they get more interesting results, he wrote, Bob Livingston or he will drop me a line.

The senior staff had its regular luncheon meeting in my office at noon and discussed such items as the new elements, Project Whitney work, and naturally the budget.

About 2:30 p.m. Rod Spence telephoned me and said that he is working on the problem of getting more material (from the Mike thermonuclear test), possibly as much as 200 tons. I told him that we want to work up two tons ourselves and said that we will help pay the costs, in addition

to sending a man along to help. Spence said that he has asked Manning about their interest in such material. Manning still has made no comment on the element 100 question.

When I made my rounds of the labs, I noted that Health Chemistry has removed a couple of boxes from Room 105 (Cunningham's room), preparatory to rebuilding the room. I was also told that the floor in Room 107 (cave room) is contaminated.

At 3 p.m. I went to hear a lecture by Chauncey Starr, who is now the manager of the Nuclear Engineering and Manufacturing Division of North American Aviation, Inc., Downey, California.

Later I went to a local section meeting of the ACS and heard a talk by Dr. Harold G. Cassidy (Yale University). Afterwards I had a chat with Israel Cornet (Division of Mechanical Engineering) about the status of Chemical Engineering Departments at other universities. [There is an ongoing dispute here at Berkeley whether the Chemical Engineering Department should remain in the College of Chemistry or be connected with the College of Engineering.] Cornet said that he would look into the matter and get back to me.

#### Tuesday, February 10, 1953

This morning I telephoned Doyle Northrup to check on the time of the Boner Panel meeting; then I called the office of Gordon E. Dean (Chairman, Atomic Energy Commission) and made an appointment to see him (10:30 a.m. on Monday, February 16). I also wrote a note to Spof English that I will come by to see him immediately after I see Dean--sometime between 11:00 a.m. and 11:30 a.m.

A wire arrived from T. R. P. Gibb, pointing out that February 23 (the date we agreed upon for his visit) is a holiday. He said that their class schedule makes February 26 and 27 or April 6-10 better dates and asked if the urgent matters could be handled by correspondence. I spoke with our men and then wired Gibb, "February 26 and 27 entirely satisfactory. Will send brief description of our problems."

John R. Bradford (Radioisotopes Laboratory, Case Institute of Technology, Cleveland) wrote on January 29 and requested information on the transuranium elements for a chart of isotopes they are preparing. Margie sent him a copy of our revised "Table of Isotopes," and I signed a covering letter saying that this will probably provide the data he needs.

Much of the rest of the day was taken up with conversations with our researchers.

#### Wednesday, February 11, 1953

There were the usual number of administrative matters to handle today, in addition to conversations with some of the students.

I wrote to Albert A. Caretto, Jr. (University of Rochester) that it now appears that our budget cannot stand the addition of another postdoctoral fellow next year. I mentioned, however, that we have a

closely associated program involving very interesting nuclear chemical research [Project Whitney], which is in need of men with his qualifications and suggested that, if he is interested, he should get in touch with Dr. Roger E. Batzel [who has joined this project in an administrative capacity] here at the Radiation Laboratory.

A letter also went to Don Stewart at Argonne to tell him that I plan to stop off at Argonne on Friday, February 20th on my way back from Philadelphia. I wrote that I am scheduled to arrive in Chicago at 10:05 a.m. on United Flight 633 and leave at 8 p.m. on United Flight 611 and asked him to arrange transportation to and from the airport.

At home Helen told me that, after her dental appointment, she went to Lynne's class and helped with the Valentine party.

Thursday, February 12, 1953

The meeting of the research group this morning was attended by Asaro, Carniglia, Carr, Clark, Cobble, Dauben, Dunlavey, Gray, Grover, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Ruben, Seaborg, Shudde, Smith, Stephens, and Surlis.

Jaffe gave a rather long report on the x-ray studies of a sample of the 16-hour isomer of  $\text{Am}^{242}$ , which was obtained by irradiation of  $\text{Am}^{241}$  in the Arco pile. The americium was separated by Hoff and Hulet; the beta, x-ray, and gamma spectra was run by Passell, Jaffe, and Hoff. There were about 1.5 mg total  $\text{Am}^{241}$ , and the sample gave 32 lines between 13.8 kev and the 60 kev gamma ray of  $\text{Am}^{241}$ . Jaffe said that, although these were mostly x-ray lines of neptunium, plutonium, americium, and curium, they saw three gamma rays:  $42.3 \pm 0.2$  kev,  $43.6 \pm 0.2$  kev, and  $44.8 \pm 0.2$  kev. These were of extremely low intensity and may be false. Jaffe went on to discuss their interpretations of some of the x-rays in terms of branching ratios. For example, they obtained  $L_{II}(\text{Cm}/\text{Pu}) = 3.4$  and  $L_{III}(\text{Cm}/\text{Pu}) = 3.8$ , in rather good agreement with the bombardment work of Higgins and Thompson. Jaffe also described the intensity corrections they made. There was discussion about type of radiation, conversion coefficients, etc., and Jaffe concluded that the energy difference between the two americium isomers must be regarded as unknown although they may yet find some indications of decay to different curium levels in the beta spectrum of the ground state.

Hoff presented the conversion electron spectrum that Passell obtained on the beta-ray spectrometer with the same sample, which he compared with O'Kelley's original work. Hoff said that two of the gamma rays that Jaffe observed (42.3 and 44.8 kev) differ by 2.5 kev, compared with their 2.3 kev difference. He noted that their  $L_{II}-L_{III}$  intensity ratios are not sufficiently accurate to have any real significance although they compare rather well with those of Jaffe. He presented the decay scheme  $\text{Am}^{242m} (\beta^-) \rightarrow \text{Cm}^{242}$  (41 kev state) and  $\text{Am}^{242m} (\text{EC}) \rightarrow \text{Pu}^{242}$  (43 kev state) and said that it seems clear that O'Kelley's values need revision in light of these new experimental data.

Passell reported that, using a sample of about the same strength as O'Kelley's, they obtained by a Fermi plot of the beta spectrum of  $\text{Am}^{242m}$  an  $E_{\text{max}}$  of  $618 \pm 5$  kev, compared with O'Kelley's value of 628

$\pm 5$  kev. With this new larger sample, the  $E_{max}$  was  $606 \pm 5$  kev; they have no very satisfactory explanation of the change.

\* \* \* \*

After talking with some of my colleagues in the Chemistry Department office, I went up to the hill, made some phone calls, and answered a few letters.

One of the problems that I plan to check into in Washington is our concern over a problem that has arisen about Al Ghiorso's security clearance. He has received word from AEC headquarters that he must undergo a security check in view of some new derogatory information they have. Actually, Al's security clearance has been somewhat of a problem since Met Lab days because of his liberal stance. This present situation is very serious since loss of his security clearance would mean he would have to discontinue his present research, including his interesting work on the heavy isotopes we are finding in the Mike debris. His job at the Radiation Laboratory would also be in jeopardy. The problem is exacerbated by Al's intransigent attitude. He maintains, with some justification, that the things they are questioning are none of their business, but I am trying to convince him that some cooperation is in his best interest. Again this afternoon I phoned and talked with Wally (W. B.) Reynolds about the matter.

Alan F. Clifford (whom I have known since Met Lab days) inquired on February 4 about academic positions since his stay as a Guggenheim Fellow at Cambridge is half gone and he resigned his position at Illinois Tech before he went abroad. In my response I wrote that I have no suggestions at the present but suggested that he have Professor Emeléus, with whom he has been working, write me a reference letter.

Geoff Wilkinson wrote on January 28 about a fellowship they have open at Harvard. I replied to this that I have passed his letter around among the fellows; however, I explained that this appears to be a bad year from the standpoint of our supplying people to other places because of a new project started here last fall, for which we are hiring a number of the best people.

Arthur Rose (The Pennsylvania State College) wrote on February 3, asking that I chair the meetings of the ACS Council Publications Committee at Los Angeles since it is almost certain that he will be unable to attend. Rose said that he has asked the Washington office to send me the information on the time and place as soon as it is available. He also requested that I prepare a one-page statement of my opinion on where we are and which way we are going with regard to making the Journal of the American Chemical Society more acceptable to theoretical and nuclear chemists. In my reply to Dr. Rose I said that I shall try to act as chairman for the two sessions provided there is no irreconcilable conflict with my committee schedules, which I do not as yet have. I asked that he send me the name of a substitute should a conflict take place and also the name of someone to act as a secretary. On the other matter I wrote:

Although I have been interested in segregating the semi-monthly

publication in the Journal so that physical, inorganic, nuclear, and theoretical chemistry alternates with organic and biochemistry, the opposition to this has been considerable, and I am not sure that I care enough about it to bother any more. I have primarily interested in this from the standpoint of nuclear chemistry. However, the advantages, as I now see them, are more to the American Chemical Society than to the nuclear chemists because the latter already have a very satisfactory publication medium in The Physical Review, and it is perhaps getting a little late to try to build up as a substitute a reading audience (which should include physicists) for the Journal.

I also replied to a February 4 letter from Ray Sheline, who said that he has heard that someone here has found the isotope  $Mg^{28}$ . Since he has been working on the isotope all fall, has sent a note in to The Physical Review, and is ready to publish the completed work, he suggested that perhaps our work could be published simultaneously. I wrote that Manfred Lindner, who has also run into the isotope, will write to Sheline about his plans. I apologized for failing to include a reference to his work on this in the "Table of Isotopes" and explained that we will have no opportunity for changes in proof because the "Table" is being reproduced photographically, but we are going to try to send in some substitute sheets with his information on them in the hope that they can be included before press time.

A letter arrived today from Nathan Sugarman, who said that he has been asked to serve as Chairman of the March 19 morning session of the Division of Physical and Inorganic Chemistry of the ACS meeting in Los Angeles. Sugarman asked me to designate two members of our laboratory to act as discussion leaders for two papers, "Studies of the Solvent Extraction Behavior of the Transition Elements. IV. Isolation of Gram Quantities of  $Th^{230}$  (Ionium) from a Pitchblende Residue" and "Some Chemical and Magnetic Properties of Technetium and Rhenium." I agreed to do this.

At about 1:50 p.m. I telephoned Rod Spence about the status of the new material and learned that he just received a call from Manning, who said that they want about a ton of the new material (Spence still has received no word from them about element 100). The present plans are for Los Alamos to pick up four tons of material (two for here and two for Chicago) and we will give them some of the final product after we have isolated it. The delivery time in Oakland of the four tons will be in three to four weeks, if by boat.

Then, somewhat after 2 p.m., I went to Ernest Lawrence's office to meet and speak with S. Sambursky.

Friday, February 13, 1953

Since I will report on our new elements at the Bonner Panel meeting next week and will stop at Argonne on my way home, I asked Al Ghiorso to write down for me our information on elements 99 and 100. He stopped in my office this morning and handed me a sheet labelled "Facts and Artifacts," with pertinent data on each isotope and the dates of the experiments.

I wrote to Dr. Harold Cassidy, who is presently on a visit in Bolinas, to ask if I could borrow a copy of the speech he gave Monday night (ACS section meeting) for it is similar to one I am scheduled to give as the Charter Day address on the Riverside campus of the University of California in a few weeks. I told Cassidy that I am especially interested in some of the quotes he used.

I sent a classified letter to Dr. Thomas R. P. Gibb, Jr., describing in some detail the problem about which we wish to consult with him. The letter begins:

One of our physics groups is engaged in the conception and, in some cases, testing of novel fission reaction systems. Some of these involve the use of hydrogen as a neutron moderator to increase the efficiency of the system. The denser the hydrogen is packed the more efficient is the moderation. Our central problem is to discover those compounds or chemical systems which provide hydrogen in the most concentrated form. There are some restrictions on neutron capture cross sections and other considerations which limit the elements which can be considered. In some applications there is a high premium on the use of low Z materials even at some decrease in hydrogen concentration....

The letter went on to give a few more details of the work and to say that it would be much more satisfactory to talk about the problem in person, and that we hope he will decide to visit us on February 26 and 27 to discuss these matters with me and particularly with Drs. Hyde and Orlemann.

In today's mail was a February 9 letter from Edwin O. Wiig (University of Rochester), describing the work Albert A. Caretto, Jr. has done with him (Ph.D. research) and recommending him for a postdoctoral fellowship or a similar position.

I also received an analysis of the Departments of Chemistry and Chemical Engineering at the University of Washington and the California Institute of Technology from Israel Cornet, information relevant to my request to him last Monday evening. This is in connection with concerns about the place of our own Chemical Engineering Department in the hierarchy here at Berkeley. With respect to this broad question Israel, a fellow student at UCLA, had this to say:

Glenn, I hope this information will be of assistance to you. I have tried to keep out of the Chemical Engineering dispute, first, by doing only that work which seemed worth doing and which was not being done by other men in your College or in mine, and, second, by not looking too deeply into the problems involved. I do believe that the welfare of the University of California should come first, and I hope we will have a group in Chemical Engineering with the same repute and distinction which several other groups here now enjoy.

You may recall that I have had the same type of background and education which many of the men in your College have. I have lost none of my respect and esteem for my colleagues in Chemistry and Physics. However, I have gained some appreciation of the problems of

a four year professional school--and I am a registered professional engineer, with experience in industry. I do not doubt that a good man with a classical education in chemistry, or physics, or mathematics, can become a good chemical engineer. For most chemical engineers, these are not the proper paths.

In a four-year professional school, one must place considerable emphasis on teaching ability, on professional competence, and on the proper balance between the art and the science. Perhaps this is why, where there is a distinguished Chemical Engineering faculty, Chemical Engineering is normally independent of Chemistry, in most universities in the United States.

I made my usual rounds of the labs to talk with the fellows about their work.

When I arrived home, Helen told me that her mother suffered a stroke today. [She has been living in a rest home on Fulton St. in Berkeley and has been failing recently.] Helen said that she was taken to Herrick Hospital and that her condition is very critical.

Helen and I went to Cal's basketball game with Mrs. Greg (Virginia) Engelhard (UCLA beat the Bears by a score of 67 to 63), and afterwards we got together with the Engelhards and the Miles Hudsons.

#### Saturday, February 14, 1953

Some of my morning was spent with the kids. I gave Helen the usual box of chocolates for Valentine's Day.

Later in the morning I went into Berkeley in order to attend a luncheon meeting of the District Chairmen of the Berkeley Alumni House Committee, which was held at the Men's Faculty Club. Among the people with whom I spoke were Stan McCaffrey (Executive Secretary of the University of California Berkeley Alumni Association) and Milton H. Esberg, Jr. (Chairman, Alumni House Finance Committee). I made a few general remarks, pledging my support for the project of building an Alumni House on the Berkeley campus.

After dinner Iz and Lee Perlman went with me to the Cal-UCLA basketball game. Tonight Cal was victorious, 70-62. The Bears have some good players, such as Tom Greenleaf, Bob McKeen, Bob Matheny (only 5'11" but feisty), and Bob Albo.

#### Sunday, February 15, 1953

A lab driver picked me up at 7:00 a.m. and drove me to the San Francisco Airport for United Flight 602 (8:45 a.m.) to Chicago. I arrived in Chicago at 5:30 p.m. and transferred to United Flight 640, which arrived in Washington at 10:25 p.m. I checked in at the Hotel Statler.

#### Monday, February 16, 1953

In Washington. I went to the AEC Building (1901 Constitution Ave.,

N.W.), where I talked with a number of people, including AEC Commissioner Henry D. Smyth, AEC Chairman Gordon E. Dean (I had a 10:30 a.m. appointment with Dean), and Deputy General Manager Walter J. Williams. One of the things I discussed was the problem that has arisen about Al Ghiorso's security clearance. Smyth asked me what I thought about the idea of Wendell Latimer becoming a Commissioner, and I supported the idea. I also met with Tom (Thomas H.) Johnson (Director of Research) and Spof English, mentioned Al Ghiorso's security problem, and spoke about our need for funding for the AFOAT program, i.e., our work on the debris and coral from the Pacific to continue our study of heavy isotopes. It was suggested that we draw up a budget in terms of men and have our Business Manager, W. B. Reynolds, submit it to them.

I then got together with English and men in his office--Dan Miller, George Kavanagh--and Rod Spence to talk about the financing for getting new samples. I learned that we will be getting samples from the Edna and Flora sites--they described the location of these sites in relation to Elugelab (the island that was erased by the Mike blast) and mentioned that Gene is about as close (1/4 mile) to the center of the blast as Edna, but the material there is not as hot. Tracerlab (Berkeley) may receive funding of \$50,000 to \$100,000 to help with this program.

On another matter Rod Spence mentioned that he thinks the beta spectrometer of Fred H. Schmidt (University of Washington) has the best combination of geometry and resolution of any he has seen.

Spof English and I talked about the editorial I wrote for Reactor Science & Technology, which was turned down because of classified information. Spof said he would look into the matter.

Also today, I contacted and saw Donald Lane, who discussed with me his preparation of the Applicants' Reply to Memorandum of General Counsel. Lane said that he plans to prepare the reply clearly to show the Board how we feel about the proceeding and also to show the difficulties created by Captain Lavender's methods. I agreed with this approach.

Tuesday, February 17, 1953

In Washington. Today I attended a meeting of the IO-7 (Bonner) Panel. This "Long Range Detection Panel" is composed of Dr. Charles P. Bonner (Chairman) (Dean of Arts and Sciences, University of Texas); Dr. Athelstan Spilhaus (Dean of the Institute of Technology, University of Minnesota); Dr. Joseph C. Boyce (Associate Director, Argonne National Laboratory); Dr. James B. Macelwane (Dean of the Institute of Technology, St. Louis, Missouri); and me. Among the items discussed was a man for head of the Air Force Cambridge Laboratory--the salary is between \$10,000 and \$12,000. One suggestion was Craig M. Crenshaw, a physicist with the Signal Corps. We also discussed the data from JOE, the first Soviet atomic bomb test, which took place on August 29, 1949.

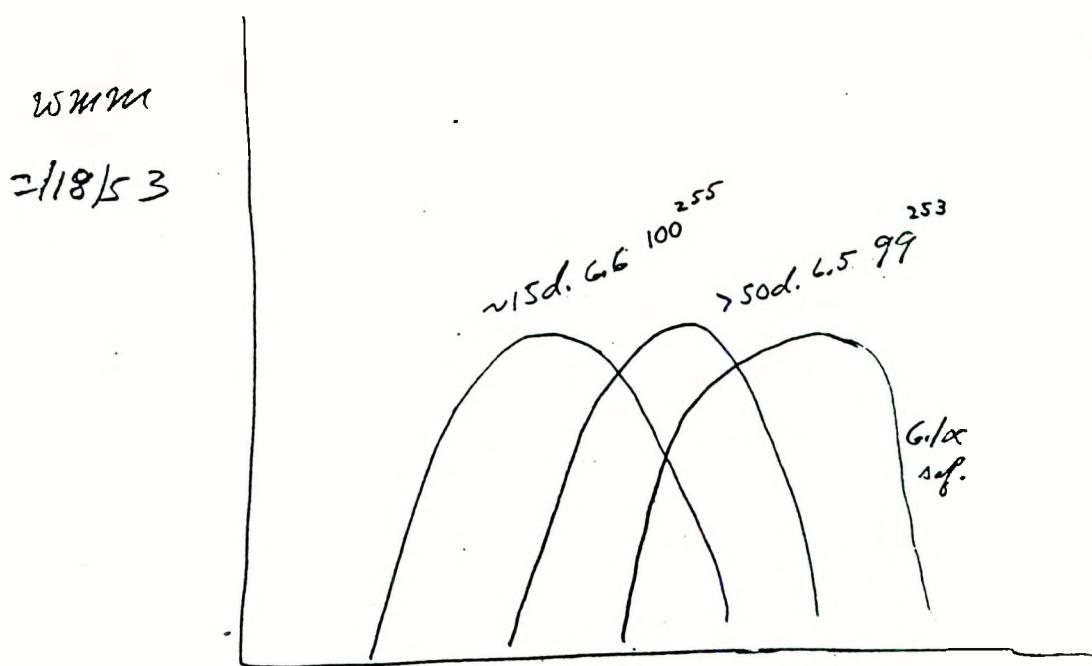
[In Berkeley Iz gave the first Chemistry 223 lecture for this semester. Members of this small class are Peter R. Gray, LeRoy G. Green, James R. Grover, Richard M. Lessler, Ralph McLaughlin, Hirdaya Mathur, David A. Pickler, Bruce A. Raby, Rex Shudde, Warren G. Smith, Frank

Stephens, Jr., Joseph Surlis, Jr., and Vernon Troutner. Our du Pont men, Walter Laird and Mark Snyder, are auditing the class.]

Wednesday, February 18, 1953

In Washington. At the meeting of the Boner Panel, which was also attended by a group from Argonne (Winston M. Manning, Sherman Fried, and G. Pyle), I told about the Berkeley results on element 99 and element 100 (this was the first time the Argonne group heard our results on element 100). I reported that the UCRL group had chemically identified a 22-day, 6.61 Mev alpha activity due to element 99 and presumably with a mass number of 253 and that this activity is sustained by a  $Cf^{253}$  beta emitting parent with a half-life of approximately 100 days. I also reported the chemical identification of an approximately 1 day, 7.0-7.1 Mev alpha activity due to element 100, probably corresponding to mass number 255 and which is sustained by a long-lived, beta emitting element 99 parent. I also reported a 6.1 Mev alpha emitter in the californium fraction and that the high spontaneous fission activity of the californium fraction decayed with a half-life of about two months.

Manning reported that the Argonne group had observed and chemically identified a greater than 50 day, 6.5 Mev isotope of element 99 presumably with mass number 253. He also reported, which I heard for the first time, that the Argonne group had chemically identified an approximately 15 day, 6.6 Mev alpha emitter due to element 100, presumably with mass number 254. He reported that the californium fraction showed spontaneous fission and a 6.1 Mev alpha emitter which were chemically inseparable. Manning showed a rough elution curve with three severely overlapping peaks of about equal intensity, which he attributed in order (left to right) to the three alpha emitters, the approximately 15 day, 6.6 Mev  $100^{255}$ ; the >50 day, 6.5 Mev  $99^{253}$ ; and the 6.1 Mev californium isotope. I made a sketch to study and to show the fellows at Berkeley:



Later in the afternoon I pointed out to Manning, Fried, and Pyle that I felt certain that their approximately 15 day, 6.6 Mev  $^{100}_{\alpha}$  alpha emitter was actually the 20 day, 6.6 Mev  $^{99}_{\alpha}$  alpha emitter; thus they had changed their assignment of the 6.6 Mev alpha emitter from 99 to 100 in just the reverse order of our change from 100 to 99 for this activity. I told them that I felt sure that their >50 day, 6.5 Mev  $^{99}_{\alpha}$  was no single activity at all but was, in reality, caused by their not recognizing the  $Cf^{253} \beta^- \rightarrow 99^{253}$  decay sequence so that incompletely separated elution fractions near the californium peak would in actuality show greater than 50 day decay for the 6.6 Mev alpha particle (of  $^{99}_{\alpha}$ ). [This interpretation, of course, assumed that the "two" Argonne alpha particles, 6.6 and 6.5 Mev, were actually of identical energy.] I tried to convince Manning, Fried, and Pyle of this interpretation but without success.

One thing that Manning, Spence, and I agreed upon was that the three laboratories will submit reports to Los Alamos for editing and coordination by about the middle of March for issuance as a part of the Ivy Report (the report on the Mike thermonuclear weapons test shot).

Thursday, February 19, 1953

In Washington. I went to the AEC to meet with John A. Waters (Director of Security) to discuss Albert Ghiorso's security case and to attempt to convince him to renew Al's security clearance. I went over many points with Waters, which are to be covered in a letter that I am to write to Walter J. Williams (Deputy General Manager). I described how long I have known Al, how closely associated I have been with him both officially and socially, my knowledge of his outside (of the laboratory) activities and his closest associates, their identity and interests, my visits to his home, Wilma's associates and their interests and political ideology (it was alleged that prior to their marriage Wilma had some friends who were associated with the U. S. Communist Party), why Al and Wilma subscribed to The People's World, their attitude toward Security Regulations, my opinion that they do not present a security risk and my reasons, and Al's value to our research program and the consequences of his loss to our program. Waters asked me to put this all in an affirmative light in my letter to Williams, adding that this reinvestigation was initiated by their subscription to The People's World.

After my meeting with Waters, I telephoned Spof English about my editorial for Reactor Science & Technology. The conclusion was reached that I should send the present version to the Commission, but I should contact Frank Ward about revising this version to make it acceptable for publication. I also talked with Charles L. Marshall (Declassification Officer) for a bit about the declassification of information about transcalifornium elements. In addition, I phoned Robert F. Gould (ACS News Service) and agreed to send him a copy of the "Table of Isotopes."

I left Washington at 1 p.m. on the Pennsylvania Railroad (parlor car) bound for Philadelphia, where I was scheduled to arrive at 3:24 p.m. A reservation had been made for me at the Hotel Warwick (17th and Locust Sts.), where I checked in and rested until somewhat before 6 p.m. when I went to Franklin Hall at the Franklin Institute for an informal dinner. The John Scott Award was made at a meeting of the Philadelphia Section of

the ACS (Jos. W. E. Harrisson, Chairman) at 8 p.m. Ernest T. Trigg (Vice-President of the Board of City Trusts) presented me with a copper medal, a scroll, and a check for \$1,000. [This John Scott Award was set up in 1816 by a Scotsman who never came to Philadelphia nor the United States. He left the city \$4,000, the income from which was to be used to distribute prizes to persons who had made ingenious inventions or discoveries. Among others who have received the award are Madame Curie, Orville Wright, Guglielmo Marconi, Thomas Edison, and Selman A. Waksman.] I spoke extemporaneously on the "Present Status of the Transuranium Elements" (not mentioning, of course, elements 99 and 100).

I met a number of people at the meeting, including Robert K. (Buck) Sawyer (1st City Manager of Philadelphia), whose father was the City Engineer of Ishpeming (before Bill Gray, Peter's father).



Ernest T. Trigg presenting Scott Medal to Glenn T. Seaborg,

[In Berkeley the research group met as usual with the following people attending: Asaro, Carniglia, Carr, Clark, Cobble, Cunningham, Glass, Gray, Grover, Hoff, Hollander, Hyde, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Ruben, Shudde, Smith, Stephens, and Templeton.

Michel again reported on work with Walter Nervik on the mass 2/19/53-4 assignments of heavy rare earth activities with the mass spectrograph, saying that the most recent work was performed with photographic transfer plates from the conventional Nier spectrograph rather than the time of flight spectrograph in order to make a quick survey of the whole region. They looked at erbium, thulium, and ytterbium--no lines were seen for lutetium although it was also run. Michel went into some detail about the work and the lines they saw.

Asaro said that he looked at the gamma rays from a sample of  $Cm^{242}$ , obtained from the latest  $Am^{241}$  neutron bombardment in the MTR. He observed gamma rays of 40 kev, 102 kev, and 158 kev with the intensity ratio of the 158 kev gamma ray to the 40 kev gamma ray being 3.6. In the discussion Perlman said there is one 44 kev gamma ray per 1,000 alpha particles and one 102 kev gamma ray per 10,000 alpha particles and this takes into account the conversion coefficient and the alpha group abundance. Asaro went on to discuss spins, crossover transitions, etc. for even-even nuclei, making comparisons with the rotational model of the nucleus developed independently by John Rasmussen and Aage Bohr. Perlman made some generalizations about hindrance factors in the alpha decay of various alpha groups in even-even nuclei. Asaro also talked for a while about ionization by electron coincidence in the alpha pulse analyzer and compared the pulse analyzer results with the alpha spectrograph results for the isotope  $Cm^{243}$ .

\* \* \* \* \*

In January I was asked by Dean Alva R. Davis to chair a committee, composed of Robert B. Brode, Charles B. Morrey, Otto Struve, and John Verhoogen, to consider the worthiness of Edward Teller to be appointed a Professor of Physics for the academic year 1953-54 (1/3--Department of Physics, 2/3--Radiation Laboratory). I made the necessary contacts and set up a meeting for Tuesday, February 24 at 4 p.m. in my campus office (Room 23, Lewis Hall). Today Doral received a call from Dean Davis' office, saying that because it was urgent (other outside offers, timing of Regents' meeting, date that catalogs go to press), another committee was being formed. Doral arranged to have Teller's file returned to Davis' office and then cancelled the February 24 meeting with the other members of the committee.]

Friday, February 20, 1953

At 7 a.m. I left Philadelphia on United Flight 633 and arrived in Chicago at 10:05 a.m. An Argonne driver met me and drove me to Argonne. There arrangements were made for another meeting in the conference room in "F" corridor primarily to discuss my interpretation of their results. Essentially the same people were present, except for Studier, as at the January 7 meeting. I pointed out that it was my opinion that the hypothesis of the existence of two 6.6 Mev alpha emitters with anything

like equal yields and neighboring atomic numbers is in violent disagreement with any sensible alpha systematics (plots of alpha energy vs A for constant Z) in view of the small difference in mass number assignments that would have to be made in order to be consistent with the Mike isotope yield curves (curves showing regular diminution of heavy isotope yields with increasing mass number). A great deal of time was spent discussing the "pulse analysis-elution" diagram that Manning reported to the Boner Panel on Wednesday (February 18). There was much disagreement as we discussed a number of its important features. The "pulse analysis-elution" curves corresponding to the approximately 15 day, 6.6 Mev  $100^{255}$  alpha particle and the greater than 50 day, 6.5 Mev  $99^{253}$  alpha particle differed by only about one drop at these two peaks, and I raised the question whether this was enough to indicate a whole atomic number difference between them with reference to the rare earth elution position analogy. There was a great deal of discussion, which involved especially Mech, in which I maintained that the pulse analyses data didn't justify the resolution of the two alpha particles into the two energies, 6.6 and 6.5 Mev. I was not successful in convincing anyone of this. The heights of the ~15 day  $100^{255}$  alpha particle and the >50 day 6.5 Mev alpha particle pulse analysis-elution peaks were about equal; these facts suggested to me that the Argonne group had indeed changed the assignment of the ~20 day, 6.6 Mev  $99^{253}$  alpha particle to ~15 day, 6.6 Mev  $100^{255}$  and that the >50 day, 6.5 Mev  $99^{253}$  alpha particle was caused by an incomplete chemical separation and a lack of recognition of the  $Cf^{253} \beta^- \rightarrow 99^{253}$  decay sequence. However, I was unsuccessful in convincing the group that the 6.6 Mev  $99^{253}$  alpha particle was sustained by a long-lived beta emitting  $Cf^{253}$  parent. I suggested that this  $Cf^{253}$  might exist in two isomeric beta emitting states.

Later I got together and talked with Steve Lawroski about possible engineers for Project Whitney. He suggested several names: Charles E. Stevenson, Walter Rodgers, John H. Schraadt, Leslie Burris, Jr., Milton Levenson, Harold M. Feder (will get his Ph.D. with Taube this spring). Of the others, only Stevenson has a Ph.D.

I left Chicago about 8 p.m. on United Flight 611, bound for Oakland. I'm afraid that nothing really was accomplished by my meeting with the Argonne group.

Saturday, February 21, 1953

I arrived in Oakland at nearly 4 a.m.; I was met and driven home by a lab driver. After a couple of hours of sleep, I got up, greeted the children, and looked over some of the mail that Iz had brought home. This included such things as thank-you letters from people such as Stan McCaffrey for my participation in last Saturday's Alumni House luncheon meeting, etc. I telephoned Al and Stan to relate some of the information I picked up on this trip about the Argonne work on elements 99 and 100.

Later I went to campus to attend the UC Alumni Baseball Group luncheon in Stiles Hall, presided over by the irrepressible Clint Evans; this was followed by a game between the Alumni and the Varsity. The Varsity won by a score of 10 to 3.

Sunday, February 22, 1953

I spent some time with the kids. Judy Perlman (the Perlmans elder daughter) came over and said that the Hollanders had taken her and her sister Alice to the aquarium in San Francisco, where Dr. Earl Herald (whom Jack had met when he helped me with the "Science in Action" show) had given them a "behind the tanks" tour. Dr. Herald, Judy said, had given her some good advice about fish for her home aquarium.

Later I went out to Contra Costa Golf Club with Art Rinne, Dan Wilkes, and Luis Alvarez for 18 holes of golf (AR-83, DW-80, LA-91, GTS-97). I certainly am out of practice.

Monday, February 23, 1953

Although this is a holiday, I did go into the lab eventually to talk with Stan and Al and to look over some of the material that had accumulated on my desk. Harold G. Cassidy sent me a copy of the talk he gave before the ACS Section Meeting (February 9), which I requested. He asked that I return it to him at Yale and also said he would appreciate a copy of my talk.

I read Joe Kennedy's response to my inquiry about Norman Bonner. Joe wrote, "His leaving here was the result of budget cuts in our department and did not indicate any dissatisfaction on either side. I have a high regard for his technical skill. Especially in the laboratory he is thorough, methodical, and effective. In my opinion he is not very imaginative and for this reason should not be expected to originate or direct a research program." Joe added a P.S. that he will be in Washington on February 20 and intends to call Lane. I think we shall probably offer Bonner a position with Street's Whitney program.

Later I attended an Athletic Board dinner meeting at the Durant Hotel.

Tuesday, February 24, 1953

After the usual phone calls and conferences, I tackled my stack of mail.

Yesterday I read a response from Thomas R. P. Gibb, Jr. about our invitation to visit and consult with us. Gibb said he will arrive in San Francisco on United Flight 627 at 7:10 p.m. on February 26. I discussed this with Earl and Ed Orlemann and then wired Gibb: "DR. ORLEMAN WILL MEET YOU ON ARRIVAL AT AIRPORT WEDNESDAY UNLESS YOU INDICATE OTHER ARRANGEMENTS." [Later in the day Gibb responded with "WILL MEET DR. ORLEMAN AS SUGGESTED. THANKS.]"

I looked over a memorandum prepared by Hyde and Orlemann on "Hydrogenous Materials" for the Whitney program. This is based on literature and discussions with various individuals concerned with such materials, and I put it aside to study later.

Richard H. Andrews (Secretary, The American-Scandanavian Foundation) wrote on February 12 that they would like to feature me in their column entitled "Member-of-the-Month" in SCAN. He asked for a 400-word article

and a glossy photograph. I put together a brief article about my background and my trips to Sweden, which I mailed to Andrews today, along with the requested photograph, adding that I am pleased and honored to have been selected.

My friend John Voelker (Ishpeming) wrote on February 17 to ask permission to print the article that he wrote about me last winter in the Daily Mining Journal. I wrote today and said that I have no objection. I also told Voelker that one of my graduate students now is Peter R. Gray, born in [REDACTED] and son of Bill Gray who was a former [REDACTED] city engineer. I added that Bill Gray's sister, Dorice Gray, was in the same grammar school class as I was and I remember regarding her as the prettiest girl in the class. I also sent Voelker a clipping about the Scott Award at which I met the present city manager of Philadelphia, Robert K. "Buck" Sawyer, whom I learned was in the grammar school class just one year ahead of me. Finally, I said that we all enjoyed our visit to Ishpeming last summer and that the kids still speak of the fish they caught in Dead River.

I looked over this morning's mail and then went down to campus, where I gave the Chemistry 223 lecture on components of nuclear forces and the meson field theory. I talked with some of my colleagues in the Department of Chemistry office later and then returned to the hill.

As usual I made the rounds of the labs, including a trip through the labs in Bldg. 50, where I was introduced to a new member of Eugene Huffman's group, Eugene Vestal Clark, Jr.

Eugene Clark



Wednesday, February 25, 1953

Yesterday Dean Pitzer showed me some correspondence with Bruce R. McGarvey (University of Illinois), who will be an instructor in the Chemistry Department next fall and who may be interested in doing research in affiliation with the Radiation Laboratory group. I wrote McGarvey today and described our interest in nuclear magnetic resonance, particularly in its application to the rare earth and actinide elements from the point of view of obtaining fundamental nuclear information such as on spins, information on covalent binding, and possibly even on atomic ground states. I told McGarvey about our setup and the clearance procedures.

Also yesterday a note arrived from Bill (W. H.) Johnston (Purdue University), commenting that they are looking forward to my visit next fall and mentioning that he hopes to see me at the Los Angeles ACS

meeting next month. Johnston asked if it would be convenient to lose his membership in the "Never-been-to-Berkeley Club" after the ACS meetings. I replied today that we shall be glad to have him visit. I noted that I have to remain in Los Angeles the week following the ACS meetings but that Iz Perlman, Dave Templeton, Earl Hyde, Stan Thompson, Al Ghiorso, et al. will all be around to see him.

A similar note, dated February 19, also arrived from Jack (John W., Jr.) Irvine (Massachusetts Institute of Technology), who included his Q-clearance number. I wrote the same response to him.

The local papers carried a press release about my receiving the Scott Medal and, since the secretary to G. Curtis Pritchard (Secretary, Board of Directors of City Trusts) asked that I send her any publicity about the award, I had Doral clip the items and mail them to Pritchard today.

Today I received a nice handwritten note from Lewis Strauss, "The news this morning about you and the John Scott Award make mighty agreeable reading for this retired bureaucrat. Congratulations." I immediately sent a thank-you note, addressed to his office (Room 5600, 30 Rockefeller Plaza, New York 20, New York.)

I had lunch in the cafeteria with William Angus MacFarlane (British Scientific Mission to this country) and some of the other lab people. Then, about 2 p.m. Don Cooksey brought Dr. MacFarlane around to my office to visit. We talked about a number of general things, such as how Nobel Prize winners are selected. On more specific topics MacFarlane said that Dr. Spence (of Harwell) is interested in methods for accelerating heavy ions such as carbon and the chemistry of americium and curium. In both cases I told him that there is no new information beyond what is available in the open literature.

I have given considerable thought to the matter of Al Ghiorso's clearance. Today I mailed the following well-composed letter to Walter J. Williams (Director, Division of Production, AEC, Washington), which I sincerely hope will handle the problem.

February 25, 1953

Mr. Walter J. Williams, Director  
Division of Production  
U.S. Atomic Energy Commission  
Washington 25, D. C.

Dear Walt:

I am writing to give you as much pertinent information as possible regarding Albert Ghiorso because, as you know, I am extremely concerned about this case.

I shall begin by outlining for you the extent of my personal acquaintance with Albert and Wilma Ghiorso. I have been intimately acquainted with Albert for somewhat more than 11 years and with Wilma, a year or two longer than this; thus I have known them both since before their marriage about 11 years ago. Mrs. Seaborg has been well acquainted with Albert for about the same period as I, has been intimately acquainted with Wilma for about 15 years, and joins me in these statements regarding our association with them. Mrs. Seaborg and I feel that Albert and Wilma Ghiorso have been the closest of all of our friends during essentially all of this period. I might add that my official connection with Albert Ghiorso began shortly less than 11 years ago when he joined my chemistry group at the Metallurgical Laboratory of the University of Chicago in the summer of 1942.

As I have indicated, our social connection with Albert and Wilma Ghiorso has been very close during the last 11 years. In fact during the period from 1944 to 1946 we occupied apartments across the hall from each other in Chicago and saw each other almost daily; during this period the four of us played cards together countless evenings. Since the return in 1946 of both families to the San Francisco Bay Area in California, where Ghiorso continued as a member of my group at the Radiation Laboratory of the University of California and where he has been ever since, we have not lived in such close proximity, but we have continued our close social relationship. Our families visit each other quite often as, for example, last Thanksgiving Day when we had dinner together at my home.

I am sure that you would be interested in my knowledge about Ghiorso's interests and activities outside of my official and social contacts with him. So far as the Chicago period is concerned, I would say that his closest friends, other than Mrs. Seaborg and I, were probably Leonard and Alice Katzin. Katzin was a member of my chemistry group at the Metallurgical Laboratory, and I would say that the Katzin's have rather ordinary or average interests and displayed no unusual interest in politics. Since our return to California the Ghiorzos have been so occupied with their two children, Wilma's health has been so poor, and he has given so much of his time to his laboratory program, including evenings and weekends, that as a result their social contacts have been so few that I cannot single out any special social contacts other than with our family. Albert himself plays golf often and takes many business trips with Isadore Perlman and Stanley Thompson of our Laboratory, giving them much contact with him outside of the laboratory, and they are in complete agreement with my impression of him.

continued

I should perhaps make some special mention of my knowledge of some of Wilma Ghiorso's associations since some question has been raised in regard to her former roommate Mrs. Vivian Boarman (Vivian Child). During Mrs. Seaborg's and my acquaintance with Wilma, we have found nothing to excite our suspicion or even to attract our attention so far as possible adverse political ideology is concerned. Wilma's association with Mrs. Boarman precedes my acquaintance with her and I have never met Mrs. Boarman. Mrs. Seaborg met Mrs. Boarman during the earliest part of her (Mrs. Seaborg's) acquaintance with Wilma and had little social contact with her. It is Mrs. Seaborg's impression that Wilma roomed with Mrs. Boarman solely as a matter of economic convenience, that they had practically no mutual friends and little common interests other than music and that, in fact, there was no particular bond between them.

I think that I should also comment on the Ghiorso subscription to the People's World since this is one of the items in question. I feel completely confident that this subscription is only the result of the feeling by the Ghiorzos that they have the right to subscribe to and read any legally published material. Ghiorso has the deep feeling that such rights should not be abridged by any extra-legal means and his complete honesty and candor in all matters makes me certain that nothing more is involved here. I have discussed with him the extent of his believing what he reads in the People's World, and he has made it clear to me that he believes that most of it is "hogwash" (to quote him). He says, however, that he does find in this and other media some information which is not printed in the daily press, and therefore sometimes gives him the opportunity to obtain more information on both sides of questions. He also adds that Mrs. Ghiorso did not subscribe to the People's World as the result of solicitation or introduction to it by Mrs. Boarman, but rather as the result of curiosity occasioned upon encountering it for the first time at a newsstand. I should also add that there has never been any attempt by the Ghiorzos to hide the fact that they read the People's World. In fact, they are characterized by an extreme frankness in all such matters and I have found conservative people to remark after a typical conversation with Albert that they found him "refreshing".

I should also like to say a word about Ghiorso's attitude about secrecy and security. He is thoroughly in accord with the necessity for secrecy and security, and is, in fact, less vocal than the average scientist with respect to the adverse effects of secrecy on the progress of science. He is moderate in all of his habits and on the infrequent occasions in which I have seen him engage in social drinking, I have never seen him lose his sobriety. I feel that Ghiorso does not constitute the slightest security risk.

A very important point is his value to the research program of our group at the Radiation Laboratory. I would rate him as the most valuable laboratory man we have in our entire group of some 100 scientists, and his loss would virtually cripple the major part of our research effort. He is responsible for the instrumentation in the most crucial parts of our program and has the greatest aptitude and genius I have ever seen for the construction and especially the actual use of electronic radiation detection instruments in collaboration with the work of chemists. I should add that he has been a key man in the work of my chemistry groups ever since about six months after he joined us in the summer of 1942. He made decisive contributions to the chemical extraction process which was put into operation at the Hanford chemical plant when it started in 1944 and which has been in operation until very recently. He has been a key man in the weapons research which we have been doing in collaboration with Los Alamos, having

made some of the most crucial measurements in this regard. He has been the backbone of our laboratory's contribution to the long-range detection of foreign nuclear explosions having been the first to positively identify the explosive ingredient. He has made numerous contributions of the utmost importance to basic nuclear science, and I need only illustrate this by pointing out that he is a co-discoverer of the transuranium elements americium (atomic number 95), curium (96), berkelium (97), and californium (98). There are other contributions of a similar type which he has made and with which I believe you are familiar but which I do not feel that I should mention here because I am trying to keep the discussion on an unclassified level.

I hope that this information will be useful to you. I have tried to include everything that occurs to me to be of possible value and would be happy to elaborate further on any points where this might seem worthwhile. I want to emphasize that I have complete trust in Albert and Wilma Ghiorso and haven't the least doubt in my own mind that they are entirely reliable and that our country is not jeopardizing its security in the slightest in retaining his services. His scientific contributions to his country have been outstanding, almost unmatched, and in my opinion a substantial loss to our country's security would result if Ghiorso's crucial contributions were not to continue. Furthermore, if such action were taken I feel certain that a grievous blow would be dealt to the morale of his many colleagues since the esteem with which they hold him as a person and as a scientist can hardly be exaggerated.

Cordially yours,

Glenn T. Seaborg

GTS/db

Thursday, February 26, 1953

The group meeting this morning was attended by Asaro, Batzel, Carniglia, Carr, Clark, Cobble, Cunningham, Dauben, Dunlavey, Feay, Glass, Gray, Grover, Hoff, Hulet, Jaffe, Kalkstein, Lessler, Levy, McLaughlin, Michel, Nervik, Passell, Perlman, Raby, Seaborg, Shudde, Smith, Stephens, Surls, and Templeton.

Dunlavey described a recent experiment in which he bombarded neodymium on the 60-inch cyclotron with helium ions (energy from 40 to 0 Mev). He was able to set a lower limit of approximately  $10^6$  years for the half-life of  $\text{Sm}^{146}$  and said that the limits of detectability in their photographic emulsion are such that they should see the  $\text{Sm}^{146}$  alpha particles if it has a half-life of less than  $10^9$  years.

Hulet reported that he and Hoff bombarded essentially pure  $\text{Cm}^{242}$  with helium ions to make  $\text{Bk}^{245}$ , which they studied on a scintillation pulse analyzer. They saw the following gamma rays (kev): 43, 67, 245, 330?, 510?, and a 112 kev K x-ray. They have not completed calculations involving the L x-rays. They set a lower limit of approximately 2000 years on the  $\text{Cm}^{245}$  half-life, the daughter formed by the electron capture decay of  $\text{Bk}^{245}$ .

Carniglia described in some detail a two-ended diffusion crucible for vapor pressure measurements. He presented the results of some trial runs.

Nervik reported on his and Michel's experiments on the mass assignments of thulium. They have seen both the previously reported 7.7-hour  $\text{Tm}^{166}$  and 9.6-day  $\text{Tm}^{167}$  in milking experiments and a two-component decay in the erbium elution peak. The long-lived component of the erbium peak appeared to be due to thulium contamination, but the short-lived component appeared to be due to a 10.2-hour erbium ( $\text{Er}^{165}$ ). Nervik said they suspect the 10.2-hour erbium is the daughter of the 26-hour thulium that they have also observed. Since  $\text{Er}^{165}$  has been reported as having a half-life of 10.0 hours and 11 hours, they believe they have established  $\text{Tm}^{165}$  with a 26-hour half-life. He added that this  $\text{Er}^{165}$  is the 10-hour erbium they were unable to resolve from their gross erbium decay curve and noted that on Fred Reynolds' mass spectrograph they see mass numbers 160, 161, and 165 in the gross erbium fraction.

\* \* \* \* \*

I gave the Chemistry 223 lecture, again on mesons, and then went to San Francisco with Greg Engelhard for a 1 p.m. luncheon meeting with Alfred Masters (Athletic Director, Stanford) and Willis O. Hunter (Athletic Director, Southern California) for a discussion of the Cannamela incident.

I had made arrangements to stop in to see John Francis Neylan after this meeting, and I spent about an hour with him (2:45 - 3:45 p.m.) in his office, reviewing the progress to date in our plutonium patent compensation case and mentioning that we may want his modified disclaimer signed one of these days. Neylan seemed agreeable.

Back in my office at the lab I signed six copies of a letter supporting the nomination of Dr. Hoylande D. Young for the Austin M. Patterson Award for achievement in chemical documentation. This was requested some time ago by Mildred Oncken of the Chicago Section of the ACS. My letter emphasized Dr. Young's role in the editing and preparation of the technical reports known as The National Nuclear Energy Series. The copies were mailed to Miss Oncken for forwarding to James F. Corwin (Antioch College, Yellow Springs, Ohio), Chairman of the Awards Committee of the ACS.

Friday, February 27, 1953

After the usual phone calls, etc., I met and spoke for a while with Gibb, who arrived Wednesday evening to consult with Project Whitney people about their hydride problem. Because of his expertise with hydrides, we would like to convince Gibb to accept the position as Head of Inorganic Chemistry for our Whitney Project.

I then took care of some of my correspondence. I wrote to Norman Bonner. I explained that, as he may have heard, we do not have funds to offer Bob Wolke a postdoctoral fellowship. I said that we were interested to learn that he (Bonner) is considering changing employment and then mentioned the new project starting up in Livermore, stating that it is not the same as that being operated by California Research and Development, which I mentioned to Wolke. I suggested that a salary of something like \$700 per month would be appropriate and that he should let us know if he is interested.

In a February 11 letter, Ron Brightsen asked that, if his course credits for a Ph.D. from MIT are accepted here, he would be able to work fulltime, thus earning \$302 monthly instead of \$151. He also asked if all of his research will be credited toward his thesis. In today's response I said that perhaps I was not as clear as I should have been in my letter of December 4, 1952, noting that even though we can start him at a third year salary rate, it does not mean that he could obtain his Ph.D. in one year, although he might be able to achieve it in two years. I also explained that the salary must be at the half-time rate during the nine-month school year for any graduate student until he receives his Ph.D. I told Brightsen, however, that all of the work under this arrangement would be on his thesis problem, and I explained our policy with regard to course work and the preliminary examination. Finally, I added that the out-of-state tuition is waived in the case of graduate students who come with and maintain distinguished grades in research and courses; this leaves only the incidental fee of \$37 per semester.

I also answered a February 10 letter from Raymond R. Edwards, an old friend now at the University of Arkansas. Edwards recommended a young man, James R. Day, who is just receiving his Ph.D. from the University of Arkansas. I explained that our budget is such that we are very limited with respect to postdoctoral positions, and I asked Ray to inform Day of this situation.

In today's mail was the copy of a letter from James H. Stack (ACS News Service), addressed to Waldemar Kaempffert (Science Editor, The New York Times), saying he was gratified to see the recognition about the

Scott Award, but pointing out that I and Dr. McMillan received the Nobel Prize in 1951 in Chemistry, not in Physics.

In connection with PCC business, a letter, dated February 20, arrived from Mac (Claude E.) Jones (Faculty Athletic Representative at UCLA). Jones questioned the reason for the vote against the Spring basketball practice and asked how the press in the Bay Area knew about the vote before the people at UCLA did. I decided to telephone Jones with the explanation and then take the opportunity to talk with him about the Cannamela case and try to enlist his support for my planned attempts to get the PCC to take some sort of censure action against USC in this connection. Jones' response was vague; it is difficult to communicate with this fellow.

Saturday, February 28, 1953

The kids took up a lot of my time today, but I also worked on various preparations (and talks) in connection with my trip to Los Angeles this coming month.

Sunday, March 1, 1953

I worked on some of my writing and speaking projects; then the kids and I helped Helen celebrate her birthday--a day early.

Monday, March 2, 1953

I made a few phone calls and then dictated responses to a few letters that have arrived in the last few days. I also had a meeting to discuss the problem about finding a magnet for McGarvey (Bruce R.), the new instructor on campus.

A short note went to Walter J. Williams, the AEC official involved with Al Ghiorso's clearance. I enclosed a reprint of my Nobel lecture, a supply of which have recently arrived, and referred to my reference to Ghiorso on page 154, in which I said:

I feel that I should especially single out the contributions of my colleague ALBERT GHIORSO whose name, as you may have noticed, has come up time after time as I have gone through my description here of the work on the transuranium elements. GHIORSO has been a key man throughout and is one of those individuals without whom a laboratory such as ours simply could not run.

I recently agreed to speak in San Francisco at the John Ericsson memorial celebration, sponsored by The Swedish-American Patriotic League, on Tuesday, March 10. I sent a confirmatory note this morning in reply to Hugo Olsson's letter of February 24, in which he gave me some information about the program. Olsson is secretary of the group.

I also wrote a note to Provost Gordon S. Watkins (Riverside campus) to inform him that the arrangements outlined in his letter of February 11 for my Charter Day Address and my Present Day Club appearance seem satisfactory to me.

Ray Sheline wrote on February 18 to update me on his work on Mg<sup>28</sup>, saying that he had heard from Manfred Lindner about Lindner's work on the isotope. Sheline also asked about receiving UCRL reports. I had Doral check and today explained to Sheline that I understand that, in order to receive the reports regularly, he must get Atomic Energy Commission approval and that the person to contact is Dr. A. F. Thompson, Technical Information Services, U. S. Atomic Energy Commission, Washington 25, D. C. I also explained that Dr. Lindner's reports are issued by the California Research and Development Company rather than by the Radiation Laboratory.

At 10:15 a.m. I went down to campus to participate in the qualifying examination of Jose F. Fernandez in Room 202, Lewis Hall. Fernandez' topics were (1) The conductance of aluminum complexes in non-aqueous media and (2) The mechanism of the diazotization reaction. His committee was composed of Gerhard K. Rollefson (chairman), Chaim Richman, Stephen P. Dilberto, Rollie J. Myers, George C. Pimentel, and Glenn T. Seaborg.

I returned to the hill in time for the senior staff luncheon meeting in my office at which a number of topics were discussed, including

yesterday's pulse analysis of the sample from the work-up of 75 pounds (a garbage can full) of coral, which confirmed our earlier work on the identification of a 1-day, 7.1 Mev alpha emitter of element 100.

A long interesting letter arrived from John Rasmussen, in which he made a few requests and asked a number of questions, described some of the work at the Nobelinstitutet för Fysik, spoke of the lectures being given (in English) by Aage Bohr (the first lecture dealt with the fundamentals of a nuclear structure model combining features of the single particle shell model and the collective or liquid drop model). John also discussed the life of a non-Swedish speaking American in Sweden, the cost of living in Sweden, and even the problems with Swedish typewriters. I routed the letter to a number of people around the lab.

At about 2:30 p.m. I telephoned Rod Spence and told him that we have decided to hold to the March 15th date for our report. I told him that we have worked up one garbage can of coral [the men are working up this material in the "Annex" behind Bldg. 5] and found 0.5-1 c/m of the 7.1 Mev alpha-particle emitter and about 25 c/m of the 6.6 and 6.1 Mev alpha-particle emitters. The fellows, I said, believe it was a good run. Spence told me that our two tons of additional material will arrive in 250-pound boxes, delivered by a C-54 plane at Travis Field the week of the 16th to the 21st, noting that the Argonne portion will be in separate boxes. Spence added that we will discuss the 100-ton project after the survey from these boxes is complete.

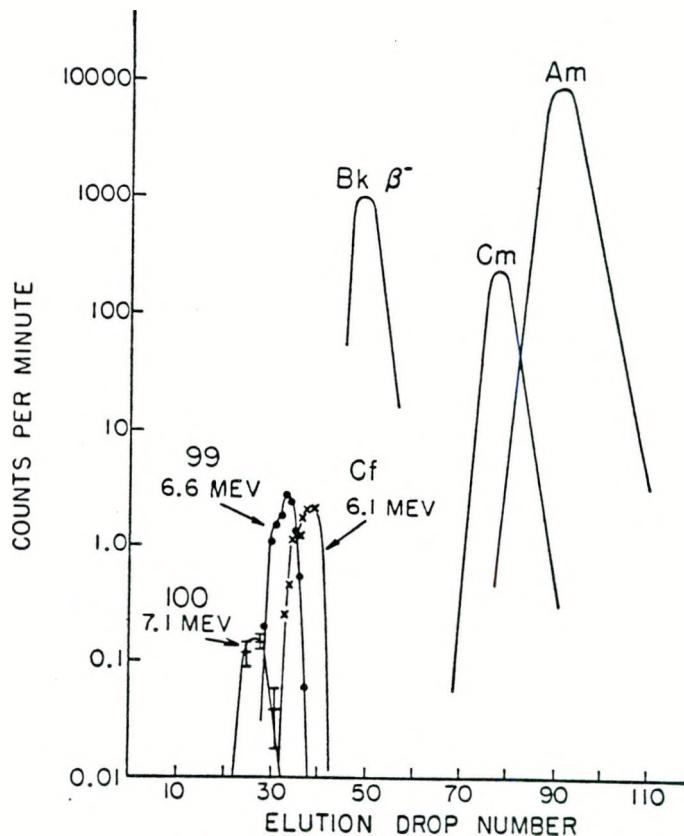
Tuesday, March 3, 1953

I had my usual phone calls and conversations this morning before going to campus to give the Chemistry 223 lecture.

After stopping in the Department of Chemistry office and having lunch at the Faculty Club, I had a 1 p.m. appointment with Herman A. Spindt (Director of Admission) to discuss the mechanism for handling eligibility, etc. of athletes.

Larry Blake, Greg Engelhard, Al Ghiorso, and I went out to Mira Vista for a round of golf (LB-95, GE-105, AG-93, GTS-100). Larry and Greg won our low ball, low total match, 1 up.

Back at the lab Al showed me the curve of the elution data for element 100 taken on Sunday, March 1, which he has now plotted, together with the elution data for the earlier elements.



Wednesday, March 4, 1953

This morning I picked up Paul S. Olmstead (Bell Laboratories) at the Durant Hotel at about 9 a.m. and drove him to the hill for a 9:30 a.m. appointment.

A telegram arrived yesterday afternoon from Dean Thomas S. Hall (Washington University, Saint Louis) requesting a telegraphic evaluation of Arthur C. Wahl for promotional reasons. I responded this morning with:

DR. A. C. WAHL HAS MY HIGHEST RECOMMENDATION. HE IS A METICULOUS INDUSTRIOUS AND PROLIFIC WORKER WELL LIKED BY HIS COLLEAGUES. I CONSIDER HIM TO BE ONE OF THE OUTSTANDING NUCLEAR CHEMISTS IN THIS COUNTRY AND IN THE WORLD.

Thursday, March 5, 1953

This "first of the month" research group meeting was attended by Asaro, Batzel, Carr, Clark, Cobble, Conway, Cunningham, Dauben, Feay, Gunn, Grover, Hollander, Hyde, Jaffe, Kofstad, Lessler, Levy, Mathur, Michel, Nervik, Perlman, Mangipudi Venkata Ramaniah (an Indian student from Andhra University who wants to enter UC-Berkeley as a graduate student), Ruben, Seaborg, Shudde, Slater, Smith, Stephens, Surls, and Templeton.

Kofstad was the first to report and described a 117-minute

positron-emitting niobium activity found in the spallation products of silver (340 Mev protons). The positron energy is 2.9 Mev (bender) and 2.7 Mev (aluminum absorption). After 24 hours, he milked 78-hour  $Zr^{89}$  from the niobium and therefore ascribes the new activity to  $Nb^{89}$ . Hyde pointed out that Charles Levine, using anion column procedures (zirconium with protons) about a year and a half ago, found 78-hour  $Zr^{89}$  growing into his niobium fraction; the half-life of the niobium parent was 2 hours. We discussed Kofstad's chemistry. Kofstad added that he has indications that  $Nb^{89}$  decays approximately one-third by positron emission; he did not investigate gamma rays. Kofstad also mentioned that he has obtained, from the spallation of silver with protons,  $Mg^{28}$  ( $1.1 \times 10^{-3}$  mb) and  $C1^{38}$ ) ( $1.3 \times 10^{-3}$  mb).  $Mo^{90}$  was obtained but has not been resolved from the 15-hour  $Nb^{90}$  daughter.

Gunn talked about his recent work, using the semi-adiabatic microcalorimeter with isothermal environment, for the calorimetric assay of samples of americium and curium.

Templeton spoke about his re-examination of x-ray diffraction photographs of cyclobutane taken by Giles Carter and said that the high temperature form, stable at  $-100^{\circ}\text{C}$ , is found to be body-centered cubic, with  $a = 6.06 \pm 0.03$  Å. He went on to say that assumption of two 3/5/53-2 molecules in the unit cell yields the reasonable density of  $0.84 \text{ g/cm}^3$ , and to account for the symmetry properties, rotational disorder of the molecules is assumed.

Asaro again talked about empirical relations between the excited states of even-even nuclei, in which the ground state has a spin of 0, the first excited state is +2, and the second excited state, at least in some cases, has the spectroscopic state +4. He spoke about the theory based on the rotational model of the nucleus, in which it follows that the ratio of the energies of the first two excited states should be  $E_2/E_1 = (4 \times 5)/(2 \times 3) = 3.3$ , and then went on to give a figure showing a plot of this ratio for those nuclides for which the value is known. He pointed out the point for  $Th^{230}$  is on the line; those of  $Th^{228}$  and  $Ra^{224}$  are not and suggested the effect may be due to a closed neutron subshell. Asaro went on to discuss his investigation of the alpha decay of  $U^{232}$ , which had been previously shown to have two alpha groups, separated by 58 kev, corresponding to the separation between the first excited level and the ground state of the daughter,  $Th^{228}$ . The estimated energy value (from the rotational model) for the excited state was about 183 kev (close to  $58 \times 3.3$ ) and the calculated abundance of the corresponding alpha group of  $U^{232}$  was 0.53%. They investigated a new sample of  $U^{232}$  and found a gamma ray at a level of 189 kev with an abundance of 0.55%.

Asaro said that in the beta decay of  $Ac^{228}$  to  $Th^{228}$ , gamma rays of 58 and 129 kev have been found; he stated that the level spacing decreases in going away from a closed shell--for lead it is 2.5 Mev and for curium it is 44 kev. Continuing, he added that  $Pu^{238}$  is similar to  $Th^{228}$  and that, in the beta decay of  $Np^{238}$ , conversion lines corresponding to 43 and 103 kev gamma rays have been found. He presented a decay scheme for  $Np^{238}$ , noting that there must be an unobserved beta particle in coincidence with the 103 kev gamma ray and saying that a coincidence has been observed for the gamma ray and a high energy beta group that has not

been definitely assigned. Perlman added that this will change the decay energy of  $\text{Np}^{238}$  by 100 kev and reconcile some spin values.

\* \* \* \* \*

After giving the Chemistry 223 lecture, I stopped in the Department of Chemistry office, where I spoke with Dean Pitzer about a young man, Stephen Kahn, who is applying for graduate work at Berkeley and for whom I have received several letters of recommendation (from Harry S. Mosher and George S. Parks at Stanford; Saul J. Abraham for whom Kahn worked on a project for the National Institutes of Health; and Charles R. Maxwell of the National Institutes of Health). Kahn's undergraduate work was taken at Stanford. I told Pitzer that I would send copies of the letters down to him (I have already had the letters shown to Iz and Hyde). Pitzer also told me that he has accepted Robert F. Nickerson from Tufts College for graduate work. Professor T. R. P. Gibb recommended this student who, because of his background, may be useful to our Whitney program.

At 2 p.m. I chaired the qualifying examination of Thomas O. Passell (Room 202, Lewis Hall). Other committee members were C. Arthur Knight, William A. Nierenberg, Rollie J. Myers, Gerhard K. Rollefson, and David H. Templeton. Passell defended the propositions (major) "It is proposed to recheck the previous determinations of the heat capacity of graphite in the temperature range (298°K to 1400°K) using the 'method of mixtures' on carefully purified samples of graphite from different sources" and (minor) "It is proposed to review the various mechanisms that have been proposed for the reduction of  $\text{HNO}_3$  in aqueous solutions." Passell, who is one of Perlman's research students, passed his examination.

Afterwards I went up to the hill and looked over my mail. I had received a note from G. Curtis Pritchard, thanking me for the clippings about the Scott Award and stating that the dinner and meeting in the Franklin Institute was one of the nicest Scott Award affairs which he had experienced in his four years of association with the Board.

From Louis N. Ridenour I received an agreement to sign about the publication of my edited text in connection with the Extension Division course entitled "Modern Physics for the Engineer." I asked Doral to witness my signature.

I also checked on some of the research in Bldg. 5 and noticed that Health Chemistry (Manley Wu, et al.) were tearing out the contaminated hoods and sink of Room 105 (Cunningham), preparatory to replacing them.

Friday, March 6, 1953

The first thing this morning a wire arrived from S. G. English, stating that Doyle Northrup and Wilbert H. Urry will join him on the Berkeley trip next week. They plan to arrive Tuesday night and, if convenient, spend Wednesday at Livermore for general discussions with Ken Street and others, including me. English asked that I inform Street and find out about transportation; he noted that they will be staying at the Claremont. I checked with Street and wired back, "LOOKING FORWARD TO YOUR VISIT."

After a number of phone calls, I looked over some of the research, talked with a few students, and then dictated a number of letters.

I answered, point by point, the questions and comments that John Rasmussen wrote in his letter of February 11 and then said we were happy to hear from him and that I hope that he will continue to take time to let us know how he is getting along.

In a letter to Robert F. Nickerson, the new graduate student whom Dean Pitzer has admitted, I described our program at the Radiation Laboratory and the clearance procedure. Because of his special background (having worked with Professor Gibb), I said that we probably will be able to pay his transportation cost.

A letter went to Professor F. A. Paneth (Durham, England) to say that it appears quite definite that I shall not be able to attend the meeting of our committee (Joint Commission on Radioactivity) in Stockholm next summer and that I want him to feel free to distribute the travel money to the other participants. I added that I shall be very interested in reading the minutes of the meeting.

P. Rosbaud (Pergamon Press, Ltd., London) wrote on February 19 to ask my opinion and advice about the establishment of an Anglo-American journal of inorganic and radiochemistry. He enclosed some descriptive material, which I discussed with a number of colleagues. Today I replied that he is correct in surmising that I am interested, especially with respect to radiochemistry and more broadly to the publication of chemists' contributions to nuclear science in general. Copies of my response were sent to Warren C. Johnson (with whom Rosbaud has already had correspondence) and W. A. Noyes, Jr.

Tom Jones, whom I have known since Met Lab days, spoke to me at the Atlantic City ACS meeting about the possibility of my visiting Haverford College (Haverford, Pennsylvania) for several lectures, and a few days ago I received a letter from him outlining the type of series they would like and saying that President Gilbert F. White has authorized him to suggest an honorarium of \$1000 plus travel expenses. Jones also offered a suite of rooms so that Helen can accompany me. Today I wrote and accepted the invitation for the week beginning Monday, October 12, 1953. (I explained that, as Faculty Athletic Representative for the University of California, I shall be in Philadelphia for the Pennsylvania-California football game on October 10.) I also suggested possible speakers for other lecturers in the proposed series and asked for a little more detail of the number and types of discussions he has in mind for me. Finally, I added that we appreciate the invitation for Mrs. Seaborg to accompany me but, since this may offer difficulty with respect to our children, this may not be possible.

To J. L. Culbertson (Chemistry Department Chairman, The State College of Washington), who wrote on February 27 to ask about John Rasmussen whose name had been suggested by Ray Sheline as a possibility for a position in their department, I wrote that Rasmussen is, indeed, an outstanding man and that we are offering him a permanent position on our staff. I suggested that perhaps we will have the chance to chat at the ACS meeting in Los Angeles.

F. W. Melpolder (The Atlantic Refining Company, Philadelphia) sent me a copy of the abstract of my talk before the Philadelphia Section of the ACS (the Scott Award talk), requesting that I make the necessary corrections in order that they may publish it in the Catalyst. I looked it over, made a few minor corrections, and returned it today.

Today's mail brought a transcript of a discussion on the social and international implications of science held two weeks ago at the Council on Foreign Relations, sent to me from The Institute for Advanced Study (Princeton) by J. Robert Oppenheimer, who participated in the discussion. This should be useful in the preparation of my Charter Day Address at Riverside.

Saturday, March 7, 1953

I played with the kids and worked over material for upcoming talks.

Sunday, March 8, 1953

Today's papers contained articles on the March 5 death of Josef Stalin. I also read about Cal's performance in the basketball playoff games in Seattle. The Bears, under Nibs Price, won the PCC Southern Division Championship but lost to the University of Washington Friday (47 to 60) and Saturday (57 to 80).

Although I worked on my Charter Day address, other talks, and on the report that Stan, Al, Gary, and I are to submit on the new elements, I still found time to act in a fatherly capacity.

Monday, March 9, 1953

After the usual phone calls, I took care of some administrative matters, including writing a memorandum to George Everson about the employment of the graduate student recommended by T. R. P. Gibb, Jr., Robert F. Nickerson. I sent Everson a copy of the letter I wrote to Nickerson on Friday and emphasized that, since he will be of particular value to the Whitney Project, we want to have arrangements made to pay for his transportation to Berkeley.

In a brief letter to Bernard Harvey, who recently wrote that he wants to delay his arrival in Berkeley, I replied that his later arrival date is certainly satisfactory and adding that I am sure he understands that he will be welcome at any time that suits his convenience.

Before the noon senior staff luncheon meeting, Ken Street and I conferred about some of the staffing problems on Project Whitney. One of the names recently suggested to me for the Whitney group is Harry Bowman, who presently works under Hugh Farnsworth. I talked with Street about the visit of English, Northrup, and Urry who will arrive Tuesday night and asked him to handle the transportation to Livermore.

In the mail, much of which fortunately requires no attention were two more letters of recommendation for Stephen Kahn: from Charles R. Maxwell and from Howard L. Andrews (National Institutes of Health). I told Doral to have them copied for Dean Pitzer.

Bob (Robert A.) Naumann (Princeton) returned some slides that Earl Hyde had sent him late last year. He wrote, "I am glad to hear the news about the new elements and am sure you are all well pleased to have rounded out the table to an even 100." Bob apparently has learned about our recent work. I routed the letter to Earl.

There were also a couple of notes--from Helen M. Kiley with copies of the publicity concerning the John Scott Medal Award and from Mildred Oncken, who thanked me for my letter about Hoylande D. Young.

At 2 p.m. I had an appointment with Nevin K. Hiester (Stanford Research Institute) to talk with him about the position of Head of Chemical Engineering of Project Whitney.

After checking some of the research (I saw Hulet loading a 2  $\mu$ g target of americium in the south hood of Room 104, Bldg. 5, for a bombardment on Wednesday), I spent much of the rest of the afternoon working on my Charter Day address, which I am titling "Dawn of the Atomic Age."

Tuesday, March 10, 1953

After the usual administrative matters and phone calls, I went down to campus and gave the Chemistry 223 lecture--I assigned a problem set and talked about the mass equation. I stopped in the Department office before returning to the hill and learned that Alfred Chetham-Strode (Nucleonics Division, Hanford) has been accepted as a graduate student. When I got back to my office, I asked Doral to write our regular letter to Chetham-Strode offering him a position with our group.

In today's mail was a nice letter from Harry Emeléus, describing the work Alan Clifford did while on a Guggenheim Fellowship at Cambridge. He also wrote, "I can also vouch for the fact that he is a good teacher and that he has built up a very interesting group of research projects which, given the opportunity, he should be able to carry through. It only remains to say that we have liked having both him and Mrs. Clifford with us and that they have fitted in well into the laboratory and general social life."

I made a tour of some of the labs, including the third floor of Bldg. 50 where I met Frances T. Jewel (Chemist P2, 3.2), who will work for Hyde and Newton.

After dinner I went to The Swedish American Hall (2174 Market Street, San Francisco) for the John Ericsson memorial celebration. The program began at 8:20 p.m. with songs by Miss Britta Ekwall (soprano), I spoke for a short time on atomic energy, and the program concluded with music by Verne M. Sellin (violinist, San Francisco Symphony). During the refreshments after the program I met a number of members of The Swedish-American Patriotic League, including Hugo Olsson (secretary).

Wednesday, March 11, 1953

I returned some signed and notarized papers to R. C. Ploss about my transfer to the State Employees' Retirement System, also requesting that

he add the name of my youngest child Stephen Keith Seaborg to Form 240, Item 8b.

Most of the day was taken up with the visit of English, Northrup, and Urry to Berkeley and Livermore.

Thursday, March 12, 1953

The research group meeting this morning was attended by Asaro, Carniglia, Carr, Clark, Cobble, Dauben, Gray, Grover, Hoff, Hollander, Kalkstein, Lessler, Levy, Michel, Meinke, Nervik, Raby, Ruben, Seaborg, Smith, Stephens, and Templeton.

Stephens gave an extensive report on a column separation of radium and other daughters from thorium using Dowex-50 resin. The best results were obtained by eluting with lactic acid solution (pH between 1.1 and 4.8).

Asaro talked about alpha decay of several isotopes, including  $\text{Th}^{228}$  in which an alpha group decaying to a level at 217 kev and one to a level of 84 kev have been seen by another laboratory. A 221 kev gamma ray has been seen in the sodium iodide scintillation spectrometer (decay directly to the ground state). He added there is another level at 169 kev above the 84 kev level but they are not sure of its origin. Asaro went into some detail about these excited states. He spoke about  $\text{Pu}^{242}$ , produced in the Arco pile by the neutron bombardment of  $\text{Am}^{241}$  and purified by Higgins and Thompson, which was shown to have an alpha group at 4.898 Mev. A second group was found at 45 kev lower in decay energy in 20% abundance (compared with other group); this could belong either to  $\text{Pu}^{239}$  or to  $\text{Pu}^{242}$  from intensity considerations but at present will be assigned to  $\text{Pu}^{242}$ . Asaro reported that the alpha particle energy (ground state transition) of  $\text{U}^{232}$  is 5.318 Mev, which agrees well with the pulse analyzer value of 5.31 Mev. In speaking of  $\text{Pu}^{239}$  he said there is probably a 100 kev conversion electron in coincidence with one of the three high intensity alpha groups, which would indicate that they have not yet seen the ground state transition of  $\text{Pu}^{239}$  alpha decay. Asaro mentioned some nuclear emulsion work of other laboratories, which have reported 100 kev electrons in coincidence with 0.5% of the  $\text{Pu}^{239}$  alpha particles.

Templeton told the group about his calculation of crystal energies with Madelung constants.

\* \* \* \* \*

After giving the Chemistry 223 lecture on mirror nuclei, the Fermi theory, Gamow-Teller rules, etc., I went up to the hill.

One of the men suggested by Dr. Thomas Gibb as a possible candidate to work on Project Whitney's hydride problem was W. Kermit Anderson of Argonne. Today I signed a letter, written by Earl, to Anderson briefly describing the problem and saying that if, as Dr. Gibb suggested, he might be interested, he should inform me and I will arrange an interview.

In today's mail was a letter from Norman Bonner, requesting more

information about the position in Livermore that I had mentioned to him. I routed the letter to Earl for answering.

I also received a note from Dan (Daniel R.) Miller, saying that he and George Kavanagh plan to be in the Bay Area from March 26 through March 30 to visit UCRL, Livermore, and NRDL. They would like to spend the 30th on the hill with Iz, Stan, and me, if it is convenient. Dan added that he will call on March 26 or 27 to confirm these plans.

Stan, Gary, Al, and I again worked on the draft about the Mike work that we are to submit to Spence on the 15th.

Friday, March 13, 1953

Since I am going to be out of town for some time, I attempted to clear my desk this morning and take care of urgent phone calls.

On March 5 Howard S. Cook (Public Information, Riverside) wrote for an advance copy of my talk at the Riverside Charter Day banquet to be used for the press coverage. He also mentioned that Joe Wimer (sports editor of the Press-Enterprise in Riverside) would like to chat with me for a few minutes about the Pacific Coast Conference during my visit. Today I sent Cook a rough draft of much of my talk and requested that, if he participates in the preparation of any press release, he emphasize the central theme which I regard as the need for greater education in scientific matters of the general public as a prerequisite to the solution of any dilemma which we might be in, through the operation of our normal democratic processes. I added that I shall be glad to see Joe Wimer sometime during the afternoon of Wednesday, March 25.

I also wrote to Warren H. Crowell regarding the Dickson Award at UCLA. I said that I shall plan to join him at the reception preceding the Charter Celebration Dinner at the Beverly Hills Hotel and asked if he could have someone pick me up at my parents' home in South Gate at about 5:30 p.m. I explained that I shall be in Los Angeles all of next week for the ACS meeting and can be reached, if necessary, at the Statler Hotel or at my parents' home.

H. I. Schlesinger (The University of Chicago) wrote on March 5 about the Rosbaud letter and asked the opinion of me and others in my institution about the proposal. In my reply to Schlesinger today I stated that I wrote Rosbaud that I would delay expressing an opinion until I have a chance to canvass the feelings of a number of associates. I said that I plan to discuss it with a number of people at the meeting of the ACS in Los Angeles next week, adding that I am not too satisfied with the present status of the Journal of the American Chemical Society and have been advocating that, in its semi-monthly publication schedule, it should segregate its material and alternate physical, inorganic, and nuclear chemistry in one issue and organic and biochemistry in the other. I pointed out that nuclear chemists have solved their problem by publishing extensively in The Physical Review. I told Schlesinger that I shall let him know how our ideas develop at the ACS meeting.

In the mail that arrived today was a letter from W. Albert Noyes, Jr. (Editor, Journal of the American Chemical Society) on the same subject.

Noyes said his own personal view is to give his blessing to the formation of more specialized journals and that, only in this way, can the Journal of the American Chemical Society be reduced to manageable proportions; he pointed out, however, that specialized journals are apt to be expensive. Noyes added that he understands that the motion of the Editorial Board for dividing the JACS by subject matter was referred by Jake Warner to the Board of Directors, which referred the question to its Publications Committee for study. Noyes reported that he has heard nothing from the Publications Committee and that Jake Warner will not be in Los Angeles; however, Farrington Daniels will be at the meeting and can probably tell us what is going on.

Alan F. Clifford wrote again on March 1 to thank me for my letter and to say that he has requested that Professor Emeléus write a letter of recommendation (which I have now received). Clifford said that there appears to be a good prospect of a position at Pennsylvania State College, and he asked that I write to Dr. Fernelius in his behalf. I signed a letter that Earl prepared, addressed to W. Conard Fernelius, describing my early contact with Clifford during the Met Lab days, and my following his work through graduate school and postdoctoral days, in addition to his work under his Guggenheim Fellowship at Cambridge. I concluded, "To sum up, Dr. Clifford is, in my opinion, well qualified for the academic career which he has chosen. He was an earnest student, has a good personality for contact with other scholars and students and apparently is a good teacher."

There was also an anonymous letter, dated March 12, regarding ground state terms, which I routed to Harold Jaffe for his study and opinion.

Doral told me that the paper, "An Ion Exchange Study of Possible Hybridized 5f Bonding in the Actinides" (UCRL-1434 Rev) by R. M. Diamond, K. Street, Jr., and G. T. Seaborg, has been declassified with deletions. I looked it over and then wrote Dick at Harvard, sending him a list of the deletions and saying that I do not think they seem central to our argument. I suggested that he go ahead and put on the final touches for preparation for publication.

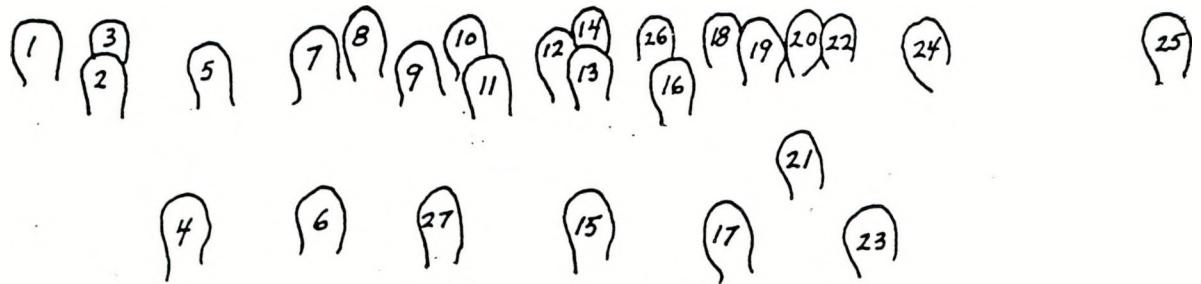
Jim Wallmann accepted our offer to return to Berkeley to work with Burris Cunningham. Yesterday was his first day on the payroll and, on my rounds of the labs today, I talked with him for a while.

James Wallmann



My day was interrupted in the late morning by a visit from the Editorial Staff of the ACS. The staff is having a tour of the Bay Area on their way to the ACS meeting in Los Angeles next week. After a tour

of the lab, we (members of the staff of ACS Applied Publications, local ACS California Section members, and UCRL personnel) had lunch in the cafeteria, followed by the usual picture taking on the cafeteria patio.



Partial photograph identification: 1. Rodney N. Hader, 2. Mrs. Lottie G. Hader, 3. William Q. Hull, 4. James H. Stack, 5. Howard J. Sanders, 6. James M. Crowe, 7. Miss Stella Anderson, 8. Dr. Richard L. Kenyon, 9. Miss Margaret Summers, 10. Merald Lue (Reinhold Publishing Corp.), 11. Dr. and Mrs. Murphy's daughter, 12. Robert G. Gibbs, 13. Dr. Walter J. Murphy, 14. Dr. Richard Newhall (California Section) ? 15. Dr. A. George Stern (active in California Section), 16. Mrs. Gertrude Murphy, 17. DeWitt O. Myatt, 18. Roy Avery, 19. Mrs. Esther Kuney, 20. Joseph H. Kuney, 21. Will Shearon, 22. Dr. Alan Nixon, 23. Russell Flolo (spelling ?), 24. Roy M. McDonald (McDonald-Thompson Advertising Agency), 25. Dr. Robert Stirton (Standard Oil of California), 26. Robert Gould, 27. Glenn T. Seaborg.



Saturday, March 14, 1953

One of the lab drivers picked me up in Lafayette about 11:15 a.m. and drove me to San Francisco in time to catch United Flight 676 (1 p.m.) for Los Angeles. Since I will be gone for such an extended period, Doral will handle what mail she can and forward to Los Angeles anything that needs my personal attention. I will also keep in touch with the lab by phone.

The plane arrived at about 2:40 p.m., and I went out to South Gate to visit with my parents. There I telephoned J. Till Neville about arrangements for tomorrow's television broadcast and then brought my parents up to date about the activities of their grandchildren.

[In Berkeley, a letter arrived from Ellison H. Taylor (Oak Ridge) telling about some of the people who plan to visit Berkeley after the ACS meeting--Charles H. Secoy, who knows no one at the laboratory and who will be here on Friday, March 20th; Glen Clewett (a chemistry classmate at UCLA with me); and Gus A. Ropp. Earl will make arrangements for these visitors.]

Sunday, March 15, 1953

In Los Angeles. After a leisurely morning with my parents, I checked in at the Statler Hotel (\$9.50/day). Several other people from our Berkeley group are here for the American Chemical Society meeting--James Cobble (staying at the Mayflower), Earl Hyde (Biltmore), Edwin Orleman (Biltmore), and Amos Newton (Statler). Jack and Margie Hollander also came down in order that Jack could help me with tonight's television show.

Registration for the meeting was held from 2 until 6 p.m. on the Ballroom Floor of the Statler Hotel, and I met and chatted with a number of acquaintances. Jacob Kleinberg (a well-known inorganic chemist from the University of Kansas) approached me about giving a talk at a Fall Symposium in the Division of Chemical Education. He said he would be glad to visit Berkeley to discuss the possibility of a position with Project Whitney. [Kleinberg is being considered for Associate Director of Chemistry.]

The Southern California Section sponsored a social hour in the Renaissance Room of the Biltmore Hotel from 8 until 10 p.m., but I was picked up at 6:45 p.m. by J. Till Neville to get ready for the 10 until 10:15 p.m. TV program with Hollander, "Radiation Chemists in Action," broadcast over Station KTLA as the science page of "Magazine of the Week." This was patterned after the "Science in Action" program with Dr. Earl Herald last month. [See entry February 3, 1950.] Again, I began by discussing the periodic table, uranium, and the transuranium elements, followed by a demonstration of radioactivity in which I irradiated a silver dollar with neutrons followed by holding it near a Geiger counter; I talked about plutonium and its production and nuclear fission. After my discussion, Hollander demonstrated fission with the mousetrap arrangement in which the traps were set with "neutron" corks, into which a "neutron" cork was dropped, releasing two more "neutron" corks, etc., until all the mousetraps were sprung. [This TV show was available for viewing by the ACS meeting attendees on four TV sets in the

Renaissance Room of the Statler Hotel.]

Monday, March 16, 1953

In Los Angeles. The Committee Advisory to the Chemical Corps, of which I am a member, met at 9:30 a.m. in Conference Room 3 of the Biltmore Hotel. I stayed for a few minutes but then went to the 10 a.m. closed meeting of the Council Committee on Publications (St. Louis Room, Statler Hotel), at which I acted as chairman. The meeting was poorly attended with only Leigh C. Anderson, Howard S. Nutting, Bernard L. Oser, and I present. The two items considered at this meeting were (1) To support the action of the Board of Directors by recommending "that the subscription rates for the journals published by The Society shall be unchanged from those set for 1953" and (2) "To re-recommend to the Council and Board Committees on Publication that some additional funds be appropriated to Chemical Abstracts so that its future budgets can contain specific provision for further research and developmental work on new methods and on nomenclature problems, and for the necessary additional persons to do such work." Written ballots were received in support of these items from committee members Ludwig F. Audrieth, John A. Leermakers, William T. Miller, Melvin C. Molstad, Vincent Sauchelli, and Arthur Rose; members present also voted unanimously in support of the items. We also discussed the fact that Pergamon Press, Ltd. (Rosbaud) has been canvassing people in the United States about the desirability of publishing an Anglo-American journal on inorganic and radiochemistry.

I ran into Henry Taube and took the opportunity to ask him about Harold M. Feder, who has been recommended for our Project Whitney chemical engineering program and who is a student of Taube. Taube reported that Feder is very good and very versatile.

At noon I went to a Research Subcommittee meeting of the ACS Committee Advisory to the Chemical Corps (Biltmore), which was held mainly in Arthur C. Cope's room. Also attending were Henry F. Johnstone, Carl B. Marquand, and Jonathan W. Williams. We reviewed the "Special Report of Non-Resident Research and Development 1946-1951 (CRLR-40)" and concluded that the Chemical Corps should be commended for preparing the report with its critical evaluation of the productivity of each of its research and development contracts. Some of our comments included the question as to whether or not the Chemical Corps took advantage of these evaluations in determining where to place its current contracts and whether the Corps would be well advised to let a smaller number of large contracts rather than a large number of small contracts. We went over the advantages of a smaller number of large contracts, noting that this may present advantages when urgent jobs must be done rapidly although it did not guarantee success of the Chemical Corps research and development contract program. The committee was of the opinion that it could not help the Chemical Corps by suggesting other possible large contractors for the choice of contractor would vary according to the specific research and development job to be done.

For a while I attended a 2:30 p.m. meeting of the Council Policy Committee in the Foy Room of the Statler, presided over by Farrington Daniels (President of the Society and Chairman of the Committee). The following voting members attended: Henry E. Bent, Cecil L. Brown,

Farrington Daniels, Alden H. Emery, C. Harold Fisher, Harry L. Fisher, Arthur B. Hersberger, Wayne W. Hilty, A. L. Marshall, John H. Nair, Clifford F. Rassweiler, Lloyd H. Reyerson, and William G. Young. Non-voting members present were Herman S. Bloch, Wallace R. Brode, Carl F. Graham, William A. Pardee, Charles L. Thomas, and William von Fischer. I was present as a representative of Arthur Rose (Chairman, Committee on Publications) and Frank E. Brown represented Britton A. Shippy (Chairman, Committee on Membership Affairs). Elisha Hanson (Counsel) was present by invitation. The Committee, among other things, endorsed the provision of funds to Chemical Abstracts, authorized appointment of a committee to study the standardization activities of the ACS, voted that Clifford F. Rassweiler be re-elected Vice-Chairman of the Council Policy Committee, voted that the ACS nominate Randolph T. Major and Walter A. Schmidt for the Perkins Medal.

Henry C. Froula (Engineering Lecturer, UCLA) picked me up at 4:30 p.m. and drove me to UCLA, where I met Engineering Dean L. M. K. Boelter (an old Berkeley friend) in his office (Room 3066, Engineering Bldg.) at 5:15 p.m. to go on to have dinner. Then, from 7 to 9:30 p.m., I lectured (Room 2250, Chemistry and Geology Bldg.) on "The Actinide Elements and Nuclear Power." This was the talk for the University of California Extension Division series of lectures, "Modern Physics for the Engineer," about which L. N. Ridenour and I have corresponded. I discussed the discovery of the transuranium elements and the radioactive and fission properties of their important isotopes and correlated the chemical properties of the elements with atomic number greater than 89 as a rare-earth-like transition series. I then talked about the role of the heavy elements in atomic power and the problems to be solved, progress to date, and future prospects in the field. The lecture, followed by a question and answer periods, was well attended and well received.

I got back to the Statler in time to drop in the Pacific Ballroom to attend the ACS Meeting Mixer for a bit and to see some old friends.

Tuesday, March 17, 1953

In Los Angeles. Ed Orlemann and I had breakfast with James C. Warf (USC), who is considered an expert on hydrides, in order to discuss some of our ongoing problems.

At 9:30 a.m. the ACS Council Meeting, which I attended, was held in the Golden State Room of the Statler.

During the morning I received a telephone call from Eugene Burdick, who spoke to me about the Bay Area Education Television Association, Inc., "BAETA," which is trying to obtain VHF Channel 9 (San Francisco) as its outlet. Burdick said that both UC and the Ford Foundation are interested and that the Regents' Finance Committee has recommended putting the station in Dwinelle Hall. Burdick asked for my thoughts about this proposition and my suggestions as to what type of program on which we can help. He mentioned that he is also asking Wurster (William W.), Odegard (Peter H.), Stanley (Wendell M.), and Hicks (John D.) for their opinions. I reacted favorably.

At noon I went to a luncheon of the American Institute of Chemists

(Boston Room, Statler Hotel).

The Symposium on Chemistry of Certain Less Familiar Elements was presided over by James C. Warf in the Galeria Room of the Hotel Biltmore. At 2 p.m. I heard Earl Hyde speak on "The Present Status of Elements 85 and 87."

Then, at 3 p.m. I chaired the open meeting of the Council Committee on Publications (St. Louis Room, Statler).

At 5:15 p.m. I dropped in at the Social Hour, sponsored by the University of California (Cleveland Room, Statler). I ran into George Watt, who spoke about a number of young men, possibilities for our new Whitney program. He said that Robert E. McCoskey is not nearly so good as Harold I. Hahn; he is steady but unspectacular. We also again talked about Gregory R. Choppin, Watt's Ph.D. student, about whom he is most enthusiastic, and I told Watt to tell Choppin that we can offer him \$575/month to work with Stan Thompson.

Finally, I attended the 6:30 p.m. dinner meeting of the Editorial Board of the Journal of the American Chemical Society in the Boston Room (Statler).

Wednesday, March 18, 1953

In Los Angeles. Ed Orlemann and I talked for a while with Isadore Shapiro (Olin Mathieson Chemical Corp., Pasadena) about borohydrides. I looked up Philip W. West, who is being considered for a Boyd Professorship at Louisiana State University. We had a rather long discussion about his work, and I was quite impressed. When I saw G(lenford) H. Clewett (Oak Ridge), he asked about the degrees of enrichment the lithium for use in the Whitney program requires. L(lloyd) H. Reyerson (University of Minnesota) mentioned that he is working with Harold Stassen to bring 1000 foreign scientists to the United States. At 2:30 p.m. Ed Orlemann and I again met J. C. Warf in the Biltmore Ballroom to offer him a position as Head of Analytical Chemistry for Project Whitney.

At 4 p.m. I took a limousine from the Statler to the Burbank Airport, where I caught California Central Airlines Flight 55 at 5 p.m. I arrived at Inyokern at 5:40 p.m., where I was met by William B. Plum and Mr. McElwee (my contacts for the Naval Ordnance Test Station at China Lake) and had dinner with them. I spent the night at the Test Station.

Thursday, March 19, 1953

In Inyokern. After a morning spent touring the Naval Ordnance Test Station, I repeated my Monday Extension Division lecture, "The Actinide Elements and Nuclear Power," from 1:30 until 4 p.m. at the Michelson Laboratory of China Lake. Again the talk included a discussion period and was well received. At about 5 p.m. I left Inyokern by private car (I cancelled my return plane reservation) and was driven to my parents' home in South Gate (9237 San Antonio Ave.). I arrived about 9 p.m., looked over my mail, and telephoned my friend Clayton Sheldon to suggest that he arrange for a golf match on Saturday.

[In Berkeley, the research group met as usual with the following people in attendance: Carr, Clark, Conway, Cunningham, Dauben, Feay, Gray, Grover, Hoff, Hollander, Jaffe, Lessler, Levy, Passell, Perlman, Raby, Ruben, Smith, Stephens, and Templeton.

Conway gave a report on his work in atomic spectroscopy, saying that they have been measuring the variations in the intensities of certain lines in the atomic spectrum of plutonium as a function of temperature. He described his apparatus and said they looked at plutonium hydroxide from 2100°C to 2600°C. He explained that the Pu I lines are intensified at low temperatures and the Pu II lines get stronger with an increase in temperature while the Pu I lines get weaker. Perlman asked which lines show the greatest isotope shift and what transitions they represent. Conway's response was the lines at 4021.4 and 3958.8 Å and by analogy with uranium and thorium, these lines involve the 7s<sup>2</sup> electrons. He said they eventually will be able to pin down the exact transition involved by observing the Zeeman patterns and that the magnet for this experiment will arrive soon. There was discussion about how this isotope shift can be used to determine isotopic abundance, the size of the samples needed, etc. Conway added that the method will probably not replace the mass spectrograph but it does take less manpower and there are fewer difficulties in source preparation; thus it should be cheaper and faster for routine analysis.

Hoff reported additional work on Pu<sup>237</sup>, saying they found the expected 40-day half-life (after chemical separation) in a proton bombardment of Np<sup>237</sup> and in a helium ion bombardment of U<sup>235</sup>; in the first bombardment there was a long-lived (greater than 1-1/2 year) impurity, probably a fission product. Data from the second bombardment indicates a 70 kev gamma ray, an E<sub>1</sub> transition. He presented a possible decay scheme, noting that the 70 kev gamma ray does not fit the levels of Np<sup>237</sup> seen in the alpha decay of Am<sup>241</sup> and saying that, if the 71 kev level of the decay scheme were populated by K capture, one should presumably observe the 60 kev gamma ray (transition from the 71 kev to the 11 kev level) as in alpha decay. In the discussion Perlman said that this is an unusually simple spectrum where so many states are available for decay and that work should continue until the reason for the simplicity is found.

\* \* \* \* \*

Stephen Kahn, the young man for whom I had received a number of letters of recommendations, began working as a Chemist P-2, 3.2 with our group today. For personal reasons he has decided to delay graduate school. Presently he will be under Stan Thompson's supervision, processing coral from the Pacific in the Annex behind Bldg. 5.]

Stephen Kahn



Friday, March 20, 1953

In South Gate. The first thing I did this morning was to phone Doral to take care of a couple of items of business relating to the material Doral had forwarded. I read a memorandum from Dean H. E. Stone, who requested nominations for the Jake Gimbel award for 1952-53. [The prize is \$25.00 in cash and a gold medal "to the senior who, in his college activity, shall have represented the best attitude toward athletics in the University."] Stone also said that he wants to have a meeting of the Jake Gimbel Prize Committee on March 31 at 1:00 p.m. in his office, Room 201, Administration Bldg. I noted "will attend, no nomination to make" and returned the memo to Doral.

Before I left Berkeley, I had made arrangements to be picked up at my parents' home about 5:30 p.m.; however, I changed these arrangements and was picked up this morning and was driven to UCLA in time to join the 10 a.m. Charter Day Academic Procession at the 1st floor of the Administration Building. The Charter Day Ceremonies took place in Royce Hall Auditorium at 10:30 a.m. President Robert Gordon Sproul presided at Dr. Raymond B. Allen's inauguration as the first chancellor of UCLA. Chancellor Allen spoke on the "Prefectibility of Man." There were other speeches of welcome: Edward A. Dickson (Chairman, Board of Regents), Lee A. DuBridge (President of Cal Tech), and Warren C. Crowell (President of the UCLA Alumni Association).

I stayed on campus, where I learned that I was supposed to have a tux for tonight's programs. Fortunately, Crowell, who is about my size, offered me his second tux; I changed at his home and then rode with the Crowells to the Beverly Hills Hotel for a 6:30 p.m. reception in the Rodeo Room. The banquet, at which I was presented with the Regent Edward A. Dickson Achievement Award (Alumnus of the Year Award), was held in the Crystal Room. Following a few remarks by Warren Crowell (who presented the award and acted as toastmaster), I responded with:

I want to thank you, Mr. Crowell, and you fellow members of the UCLA Alumni Association. I am proud to be an alumnus of UCLA and a member of this organization and extremely happy to be given this special distinction this evening. I feel that this is more than a recognition of my own work but that it is more broadly a recognition of science in our great University. And I want to say here that the research in nuclear science and nuclear energy which has been carried on in our University owes its success in large measure to its enlightened administration under President Sproul and the Board of Regents with the strong backing of Governor Warren and our State Legislature. May I thank you again for the high honor you have bestowed upon me tonight.

The program included remarks by Clark Kerr (Chancellor of the University of California at Berkeley), by Raymond B. Allen (Chancellor of UCLA), and finally by President Robert G. Sproul. The Alumni Association also presented a portrait of Regent Dickson to the campus. Attendees at the banquet included such people as Mr. and Mrs. Brodie E. Ahlport, Mr. and Mrs. Edward W. Carter, Mr. and Mrs. Warren H. Crowell, Mr. and Mrs. W. Thomas Davis, Mr. and Mrs. Leo P. Delsasso, Mr. and Mrs. Edward A. Dickson, Mr. and Mrs. John B. Jackson, Donald H. McLaughlin, Mr. and Mrs.

Edwin W. Pauley, and Mr. and Mrs. Roy E. Simpson.

Again, I spent the night with my parents. It really has been a most satisfying day.

Saturday, March 21, 1953

In South Gate. After a leisurely breakfast with my parents, I went out to Hacienda Golf Club (La Habra Heights) with Clayton Sheldon for a game with Harry Board and Wilhemson (HB-85, W-91, GTS-98, CES-118). Wilhemson and I won the low ball-low total match, 8 and 6.

[In Berkeley, copies of MB-IP-740, dated March 19, 1953 and entitled "New Elements and Heavy Isotopes in Explosion Debris" by A. Ghiorso, S. G. Thompson, G. H. Higgins, and G. T. Seaborg, were mailed to Rod Spence. This is the summary of the Berkeley work, which Spence requested be sent to him by March 15 to be put together there with the work of other laboratories in a combined Ivy Report. It included our data showing that  $Cf^{253}$  decays by beta emission with an ~30-day half-life to  $99^{253}$  (6.6 Mev alpha particle, ~22 days) and that  $99^{255}$  decays by beta emission with an ~30-day half-life to  $100^{255}$  (7.1 Mev alpha particle, 16 hours). Data on californium, berkelium, curium, and americium activities were included, along with a curve of the elution data taken on March 1. The Summary and Conclusion section included the following:

The results of the work reported here show that a large number of new heavy isotopes were produced in the Ivy-Mike test.

Of particular interest is the fact that isotopes of elements 99 and 100 have been observed. The isotope of element 100 observed has a half-life of ~16 hours and decays by the emission of 7.1 Mev alpha particles. A tentative mass assignment of this isotope is  $100^{255}$ . Spontaneous fission events are also observed in a position roughly corresponding to the element 100 fractions which are possibly due to another isotope of element 100, but it is also possible that they are due to an isotope of even higher Z.

An isotope of element 99 emitting 6.6 Mev alpha particles and decaying with a half-life of ~22 days (probably  $99^{253}$ ) has been observed. An activity which is apparently due to the same isotope is observed to grow into the californium fraction and its beta-emitting californium parent, probably  $Cf^{253}$ , has a half-life of about one month. Another isotope of element 99, decaying by beta-particle emission with a half-life of approximately a month, is observed through its 16-hour 7.1 Mev alpha-emitting element 100 daughter (tentative mass number assignment 255).

Other alpha particles in the transcalifornium fractions differing in energy from 6.6 Mev by more than 0.2 Mev could not have been present with an intensity as much as 5% of that of the 6.6 Mev peak at about six weeks after the time of the explosion; at about ten weeks there were no alpha particles differing by more than 0.2 Mev from 6.6 or 7.1 Mev present to an intensity as great as 2% of that of the 6.6 Mev peak. Two alpha-emitting californium isotopes were observed, one of the 6.1 Mev alpha energy (probably  $Cf^{249}$  or  $Cf^{252}$ ) and the

other of 5.8 Mev alpha energy (possibly Cf<sup>250</sup> or Cf<sup>251</sup>). The latter isotope has been found to grow in the separated berkelium fraction and would be the daughter of long-lived Bk<sup>250</sup> or Bk<sup>251</sup>. A third californium isotope known to be present is the beta emitter of ~1 month half-life whose daughter has about the same properties as those of the 6.6 Mev alpha-emitting isotope of 99 (tentative mass number 253). If there is only one 6.6 Mev 99 alpha activity, it is necessary to postulate the existence of a short-lived isomer of Cf<sup>253</sup> to account for the relatively higher original yield of the 6.6 Mev alpha activity. Also observed in the californium fraction is an isotope decaying by spontaneous fission with a half-life of about two months. Its assignment to Cf<sup>256</sup> is postulated.

A beta-emitting isotope of berkelium of half-life longer than one month is observed. The suggested mass assignment is 250 or 251.

Two alpha-emitting curium isotopes of long half-life, probably Cm<sup>245</sup> and Cm<sup>246</sup>, are observed. The half-life of Cm<sup>245</sup> determined in these experiments is entirely inconsistent with that determined in previous experiments in this laboratory using cyclotron-produced materials. The reason for the discrepancy is not known at this time.

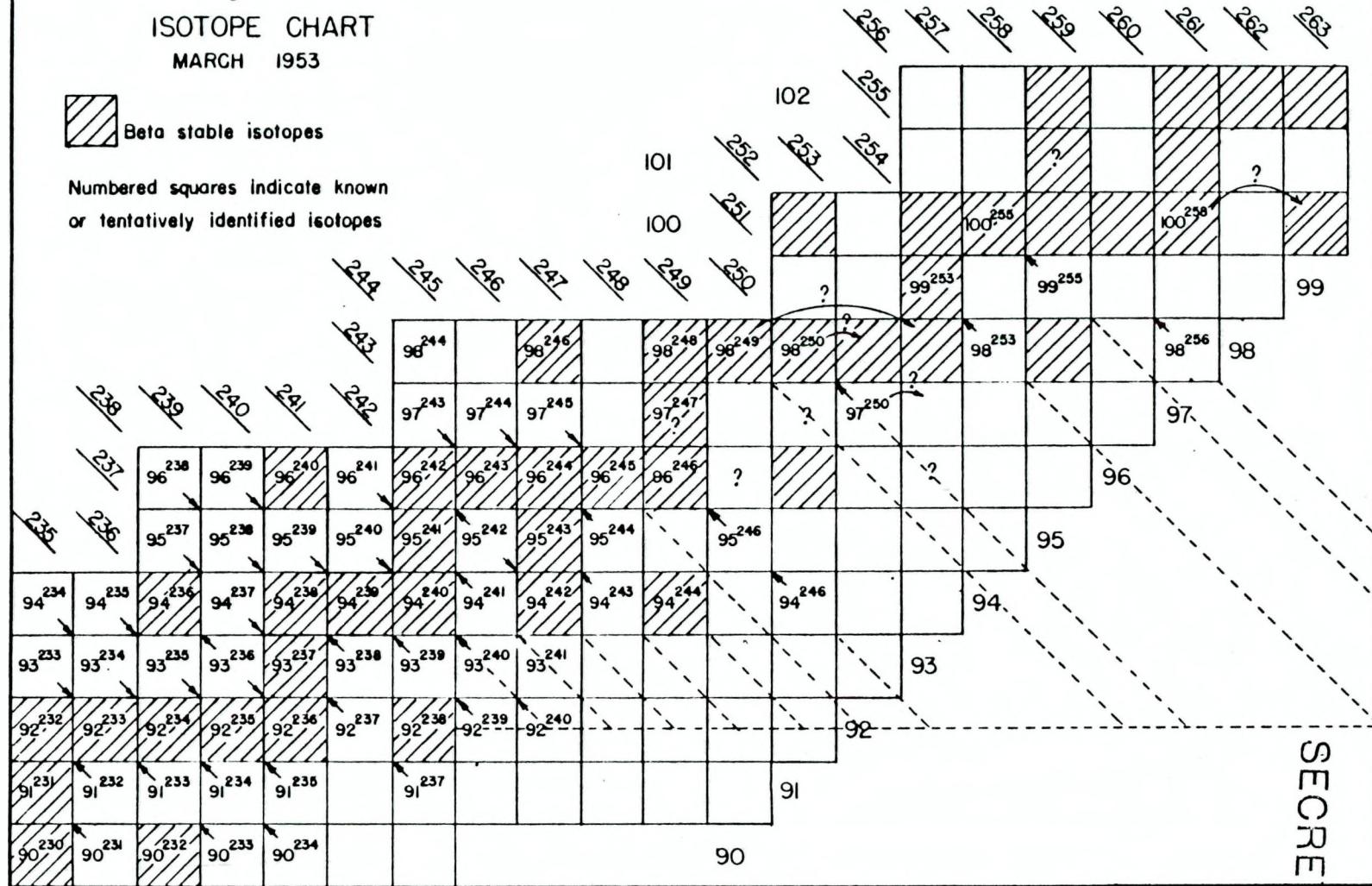
Some of the new heavy isotopes show relatively high spontaneous fission rates and are useful in extending the systematics of spontaneous fission....

Also appended is a copy of Figure 17 that summarizes the data in the form of an Isotope Chart.

Fig. 17  
ISOTOPE CHART  
MARCH 1953

 Beta stable isotopes

Numbered squares indicate known  
or tentatively identified isotopes



TOP SECRET

Spence was sent, in addition to his own copy, copies for D. L. Northrup, W. M. Manning, and Spof English. Other copies were distributed, besides to our own people, to Herb York and Ed McMillan.]

Sunday, March 22, 1953

In South Gate. I spent the day with my parents and friends. I also looked over the speaking copy for my Riverside Charter Day Address, "Dawn of the Atomic Age." This is my first major speech on a sociological, non-scientific topic, and I have worked hard on the text.

Monday, March 23, 1953

In South Gate. I travelled by bus and taxi to UCLA in Westwood to spend the day visiting with my friends in the Department of Chemistry. Among those I spoke with were Saul Winstein, (J.) Blaine Ramsey, William R. Crowell, Bill (William G.) Young, Francis E. Blacet, Jim (James D.) McCullough, Bill (William G., Jr.) McMillan, Tom (Thomas L.) Jacobs, Ted (Theodore A.) Geissman, and Clifford S. Garner.

Saul took me home to have dinner with him, Sylvia, and their children Bruce and Carolee. Later he drove me back to South Gate, where I received a telephone call from Helen, informing me that her mother, Iva Griggs, died today. This is a sad event but not unexpected.

Tuesday, March 24, 1953

In South Gate. This morning I again went over my Charter Day speech .

About 3 p.m., I was picked up and driven to the Mission Inn in Riverside for the annual Charter Banquet. We arrived at 6 p.m., and the program, this year honoring the 85th Anniversary of the University of California, began at 7:00 p.m. and was presided over by the Honorable John G. Gabbert (Judge of the Superior Court) and arranged by Provost Gordon S. Watkins. After the usual type of preliminaries--invocation, dinner, etc.-- Provost Watkins spoke first on "The Riverside Campus: Progress Report," and then I was introduced. I spoke to about 300 people on "Dawn of the Atomic Age." Although I covered briefly atomic energy, both pros and cons, the main emphasis of my talk was on America's need for more scientists and more understanding of science by the man in the street. I told the audience that students are presently shying away from scientific courses and thus causing a serious shortage of scientists at the time of history's most scientific age. I reported that, in this country, there will be only 40% as many people receiving the bachelor's degree in chemistry in 1954 as received the same degree in 1950 while the Soviet Union estimates that the number of chemists in that country will double during the same period. I concluded with:

The scientist is busily engaged in exploiting the tracer techniques in the solution of a myriad of basic scientific and technological problems and in converting the dream of atomic power to reality. In due course we can expect a host of substantial benefits as a result of his labors and indeed many have already appeared.

At the same time, because of his love of the free society in which he

lives and works, he is glad to throw his technical skill into the struggle to keep this country as strong militarily as he can. He may do this uneasily, knowing better than anyone else how deadly his weapons are and how illusory may be the security which atomic supremacy seems to offer. He cannot provide the answers to the political dilemmas facing us but he can help his fellow citizens to understand the basic facts which give rise to his fears and which cannot be ignored. Let us hope that in exploring the paths out of the wilderness we inhabit today we will act from a reasonable appreciation of these facts and not from a smug unwarranted feeling of impregnable military superiority nor from panicky unreasoning fear. Then perhaps we can look back with gratitude rather than regret to the dawn of the atomic age.

The program concluded with the singing of the Alma Mater and picture taking.



Judge John G. Gabbert, Glenn T. Seaborg, Dr. Gordon S. Watkins

Wednesday, March 25, 1953

In Riverside. I had a very comfortable night at the famous Mission Inn (Helen and I spent a holiday here a few years ago). Dr. Robert D. Huntoon took me on a tour of the Corona Labs of the National Bureau of Standards during the morning, and at 3 p.m. I spoke on "The Transuranium Elements."

Later I was interviewed by Joe Wimer, Sports Editor of the Riverside Press-Enterprise, about my job as Berkeley's Faculty Athletic Representative to the Pacific Coast Conference, my interest in sports, my days at UCLA when I saw Coach Bill Spaulding's team win its first PCC game (against Montana) and the first UCLA-Cal game (0-0 deadlock). I told Wimer that I favor the Rose Bowl game and also that I love golf. Wimer showed me a picture of myself, taken several years ago and which I have never seen, standing in line with Bill Dauben to get Rose Bowl tickets.



Glenn Seaborg (center, dark sweater) and Bill Dauben (center with hat)

At the same time last fall that Gordon Watkins approached me about the Charter Day talk, he first asked me to speak to The Present Day Club, a men's civic forum. This evening I gave a well-received talk to the group, which met at the Riverside Woman's Club (4092 10th Street at Walnut). The talk, entitled "Science and the Atomic Age," was followed by questions and lively discussion. Later I was driven back to South Gate.

[In Berkeley, Edward Shannon Clark, Jr., Templeton's research student, passed his prelims. His committee consisted of George C. Pimentel, William D. Gwinn, Donald S. Noyce, Theodore Vermeulen, Walter D. Knight, and Stanley G. Thompson.]

Thursday, March 26, 1953

Since I was driven back to South Gate last evening, I was able to catch an early flight (United Flight 659, 9 a.m.) to San Francisco, arriving at 10:40 a.m. A lab driver met me and drove me directly to the lab.

I talked with the fellows, learned about some of last week's visitors, and then tackled my mail.

Another long letter had arrived from John Rasmussen. John enclosed an abstract of the paper he will present at the International Congress in July and asked me some questions about it. He mentioned that Professor Manne Siegbahn left for the United States the first of this month and intends to visit Berkeley toward the end of his stay. John added that he had mentioned to Professor Siegbahn my suggestion of an exchange of one of their men with Stan. He described the work he is doing and the equipment the Swedes lack and that which is particularly good. John is a keen observer, and I think we were wise in encouraging him to take this trip.

I read a March 12 letter from F. A. Paneth, saying that he is sorry that I will not be able to participate in the Stockholm meeting. He wrote that he believes the meeting will be devoted to methods of comparison of radium standards, standards of artificial radionuclides, and questions of terminology and symbols. He mentioned that Otto Hahn believes that the group can come to an agreement on the matter of the placement of the mass number--the English have suggested the mass number be placed on the left hand top corner of an atomic symbol while, in our country, the right hand top corner seems to be almost universally used.

A March 20 letter had arrived from W. Kermit Anderson (Argonne), who expressed interest in learning more about the Whitney hydride work. Doral had already routed the letter to Perlman and Hyde and sent copies to Street and Orlemann.

There was also a six-page memorandum from Ed Orlemann for me to read on information he had obtained in Los Alamos (March 5-6) on hydride research.

I also received a letter from John Voelker, who said his article on me appeared in the Mining Journal on March 14 and that the newspaper is sending me six copies. Voelker apologized for the fact that the paper quoted from my last letter to him without permission, saying that he does not want me to think that he goes running around the country displaying our modest correspondence.

I noted a memorandum from Bill (William A.) Nierenberg about the University Extension Course, noting that this year the speakers are asked to prepare a two or three page outline to be distributed shortly before

the talk. Bill said he will telephone me two or three days before the lecture to check on dinner arrangements and lantern slides.

There was also a thank-you letter from Thomas R. P. Gibb, Jr. for our hospitality and a confirmation of his refusal of our employment offer. Doral had already sent copies of this to the interested parties.

W. E. Forsythe (Smithsonian Institution) sent me a copy of the table that I helped prepare for the new Edition of the Smithsonian Physical Tables. I routed it to the Hollanders for checking.

In addition, there was another letter of recommendation for Stephen Kahn (from Ludwig Rosenstein); again Doral had already sent a copy down to Dean Pitzer.

Although I was not present, the research group met as usual this morning with the following in attendance: Batzel, Carr, Clark, Cobble, Feay, Glass, Gunn, Grover, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Ruben, Smith, Stevenson, Surls, and Templeton.

Michel reported first on a new tube for the time-of-flight mass spectrometer. He described the tube and told of its advantages.

Levy spoke for some time on his continuation of Biller's work on the yield distribution between isomers. He is looking at the isomers of  $\text{Co}^{58}$ , produced by bombarding  $\text{Mn}^{55}$  with low energy helium ions and discussed the method of calculating the relative yields of the two isomers. Levy said that he does not have much data to report and that he has had some experimental difficulty.

Perlman drew a partial decay scheme for  $\text{Np}^{238}$  that he explained was not proved but was consistent with information available at present. He then went on to explain why he believes that  $\text{Np}^{238}$  does not decay by electron capture to uranium. He explained that, if the ground state of  $\text{U}^{238}$  and the first excited state have the same parity as those for  $\text{Pu}^{238}$ , the decay of  $\text{Np}^{238}$  to the lower states of  $\text{U}^{238}$  would involve a change in parity and presumably also be second forbidden. Perlman said that the decay to the higher levels is ruled out because there is not enough energy available; consequently, the log ft value for this transition is about 9, which would correspond to a very long half-life.

\* \* \* \* \*

Friday, March 27, 1953

This morning I had a number of conferences and phone calls and then took care of some of my correspondence.

One of the first things I did was to send my revised editorial for Reactor Science & Technology to Frank R. Ward, stating that the deletions and changes he suggested in his letter of February 4 have been made on pages 2 and 3.

On March 6 Dean Charles E. Smith (Louisiana State University) asked

for an evaluation of Philip W. West, who has been nominated for a Boyd Professorship. I talked with some of my colleagues in the Department and then met with West while I was in Los Angeles. Today I wrote, explaining my reply was delayed because of an extended absence from Berkeley and then said that my opinion is that Dr. West is worthy of this honor. I wrote that I am acquainted with him and am aware of his work in a general way and know that he has been very productive of significant research papers on a diversity of topics within the field of analytical chemistry. I went on to add that my associates at the University of California who are specialists in analytical chemistry assure me that his professional stature is quite high.

I looked over a report that Earl Hyde prepared for me to cover an informal meeting on March 25 about the budget proposal aspects of our proposed linear accelerator for heavy ions up to  $C^{3+}$  and  $Ne^{6+}$  with energies of 10 Mev per nucleon. Hyde said others in attendance were Chris Andresson, W. B. Reynolds, Alton Wilson, Cornelius A. Tobias, Craig Nunan, Bob E. Watt, and one other. Hyde reported that Nunan gave some technical details and a guess as to cost (\$340,000). The group discussed sites. Hyde then wrote that we should (1) supply Andresson with justification material, (2) decide on our requirements about location, (3) supply Nunan with a better idea about how much space we need in the building for counting equipment, etc. and (4) decide if we want to devote manpower to help Nunan with the ion source problems. A preliminary proposal will be made in April to the AEC. Toward the end of this year a rather complete description would have to be ready for the Bureau of the Budget. Congressional consideration would have to come in the spring of 1954 to have funds available by fiscal year 1955 (July 1, 1954-June 30, 1955).

I spent some time in the labs checking on the research and talking with various people. I was particularly interested in watching the work of Stan Thompson's men, who are processing more of the coral from the Mike explosion.

After lunch I went down to campus for the 2:15 p.m. prelim of James S. Kane in Room 202, Lewis Hall. His committee was composed of W. M. Latimer (chairman), Charles W. Tobias, George C. Pimentel, Burton J. Moyer, Abraham Seidenberg, and me. Kane defended the propositions: (Major) "It is proposed that hydrogen bonding is responsible for the extraction of certain metal ions into organic solvents" and (Minor) "An explanation is proposed for a discrepancy in the transference number of sodium ion in sodium silicate glasses when calculated from self diffusion coefficients or by electrical conductivity measurements." Kane, who is one of Leo Brewer's graduate students, did very well.

#### Saturday, March 28, 1953

I caught up with some of my journal reading today and also played with the kids.

#### Sunday, March 29, 1953

Again I read, played with the kids, etc.

Monday, March 30, 1953

Before talking with Dan Miller and George Kavanagh, who are visiting the lab today, I signed a rather detailed, classified response, which Earl prepared, to W. Kermit Anderson's request for more information on the type of program in hydride chemistry we are contemplating at Livermore. Earl explained about our plans for the chemistry group there and then described our interest in hydride chemistry, explaining that many of the problems are not treated adequately in the available scientific literature. The letter went on to state that, if he is interested in such a program, I will have our personnel office send him a formal invitation to travel to Berkeley for an interview.

I also signed a memorandum to George Everson to inform him that we should like him to make a formal offer of employment to Gregory Choppin, who is highly recommended by Professor George Watt, whose judgment we value. We want Choppin to work with Dr. Stanley Thompson on our heavy element program. I explained that Choppin has already had several offers from industry, the lowest of which was \$600/month, and offers for academic positions from places such as the University of Illinois, and we therefore want Everson to offer him the somewhat high salary of \$575/month.

Last year when I was at Argonne (and later here in Berkeley) Dr. M. D'hont of Belgium talked with me about sending some Belgian scientists here for training in radiochemistry, and today Professor Raymond Brecktot and M. Pierre C. DeMeere, also Belgian, stopped up at the lab to discuss the same matter. I talked with them in Bldg. 4 for a bit, but we reached no decision. Earl took them to the cafeteria for lunch and then showed them a couple of the labs in Bldg. 50 while I talked with Miller and Kavanagh.

After our noontime senior staff meeting, I went out to Mira Vista with Luis Alvarez and Al Ghiorso (LWA-98, AG-95, GTS-90). My travels do not seem to have adversely affected my golf game.

When I returned to my office, I learned that Harold Jaffe passed his prelims today. Jaffe's committee was composed of Robert E. Connick (Chairman), David H. Templeton, George C. Pimentel, Abraham Seidenberg, Theodore Vermeulen, and Arthur F. Kip.

Tuesday, March 31, 1953

There were a few phone calls, including one to J. S. Norton to discuss problems in electronics, then I talked with some of the fellows and took care of some mail.

On March 18 Ron Brightsen wrote another letter, in which he said that he will know in six to eight weeks whether he will be able to manage at Berkeley financially. He also asked if he could have about an hour of time at the next Gordon Conference to speak on periodicity in beta stable nuclides and the limits of beta stability for both odd and even A. I had heard in Los Angeles that Brightsen may receive a Westinghouse fellowship. Before going to campus to give the Chemistry 223 lecture, I dropped him a note, saying that since there seems to be a fair chance

that his financial problem may be solved by a Westinghouse fellowship, perhaps we should take some steps towards clearing his admittance to the Graduate School. I asked for a transcript of his college work, which will be passed upon by Dean K. S. Pitzer of the Chemistry Department

In today's mail was an acceptance letter to my offer of employment to A. Chetham-Strode, Jr. (from Hanford). I told Doral to show the letter to Iz and Earl and to make a copy for Dean Pitzer and George Everson.

After lunch I went to a meeting of the Jake Gimbel Prize Committee, which met in Dean Stone's office in the Administration Building.

About 3:30 p.m. Vance Cooper telephoned from Richland with the names of a couple of prospective employees for Livermore: J. L. Schwennesen and Al Katzer. Vance described the men, saying that neither has a Ph.D., but he rated Katzer the better. He also told me that he is not interested in a position with Livermore himself, but he did add that he plans to come through Berkeley in September.

Wednesday, April 1, 1953

This morning I telephoned Steve Lawroski at Argonne, chatted, and then told him that we want to contact Feder (Harold M.) about working on Project Whitney. Steve said that he would have Feder call me back, mentioning first that Feder is leaving for England next week. A short time later Feder called back and said that he is interested in the position (possible head of the chemical engineering group). He reported that he is leaving for England on April 10 but will be back on April 22 and is willing to visit us on the week of April 27.

In the meantime I began dictating a number of letters. I wrote to Dr. Donald M. Wroughton (Manager, Chemistry Subdivision, Materials Department, Atomic Power Division, Westinghouse, Pittsburgh) to give him my appraisal of Ron Brightsen, who is under consideration for special Westinghouse fellowship support so that he can return to graduate school. I spoke of Brightsen's work and said that he can already be regarded as a scientist of stature. I then went on to state that there is a critical shortage of nuclear scientists at the doctorate level in this country and that I do not believe that we should overlook the opportunity to add a man of Mr. Brightsen's ability to this group.

I replied to John Rasmussen's letter of March 20, saying that I think the title and abstract of the paper for the International Congress covers the situation very well. In response to another question John posed, I said that the optical spectroscopists have not carried through any analyses of the neptunium and plutonium spectra as was done for uranium and therefore the ground states for these gaseous atoms have not yet been determined by this method. I added that, with respect to the aqueous ions of uranium, neptunium, and plutonium, there is the mass of data on magnetic susceptibilities covered to some extent in my Nucleonics article, which I hope to revise. I wrote that we shall be interested in his results on  $\text{Np}^{238}$ , which Frank Asaro is studying, and that Fred Reynolds would like to obtain detailed drawings and design data on the Curt Mileikowsky spectrometer. Finally, I added that we are looking forward to seeing Professor Siegbahn when he visits here.

I thanked J. Robert Oppenheimer for the copy of his talk to the Council on Foreign Relations, noting that it arrived shortly before I was scheduled to embark on a similar venture in connection with the University of California Charter Day observances on the Riverside campus. I said that I took the liberty of working one or two of his thoughts into my material; I sent him a copy of my rough draft, noting that it served only as an outline since I did not follow it verbatim.

Then, I returned Dr. Harold G. Cassidy's manuscript to him at Yale and thanked him for the loan. I also sent him a draft copy of my Riverside Charter Day address, again adding that I didn't follow the manuscript verbatim.

To John D. Voelker (Ishpeming), I said that I was interested to see his article in the Mining Journal and that I have received several copies from various sources. I added that I don't think that he should worry about the quote from my letter for it is clear to me how it came about; however, I added, it does illustrate that one has to be careful lest

similar incidents occur in which the outcome could be less happy. Finally, I wrote that I was surprised to learn that his wife is joining the AEC family (which Voelker had mentioned in his letter) and that there is going to be a regional office in Ishpeming.

I also returned the proof for the table in the Smithsonian Physical Tables to W. E. Forysthe (Cleveland), noting that there are a few corrections marked in green pencil and suggesting that he check the atomic weights against the latest values adopted by the Commission on Atomic Weights as given in J. Am. Chem. Soc. 74, 2447 (1952).

A nice note arrived from Bill (W. H.) Johnston (Purdue University), who visited the lab after the ACS convention in Los Angeles. Johnston said he enjoyed the talks with our men, the tour of the installation and is looking forward to my visit next fall.

I also received a thank-you letter from Provost Gordon S. Watkins (Riverside) for my Charter Day address and my talk before The Present Day Club.

I then spent some time reading reports and journals, but I also made the rounds of the labs to look in on the research. Some of the chemists (and health chemists) are involved with the processing of Pay Dirt (a 1000-pound sample of coral from the Pacific "Mike" thermonuclear test) in a shack, called Bldg. 5 Annex, behind Bldg. 5.

I also learned that my student, Hirdaya Behari Mathur, passed his preliminary examination today with the comment "good." His committee was composed of Donald S. McClure, Wendell M. Latimer, Charles W. Tobias, Raymond H. Sciobereti, Arthur F. Kip, and David H. Templeton (absent).

Thursday, April 2, 1953

This "first of the month" meeting of the research group was attended by Asaro, Behman, Carniglia, Carr, Clark, Cobble, Cunningham, Glass, Gunn, Grover, Gray, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Lessler, Levy, Nervik, Passell, Perlman, Ramaniah, Ruben, Seaborg, Slater, Smith, Stephens, and Wallmann.

Gunn gave a detailed report on his study of the reaction:  $\text{Am}^{0+} + 4\text{H}^+ + 2\text{Fe}^{++} = \text{Am}^{+++} + 2\text{Fe}^{+++} + 2\text{H}_2\text{O}$  in 1 M perchloric acid. Gunn estimates his accuracy on the order of 1% and gave a preliminary oxidation potential determination of -1.79 v, which may be compared with an estimate of -1.72 v. There was some discussion about the work, and then Gunn went on to show a diagram of the setup they will use for future studies with americium.

Nervik talked about the decay scheme of  $\text{Tm}^{167}$  (9.6 days), which he milked from  $\text{Yb}^{167}$ . The  $\text{Tm}^{169}$  was placed in a beta spectrometer and counted, then "swept" with a scintillation counter, which revealed five gamma rays instead of two: 49 kev (relative abundance 1), 115 kev (0.02), 202 kev (0.29), 515 kev (0.09), and 720 kev (0.18). He found two conversion electrons--144.6 kev K electron conversion and 193.0 kev L electron conversion from the 202 kev gamma ray. Kalkstein suggested, and Gunn agreed, that the peak at 115 kev may be the Compton peak of the 202

kev gamma ray.

Passell spoke briefly on the proposed construction of a twin lens spectrometer, saying that the use of more compact ironclad thin lenses is being considered in spite of hysteresis effects of iron.

Jaffe told the group about a new method developed by Health Chemistry to mount samples for the bent crystal spectrometer in order to cut down on radiation hazard.

Slater presented cross section values for the reaction  $Mn^{55}(d,p)Mn^{56}$ : 50 Mev = 11 mb, 100 Mev = 3.2 mb, 150 Mev = 1.2 mb, 190 Mev = 0.6 mb. He said the slope of the high energy excitation function is about the same as for the corresponding reaction on  $Pd^{110}$ .

Hollander discussed a method for estimating electron capture disintegration energies from a knowledge of the  $EC/\beta^-$  branching ratio and compared the results using this method with the results from closed cycle determinations for  $Pa^{230}$ ,  $Np^{236}$ , and  $Am^{242m}$ .

\* \* \* \* \*

After the Chemistry 223 lecture (on photoelectric effect, Compton effect, pair production), I stopped in at the Department of Chemistry office to talk with Miss Kittredge and others.

Then, at 1:30 p.m., I had a meeting of the committee appointed by President Robert Gordon Sproul to consider whether the campus at LaJolla should be expanded to a full-fledged campus status (the Seaborg Committee).

In today's mail was a March 30 letter from Ed Westrum with ten copies and a ditto master of the paper "The Heat of Formation of Thorium Sesquisulfide" by Eyring and Westrum, which he wants submitted for declassification and publication in the Journal of the American Chemical Society. Westrum noted that the final values in Reference 9 have already been declassified and said that, unless advised to the contrary, he will assume that the reprint order should still be placed. He said that their low temperature program at the University of Michigan is going full blast and that he thinks they have turned out a tremendous quantity of sound science in the last nine months. Finally, he added that he had a pleasant visit at Berkeley and UCRL last week.

Friday, April 3, 1953

Helen and I have been generally satisfied with our home in Lafayette, but we have had problems with some of the construction and have had difficulties getting our contractor, Hans Rodde, to correct them. Presently, the roof over our master bedroom is leaking, and even though the contract reads that the roof shall be maintained in a watertight condition for a period of two years from the date of completion (July 2, 1951), we have had little success in having this repaired. I have had a number of conversations with Bill Rice (our architect); Wednesday afternoon I telephoned Rice and asked for the address of the State Board of Contractors, which he gave me--Registrar of Contractors, Contractors

State License Board, Sacramento. This morning I wrote the Board, describing in detail the problems and concluding with:

Although I feel that the money involved is of importance, I am coming to the view that of perhaps even greater importance is my duty as a citizen to take such steps as I can to see that this doesn't happen to someone less able to afford it. I am writing to you before taking legal action and before contacting other organizations and regulatory agencies who might be interested with the additional thought that you can ascertain whether I have misunderstood Mr. Rodde and Mr. Padelford's [the roofing subcontractor] attitude. I am wondering whether, and a number of my friends definitely feel that, a matter of this sort should be followed through to the end, however much time and effort this might take, in order that a small contribution to the principle of ethics in the building business might be made.

Copies of the letter were sent to C. R. Padelford, W. M. Rice, and H. Rodde.

I returned corrected galley proofs for Chapters 11 ("Nuclear Properties of the Neptunium Isotopes" by G. T. Seaborg), 13 ("Nuclear Properties of the Transplutonium Nuclides" by G. T. Seaborg), and 14 ("The Chemistry of the Transplutonium Elements" by I. Perlman and K. Street, Jr.) of PPR, Volume 14A to Joe Katz, explaining that, if these don't move into production within about a month, we shall want Chapters 11 and 13 back for further revision. I also said that we shall need to see page proof on these chapters and possibly on Chapters 7, 17, and 20, if these require a similar number of changes in the galley stage.

A memorandum went to Donald Cooksey, describing the visit of Professor Raymond Brecktot and Pierre C. DeMeere to our group on Monday, March 30.

I filled out and signed a support form for Asher Jacob Margolis' application for membership in the American Institute of Chemists. My generally favorable comments were similar to others I have written for Margolis, whom I have known since he worked in my Chemistry Section at the wartime Met Lab.

Then I telephoned and talked with Wallace Hamilton of Berkeley's listener-sponsored radio station, KPFA, about arrangements being made for having my Extension Division talk on April 6 recorded for later broadcast over their station.

Jim Cobble stopped in my office and gave me a copy of a letter he had written to Dick Diamond on a slightly different treatment of Diamond's data on evidence for "f" electron bonding.

Saturday, April 4, 1953

The kids took some attention today, but I also spent some time in my study reading journals, etc.

Sunday, April 5, 1953 (Easter)

The kids were excited and pleased with their Easter egg hunt this morning.

I again worked in my study--I am scheduled for two talks this coming week, a repeat of my Extension Division lecture and one to the Golden Gate Paint, Varnish and Lacquer Association, arranged by my Met Lab friend Bart Sutton.

Monday, April 6, 1953

This was a rather busy morning with numerous phone calls and meetings. I called Henry Taube about prospective Project Whitney personnel and talked with Ernest Lawrence and Luis Alvarez about problems of locating people for the Whitney Project. I also went to a meeting with George Everson about graduate student salaries, which I try to keep increasing.

A March 30 letter of appreciation arrived today from Tom Jones (Haverford College) for my acceptance of his invitation to speak there. Jones reported that Fermi agreed to speak during November and thanked me for suggestions for other visitors, adding that the high cost of transportation limits the number of people they can bring from the West Coast.

Charles E. Smith (Louisiana State University) thanked me for my appraisal of Philip W. West.

I also received a letter from W. Kermit Anderson (Argonne), expressing interest in the work I recently described (Project Whitney) and suggesting that, if my organization desires a personal interview, this would now be in order.

The senior staff had its usual meeting in my office at lunchtime; one of the subjects discussed was the processing of the coral in the Annex behind Bldg. 5.

After dinner I repeated my Extension Division talk, "The Actinide Elements and Nuclear Power," on the Berkeley campus. This time the talk was recorded by radio station KPFA for later broadcast. Others in this series of weekly talks include Edwin McMillan, Wolfgang Panofsky, Alvin Weinberg, David T. Griggs, Simon Ramo, Edward Teller, Frederick Seitz, and Charles Kittel.

Tuesday, April 7, 1953

After the usual phone calls and conversations with some of the fellows, I went down to campus and lectured to the Chemistry 223 class on alpha decay. I then stopped in at the Department of Chemistry office, where Pitzer told me about a new graduate student, an Air Force man, Lt. Mitchell Garth Florence.

Also today I attended a meeting of the faculty of the College of Chemistry.

In addition to attending to the routine matters, I worked over some material for my talk Friday noon to the Golden Gate Paint, Varnish and Lacquer Association.

Wednesday, April 8, 1953

There were the usual phone calls, etc. this morning. Doral took care of the paper work involved with the paper Ed Westrum sent recently--it has to be sent to Oak Ridge for declassification. I signed a letter to Ed, telling him about this and also explaining that there are deliberations going on between our Information Division and our Purchasing Department about reprint order numbers, which will be solved by the time we hear about declassification. We will inform him about this then.

Then, at 10 a.m. I met with Professor Manne Siegbahn, who has been visiting this country for a couple of months. We talked about a number of matters, including John Rasmussen's present visit in Sweden. Ingmar Bergström will probably visit our laboratory for six months soon, and I mentioned that I would like to arrange some sort of exchange so that Stan Thompson can spend some time at Siegbahn's laboratory.

At 4 p.m. I attended a reception for Chancellor Clark Kerr; this was followed by a dinner at the Men's Faculty Club.

Thursday, April 9, 1953

The research group meeting this morning was attended by Behman, Carniglia, Carr, Cobble, Cunningham, Dauben, Glass, Gray, Grover, Gunn, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Ruben, Seaborg, Smith, Stephens, and Templeton.

Hoff reported on the study of the 70 kev radiation found in the gamma spectrum of  $Pu^{237}$ , which has been suggested as being the "escape" peak of the K x-rays, if not an actual gamma ray. They found, using the scintillation spectrometer, that the 60 kev gamma ray from  $Am^{241}$  is not the same as the radiation in the 70 kev region of  $Pu^{237}$  (both of which decay to  $Np^{237}$ ). Hoff said that K x-rays and the 70 kev radiation are observed in the decay of  $Pu^{234}$  and  $Bk^{245}$ ; the 70 kev radiation has been assumed to be the x-ray escape peak since  $Pu^{234}$  is known not to have any 70 kev gamma ray. In the  $Bk^{245}$  decay, Hoff said, the escape peak is about 40% of the intensity of the K x-ray; since the  $Pu^{237}$  was observed on the same crystal, they expected the same percentage abundance. However, the 70 kev radiation was seen in 70% abundance, which probably means that there is some 60 kev radiation not resolved from the escape peak. There was considerable discussion about the interpretation of this.

Gunn said that he made four calorimeter runs involving the  $Fe^{++} \rightarrow Fe^{+++}$  couple and determined the heat of solution,  $\Delta H$ , to be 91.4 kcal/mole for the reaction  $Am^{+++} + 2H_2O = AmO_2^+ + H_2 + 2H^+$ . Using published and estimated  $\Delta S$  values, they found  $E_f = -1.78$  volts (Latimer's value is  $E_f = -1.72$  volts, which Cunningham said they question). In response to my query, Gunn discussed the potential scheme for  $Am^0 \rightarrow Am^{+++} \rightarrow Am^{+++} \rightarrow Am^+O_2 \rightarrow Am^{++}O_2$ .

Hulet described some new fast chemistry for berkelium, in which he oxidizes the berkelium with sodium bismuthate and extracts with tri-n-butyl-phosphate, resulting in a quantitative extraction of all  $Bk^{++++}$  from 1 M nitric acid. He added that Peter Gray found the distribution ratio of americium tracer between 1 M nitric acid and tri-n-butyl phosphate as TBP/water =  $\sim 0.15$ , but Hulet found this ratio to be 1.0, adding that his ratio is very likely too high because of a plutonium impurity. Hulet also said that he makes a fluoride precipitation before the extraction to isolate only rare earths and actinides; citrate elution from Dowex 50 ammonium form resin is made after the extraction to separate the remaining americium. He is developing this fast chemistry to look for  $Bk^{246}$  from the helium ion bombardment of  $Am^{243}$ .

Michel again reported on his two-run, time-of-flight mass spectrometer study, including yields, of the thulium fraction from the high energy spallation of tantalum. They observed 9.6-day  $Tm^{167}$ , 7.7-hour  $Tm^{166}$ , and  $\sim 29$ -hour  $Tm^{165}$  (this was previously unreported). Michel said that the mass number 166 activity has a complex decay curve which may be due to the 60-hour  $Yb^{166}$  parent as well as  $Tm^{166}$ . They ran the erbium fraction without much success and the ytterbium fraction: 32-day  $Yb^{169}$  (745 c/m) and 60-hour  $Yb^{166}$  (1,140 c/m), adding that they are surprised that they made so much of the 32-day isotope. There was discussion about the ionization efficiency.

\* \* \* \* \*

After the Chemistry 223 lecture (again on alpha decay), I stopped in the Department office before going up to the hill.

I signed the usual letter about AEC clearances to the prospective new Air Force graduate student, Lt. Mitchell Garth Florence. I asked that he inform us when his plans are complete so that we may know when to expect him.

Then, I read an April 6 letter from Ernest H. Wakefield (President, Radiation Counter Laboratories, Inc., Skokie, Illinois), saying that he plans to visit the University on Tuesday, April 14. He has written Dean Pitzer about this and would be pleased to take me to luncheon on that date. This, he said, will be the first trip to California for both him and Mrs. Wakefield.

I checked with Stan and Al about their work and then telephoned Rod Spence, who reported that Charlie Browne is weaving the Argonne, Los Alamos, and Rad Lab reports into one coherent Ivy report under a combined authorship (the title page has names in alphabetical authorship, segregated into three groups, one for each lab). Browne may use some verbatim versions and expects to complete the job in one to two weeks. Spence suggested that, after the Ivy version, possibly each laboratory should prepare and issue secret, project-wide versions and then later declassifiable versions can be prepared. I told Spence about  $Cf^{250}$  (60,000 year spontaneous fission half-life and daughter of  $Bk^{250}$ ) and our idea that the high spontaneous fission rate in californium may be a singularity. Spence said that Argonne is still sticking to their version about the new elements, i.e., that there is a 15-day, 6.6 Mev element 100

alpha emitter. I also reported that Nelson Garden's group had to repack Argonne's portion of the coral and that Nels is very annoyed that Argonne did not send a truck to pick it up. I added that we may still be interested in a bigger commercial-type operation of processing coral on a 10-ton scale. Finally, Spence reported that he has sent a copy of our report to Manning but not to English or Urry; however, Spof English has been told that Spence was given a copy of our report, which he can have for the asking. (The conversation lasted for about a half hour: 1:30 until 2 p.m.)

About 3:30 p.m. I met with Wally Reynolds to urge action on our budgetary request for construction of a heavy ion linear accelerator.

Back in my office I telephoned Wendell Latimer to tell him that I plan to nominate Burris Cunningham for the Fisher Award in Analytical Chemistry. Latimer is willing to write a seconding letter.

Friday, April 10, 1953

Wednesday I received our annual invitation from Stan McCaffrey for our family to be guests of the California Alumni Association at the Pinecrest Lair of the Golden Bear this coming summer. I checked with Helen and today returned the card accepting the invitation for the week of July 19. I also told Doral to phone McCaffrey's secretary to tell her that we will accept this year.

I returned PPR, Volume 14A, Chapter 6 ("The Chemistry of Uranium" by H. R. Hoekstra and J. J. Katz) to Katz today, saying that I think it is now in really good shape. I suggested that it is probably worthwhile to bring Table 6.1 (Radioactive Constants of Natural and Synthetic Uranium Isotopes) up to date through the use of the new Berkeley "Table of Isotopes" and then pointed out that, although it may not be too easy, the information in Chapter 3 ("Nuclear Properties of Uranium, Protactinium, and Thorium Isotopes" by L. I. Katzin) should be made consistent with the revised Table 6.1.

I also sent Donald Lane a copy of a revision of the University of California Regulation No. 23 concerning patent policy.

I looked over today's mail, which contained a letter from John Voelker, saying that he was relieved that I was not upset about the Mining Journal quoting from my letter to him. He also explained that the Ishpeming office of the AEC, which he mentioned in a previous letter, is a sub-office rather than a regional office. Finally, he added that I should let him know, when and if I come North, so that he can have some trout on hand for my enjoyment.

I also noted an April 3 letter from O. B. J. Fraser (Chairman, Advisory Group on Policy Membership Committee, American Institute of Chemists), describing plans for increasing the membership of the organization.

I left the lab in time to meet Bart Sutton in the lobby of the Palace Hotel about noon in order to go to a 12:15 p.m. luncheon meeting of The Golden Gate Paint, Varnish and Lacquer Association in the California

Room. I spoke to the group on "Industrial Atomic Energy." This was arranged some time ago by Bart of du Pont's San Francisco office.

Saturday, April 11, 1953

I worked on some writing projects in my study and played with the kids.

Sunday, April 12, 1953

I again read, wrote, and played with the kids. One project that has consumed some of my time recently is the reading of galley proof for chapters in Volume 14A (The Actinide Elements of the Plutonium Project Record.)

Monday, April 13, 1953

Again there were a number of phone calls the first thing this morning.

I returned the galley proof of PPR, Volume 14A, Chapter 7 ("Nuclear Properties of the Plutonium Isotopes" by G. T. Seaborg) to Joe Katz this morning and posed several questions in my covering letter. I suggested that perhaps Winston Manning would like to make a quick reading of it to make certain the Argonne work is covered correctly. In addition, I mentioned that I notice we have used a number of different values for the half-life of  $U^{240}$  throughout the book, said that probably the closest to the correct one is 14 hours, and suggested that it is probably worthwhile to standardize this in the rest of the book. Finally, I asked why the policy in the whole book isn't to indicate which reports in the references are unclassified or declassified.

While I was at the Los Angeles ACS meeting, I talked with Professor Jacob Kleinberg, not only about Project Whitney, but also about presenting a paper at the fall symposium of the Division of Chemical Education. Kleinberg wrote on April 2 to withdraw his name from consideration of a leadership position with Project Whitney. He also gave me details of the fall symposium, and today I wrote that I will present a talk entitled "Oxidation States of the Actinide Elements." I told him, however, that I will not be able to prepare a full paper for publication and, if this is a requirement, I will be glad to find him someone else to talk.

Daniel D. Cubicciotti (North American Aviation, Downey, California), who was an undergraduate student here at Berkeley before the war, a graduate student during the war (Ph.D., 1946), and who also worked on the Manhattan Project here during the war, wrote on April 8 that he would like to visit our laboratories to observe our techniques in high level alpha work. I wrote Dan that he is welcome to drop in anytime to talk with our people; however, there is nothing in the way of pile irradiations being worked up at present and it may be several months before a new run is started. In the meantime, I said, most of the equipment has been dismantled for decontamination. I added that Argonne also engages in considerable alpha work and might have techniques and equipment of interest to him.

At our noontime senior staff meeting I brought up a number of matters, including a letter from Professor Hermann Schlesinger of the University of Chicago requesting some information, the impending visit of Ingmar Bergström to our lab (which Manne Siegbahn mentioned), the question of distribution of the notes covering the meetings of the Argonne chemistry group, which we receive, and a possible visit by Rod Spence.

Later I worked on a few papers and visited some of the labs.

Tuesday, April 14, 1953

This morning I telephoned Don Cooksey to talk with him about Per Kofstad, whose father is again inquiring about his progress; I gave a favorable report.

I also talked by phone with Greg Engelhard about some athletic matters, including the meeting of the Presidents and the Faculty Athletic Representatives scheduled for next Monday.

I have read the galley proof of J. C. Hindman's Chapter 9 ("Ionic and Molecular Species of Plutonium in Solution") of PPR, Volume 14A and this morning returned it to Joe Katz, along with a covering letter expressing my enthusiasm about the chapter. I told Joe that I hesitate to make any suggestions in view of the excellence of the work; however, I discussed various parts of the material with Burris and pencilled in a few suggestions. I added that Clark may not want to adopt any of these.

I then went down to campus, gave the Chemistry 223 lecture (on  $\beta$  stable isotopes in the heavy region, alpha decay below lead, subshells past lead) and then met and had lunch with Ernest Wakefield (Radiation Counter Laboratories), his wife, and Al Ghiorso. Al and I gave the Wakefields a tour of some of the campus

Back on the hill I looked over the mail that arrived this morning, which included five copies of the March issue of SCAN, the American-Scandinavian Foundation's monthly bulletin, in which I was featured as the "Member of the Month," from George E. Hanson (Director of Development). The article contained my brief description of the Nobel ceremonies.

I also received a thank-you card, partly in Swedish, from Karl-Erik Zimen for some reprints. Zimen said that he will be at Yale University from April 22 until August.

Bart Sutton also wrote to thank me for my talk before the Golden Gate Paint, Varnish and Lacquer Association last Friday. He said the attendance at the meeting was over twice the average attendance.

I made the usual rounds of the labs to check on the research; then Perlman and I worked out a midterm for the Chemistry 223 class for Thursday.

Wednesday, April 15, 1953

I have studied and discussed with some of our people Dick Diamond's March 26 long response to the information that his thesis ("An Ion Exchange Study of Possible Hybridized 5f Bonding in the Actinides") has been declassified with deletions. Dick presented some ideas he has had about the paper, and today I wrote, saying that I believe I am in general agreement with his proposed method of proceeding with it. I made some specific suggestions on the paper and then told him that I have asked James W. Cobble, a postdoctoral men, to carry on research along the general line of 5f bond hybridization. [Jim has already written to Dick.] Finally, I said I was pleased to see his "Letter" in The Physical Review and happy to hear about his and Marian's impending blessed event.

Another galley proof (Chapter 12, PPR 14A, "The Chemistry of Neptunium" by B. B. Cunningham and J. C. Hindman) was returned to Joe Katz today. In the accompanying letter I wrote that Cunningham has made his corrections with the traditional red pencil and that I presume that he and Clark will check to see that the queries to the author have been taken care of since we can not handle this in the absence of the manuscript.

Today's mail brought a nice note from George S. Worrell (Executive Secretary of The Golden Gate Paint, Varnish and Lacquer Association) thanking me for my talk there last Friday.

I also mailed eight copies of the following letter to R. M. Warren (Assistant Secretary, American Chemical Society) to nominate Dr. Burris B. Cunningham for the Fisher Award in Analytical Chemistry. An additional copy of the letter went to Professor John E. Willard (University of Wisconsin), requesting that he write one of the seconding letters--Professor Latimer is writing the other.

April 15, 1953

Mr. R. M. Warren, Assistant Secretary  
American Chemical Society  
1155 Sixteenth Street, N. W.  
Washington 6, D. C.

Dear Mr. Warren:

I am writing this letter to nominate Dr. Burris B. Cunningham for the Fisher Award in Analytical Chemistry.

I have been in close contact with Professor Cunningham and his work since he came to the Metallurgical Laboratory at the University of Chicago in 1942 to work on the Plutonium Project of the nuclear weapon development program. I believe that he is one of the best experimentalists that I have ever known, and in the specialized and important field of ultramicrochemistry, he is probably one of the top few, or perhaps the very top man. In addition, he has a good, thorough and sound understanding of the theoretical aspects of chemistry, especially analytical and inorganic chemistry, and he also has a thorough knowledge of the important new field of nuclear chemistry. Previous to 1942 he had obtained a good knowledge and training in the field of biochemistry, in which field he did his graduate work and conducted a number of research problems at this University. During this period he published several papers in the field of ultramicroanalysis, and, with L. G. Saywell, devised the colometric ortho-phenanthroline method for the determination of iron, a distinct advance over previous methods for this important determination.

The following is a recapitulation of Dr. Cunningham's achievements on which this nomination is largely based:

- (1) With one co-worker he was personally responsible for the first isolation of pure compounds of the new synthetic element plutonium. This required the most advanced analytical techniques and has great historic significance, not only because it was the first isolation of the important element plutonium, but also because it was the first isolation in weighable amount of a pure compound of a synthetic isotope of any element.

This was accomplished at a time when the total supply of plutonium, which had been made by means of the Berkeley 60-inch cyclotron, amounted to a few micrograms

and the work was done through the development and use of extraordinary techniques in the field of ultramicro-chemistry. This was done at the Metallurgical Laboratory in August of 1942 and the event will surely go down in scientific history as one of the outstanding achievements in science.

- (2) During the early part of 1943, at the Metallurgical Laboratory, he performed the crucial experiments, using microgram amounts of plutonium and working on the ultramicro scale of operation, on the chemical separation process which was installed at the plutonium manufacturing plant at Hanford, Washington. It is my belief that without these tests it would not have been possible to have had plutonium ready in time for use in the Nagasaki atomic bomb. It was necessary, and fortunately Cunningham was able to study the separation process at the actual concentrations of plutonium which were to exist in the Hanford plant and he was able to do this even though only microgram amounts were at his disposal. This was a first-rate piece of scientific work as well as being of extreme practical importance.
- (3) At the Metallurgical Laboratory, he and his co-workers were responsible for a thorough ultramicrochemical investigation of the analytical chemical properties of plutonium, including the preparation and identification of a large number of pure compounds. This information was of importance in connection with other phases of the plutonium project. This was done at a time when the total supply of plutonium amounted to no more than tens and hundreds of micrograms and was accomplished through the use of the techniques of ultramicrochemistry in which the volumes of solution sometimes amounted to fractions of microliters. He and his co-workers were able to study the solubility properties of a number of plutonium compounds and also to establish some of the oxidation states and even measure the oxidation potentials. He was the first to establish the oxidation number of plutonium in any solid compound. This was done for the tetrailodate by ultramicro titrimetric iodine analysis, generally considered impossible before then.
- (4) The first isolation of neptunium in the form of pure compounds was performed by members of his group (L. B. Magnusson and T. J. LaChapelle) at the Metallurgical Laboratory. This was done with the isotope  $Np^{237}$  which is produced as a byproduct in the uranium chain reacting pile and the work was done on the ultramicro scale.

(5) He was the first man to isolate pure compounds of the synthetic element americium. This was done by Cunningham himself, and again the work was done on the ultramicroscale with extremely small amounts of material. He was able to weigh this material and hence to determine its specific alpha particle activity, and the presently accepted value of 475 years for the half-life of this isotope,  $Am^{241}$ , is due to him.

During the peak of the work at the Metallurgical Laboratory in 1944 and 1945, he was in complete charge of the work of some thirty-five scientists composed of men with bachelors and doctors degrees. He ran this program efficiently, serving as a source of inspiration to the men who were working under his direction. This group continued to investigate the basic chemical properties of plutonium using larger amounts of material as these became available and also carried on other investigations essential to the success of the Plutonium Project. As a result of his direction of this work, together with the researches described above, Cunningham became, and is widely known and recognized as a scientist of outstanding ability throughout the whole of the large Plutonium Project.

Since his return to the University of California in 1946, he has continued a research program which is distinguished by its versatility and productivity. A part of this work of fundamental importance to ultramicroanalysis was the derivation and publication of equations giving the basic theory of operation of the most important types of ultramicrobalances. The theoretical information has been applied by Cunningham and his students to the design and construction of practical and reliable balances of this kind. Balance design and construction has been followed by the development of a concomitant technique of precipitation, filtration and weighing applicable to ultramicrogravimetric analysis. He and his students have continued his important and significant work in the field of the transuranium elements and have also carried on investigations of the homologous elements in the rare earth group. He has made magnetic susceptibility measurements on compounds of americium and curium through the use of an ultrasensitive magnetic susceptibility apparatus with which measurements on microgram amounts of material are possible. He and his students have carried on thermochemical investigations of americium and many of the rare earth elements. This work has included the synthesis of new compounds of these elements, of which the compound  $TbF_4$  is an important example. The program has included very interesting measurements on the optical absorption spectra of compounds of actinide and homologous lanthanide elements. In addition, work of extraordinarily difficult character is being carried on with the highly radioactive curium ( $Cm^{242}$ ) using weighable amounts of material to make crystallographic and other studies. The versatility of his research is illustrated by the fact that he has work getting underway on the Zeeman effect and nuclear spin determinations in connection with optical spectra of the transuranium elements and also, collaborating with physicists, work on nuclear and electronic paramagnetic resonance on these heavy isotopes.

One outstanding aspect of Dr. Cunningham's research program in analytical, inorganic and physical chemistry is his extraordinary facility in devising, building and using special apparatus. This is in large measure responsible for his success in making measurements that are beyond the reach of most other investigators. Thus, he has developed an ultrasensitive magnetic susceptibility apparatus, working on the torsion principle with quartz fibers, which is capable of measuring the magnetic susceptibilities of amounts as small as micro-gram quantities of paramagnetic substances. He has developed microcalorimetric apparatus with which it is possible to make thermo-chemical measurements on amounts of material smaller than ever before attempted. He is also constructing quartz fiber torsion ultramicrobalances which will extend significantly the sensitivity and the reliability range beyond anything yet accomplished. He is continuing to develop new ultramicroanalytical methods and is concurrently engaged in writing several books covering the general microchemical field.

The work of Dr. Cunningham and his students in the field of strictly nuclear chemistry is also impressive. Thus they have determined through specific activity measurements the half-lives of the important uranium isotopes, U<sup>234</sup>, U<sup>235</sup> and U<sup>236</sup> with a degree of accuracy not yet approached by other investigators. He and his students have discovered a number of interesting new reactions as a result of their work with the 184-inch cyclotron. They were among the first to demonstrate that unusual nuclear transformations take place upon bombardment with 190 Mev deuterons and 380 Mev helium ions, leading to the expulsion of as many as 30 or 40 neutrons or protons in a single reaction.

Cunningham is a quiet and unassuming person with a pleasant personality and an excellent ability to get along with people. He is a real scientific scholar, very careful in all of his work, and at the same time, has the ability to get things done. I have the impression that he is well liked by every person who has worked with him, those who have worked under his direction as well as those who have been associated with him on the same level or who have served as his superiors.

Cunningham is continuing to carry on his very diversified and productive research program and has a large number of problems underway. On the basis of his accomplishments in the past and the promise which his present program holds for the future, I feel that I can recommend him for the Fisher Award without reservation.

Very truly yours,

  
G. T. Seaborg

GTS/lmh

Thursday, April 16, 1953

The research group meeting this morning was attended by Asaro, Batzel, Carniglia, Carr, Cobble, Dauben, Feay, Glass, Gunn, Gray, Hoff, Hollander, Hyde, Jaffe, Kalkstein, Levy, Michel, Nervik, Passell, Perlman, Raby, Seaborg, Slater, Smith, Stephens, and Templeton.

Nervik again talked about the bombardment of tantalum with full energy protons in the bevatron to produce  $Tm^{168}$  (85 days) and  $Tm^{169}$  (9.6 days), saying that they do not know what order the particles that are emitted in the reaction come out. In the discussion he spoke of their reasoning that indicates that the overwhelming spallation process (change of 15 mass units in this case) is one which involves emission of neutrons to give nuclides farther out on the mass number 167 chain than  $Tm^{167}$  (~1 hour). Nervik said these results are not too surprising in view of other data, but they serve as a good indication of what direct spallation processes are occurring in these bombardments. I mentioned that Glass and Cobble are going to continue the problem of competition between fission and spallation in the transuranium region and that perhaps this could be done with elements just below uranium also in order to get more information on how the competition varies with atomic number.

Slater described his study of the alpha and gamma spectra of  $U^{230}$  and daughters produced via  $Pa^{230}$  decay from the bombardment of thorium with 145 Mev protons. He described his chemical separation procedure. Asaro examined the final sample in the alpha-ray spectrograph. Asaro said that they do not know exactly what proportion of activity belongs to each isotope because an appreciable portion of the daughter nuclei may recoil off the counting plate in vacuum. He talked about the predicted levels compared with the alpha decay scheme for  $Th^{228}$  and suggested the following energy level scheme for  $Th^{226}$ , the alpha decay daughter of  $U^{230}$ : 0 kev, 71 kev (0.48%), 204 kev (0.48%), 227 kev (0.46%). He added that the observed decay to an odd level (204 kev) has hitherto been observed only in the case of  $Th^{228}$  and they did not know how it would occur in  $U^{230}$ , but it seems to occur with about the same hindrance factor as in  $Th^{228}$ . For  $Th^{226}$  they observed levels of 0 kev, 110 kev, 247 kev (1%), 297 (1%) in the daughter  $Ra^{222}$ . Asaro said that the predicted abundance for the  $Th^{226}$  alpha particles was based on the hindrance of the third and fourth alpha groups of  $Th^{228}$ . I noted that large hindrance changes may have to be related to spin for most of the models come out with large hindrances when the spin is large, but Perlman said they have been thinking along a different line. He said that a possible explanation may lie in the assumption of a progressive change in charge asymmetry on leaving the closed shells in the vicinity of lead, adding that the potential barrier will then be spherically non-symmetrical and, if the alpha particles of a type have a preferred direction of emission, any progressive change in charge distribution will be reflected in a progressive change in the ease with which the alpha particle can leave.

Asaro then said they also studied the gamma rays with samples of  $U^{230}$  (and daughter) that had been purified by ether extraction. For  $Ra^{222}$  they found only one alpha group but they predict and expect a gamma ray of 330 to 350 kev. They positively observed gamma rays of 72, 110, 197, 243, and 336 kev and perhaps a 133 and a 158 kev gamma ray. Asaro said they collected recoils from  $U^{230}$ , did not observe the 72 kev gamma ray, but

did observe the 110 kev gamma as well as the 243 and 333 kev gamma rays. The 158 kev gamma ray may have been observed, but it could not be determined whether the 197 kev gamma ray was in the recoils or not. Finally, Asaro said that  $\text{Th}^{228}$  might be expected to be present in the sample put in the alpha particle spectrograph. They found another alpha group that would have been super-allowed if it belonged to any of the above isotopes. He reported that, if  $\text{Th}^{228}$  was responsible for it, this can be used as a calibration standard and the  $\text{U}^{230}$  alpha energy is then 5.89 Mev.

\* \* \* \* \*

The Chemistry 223 class was given the following midterm today. I then stopped in the Department of Chemistry office before going up the hill.

Chemistry 223

Midterm Examination  
April 16, 1953

1. (15)

Show by closed cycle calculations how you would predict whether or not  $\text{Em}^{218}$  was beta-stable.

2. (15)

An odd mass number nuclide beta decays. The odd neutron in the emitting nucleus is in the  $g\ 9/2$  state. The odd proton in the product nucleus is in the  $f\ 5/2$  state. Classify this beta decay according to Gamow-Teller rules and give your reasons.

3. (15)

A deuteron with an energy of 10 Mev has a range of  $150\ \text{mg/cm}^2$  in a certain material. What would be the range of a 15 Mev  $\text{He}^3$  particle in the same material?

4. (15)

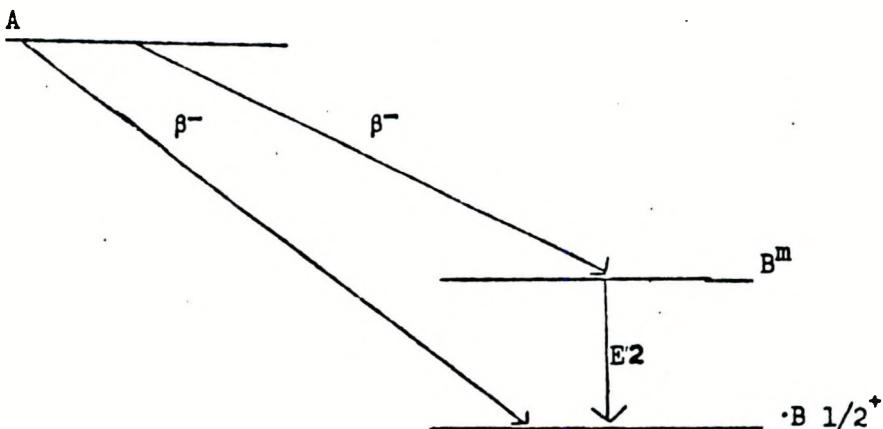
No alpha decay has been seen so far in any of the lead isotopes. In which of the known lead isotopes (stable and unstable) would you have the best chance of observing alpha decay (give reasons), and how would you go about it?

5. (15)

What type of isomeric transitions would you expect to find in even-odd nuclides in the region just below  $N = 82$ ?

6. (25)

Given:



The two  $\beta^-$  transitions from A to  $B^M$  and from A to B are both first forbidden. The transition from  $B^M$  to B is an E2 transition.

- What are the spins and parity of A and  $B^M$ ?
- What are the spectroscopic states of A, B, and  $B^M$  assuming  $28 < Z < 50$

and  $50 < N < 82$ ?

Give your reasoning.

Friday, April 17, 1953

I made several calls this morning, including one to Don Cooksey, and also one to Eugene Burdick about the meeting Monday with the PCC Presidents and Faculty Athletic Representatives.

On March 20 Hari Sharma, my former graduate student who is now in India, sent a paper on  $Mn^{57}$  that he had prepared from his thesis work, which he proposed sending to The Physical Review as a "Letter to the Editor." Hari wrote that he now has a job as a Junior Scientist with the Atomic Energy Commission at \$80 per month, but the radiochemical section is not at all developed and he has hardly anything to do. Earl critically studied the paper and prepared a detailed response:

...On the basis of your information, the assignment of these activities to  $Mn^{57}$  seems very reasonable and I have no better alternate assignments, but I think I should want to have more certain proof before I postulated a pair of isomers as unusual as this in a journal publication. This is an interesting question and I wish it were possible to have some more work on it. Perhaps we can have someone undertake it...

Stan, Al, and I had a long conversation about their work and the Argonne report to Spence (ANL-WMM-1076 rev., March 20, 1953, which we received on March 27, 1953), and we decided that I should again talk with Rod Spence. I learned that Spence was at Argonne today, so I telephoned Winston Manning and arranged to talk with Spence. First I asked Manning about Chapter 20 of PPR, Volume 14A ("Slow-Neutron and Spontaneous-Fission Properties of Heavy Nuclei"), and he said that John Huizenga is working on it and trying to decide whether to collaborate as a co-author of the chapter. I brought up the subject of their report to Spence and asked if he and Huizenga have studied it carefully. Manning said they have and that they still stick to their interpretation--they can find no growth of the 6.6 Mev alpha-particle activity in the californium fraction. Manning said that his men are making another chemical separation, which will be finished next week, and they will know more then. I urged Manning to study the data carefully. On another subject Manning mentioned that he and his family plan to take a June vacation in Colorado but they are not coming as far west as California. I invited them to stay with Helen and me if they change their minds.

Rod Spence then got on the phone (by now it was about 11 a.m.), and I invited him to visit Berkeley, subject to AEC arrangements. I told Rod about our success in working up the coral from the Mike shot and said that we may want 10 tons, if our success holds up; however, we will tell him in about a week. Rod said that he doesn't have a survey of the island sources of coral yet but will look into it and let us know. I told Rod that we will continue to give him one-third of what we recover. Spence then told me that Manning called him yesterday with complaints about our March 19, 1953 report; the date of the first identification of the 6.6 Mev alpha-particle emitter as being element 99 was questioned. I pointed out that, even as of April 16, 1953, (the date of Manning's phone call to Spence in which he said they still stuck by their March 20, 1953 statement), they assigned their 15-day 6.6 Mev alpha-particle emitting activity to element 100. We agreed that the dates wouldn't be

emphasized in the Mike report. Spence said that he questioned Manning in regard to the statement in their report that they had seen the 7.0 Mev alpha-particle emitting activity repeatedly.

I then asked Spence to send Spof English his copy of our report (MB-IP-741), which could be used by the proper people in Washington to decide what parts should be deleted in the Radiation Laboratory version. Rod said that he will telephone English and then let me know. Rod reported that Charlie Browne is in the midst of an experiment with excellent elution results (as shown by americium-cerium separation) but didn't have element 99 data yet--the elution was too slow to get the 7.0 Mev alpha-particle activity in a separate fraction this time. Spence and I agreed they would have to be the referee to decide experimentally the 6.6 Mev alpha-activity question. I told him that we are anxious to have them do this and that they should hurry due to the 20-day half-life. On another matter I told Rod about our approximately 0.1 Mev beta-emitting berkelium isotope with its 5.8-Mev alpha-emitting californium daughter, which now has no spontaneous fission, that we assign to Bk<sup>249</sup> and said that he should do what he wants about changing the Mike report on this--we would just as soon leave the Mike report with data as of dates of submission, approximately March 20. Rod announced that Browne has completed the report and it will be typed next week; we should get a draft copy for correction in about a week.

After lunch I went down to campus (Room 202, Lewis Hall) for the 2:15 p.m. prelims of David Alan Dows. In addition to me, the committee was composed of Donald S. McClure, Charles W. Tobias, Wendell M. Latimer, Roderick Craig, and Wilson M. Powell. Dows easily passed the examination.

Afterward I stopped in the Chemistry Department office. Dean Pitzer told me about a possible graduate student, James F. Schooley of Indiana University, who has applied to the U. S. Air Force for assignment to their Institute of Technology. Pitzer said that, on the basis of recommendations from Ralph L. Seifert and Harry G. Day, he will admit Schooley to Graduate School here if the Air Force assignment is not available. Schooley is interested in nuclear chemistry and has been a member of Indiana's national championship basketball team, something that certainly interests me.

I looked over the midterms, which our reader has corrected. The class average was 66.8 with the highest scores being received by Peter Gray (82), James Grover (85), and Frank Stephens (86).

#### Saturday, April 18, 1953

This was a usual Saturday, spent with the kids and reading.

#### Sunday, April 19, 1953

Again I did some reading and writing in my study, but later the kids and Helen helped me celebrate my birthday appropriately.

#### Monday, April 20, 1953

At 9:30 a.m. I went to the St. Francis Hotel in San Francisco for a

meeting of the Presidents and the Faculty Athletic Representatives of the Pacific Coast Intercollegiate Athletic Conference. Present were Presidents J. E. Buchanan (Idaho), Fred D. Fagg, Jr. (USC), Henry M. Schmitz (Washington), Robert G. Sproul (UC), A. L. Strand (Oregon State), J. E. Wallace Sterling (Stanford); Faculty Representatives Glenn W. Holcomb (Oregon State), Orlando J. Hollis (Oregon), Claude E. Jones (UCLA), T. S. Kerr (Idaho), Rixford K. Snyder (Stanford), Hugh C. Willett (USC), and me. Also attending were PCC Commissioner Victor O. Schmidt, Wilbur C. Johns (UCLA), Chancellors Ray Allen (UCLA) and Clark Kerr (UC), R. S. Johnson, (UC, Secretary)--President Sproul acted as chairman. The first part of the meeting was devoted to a continued discussion of a report (Johns, chairman) of a special committee on recruiting and allied problems. This was first presented last December, at which time there was some confusion about some of the provisions, in particular about subsidies. There were concerns about living standards, enforcement, professionalism. Eventually, the presidents reaffirmed, without dissenting voice, their action of January 3, 1953, in which "There was a unanimous expression of determination to go no further down the road of professionalism, and, instead, to undertake definite and vigorous steps to reverse the unhealthy trends of recent years." After some unresolved discussion about on/off campus jobs, the presidents agreed unanimously that the scholastically ineligible student should be eliminated from athletic practice as well as competition. They also agreed (no votes, Fagg and Strand) that freshmen should be prohibited from engaging in outside athletic competition. Other matters covered included agreement that off-season practice should be eliminated (5 to 2), agreement that time of participation by students in varsity athletics be limited, and agreement that there be a limitation on number of trips involving absences from class and work. The presidents agreed to take no part in negotiations relative to the renewal of Rose Bowl agreement; there were a couple of other minor items discussed before the 4 p.m. adjournment.

Tuesday, April 21, 1953

At the lab this morning I looked over the mail, talked with some of the fellows, and had a number of phone calls, including one to Ian Turner, who is one of Clark Kerr's assistants, about some rumors I have heard about reorganization of Mira Vista Golf Club. I also returned a call to President Sproul, who urged me to accept the invitation of UC Regent Earl J. Fenston to speak at the Hanford, California, Chamber of Commerce annual installation dinner. Sproul emphasized that Fenston is a good Regent (very helpful to the University).

I wrote to James F. Schooley, the possible graduate student about whom Pitzer told me last week, to describe our program and to offer him a position here at the Radiation Laboratory if he comes to Berkeley. I added:

As a sports enthusiast and also in my official capacity as our campus Faculty Athletic Representative I followed the progress of your basketball team during the season just completed. It strikes me that a man of your ability could be of help to our intramural, nuclear chemistry basketball team, whose record during recent years could stand improvement.

I went down to campus and, after returning and working the midterm for the class, gave the Chemistry 223 lecture on nuclear reactions.

Olaf Lundberg entertained a number of Swedish-loving faculty members at a luncheon at the Faculty Club in honor of Mrs. Heilbrun (head of the Swedish-American Society and wife of the publisher of Stockholm's Dagens Nyheter). Others present included Eric C. Bellquist, Arthur C. Brodeur, Frank L. Kidner, Thomas C. Blaisdell, Malcolm Davisson, et al.

A teletype arrived from W. Kermit Anderson (Argonne), "PLAN ARRIVAL SAN FRANCISCO TWA FLIGHT 35 APRIL 26 AT 5:15 PM IF NOT SATISFACTORY PLEASE ADVISE." After checking with some of the Project Whitney people and making some arrangements, I teletyped back, "UCRL DRIVER WILL MEET YOUR TWA FLIGHT 35 AT SAN FRANCISCO AIRPORT. RESERVATION MADE AT DURANT HOTEL, BERKELEY. PLEASE HAVE YOUR SECURITY OFFICE FORWARD CLEARANCE TO SAN FRANCISCO AREA. YOU WILL BE CONTACTED AT HOTEL MONDAY MORNING."

In today's mail was a \$150 check from Hugh Handsfield (McGraw-Hill) for my chapter, "The Actinide Elements and Atomic Power," which will be included in the book manuscript of the course, Modern Physics for the Engineer, edited by Dr. Louis Ridenour.

Wednesday, April 22, 1953

I wrote the following today to Chancellor Clark Kerr:

I am writing in response to your request for my opinion as to the desirability of participation by the University of California in the launching and operation of educational, non-commercial television in the Bay Area.

I feel very definitely that such participation by our University would be a real service to our state. There are, in my opinion, tremendous cultural and educational opportunities in such an undertaking and it seems to me that these very properly fall within the province of our University.

As an example, let me mention a potentially useful service within my own field, science, and in particular, atomic energy. It is my opinion that a prerequisite to a solution of our country's international problems, augmented by the weapons provided by science, is a better understanding of science by the average citizen of our country. I believe that a number of my colleagues could help through participation in educational TV programs in this general area.

I am impressed by the fact that channel 9, a rare very high frequency channel, is presently being offered. This channel has the advantages of better reception in the hilly terrain of the Bay Area and capability of reception by all sets now in use and it may not be long available.

Another long interesting letter arrived from John Rasmussen. After covering a number of scientific points, John wrote:

I've written about the spectrographs and some of the work here in glowing terms in past letters, perhaps sounding like one of the Rover Boys in Stockholm. There is, of course, another side to the story, too. I've gained a new appreciation of the team spirit, enthusiasm for the work, and the general readiness to give and receive advice and help, such as has been realized in relatively large measure in our Berkeley group. Team work seems still a relatively new concept to a Ph.D. or Ph.D. candidate over here. For one thing, their enormously lengthy process of getting a degree puts undue stress on the work's being done without help of others and on its being a lengthy contribution to science. The system produces scientists of generally high quality (but in small numbers) who may continue to make scholarly contributions by themselves in a small area, but these men are not ordinarily interested in teaming up in a broad coordinated research program covering all phases of a field of research. Their work is generally excellent, but it proceeds at a slower pace when carried on so independently. Particularly in the problems involving the skills traditionally physical and chemical, where both are needed in a major proportion, they have not found a very satisfactory solution. The chemist helping make separations for an investigation of radioactivity is expected to assume purely the disinterested technician's role. As we all know so well, it just doesn't work to try to compartmentalize the physical and chemical aspects of many nuclear problems. The decisions involving compromise between purity and yield which the chemist must make can hardly be made intelligently without intimate knowledge of the physical problem, and conversely, the physical experiment cannot be designed optimally without appreciation of the possibilities and limitations of the chemistry involved. I guess I've shattered some cherished traditions by just going ahead and helping on the physical work anyway. I hope I don't sound too critical, for I don't feel that way at all. I have the highest regard for those here with whom I have worked, and they in turn have showed me and my wife the greatest hospitality. I'm simply reporting that in many ways research is done differently from Berkeley. There are both advantages and disadvantages....

It was a very thoughtful letter, which I routed to a number of the fellows (Perlman, Hyde, Thompson, Ghiors, et al.). John also mentioned that the second scientific attaché with the American Embassy, Bob Loftness, whose position is being eliminated in the current Republican economy drive, is going to return to the States soon to look around for another job.

At dinner the Seaborgs celebrated young David's 4th birthday with cake and gifts.

Thursday, April 23, 1953

The meeting of the research group this morning was attended by Asaro, Carniglia, Carr, Clark, Cobble, Cunningham, Dauben, Feay, Glass, Gray, Grover, Hollander, Hulet, Jaffe, Kalkstein, Lessler, Levy, Nervik, Passell, Perlman, Raby, Ruben, Seaborg, Shudde, Stephens, Surls, and Templeton.

Levy reported at length on his study of the relative yields in the production of nuclear isomers. He went over the case of  $\text{Co}^{58m}$ ,  $\text{Co}^{58}$  produced by helium ions on  $\text{Mn}^{55}$  and the case of  $\text{Hg}^{197m_2}$ ,  $\text{Hg}^{197}$  produced by protons on  $\text{Au}^{197}$ . Levy then proposed an explanation of the behavior of the cross-section ratio with projectile energy, which is consistent with his data and with the findings of Biller who showed that, in the fast proton-induced fission of bismuth, the resultant isomer pairs always showed predominance of the isomer of higher spin over that of lower spin, often to the exclusion of the latter. He discussed two limiting cases: (1) The compound nucleus is very highly excited and (2) Excitation of the compound nucleus is just over the threshold for the reaction. The study evoked much discussion, and I brought up the information that John Rasmussen related in his recent letter that some European laboratories have positive results toward the excitation of lower to higher isomeric state by the I.T. radiation itself.

Hollander spoke for a while on the theory of core isomerism, citing  $\text{Mo}^{93m}$ ,  $\text{Mo}^{93}$ .

\* \* \* \* \*

After the Chemistry 223 lecture during which I continued the discussion of nuclear reactions, I stopped in the Department of Chemistry office before going up to my office on the hill.

In the mail that arrived today was a form-type letter from Winston Manning, who in his role as a member of the Canvassing Committee seeking nominations, requested nominations for the 1954 ACS Award in Pure Chemistry.

Ron Brightsen wrote on April 17 that he was disappointed that we couldn't grant him the fellowship we discussed, but that he was collecting the transcripts of his work to send to us. He also again inquired about time to present his work on nuclear shell structure and beta stability at the Gordon Conference. I replied today that we are looking forward to receiving his transcripts and that we hope he receives the Westinghouse fellowship for which he is applying. With regard to the Gordon Conference, I wrote that he would be very welcome to present his material at the session of which I shall be chairman, but I have heard that Charles [Coryell] has designated Truman P. Kohman as chairman of one of the sessions on nuclear systematics and he may prefer that session provided Truman has a place left. I wrote that I shall probably schedule three papers--John R. Huizenga on "Heavy Element Fission", Frank Asaro on "Complex Alpha Spectra," leaving one space open. I asked Brightsen to let me know his decision as soon as possible.

I wrote to Steve Lawroski to tell him that we would like Charles R. Wilke (Associate Professor of Chemical Engineering and also associated with our group) to visit Argonne and his group on his trip to the midwest within the next two weeks. I gave some details of the trip.

I spent some time in the labs looking over the experiments and talking with the researchers.

Later I went to the Claremont Hotel for a dinner meeting of the Bear

Backers (cocktails at 6 p.m. and dinner at 7 p.m.). I made a few remarks about my role as Faculty Athletic Representative with some attempts at humor, needed to capture the attention of this hard drinking crowd.

Friday, April 24, 1953

Included in this morning's phone calls were ones to Joe Katz (about material for Volume 14A of the PPR) and to John Huizenga. With Huizenga I discussed our views on spontaneous fission systematics, mentioning our plots of June 1951. I told him that the Argonne report is not clear about his contribution to the systematics and then went on to talk about our explanation of the Argonne results on the "Mike" explosion. I also suggested that it would be worth while to have some sort of systematics session at this year's Gordon Conference, and he agreed. Finally, I asked if he would be willing to collaborate, as a co-author, on a revision to Chapter 20 ("Slow-Neutron and Spontaneous-Fission Properties of the Heavy Elements") of PPR, Volume 14A. Huizenga said he will get a copy from Joe, look it over, and then let me know. Later I wrote Huizenga to ask him to visit Berkeley, if he believes the changes in Chapter 20 warrant a conference,

Norman Bonner wrote on April 14 with more questions about the position we offered him at Livermore, saying that the position sounds extremely attractive but there are some problems he would not like to work on and that he wouldn't want to give up a job that he likes for one of an unknown character. In my response today I wrote that it is only fair to stress that there will not be "the diversity of research interest nor freedom of choice traditional to the Berkeley site" as stressed in Dr. Hyde's letter to him of March 24. I said that to promise more would not be honest and that all people at the Livermore site have to be ready to undertake whatever practical research jobs are handed to them. I wrote, "My own impression, Norman, is that you might very well not like this and that you therefore probably shouldn't take a chance on such a position." I added that the work would be similar to the practical aspects of that which goes on in Rod Spence's group in the J Division at Los Alamos, where Norman has worked. Finally, I told him about the housing difficulties in Livermore itself at the present time.

This morning I wrote to Colonel W. R. Middleton (Chief, Civilian Institutions Division, U. S. Air Force Institute of Technology, Wright-Patterson Air Force Base, Dayton, Ohio) to inform him that, at his request, Dean K. S. Pitzer is accepting Stanley J. Lokken and John Gilmore to do work toward an advanced degree in nuclear chemistry. I explained our AEC clearance procedures and said that I have asked Mr. George Everson to send him several copies of the Personnel Security Questionnaire for these men to fill out. I then went on to tell Middleton that James F. Schooley, who has also applied for admission to his Institute, has been accepted for graduate work here. In the event that he is accepted at Middleton's Institute, we will be very glad to have him in our group.

I also filled out and signed inquiry forms for Darrell C. Feay and Louis M. Slater, who are applying for positions at the Naval Ordnance Test Station at Inyokern.

At 2 p.m. Herbert R. Johnston, who is working on his Ph.D. in Electrical Engineering, came in to report to Al, Herman, and me about his work. We have had conversations with him on previous occasions about a permanent position with us.

A telegram arrived this afternoon from Harold Dodgen (Department of Chemistry, Washington State College): "THERE IS GREAT INTEREST IN HAVING YOU GIVE THE PRINCIPAL ADDRESS AT THE NORTHWEST REGIONAL MEETING OF THE AMERICAN CHEMICAL SOCIETY BEING PLANNED FOR JUNE 12 AND 13, 1953. THE MEETING WILL BE ATTENDED BY ACS MEMBERS OF WASHINGTON, OREGON, IDAHO, AND MONTANA, WE CAN PAY ALL EXPENSES. PLEASE CONSIDER IT AND I WILL CALL YOU AT 3 PM ON MONDAY APRIL 27."

When I toured Bldg. 5 today, I noticed that Health Chemistry had moved Box 245 into the Cave Room. This will be used for working up the sample (an heavy element fraction from processing of the large sample of coral from the Mike explosion), which was sent to Arco for neutron irradiation.

Saturday, April 25, 1953

I worked in my study on my writing projects for a while. The proofing of chapters for PPR, Volume 14A, has taken up a lot of time recently. Fortunately, John Huizenga seems interested in helping to revise Chapter 20, "Slow-Neutron and Spontaneous-Fission Properties of Heavy Nuclei."

Sunday, April 26, 1953

The kids kept me entertained for much of the day although I was able to work on my writing projects for a while.

Monday, April 27, 1953

One of the first things I did this morning was to check with Al and Stan about the progress on the sample received from Arco on Saturday evening and which they worked up Saturday evening and yesterday.

Since this is spring recess, I made the rounds of the labs to speak with some of the students, including Walter Nervik, about some suggestions for their research. Later I dictated a few letters.

Truman Kohman wrote on April 17 about several things; he asked for additional copies of UCRL-1928-Rev. ("Table of Isotopes") and about its publication date. I sent him the copies he requested and said the publication was delayed until the April issue of Reviews of Modern Physics, which will probably not appear until July. With regard to PPR, Volume 14A, I explained that it is now in press and we have read galley proof on about half of the chapters; it will probably appear in print toward the end of the year. Volume 17B, I reported, has already appeared as a secret volume and will not appear in the declassified form. Kohman enclosed a copy of their work on  $Mg^{28}$ , which he reported conflicts with the latest work of Sheline. I also answered Kohman's query about my comment in a letter to Donald M. Wroughton about the shortage of nuclear scientists at the doctorate level in this country by saying that I should

have added the adjective "good" nuclear scientists. I explained that we are having difficulty staffing a large group at Livermore and that, in the case of our own Ph.D.'s, more positions have been offered than we can fill. I added that, if there is a drastic economy drive, the situation may change. This, I wrote, is a matter of politics and his predictions are certainly as good as mine on this point.

I also answered an April 20 letter from Truman Kohman, requesting an evaluation of B. C. Haldar. This was similar to others I have written for Haldar. I concluded with "In summary, I would say that I don't regard him as leaving here a finished nuclear chemist in view of his short stay, but he did obtain a pretty good amount of experience and he is a pretty good man."

A similar letter about Haldar went to L. M. Harvey (Secretary of the Board of Research Studies, University of Cambridge, Cambridge, England). I concluded with "I feel he is worthy by scientific maturity and personal excellence to be awarded an Imperial Chemical Industries Research Fellowship."

A number of matters were covered at today's senior staff luncheon meeting in my office, such as this year's Gordon Conference, the fellow John Rasmussen mentioned (Bob Loftness, who is looking for a new position) as a possibility to work with Street or Batzel, student information files, Bob Ward as a possible Chemistry 180 (undergraduate research) student, etc.

Captain Brown and Sgt. Waller of the Rad Lab Security Guards met with me to discuss some of our chronic problems--not locking files, leaving out reports, etc. Most of these "crimes" are committed by graduate students, and I promised to speak with the offenders.

After my 3 p.m. phone call from Harold Dodgen, during which I agreed to speak at the Northwest Regional Meeting of the ACS on Friday evening, June 12 on "Present Status of Transuranium Elements," Al and I went up to Tilden Park for nine holes of golf (GTS-48, AG-44).

#### Tuesday, April 28, 1953

This morning I made a few phone calls, including one to Joe Katz about PPR, Volume 14A. I also telephoned Bob Connick regarding his chapter 8 ("Oxidation States, Potentials, Equilibria, and Oxidation-Reduction Reactions of Plutonium") in the volume.

W. Kermit Anderson has been visiting here and Livermore on an interview trip for a position with Project Whitney, and I spoke with him for a while today. Unfortunately, he has severe vision problems, which may preclude our hiring him.

In today's mail was a note from Jacob Kleinberg (The University of Kansas) with more details about my paper on "Oxidation States of the Actinide Elements" for the fall symposium of the Division of Chemical Education. Kleinberg wrote that the 500-word abstract is due June 1 but it is not necessary for a full paper to be prepared for publication.

I then signed the usual letter to an Allan W. Stoner about our research program at the Radiation Laboratory. Dean Pitzer has accepted Stoner as a graduate student; and he (Stoner) has expressed an interest in nuclear chemistry. Stoner is also from Indiana University, as is James Schooley, but I have no information about his basketball ability.

Much of the rest of the day was taken up with talking with graduate students about their research.

Later I contacted Bill Rice (our architect) to tell him about the non-committal response I received from the State Board of Contractors in response to my April 3 letter to them regarding my complaint about our roofing contractor and our leaky roof. I asked Bill to write a "To Whom It May Concern" letter about the problem and its history.

Wednesday, April 29, 1953

After taking care of some administrative matters, I wrote to Herbert E. Morey (Principal of my alma mater, David Starr Jordan High School) to accept his invitation to deliver the commencement address on June 19.

In the mail was a handwritten thank-you letter from James F. Schooley for my offer of a position at the Radiation Laboratory. Schooley said he has not yet heard positively from the Air Force Institute but he will write when he does.

Again, in addition to working on my writing projects, I visited the labs and talked with the students. I learned that the floor of Room 103 (Bldg. 5) was again contaminated from Darrell Feay's americium-curium box. Several people ended up with hot feet while the floor in the area had up to 10,000 alpha c/m.

Thursday, April 30, 1953

The group meeting this morning was attended by Asaro, Carniglia, Clark, Cobble, Feay, Gray, Grover, Hoff, Hyde, Kalkstein, Lessler, Nervik, Passell, Raby, Ruben, Seaborg, Shudde, Smith, and Surls.

Shudde spoke about design changes being developed for the alpha ray spectrometer for use at the inside edge of the cyclotron with photographic plates for short-lived alpha emitters.

Hoff then presented some results of beta spectrometer runs on  $At^{210}$  and  $At^{211}$ , which were produced by helium ion bombardments of bismuth in the 60-inch cyclotron. He said that  $At^{211}$  was found to be free of any conversion electrons other than the Auger peaks from x-radiation; thus the decay of  $At^{211}$  may be represented as a ground to ground state transition. For  $At^{210}$  the conversion electrons from the gamma rays confirmed the findings on the 50-channel scintillation spectrometer, giving a level scheme in  $Po^{210}$  similar to that proposed on the basis of data on the latter instrument. However, additional conversion electrons were found of an apparently almost completely converted gamma ray of 44.1 kev. A rather small but definite conversion peak at 96.2 kev, which might be considered a K conversion line of a 190 kev gamma ray, was seen. Hoff offered two possible decay schemes. He also reported that

the electrons from the 1.42-1.48 Mev gamma ray were in too low abundance to be observed; however, those due to the 1.18 Mev gamma ray and the 0.239 Mev gamma ray were in relatively high abundance with the K/L conversion ratio of the 0.239 Mev gamma ray being about 1/2. I suggested that he should try setting limits on the relative abundances of the 190 kev and the 44.1 kev gamma rays as best he can--this may tell whether they are in cascade.

Carniglia described the twin apparatus for vapor pressure comparisons by the method of molecular effusion and presented some calibration data of the thermocouple. There was quite a bit of discussion about the device.

Asaro again talked about their work on the alpha-particle energies in the  $U^{230}$  series. He reported that the small alpha peak reported two weeks ago was due to a contaminant. Asaro compared their data with pulse analyzer data, which showed the latter to be consistently low:  $U^{230} = 5.888$  Mev (his data), 5.86 (pulse analyzer);  $Th^{226} = 6.343$  Mev (his data), 6.30 (pulse analyzer);  $Ra^{222} = 6.568$  Mev (his data), 6.51 (pulse analyzer). Asaro said they reconfirmed the energy of  $U^{230}$  (5.89 Mev) within 2 kev by calibration with RaA at 5.998 Mev.

Asaro then went on to talk about some calculations of the radii of heavy, even-even nuclei from alpha decay theory and presented the results of such calculations for uranium isotopes. He said that they are trying to determine if, for a given Z, the radii are essentially constant. After a bit of discussion, I added that the shell model shows the proton shells following 82 protons to be  $h_{9/2}$ ,  $f_{7/2}$ , and  $f_{5/2}$  in that order. We now think the order is  $f_{7/2}$ ,  $f_{5/2}$ , and  $h_{9/2}$  instead. These nuclear data on  $r_o$  seem to show subshells at 90 and 96 protons, as would be predicted by this sequence; other phenomena point to a subshell at Z = 96. I suggested that it will be interesting to see if we can note any effect within a given Z due to the effect of a neutron subshell or other effects.

\* \* \* \* \*

Back up on the hill I received a call from George Watt, saying that Gregory Choppin has accepted our offer but he wants to come before July 15 (the date estimated for his Q clearance). I asked Watt to tell Choppin to come by June 15 (the estimated date of his interim clearance). We will let him know as soon as the interim clearance comes through.

I looked over the mail and found a letter from C. F. Dickinson (Secretary-Manager, Chamber of Commerce, Hanford, California) confirming an invitation from Mr. Earl Fenston (owner-publisher of the Hanford Sentinel-Journal) to be an honored guest and principal speaker at their annual installation dinner of the Chamber of Commerce at 7:30 p.m. on Monday, May 18. Dickinson asked for confirmation and some publicity information. This is the invitation that President Sproul told me about and urged me to accept; I shall do so.

Also in today's mail was the awaited draft of the Ivy report on the heavy nuclides in Mike debris, which Charlie (Charles I.) Browne assembled from material submitted by us, Argonne, and Los Alamos.

Charlie wrote, "In general, it is our strong preference that this report be only an expression of the interpretations of the data as of the report date, and serve in no way as a documentary statement insofar as priority of discovery of the new elements is concerned." Stan, Al, and I spent much of the afternoon looking over and discussing the report.

Doral had our phone and room directory updated; the following list includes not only our students and employees, but also some CRD personnel and Project Whitney people located in our buildings.

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg</u>
Abrams, Dorothy	396	203D	4
	378	339	50
Asaro, Frank	397	107	4
Balkwell, William R.	319	351	50
Barton, George W.	372	311	50
	397	107	4
Batzel, Roger E.	319	350A	50
Boegner, Richard L.	395	103	4
Buchholz, Doral	246	114	5
Bystroff, Roman	250	202	4
Carlson, Margot M.	396	102	4
	380	346A	50
Carniglia, Stephen C.	349	105	5
Carr, Robert J.	250	201B	4
Case, Marjory	397	345	50
Clark, Edward S.	7-8159	318	Lewis
Clark, Eugene V.	379	345	50
Cobble, James. W.	250	201B	4
Coleman, George H.	319	350A	50
Conway, John G.	374	316	50
Cook, Marshall W.	467	203E	4
Cunningham, Burris B.	349	105	5
Dauben, Carol H.	7-8159	318	Lewis
Davis, Mildred J.	246	114	5
DeLaRue, Robert E.	467	203E	4
DiGrazia, Herbert X.	470	355	50
Dunlavey, Dean C.	378	341	50
Feay, Darrell C.	349	105	5
Fick, J. Leonard	7-472	101	Gilman
Franke, Antoine A.	379	345	50
Gallagher, M. Fran	393	203F	4
Garrett, Roberta	376	332	50
Ghiorso, Albert	358	111	5
Gilbert, Richard S.	393	203F	4
Glass, Richard A.	250	201B	4
	260	102	5
Goda, Lilly	376	332	50
Goeckermann, Robert H.	319	350	50
Gray, Peter R.	349	104	5
Grover, James R.	379	330	50
Gunn, Stuart R.	250	202	4
Hanna, Lorraine M.	246	114	5

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg</u>
Hartzell, Alfred J.	395	103C	4
Heppler, Winifred	349	105	5
Hicks, Harry G.	393	203F	4
Hoff, Richard W.	349	104	5
Hollander, Jack M.	246	113	5
Hollander, Margie J.	246	114	5
Huffman, Eugene H.	470	340B	50
Hulet, E. Kenneth	260	104	5
Hunt, Charles d'A	7-472	221	Gilman
	466	203E	4
Hutchin, William H.	319	352	50
Hyde, Earl K.	345	110	4
Iddings, Glen M.	466	203C	4
Jaffe, Harold	397	109	4
Jensen, Hildred	397	109	4
Jewell, Frances T.	381	353-4	50
Kahn, Stephen	260	107	5
Kalkstein, Marvin I.	377	342	50
	376	342	50
	395	103	4
Kalm, Louise	246	114	5
King, Emily B.	319	350A	50
Koch, Charles W.	7-8207	27	Lewis
Kofstad, Per	380	344	50
Laird, Walter J.	349	103	5
	346		5A
Larsh, Almon E.	358	111	5
Latell, Frances	466	102	4
Lessler, Richard M.	250	201B	4
Levy, Harris B.	379	345	50
Lindner, Manfred	393	203F	4
Lundin, Robert E.	7-8159	306	Lewis
Lynch, Edward J.	7-472	221	Gilman
Maguire, Patricia W.	250	201A	4
Maimoni, Arturo	467	203E	4
Mathur, Hirdaya B.	380	344	50
McKennon, Docia B.	260	107	5
McLaughlin, Ralph D.	374	316	50
Michel, Maynard C.	395	103	4
	379	345	50
	372	311	50
Mohler, Bobby Ann	381	353-4	50
Motte, Eugene	7-472	221	Gilman
Nervik, Walter E.	378	343	50
Newton, Amos S.	381	353-4	50
Niday, James B.	319	352	50
O'Kelley, G. Davis	250	201A	4
Osborne, Robert N.	393	203F	4
Oswalt, Robert L.	470	340	50
Passell, Thomas O.	397	109	4
Perlman, Isadore	246	115	5
Peterson, Alfred W.	7-592	221	Gilman
Piontek, Therese	395	103	4

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg</u>
Potter, Elinor G.	395	103	4
Powers, John E.	7-472	222	Gilman
Raby, Bruce Alan	345	110	4
Ramaniah, M. Venkata	378	343	50
Reynolds, Fred L.	397	108	4
Robbers, Gloria B.	396	102A	4
Robinson, Herman P.	395	103	4
Rothman, Albert	7-472	318	Gilman
Ruben, Helena W.	7-8159	318	Lewis
Sciamanna, Aldo F.	470	355	50
Seaborg, Glenn T.	246	114A	5
Shalimoff, George	470	355	50
Shudde, Rex H.	378	341	50
Slater, Louis M.	379	347	50
Smith, Helen R.	250	202	4
Smith, Warren G.	377	342	50
Snyder, Mark D.	349	103	5
	346		5A
Spencer, Harry E.	7-8159	306	Lewis
Stephens, Frank S.	397	107	4
Surls, Joan E.	466	203E	4
Surls, Joseph P., Jr.	349	103	5
Tarrant, James R.	250	201A	4
Templeton, David H.	7-649	106	Gilman
	7-8159	318	Lewis
Tellefsen, Robert	466	203C	4
Tewes, Howard	319	351	50
Thompson, Stanley G.	260	102	5
Tolman, Laurin F.	372	311	50
Troutner, Vernon H.	250	202	4
Tuttle, William N.	374	316	50
Vermeulen, Theodore	7-473	211	Gilman
Wallmann, James C.	349	105	5
Williams, Lawrence A.	470	355	50



Goeckermann



Fick



Piontek



Lynch



Maguire



Mohler

Friday, May 1, 1953

One of the more important things I did this morning was to write to Charlie (Charles I.) Browne at Los Alamos to comment on his edited Ivy report. I incorporated comments of Stan and Al and wrote that we find the report satisfactory and that our only suggestions concern an omitted reference and a few changes in his summary table. I asked that he add a second sentence to Paragraph 9.2 something like, "J. R. Huizenga has pointed out that spontaneous fission rates at constant Z increase with A at sufficiently high values of A, in disagreement with the simpler systematics of Seaborg which predict for a given Z continually decreasing spontaneous fission rate with increasing A" with a reference: J. R. Huizenga, private communication to G. T. Seaborg, January 7, 1953." In the summary table of Chapter 11, I suggested that  $Pu^{244}$  be designated as beta stable and that  $Bk^{249}/Bk^{252}$  be deleted as there are no direct experimental observations. I pointed out that, in the case of the two items now entered under  $99^{253}$  and  $100^{255}$ , we feel the facts are more properly represented by the following:  $99^{253}$ , 6.6 Mev alpha, 35-50 days, ANL interpretation, ( $98^{253} \rightarrow 99^{253}$  UCRL interpretation); and  $99^{253}$  or  $100^{255}$ , 6.6 Mev alpha, 16-22 days,  $99^{253}$  (UCRL interpretation),  $100^{255}$  (ANL interpretation). I wrote that the points of disagreement here are sharp and clear-cut and are essentially experimental in nature; I then attempted to clarify our interpretation of the data to Browne. Finally, I added that we favor retention of the dates in the text but will be glad to abide by his decision.

I also wrote a couple of other notes, one to C. F. Dickinson (Chamber of Commerce, Hanford, California) in which I accepted the invitation in his letter of April 28 to speak at their annual installation dinner meeting on May 18. The title of my talk, I wrote, will be "Future of Atomic Energy;" I enclosed some biographical information and asked that someone pick me up at the Hotel Californian in Fresno in time for the 7:30 p.m. meeting.

To W. Kermit Anderson I wrote that we have reluctantly come to the conclusion that, because of his impaired vision, the laboratory can not offer him employment as we had hoped to do when we originally wrote to him. I said, "We appreciate very much your kindness in visiting us, and I personally enjoyed very much the fine opportunity that we had to talk. I am genuinely sorry that it has worked out in this way."

At 10:30 a.m. I met with Mike Koll (of the Alumni Association) and Fran Watson (of Mira Vista Country Club). I (and other members of the University of California Faculty Golf Club) have long felt that the University should, as other big universities do, have its own golf course; we have recently learned that Mira Vista Country Club is being reorganized, and we will no longer be able to play there on a daily basis (without being members of the Country Club). In our conference today we talked about what the University's role could be in the present situation and how the golf course could be made accessible to the University staff.

In the mail was an April 28 letter from Wm. C. Markley, Jr. (USAF Institute of Technology, Wright-Patterson Air Force Base), stating they have just received James F. Schooley's application, have approved him for graduate training as an Air Force officer, and that he will report to

Berkeley in time to begin study next September. Markley explained their policy regarding research fellowships for their officers. I told Doral to have a copy made for Pitzer and to show the letter to Earl and Iz.

Later I met and talked with Dan (Daniel D.) Cubicciotti, who is consulting with some of our people, including Stan Thompson, on techniques for handling high level alpha radioactivity. Dan received his Ph.D. here in 1946 and is now with North American Aviation in Downey, California.

I also tried unsuccessfully to reach my friend Lee DuBridge (President, Cal Tech) to discuss H.B. 4304, which Representative John W. McCormack submitted in the House and a copy of which he sent me for perusal and support--I shall try to reach DuBridge next week. This legislation is concerned with the reorganization of the administration of the research and other functions in the Department of Defense.

Saturday, May 2, 1953

After working outdoors in the yard with the kids for a while, I spent some time in my study--in addition to proofing some of the PPR, Volume 14A material, I also worked on a couple of talks coming up.

Sunday, May 3, 1953

Again I played with the kids and worked in my study.

Monday, May 4, 1953

I made a few phone calls, including one to Herman Spindt about some problems relating to the admission of freshman athletes and also one to Ernest Lawrence about the appointment of John Rasmussen as an Assistant Professor in the Radiation Laboratory.

I then responded to Rasmussen's letter of April 17, commenting on the many points he made. I wrote that we were particularly interested in his comparison of the different basic philosophies in the operation of our Berkeley group to Swedish groups with whom he has had contact. In response to his query I told him that the Templetons are not going to Sweden until next year, so he will have sufficient time to pass on helpful information to them. I wrote, "As you may have heard, Ingmar Bergström is coming to spend a month or two in our laboratory beginning in August; so we are all, particularly Fred Reynolds, looking forward to this. I spoke to Professor Manne Siegbahn regarding an exchange arrangement next year involving Stanley Thompson and he was in favor of it." I said that I had passed on his comments about Bob Loftness, whom I met during our 1951 visit to Sweden, to Street and Batzel; but, I explained, they are in the throes of a manpower freeze which perhaps is becoming rather general as part of the economy program of the new administration in Washington. Finally, I added that his suggestion concerning a seminar next fall for students who have had Chemistry 223, covering the rudiments of the strong surface coupling nuclear model and possible applications to experiment, seems like a good one and we should discuss it as soon as he returns. [Iz, I, and our other academic people have been most impressed with John Rasmussen, and we have decided to

support his appointment as an assistant professor in the Radiation Laboratory this year.]

I also wrote to Colonel Markley to acknowledge his letter about James F. Schooley and to inform him that we have asked George Everson, Personnel Director of the Radiation Laboratory, to send him the forms necessary to initiate AEC clearance for Schooley.

A May 1 letter arrived from Donald Lane:

The Patent Compensation Board convened Wednesday, on another docket, and invited me to appear that afternoon.

The Board announced that it was their unanimous opinion that eng 30 was executed conditionally and subject to the condition that a disclaimer would be procured, and that since the disclaimer was not procured, the contract did not and never has become effective. The Board also ruled that an award is not conditioned on ownership on any rights in the inventor at the time he pursues his claim, and that applicants may proceed upon the application for an award regardless of the invalidity of eng 30.

The full text of the announcement and of the discussion which followed is being copied, and copies will be mailed for you and your associates within a few days. The matter has been discussed briefly with Dr. Segrè by phone this morning.

While I am disappointed that the Board should invalidate this contract that everyone for seven years has thought valid and binding, the invalidation will primarily serve to make research people dealing with the Government more wary and disgusted, and may make determination of the value of your inventions somewhat more difficult. On the other hand, it means the Government probably has no license under those inventions made prior to April 1, 1941, and hence makes it important to determine the precise nature of those inventions, as well as the nature of the later inventions.

The Board has directed counsel to identify applicants' contributions, and office of General Counsel to identify its defenses.

In response to my question, Mr. Anderson stated that his office had made very little progress in trying to analyze the subject matter of the claims.

As the matter now stands, General Counsel is to loan to me copies of the patent applications and the original notebooks, and Applicants are to boil down the patent claims to typical groups or representative subject matter, separated if possible with respect to "before" and "after" the April 1, 1941 date. If the General Counsel will agree with our analysis, that would be progress, and if he does not, we will then begin to know what issues need to be proved by testimony.

I expect that I can reduce 196 claims to some reasonable grouping of subject matter, but interpretation of any workbooks or notebooks will

be left to Dr. Wahl and you. I will advise you further after we see what documents are made available to me by General Counsel.

The opinion of the Patent Compensation Board was both a surprise and a disappointment.

I read a nice note from J. W. E. Harrisson and James W. Wilson (Philadelphia Section, American Chemical Society) about my talk before their Section in connection with the John Scott Award on February 19.

Victor O. Schmidt (Commissioner of the Pacific Coast Intercollegiate Athletic Conference) is in town, and I spent some time, including lunch, with him talking about various athletic problems. This was largely a session to get acquainted.

I also managed to get to a talk by Edward Teller about the progress in organizing the Livermore Laboratory.

Al Ghiorso and I relaxed on the Tilden Park Golf Course with nine holes of golf (GTS-48, AG-44).

Tuesday, May 5, 1953

Rod Spence is visiting the area through Thursday; I talked with him for a while and then left him with Stan and Al while I went down to campus for a 10:30 a.m. meeting with Vic Schmidt and others of our Athletic Department.

In today's mail was a letter from Shizuo Fujiwara (University of Electro-Communications, Neguroku, Japan), with whom I have had considerable correspondence. Fujiwara reported that he was been awarded a postdoctoral fellowship by the University of Illinois (to work with Herbert S. Gutowsky), where he will stay for about two years. He wrote, "If it is possible, I should like to study in your laboratory for a few months in the summer of 1954. It would be the supreme happiness for me if you would give me the admission and experimental facilities." Fujiwara then asked for assistance in obtaining an "irradiated unit Sb<sup>125</sup>" to complete the work in hand before leaving Japan since it takes about a year to get such material in Japan. Margie checked with the local AEC office and found there was no mechanism by which he could receive the material as soon as he wants it. She then drafted a letter for me to sign, addressed to Fugiwara with this information and said that we shall be happy to have him use our facilities during the summer of 1954.

Later Rod Spence and I again got together to talk, particularly about the Mike work.

Wednesday, May 6, 1953

This morning I replied to some questions raised by Allan Stoner, a new graduate student, in a May 2 letter. I gave him more details about the position we offered him; I said that, in practice, the position offered is more like a non-teaching fellowship than a job with assigned

work. I also gave him more information about tuition, the non-resident fee (which is usually waived in cases of graduate students who come with and maintain distinguished grades in research and courses--this should apply to him), and housing costs.

I returned to Joe Katz the manuscript and corrected galley proof of PPR, Chapter 8 ("Oxidation States, Potentials, Equilibria, and Oxidation-Reduction Reactions of Plutonium" by R. E. Connick).

Much of my day was spent with Rod Spence and our fellows.

Dana Murdock (a lawyer involved with the reorganization of Mira Vista Country Club) telephoned me about their proposals for memberships for the University. The ASUC will have six full memberships (with a transfer fee). For University staff and alumni, they propose an institutional membership of \$300/month (with no transfer fee) and each staff member and/or alumnus may have a full membership at \$10/month (plus tax) until the Club is full. I telephoned Ian Turner in Chancellor Kerr's office to relate this information, with the hope that Kerr will elect to have the Berkeley campus participate.

In the afternoon a wire arrived from J. R. Cook (du Pont, Wilmington) requesting verification of dates of Richard William Hoff's graduate work (Ph.D. expected August 1953) and my opinion on his character. In a collect night letter I wired, "REGARDING RICHARD WILLIAM HOFF. DATES ARE CORRECT. HAVE HIGH OPINION OF HIS ABILITY. HAVE FOUND HIM DEPENDABLE AND HONEST WITH EVERY INDICATION OF LOYALTY TO UNITED STATES."

Thursday, May 7, 1953

Present at this "open" meeting of the research group were Asaro, Carr, Cobble, Cunningham, Dauben, Glass, Gray, Grover, Gunn, Hollander, Hyde, Kalkstein, Kofstad, Levy, Mathur, Michel, Nervik, Passell, Perlman, Raby, Ramaniah, Seaborg, Shudde, Slater, Smith, Rod Spence, and Stephens.

Kofstad reviewed the work that has previously been done on the spallation of silver with 340 Mev protons. Earlier he reported a chemical separation, which gave a cross section of  $4 \times 10^{-3}$  mb for the production of  $\text{C}^{11}$ . More recently he tried a different chemical separation (basic oxidation of target with hypochlorite ion which allows carbonate carrier to exchange); the yield of  $\text{C}^{11}$  this time =  $1.4 \times 10^{-2}$  mb, which compares with  $7 \times 10^{-3}$  mb for  $\text{Na}^{24}$  and about  $10^{-3}$  mb for such isotopes as  $\text{Mg}^{24}$ ,  $\text{Cl}^{38}$ ,  $\text{Cl}^{39}$ ,  $\text{Co}^{61}$ ,  $\text{Mn}^{53}$ , etc. Kofstad noted that in the case of copper spallation, all the medium-weight products have a yield cross section of  $\sim 10^{-3}$  mb, and the cross section begins climbing at  $\text{Na}^{24}$  and reaches  $10^{-1}$  mb at  $\text{Be}^7$ . I pointed out to Kofstad that it should be remembered that the carbon yield value will be quite seriously affected by impurities, and he described the care he took in the experiments.

Hyde reported some results of the bombardment of copper with 1.2 Bev protons in the Brookhaven Cosmotron by Miller (J. M.) and Friedlander (G.).

Michel presented, in some detail, his results from the separation and

assignment of the isotopes of erbium, thulium, and ytterbium formed in tantalum spallation with 340 Mev protons, using the time-of-flight mass separator. He noted that  $Tm^{165}$  (29-30 hours),  $Er^{160}$  (30 hours), and  $Er^{161}$  (3.5 hours) have not been reported previously. Michel also reported some results from the helium ion bombardments (60-inch cyclotron) of gold; the separation was carried out 24 hours after bombardment so that all the 1.8-hour  $Tl^{198}$  activity had decayed. He saw  $Tl^{198}$  ( $5.3 \pm 0.2$  hour),  $Tl^{199}$  ( $7.4 \pm 0.2$  hour), and  $Tl^{200}$  ( $28 \pm 1$  hour). Michel stated that this indicates that  $Tl^{198}$  consists of an isomer pair of 1.8 hours and 5.3 hours with the 5.3-hour isotope having only two prominent gamma rays.

Mathur described his study of xenon, which has two isomers of mass number 127. He reported that  $Cs^{127}$  can decay to either of these states although the decay to the upper state has not been demonstrated as yet. He attempted to determine the branching decay to the 75-second upper state by observing the gamma rays from  $Cs^{127}$  decay, which was prepared by bombarding  $I^{127}$  with helium ions and then mass separating on the time-of-flight isotope separator. The half-life, previously reported as 5.5 hours, was found to be 6 hours 20 minutes and 6 hours 18 minutes in two separate samples. The  $Cs^{127}$  gamma spectrum showed gamma rays of 380 kev, 125 kev, and 60 kev while a 175 kev gamma ray occurs in the decay of 75-second  $Xe^{127}$ . A reevaluation of previous work indicates there are two positron groups associated with  $Cs^{127}$ , 1.3 Mev and 900 kev energy. Mathur described the device used unsuccessfully to see the 75-second xenon from the  $Cs^{127}$  decay. Earl and I talked about the work, and I noted that Mathur should be able to set a good limit on the branching to the 75-second state, of the order of  $10^{-5}$  or so, from the upper limit of the ratio of 75-second to 34-day  $Xe^{127}$  observed in his experiment.

\* \* \* \* \*

After the Chemistry 223 lecture, I stopped in the Department of Chemistry office.

Friday, May 8, 1953

After the usual number of phone calls, conferences, etc., I wrote a letter to Dean K. S. Pitzer strongly recommending that J. O. Rasmussen be given an appointment as Assistant Professor of Chemistry in the Radiation Laboratory effective July 1, 1953. I wrote in part:

Dr. Rasmussen is a truly outstanding man rating among the first three (D. H. Templeton and K. Street, Jr., being the other two) with respect to overall ability and promise of all our Ph.D. graduates in nuclear chemistry since the war. He is very outstanding, both in experimental and theoretical work, and has a thorough background covering all the important phases of nuclear science. One seldom encounters a man of Rasmussen's overall ability, a fact that is readily evident after a few conversations with him, and he has received flattering offers from some of the main Atomic Energy Commission laboratories...

I went on to describe Rasmussen's thesis work, his independent conception of the nuclear model of the rigid rotator as an explanation

for the existence and energy spacing of the first excited states with spins 2 and 4 which seem to be almost universally present among even-even nuclei. I spoke of his participation within our group, his personality, and the fact that he is spending the current semester as a visiting professor at Nobelinstitutet för Fysik in Stockholm. I included with this letter, a list of his reports and publications. For this appointment I have enlisted the support of both Ernest Lawrence and Ed McMillan.

To Joe Katz I mailed the McGraw-Hill Market Survey for PPR, Volume 14A, and the enclosing letter to Handsfield for his added signature and transmittal. I told him to fill in his own biographical data in Section 5 and add a sheet with his comments, if he desires.

About 10:20 a.m. I telephoned Rod Spence, now back in Los Alamos, and told him that our yield of heavy isotopes on the 1000 pounds of coral from the Mike shot is holding up. Spence said the collection of still larger amounts of coral may be feasible because a sink hole on another island has a slurry that is pretty good or maybe even better than the material we have received so far. Holmes and Narver (a San Francisco outfit) is in charge of the collection procedure and supplies all the labor. Spence said that he will get a sample of this slurry. On another matter he reported they have had no comments yet from Argonne about the draft of Browne's Ivy report. They (Los Alamos) believe our conclusions are correct. Spence and I went on to discuss our Arco neutron bombardment of a heavy element fraction from the Mike coral material processing and also our thoughts on the properties of Cf<sup>251</sup>. With regard to the questions of deletions for the UCRL unclassified version of the report, Spence said this will be settled by Ralph Carlisle Smith (classification officer at Los Alamos) and that he will get in touch with Smith and let us know. I asked about our inclusion of some of this information in our report for the AEC Highlights, and Spence said that he will ask Spof English about this. Spence also requested some cyclotron-produced Pu<sup>236</sup> for use as tracer in their experiments, and I told him that we will send him some in two or three weeks without charge.

In today's mail was the huge transcript of the prehearing conference from Lane, which I put aside to study later.

There was also a letter from our new graduate student, Allan Stoner, stating that he has received an excellent summer job from which he will be able to save enough money for out-of-state tuition and therefore he accepts our offer of employment starting next September. I immediately acknowledged the letter and again explained that the question of whether he will have to pay this tuition is decided by the Graduate Division on the basis of his undergraduate record.

Much of the rest of the day was spent visiting the labs and talking with the men.

Saturday, May 9, 1953

After playing with the kids for a while, I worked in my study--on upcoming talks, on proofs for PPR, Volume 14A, and particularly on studying the material I have received from Donald Lane.

Sunday, May 10, 1953

Again I worked in my study for much of the day, interrupted frequently by active youngsters.

Monday, May 11, 1953

I took care of a number of administrative matters this morning, in addition to talking with Emilio Segrè about Lane's recent communication. Emilio agreed that it might be worthwhile for me to talk with John Francis Neylan.

Doral prepared a response for me to sign to a letter I received Friday from Michael J. Koll (Director of Camping Operations, California Alumni Association), who asked for the names and ages of our children.

In today's mail was a letter from Wayne Meinke (University of Michigan) with some clippings about a reactor they will get. Wayne wrote that by the time it and the new Phoenix Bldg. are set up, they will have top-notch facilities. He added, "When I consider how close I came to quitting here two years ago, I appreciate more and more your advice to stick it out for another semester or two." Wayne said that he is looking forward to the Gordon Conference this year and that he hopes the list of applicants won't be so large that two of his graduate students will have difficulty obtaining a bid to attend.

A number of topics were covered at our noon luncheon meeting of the senior staff, including the handling of reports, the new instructor Bruce McGarvey (who might possibly start his research program by working with Cunningham), the fact that John Huizenga will visit us from Wednesday to Friday, primarily to consult with me on the revision to Chapter 20 of PPR, Volume 14A ("Slow-Neutron and Spontaneous-Neutron Fission Properties of the Heavy Nuclei"), and some matters concerned with equipment for Project Whitney.

I visited some of the labs to talk with the students about the progress of their work.

About 4 p.m. I reached John Francis Neylan by telephone and talked with him for half an hour. I told him about the Patent Compensation Board's adverse ruling. His response was that he thought we had been tricked and that, all in all, the whole legal procedure had been unfair. Neylan and I agreed that I will send him a copy of the Board's decision and Lane's and Anderson's briefs for his review. I told Neylan about the April 1, 1941 retroactive date and its lack of application, especially to chemical work. I also mentioned that the eng-30 contract was already invalid during all the time we have been negotiating. Neylan said that he will be glad to help in any way that he can--after he looks over the documents I am to send him, he will call me or we may get together. At the end of the conversation Neylan summarized, quite accurately, his understanding of the whole history of our research in the physics and chemistry of plutonium and the negotiations, including the significance of the April 1, 1941 date, Vannevar Bush's demand that the University and the inventors negotiate a settlement so the government could use our

discoveries, the early role of Lawrence and the Regents, Sproul's abortive attempt to negotiate with Bush and Lavender's subsequent approach directly to us (the inventors) to seek an agreement.

I then talked with Segrè about the matter. We decided that we should contact Joe Kennedy and Art Wahl, give them our point of view, and seek theirs.

Tuesday, May 12, 1953

The first thing this morning I mailed the documents that I said I would send John Francis Neylan.

I then dictated a long letter to Joe Kennedy:

Emilio and I have discussed the Patent Compensation Board's ruling and feel that there should be an exchange of viewpoints with you people before writing Lane. We are, of course, disappointed and actually quite surprised at this decision. The net result makes it look, one way or another, as if we have received a raw deal in this whole eng-30 matter.

Emilio happened to be in Washington for the meeting of the American Physical Society at about the time of the decision, and he, therefore, had a chance to talk with Lane. He said that Lane was very surprised and seemed to regard it as a very bad ruling, one of a type almost without analogy in his experience. Lane didn't feel that an appeal would be very helpful, thinking of course in terms of an appeal to the Patent Compensation Board. Incidentally, Emilio ran into Smith's wartime helper while he was in Washington (a man by the name of Jaquot), and he was outraged when he heard of the decision.

Before going on to summarize some of the thoughts of Emilio and myself, I should like to mention that I called Neylan yesterday to inform him of the Board's decision since obviously this is a matter on which he must be informed. As you might suspect, Neylan was outraged and felt sure that we had been tricked from the beginning and that the whole matter was a very unfair procedure from the legal point of view. He felt that the disclaimer was attached in the manner that it was, and not referred to in the text, in order that it could eventually be played this way if this should become necessary. He was, of course, unhappy to learn that our protracted negotiations of a couple of years ago were a waste of time since the eng-30 was already invalid because the disclaimer was not signed earlier. I also mentioned to him the alternative procedure suggested by the Patent Compensation Board, namely, to go through claim by claim with respect to the April 1, 1941 date, and I indicated that this was tedious and difficult and of course had already been done in order to arrive at eng-30; I told him that the April 1, 1941 date was not valid from our point of view and explained why. He said that he would be glad to help us in any way that he could and asked if might have a copy of the Board's decision to study. I sent him today a copy of the minutes of the Board's meeting in which the decision was given, together with copies of Lane's brief, Anderson's answering brief, and Lane's comments on the latter. I presume we will hear

further from Neylan after he has studied these.

Emilio and I discussed numerous methods of procedure from here on. I would guess that I am summarizing the conclusion if I should say that we think that we should proceed with the Board's suggestion of analyzing the various claims but keep in mind the strong possibility of pursuing other courses, perhaps in parallel, or after this procedure runs into difficulties and unreasonable delays. This, of course, will immediately raise the question of the April 1, 1941 date, and we believe that this should be contested. Obviously this doesn't apply to Wahl's chemistry experiments. There is also some question as to whether it shouldn't apply to the fast neutron fission, rather than the slow neutron fission, of  $Pu^{239}$ . I am enclosing a copy of the contract NDCrc-201, which pertains to the famous April 1, 1941 date. The wording of this contract is interesting and may offer some possibilities.

The other steps which we discussed have to do with utilizing other channels; for example, we thought in terms of Lewis Strauss, who is acting as President Eisenhower's chief advisor or coordinator on atomic energy matters and whom I know quite well. Another possibility is the Joint Congressional Committee which might be approached, for example, through Senator William Knowland; this approach might possibly be very effective, but would have the disadvantage of possibly leading to very distasteful publicity. There is also the possibility of approaching the Commission itself in some manner. Any of these approaches might antagonize the Patent Compensation Board but, on the other hand, would put the matter on a different plane where the Patent Compensation Board might not any longer hold a determinative position.

This has been only a brief résumé of our thoughts and we look forward to learning your comments.

I returned the corrected galley proof of PPR, Volume 14A, Chapter 17 ("Correlation of Properties as Actinide Transition Series" by G. T. Seaborg) to Joe Katz. In this case there were several changes, including a new figure for Figure 17.1 and additions to Figure 17.4 (adding 104, 105, 106 below hafnium, tantalum, tungsten). I also asked Joe to check some values and references for me. In addition, I mailed Joe the corrected galley to Chapter 10 ("Preparation and Properties of the Compounds of Plutonium" by B. B. Cunningham).

I finally reached Lee DuBridge by phone and talked with him about H.B. 4304, a reorganization plan for DOD that Massachusetts Representative John W. McCormack sent me in April, and the President's own DOD reorganization plan. The latter contemplates an Assistant Secretary for Research and Development, an Assistant Secretary for Applications Engineering, and four other Assistant Secretaries for Information, Finance, Health, and Medicine. McCormack's bill contemplates an Assistant Secretary for Research and Development and, in addition, such an Assistant Secretary for each of the Departments of Army, Navy, and Air Force. After my conversation with DuBridge, I wrote McCormack that I am in sympathy with his objective but I wonder if the President's reorganization plan won't accomplish this along with other worthwhile

aims, and if so, whether it wouldn't perhaps be better to support it now and wait to see how it develops. I added that I certainly feel that something along these lines has become necessary, and it pleases me to learn of his great interest in these matters.

I made my usual tour through some of the labs to see how the research is progressing.

At 6 p.m. I attended a dinner meeting of the Athletic Advisory Board at the Durant Hotel.

Wednesday, May 13, 1953

John Huizenga arrived from Argonne, and we spent the day working on a revision to Chapter 20 ("Slow-Neutron and Spontaneous Fission Properties of Heavy Nuclei") of PPR, Volume 14A. Huizenga agreed to be a co-author of the chapter, along with Winston Manning and me. I also telephoned James G. Beckerley (AEC, Washington) to discuss with him the possibility of including some presently classified spontaneous fission data in this chapter.

Thursday, May 14, 1953

Present at the research group meeting this morning were Behman, Carniglia, Carr, Clark, Cobble, Dauben, Dunlavey, Feay, Glass, Gray, Grover, Gunn, Hoff, Hollander, Huizenga, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Ruben, Shudde, Slater, Smith, Stephens, Surls, Templeton, and Wallmann.

Dunlavey spoke with considerable detail about his efforts to find the expected alpha activity of  $\text{Sm}^{146}$  with nuclear emulsions. The samarium isotopes were produced by intense bombardment of neodymium with 40 Mev helium ions, followed by column separations. He found alpha tracks of four 2.1-2.2 Mev alpha particles, originating from microgram amounts of natural samarium used for mass tracer; and three 2.5-2.6 Mev alpha particles, presumed to be from  $\text{Sm}^{146}$ . His calculations yielded an approximate half-life of  $5 \times 10^7$  years for  $\text{Sm}^{146}$ . Dunlavey added that his alpha energy measurements are probably accurate within 0.1 Mev, but the half-life computation is subject to some approximation and its accuracy correspondingly is reduced by a factor of several.

Gunn talked about their studies of the disproportionation and repropportionation of  $\text{AmO}_2^+$ . To date neither the disproportionation nor the repropportionation has been observed in 1 M perchloric acid. He described the experimental details. In the discussion I asked how much  $\text{Am}^{241}$  he uses in a run, and Gunn replied that he uses 1 to 2 mg. I said that in several years (at the outside) that much  $\text{Am}^{243}$  will be available, free from  $\text{Am}^{241}$ , and much easier to use.

Dauben reported on the x-ray crystal structure of lithium amide ( $\text{LiNH}_2$ ), which was provided by Bill Jolly. She said that, from the intensities of the lines, it appears that the crystal structure is pseudocubic and quite close to the zinc sulfide structure. Templeton added that this is an interesting case because no amide has been carefully worked on as yet. Dauben said they do not know how pure the

sample was. Her patterns for  $\text{NaNH}_2$  and  $\text{KNH}_2$  are similar but not like  $\text{LiNH}_2$ ; she reported her data for  $\text{KNH}_2$  differ rather seriously from data given by George Watt for  $\text{KNH}_2$ .

Passell talked about his work on the decay scheme of the  $\text{Am}^{242}$  isomers as the result of the discovery of conversion electrons of a gamma ray of about 100 kev. He said that it is still remotely possible, however, that these peaks are Auger conversions of the K x-ray from the EC branching. He presented and critiqued possible decay schemes, about which there was considerable discussion. Huizenga added that the beta decay data of the heavy odd-odd nuclides is frequently hard to interpret-- $\text{Pa}^{234m}$  and  $\text{Pa}^{234}$  are particularly difficult.

Huizenga spoke for the rest of the meeting about some studies he is pursuing at Argonne on the competition between decay modes of excited states of heavy nuclei. He said they have been bombarding  $\text{U}^{238}$  and other nuclei with the gamma rays from the University of Illinois betatron and observing the ratio of fission to neutron emission obtained for various maximum gamma energies. He went into some detail about the procedure and their results.

\* \* \* \*

After the meeting, I went up to the hill and again worked with John Huizenga. I also responded to the teletype Beckerley sent me yesterday after our telephone conversation, requesting a list of chapters in PPR, Volume 14A that would be modified by inclusion of presently classified spontaneous fission data. My reply was "ONLY CHAPTER 20 NEED BE MODIFIED. COULD ADD A SINGLE SPONTANEOUS FISSION ITEM TO CHAPTER 3 AND ONE TO CHAPTER 7 IN PAGE PROOF BUT THIS NOT NECESSARY."

I also took a few minutes out to phone Rixford Snyder at Stanford about some athletic matters and to arrange to see him next week.

In the late afternoon Al Ghiorso and I took John Huizenga to Tilden Park for nine holes of golf (GTS-45, JRH-53, AG-48) and conversation.

Friday, May 15, 1953

I again worked with John Huizenga for much of the day.

The report, UCRL-2175 "The Heat of Formation of Thorium Sesquisulfide" by Eyring and Westrum, was declassified without deletion. I signed a note to Ed Westrum informing him of this, enclosing copies of the report for his use, and giving him the reprint order number.

In today's mail was another letter from Norman Bonner with more questions about the Livermore position. He also asked, "Why in Heaven's name was Livermore picked as a location?"

Ellis Steinberg sent a copy of the declassified report about the spontaneous fission products of  $\text{Cm}^{242}$  (this is work he and Larry Glendenin did in Berkeley last summer) he will present at the Gordon Conference in June. Ellis reported that they are preparing a more detailed paper for publication and asked for comments about the material.

About 4 p.m. John Francis Neylan telephoned me (he had received the documents I sent him) and said that he was "sick" after reading the record saying, "You were jobbed." Neylan mentioned that any three reputable lawyers would call this "sharp practice on the part of the Government." I explained to him that the notebooks are in the possession of the Government and he said he saw the difficulty of going back to 1940-41 notebooks. Neylan told me that the Board of Regents will meet next week and that he will insist on a meeting of the "Committee" (The Regents' Committee on Atomic Energy Commission Contracts). He also mentioned that he is not sure what can be done and that he suspects the Government thinks the inventors and Regents are not getting along well. At this point he assured me again that the inventors have the good will of the Regents. Neylan went on to state that he considers that contract eng-30 and the disclaimer were deliberately made "separable." Finally, he asked if I would hold myself available for the Board of Regents' meeting in San Francisco on May 22, 1953, and I agreed. Neylan added that he will try to think of something.

Saturday, May 16, 1953

Much of my day was taken up with the kids.

Sunday, May 17, 1953

Again I played with the kids although I also went over my notes for my talk tomorrow.

Monday, May 18, 1953

A laboratory driver picked me up at home at about 9:15 a.m. and drove me to Pittsburg in time to catch Santa Fe Train No. 60 (Car 600, Seat 25) at 10:28 a.m. We travelled through a heavy valley fog and arrived in Fresno at 1:10 p.m., where I was met by Regent Earl J. Fenston. I had made reservations for the Hotel Californian in Fresno, but Regent Earl Fenston insisted I cancel these and stay in his home instead. I rode with Earl and his wife to Hanford where, at 6 p.m., we attended the annual installation dinner meeting of the Chamber of Commerce. The program consisted of the usual Pledge of Allegiance, Invocation (by Reverend G. R. Meyer), Welcoming Address (by R. E. Wiltshire, President), Installation of Officers (by Judge Clark Clement, Kings County Superior Court, Installing Officer), my introduction by Earl Fenston, and then my talk. I spoke on "Future of Atomic Energy" to the group. The talk was well received. After the question period, we adjourned to the Elks Hall for a dance.



Glenn Seaborg addressing Hanford Chamber  
of Commerce dinner, May 18, 1953

Tuesday, May 19, 1953

In Fresno. I was scheduled to leave Fresno at 8:05 a.m., but the plane (United Flight 35, a Convair) was late. I had scheduled a 1:30 p.m. appointment with Rixford Snyder today, but when I saw that we were going to be late, I telephoned him and cancelled the appointment. Our plane finally left about 10 a.m. and arrived in San Francisco at 11 a.m. I was picked up and driven to the lab, where I arrived about 11:45 a.m.

At the lab I checked my mail. Yesterday a copy of Marty Studier's response to Browne's combined Ivy report arrived, which I read. To my surprise he claimed that the Argonne group had been observing the  $Cf^{253}$   $\beta^- \rightarrow 99^{253}$  decay sequence, with a half-life of 16 days for  $Cf^{253}$  and of

22 days for  $^{99\text{m}}\text{Cs}$  (decaying by the emission of a 6.56 Mev alpha particle)! Actually, they have been adamant in claiming that the 6.6 Mev alpha-emitting  $^{99\text{m}}\text{Cs}$  does not grow from a  $^{253}\text{Cf}$  beta-emitting parent and that there is an alpha-emitting isotope of element 100 of similar half-life and energy to that of  $^{99\text{m}}\text{Cs}$ . We claim that their element 100 isotope does not exist but is an artifact caused by their lack of recognition that the single 6.6 Mev alpha activity keeps growing in from the  $^{253}\text{Cf}$  beta decay. I then looked up Stan and Al to discuss it.

Then about 2 p.m. Stan, Al, and I telephoned Los Alamos and spoke with both Rod Spence and Charlie Browne (a conference call). Spence and Browne were as amazed as we are by Studier's letter. We agreed that the way to handle this change of position by Argonne is to issue the Ivy report without any change in the originally submitted documents. Rod said that he will write Manning to this effect (with a carbon to me). On a somewhat different matter Spence reported on the sludge he mentioned while he was here, saying that it has about the same specific activity as our two-ton sample—he will get some for us on the same basis as before (1/3 of the product to go to Los Alamos). Ghiorso got on the phone and told Browne about the arrangements for shipping the  $^{236}\text{Pu}$  sample to Los Alamos. Stan then spoke to Browne about the results of the MTR bombardment, including our observation of the 6.05 Mev californium alpha activity. Stan said that we will work up the big earth samples from next spring's tests (~4 tons) on the same basis as in the past (1/3 of the product to go to Los Alamos).

I rated as "excellent" a National Science Foundation Research Proposal for Mark G. Inghram on "Mass Spectrometric Investigations" and returned it to Washington.

I also signed a letter that Earl prepared, addressed to Bruce R. McGarvey, the incoming instructor, explaining that we have decided to delay the purchase of a magnet until after he arrives in Berkeley since we have found we can carry the funds forward into the new fiscal year. This way, I wrote, we will be more certain to get a magnet that can serve our mutual interests.

Today's mail had a thank-you letter from Shizuo Fujiwara, who said that he will write us when he is settled at the University of Illinois.

Ron Brightsen wrote again about the Gordon Conference, still requesting 40 to 60 minutes to speak on "Nuclear Shell Structure and Beta Stability." Brightsen said that he will let me know promptly about the Westinghouse fellowship, whose winner will be announced within a week.

Cas (Casimer J.) Borkowski (Oak Ridge and a member of my Chemistry Section at the wartime Metallurgical Laboratory) is spending the week in the area, and I spent some time with him during the afternoon. Cas is an outstanding man on radiation detection instruments of all kinds, and we are offering him the position of Head of the Instruments Group in the Whitney Project at Livermore. However, I am afraid that the salary we are able to offer him (\$925/month) is not sufficient to move him from Oak Ridge.

Wednesday, May 20, 1953

Emilio Segrè got together with me; we talked for quite a while about our patent claim, Neylan's comments, and our future plans. I will again be talking with Neylan on Friday.

Afterwards, I telephoned Greg Engelhard about some athletic matters. I also signed a couple of letters, one being a covering letter to Joe Katz for the return of PPR, Chapter 18 ("Crystal Chemistry of the 5f Elements" by W. H. Zachariasen). I commented that I made no change in this chapter.

I also signed a letter of recommendation, prepared by Earl, to E. V. Kleber (Research Chemicals, Inc., Burbank, California) for M. V. Ramaniah, who is being considered for summer employment. I pointed out that Ramaniah has been with my group for only a few months, but I believe his background qualifies him to fit in Kleber's program.

Later in the afternoon I talked with I. Zartman of the Atomic Energy Commission in Washington about our continuing need for neutron irradiations in the MTR.

Thursday, May 21, 1953

In attendance at the group meeting this morning were Asaro, Behman, Carr, Cobble, Cunningham, Dauben, Feay, Gray, Grover, Hoff, Hollander, Huffman, Hulet, Hyde, Jaffe, Kofstad, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Robinson, Ruben, Seaborg, Shudde, Slater, Smith, and Stephens.

Nervik updated his work on the study of the isotopes of erbium and thulium, which were produced by the bombardment of tantalum with protons in the 184-inch cyclotron. In the first experiment they saw the ~3-hour  $Tm^{163}$  and a ~10-day thulium activity, first thought to be due to  $Tm^{167}$  produced from an impurity in the target; this was ruled out because there is too much activity in the second experiment to be accounted for by an impurity. They then thought the 10-day activity might be an isomer of  $Er^{161}$  fed by  $Tm^{161}$ , but this didn't show up on an experiment with the time-of-flight mass spectrograph. In the latter experiment they saw 700 Nucleometer counts of  $Tm^{163}$  above the background with a half-life of ~3 hours. The erbium decay curve was resolved into two half-lives of ~4 hours and ~50 hours. (In these cases the Nucleometer was not working well.) Nervik presented possible decay schemes and said the work is continuing.

Robinson described a method they have developed for ruling quartz with a diamond point (the Bureau of Standards has reported this is virtually impossible). Robinson said they desired lines certified to a distance of 0.1 micron and found they could come close to achieving this when they evaporated 10-20 microgram/cm<sup>2</sup> of lead onto the quartz surface and then heated the quartz to red heat in air before they ruled the lines onto the surface. The purpose of the lines was to calibrate a traveling microscope.

Hulet told about his bombardments of targets containing 90%  $Am^{241}$  and

10% Am<sup>243</sup> with 27 Mev helium ions. The first bombardment was for 5 hours and the second for 15 hours. They followed the decay of the purified berkelium fraction with the Nucleometer; in a second bombardment they followed the decay of the K x-rays with the 50-channel gamma analyzer and also again followed the gross decay with the Nucleometer. They found an approximately 1.9-day half-life, which is believed to be due to Bk<sup>246</sup>. The ratio by mass of Bk<sup>246</sup> to Bk<sup>245</sup> (5 days half-life) was ~1/4. I mentioned that Bk<sup>246</sup> can decay by beta emission as well as electron capture and asked what energy is predicted from closed cycles for beta emission; Glass said the energy for beta emission is ~20 kev while the energy for electron capture is ~1.6 Mev.

Jaffe said that, as the result of a discrepancy in energy values measured in our bent crystal spectrometer for uranium L x-rays, they checked the K x-rays of silver and then had a new and more accurately graduated "arc" (bent crystal) made in the machine shop. Using this they found the energy of U L<sub>B1</sub> to be 17.24 ± 0.02 kev, compared with the literature value of 17.22 kev. Thus, it appears that the previously observed "shift" was actually instrumental in nature.

Glass reported some changes in the closed energy cycles, noting that the UZ ground state of Pa<sup>234</sup> is about 400 kev below the isomeric state (UX<sub>2</sub>) and the total disintegration energy of Np<sup>237</sup> now appears to be more than 100 kev greater than the observed alpha energy.

I added that the Canadians have measured the half-life of Pu<sup>241</sup> as 13.0 ± 0.5 years by mass spectrographic analysis; this agrees well with our value of 14 years.

\* \* \* \* \*

The Chemistry 223 students turned in their term papers today. I stopped in the Department of Chemistry office before going up to the hill and found a memo from Clark Kerr requesting that I serve as a member of the Committee on Television for the academic year 1953-54. The Committee is chaired by Dr. Donald Coney (Professor of Librarianship). This should be an interesting committee on which to serve.

I also received the first ten galleys of my chapter "The Actinide Series" for M. Cannon Sneed's book, Comprehensive Inorganic Chemistry. Some of the office staff will read it before I go over it. Sneed wrote that he will be happy to send me page proof if I desire it.

In a letter addressed to Iz, Kay Way, Coryell, and me, Truman Kohman, in his usual manner, sent a list of five ground rules for the three sessions on Nuclear Disintegration Systematics at the 1953 Gordon Research Conference on Nuclear Chemistry in order to let each of the discussion leaders know just what he can or should do to round out the program. Truman asked for a prompt reply, and I immediately wrote that I am in agreement with his letter. I also gave him the title of Frank Asaro's talk ("Complex Alpha Spectra") and the title for John Huizenga's talk ("Heavy Element Fission").

C. B. Holden (Fresno Sub-Section, California Section, ACS) invited me on May 20 to speak before their group some time in October, November, or

December). Again, I immediately replied and declined the invitation, citing my extremely heavy schedule.

Bill Rice, in response to my request, provided me with a detailed statement of the facts concerning the design, specification, construction, and subsequent difficulties concerning the roof of our home in Lafayette. I mailed this, along with a covering letter, to the Registrar of Contractors in Sacramento today. [After talking with H. Rodde, our contractor, I have come to the conclusion that the real difficulty lies more with the roofing subcontractor than with Mr. Rodde. Rice's evaluation seems to confirm this.]

From 3 until 3:30 p.m. I had a conference with W. B. Reynolds to discuss the problems we are having recruiting men for Project Whitney because our salaries are too low. I agreed to provide him with a list of men we have lost because of this.

Friday, May 22, 1953

This morning I sent a note to Karl-Erik Zimen, presently at Yale University, to suggest that perhaps we can see him at the Gordon Research Conference on Nuclear Chemistry to be held in New Hampshire during the week of June 22. I enclosed a copy of the announcement in the unlikely event that he hadn't heard of the conference.

The abstract of my paper on "Oxidation States of the Actinide Elements" for the fall symposium of the ACS Division of Chemical Education was mailed to Professor Jacob Kleinberg (The University of Kansas), along with a note saying that I would rather not have the talk transcribed nor have any steps taken toward its publication.

Much of the rest of my day was spent proofing the Sneed article.

John Francis Neylan telephoned me about 11 a.m. and said that he has given a lot of thought to the whole patent matter and that he had talked with some of the members of the Regents' Committee on Atomic Energy Commission Contracts, which was having a luncheon meeting at 12:30 p.m. today. [Neylan asked me last week to hold myself available to attend this meeting, but he decided this was unnecessary.] He again mentioned that they will try to be as helpful as possible to us without going any further than the Regents already had in giving up the "rights of the University." Neylan said that he has found, from conversations with Underhill, that Lavender had tried to trick the Regents twice in this matter (and had lied to them in another case). In reply to his question about whether the inventors had employed a lawyer to get contract eng-30 (stating that this was important), I read him the supplementary agreement (Modification No. 1 of eng-30) of September 21, 1945 and said that, as far as I can remember, Lavender approached us first. I also read him Lavender's letter of August 15, 1944 to me, which impressed him very much. Neylan said that he will try to get a subcommittee (which might include McLaughlin and himself) to look into this. Neylan concluded that perhaps all the Committee can do is to give us moral support in the form of a letter to the Atomic Energy Commission.

Later in the afternoon I sent Neylan a copy of Modification No. 1

(Supplementary Agreement of September 21, 1945) [attached] and a copy of the following correspondence: letter of August 10, 1944 from Seaborg to Lavender; letter of August 15, 1944 from Lavender to Seaborg [see Journal of December 19, 1950]; letter of August 31, 1944 from Seaborg to Lavender [attached]; letter of September 11, 1944 from Seaborg to Lavender [attached].

C O P Y

CONTRACT NO. W-28-094-eng-30

MODIFICATION NO. 1

SUPPLEMENTAL AGREEMENT

THIS SUPPLEMENTAL AGREEMENT, entered into this 21st day of September, 1945, by and between THE UNITED STATES OF AMERICA, (hereinafter called the "Government") represented by the Contracting Officer executing this agreement, and DRS. JOSEPH W. KENNEDY of Santa Fe, New Mexico and Berkeley, California; GLENN T. SEABORG of Chicago, Illinois; ARTHUR C. WAHL of Santa Fe, New Mexico and Berkeley, California; and EMILIO SEGRE, of Santa Fe, New Mexico and Berkeley, California, (hereinafter referred to as the "Inventors").

WITNESSETH THAT:

WHEREAS, on the 17th day of September, 1945, the parties hereto entered into Contract No. W-28-094-eng-30 for patent rights; and

WHEREAS, it is found advantageous and in the best interests of the Government to modify the said contract as hereinafter provided; and

WHEREAS, this modification is authorized by the First War Powers Act, 1941, and Executive Order No. 9001.

NOW THEREFORE, the said agreement is hereby modified in the following particulars, but in no others:

Add the following paragraphs to the Agreement:

"8. No member of or delegate to Congress or resident commissioner shall be admitted to any share or part of this agreement or to any benefit that may arise therefrom, but this provision shall not be construed to extend to this agreement if made with a corporation for its general benefit.

9. The INVENTORS warrant that they have not employed any person to solicit or secure this agreement upon any agreement for a commission, percentage, brokerage, or contingent fee. Breach of this warranty shall give the Government the right to annul the agreement, or, in its discretion, to deduct from the agreement price or consideration the amount of such commission, percentage, brokerage, or contingent fees.

CONTRACT NO. W-28-094-eng-30

MODIFICATION NO. 1

This warranty shall not apply to commissions payable by INVENTORS upon agreements or sales secured or made through bona fide established commercial or selling agencies maintained by the INVENTORS for the purpose of securing business."

IN WITNESS WHEREOF, the parties hereto have executed this Agreement as of the day and year first above written.

THE UNITED STATES OF AMERICA

BY (K. D. Nichols (Signature))

\_\_\_\_\_  
Witness

\_\_\_\_\_  
Joseph W. Kennedy

\_\_\_\_\_  
Witness

\_\_\_\_\_  
Glenn T. Seaborg

\_\_\_\_\_  
Witness

\_\_\_\_\_  
Arthur C. Wahl

\_\_\_\_\_  
Witness

\_\_\_\_\_  
Emilio Segré

copy

Metallurgical Laboratory

August 31, 1944

Capt. Robert A. Lavender  
Advisor on Patent Matters, O.S.R.D.  
1530 P Street, N. W.  
Washington, D. C.

Dear Capt. Lavender:

I am enclosing the definitions of our inventions together with photostats of our personal records for your custody and use on a confidential basis for the purpose of the discussions which we hope to have with you in the near future. The enclosures consist of two copies of the patent application entitled "Methods for Use of Compositions of a Radioactive Isotope" with Segre, Kennedy, and Seaborg as the inventors (copies 1 and 2 of MUC-GTS-941) together with two photostatic copies of four excerpts from our personal records to substantiate this invention (copies 1 and 2 of MUC-GTS-965), and two copies of the patent application entitled "Methods for Preparation of Compositions of a Radioactive Isotope" with Seaborg, Wahl, and Kennedy as the inventors (copies 1 and 2 of MUC-GTS-942) together with two photostatic copies of seven excerpts from our personal records to substantiate this invention (copies 1 and 2 of Series A).

In reference to your letter to me of August 15, 1944, it is the belief of both the inventors and the University that an early conclusion to this matter can be reached only by means of a discussion involving these two parties and your office. Therefore it is still our hope that it will be possible for Dr. Kennedy, Mr. Underhill, Mr. Conard, and I to have this discussion with you and Dr. Bush in Washington in the near future. It is in view of the difficulty of getting together so many people, all very busy with tight time schedules, that we tentatively suggested rather far in advance the days Friday, September 15 and Saturday, September 16 for your possible approval.

Sincerely yours,

GTS:EES

c/c to V. Bush

J. W. Kennedy - E. Segre - A. C. Wahl  
R. M. Underhill - A. H. Conard

G. T. Seaborg

September 11, 1944

Capt. Robert A. Lavender  
Advisor on Patent Matters, C.S.R.D.  
1530 P Street, N.W.  
Washington, D. C.

Dear Capt. Lavender:

Since we have received no confirmation from you in respect to our proposal for a meeting of Dr. Kennedy, Mr. Conard, and me with you and Dr. Bush at the end of this week, we have proceeded on the assumption that this meeting will not be held. In view of the distances that some of these people would have to travel in order to get to this meeting by the proposed time it was necessary for us to make a decision on this question no later than today.

Sincerely yours,

GTS:EES

c/o to V. Bush

G. T. Seaborg

J. W. Kennedy - E. Segre - A. C. Wahl  
R. M. Underhill - A. H. Conard

Saturday, May 23, 1953

I continued working on the proof for my Sneed chapter. This is Chapter 3 entitled "The Actinide Series" for Volume One of the series of volumes Comprehensive Inorganic Chemistry, edited by M. Cannon Sneed, J. Lewis Maynard, and Robert C. Brastad. The other chapters are "Principles of Atomic and Molecular Structure" by W. N. Lipscomb and "Theoretical and Applied Nuclear Chemistry" by P. R. O'Connor.

Sunday, May 24, 1953

In addition to working on the proof of the Sneed manuscript, I looked over some of the papers submitted by the Chemistry 223 students. Papers this year include such titles as "5g Orbitals in Atomic Structure" by Peter R. Gray, "Nuclear Radii Obtained from Different Nuclear Phenomena" by LeRoy Green, "The Development of the Nuclear Shell Model" by Ralph McLaughlin, "Information on Nuclear Spectroscopic States from Reactions of the d,p Type" by H. B. Mathur, "Non-Zero Value for Delta for Odd A Nuclei in the Mass Equation" by D. A. Pickler, "The Mechanism of High Energy Reactions" by Bruce Raby, "Internal Pair Formation" by Rex Shudde, "Mesic Atoms" by Warren G. Smith, "Isomerism and Excited States in Even-Even Nuclei" by Frank Stephens, Jr., "Multipolarity of Nuclear Radiation from Internal Conversion Coefficients" by Joe P. Surls, and "Measurement of Fission Fragment Kinetic Energies" by Vernon Troutner,

Bernard Harvey telephoned to report that he arrived in Berkeley last evening, after driving with his family from Chalk River, and checked into the Durant Hotel. He will come up to the lab tomorrow and go through the formalities of employment.

Monday, May 25, 1953

After the usual phone calls, including calls to Greg Engelhard and Bill Rice, I replied to Norman Bonner's latest queries about the position in Livermore, saying that the offer we made was as definite as it sounded in my first letter, but the "discouraging" follow-up was prompted by his reference to a "ten-foot pole" without amplification. I wrote that I was concerned that he should have no illusions as to the nature of the job. I then went on to say the Livermore Laboratory was established as a permanent AEC installation, and we here assume that it will be such. However, any one of the possibilities he mentioned could conceivably occur, but I think the chances that any will happen are very small. I then gave Bonner more details of the setup and said that, if he would like a firm offer, I will request our Personnel Department to make one on about the basis I discussed previously. [Apparently Bonner's concerns are not as serious as he stated in his correspondence and he really wants a job at the Livermore Laboratory.]

A letter also went to Winston Manning this morning, noting that we see from Studier's letter of May 13 to Browne that his group has finally come to agree with our contention that  $99^{253}$  is supported by a beta-emitting  $Cf^{253}$  parent. I also commented on our other points of disagreement in Studier's letter. Finally, I added that I would like to stop at Argonne for a visit on Friday, June 26, 1953 on my way home a day early from the Gordon Research Conference, if this is agreeable with him.

In today's mail was a May 21 response from Art Wahl to my letter to him and Joe Kennedy about the Patent Compensation Board's ruling. Art wrote:

Joe's and my reactions to the Patent Compensation Board's ruling were similar to yours and Emilio's. We were surprised and disappointed at first, but now after studying the transcript of the prehearing conference we are more optimistic and feel the Board may actually still be sympathetic toward us and be in a mood to get things settled. We agree with you that we should proceed with the Board's suggestion of analyzing the various claims. We believe it would be best at the present time not to chance antagonizing the Board by using other channels to go around and over them. Later if things bog down some of your high level contacts should help greatly to expedite matters.

If, as it appears to us, the Board is in a mood to settle the issues without haggling over legal technicalities, we favor not making an issue of the April 1 date at the present time. (Lane might want to reserve it for a possible point of contention at a later date.) Settling the question of the April 1 date would be time consuming, and may not be very important to our case. Even if the government should claim the April 1 date applies to the chemical experiments, and to my knowledge it didn't, the composition of matter and chemical process claims are essentially all based on work done before April 1. Later work may, of course, help support these claims, but as I remember (Stuart W.) Scott found a basis for nearly all such claims in my early notebooks. The later supporting experiments might be of importance in a patent case processed by the usual legal procedures, but may be of little importance in an award application.

As Joe remembers, the contract was for the fast neutron experiments, not the slow; in any case he says the slow neutron measurements were made two days before April 1.

I also received a nice note and a check for \$75 from C. F. Dickinson (Secretary-Manager, Hanford Chamber of Commerce) for my talk there last Monday night.

Charlie Browne wrote to request a copy of Dean Dunlavey's thesis so that they may try employing his methods to detect alpha fine structure, etc. in their planned experiments with the nuclides of recent interest. Browne also mentioned that he replied to Studier's May 13 letter, outlining their policy in regard to additional data. Charlie said that he believes I will find from my copy of the letter (which I received today) that the policy expressed is in essential agreement with my opinion on the matter. I immediately had Doral send a copy of Dunlavey's thesis to Browne.

Also in today's mail was a letter from Bob (Robert A.) Naumann at Princeton, thanking me for our hospitality last summer and informing me that he is sending, under separate cover, a copy of his thesis. Naumann said that he has accepted an offer of one year's employment at Princeton.

At 11 a.m. John Francis Neylan telephoned to say that he discussed

the whole matter at length with the Committee on Atomic Energy Commission Contracts both at the luncheon meeting on May 22, 1953 and after the Board of Regents meeting. Neylan said that both committee meetings were attended by Brodie E. Ahlport, Edwin W. Pauley, Robert G. Sproul, Edward A. Dickson, and himself and that Donald H. McLaughlin and Arthur J. McFadden were absent. Neylan reported their conversations boiled down to the following: (1) the Committee has complete sympathy with the inventors, (2) they wonder how they could get into the matter. They wondered what we proposed to do. If we appeal and the Patent Compensation Board is reversed, the inventors can, of course, go back to contract eng-30. On the other hand, if we lose our appeal, we can then go back to review the early negotiations. In that case the University could be logically brought in to help show the bad faith of Lavender in all the early dealings.

Neylan told me that he will confer (as an appointed subcommittee) with Segré and me and our associates if we wish. I told him that we have now heard from Kennedy and he and Wahl favor following the Patent Compensation Board's recommendation to carry on a claim analysis. I also mentioned Segré's idea of contacting Lavender. Neylan said he thought that Lavender would have to admit that the disclaimer was a "separable document" to preserve his own self-respect. Neylan again mentioned that the Regents can logically come in to help only on the basis of some kind of rehearing in which perhaps the records could be obtained by subpoena. He also pointed out that on page 88 of the transcript the statement "in the absence of any such evidence" was bad, implying a sort of procedural mistake by the Patent Compensation Board. At this point I mentioned again the possibility of the Regents helping us about the April 1, 1941 date, and Neylan implied they will be as helpful as possible. I also said that I want to have Segré included in any possible conference with him (Neylan), and Neylan agreed. Finally, I said that Segré and I will call him as to whether we want to confer with him, saying that we may go ahead as the Patent Compensation Board suggested until we hit a snag.

The senior staff had its usual luncheon meeting, during which we welcomed Bernard Harvey, in my office at noon.

Bernard Harvey



After lunch I got in touch with Segré to tell him about my conversation with Neylan.

Tuesday, May 26, 1953

This morning I talked with some of the men and made a few phone calls, including one to Greg Engelhard and one to Bill Rice, and then

returned the corrected galley proof of Chapter 3, "The Actinide Series," of Sneed's Comprehensive Inorganic Chemistry to him. This chapter was written some time ago and, although Katz helped me update it about a year ago, there were many changes and corrections in the proof, so I suggested that it might be advisable for Sneed to send me page proof. I also enclosed a revised drawing for Figure 2 (Relative elution of homologous actinides and lanthanides) and asked that he substitute it for the one he now has. [The information hasn't been declassified yet, but we now know the elution positions of elements 99 and 100 so they could be drawn in better than the dotted predicted positions.

At 11:45 a.m. Winston Manning telephoned to tell us that Sherman Fried is making a circle tour and will stop in Berkeley for two days (June 1, 2, or 3) to discuss the handling of the Mike matter. Winston said that Sherman is still involved in the matter and will come prepared to represent the Argonne group. He also said I am welcome to stop in on June 26 although he may be out of town. On a less controversial matter, Winston told me that Larry Glendenin, Paul Fields, John Huizenga, and Ellis Steinberg plan to attend the Gordon Conference in June.

After reporting on this conversation to Stan and Al, I went down to campus to meet at 1:30 p.m. with Rixford Snyder (Faculty Athletic Representative at Stanford) and discuss, among other things, my plans for handling the Pat Cannamela case at the Winter Meeting of the PCC.

I went back to the hill and had, at 2:15 p.m., my regular physical examination with the lab doctor, Frederick M. Hebert, M.D. My blood pressure was 134/80.

Wednesday, May 27, 1953

After receiving Wahl's letter the day before yesterday and consulting with Segrè, I wrote the following to Lane today:

Following your letter of May 1, 1953 and your letter of May 4, 1953 with the enclosed reporter's transcript of the prehearing conference of April 29, 1953, Segrè and I have conferred and also have corresponded with Kennedy and Wahl on the matter. We were quite surprised and disappointed at the decision and feel that it was an unjust one.

Although we have discussed other possibilities which I shall mention later, we have concluded that it is probably best to go ahead with an analysis of the claims, as the Patent Compensation Board suggests, despite a feeling on our part, on the basis of previous experience, that this may be a long process. This should show whether it is possible to obtain fair cooperation from the AEC or whether we will meet sharp legalistic opposition. Thus, we should like for you to get copies of the patent applications and the original notebooks as soon as possible in order to start separation of the patent claims with respect to before and after the April 1, 1941 date. I should like to suggest that when you need the help of the inventors in this process you deal directly with Drs. Wahl and Kennedy, at least at first, in the interest of saving time. We should also like to emphasize again that April 1, 1941 is a fictitious date, especially

so far as the chemical experiments (Case S-52) are concerned since this work received no government support during the whole of 1941. Even with respect to Case S-61, there is some question as to whether it shouldn't apply to the fast neutron fission rather than the slow neutron fission of  $Pu^{239}$ . You can see from the attached [see Journal January 11, 1952] copy of contract No. NDCrc-201, which pertains to the famous April 1, 1941 date, that the wording deserves some study. In the interests of saving time, we do not favor making an issue of the April 1, 1941 date at the present time since the crucial experiments were done before this date. However, the notebook documentation as pertaining to Case S-61 is much better a little after this date and, of course, a number of the detailed chemical experiments took place during 1941 after this date. I don't know just how it would be done, but we would like you to bear in mind the possibility of making some sort of reservation for a possible later point of contention on this April 1, 1941 date.

As mentioned above, we have considered other possibilities, very likely some of them along the same lines as your own thoughts. Without delaying the process of going ahead to analyze the claims as suggested by the Board, perhaps some of these courses could be quietly pursued or at least discussed in parallel.

If Contract eng-30 is invalid, I suppose that it would follow that the Government has no right to our assignments. The question then arises whether we should ask for the return of these assignments and we think that this should be left to your judgment.

A possibility that shouldn't be overlooked is that of contacting Lavender to ascertain whether he regards the Board's ruling as being in the spirit of the original agreement. It might appear that the ruling places him in a bad position and in order to preserve his self-respect he might be willing to testify that the disclaimer was a "separable document" used for the purpose of trying to get the University's signature but in a manner which would make it possible to make a contract with the inventors in case agreement with the University should prove impossible. (It is very interesting here to note the last paragraph in Lavender's letter of August 15, 1944 to me of which I am enclosing a copy together with the attendant correspondence.) In this connection the opinion of some other members of Lavender's organization at the time that the contract eng-30 was negotiated and signed might now also be of interest: for example, Segrè during his recent trip to Washington happened to run into Jaquot, who was a wartime assistant of Ralph Carlisle Smith at Los Alamos and now with C.I.A. and who is acquainted with the contract eng-30 negotiations, and he was deeply shocked to hear of the Board's ruling. It might be interesting to learn the reactions of Scott who helped intimately in the negotiations surrounding contract eng-30; I am not certain of the whereabouts of Scott, the last I heard years ago being that he was somewhere at Oak Ridge. You probably know where Lavender can be reached and if you feel that the idea has any merit, we should be glad to have you discuss the matter with him.

In the course of our discussion about the possibility of some kind of

an appeal, it has occurred to us that it might be taken up with people beyond the Patent Compensation Board. For example, although it didn't seem worthwhile calling this confused patent position to the attention of those concerned with the recently initiated drive toward greater industrial participation because the matter seemed on the road to settlement, it might now be almost a matter of duty to point out in the proper places that the patent position is clouded. Perhaps the Joint Congressional Committee or the Atomic Energy Commission itself should be approached or perhaps the matter should be discussed with Lewis Strauss, who is acting as President Eisenhower's chief advisor or coordinator on atomic energy matters. Any of these approaches might antagonize the Patent Compensation Board, but on the other hand, it would probably place the matter on a plane where the Patent Compensation Board might no longer hold a determinative position. Perhaps these procedures should be thought of more in the light of recourses to be resorted to in case the claim analysis procedure proceeds at an intolerably slow pace, as we frankly suspect that it will. However, any such procedure would have the disadvantage of possibly leading to very distasteful publicity.

Since this was a matter on which he must be informed, I called Neylan to acquaint him with the Patent Compensation Board's decision. He was outraged and feels that the whole handling of the contract eng-30 negotiations by the Government was a case of sharp and unfair treatment of the inventors by the Government. He called a meeting of the Committee on Atomic Energy Commission Contracts of the Board of Regents attended by Board Chairman Dickson, Regents Ahlport, Pauley, and Neylan, as well as President Sproul, and they discussed the matter in some detail. The net result was that they would like to be as helpful as possible to us, within the framework of our negotiations with them. Neylan said that it is difficult to see how the University could get into the matter as it now stands, but if, for example, we were to go into some kind of an appeal procedure, the University could be brought in to show the bad faith of Lavender in the early negotiations. I mentioned to him the possibility that the University might be helpful with respect to the real intent of the April 1, 1941 contract date, and he indicated that they would be as helpful as possible if this became an issue. He has said that he stands ready to discuss the matter with Segre and me if we think that this would be worthwhile, and it is quite possible that we will do this although we have not yet decided whether we think that this would be profitable. Incidentally, Neylan seemed to be especially impressed by the phrase "in the absence of any such evidence" on page 88 of the transcript, also by the Supplemental Agreement of September 21, 1945 (Modification No. 1), also by Lavender's letter of August 15, 1944 to me and again by the concept of a "separable disclaimer" attached to eng-30.

In summary then, we should like you to proceed as rapidly as possible with the Board's suggestion that we undertake analysis of the patent claims with respect to the April 1, 1941 date, and, in parallel, for you to consider with us other possibilities.

I mailed a copy of the letter with its enclosures to Wahl and Kennedy.

Also, during the morning I telephoned Steve Lawroski at Argonne to tell him that we are interested in hiring Martin J. Steindler because of his hydride training. Steve said that he will confer with Feder (who is apparently not interested in our offer to him of the position of Head of Chemical Engineering at Livermore, possibly because the salary of \$800/month is too low) and Richard C. Vogel and call me back tomorrow.

In other correspondence today I signed a letter that Earl prepared, addressed to Dr. Jagdish Shankar (Chemist, Atomic Energy Commission, Bombay, India), describing the status of M. V. Ramaniah.

Thursday, May 28, 1953

This morning's meeting was attended by Asaro, Behman, Carr, Max Chapman (a health chemist), Clark, Cobble, Cunningham, Dauben, Glass, Gray, Grover, Gunn, Harvey, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Reynolds, Ruben, Seaborg, Shudde, Smith, Stephens, and Templeton.

Asaro reported on some measurements on the gamma rays in the decay of a purified sample of  $U^{232}$ , obtained from California Research and Development Corporation. He spoke of the alpha spectrum and said, by analogy with other even-even nuclei, they expected to see a 58 kev and a 131 kev gamma ray, both E2 transitions. They observed gamma rays with energy of 57 kev and 132 kev; preliminary experiments indicate both are E2 transitions. In response to a remark by Perlman, Asaro reviewed the results on  $U^{230}$ . There followed considerable discussion about parity states in even-even nuclei.

Gunn described work, using a two-compartment cell, on the electrolytic oxidation of americium. He went into details about three runs. In the third experiment, for example, they started with 4 mg americium in 0.9 ml of 1 M nitric acid. The starting current was 100 milliamps. After 20 minutes the current fell off, and at 30 minutes it was down to 40 milliamps, and after 40 minutes it was only 20 milliamps. At this point the oxidation to Am(VI) was  $98 \pm 2\%$  complete. He went into some detail about the extraction procedure. In response to a question from Hyde, Gunn said he believes this method of separation can be used with tracer amounts of americium and curium. Harvey, attending his first meeting, asked if the oxidation would work in the presence of a large amount of curium, and Gunn said that it probably would although they may get less complete oxidation because of the presence of more hydrogen peroxide from the alpha decay.

Grover reported on their progress on  $4\pi$  counting with liquid scintillation phosphors. He described the setup and said they want to dissolve the activity directly in the scintillator; therefore they need a phosphor solvent that will be miscible with water. Kalkstein added that the objective is 100% counting for absolute beta counting. I pointed out the advantage in counting alpha particles by this method is much less than for beta particles.

Kalkstein also reported on some work being done at Los Alamos on an attempt to detect neutrinos from the decay of fission products formed in the Los Alamos reactor.

\* \* \* \* \*

There was the usual Chemistry 223 lecture, and I stopped in and talked with some of the people in the Department of Chemistry before going up to the hill.

Steve Lawroski called me back as he promised yesterday. With regard to Steindler, Steve said that he is expected to report for work at Argonne on June 30 and that he (Steve) would be unhappy if we were to interview him. I gave him some of the background for the urgency, and then Steve said that he will talk with Steindler about it; Steve suggested I call back next week.

I then telephoned and talked with H. P. Everest (Faculty Athletic Representative, University of Washington) about the Cannamela case

Later I signed a memorandum to W. B. Reynolds, listing the men (and their positions) we have attempted to recruit for Project Whitney, but who have turned us down, primarily because of salary. I noted the possible acceptable salary: Clark Egan (Head of Cryogenics), offered \$1000/month, possible acceptable salary \$1300/month; D. F. Peppard (Associate Director of Chemistry), \$900, \$1200; H. M. Feder (Head of Chemical Engineering), \$800, \$1000; C. J. Borkowski (Head of Instruments Group), \$925, \$1100.

At home I received an acknowledgment to my May 21 letter to the Contractors State License Board from N. J. Morrisey (Registrar of Contractors), stating my letter had been referred to the District Office in San Francisco.

Friday, May 29, 1953

Much of my day was spent talking with various people about their research.

On May 25 Loren V. Kottner (Assistant Director, Wilson Compton Union, State College of Washington) wrote that Professor George B. King requested a reservation for me on the night of June 12 in the Wilson Compton Union in connection with my upcoming talk there, and he (Kottner) has reserved a single room. I promptly wrote Kottner that I plan to arrive via Pullman around noon.

Other notes went to William W. Rubey (Chairman, National Research Council) in response to his letter of May 8. I explained that, because of my heavy schedule, I am not planning to attend the XIIIth International Congress of Pure and Applied Chemistry in Stockholm this summer and therefore cannot represent the National Research Council.

I also wrote and thanked Earl J. Fenston for the clipping from the Hanford Sentinel (of which he is publisher) that I received Wednesday about my visit there. The following article appeared on the front page of the May 19 paper.

## Nobel Prize-Winner

# Public Must Be Informed On Science, Seaborg Says

One of the world's great chemists spoke in Hanford Monday night on the future of atomic energy.

Barely seven hours after he finished his talk here, he, and countless thousands of others in this area, were jolted out of their sleep by the latest blast of atomic fission some 400 miles distant.

Dr. Glenn Seaborg of the University of California, spoke realistically of the harnessing of atomic energy. He brought his subject down to the blunt details of what can be expected of science in controlling such energy for useful purposes.

### Speaks at Chamber Dinner

The scientist's talk was delivered before 200 persons attending the annual Chamber of Commerce dinner.

Dr. Seaborg warned his listeners he felt it is "necessary for the American public to learn a great deal more about science and such matters than it knows today. If we are to solve the great problems

— we have to know a great deal more as to what it is all about."

Taking from his pocket a small cube, perhaps an inch square, Dr. Seaborg held it up before the crowd and remarked that if this represented one pound of uranium — and if all of it were to undergo fissionary action to be converted into plutonium—it would liberate energy equivalent to 2,000 tons of coal, or ten million kilowatt hours of energy.

### Many Conversion Problems

The problems involved in effecting such a conversion are many, according to Dr. Seaborg, and to get an atomic pile, or nuclear reactor to provoke such a reaction poses formidable difficulties.

For one thing, atomic energy releases tremendous radioactivity and therefore shields must be used for protection. Thus, perhaps 100 tons of shielding, such materials as concrete or lead, are needed for protection.

This would make use of atomic energy in running mobile vehicles or automobiles virtually impossible. This is not true, however, of ocean-going vessels and atomic powered craft are under experimentation now. It also may work for airplanes.

### Problems of Engineering

There are other great difficulties in obtaining fission for peaceful uses. Most of these problems are ones of engineering, and they are without precedent.

For example, what structural material would be used, since all ordinary materials won't work, as they absorb neutrons and thus kill the reaction.

There is the heat transfer problem—extracting the great amount of heat from a piece of material of tiny dimensions. Atomic energy is conversion of heat and it cannot be done by conventional methods.

### Slow Heat Development

There is the problem of control itself, to develop heat slowly, as in a machine. "We must be careful to control it, so it is foolproof and will not go off as an atomic bomb."

There is the problem of chemical separation. In a sense, then, the ashes of the fire must be removed, too, because they accumulate. The question of waste disposal is a tremendous one, Dr. Seaborg asserted.

As to prospects for the future: The speaker said the consensus is that these problems will be solved—but not in the immediate future. Perhaps there will be large-scale plants to develop atomic energy, but it may be two decades before such power comes from fission and not from ordinary chemical sources.

### Industrial Proposals Made

Proposals have been made by private industrial leaders whereby they might lease uranium from the government, convert it to plutonium and sell it back after developing the energy, according to Dr. Seaborg.

As to whether the economics of atomic energy, the chemist said he couldn't answer this question. "It might be possible it will — it depends upon the availability of the raw materials." No price can be put on it, he stated.

Dr. Seaborg then traced the possibilities in medicine and peacetime uses and its use in radioactive tracers, in science, agriculture, industry and other phases of life. He told of shipments of radioactive materials made to hospitals, laboratories and centers. He concluded that perhaps the greatest possibilities are in the field of medicine.

Saturday, May 30, 1953

There were the usual family activities in the morning--I did do some reading, etc. I received a communication from Dennis V. Carey (District Supervisor, San Francisco) to my letter to the Contractors State License Board. Carey said an investigator, A. W. Bepler, will contact me in a few days.

A number of neighborhood kids joined Pete this afternoon for a birthday party.

Sunday, May 31, 1953

In today's paper I read about the track meet held yesterday at the Stanford Stadium in Palo Alto. Our star, Lon Spurrier, ran the 880 in 1.52.7.

I have been following with considerable interest Cal's tennis team. Last year Cliff Mayne and Hugh Ditzler won the NCAA doubles championship, and this year the team has already won first place in the PCC (Southern Division). The team will soon travel East to compete in the Eastern Intercollegiate Tennis Championships at West Point, New York, and then in the NCAA tourney. Tennis is really a great sport; possibly Helen and I can, sometime in the future, build our own court for the kids.

Although I worked in my study for a while, I also joined the family in helping Pete celebrate his 7th birthday.

Monday, June 1, 1953

Sherman Fried is in town. Naturally, much of the day consisted of conferences with him (and Stan and Al) to describe our position on the Mike work. (There has been little laboratory activity in the Bldg. 5 Annex lately since the Paydirt (Mike) project has been shut down for repairs. They expect to start up again about the 10th of this month.)

Today's mail brought a letter (ANL-WMM-1106), dated May 29, from Winston Manning in which he went over the points I made to him in my letter of May 25. I routed the letter to Stan and Al, and we will discuss it with Fried.

A copy of the revised Chapter 20 (PPR, Volume 14A) arrived from John Huizenga for my comments and additions. Huizenga pointed out some additions to the tables--I will review it later.

One of the items I brought up at the regular Monday noon senior staff meeting was the question of a research problem for Jim Grover--I asked for suggestions.

At 1:30 p.m. in Room 363, LeConte Hall, I took part in the prelims of John Peter Hobson. Other members of the committee were Arthur F. Kip, Emilio Segrè, William A. Nierenberg, and Henry B. Silsbee. Subjects considered were (1) Atomic and molecular beams, (2) The shell model with emphasis on nuclear magnetic moments and quadrupole moments, and (3) Kinetic theory.

Back on the hill at 3 p.m., I conferred with Livingston (Robert S.) of Oak Ridge.

Tuesday, June 2, 1953

As Lawroski suggested when I talked with him last week, I telephoned again to ask about Steindler (Martin J.). Steve reported that Harold Feder had talked with Steindler, who indicated that a visit to us had been set up. I assumed that, on this basis, it is ok for us to have Steindler come out to talk with us. I talked with Earl about this, and we concluded that the best thing to do is to have him (Earl) stop in Chicago on his way to the Gordon Conference to interview Steindler.

I also telephoned Joe Katz about PPR, Volume 14A, saying that I am sending him two copies of the table of atomic mass values (isotopes of actinium through californium) for publication as an Appendix to the Volume. In addition, I mentioned that I plan to stop in Chicago on June 26.

A note also went to my friend Clayton Sheldon in South Gate to point out that I shall arrive in South Gate late Thursday night, June 18th, in order to give the commencement address at Jordan High School the following night. I wrote, "I would be able to get out for some golf on Friday and I wonder if you would be able to make it. I think that it would be necessary for me to finish by about 3:00 to 3:30 p.m. so that at least nine holes out of the eighteen would have to be played in the morning."

At 10:30 a.m. I met with George Everson to discuss salaries, particularly those of the Whitney personnel. I then got together with Greg Engelhard for lunch and for a discussion of the upcoming PCC meeting.

Although I had a 3 p.m. meeting with Roger Batzel, much of the afternoon was spent with Sherman Fried. Altogether, I probably conferred with him, Stan, and Al for some six hours on this visit; Stan and Al talked with him an additional amount of time while I was doing other things. Many points were covered in these conferences. We pointed out that the  $^{99\alpha}_{253}$  6.6 Mev alpha activity was established by us on December 20, 1952 on the third elution run (Cit-II), which was not covered in our December 19, 1952 memo that was based on two previous elution runs finished before December 19. We explained that the elution curve for this third run was not in our March 20, 1953 memo because the emphasis here was on what we thought was a clear-cut discrepancy of facts developed at the Boner Panel meeting on February 18, 1953. We hold that Argonne hadn't yet identified 99 at that time, since on February 18, 1953 they assigned the 6.6 Mev alpha particle to both 99 and 100--also this was still their position on March 20, 1953 and on May 13, 1953 (date of Studier's letter). Thus, it appeared that it was a clear-cut question of facts--we say that the 6.6 Mev alpha activity due to element 99 is assigned by Argonne to both element 99 and element 100. Fried implied that the Argonne position is that, even with a correct 6.6 Mev alpha-emitting element 99 and an incorrect 6.6 Mev alpha-emitting element 100, Argonne can maintain a claim on element 99. We pointed out that our December 20, 1952 date is well documented, known here at the time, and we are certain that it never will be subject to dispute by anyone other than Argonne people.

Fried said that the Argonne position is that we should withdraw our claim to element 99 with the implication that they might withdraw their claim to element 100--I pointed out that it was even difficult to see how they could make the former suggestion seriously in view of the facts. Fried said that Argonne was working on the transcalifornium fraction before my call to Manning on December 19, 1952--he couldn't explain why Manning didn't at least answer my telephone request for more material. Fried apologized for the large group that met with me at Argonne on January 7, 1953. He went on to say that he and others had the impression that we still didn't know that the 6.6 Mev alpha emitter was element 99 when I came to Argonne on January 7, 1953. My response was that I thought I made this clear--short of bragging.

Stan, Al, and I pointed out the difficulties with assigning their (Argonne's) 6.35 Mev alpha particle to element 100, showing that they always seemed to have a 6.77 Mev alpha particle (presumably  $^{216}\text{Po}$ ) with it in about half the abundance of the 6.35 Mev alpha particle as expected, for  $^{220}\text{Rn}$ , that the 6.35 Mev alpha activity didn't peak at any Z in their run No. 2 shown in their memo of March 20, 1953, that the 6.35 Mev alpha activity should have 600 days to 5 years half-life for Z = 100, that 6.35 Mev alpha activity as short-range group of 6.6 Mev means 6.35/6.6 ratio is greater than 20; this is inconsistent with their present view of a very small amount of 6.6 Mev element 100 alpha activity. With respect to their 6.6 Mev element 100 alpha particle, I pointed out my pleading phone calls to Manning to place their data on record with Rod Spence (this was before I knew what the activity was) as

I had done with our 16-hour, 7.1 Mev element 100 alpha particle. I noted that Manning said that he would but then he did not do it--the first knowledge of Argonne's 6.6 Mev alpha particle came to Spence and me at the Boner Panel meeting of February 18, 1953.

I told Fried that I thought it would take nerve for the Argonne people to claim element 100 on the basis of of the 6.6 Mev alpha particle, in view of this record. In addition, I pointed out that Manning, at the same Boner Panel meeting, had given an elution curve with peaks one drop apart for the 15-day 6.6 Mev alpha particle of element 100, the greater than 50-day 6.6 Mev alpha particle of element 99, and the 6.1 Mev alpha particle of californium, with the 100 and the 99 peaks of equal intensity. The Argonne people had done the same thing at the meeting of February 20, 1953 with me at Argonne, and I am sure of the latter by memory, and also the  $Cf^{253}$  isomer hypothesis (80% short-lived accurately reproduced by Argonne in the March 20, 1953 memo) made no sense as an alternate explanation of their results unless they were speaking of the intensity of their 6.6 Mev alpha-emitting isotope of element 100 as equal to or greater than the intensity of their 6.6 Mev alpha-emitting isotope of element 99 at the time (February 20, 1953). I pointed out to Fried that perhaps lower yield of higher mass element 100 isotopes of possible energies of ~6.6 and 6.35 Mev would eventually be found, the former with a half-life greater than 40 days and the later with a half-life greater than 600 days and that we hoped that they would not try to identify these with their 10-day element 100 isotope. Fried said he was willing to go on record that their 6.6 Mev alpha-emitting isotope of element 100 had a half-life less than their 6.6 Mev alpha-emitting isotope of element 99; he presumably wouldn't claim that a longer-lived isotope would be the same isotope.

I asked Fried to send me an alpha decay curve of the 10-day, 6.6 Mev isotope of element 100, and he said he would. Fried phoned Chicago to get the intensities of their 6.6 Mev element 100 alpha activity compared with their 6.6 Mev element 99 alpha activity and reported the ratio was ~10% on December 22, 1952 and fell to ~1% in March and to ~0.1% on May 20, 1953 (i.e., ~0.02 c/m absolute). I then pointed out that this is a very different story than that told me on February 18, 1953 and February 20, 1953 when the intensity of the 6.6 Mev element 100 isotope was equal to or greater than that of the 6.6 Mev element 99 isotope. I also told Fried that I thought it was very peculiar that they had not presented an elution diagram in their report of March 20, 1953 showing some separation of the 6.6 Mev element 100 alpha activity and the 6.6 Mev element 99 alpha activity; I suggested this was due to the fact that they didn't have such data.

In my rounds of the labs I noted that special equipment is being moved into Building 5A (Annex) for Higgins and Crane to load an americium slug for Arco. The fellows said that the process may take several days.

At 6:30 p.m. I went to a dinner meeting of the Athletic Advisory Board in the Durant Hotel.

Wednesday, June 3, 1953

This morning I had a 9 a.m. meeting with Clark Kerr to discuss the

upcoming PCC meeting and my proposed action, which Kerr supports, about the Cannamela incident. Before going up to the hill, I stopped in the Chemistry Department office to chat with some of my colleagues.

On the hill I made several phone calls and dictated a few letters before visiting the labs. I telephoned Rix Snyder at Stanford and Stan Freeborn at Davis to speak with them about the proposed action on the Pat Cannamela case. In addition, I called to ask Greg Engelhard for a list of our ineligible athletes.

I responded to a May 29 letter from Herbert E. Morey (Principal, David Starr Jordan High School), who wrote about plans for the graduation exercises on June 19. Morey said that three students will speak on the theme, "Duty, Honor, Country--Our Ever Widening Responsibilities," but he feels that it is better not to suggest a topic for my speech, knowing that anything I choose will be challenging to the students. I replied that I will speak on something along the lines of "The Future and Atomic Energy." I also explained that I shall arrive in the area either late Thursday night, June 18, or on the morning of June 19 and shall stay at the home of my parents in South Gate until Saturday morning, adding that I am not sure how many people will come over to Jordan with me but three might be a good guess.

A note went to Harold Dodgen (State College of Washington) to give him my travel plans for my talk there on June 12.

I acknowledged a May 27 letter from Arthur W. Fairhall (MIT), who sent a draft of his photo-spallation paper, which he plans to submit to The Physical Review. I commented that the draft, which covers work done using the MIT 320 Mev synchrotron, looks like a fine piece of work although I have not yet had time to give it the study it deserves. I made a few suggestions and said that I am looking forward to hearing his discussion of the work at the Gordon Conference.

A descriptive letter about our graduate training program in nuclear chemistry went to John Gilmore, Fargo, North Dakota, in response to a recent request.

Then I returned the revised Chapter 20 (PPR, Volume 14A) to John Huizenga with a few comments and suggestions. I pointed out that the English can stand further polishing and mentioned a few things about Table 2, adding that, if my suggestions are acceptable to him, I can see no reason for exchanging another draft copy.

I also returned the galley of PPR, Volume 14A, Chapter 19 ("Optical Properties of Some Compounds of Uranium, Plutonium, and Other Elements" by E. Staritzky and A. L. Truitt of Los Alamos) to Joe Katz, saying that I haven't made any changes and that he can decide whether Staritzky's suggestion should be adopted.

Thursday, June 4, 1953

In attendance at this "first of the month" group meeting were Asaro, Carniglia, Carr, Clark, Cobble, Feay, Glass, Gray, Grover, Hollander, Hulet, Hyde, Harvey, Jaffe, Kalkstein, Kofstad, Lessler, Levy, Mathur,

Michel, Nervik, Passell, Perlman, Ramaniah, Ruben, Seaborg, Shudde, Slater, Stephens, and Templeton.

First Mathur spoke on his study of the gamma rays of  $\text{Cs}^{125}$  and  $\text{Cs}^{127}$ . He produced  $\text{Cs}^{125}$  by a bombardment of  $\text{I}^{127}$  with helium ions, followed by a separation on the mass spectrograph. Then the separated activity (18,000 c/m) was examined in the sodium iodide scintillation counter, and annihilation radiation and a 115 kev photon were observed. He and Kalkstein studied the positron-gamma ray delayed coincidences of unseparated  $\text{Cs}^{125}$  and found less than  $10^{-8}$  seconds delay by any photons emitted by  $\text{Cs}^{125}$ , which indicates the 115 kev gamma ray arises from an  $M_1$  transition. He presented a possible decay scheme, which he compared with the similar one of  $\text{Cs}^{127}$  reported previously. Hyde added that there is apparently one gamma ray (115 kev) per positron in  $\text{Cs}^{125}$ . Mathur then presented some information on xenon isotopes and their iodine daughters. He said that  $\text{I}^{127}(\text{p},\text{xn})$  reactions give:  $\text{Xe}^{121}$ , 45-minute half-life;  $\text{Xe}^{122}$ , electron capture with 19-hour half-life;  $\text{Xe}^{123}$ , 1.7 Mev positron emission with a 1.8-hour half-life. Mathur noted that a 170 kev gamma ray was found in  $\text{Xe}^{122}$ , while a 153 kev gamma ray was found in  $\text{Xe}^{123}$ . Mathur's summary of the iodine daughters is as follows:  $\text{I}^{121}$ , 1.8-hour half-life, 210 kev gamma ray;  $\text{I}^{122}$ , 3.5-minute half-life, no gamma ray;  $\text{I}^{123}$ , 13-hour half-life, 160 kev gamma ray. He said that  $\text{I}^{123}$  decays by electron capture--he attempted to determine if it also decays by positron emission but found no annihilation radiation from positrons.

Asaro presented a decay scheme for  $\text{U}^{232}$ , based on the observed gamma rays (57, 132, ~90 kev) and their conversion coefficients. Asaro compared the determined conversion coefficients with the theoretical L conversion coefficients and went into some detail about the work. He gave some information about possible gamma rays with energies above 130 kev. In speaking about  $\text{Np}^{237}$ , Asaro said he found that 91 kev gamma rays are in coincidence with 28% of the  $\text{Np}^{237}$  alpha particles and that the 144 and 193 kev gamma rays are each in coincidence with about 2% of the  $\text{Np}^{237}$  alpha particles.

Jaffe discussed his reexamination of  $\text{Am}^{241}$  on the bent crystal spectrometer--he saw 15 neptunium L x-rays as well as two americium x-rays and the two well-known gamma rays. The energy of the dominant gamma ray is  $59.65 \pm 0.13$  kev, accompanied by  $29.81 \pm 0.03$  kev (second order line). He said the second order line gives  $59.62 \pm 0.06$  kev, which is probably the best energy for this transition in  $\text{Np}^{237}$ , noting that Browne found  $59.78 \pm 0.04$  and  $29.82 \pm 0.04$  kev; however, there has been a change in the spectrometer and the two values are actually in essential agreement. Jaffe also spoke about the lower energy transition, measured as  $26.38 \pm 0.04$  kev while Browne found  $26.43 \pm 0.03$ . Jaffe then went on to give the relative intensities of the various transitions in  $\text{Np}^{237}$  as compared with the intensities found by Browne in  $\text{Pa}^{233}$ .

Harvey told the group about experiments that he and H. G. Jackson performed at Chalk River on the alpha particle energy of  $\text{Th}^{232}$  and  $\text{U}^{238}$ . He described the experiments and the equipment.

\* \* \* \* \*

I went up to the hill after the meeting and, at the end of a telephone conversation from Rod Spence and Charlie Browne to Al Ghiorso, I spoke with Spence. I asked if he remembered (and would be willing to be quoted) that I called him on approximately February 2 to tell him about our element 99 isotope that decayed by beta emission to the element 100 isotope (7.1 Mev alpha emitting, 1-day) and that Manning did not tell him anything about the 6.6 Mev element 100 alpha emitter until the Boner panel meeting of February 18. Spence responded that he remembered and that he would be willing to confirm this. He also said that the Los Alamos group has not yet confirmed the 7.1 Mev alpha emitter of element 100 nor the 6.6 Mev alpha emitter of element 99 (with very little or no 6.6 Mev element 100 alpha emitter) by elution but hoped to do so with the sample we sent them early this week (their 1/3 share from the 1000 pounds of paydirt). I apologized for saying they had confirmed our results in my letter to Manning a few weeks ago and explained why I thought they would be ahead of Chicago or anyone else in confirming the 6.6 Mev element 99 alpha emitter, if they established  $Z$  as 99. I told Spence that we still want no changes in the Mike report, adding that Sherman Fried's visit was very worthwhile although little progress was made largely because of the rest of the Argonne group. In addition, I added that eventually an approximately 6.6 Mev alpha emitter of element 100 might be found with  $A = 257-260$ , but this is not Argonne's element 100 alpha emitter of 6.6 Mev of February and March 1953. I mentioned to both Spence and Browne that Argonne's element 100 6.6 Mev alpha emitter is now down to 1% of their original effect and asked Browne to set limits on the element 100 6.6 Mev alpha emitter in case Argonne again makes it long-lived.

Al and I had a long discussion after this call, and I said that I shall reply to Manning's May 29 letter tomorrow.

On May 20 Willard R. Espy (Mount Kisco, New York) described a book he plans to write on the natural history of interdependence and asked me to refer him to any passage, especially from my own works, on interdependence at work in the field of chemistry. He also sent me a reprint of an earlier book, Bold New Program. I signed a response today, saying that although the interdependence between science and society has become increasingly apparent to many of us, my own writings have been largely technical and probably not too relevant to his present study. I referred Espy to speeches by Gerard Piel (Editor of Scientific American) and David Sarnoff (Radio Corporation of America).

Later, after looking in on the lab work, I telephoned Bill Rice--Helen and I decided recently that we need a storage room since our carport now contains two cars plus children's play equipment. Rice has designed such a room, and I talked with him about the materials and permits we need for its construction.

Friday, June 5, 1953

Since I will be out of town for several days, I tried to clear my desk of pending business.

A memorandum went to W. B. Reynolds about the temporary building built just above Bldg. 5 for processing the coral from the Pacific

tests. I explained that our operations have shown that it is important that we have equivalent facilities available for next spring's Castle shots and, until that time, we shall use the present facilities for treating more coral from the Mike shot. We have no objections to the present building being torn down, I wrote, provided an equivalent facility can be made available somewhere in the 184-inch area.

With regard to some personal business, I applied for a building and electrical permit for our storage room from the Building Inspection Department (Martinez, California) and ordered the lumber for the room from Piedmont Lumber and Mill Company (Oakland), suppliers of the lumber for our house.

In today's mail was a memorandum from Donald Coney about the purpose and plans for the Committee on Television, of which Coney is Chairman. He suggested we address the following questions:

1. What facilities on the Berkeley campus should be made available to BAETA under provisions of The Regents resolution of April 24, 1953?
2. What type of University organization should be established to carry out the intent of paragraph 5 of The Regents resolution?
3. What should be the relation of the Berkeley campus Administration to BAETA? What should be the relation of the University to BAETA?
4. What steps should be undertaken to establish a programming group on the Berkeley campus?

[BAETA is the Bay Area Educational Television Association that is concerned with the start-up of an educational TV channel in the Bay Area.] Coney enclosed the following copy of the Regents' Resolution passed at the 24 April 1953 meeting and said his secretary will get in touch with me to determine the possibilities for an early meeting.

Revised  
Action  
Approved JUN 1 1953  
ADM  
PHB

EDUCATIONAL TELEVISION:

F. That the officers of The Regents be authorized to execute an agreement with the Bay Area Educational Television Association (BAETA) to permit the use, during specified hours of the afternoon and evening, of sound recording space now available in the basement of Dwinelle Hall, Berkeley campus, by schools, colleges and other educational agencies participating in the BAETA program for the originating of educational television programs in the East Bay, said agreement to be upon the following terms and conditions:

1. BAETA shall purchase, install, maintain and operate all equipment in the Dwinelle Hall studio needed to originate television programs, and shall send such programs to its transmitter in San Francisco, without cost to the Regents.

2. BAETA shall pay to The Regents, monthly, an amount to be mutually agreed upon for electric power and extra custodial service which operation of the Dwinelle Hall Studio might require.

3. BAETA shall credit The Regents, as compensation for studio space, with the part of the nominal contribution toward the overhead of BAETA which the University would be expected to pay if it becomes a participating member of BAETA.

4. BAETA shall make the Dwinelle Hall studio available to students of the University, members of the Radio and Television Workshop, and others, as far as possible, when not on the air.

5. BAETA shall submit to any University authority which The Regents may designate, a complete list of programs to be originated from the Dwinelle Hall Studio sufficiently far in advance to permit a check of the nature of these programs and of the sponsors thereof, and to refrain from originating in the Dwinelle Hall studio any program to which the University objects under general policies covering the use of University facilities by outside agencies.

6. No broadcast by BAETA, or by a participating member of BAETA, shall include an announcement to the effect that such broadcast originated from the Dwinelle Hall studio or from the campus of the University, without prior consent of the University to such announcement.

7. BAETA shall indemnify The Regents against any claims for libel rising out of the use by BAETA of the Dwinelle Hall studio.

8. The term of the agreement shall be for the normal life of the original complement of equipment to be installed in the Dwinelle Hall studio, as agreed to by the Committee for Adult Education of the Ford Foundation, but for not more than 8 years; provided, however, that the agreement may be cancelled upon failure of BAETA to continue a regular program of telecasting.

**Note:** It is understood that nothing in the agreement shall commit the University to active participation as a member of BAETA.

Considerable time was then spent preparing the following response to Winston Manning's letter of May 29, 1953, which was in reply to my letter of May 25, 1953:

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June 5, 1953

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Dr. W. M. Manning  
Argonne National Laboratory  
Post Office Box 299  
Lemont, Illinois

Dear Winston:

This is in reply to your letter of May 29, 1953 in reply to mine of May 25, 1953.

I stated that your group had come to agree on the basis of Studier's letter of May 13, 1953 with our view concerning a beta-emitting Cf<sup>253</sup> parent because, from our point of view, ANL-WMM-1076 clearly leads to the impression that you did not think much of the idea of such a Cf<sup>253</sup> beta-emitter at the time of writing it, even though you included a hedge on the point. With respect to your question concerning isomeric Cf<sup>253</sup>, I agree that if the daughter 99 is longer-lived than the parent californium, it would not be necessary to postulate such isomers, and this could eventually prove to be the case. I agree with you that the detailed properties of any particular isotope of a previously known element (like Cf<sup>253</sup>) is a matter of intrinsically minor importance compared to the question of the isotopes of the new elements.

With respect to the second paragraph of my letter, I should have said that any laboratory (and I should not have mentioned Los Alamos without personally checking with them, since I understand now that they have not yet made the identification) that assigned the 6.6 Mev alpha emitter to atomic number 99 would, from our point of view, be the first to confirm the discovery of element 99. As I understand the information which Sherman gave us last Tuesday, your group, on the basis of an elution separation done within the last couple of weeks, now assigns all except possibly about 0.02 counts per minute of the 6.6 Mev alpha activity to element 99 and therefore, from our point of view, you would come into this category.

If I understood Sherman correctly, you people are now taking the position that the "6.6 Mev 100 alpha" had an intensity of the order of a few percent (or less) of that of the 6.6 Mev 99 alpha, at about the time of my February 20, 1953 visit to Argonne. This is in sharp variance with the view presented to me at that time when these two intensities were about equal. In fact I very clearly remember seeing an elution curve at the time of that visit in which these intensities were about equal and in which the 100 and 99 peaks badly overlapped with a difference of about one elution drop at the peaks. I wonder if you would be willing to look at that elution

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National Defense of the United States within the  
meaning of the Espionage Act of 1917, U. S. C.  
Section 7, Title 18, and its transmission  
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curve again, which by the way, was essentially the same as that presented by you at the Boner Panel meeting. I don't see how you could maintain now, even on the improbable assumption that in retrospect you are now correct in maintaining that a few percent of the 6.6 Mev alpha was 100 on February 20, that you had established this on February 20 if you were observing 10 or possibly 100 times as much 6.6 Mev 99 alpha in the same elution position on February 20. In fact, I would regard it as ludicrous for you to maintain that you were able to establish on about February 20 an effect of the order of a few percent on the basis of the elution data that you showed me at that time, and perhaps I do you an injustice even to imply that this could conceivably be your intention.

As I pointed out to Sherman, and he asked me especially to include this point in my letter to you, there could be a 100 alpha emitter of very low intensity of energy somewhere in the neighborhood of 6.6 Mev with mass number in the general area of 257-260. This follows from the assignment of a 7.1 Mev alpha to a 100 isotope of mass about 255, together with an alpha energy versus mass number line plotted in the alpha systematics very roughly parallel to corresponding lines for other heavy elements. This should decay according to our best nuclear radii estimations at a maximum rate corresponding very roughly to a 40-day half-life if it is of the even-even type and perhaps slower than this if it is the odd neutron type. Incidentally, this rate cannot be speeded up through the presence of a fine structure long range alpha unless that alpha is present in proportionately larger amount than the 6.6 Mev alpha. I do hope that you won't confuse such a potentially possible alpha emitter with your "10-day 6.6 Mev 100 alpha emitter;" Sherman assured us that it was at least his understanding that the ANL "6.6 Mev 100 alpha" was shorter-lived than the approximately 20-day 6.6 Mev 99 alpha and therefore if a longer-lived 6.6 Mev 100 alpha emitter is eventually established, he for one will recognize it as a different one. The same considerations apply to a 6.35 Mev 100 alpha emitter, although in this case the mass number may be above the region of beta stability, or in the range of high spontaneous fission rates on Huizenga's ideas, or in any case of such small yield and long half-life (about 1-2 years if it is of the even-even type) that even if alpha decay is the rate-determining decay process it could very well be impossible to find it with any amount of material which can be worked up from the Mike shot.

With respect to your 6.35 Mev activity, we can't claim to have a complete explanation, but I would like to point out that in about every case where the pulse analyzer data covered the range you had an amount of about 6.77 Mev alpha activity present in about half the intensity of the 6.35 Mev alpha activity. This fits perfectly with the hypothesis that somehow, not for us to explain, you had gaseous thoron (100% counting efficiency) in equilibrium with the  $Po^{216}$  daughter (50% counting efficiency due to recoil onto the walls).

Sherman discussed with Al the possible discrepancy between figures 11 and 14 in MB-IP-740. The effect is apparently explained by the statistics

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of small counting rates, but again let me emphasize that it makes no sense to discuss an effect of this size (pertaining to March 1, 1953) in relation to the position which you people presented to me a little previously to this (February 20, 1953), when you were discussing 99 and 100 6.6 Mev alphas of approximately equal intensities.

With best wishes,

Cordially yours,

Glenn T. Seaborg

GTS/lmh

cc: R. W. Spence - C. L. Browne  
Sherman Fried

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Carbons of this were sent to Spence/Browne at Los Alamos and to Sherman Fried at Argonne.

Saturday, June 6, 1953

Most of the day was spent with Helen and the kids. Today is Helen's and my 11th wedding anniversary.

Sunday, June 7, 1953

A lab driver picked me up at home, drove me to the San Francisco Airport, where I caught Western Flight 606 (a DC-6B) at 9:45 a.m. to Seattle, arriving at 12:15 p.m. Others from Berkeley attending the meeting of the Pacific Coast Intercollegiate Athletic Conference include Brutus Hamilton (Athletic Director), Greg Engelhard (Director of General Activities of ASUC), George Briggs (Assistant Athletic Director), and Lynn Waldorf (Head Football Coach).

The first session of the Pacific Coast Intercollegiate Athletic Conference meeting took place at 8:35 p.m. in the Washington Athletic Club, where we are staying (6th and Union Sts., Seattle), and was a joint meeting of Faculty Representatives, Directors, Football Coaches, and Business Managers. The President, Dean Thomas S. Kerr (University of Idaho) was delayed, and Dean Orlando John Hollis (University of Oregon) acted as President and introduced new faculty representatives (Claude Jones of UCLA and me) and other people who were attending a Conference meeting for the first time. George Briggs (Assistant Director of Athletics) was the person introduced from Berkeley. Various committees were appointed, and we heard a number of reports, including the Commissioner's report. Finally, at 10 p.m., Dean Thomas S. Kerr, President of the Conference, arrived and took the chair. After approving the minutes of the December 1952 meeting, we adjourned at 10:10 p.m.

Monday, June 8, 1953

In Seattle. The Faculty Representatives met at 9:05 a.m., in a meeting called to order by President Kerr. A number of items were considered, including special student petitions and the question of abolishing outside athletic competition for freshmen. This second session adjourned at 11:25 a.m., but we met again at 11:30 a.m. with the Athletic Directors. Again the meeting was called to order by President Kerr. Final action was taken on a number of special student petitions (there were no petitions from Berkeley). Near the end of the session a number of representatives of the Tournament of Roses Association, including President of the Association, Mr. Harry Hurry, met with us. We adjourned at 12 noon.

At 2:05 p.m. the Faculty Representatives again met and considered such items as the adoption of a "C" average in high school in basic academic subjects as a prerequisite for first year varsity competition, the adoption of a "C" average in junior college as a prerequisite for first year varsity competition, and a proposal that students when found to be scholastically ineligible for competition shall be ineligible for practice (I certainly am in favor of all of these proposals). This fourth session adjourned at 3:50 p.m.

A joint meeting of the Faculty Representatives and the Athletic Directors began at 4 p.m. I was disappointed that the representatives

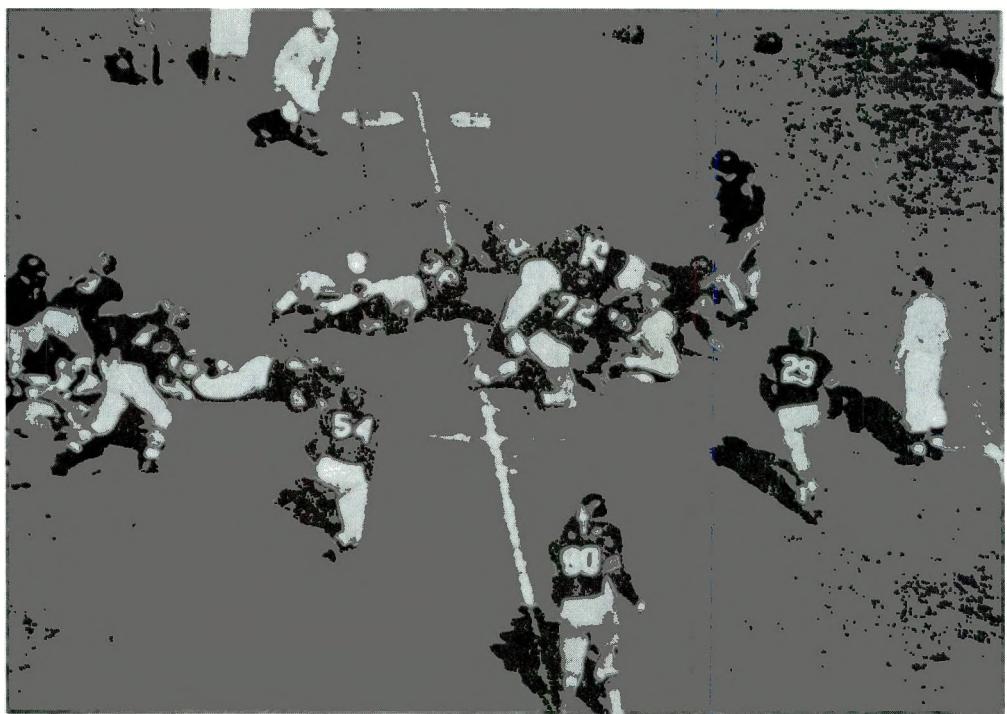
defeated both the "C" average rule for high school students and for junior college transfers (California, Stanford, and UCLA voted for the rule). The motion that students, when found scholastically ineligible for competition, are ineligible for practice did carry unanimously. An agenda item about the number of baseball games was presented. We finally approved an amended motion that limits the number of baseball games to 30 for varsity and 30 for junior varsity (exclusive of any playoff games). Both USC and UCLA voted against this. This fifth session adjourned at 5:35 p.m.

The Faculty Athletic Representatives, as is customary for one evening of these meetings, got together for drinks and a social session; this was followed by dinner together.

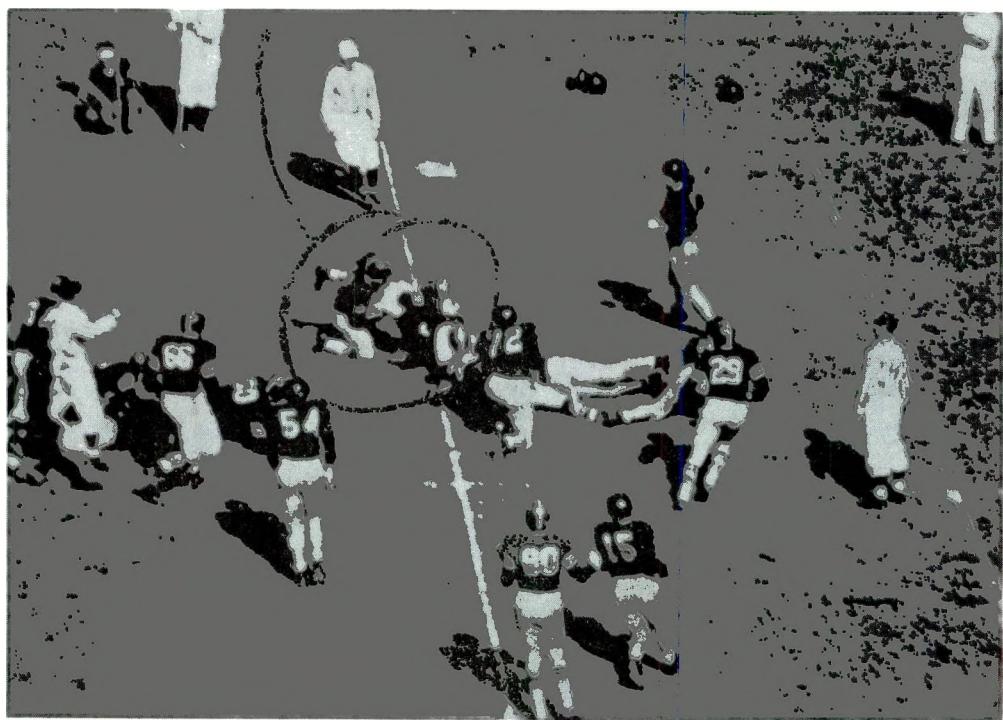
Tuesday, June 9, 1953

In Seattle. The Faculty Representatives met from 9 a.m. until 10:55 a.m. and considered a number of items. Then, at 11:05 a.m., we held a joint meeting with the Athletic Directors (the seventh session), at which time we voted on a number of motions. One, which passed unanimously, makes any student ineligible if he disposes of any prize for monetary consideration. We adjourned at 12:05 p.m. for lunch.

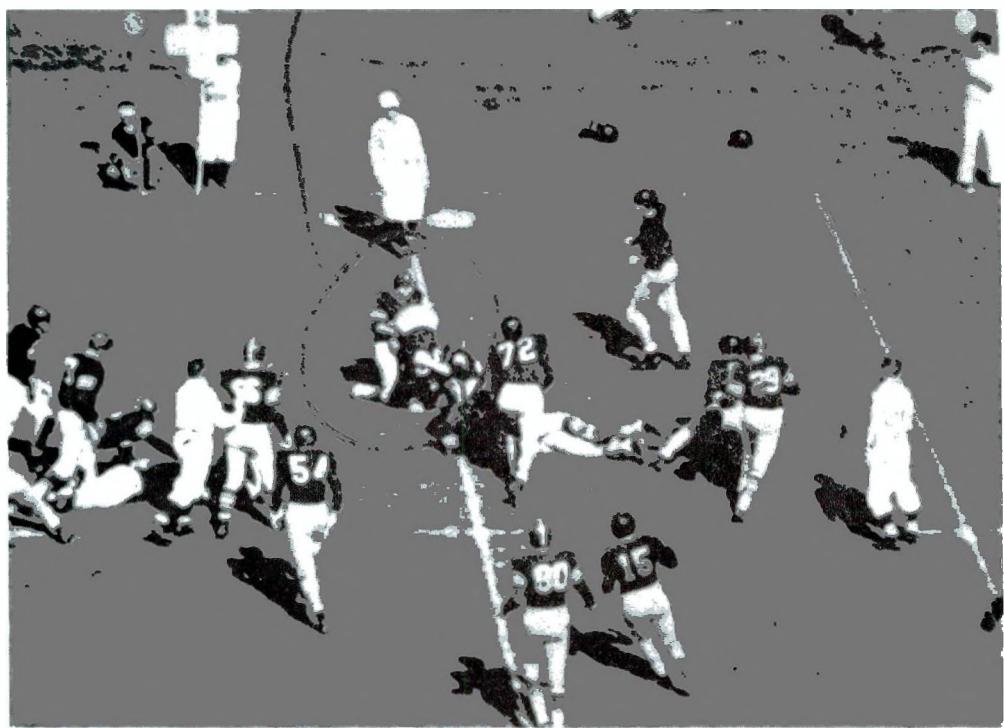
At the 2 p.m. session of the Faculty Representatives we discussed spring football practice--most of the representatives seem to be against such practice. For the rest of the session--until 4 p.m--I brought up and we went over the Pat Cannamela case. At the USC-California game on October 20, 1951 Cannamela deliberately twisted Johnny Olszewski's knee after the play was over. Olszewski's knee was badly injured. My presentation, which included still photographs of the action, had a dramatic effect on the Faculty Representatives, and Hugh Willett (representing USC) was placed in a difficult posture.



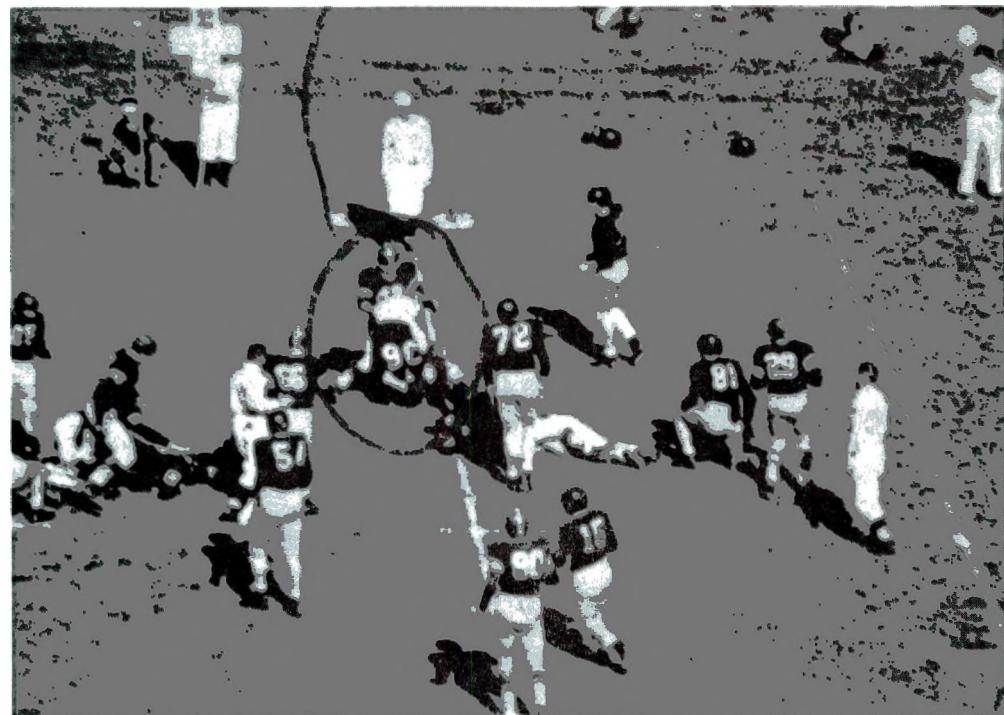
1-A Tackle completed



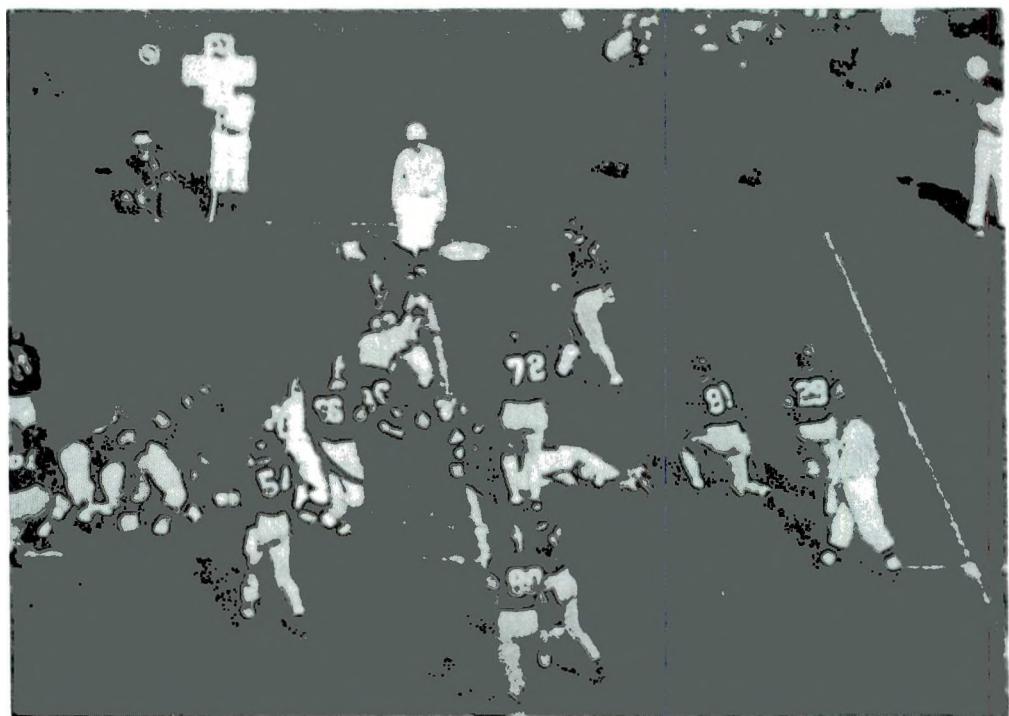
1-B Cannamela acts



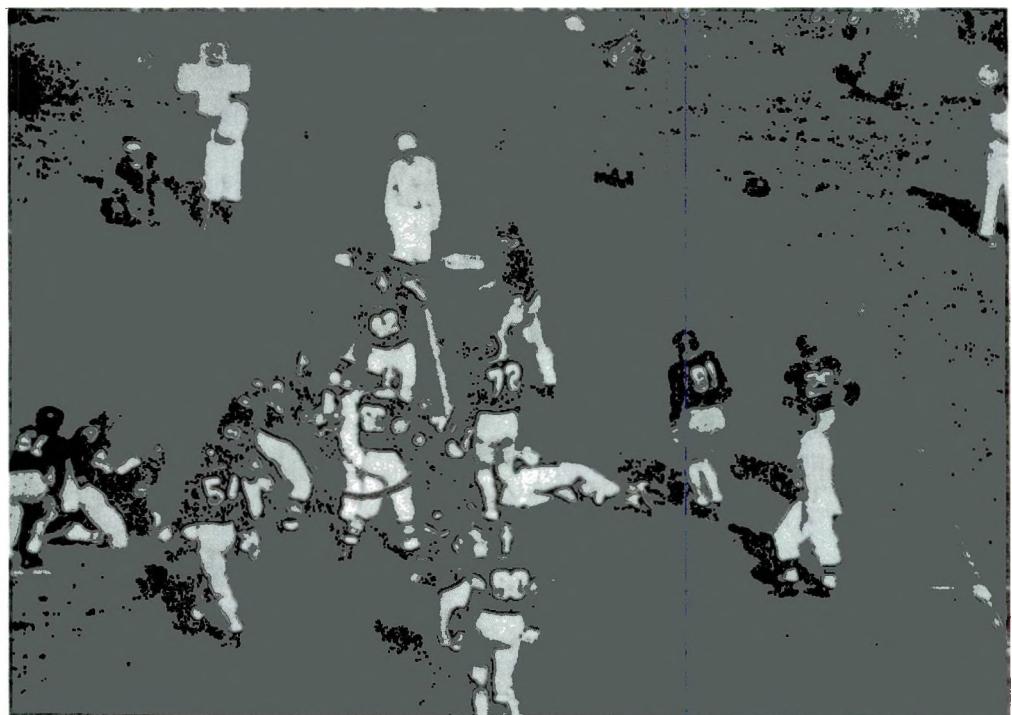
1-C Cannamela acts



1-D Cannamela acts



1-E Cannamela acts



1-F Action completed

At our ninth session (4:05 until 4:25 p.m) the Faculty Representatives met with the Athletic Directors and heard Willis O. Hunter (Athletic Director of the University of Southern California, chairman of the Rose Bowl Committee) present a report on the proposals for radio and television rights for the Rose Bowl games. We passed a motion authorizing the Committee to negotiate the sale and contract for radio broadcast and television of the Rose Bowl game for the three-year period following the expiration of the present contract for not less than \$500,000.00 per year, and that the chairman of the Rose Bowl Committee be authorized to sign said contract.

There was a special joint meeting of Faculty Representatives, Directors, and Coaches from 4:30 until 5:00 p.m., at which time we heard and accepted a report of a committee chaired by Hugh P. Everest (University of Washington) on the regulation of organized recruiting.

The Faculty Representatives and Directors met in a joint session from 5:02 p.m. until 5:30 p.m. A motion was defeated (3 for, 6 against) that that there be no outside athletic competition for freshman students. We discussed a recommendation by coaches and directors that there be a limitation of 8 1/2 hours per week for freshman football practice. Another recommendation by the coaches that we discussed was that, after the opening of classes, all varsity practice activities be limited to 11 hours per week.

[In Berkeley a wire arrived from Ron Brightsen: "FELLOWSHIP COMMITTEE HAS NOT REACHED A DECISION AS YET WILL NOTIFY YOU PROMPTLY WHEN I HEAR."]

One of our secretaries, Mildred J. Davis, terminated today because she is about to have her second child. Millie has been an excellent secretary and did a beautiful job typing the latest "Table of Isotopes."

The Chemistry 223 class had the following final examination, administered by Iz Perlman, today:]

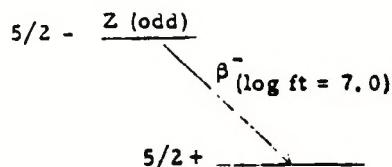
Chemistry 223

Final Examination  
June 9, 1953

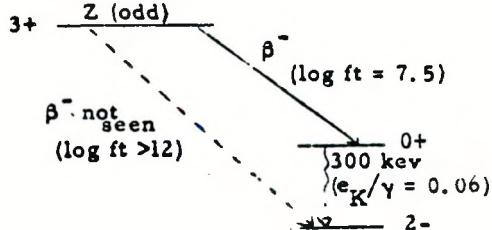
I (25)

For the following decay schemes and associated data, the transitions as indicated may be considered well established. However, some of the spectroscopic designations may not be consistent with the information given. Comment briefly on the spectroscopic assignments and make such changes as seem necessary. Consult your lecture notes for shell model sequence and use Figs. 9 a, b, p. 87 of Halliday for conversion coefficients. The ft values for allowed, 1st forbidden and 2nd forbidden transitions may be taken respectively as: 5-6, 7-9, >10.

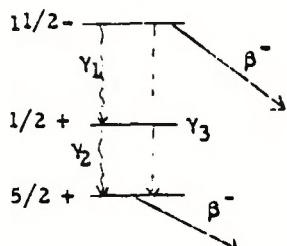
(a)  $A = 151$



(b)  $A$  even;  $Z$  in range 75-80

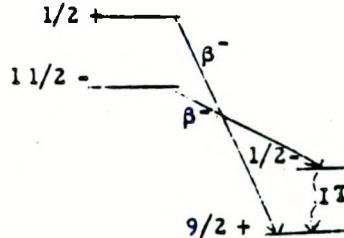


(c)



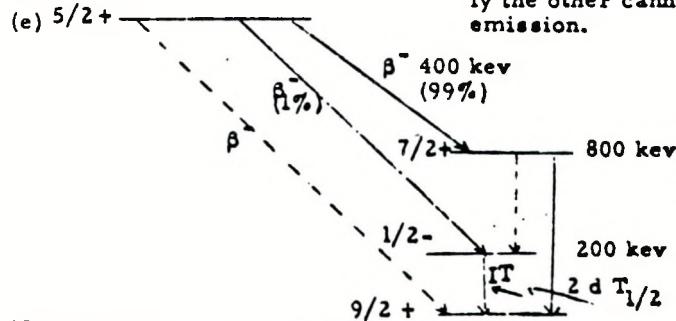
Note:  $\gamma_3$  not seen; two  $\beta^-$  groups seen but energy states to which they lead not defined.

(d)



Note: Ground state of daughter has a measured spin value; only one of the isomeric transitions has been seen, presumably the other cannot compete with  $\beta^-$  emission.

(e)



Note: Broken lines designate transitions not observed because of low intensity.

II (25)

(6) (a) Calculate the range in centimeters of air for an 100-Mev stripped  $\text{Cl}^{12}$  atom using a curve on p. 124 of Halliday.

(5) (b) Explain why the absorption coefficient (expressed in  $(\text{mg}/\text{cm}^2)^{-1}$ ) for electrons is not a very sensitive function of the atomic number of the absorbing medium for electrons of less than a few Mev but does not depend on the atomic number for high energy electrons.

(7) (c) When one is measuring  $\gamma$ -rays with a crystal scintillator one can get a small peak which arises from photons which have undergone  $180^\circ$  Compton scattering. If one is measuring a  $\gamma$ -ray of 750 kev at what energy does the second peak appear.

(7) (d) Draw a curve for the linear absorption coefficient for photons in the range 1-10 Mev in an hypothetical electron gas of 1 atmosphere pressure. Give an approximate scale to the coordinate expressing the absorption coefficient. How could an experiment approaching this condition be designed?

III (30)

(5) (a) If one received a sample of uranium containing fission products, how could one tell whether the uranium had been irradiated with slow neutrons or high energy protons.

(5) (b) Explain why bismuth with a calculated excitation threshold for fission of 15-20 Mev will not undergo fission perceptibly when excited to this energy.

(5) (c) Using a cyclotron with a time-of-flight neutron velocity selector, at what interval after the cyclotron pulse would an 100 ev neutron arrive at the detector which is 10 meters from the source.

(10) (d) Using 2.10 Mev protons on lithium to give neutrons by the  $\text{Li}^7\text{-p-n}$  reaction, what will be the energy of the neutrons in the forward direction?

(5) (e) Explain why the maximum  $p, 2n$  cross section for a heavy element is greater than the maximum  $p, n$  cross section.

IV (10)

If one wishes to consider the possibility that a plutonium isotope exists in nature by virtue of having a half-life comparable with the age of the earth, what isotope would have a chance of meeting this requirement. In answering, state fully the reason for choosing the only eligible candidate and what you estimate its alpha energy and alpha half-life to be.

V (10)

The radiative capture ( $n, \gamma$ ) cross section of the nucleus  $Z^A$  is found to have a sharp maximum of 2000 barns at a neutron energy of 1 ev. Using Breit-Wigner one-level considerations, calculate the cross section for resonance (inelastic) scattering of 1 ev neutrons on  $Z^A$ .

Assume (a) the probability for neutron re-emission from the nucleus is much less than the probability for  $\gamma$ -emission,  
and (b) the mean lifetime for  $\gamma$ -emission is  $10^{-14}$  seconds.

Wednesday, June 10, 1953

In Seattle. The Faculty Representatives met in its twelfth session, again conducted by President Kerr, from 9:05 a.m. until 11:45 a.m. In addition to continued discussion about USC and the Cannamela case, we also talked about the division of Rose Bowl receipts and television receipts. The Athletic Directors met with us for a short session before lunch, at which the Secretary (Emmett B. Moore, State College of Washington) told the directors about our recommendation that there be a division (sharing equally among all members except the University of Idaho) of all telecasting receipts from games involving any member institution.

From 2:00 p.m. until 4:00 p.m. the Faculty Representatives met, again talking about the Cannamela case, limitation on varsity practice sessions, division of Rose Bowl receipts, etc. Willett is having increasing difficulty defending Cannamela's savagery.

At the 4:05 p.m. joint meeting with the Athletic Directors, we voted (6 for--Idaho, UCLA, and USC against) on the following division of Rose Bowl receipts: After the expenses of the participating member institution has been deducted, the Conference share of the Rose Bowl receipts be divided in such manner that the University of Idaho receive 5% of the amount to be distributed among member institutions, and that each other institution receive 11.875% of the amount, provided that from the distributive share of the Conference for the 1954 Rose Bowl game the University of Idaho participate equally with the other member institutions up to the amount of \$26,000.00, and that the balance, if any, of the Conference distributive share for that (1954) game be distributed equally between the other eight member institutions.

It was also moved and seconded that it is the judgment of the Conference that there have been abnormally high numbers of cases of illegal football play during the 1951 and 1952 seasons involving members of the University of Southern California teams; that in some of those cases members of other teams were subjected to serious injury; that there is some evidence indicating a lack of proper effort on the part of the coaching staff of the University of Southern California to guard against a continuance of illegal play; that the Commissioner be directed to inform the President of the University of Southern California of the foregoing and to request that he review this situation and report in writing through his faculty representative what corrective action has been taken or his reasons for taking no action to the December 1953 Meeting of the Conference, at which time further consideration will be given to this matter by the Conference. This motion carried (7 for, 1 against, 1 abstaining) (UCLA voting against, USC abstaining). I felt Jones of UCLA voted in an irresponsible manner, but I was happy to see that Willett of USC took a reasonable stand.

We also voted to limit all forms of Conference playing season varsity football practice to 10 hours per week and varsity basketball practice to 10 hours per week; freshman football practice was limited to 8 hours per week. In further action we amended the Athletic Code so that the opening day for basketball practice shall be the first day of November with the closing date being the last scheduled Conference game with the number of

games limited to 27 varsity, 27 junior varsity, and 10 freshman. We amended (unanimous vote) Note 2N2 to read: A student with deficient Carnegie units who establishes residence at a four-year institution, takes collegiate work for removal of such deficiency, and later transfers to a member institution, is subject to the transfer rule and the transfer penalty. The Representatives took action on a number of other agenda items, but none more important to me than the USC case, which turned out quite satisfactorily.

Thursday, June 11, 1953

In Seattle. Institutional violations, reported by the Commissioner, were considered this morning. California had one case of an alleged illegal interview of a prospective student by an assistant football coach--Stanford had the same. Then, from 11:40 a.m. until 12:10 p.m., we met with the Athletic Directors and discussed Rose Bowl receipts, etc.

From 1:00 p.m. until 1:45 p.m. we again discussed institutional violations. At 1:50 p.m. the Athletic Directors again met with us until nearly 4 p.m. while we took final action on a number of agenda items. Among the more important actions was the decision about television receipts in which it was decided that, after the deduction of the NCAA assessment, the gross proceeds received by any competing member institution from the sale of live or post television of all football games participated in by a member institution shall be reduced by the retention of 20% thereof and the balance paid to the Conference Treasurer to be distributed as follows: 5% to the University of Idaho, and 11.875% to each of the other eight member institutions. This carried by a vote of 6 to 3 with Idaho, Oregon, and Washington State voting against it. The measure was amended by a vote of 6 for and 3 against to be effective for the year 1953-54 only, with UCLA, USC, and UC voting against the amendment.

We voted unanimously to have a Conference Committee (H. P. Everest, Victor O. Schmidt, Hugh C. Willett, Orlando John Hollis, and Alfred R. Masters) meet with and negotiate a new contract with the Big Ten Conference for the Rose Bowl for a period of three years and also negotiate a renewal of the present contract between the Conference and the Pasadena Tournament of Roses.

Other action included the removal of the coaches' veto on the selection of football officials; approval for the Conference to permit telecasting of "sell-out" football games in 1953 as permitted under the NCAA television plan, and approval of live telecasting of night football games in 1953, again in accordance to NCAA policy.

One of the more important actions to me was the unanimous approval of a recommendation to the Commissioner that he be authorized to include in the mechanics of football officiating in games between Conference members the following arrangement regarding the commission of personal fouls: that any player guilty of a personal foul of the type requiring that he be reported to the Commissioner's office by name, be immediately and clearly identified by the official who calls the foul.

A motion also carried unanimously that, by the fall of 1954, all

football jerseys be numbered as follows: Ends, 80's; Tackles, 70's; Guards, 60's; Centers, 50's; and Backs, 10-49. The Conference representative should press this matter for adoption by the NCAA Rules Committee.

Final action was taken on the institutional violations. In the case of an alleged interview of a prospective student by an assistant football coach of the University of California, the matter was unanimously closed. The Spring 1953 Meeting of the Conference adjourned "sine die" at 3:55 p.m.

[In Berkeley the research group met as usual with the following people attending: Asaro, Cobble, Glass, Grover, Gunn, Hoff, Hollander, Hyde, Jaffe, Levy, Michel, Nervik, Passell, Perlman, Ruben, Slater, and Wallmann.

Asaro reported that they have been working with  $\text{Th}^{228}$ , looking for the second alpha group of thoron ( $\text{Em}^{220}$ ); they had predicted the first excited state from the decay of  $\text{Em}^{220}$  should lie at ~550 kev and be in about 0.5% abundance. They found an alpha group at 542 kev below the main  $\text{Em}^{220}$  alpha peak in ~0.3% abundance (i.e., decaying to a state 542 kev above the ground state of  $\text{Po}^{216}$ ). Asaro went into some detail about their prediction as to abundance and also about their setup. In response to a question from Perlman, he said they predict the first excited state produced in the decay of  $\text{Em}^{222}$  should have an energy of ~480 kev with an intensity smaller than 0.3%.

Asaro then went on to talk about the discrepancy in the gamma rays of  $\text{Th}^{232}$ , saying that Dunlavey measured the conversion electrons in coincidence with the alpha particles of  $\text{Th}^{232}$  and found electrons from a 55 kev gamma ray while French workers found 75 kev gamma rays in coincidence with about the same fraction of alpha particles as Dunlavey. Asaro said they looked at the gamma spectrum of  $\text{Th}^{232}$  (which contained  $\text{Th}^{230}$  and  $\text{Th}^{228}$  as impurities) in a gamma counter and saw gamma rays of 86 and 63 kev.  $\text{Th}^{228}$  has a gamma ray reported at 83 or 85 kev while  $\text{Th}^{230}$  has a gamma ray of 68 kev. He said that the 63 kev gamma ray is appreciably smaller in intensity than the 68 kev gamma, but they plan to alpha pulse analyze the sample in order to determine the ionium impurity, which should help them to determine if the 63 kev gamma belongs to  $\text{Th}^{232}$ . He also looked at the gamma spectrum of  $\text{U}^{234}$  and saw L x-rays and gamma rays of energy 52, 90, and 184 kev. He noted that  $\text{U}^{235}$  (present with the  $\text{U}^{234}$ ) has a gamma ray of 184 kev, but when they determined the abundance of the 184 kev gamma ray relative to that of the alpha particles of  $\text{U}^{234}$  and used the known abundance of  $\text{U}^{235}$  and the least favorable decay scheme of  $\text{U}^{235}$ , they found the 184 kev gamma ray to be twice as intense as expected if it were due to the  $\text{U}^{235}$ . He suggested a possible energy level scheme for the daughter  $\text{Th}^{230}$ , which includes the 184 kev transition.

Slater reported on d,p excitation functions of  $\text{Zr}^{96}$  and  $\text{Pt}^{198}$  and presented plots of these, plus one of all of the d,p excitation functions he has obtained so far:  $\text{Zr}^{96}$ ,  $\text{Pt}^{198}$ ,  $\text{U}^{238}$ ,  $\text{Mn}^{55}$ ,  $\text{Bi}^{209}$ ,  $\text{Th}^{232}$ , and  $\text{Pd}^{110}$ . In the discussion about the work, Hyde asked if he still believes that there are three isomers in  $\text{In}^{117}$ ; Slater replied that he does.

\* \* \* \* \*

Friday, June 12, 1953

I left Seattle on West Coast Airlines Flight 32 at 9:30 a.m. and arrived in Pullman at 11:55 a.m. Harold W. Dodgen, my host, met me and took me to the Wilson Compton Union, where the Northwest Regional American Chemical Society Meeting is being held. I heard a few talks and met a number of people. Then, at 7:45 p.m. I gave the principal address on the "Present Status of the Transuranium Elements" in the Wilson Compton Union Ballroom. Afterwards, there was a mixer in the American Legion Building from about 9:30 p.m. until midnight, after which I spent the night in the Wilson Compton Union.

Saturday, June 13, 1953

This morning I left Pullman at 8:01 a.m. on West Coast Airlines Flight 94 (a DC-3) and flew to Boise, where I caught United Flight 172 at 11:05 a.m. This flight arrived in San Francisco at 2:55 p.m.; a lab driver picked me up and drove me to Lafayette.

Helen and the kids seemed quite pleased to see me, and young Pete produced a report card, dated June 1, promoting him to second grade with normal progress in all academic subjects (reading, arithmetic, language, music, etc.) and in all social subjects except "uses self control." His teacher Phyllis Ryerson's comments read: "Peter has grown very much this year, not only academically but socially. With continued encouragement and reassurance he should do well next year. Peter tries very hard and still gets a little upset if he can't do something perfectly. I have been very pleased to work with Peter because he is so cooperative, helpful and courteous. He has many friends now and plays freely with them at recesses..."

Sunday, June 14, 1953

Much of my day was spent resting and being entertained by the kids.

Monday, June 15, 1953

My day was taken up with phone calls, administrative matters, conversations about the research, and mail.

I looked over the Chemistry 223 examinations and grades, which Iz handled while I was away: Peter R. Gray, final examination 180, total points 531, grade A; LeRoy G. Green, 177, 401, B-; James R. Grover, 216, 576, A; Richard M. Lessler, 156, 480, B; Ralph McLaughlin, 111, 376, B-; Hirdaya Mathur, 153, 455, B; David A. Pickler, 201, 524, A; Bruce A. Raby, 144, 454, B; Rex Shudde, 243, 549, A; Warren G. Smith, 219, 533, A; Frank Stephens, Jr., 285, 656, A+; Joseph Surls, Jr., 174, 501, B+; and Vernon Troutner, 93, 369, B-.

Included in the mail I read was another (June 9) letter from Norman Bonner, who said that he would like to receive a firm offer from the Livermore Laboratory. He added, "As for the salary, the \$700 figure adds up to only about \$700 more per year, after taxes, than I would make if I stayed here and spent the summer at Los Alamos, as I ordinarily do. Considering the

usual differential between academic and non-academic jobs, a salary of at least \$750 per month would seem more reasonable." Doral had had a copy made of the letter and had sent it to Ken Street.

In a June 12 letter Herbert E. Morey (David Starr Jordan High School) wrote that he had checked with my sister Jeanette about an informal reception for me in their library at 7 o'clock Friday evening, just before the graduation exercises. Jeanette had told him that she thought it would be all right, so they went ahead with their plans; he hopes this meets with my approval. I told Doral to draft a letter for me to sign saying that I'll be glad to attend.

About 10 a.m. I received a telephone call from Charlie Browne--He told me about the coral from island Jean, which has roughly the same specific activity as previous material, i.e., ~20,000 alpha c/m/100 g. I told him that we are interested in getting up to 10 tons of it. Charlie said that the material will be slow coming and will be shipped directly here with the water evaporated. He also told me that they are combining their 1/3 from us with their 200 pound batch (slightly more than that from us, about 4/3). Charlie added that he cannot understand how Argonne's garbage can of coral could be so hot. In conclusion I agreed that we will give him 1/3 of the product from the 10 tons of material.

I signed a letter addressed to Professor Matthew L. Holt (Associate Chairman, Department of Chemistry, The University of Wisconsin), who wrote on June 2 to ask for an evaluation of Richard W. Fink. I wrote:

I knew Dr. Fink from June 1948 to September 1949 when he was a member of a group doing graduate research in the Radiation Laboratory and Department of Chemistry at the University of California. During this time he showed more than average energy in attacking problems in the laboratory and produced some rather good work. His course work, however, was poor and since we had some misgivings about the maturity of his judgment, we decided to terminate his graduate work here at the Master's degree level. I might mention that Fink did not seem to realize that he was not doing well but rather had the attitude that he understood the material well and that there was something wrong with the questions asked.

I hesitate to write such a weak letter of recommendation since it is entirely possible that Fink may have overcome some of these problems since he left our laboratory and since I do feel that he has good native ability. However, I feel that it would be unfair to you to give him an unqualified recommendation based upon our experience.

Today I returned Joe Katz' preface to PPR, Volume 14A, to him with a couple of suggestions entered in pencil. I also made a few comments about the jacket material and a slight change in Chapter 1. I added that I am looking forward to seeing him on Friday, June 26.

I looked over a June 12 letter from Charles Coryell, who included a list of invited speakers who have accepted invitations for the upcoming Gordon Conference. Coryell made some comments about the program.

We had our usual senior staff brown-bag luncheon meeting in my office at noon and discussed a variety of business, in addition to talking about the

PCC meeting, particularly about the Cannamela case.

Later I read a June 12 letter from Donald Lane, saying that he is considering the several suggestions I made in my letter of May 27. He wrote:

On May 1 I requested General Counsel to loan me the five patent applications and work books. Since the documents are classified, the AEC Security Officer had to inspect my storage facilities, and I have moved in another safe, changed the combination, and have secured security approval. Yesterday, General Counsel furnished me with photostat copies of the five patent applications and three of the work books, adding up to 798 pages, and he states that photostats of two more work books are now being made for me. After I get the subject matter of the claimed inventions analyzed and condensed, I will correspond with Drs. Wahl and Kennedy about fixing dates.

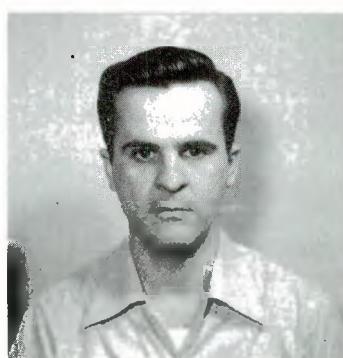
General Counsel has said that he will look into the matter of assignments made for these five applications and furnish me with data or copies. It is his understanding that two assignments were executed for each of these cases, one an assignment of title in escrow pursuant to contract eng-30 to permit the Government to get delayed prosecution procedures in the Patent Office, and another assignment probably made to give the Government title in inventions made under the contract with the Regents for research. When I can get copies of the assignments and see the dates and determine which have been recorded in the Patent Office, I can better advise you about your record title in the subject matter.

Your report that the Regents Committee on AEC Contracts now wants to be helpful is interesting. The Regents consistent refusal to execute the Certificate of Disclaimer in the form attached to eng-30 was anything but helpful, and has been the prime cause of all the difficulties in this proceeding. One possible action which you might propose to Mr. Neylan is that it would be helpful to your group if the Regents would state in writing that the Regents have no interest (or have only an implied right to use) in the inventions made by your group in this field prior to April 1, 1941. While the Regents are on record that they will not press a claim against AEC based on these inventions, it is quite possible that the Patent Compensation Board will still want to know "what is the Regents claimed interest" in order to evaluate the applicants' interest.

The important thing now seems to me to be the analysis of the several patent application claims, and I will proceed with this as promptly as possible.

Gregory Robert Choppin, our new postdoc from the University of Texas, began working today. I spent some time talking with him about his research.

Gregory Choppin



Tuesday, June 16, 1953

Our academic people and students went on the full-time summer pay schedule today. We were successful this summer in putting our foreign students, Per Kofstad and Hirdaya Mathur, on the payroll; Kofstad, however, will be leaving the end of the month to serve in the Norwegian Army.

After some administrative work and phone calls, I answered a few letters. Robley D. Evans (Massachusetts Institute of Technology) wrote on June 5, congratulated us on the excellence of the revised "Table of Isotopes," and asked for an additional two copies. These had been sent to him during my absence, but I dropped him a note to say that I am very pleased that he finds the "Table" useful.

To William H. Workman (Long Beach) who wrote on May 30 about his need for a chemist to develop a scheme that means financial independence once it is developed, I wrote that, in view of my position at the University, I am not permitted to participate in undertakings of the type which he outlined.

I also wrote to Prof. Dr. G. J. Sizoo in response to his letter of May 27 to say that it now appears definite that I shall not be able to attend the XIIIth Conference of Pure and Applied Chemistry in Stockholm this summer, but I shall be interested in reading the minutes of the meeting.

In the late morning I had a meeting with the laboratory physicians to discuss draft problems. Then, at 1:30 p.m. I met with Chancellor Kerr to report on the meeting of the PCC.

Much of the rest of the day was taken up with the students, talking about their summer research, etc.

Wednesday, June 17, 1953

After taking care of some routine matters, I went down to campus to Donald Coney's office (General Library) at 10 a.m. for the first meeting of the Television Committee. Members present were Leonard J. Black (Electrical Engineering), Thomas N. Barrows (University Extension), Donald Coney (Chairman), Edward W. Gifford (Anthropology), Lawrence A. Harper (American History), John D. Hicks (History), Harold E. Jones (Psychology), Albert G. Pickerell (Journalism), and Glenn T. Seaborg. Absent were Marie-Louise Dufrenoy (French) and Ralph Emerson (Botany). Coney first gave the events leading up to the creation of the committee, reported that the contract between BAEIA and the Regents has been signed by both parties and that it follows that the four questions directed to the Committee by the Chancellor (journal entry of June 5) will require answers shortly. The Chancellor's view, Coney said, is that question 2 (censorship) and 3 (administrative relationships) are the most important.

Coney went on to say that the question of relationships on the Berkeley campus between educational television, the administration, the faculty, and other groups, is likely to be more acute than on any other

campus of the University. He brought up such matters as space, relations with students because of the ASUC involvement, possibility of censorship, problems with unions because of student employment, etc. In our discussion we concluded that problems of management and censorship might prove more troublesome than it is worth in order to have a TV studio on the Berkeley campus. It was noted that there are fundamentally two questions: relations between the Berkeley campus, the statewide administration, and BAETA on the one hand; and on the other, the problems relating to the development of programs and the use of campus people therein.

It was observed that the Regents have not yet approved program participation by the University and that it would cost in the neighborhood of \$25,000 a year to develop and maintain a suitable program of University telecasts. Policy decisions should be made with respect to the University's objectives before an agency for developing programs be established. I offered to talk with Greg Engelhard (ASUC Director of General Activities) about ASUC's interest and plans for participation in television programming. Harper will talk with the Speech and Drama Departments about their reaction to the use of the sound recording studio and Little Theater in Dwinelle Hall by BAETA, and Pickerell with undertake to develop a statement regarding the Chancellor's question (4) on programs. We adjourned at noon and plan to meet again the last week of June.

Before going up to the hill, I stopped in the office of the Department of Chemistry to talk with some of my colleagues.

At 7 p.m. I attended the Senior Men's Banquet (Norman Mineta, student chairman) at the Hotel Claremont, where I presented the Jake Gimbel award to the senior making the biggest contribution to athletics.

Thursday, June 18, 1953

The following attended the research group meeting this morning: Asaro, Carniglia, Carr, Clark, Cobble, Feay, Glass, Gray, Grover, Hoff, Hollander, Hulet, Jaffe, Kalkstein, Levy, Nervik, Passell, Ruben, Seaborg, Slater, and Templeton. First Templeton reported that Ingmar Bergström and Robert D. Hill at the University of Illinois confirmed Michel's unpublished report of a 5-hour  $Tl^{198}$ , using a beta-ray spectrometer on  $Tl^{198-204}$  produced with deuterons on mercury. They resolved about 150 conversion electron lines. (Bergström will probably spend some time here later in the summer.)

Asaro reported that they have observed a fifth alpha group in the decay of  $Th^{228}$  to  $Ra^{224}$  at 5.137 Mev. The only possible contaminant is the 5.150 Mev alpha particle of  $Pu^{239}$  and this is too far away to account for the peak. This is the first time a fifth alpha group has been observed in an even-even nucleus; it is in 0.04% abundance relative to the total  $Th^{228}$  alpha emission.

Carniglia spoke for some time about their progress making vapor pressure measurements in the heavy elements, using the Knudsen effusion vessel. He said they have taken eight reliable points with this apparatus using  $PuF_3$ , which give a straight-line plot of  $\log p$  vs  $1/T$ .

over the temperature range 940°C to 1170°C, which he compared with O. C. Simpson's results. I pointed out that Westrum's direct determination of the melting point for  $\text{PuF}_3$  does not agree with Simpson's estimation of this value. Carniglia spoke of some changes in the equipment.

Hoff talked about the electron capture of  $\text{Np}^{234}$ , saying that he has been able to see only gamma rays at 1.57 Mev, 0.775 Mev, and a small, ill-defined peak at 0.230-0.260 Mev. He concludes that the 442 and 177 kev gamma rays reported by Orth and O'Kelley are not present or present in low abundance. Hoff presented a decay scheme for  $\text{Np}^{234}$  he worked out with his data, combined with that for  $\text{Pa}^{234}$ , to give the energy levels of the common daughter isotope  $\text{U}^{234}$ .

Hollander reviewed some of the background of carbon ion bombardments and then spoke about recent bombardments of  $\text{Cu}^{63}$  (99.7%) and  $\text{Cu}^{65}$  (99+%). The results indicate that the 35-minute activity seen two years ago is due to  $\text{Br}^{74}$ . Kalkstein looked at the gamma ray spectrum and observed  $\text{Br}^{74}$  gamma rays of ~1.3 Mev, ~0.6 Mev, and 2.08? Mev. For 1.6-hour  $\text{Br}^{75}$ , he saw a gamma ray of ~0.3 Mev. I added that we can tentatively assign the 4-minute activity to  $\text{Br}^{74}$ .

\* \* \* \*

On the hill I received a note of appreciation from Harold Dodgen for my help on their program last Friday. He suggested I send my bill to him or to their Secretary-Treasurer, Darwin L. Mayfield (University of Idaho). I promptly replied, said that I enjoyed my visit to Pullman very much and that I believe the fairest thing to do would be to divide my expenses to Pullman with those for my business in Seattle. I suggested that a bill of \$60 would be appropriate for the Pullman part of my trip.

While I was away a manuscript, "The Radical Pair Yield of Ionizing Radiation...", arrived from W. Albert Noyes (Editor, Journal of the American Chemical Society), who noted that the author feels the referees' comments are unfair. Noyes asked for my opinion, and today I wrote the the manuscript had been handed over to our expert on this subject, Dr. Amos Newton, since I was out of town. I noted that I reviewed the situation and found it so complicated that I am sending him Dr. Newton's memorandum. I added that Dr. Newton may have been a little harsh in his comments but he has made a number of suggestions that may be useful. I said I would be inclined to take seriously his main thesis that the paper is quite poorly written for I regard Dr. Newton as a first-class man and one whose judgment I usually find to be quite reliable. Finally, I commented that it appears to me if he has many cases as complicated as this one, it is a miracle that he is able to get The Journal to press.

I made a number of phone calls--to Bill Rice about our storage room, to Ian Turner about whether Kerr had made a decision about the Mira Vista Country Club proposal. Turner told me that Kerr does not believe that the University can participate; this, I believe, is unfortunate. I spoke with Greg Engelhard, who has volunteered to get some students and help me build our storage room. After visiting some of the labs, I went home early to spend some time with the kids before this next trip.

A lab driver picked me up at home about 7:30 p.m and took me to the

San Francisco Airport, where I caught the 9 p.m. United Flight 678 to Los Angeles. I arrived in Los Angeles at about 10:50 p.m. and took a taxi to my parents' home in South Gate.

Friday, June 19, 1953

In South Gate. I brought my parents up to date on our family's activities and eventually went out to Rio Hondo with Clayton Sheldon for nine holes of golf (CES-48, GTS-44). We played an additional two holes before I had to return home to get ready for the commencement speech at David Starr Jordan High School.

At 7 p.m. there was an informal reception for me in the library of David Starr Jordan High School, which my parents and Jeanette attended. It was really a thrill for me to see some of my old teachers who are still at Jordan, such as Gladys Gregg and George Mercereau. The Commencement Program, whose theme was "Duty, Honor, Country--Our Every Widening Responsibilities" began in the usual manner with the Processional "Pomp and Circumstance" at 8 p.m. There was music and talks by three students--Annie Louise Jones, Warner Davis, Corean Whitehead. I was introduced by Herbert E. Morey (Principal). I began by recalling that I graduated from Jordan 24 years ago and reminisced about some of the teachers, such as Dwight Logan Reid, Charles Ernest Hicks, Mabel Leta Gibson, and outstanding athletes (sprinters), such as Ellsworth Conway and Ashley Burch. I then went on to speak on "The Future and Atomic Energy," saying that science has made the world an interesting place, describing atomic energy, giving examples of numerous scientific developments that improved the quality of life, and adding that I envy the class of 1953 because they will live to see many more exciting developments. I am afraid, however, that I detected some resentment in the predominantly colored audience toward their role in a world dominated by white people. The program concluded with presentation of honors and awards, presentation of the class, conferring of diplomas, etc.

Saturday, June 20, 1953

I left my parents' home at 6:30 a.m. (taxi), went to the Los Angeles Airport, where I caught United Flight 608 at 8 a.m. bound for New York. I arrived in New York about 9 p.m. and took a cab to the Statler Hotel. (Iz arrived about an hour later on United Flight 602.)

Sunday, June 21, 1953

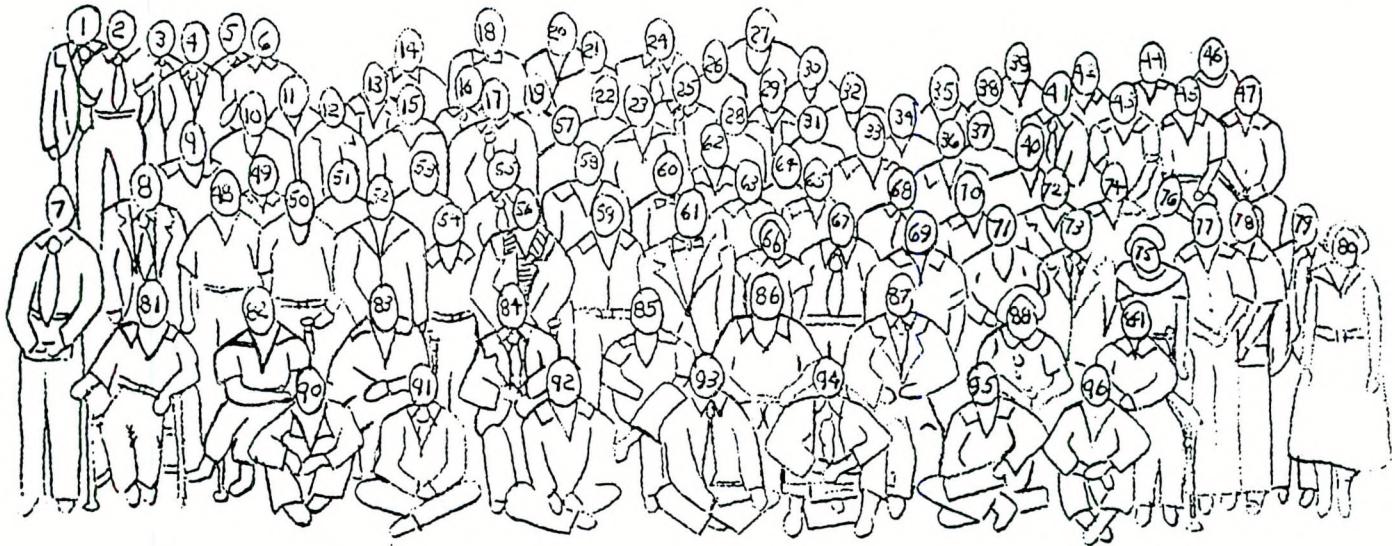
Iz and I left the Statler Hotel about 7:15 a.m. by taxi and went to LaGuardia Airport. American Airlines Flight 308 left New York City at 8:15 a.m. and got into Boston about 9:15 a.m. Here we took the Boston and Maine Railroad (train 307), which arrived in Franklin at 2:45 p.m. The trip from Franklin to New Hampton was made by cab. I was assigned to Room 11, Berry Hall. After greeting some old friends, Iz (who is co-chairman with Charles Coryell this year for this second Gordon Conference on Nuclear Chemistry) and I played nine holes of golf on the New Hampton School Golf Course, a rather short (4020 yards), par 66 layout in which the 18 hole normal round is achieved by playing the nine-hole course twice (IP-38, GTS-44).

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June 21-26 1953

1. J. W. Kennedy, 2. F. R. Mackenzie, 3. Manfred Lindner, 4. L. O. Morgan, 5. R. H. Tomlinson, 6. R. Herber, 7. W. G. Parks, 8. G. B. Cook, 9. Lester Winsberg, 10. A. R. Brosi, 11. Leo Yaffe, 12. R. B. Leachman, 13. W. W. Meinke, 14. A. W. Fairhall, 15. R. E. Batzel, 16. E. O. Wiig, 17. H. E. Menker, 18. N. Sugarman, 19. D. R. Wiles, 20. R. C. Fix, 21. W. Heckrotte, 22. R. A. James, 23. C. I. Browne, 24. Peter Kafalas, 25. R. F. Rider, 26. Gholam W. Bazorgan, 27. Sherwood Rowland, 28. R. W. Fink, 29. Paul Fields, 30. Paul M. Lantz, 31. E. K. Hyde, 32. A. A. Caretto, 33. Henry Selig, 34. John R. Huizenga, 35. Bruce Dropesky, 36. J. W. Cobble, 37. J. S. Gilmore, 38. T. P. Kohman, 39. C. G. Heininger, 40. A. W. Schardt, 41. T. V. Irvine, 42. R. M. Diamond, 43. Van Goetsenhauen, 44. S. S. Fraser, 45. N. E. Ballou, 46. T. Alex Eastwood, 47. R. A. Naumann, 48. J. M. Miller, 49. M. L. Perlman, 50. A. K. DasGupta, 51. S. N. Walton, 52. J. T. Horeczy, 53. John P. Balagna, 54. John W. Winchester, 55. Theodore C. Engelder, 56. Arthur Kant, 57. E. H. Fleming, 58. Norman Bonner, 59. H. L. Finston, 60. L. G. Stang, Jr., 61. L. S. Goldring, 62. R. Wolfgang, 63. R. B. Leachman, 64. T. C. Hoering, 65. J. Hudis, 66. H. L. Smith, 67. G. J. Atchison, 68. S. A. Reynolds, 69. T. Sugihara, 70. R. J. Prestwood, 71. T. T. Shull, 72. J. M. Hollander, 73. Nobufusa Saito, 74. R. W. Spence, 75. Joan Welker, 76. George P. Ford, 77. Ellis P. Steinberg, 78. Alex Green, 79. R. K. Sheline, 80. Marion Wood, 81. G. Friedlander, 82. Fred Brown, 83. L. E. Glendenin, 84. E. Segrè, 85. I. Perlman, 86. C. D. Coryell, 87. G. T. Seaborg, 88. Katharine Way, 89. L. W. Nordheim, 90. A. P. Baerg, 91. P. Fong, 92. F. Asaro, 93. G. Kavanagh, 94. I. Talmi, 95. R. A. Brightsen, 96. A. Zucker

There was much socializing after dinner. I talked with old acquaintances and former students, such as Karl-Erik Zimen of Göteborg, Bill Jenkins, and Norman Bonner, et al.

Monday, June 22, 1953

In New Hampton. This morning's session was devoted to Nuclear Systematics and chaired by Katharine Way (Bureau of Standards). Speakers were L. W. Nordheim (Duke) and I. Talmi (Princeton).

In the afternoon I played nine holes of golf with Rod Spence and Iz (RS-49, IP-42, GTS-41). The programs at these Conferences, with the afternoons free, are set up to allow ample time for relaxation and conversation.

I chaired the evening session, again on Nuclear Systematics, with John Huizenga and Frank Asaro as speakers. [I heard a rather amusing story about Frank Asaro, who has never been in New England during the summer and was concerned about the temperature. So, he arrived in Chicago where the temperature was over 100°, wearing a heavy winter overcoat.]

Tuesday, June 23, 1953

In New Hampton. This morning's session was on Fission, chaired by Larry (Lawrence E.) Glendenin, and speakers included G. B. Cook (Harwell), H. G. Thode (McMaster University), Ellis P. Steinberg (Argonne), Nathan Sugarman (Argonne), and Manfred Lindner (Cal Research, Berkeley). The second part of the morning had J. S. Fraser (Chalk River), Robert B. Leachman (Los Alamos), and Peter Fong (University of Chicago) as speakers.

In the afternoon Iz and I played nine holes of golf with Rod Spence and Rene J. Prestwood (RWS-40, RJP-40, IP-42, GTS-42). (Prestwood, who is now in Los Alamos, received his bachelor's degree in Berkeley in 1942; he did research in my group.) I had the opportunity to talk with Spence and Browne for a while and learned that the debris from the average weapon explosion has  $0.03 \text{ Cm}^{242} \text{ alpha particle}/\text{Pu}^{239} \text{ alpha particle}$ . This has application in long-range detection of nuclear weapon explosions. The average  $\text{Cm}^{242}/\text{Am}^{241}$  atom ratio = 11 soon after the explosion. Spence and Browne requested that I ask Al, Stan, and Bernie Harvey to determine the ratio of the absorption to fission cross section for  $\text{Am}^{241}$  for both thermal and fast neutrons at the EBR in Idaho.

Truman Kohman chaired the evening session on Nuclear Systematics with Ron Brightsen (Westinghouse) and Alex E. S. Green (Florida State) as speakers. Brightsen spoke on isotope regularities that has occupied so much of his time recently. Green reported some new constants in the Weizsächer mass equation. I learned that Kohman has found  $\text{Nd}^{144}$  with an alpha particle of about 1.9 Mev and a half-life of about  $3 \times 10^{16}$  years in natural neodymium, using photographic plates.

Wednesday, June 24, 1953

In New Hampton. I talked with Paul Fields and suggested that we

settle our dispute over the discovery of elements 99 and 100 by having a joint publication of element 99 and a Berkeley publication on element 100. Paul said he will think this over and discuss it with his colleagues. He went on to say, however, that the Argonne group now claims an earlier date (than the first revelation at the meeting of the Boner Panel in Washington in February) for their work on element 100.

Emilio Segrè chaired the morning session on High Energy Transmutations and Photonuclear Reactions with Warren Heckrotte (Berkeley), J. M. (J. Malcolm) Miller (Columbia), and Gerhart Friedlander (Brookhaven) as speakers. Friedlander gave an interesting talk about their new "Cosmotron".

Later I spoke again with Rod Spence, who did not remember the earlier date claimed by Argonne for the 6.6 Mev alpha particle of the isotope of element 100. [Our difficulties with Argonne and the Mike work are continually on all our minds.]

Rene Prestwood, Iz, Bill Jenkins, and I went out for nine holes of golf in the afternoon, but Iz had a terribly allergic reaction to a hornet sting and had to quit after one hole (RJP-35, WAJ-39, GTS-44).

Tony (Anthony L.) Turkevich chaired the evening session on High Energy Transmutations with speakers Isaac Halpern (MIT), Alexander Zucker (Oak Ridge), and Jack M. Hollander (Berkeley).

Thursday, June 25, 1953

In New Hampton. Gerhart Friedlander chaired the morning session on Nuclear Techniques, Physical. The speakers were Morris L. Perlman (Brookhaven), Albert R. Brosi (Oak Ridge), and Alois W. Schardt (Oak Ridge). I spoke with Morris Perlman about fluorescence yields in the decay process.

Joe Kennedy, Emilio Segrè, and I got together for a short, but serious discussion about our patent problems. Kennedy is going to write to Lane, and it was agreed that I should attempt to see Lewis Strauss when I am in Washington in September.

I left the Conference early, took a taxi to Franklin, and the Boston and Maine Railroad Train 307 (at 1:54 p.m.) to Boston, arriving at 4:10 p.m. There I went by cab to the airport, caught American Airlines Flight 33 at 5:15 p.m. for Chicago. The flight arrived in Chicago at 8:45 p.m., I was picked up by a lab driver and taken to the Argonne Guest House.

[In Berkeley, the group meeting was small, attended only by Carr, Clark, Dunlavey, Gunn, Jaffe, Kalkstein, Lessler, Levy, Nervik, Passell, Raby, Ruben, Shudde, Smith, and Wallmann. Lessler gave a literature report on the article by Gertrude Scharff-Goldhaber, "Excited States of Even-Even Nuclei," Phys. Rev. 90, 587 (1953).]

\* \* \* \* \*

Friday, June 26, 1953

In Chicago. I went out to Argonne where, among other things, I attempted to recruit Joe Katz for a position with Project Whitney. Joe said that he wouldn't come at \$14,000, a research professorship, and the associate directorship of the Whitney chemistry group. He said he might take an associate professorship in the Department of Chemistry (ca \$7000/year) and the associate directorship of the Whitney chemistry group, for a duration of a few years. When I asked if he would come for six months on a leave of absence basis, he said that he will think about it. He did say that Henry Taube might be interested and also suggested David M. Ritter (University of Washington) and Alfred O. Walker (National Aluminate Company, Chicago). I also told Katz about some of the background, facts, etc. of the controversy over the Mike work. I called and talked with Taube, who told me that he is not interested in a job with the Whitney Project; his wife has a hay fever problem, but he doubts that this would improve in California because it doesn't improve at Los Alamos. I then saw Harold Feder and asked if he would come to Whitney for \$900 to \$1000/month; he said he will consider the offer.

Later I got together with Sherman Fried and Martin Studier (they gave me the fission cross section of  $U^{237}$  as  $46 \pm 8$  barns--work by G. L. Pyle, S. Fried, William C. Bentley, and Jerome L. Lerner). We discussed the Mike matter for approximately 1 1/2 hours. I offered to share the discovery of element 99 if they leave the discovery of element 100 to Berkeley although they could make an independent claim on this element (this would be subject to Al's and Stan's approval). They said they wouldn't go independent on element 100 because the controversy on one element is as bad as on two elements. Studier admitted that the 6.6 Mev alpha element 100 and element 99 peaks were of equal heights on their February 20 plot and that this corresponds to the status on December 23, i.e., the extrapolation goes back to December 23; but they now say that the ratio of element 100 to element 99 was 20% on that date (later changed to 10% by Sherman Fried after Studier left). We discussed the effect of the use of wrong half-lives, and I pointed out this would make their case worse. I had the impression that Studier understood me. Fried told me that Manning admitted to him that he said that element 99 and element 100 had 6.6 Mev alpha intensities of equal heights at the Boner Panel meeting of February 18. I pointed out the analogy between our erroneous element 100 spontaneous fission and their element 100 6.6 Mev alpha particle in confusing the claims to discovery.

Studier said they now claim chemical identification of the 7 Mev alpha-emitting element 100 isotope. He added they saw a 13-day 6.6 Mev alpha particle at the leading edge of the slope of the elution curve (March 10 elution) and showed that it fits a 17-day slope. Studier noted they also saw a very very poor 10-day slope after subtracting the long-lived background (presumably not pulse analyzed) for the December 23 run; this run included a 6.1 Mev alpha particle not due to californium whose elution position was established by the measurement of spontaneous fission. They saw no element 100 spontaneous fission on their March 10 run (no counts for four days) when they saw 1000 spontaneous fission counts due to californium. I told them about our elution experiment in which our element 100 fraction was completely separated from element 99 and californium. They said their mass spectrograph measurements give

245/246 = 2.7 and 246/247 = 13. Studier said I had the advantage of knowing their data, but I pointed out there is no advantage to knowing wrong data. I added that I, on the other hand, have taken the trouble to come to try to straighten things out.

Studier gave me a plate with a few hundredths of a count/minute of their 6.35 Mev alpha particle on it with no thoron daughters (from J. Mech) for Al Ghiorso to analyze. We talked about Studier's trouble in the  $Pu^{243}$  matter but with no particular conclusion; there was no other discussion about the 6.35 Mev alpha particle. I reiterated my diagnosis that their confusion about the chemical identification of the 6.6 Mev alpha particle invalidates their claim to the discovery of element 99. Fried then informed me that he has withdrawn as a co-author of the Argonne paper on elements 99 and 100 and said he thought my proposal for a compromise constitutes "a trade." I said that this is not so. Studier was concerned whether the next nuclear weapons shot will be a good one for the observation of heavy isotopes, and I said, "Yes." Finally, I told them about our new low limits on the intensity of any 6.6 Mev alpha particle of element 100, e.g., less than 5% of that of element 99 on December 14. Studier said he was the first to have the idea that there might be two 6.6 Mev alpha particles, i.e., one due to element 99 and one due to element 100. He mentioned that Manning favored greater than 50 days for the half-life of the element 99 isotope although he (Studier) said approximately 35 days at the time. We discussed the possibility of my withdrawing as a discoverer of element 99, on the basis that this might be an inducement to them to agree to a compromise.

Later, I saw Donald Peppard and learned that he would at least consider an offer of about \$14,000/year to join Project Whitney. I also talked with Darrell Osborne for a while about the role of f electrons in contributions to entropy in  $UO_2$ ,  $NpO_2$ . Fried told me that Argonne has decided not to build a lab in the Pacific to process debris from next spring's shot.

I left Chicago at 7:55 p.m. on United Flight 611 bound for Oakland. It hasn't been a particularly satisfactory visit.

Saturday, June 27, 1953

My plane arrived in Oakland at 3:30 a.m.; I took a taxi to Lafayette (\$5.20). I rested as much as I could today with four active children around the house.

Sunday, June 28, 1953

Again I spent much of the day with Helen and the kids. Later I telephoned Greg Engelhard to talk, not only about the construction of our storage unit, but also about the role of the ASUC in educational television. Greg said they originally promised (in 1948) \$25,000 to help equip a FM radio station. The subject was reopened this year to include television--there was a meeting with George A. Pettitt (Assistant to the President). The program was somewhat different than the original plan. The ASUC agreed to participate to the extent of \$25,000, provided the University goes along and provided the decision is made before the end of next fall semester. The students are somewhat lukewarm for they fear

they would not have the opportunity to appear on programs. They say that, if the hours from 1 until 9 p.m. are usurped by other uses, they are afraid that they couldn't fit into the outside hours and that perhaps it would be difficult to get "activity experience" for the students. However, if one or two good hours could be allocated to the students, perhaps the use would be better. Some of the suggestions are Queen judging, Big Game rally, UN meeting, etc. Greg also said that the Glee Club, Treble Clef, Band, Mask and Dagger revue would be interesting to telecast from their point of view. He feels some of the lukewarm attitude would disappear if the ASUC were to get some use out of it. Their present understanding is that they will have to buy time. He said the ASUC does not seem to consider the University use of Dwinelle facilities is an encroachment of their facilities. Greg added that the \$25,000 would not be a recurring commitment; it is just a one-time contribution in order to honor their previous commitment.

Monday, June 29, 1953

I took care of some administrative matters and phone calls (including a call to Greg Engelhard about a couple of matters), met with a few new people (including Mary Joan La Salle, a new graduate student from The Cardinal Stritch College in Milwaukee who began working today, and Bruce R. McGarvey, the new instructor),



Mary Joan La Salle



Bruce McGarvey

and had a meeting about the chemistry building for Project Whitney. In addition, I looked over the mail, which included an extension of my consulting agreement with Argonne until June 30, 1954. This I signed and returned to Argonne.

The page proof to my chapter for the Sneed book arrived while I was away. I put it aside to read later this evening.

Stan, Al, and I got together to talk about the following letter that arrived from Winston Manning while I was away and also about my conversation in New Hampshire with Rod Spence and Charlie Browne and my conversations in Argonne Friday with Marty Studier and Sherm Fried.

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Argonne National Laboratory

~~DECLASSIFIED~~ *local 3*

*May 14 1953*

TELEGRAPH ALX 18 LEMONT ILL

TELEPHONE BISHOP 2-1500 CHICAGO ILL  
P.O. BOX 299 LEMONT, ILLINOIS

June 17, 1953

Dr. G. T. Seaborg  
Radiation Laboratory  
University of California  
Berkeley 4, California

Dear Glenn:

At this time I will comment on only a portion of your letter of June 5, 1953.

Concerning the question of the resolution of the 6.6 mev alpha into more than one component, I will make three points.

*Classification changed to Dr. G. T. Seaborg on 6-25-61*

*By authority of Dr. G. T. Seaborg on 6-25-61*

*Date: 6-25-61*

- (1) The amounts of 6.6 mev activity roughly indicated to you on February 20 were derived from the elution of December 23, 1952, and represented amounts estimated as characteristic of December 23 rather than the amounts existing at the time of your February 20 visit. We are surprised that this was not clear to you. Your recollection of equal intensities of 10-day and 22-day components as of February 20 would imply an overwhelming preponderance of the 10-day component in December\*. Such a preponderance would be at variance with data indicated in ANL-WMM-1076 and, I presume, in conflict with Berkeley data.
- (2) The resolution is dependent on elution position, on observed decay rates, and on the half-lives assigned to 100, to 99, and to the beta-emitting 98 parent. The amount assigned to 100 as of a given date, based on ~10, ~22, and ~16 days for the 100, 99, and 98 half-lives, is somewhat smaller than that assignable on the basis of 15, 40, and ~10 days for the three half-lives. The change due to these revised constants is less significant than the effect of point (1) above.
- (3) With the shorter half-life component eluting ahead from a citrate column, it is entirely feasible to establish an effect of the order of ten per cent on the basis of early Argonne elution data. As an example from material

\* The back extrapolation could be modified by a relatively long-lived parent of the 10-day component.

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- 2 -

To: G.T.Seaborg  
From: W.M.Manning  
June 17, 1953

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you have at hand, consider Figure IV-2 in ANL-WMM-1076. The fraction of activity indicated as 6.56 mev in drop 17 is less than 4% of the total indicated 6.56 mev activity. A shorter-lived component may be readily detected under such circumstances.

I hope you will give some further attention to the discrepancy between Figures 11 and 14 in MB-IP-740. The number of counts indicated in the 100 fraction of Figure 11 for 6.6 mev and for 7.1 mev is certainly sufficient to make any violent statistical fluctuations exceedingly improbable. The errors assigned to 7.1 mev points in Figure 14 indicate fewer counts for the measurements shown in Figure 14, but if I interpret this figure correctly, you still must have assigned nearly sixty events to 7.1 mev in the sample corresponding to drop 28, and about fifteen events to 7.1 mev in the sample corresponding to drop 25. Is there any record of the number of 6.6 mev counts for these samples at the same time? The questions of the amount of 6.6 mev activity in your 100 fraction as indicated in Figure 11, and its decay rate, seem to us to be of some importance; insofar as we can estimate from the fragmentary evidence of your Figure 11 in conjunction with Figure 14, the amount is in the range expected for early March according to Argonne data.

Sincerely yours,

*W.M. Manning*

W. M. Manning  
Chemistry Division

WMM/dr

Copies to:  
R.W.Spence  
C.I.Brown

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~~SECRET~~

After the noontime senior staff meeting in my office, I made the rounds of the labs to check on the research of the students.

Al gave me his data on the pulse analysis of the sample Marty Studier gave me on Friday. Al found no evidence for a 6.35 Mev alpha particle:  $0.0026 \pm 0.003$  c/m. I mailed this information to Studier and asked if he feels it would be worthwhile to reduce the error in the determination by further counting.

I also telephoned Dana Murdock to tell him of Clark Kerr's negative decision about the University's participation in the Mira Vista proposal for membership. Murdock said that he will talk with Fran Watson to see if something can be arranged so that we can play there on some sort of a basis.

Tuesday, June 30, 1953

I returned the page proof to my chapter for the Sneed volume to him, saying that on the whole it is very satisfactory. I wrote that I made a few pencil corrections that can be made quite easily.

In response to a June 15 letter from Professor Antonio Nasini about Professor Mario A. Rollier as an Italian representative on the Advisory Committee of the Joint Commission on Radioactivity, I wrote to Professor F. A. Paneth at the University of Durham, giving him the background of the matter and saying that I know Rollier personally and have a high opinion of him. I wrote that I should like to suggest such an appointment since I believe such representation of Italy seems to be desirable. A blind carbon of my letter went to A. Nasini.

I answered a June 30 letter from John Rasmussen, who told about their search for element 99 at the Nobelinstitutet för Fysik, using nitrogen ions on uranium. I offered some advice on the matter. In addition, I told John that it is all right with me for him to stay on there as long as possible, adding that the arrangements for his assistant professorship in the Radiation Laboratory are practically completed. I asked John to let me know as soon as possible about his desires about teaching this fall, so that I can get in touch with Miss Kittredge and see how this fits with her plans.

Ed Westrum sent me a copy of his careful analysis of the draft copy of "Classification Procedures for AEC Research Contractors," which was addressed to H. F. Carroll (Declassification Branch, AEC, Oak Ridge). Westrum is concerned about the impact of these restrictions on uranium research in both AEC laboratories and in more academic institutions. He suggested a letter of support to Carroll if my views agree with his.

I also received a consultant agreement from the Department of the Army in connection with my membership on the ACS Committee, Advisory to the Chemical Corps, extending it until June 30, 1954 (rate \$40/day).

In the afternoon Al and I went to Tilden Park and played 18 holes with George Briggs and Greg Engelhard (GB-92, GE-96, AG-93, GTS-95). Al and I won a low ball low total match by a score of 4 and 2.

Wednesday, July 1, 1953

After a number of phone calls, etc., I replied to a June 23 note from Donald A. McPherson (Row, Peterson & Company), who suggested I join him for a meal on September 10, 11, or 12 during the ACS meeting in Chicago. I wrote that I may have to go to Washington on Thursday and Friday (10 and 11); in the event I can stay over, I wrote, I tentatively suggest breakfast on Thursday, September 10.

Considerable time was spent in the labs today, particularly looking in on the research of the students.

Later, I went down to campus and at 2 p.m. I went to the second meeting of the Television Committee in Coney's office. Others present were Barrows, Coney (chairman) Gifford, Harper, Hicks, Jones, and Pickerell. I reported on my conversation with Greg Engelhard, who said that the ASUC transferred its tender to the Regents for helping to equip and operate a campus FM radio stations (\$25,000 for equipment and \$12,000 per year for a period of three years toward operating expenses) to a campus television broadcasting station, with certain provisions. I reported that ASUC opinion on a local television broadcasting station appears to be lukewarm, that Mr. Engelhard has not had a real opportunity to explore the situation since his appointment as ASUC Director of General Activities, and that the ASUC is apparently not of an opinion that the use of the FM studio space in Dwinelle Hall for a TV studio is an encroachment on ASUC rights.

Harper reported that the sound recording studio in Room 36C of Dwinelle Hall is used by the Speech Department, which expects to develop a project to manufacture audio aids for high schools. If this occurs, it will need more space than is now available. He also reported that the Dramatic Arts Department is now using storage space in the area adjacent to the proposed TV studio and into which the TV studio would be expected to expand. It was reported that cooperative use of the Little Theater space might be arranged.

Pickerell summarized his conversation with Jonathan Rice, program director of BAETA, who believes that each university participating should have a program director of its own. Stanford already has such a man and San Francisco State College is looking for a program director, and Agriculture Extension is looking for such a person. The salaries are about \$6,000 to \$7,000 for able and experienced producers. Rice also said that BAETA is considering the John O'Connell Trade School in San Francisco as an alternative to Dwinelle for its first studio. Pickerell cited the pros and cons of these sites.

The group raised a number of questions about a professional TV staff, including such points as where it should be located, from what budget its expenses should be paid, etc. We also began a discussion about broadcast control, including protection of the University from criticism resulting from inappropriate programs, although it was observed that there is very little likelihood of danger since the schools and BAETA will want to avoid criticism. It was suggested that University Regulation No. 17 be adapted to TV use as a standard for judging programs. The meeting was over at 3:30 p.m.; however, I left somewhat earlier because I had a 3:30

p.m: appointment with President Sproul to brief him on the actions taken at the meeting of the Pacific Coast Intercollegiate Athletic Conference in Seattle last month.

Thursday, July 2, 1953

The following attended the "open" meeting of our research group: Asaro, Cunningham, Dunlavey, Feay, Glass, Grover, Harvey, Hollander, Jaffe, La Salle, Levy, Mathur, Nervik, Passell, Perlman, Ruben, Seaborg, Shudde, Slater, Smith, Templeton, and Wallmann.

Mathur reported on his latest experiments on  $Cs^{127}$ , produced by 70 Mev helium ions on  $I^{127}$ ; he has now found two gamma rays, which are in cascade, by measuring the conversion electrons associated with them and by scintillation counter-pulse analyzer studies. The energies are 420 and 125 kev. He has observed two groups of positrons, the most energetic of which has an end point of 1.2 Mev. Mathur presented a decay scheme.

Levy talked about his experiments to determine the relative yield of  $Co^{58m}$  and  $Co^{58}$  from helium ion bombardments of manganese. He discussed the background of the work and said that he is presently using thinner targets in the energy range 20-25 Mev. In a recent experiment, still with a fairly thick  $MnSO_4$  target and ~23.5 Mev helium ions, he obtained a result of about 2.3 for the ratio of the relative yield of  $Co^{58m}$  to that of  $Co^{58}$ . This is being checked. There was considerable discussion about the work.

Passell presented data from a recent run on the double focusing beta spectrometer of  $Am^{241}$ , including their tentative assignments of the electron lines. He plans a confirmatory run.

\* \* \* \* \*

I stopped in the Department of Chemistry office to speak with Miss Kittredge and some of my colleagues before going up to the hill.

Charlie Browne telephoned at 10:45 a.m. to tell me that an aircraft with the incoming coral (17 tons of damp coral in metal containers that is about 1/3 by volume of water) will leave July 15 for Travis Field. He will let me know the exact time. Charlie said that he would like to visit in November to check our Castle plans (the spring 1954 tests in the Pacific); naturally, I agreed. Al got on the phone and gave Charlie the latest information on the neutron capture cross section of  $Cm^{248}$  (~0.1 b), the long-range alpha particle of  $Cf^{249}$  (~6.0 Mev), the half-life of  $99^{253}$  (~19 days), the half-life of  $Cf^{253}$  (~19.9 days). Al said that there is better than 50% probability, conservatively estimated, that an isotope of element 100 found in Paydirt, undergoes decay by spontaneous fission. Charlie asked that we send him our new information by memo or letter. Al also reported that he found only about  $0.0026 \pm 0.003$  c/m of the 6.35 Mev alpha particle on the plate that Studier gave me when I visited Argonne on June 26. Al also explained his theory that there are double sub-magic numbers in  $Cm^{248}$  ( $Z = 96$  and  $N = 152$ ). We learned that Browne is making plots of disintegration energies vs neutron numbers a la Kay Way and is having good results.

Today's mail brought me a \$60 check from Darwin L. Mayfield (Secretary-Treasurer, Washington-Idaho Border Section, ACS) for my expenses in connection with my recent talk there.

I made a few phone calls, including one to Greg about arranging a golf game, and then visited some of the labs. Some of the fellows have been loading americium slugs for Arco in Building 5A (Annex) but not without some problems, including spills, high air counts, etc.

Friday, July 3, 1953

Professor J(ulian) L. Culbertson (Chairman, Department of Chemistry, The State College of Washington) talked with me when I was in Pullman about a nuclear chemist for his department. I discussed the matter with Dick Glass, and today I wrote Culbertson that Glass indicates a sufficient amount of interest so that it seems to me that it would be worthwhile for him to correspond with Glass about the details. I wrote that Glass is a very good man whom I can recommend very highly.

After handling other administrative matters, I discussed research with some of the students and visitors. During my rounds of the labs, I learned that one of Cunningham's graduate students, Ralph McLaughlin, is scheduled to marry another member of our group, Patricia Maguire, tomorrow. The ceremony will take place in a seminary in San Anselmo (Marin County).

I then got together with Stan and Al and prepared the following response to Manning's letter of June 17:

DECLASSIFIED

Classification changed to  
D. J. Ohman 4/3-4/20-61  
by authority of  
5/5/61  
Date  
Person making change

DECLASSIFIED

~~SECRET~~  
~~SECURITY INFORMATION~~

July 3, 1953

MS-GTS No. 124

This document consists of 2  
pages and 1 figures.  
No. 3 of 3 copies, Series A

Dr. W. M. Manning  
Argonne National Laboratory  
P. O. Box 299  
Lemont, Illinois

Dear Winston:

This is in reply to your letter of June 17, 1953 which I found upon returning from my last trip to the East.

Since I misinterpreted the date to which your intensity of 6.6 Mev activity corresponds, it may be worth indicating to you some of the reasons for my conclusion. Since the intensities of the 6.6 Mev 15-day 100 alpha, 6.5 Mev >50-day alpha, and 6.1 Mev Cf alpha shown to me on an elution curve on February 20 were about equal (essentially the same information as presented by you at the Boner Panel meeting), this seemed to point to a February rather than a December date when the ratio of total ~6.6 Mev alpha to 6.1 Mev alpha was nearly 10. Also my suggested explanation of your two 6.6 Mev alphas, correctly represented in the middle of page 26 in ANL-WMM-1076, which was not accepted but certainly seemed to be understood, only makes sense on the basis of "two 6.6 Mev alphas" having comparable intensity in February. In any case my main point is not much changed, i. e., most of the original "6.6 Mev 100" alpha intensity now seems to have been due to 6.6 Mev 99 alphas.

I asked Al what his present best values are for the half-lives of the Cf<sup>253</sup> and 99<sup>253</sup> and he says they are 20 days and 19 days respectively. Surprisingly enough, from my point of view, these half-lives account for the present observed intensities of these two activities. Since your resolutions depend on these half-lives, it might be worth considering whether our values would lead you to any different conclusions.

I have studied in some detail the question which you raised in the last paragraph of your letter because if we have any evidence at all for a 6.6 Mev 100 alpha, I certainly want to be aware of it. As you indicated, the intensity of the 6.6 Mev alpha peak plotted in Figure 11 of MB-IP-740 is essentially correct with no great error due to statistical fluctuations probable. However, where the statistical fluctuations come in, and this is the central point, is for drop 29 where the intensity of the 6.6 Mev alpha is  $0.20 \pm 0.12$  counts per minute. Thus if this intensity is actually 0.3 counts per minute, which is quite probable, the elution curve for 99 in Figure 14 would take on a reasonable half-width about equal to that of

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Dr. W. M. Manning  
Lemont, Illinois

July 3, 1953  
Page Two

the Cf curve and would pass normally at drops 26-29 through the intensity of 6.6 alphas represented in Figure 11. It may not be clear from Figure 14 but plates 24, 25, and 26 were pulse analyzed together as a group; similarly the four plates 26-29 were pulse analyzed together and also the plates 30-32. Thus, I cannot give you the information which you requested for the specific drops 25 and 28. The first one-third of the 100 peak (drops 24-26) contained  $\leq 0.01$  count per minute of 6.6 Mev alpha and  $\sim 0.1$  count per minute of 7.1 Mev alpha. Since the intensity of the 7.1 Mev alpha was about 4% that of the 6.6 Mev alpha on March 1, 1953, an upper limit of 0.4% as of this date can be placed on the amount of "6.6 Mev 100 alpha" as compared to the 6.6 Mev 99 alpha. Although the decay of the 6.6 Mev alpha on plates 26-29 was not followed continuously, we have points early in March and a point taken on June 10, and these indicate a 20 day half-life as shown on the enclosed plot.

Cordially yours,

Glenn T. Seaborg

GTS:lnk  
Enc.

cc: R. W. Spence-C. I. Browne

~~SECRET~~

DECLASSIFIED

Saturday, July 4, 1953

Greg Engelhard came out this morning with a red-haired young fellow named Don Newberry, a helper in the ASUC carpenter shop, and the three of us put up a storage shed adjoining the far end of our double garage.

Helen purchased about a dozen rose bushes at Orchard's Nursery, and later, in the hot sun of the afternoon and under Helen's supervision, I planted them in the strip between our driveway and the front lawn that our gardener, Lloyd Rudholm, put in.

Sunday, July 5, 1953

Again this morning Greg Engelhard, Don Newberry, and I worked on the storage shed. It is more or less finished except for a few odds and ends.

Monday, July 6, 1953

I took care of some administrative matters and then wandered through the labs to check on the research of the students.

In today's mail was a July 1 letter from John Rasmussen. John reported that they are leaving July 4 for a vacation in England and the Birmingham conference on nuclear physics. He described the nitrogen ion bombardments of uranium they have been doing at the Nobel Institute and said that the 1-2 hour electron activity is apparently due to yttrium and not element 99, as they had hoped. He said that he will return to Berkeley at least five days before the start of fall classes, which he has learned will begin on September 21. I routed John's letter to Stan, Iz, Al, et al.

Also in the mail was a note from Darrell Osborne, who included a copy of their manuscript, "The Heat Capacity of Thorium Dioxide from 10 to 305°K. The Heat Capacity Anomalies in Uranium Dioxide and Neptunium Dioxide." Darrell told me about the paper when I was in Chicago about ten days ago. He and Westrum are submitting this to The Journal of Chemical Physics

A letter also arrived from J. W. Buchta (Editor, Reviews of Modern Physics). Buchta wrote that he is pleased with the appearance of the April issue (containing our "Table of Isotopes") and asked if he should present a formal bill covering the \$8.50/page publication cost. I immediately replied that he should submit a bill, which we need for accounting purposes.

I received a letter from W. H. (Bill) Johnston of Purdue University. Bill asked if it would be convenient to consider the early or middle part of October or toward the end of November for my visit to Purdue.

A note also arrived from M. C. Sneed, thanking me for returning the corrected page proof for my chapter so promptly and saying that they expect to have Volume I out within three months or less. Sneed wrote, "I am indeed grateful to you for writing such an outstanding chapter for our series."

One of the subjects at our brown bag senior staff luncheon meeting was the matter of suggestions for research problems for our new graduate students.

The fellows are continuing to have difficulty loading the americium slugs in Building 5A. Today one of the slugs was found to be leaking, and the Health Chemists have made an aluminum cap to be welded onto it.

Later in the afternoon I went to Tilden Park with Greg Engelhard for a little golf and much conversation--this included some discussion of the relationship of the ASUC to the proposed television station.

Tuesday, July 7, 1953

Much of my day was spent reading reports and journals although I did go through the labs to encourage the students and discuss their research.

I sent A. V. Grosse a copy of a May 29 letter I received from Dr. G. Schwarzenbach, noting that I believe it may be of interest to the ACS Committee on Foreign Compendia.

At 1 p.m. I attended the third meeting of the Television Committee in Donald Coney's office. Only the following members attended: Coney, Emerson, Gifford, Harper, and Seaborg--not even a quorum. We went over the minutes of the last meeting and suggested a few changes. I said that I will check the extracts of the ASUC minutes to ascertain whether the resolution finally adopted by the ASUC included continuing support payments by the ASUC for the BAETA TV station in Dwinelle Hall. Since so few members were present, we talked mainly about the problem of TV broadcast control and programming. With regard to University Regulation No. 17, we learned that Dean Hurford E. Stone approves speakers requested by student groups and Vice-President Baldwin M. Woods approves speakers requested by members of the faculty. We concluded that neither of these offices were appropriate agencies for carrying out the intent of paragraph 5 of the Regents' television resolution on the grounds that one is a state-wide and the other a non-academic officer. It was also concluded that neither our committee nor a technical adviser should act in this capacity. The whole matter will be discussed further at our next meeting. We also talked about programming development--we feel that the idea of censorship should be divorced from the Committee. We adjourned at 3:15 p.m.

Wednesday, July 8, 1953

Burris Cunningham reviewed a research proposal ("Kinetic Studies of Oxidation-Reduction Reactions" by A. C. Wahl) that Walter R. Kirner (Program Director for Chemistry, National Science Foundation) sent me. Today I returned the form, saying in part, "In summary, the research problems would seem to be well formulated, planned, and desirable, and on the basis of the recommended numerical rating, would be placed as 1- or 2+. Perhaps the only undesirable features of the proposal are that there seems to be some repetition of investigation on systems studied by other workers, although this is to some extent determined by the nature of the problem..." I rated the proposal as excellent.

I signed a response to a letter of July 6 from James Stokely (General Electric, Schenectady), who asked about element 100, which was reported in a new book "Inorganic Chemistry: A Text Book for Advanced Students" by E. de Barry Barnett and C. L. Wilson. I wrote that the reported discovery of element 100 is premature and it is unfortunate that this item appeared in a new book. I added that about two years ago the discovery of both elements 99 and 100 was reported in Europe, apparently based on a misinterpretation by the press of a speech in which it was stated that we were attempting to produce these elements.

A July 6 letter arrived from Donald Lane, listing the material that Roland Anderson has turned over to him. He asked if I can tell from the listing whether these workbooks probably include all that he will need to consider with respect to the S-52 series of cases. Lane also said that he now has copies of the recorded assignments relating to the patent applications.

At 6 p.m. I went to the Claremont Hotel to attend a reception for the Military Liaison Committee.

Thursday, July 9, 1953

The research group meeting this morning was attended by Asaro, Carr, Chapman, Choppin, Clark, Cobble, Feay, Glass, Grover, Harvey, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Levy, Passell, Perlman, Raby, Seaborg, Shudde, Slater, Smith, Templeton, and Wallmann

Asaro went into considerable detail about the experimental procedures and the results of his and Lou Slater's study of the gamma ray spectra of the  $U^{230}$  family. They were able to show that  $Ra^{222}$  has a 330 kev gamma ray and probably an 85 kev K x-ray in its decay. Asaro presented a decay scheme for  $Th^{226}$ .

Passell then talked about his work this past week on the gamma spectrum of  $Am^{241}$  and again presented a decay scheme.

\* \* \* \* \*

I learned the job of loading the americium slugs was completed, and they were given to the AEC courier on Wednesday for transportation to Arco.

In the afternoon I and others of our Berkeley group joined the Military Liaison Committee for a tour of the Livermore Laboratory.

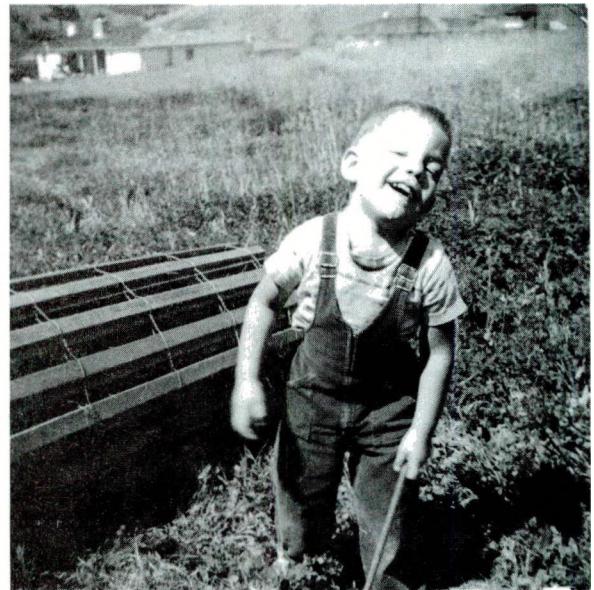
Friday, July 10, 1953

I took a day of vacation and worked around the yard for a while.

Later I went with Dan Wilkes and Al Ghiorso to the Contra Costa Golf Club for a round of golf (DW-82, AG-98, GTS-97). A fellow named Barney played along with us for the first 12 holes.



Dave and Stephen Seaborg  
1154 Glen Rd.,  
Summer 1953



Stephen Seaborg

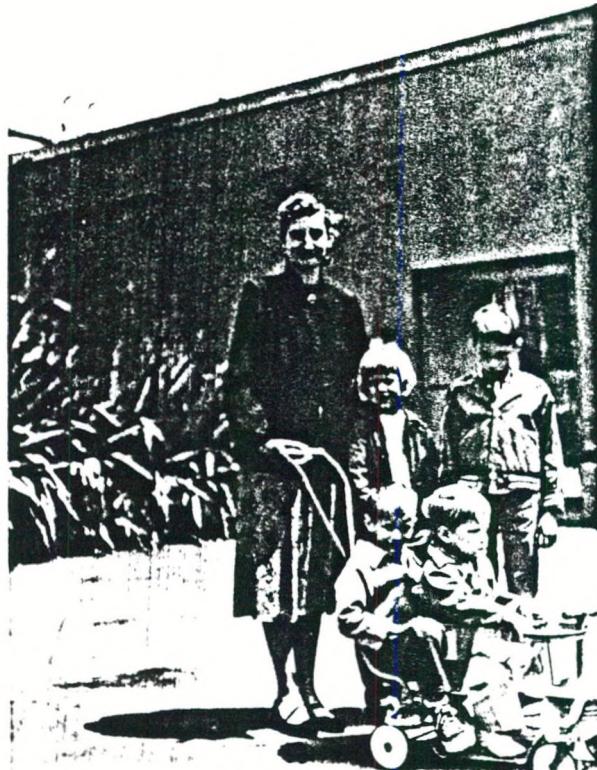


Glenn and Stephen Seaborg  
with Cricket, Summer 1953

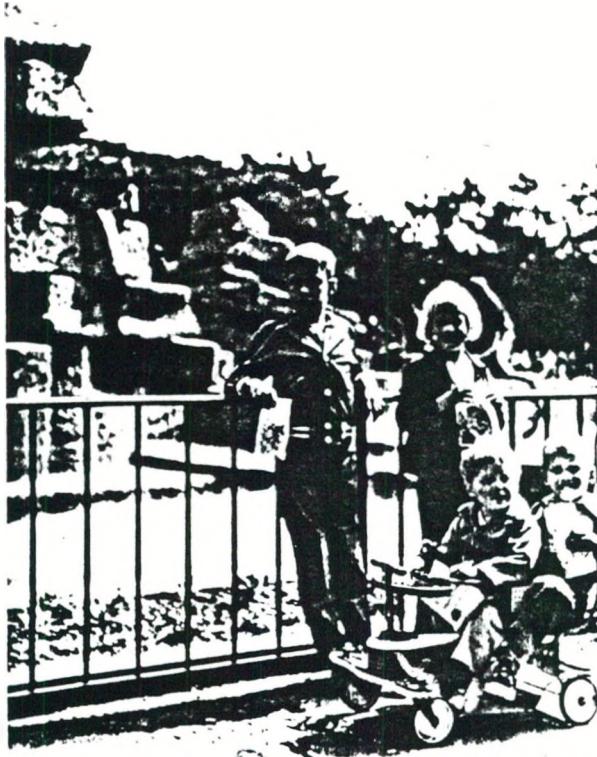
Saturday, July 11, 1953

Helen and I took the kids to San Francisco for a day at the Fleishhacker Zoo.

Helen with Lynne and Pete;  
David and Stephen in front



Pete and Lynne; Stephen and  
David in front, July 11,  
1953



Sunday, July 12, 1953

Jim and Rebecca Cason stopped out to visit us today, along with their boys, Roger and Mardy (Marsden).



Roger and Mardy Cason, Pete and Lynne Seaborg, July 12, 1953.

Monday, July 13, 1953

I made a few phone calls, including one to Greg Engelhard, and then spent some of the day in the labs with the students.

The mail brought me a nice note from Harold G. Cassidy (Yale University), thanking me for the copy of my Charter Day address. (Cassidy had lent me a copy of a talk he made earlier in the spring). In his note he wrote, "It is obvious that it [my talk] developed along quite different lines from those you may have had in mind when you asked for my manuscript. I hope, however, that having my few remarks on hand may have acted as a catalyst to stimulate your own thinking."

Shizuo Fujiwara wrote, in a letter that arrived today, that he will leave Japan by boat on July 28 and will land in Seattle on August 12. He said that he wants to visit us, including Dr. Bruce McGarvey, and the nuclear resonance groups at Stanford before going to the University of Illinois. Fujiwara wrote, "I am very happy that I can materialize my so long-cherished [sic] desire to see you and your laboratory very soon."

I also received a memo from George Everson, stating that he has had an acceptance letter from Norman Bonner [for Project Whitney], in which he states that he would like to start work at Livermore some time during the first half of October, and that if this date is unacceptable to me, he would like to know as soon as possible. Everson wrote that he replied that it was satisfactory. I routed the letter to Iz and Earl and asked that a check be made on his security clearance.

Our usual senior staff luncheon meeting was held in my office at noon.

At home today I received the annual note from the Regents about my salary for the coming year. My salary was fixed at \$9,200.00 (67% time) for the year ending June 30, 1954.

Tuesday, July 14, 1953

This morning I telephoned Greg Engelhard to discuss further the role of the AUSC in the television program, preparatory to the meeting of the Television Committee this afternoon.

Stan Thompson helped me draft a lengthy letter to R. L. Doan (Manager, Atomic Energy Division, Phillips Petroleum Company, Idaho Falls, Idaho) offering specific suggestions for a joint nuclear research program on heavy isotopes utilizing the MTR. This is the program that Stan Thompson and Al Ghiorso have discussed with Doan during their visits to Idaho on the basis of discussions with me. The letter read:

Stan Thompson has told me about his conversations with you concerning possible interest of your organization in a nuclear research program on heavy isotopes utilizing the MTR. I have discussed this with Stan and other members of our group here, and it strikes us that some such program would be very well worth undertaking. It seems to us that with the increased interest in high neutron fluxes and transmutations of macroscopic fractions of materials, the cross sections of short-lived substances become of great importance. It also seems reasonable that those working at the MTR site would best be able to carry out part of such a program. As preparation for such a program and also because of the value of the results in their own right, measurements of important capture and fission cross sections of isotopes where these are not very well known might be undertaken. Stan said that you would like us to make some specific suggestions which I will attempt to do in outline form in the following:

#### NEUTRON CROSS SECTIONS OF SHORT-LIVED ISOTOPES

(1) Fission cross section of  $U^{239}$  prepared by  $U^{238}(n,\gamma)$  using  $U^{238}$  very free from  $U^{235}$ .

(2) Fission cross section of  $U^{237}$  prepared from  $U^{236}(n,\gamma)$  using  $U^{236}$  very free from  $U^{235}$ . Perhaps the  $U^{235}$  here and in (1) could be burned out in high nvt irradiations.

(3) The fission cross section of  $\text{Pu}^{243}$  prepared from  $\text{Pu}^{242}(\text{n},\gamma)$ , the pure  $\text{Pu}^{242}$  being produced from  $\text{Am}^{241}$  in short high flux irradiations.

(4) The fission cross section of  $\text{Am}^{244}$  prepared from  $\text{Am}^{243}(\text{n},\gamma)$ , the  $\text{Am}^{243}$  being prepared from the neutron irradiation of pure  $\text{Pu}^{242}$ .

(5) The  $(\text{n},\gamma)$  cross section of  $\text{Am}^{244}$  via the chain  $\text{Am}^{243}(\text{n},\gamma)\text{Am}^{244}(\text{n},\gamma)\text{Am}^{245}$ . The unknown radioactive properties of  $\text{Am}^{245}$ , predicted to be a fairly short-lived beta emitter, would have to be established in this experiment.

#### NEUTRON CROSS SECTION OF $\text{Am}^{241}$

The neutron cross section of  $\text{Am}^{241}$  for the production of  $\text{Cm}^{242}$  (and other products such as  $\text{Am}^{242m}$ ,  $\text{Am}^{242}$ , and  $\text{Pu}^{242}$ ) is not well established with results varying by a factor of 3. The variation may be due in part to differences in energy of the bombarding neutrons. Thus, measurements might be made with thermal neutrons and in the hydraulic rabbit with and without cadmium absorbers and possibly also in the EBR. Some of these measurements would be a useful starting point as an entree into this heavy isotope research program because  $\text{Am}^{241}$  is available and the measurements are less difficult than some of the others.

#### NEUTRON ABSORPTION CROSS SECTION OF $\text{U}^{236}$ (AND $\text{U}^{235}$ )

Although the reactions  $\text{U}^{235}(\text{n},\gamma)\text{U}^{236}(\text{n},\gamma)\text{U}^{237} \rightarrow (\beta^-)$   $\text{Np}^{237}$  are important in the use of  $\text{U}^{235}$  as fuel, the cross sections at various neutron energies, especially the cross section of  $\text{U}^{236}(\text{n},\gamma)$ , are not well known. Sufficient amounts of relatively pure  $\text{U}^{236}$  are available so that its cross section can be measured by counting the beta particles of  $\text{U}^{237}$  or the alpha particles of  $\text{Np}^{237}$ .

These suggestions, illustrative of experiments very well worth doing, are given only in outline form; if they seem interesting they might be further discussed with Stan on one of his trips. If a program somewhat along these lines were undertaken, I am sure that additional suggestions would soon be forthcoming from the group undertaking the work. Some of the experiments might profitably be undertaken in collaboration with our UCRL people. All of the experiments depend, of course, on accurate neutron flux measurements, and I understand that a number of your people have made plans and good progress in this area through the use of monitors.

I feel that it is appropriate to add, bearing in mind the proper security precautions, that some of the above mentioned experiments

are of interest to the weapons program. Most of them, of course, are of importance to the high flux pile programs planned for the future where higher order neutron reactions are so significant. It seems to me that the AEC might very well be willing to support a modest program of this type in view of the overall value of such cross section measurements to their program and the general advantage of having Phillips, the organization operating the very high neutron flux facility, engaged in research involving the high neutron flux. Such a program should lead to the development of improved equipment and techniques which will be of aid to the participating laboratories in their experimental programs.

After mailing this classified letter, I telephoned Dan Miller (AEC, Washington) to tell him about the suggested program.

I also sent a note to Fujiwara to tell him that we are pleased that he will visit us although I am not yet sure whether I shall be in Berkeley during that time. I gave him my telephone number and that of Bruce McGarvey and asked that he inform us of the exact date of his visit as soon as possible.

At 2 p.m. I attended the fourth meeting of the Television Committee in Donald Coney's office. Present were Coney, Emerson, Gifford, Harper, Jones, Pickerell, and me. We went over the minutes of the meeting of July 1, and I reported that an examination of the ASUC minutes and an inquiry to Engelhard confirmed the conclusion that the ASUC would continue payment of \$12,000 a year for three years is correct. The main topic today was telecast control, and we agreed that, until the issues are more clearly defined, the nature of BAETA telecasts originating on the Berkeley campus should be checked against the principles established in University Regulation 17 and by the industry, e.g., as stated in the Television Code (National Association of Radio and Television Broadcasters, Washington, 1952). We shall study this Television Code statement before the next meeting, which will be on July 21 (I informed the group that I shall be out of town on that date). We adjourned at 4:10 p.m.

Wednesday, July 15, 1953

After a few phone calls, including one to Lee DuBridge at Cal Tech, I began clearing my desk top of reports, journals, administrative matters, and correspondence in order that I can enjoy next week's vacation at Pinecrest with my family.

Iz and I have decided that the work of both Frank Asaro and Maynard Michel, who have recently finished the work for their Ph.D. degrees, is valuable and outstanding, so that we offered, and they accepted, non-academic staff positions here at the lab. This morning I sent a memo to George Everson, in which I said that I believe I made a mistake in recommending a starting salary of \$525 per month and that, since they are very good men, I feel \$550 per month is called for in both cases.

In another memo to Everson I recommended that Charles W. Koch, who is scheduled to receive his Ph.D. at the end of the summer and will assume

an academic position on campus (yearly salary of \$5916) this fall, continue on our Radiation Laboratory payroll after September 15 at the usual one-third time basis so that he may continue to contribute to our research.

In the mail that arrived this morning was a note from Wayne Meinke, saying that he had just returned the signed copies of the renewal of his "no pay" consulting agreement with the University. Wayne went on to note that he appreciates the arrangement for it helps him keep up to date on the developments at Berkeley; he added that he felt this year's Gordon Conference in Nuclear Chemistry was a definite success.

In a letter dated July 11, C(alvin) A. VanderWerf (Secretary, Division of Chemical Education, ACS) wrote that my contribution to their symposium program at the Chicago meeting is scheduled to be given at 4:20 to 4:55 p.m. on Tuesday, September 8.

Another letter arrived from Norman Bonner (Cornell University), who said there is a highly tentative possibility that he may be able to arrange an exchange with Karl-Erik Zimen in Göteborg for a semester. Bonner asked, in view of his acceptance of the job at Livermore, how unhappy people out here would be if he didn't start work until the spring, instead of October. Bonner explained that he has always wanted to visit Scandinavia because his grandparents came from Sweden and Norway. I immediately replied that the delay would be ok with us; I asked that he let us know as soon as possible.

I made my usual rounds of the labs, particularly to talk with the students. Then at 3:45 p.m. Browne called from Los Alamos to inform us that the coral sample departed Eniwetok at 3 p.m. our time. There are two aircraft: one has 42 drums of 600 pounds a piece and the other has 15 drums, again with 600 pounds a piece. They estimate that the flight to Travis will take 20 hours. Gordon Knobeloch is the Los Alamos man aboard the flight. Charlie mentioned that he has heard that the people at Argonne have suggested the name phoenicium for element 99.

Thursday, July 16, 1953

This morning the research group meeting was attended by Asaro, Carniglia, Carr, Kenneth Richard Chapman (Master's Candidate from West Point who will do his research under my supervision),

Kenneth Chapman



Choppin, Clark, Conway, Cunningham, Dunlavey, Feay, Glass, Grover, Gunn, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Levy, Michel,

• Nervik, Passell, Perlman, Reynolds, Ruben, Seaborg, Shudde, Smith, Stephens, Surls, Templeton, and Wallmann.

Feay gave some details of his study, using curium from an MTR irradiation, on the absorption spectrum of  $\text{CmF}_3$ , in which densitometer tracings showed absorption maxima at 2826 Å(?), 2774 Å, 2680 Å, and 2369 Å; for  $\text{GdF}_3$ , the maxima were at 2834 Å, 2759 Å, 2666 Å, and 2334 Å. The peak at 2826 Å in  $\text{CmF}_3$  could be seen visually in the spectra on both plates but only on one densitometer tracing. Relative intensities in the two cases, each on a scale of ten, are (in same order as above):

$\text{CmF}_3$  = 2:10:6:4;  $\text{GdF}_3$  = 4:10:4:4. He noted that the absorption by  $\text{CmF}_3$  is much stronger than the absorption by  $\text{GdF}_3$ , and Cunningham added that this is the first time a curium absorption spectrum has been taken free from the peroxide absorption occurring in solutions. There was quite a bit of excitement and discussion about the work, with speculation about being able to assign the peaks to spectroscopic transitions.

Stephens described some recent French alpha-particle studies and gamma ray studies on  $\text{Th}^{230}$  that indicate gamma rays of 67 kev (100 relative abundance),  $150 \pm 3$  (12.5),  $200 \pm 15$  (~1),  $245 \pm 5$  (~3). Stephens said they have observed the gamma radiation in an aged sample of  $\text{Th}^{230}$  and also measured some  $\text{Ra}^{226}$  to distinguish gamma rays in the  $\text{Th}^{230}$  decay from those arising later in the chain and found  $\text{Th}^{230}$  gamma rays of 245, 185, 145, and 67 kev. He showed a decay scheme and described the 67 and 145 kev transitions as E2 and the 185 and 245 kev transitions as E1, noting that this will be the first case in this mass region in which the (1-) level lies above the (4+). I added that Feather's laboratory has made an energy level assignment in the beta decay of  $\text{Ac}^{228}$ , which is in conflict with some of our views. I said Feather has called the third level (1+) and has a complicated diagram. Feather, I noted, reports that the 2.18 Mev beta particle goes to the 67 kev level.

Asaro reported on systematics of the (1-) state and then went on to discuss conversion coefficients in transitions from the first excited states of even-even nuclei. He said that alpha-gamma studies have yielded some conversion coefficients, from which level assignments and transition orders can be deduced. Asaro said that, in the alpha decay of  $\text{U}^{230}$  to  $\text{Th}^{226}$ , a 70 kev gamma ray is found in 1.7-1.9% absolute abundance, which yields a conversion coefficient of 16-17. He added that, since all transitions to the ground state of an even-even nucleus from a level populated by alpha decay must be electric, this is an E2 transition and the first excited state is (2+).

Jaffe said they recently obtained some mass-analyzed americium from the CR-5 (Chalk River) irradiation and freed it completely of neptunium by column chemistry. The mass analysis gave:  $\text{Am}^{241}$ , 90.4%;  $\text{Am}^{242}$ , 1.13%;  $\text{Am}^{243}$ , 8.5%. They again found the 105 kev electromagnetic radiation and tentatively assign it to K x-radiation of plutonium from electron capture in  $\text{Am}^{242}$ . By comparing the relative intensities of gamma rays, he estimated the partial half-life of  $\text{Am}^{242}$  for decay by electron capture as  $850 \pm 100$  years.

\* \* \* \*

Up on the hill I worked for a while on my correspondence. I signed my usual type of letter to the draft board of Peter Gray, who has recently been classified as 1A. I wrote in part:

I cannot emphasize too strongly the importance of allowing persons such as Gray to remain in school until they complete their training. The atomic energy program in this country has throughout its entire history suffered from a shortage of personnel with sufficient technical training in this field. The University of California is almost unique in its graduate student training program under the auspices of the Atomic Energy Commission, but even here the number of persons trained has been pitifully small compared to the demand for their services in the laboratories of the Atomic Energy Commission. Yet these laboratories are of the utmost importance to the military strength of our country. Without a continuing supply of young and thoroughly trained scientists these laboratories simply cannot function successfully. In my opinion there is no comparison in the contribution that Mr. Gray can make to the strengthening of our nation's defense in serving as a completely trained scientist as compared to joining the armed forces...

In a letter dated June 19, Rachel VanderWerf of Lawrence, Kansas (her husband is Calvin A. VanderWerf, Secretary of the Division of Chemical Education, ACS) wrote that Parents' Magazine had asked her to submit an article suggesting ways in which parents can recognize and encourage their children's natural interests and gifts in the sciences. She asked for information from my childhood and from my experiences in raising my own family. I replied that my interest in science was aroused through high school courses. With our own children we have tried to patiently and seriously answer all their questions, and we have encouraged the use of the library. Our dinner table conversation, I wrote, frequently consists of simple arithmetic problems. In speaking of our four-year old, I wrote about his obsession with living things and how we have encouraged this by providing jars with ventilated tops for him and allowing him to keep his finds. We have built a bird feeder outside our breakfast room window and a small garden for each child to watch things grow. Finally, I said, "The most valuable trait a prospective scientist can own is an insatiable curiosity. Necessary traits which must develop as the child matures are a willingness to study and work hard and to see any project through to completion."

In reply to a question from F. A. Paneth (letter of July 10) about the placing of the mass number with respect to the atomic symbol in the designation of nuclides as the English Societies do (in the left hand top corner), I wrote that I actually do not have any very strong views; however, I have the general impression that American physicists in general are reluctant to change from the right to the left. I added that it might be a mistake to adopt legislation on this point at this year's meeting and suggested the decision be postponed until the question is considered further. Also, Paneth thanked me for my letter of June 30 concerning an Italian representative on the Advisory Councillors of our Joint Commission on Radioactivity of the International Union of Pure and Applied Chemistry (IUPAC); he said that at present it is not possible to add any name to the number of Advisory Councillors, but he has invited a number of experts to assist at the deliberations and is sure that all the

members will be pleased if Professor Rollier can take his place amongst them. Paneth said he has written to Professor Nasini in this sense.

I replied to Lane's letter of July 6, saying that I have the general impression that the material he has received probably covers most or all of the notebook data he will need; however, Wahl can speak better for his notebooks than I. I asked Lane if he can ascertain, without contacting him, whether and where Lavender can be reached in Washington, in the event it should be decided that it would be worthwhile to contact him.

A memo also went to George Everson, in which I said that we want him to offer Robert H. Goeckermann, presently employed with the California Research and Development Company, a position in the Whitney Chemistry Group. I wrote that Goeckermann's present salary is \$749 per month, and I would imagine that a starting salary on Whitney about equal to or slightly above this would be appropriate.

Jeanette has arrived to accompany the family on our vacation to Pinecrest and to help cope with the children.

Friday, July 17, 1953

I spent only a partial day at the lab since I wanted to help Helen get ready for our vacation, but I did call Joe Katz and James Beckerley about some declassification details about PPR, Volume 14A, particularly with Chapter 20, "Slow-Neutron and Spontaneous-Fission Properties of Heavy Nuclei" by Huizenga, Manning, and Seaborg.

Also, I had the regular lab procedures (urinalysis and blood count) at First Aid today.

At home I found a letter from my mother's doctor, Ervin L. Dreher, who reported to me about her health. He wrote in part:

She came to us with symptoms of severe cough, chills and fever (night sweats) of nearly a month's duration. On examination her temperature was 100.6, pulse 136, blood pressure 180/110 and moist rales in both lung bases. We ordered a chest x-ray, complete blood count and urinalysis at once and found bilateral pneumonitis (low grade pneumonia) with a small amount of fluid at both bases.

Her blood count was negative except for a high white cell count and the urinalysis was negative. She was placed on terramycin and a diuretic and in four days her temperature returned to normal and her symptoms improved.

However, it is felt more studies will be necessary to check her thyroid gland for overactivity (radioiodine test at Cedars of Lebanon by Dr. Jaffe). She states her pulse has been rapid for several years and now with her temperature normal, it runs over 100 beats per minute.

I have told her what we would like to do and she appears willing to return later to have these studies done. She says a year or two ago she had heart studies (electrocardiograph tracings), and nothing was

found and was then on digitalis to try to slow the rate down...

I am very pleased that Dreher, my former Berkeley student and brother of my UCLA classmate, Leonard Dreher, is taking such good care of my mother. He is very perspicacious to diagnose her need for a test of her thyroid function, a test that uses I<sup>131</sup>, which Jack Livingood and I discovered back in 1938.

After I left the lab, the afternoon mail brought the following copy of a letter from Joe Kennedy to Donald Lane. Doral phoned at home and read it to me.



July 13th, 1953

DEPARTMENT OF CHEMISTRY  
LOUDERMAN HALL

Mr. Donald E. Lane  
1331 G. Street, N. W.  
Washington 5, D. C.

Noted \_\_\_\_\_  
Action \_\_\_\_\_  
Recd. JUL 17 1953 GTS  
Ans. \_\_\_\_\_  
File \_\_\_\_\_

Dear Mr. Lane:

In June, Seaborg, Segre, and I were together in New Hampshire at a chemical conference. We used the opportunity to review the situation with regard to our claim, A E C Docket No. 7. Before and after that I went over these matters with Wahl, and this letter is a statement of the view which the four of us hold. (Since that time your letter of July 6 has been received; Wahl and I believe that the six notebooks you have are complete but we have no way to be sure).

1. It has been suggested that Captain Robert A. Lavender be asked his opinion of the status of the disclaimer to contract eng-30, in particular was it or was it not a "separable document". A Washington telephone directory lists Capt. Lavender at 815 Fifteenth Street N.W., STerling 3-7771, and presumably he is available there. Our opinion is that this is an important matter and a delicate one. We believe the approach to him should be attempted only under special conditions, with both you and one of us present. On the next occasion that one of us is in Washington, do you think we should try to see Lavender and perhaps try to get from him a signed statement on this point?

2. The one man who knows the most about the claims in our patent cases and about the notebook documentation is a Mr. Scott who wrote the final patent applications while in Lavender's office. We do not remember his first name but assume that you know him. We think he may be in the employ of the U.S. Atomic Energy Commission; he was



DEPARTMENT OF CHEMISTRY  
LOUDERMAN HALL

Mr. Donald E. Lane

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July 13th, 1953

at Oak Ridge a few years ago. It would accelerate things very much if he could interpret the claims and notebooks. Could he be loaned by the A.E.C. as an advisor to the Patent Compensation Board? This would be a very helpful idea if the person could be Scott, but might be a poor idea if anyone else were substituted for him. This is because he really understands both the cases and the notebooks.

3. We are not sure about the status of the patent applications now that eng-30 is presumed void. Does the government retain title to the applications, or has title reverted to the inventors? Would Anderson stipulate, or the Board rule, that the inventors now own these applications? (It is possible that we might want to take over their prosecution.)

4. In response to your question, we think that the Regents are willing to be as helpful as possible within the framework of their stated position. They will not make any stronger statement of disclaimer. However, they might be very helpful later if we decide to show just how arbitrary the April 1 date is, or to question whether the University of California contract NDC-rc-201 included the chemical work done by Wahl.

5. We are looking for an opportunity to make a point of the clouded patent situation in relation to the increasing interest in industrial atomic power. The newspapers indicate that arguments have arisen about the patents on inventions not yet made, although basic patents have been under confused negotiations for twelve years.

6. We believe the best course at present is to follow all suggestions of the Compensation Board and try to obtain their final action as soon as possible. But we wonder at what point and under



DEPARTMENT OF CHEMISTRY  
LOUDERMAN HALL

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Mr. Donald Lane

July 13th, 1953

what circumstances it would be advisable to seek other hearings. For example, Seaborg would like to discuss the whole matter, including Anderson's obstructions, with Strauss. Or at some point Neylan might be inclined to bring the subject before Congressman Nolan of the Joint Congressional Committee on Atomic Energy. / [sic] Knowland

Sincerely,

Joseph W. Kennedy

JWK/lb  
AIRMAIL  
c.c. A. C. Wahl, G. T. Seaborg, E. Segre

1853 • STRENGTH THROUGH TRUTH • 1953

Saturday, July 18, 1953

Helen, Jeanette, the kids, and I left this morning and drove to Pinecrest, where the summer camp, Lair of the Bear, of the University of California Alumni Association is located. We drove east from Sonora and arrived at Pinecrest in the late afternoon, after stopping for lunch en route. I kept an eye on the children while Helen and Jeanette unpacked and organized our tent cabin.



David, Lynne, and Peter (front); Stephen, Helen, and Jeanette in front of our tent cabin, Pinecrest, 1953

Sunday, July 19, 1953

At Pinecrest. Meals are served in a central dining hall here at Pinecrest although bag lunches are provided for those who want to eat elsewhere, such as at Strawberry Lake. There are play areas for youngsters while the adults can participate in volley ball games. After dinner there are campfire programs, often with a talk by a faculty lecturer.

Monday, July 20, 1953

At Pinecrest. We spent most of the day at Strawberry Lake, where we ate our bag lunches. Later I played volley ball--my height is an advantage in this game.



Stephen, Peter, David, and Lynne at Strawberry Lake, 1953

Tuesday, July 21, 1953

At Pinecrest. This evening I was the featured speaker at the evening campfire. This was a popular exposition on atomic energy, followed by a question and answer session.

[In Berkeley, the Television Committee met at 2 p.m. in the Librarian's office. Present were Coney, Emerson, Gifford, Harper, Jones, and Pickerell. One of the more important decisions today was the recommendation that the Chancellor appoint, with the advice of the Committee, a Television Coordinator who shall be paid from the Chancellor's budget and report to him through the Committee. His duties should include (a) act as technical assistant to the Committee; (b) maintain liaison with BAETA; (c) help the BAETA program director find local talent and material; (d) help members of the faculty develop TV programs or prepare for appearances on BAETA and other stations; (e) keep the Committee informed of TV interests of the other northern campuses and Agricultural Extension, and offer them such help as is wanted and can be given; (f) keep the person named by the Chancellor to represent him in matters coming under paragraphs 5 and 6 of the Regents' April 1953 TV resolution (monitoring function) informed of circumstances requiring his attention. The Committee recommended that the Chancellor appoint someone to act as his delegate to handle matters covered by paragraphs 5 and 6 (this person should be on the Berkeley campus and a member of the faculty rather than an administrative officer). It was suggested that a

Committee is too cumbersome for this purpose and that the TV Committee and the TV Coordinator should be kept free of direct responsibility for censorship. There was also a discussion about space assigned to BAETA, principles governing telecast control, selection of the TV Coordinator, etc. The meeting was adjourned at 4:25 p.m.]

Wednesday, July 22, 1953

At Pinecrest. This was a usual day with swimming in Strawberry Lake and volley ball after dinner.

Thursday, July 23, 1953

At Pinecrest.

[In Berkeley, the research group met as usual with the following in attendance: Asaro, Carr, Cobble, Cunningham, Choppin, Grover, Gunn, Hoff, Hollander, Hyde, Jaffe, Kalkstein, La Salle, Levy, Michel, Nervik, Passell, Perlman, Raby, Shudde, Slater, Smith, Stephens, Surls, Templeton, and Wallmann.

Levy gave a literature report on an article on nuclear shell structure by R. W. King and D. C. Peaslee, Phys. Rev. 90, 1001 (1953), and then Templeton gave a lecture on the potential energy of crystals.

Slater spoke about his efforts to measure the d,n and d,p excitation functions of  $\text{Th}^{232}$ . He went over the procedure and said that he finds the d,p peak is about three times the d,n peak. Perlman commented that Slater finds the cross sections converge at lower energy but one would think they would get farther apart as the energy fell. Slater added the results on the low side of the peak are unreliable.

\* \* \* \* \*

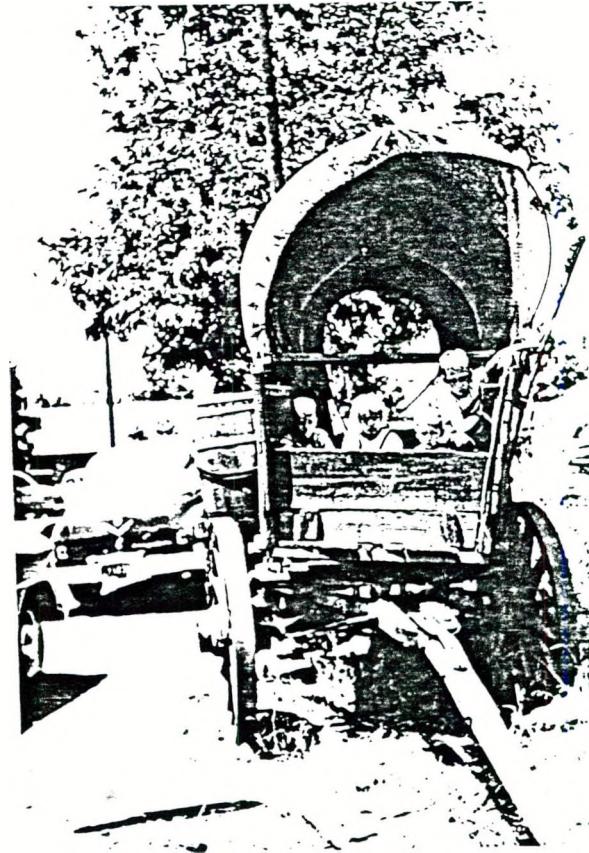
The rare-earth irradiation was returned from Arco today. The total radiation level at the surface of the container was about 500 mr/hr.]

Friday, July 24, 1953

At Pinecrest. Again we went swimming in Strawberry Lake.

Saturday, July 25, 1953

In the late morning, after Jeanette and Helen packed our car, we left for home, stopping on the way at Moaning Cave near Sonora. This was an exciting experience for the older kids.



David, Lynne, Stephen, and Peter Seaborg  
in covered wagon near Sonora, 1953

Sunday, July 26, 1953

I helped entertain the kids while Helen and Jeanette got things reorganized, the laundry done, etc. Unfortunately, Helen seems to be coming down with a cold.

Monday, July 27, 1953

I noted the following on my calendar pad this morning: "Greg (he called Tuesday) wants a conference tomorrow afternoon--mh"

After several phone calls and conversations, I looked over the stack of mail, which contained a letter from Ron Brightsen. Brightsen said that, while he was in New Hampton, he received a call from Don Wroughton of Westinghouse, who told him that the Committee decided to make no fellowship award this year because it felt that "none of the applicants had made sufficiently outstanding contribution to the welfare of the Corporation." [This is the Westinghouse fellowship that Brightsen hoped to get.] Ron went on to say "As far as I can see, there is no way at the present for me to come to Berkeley, even with a California fellowship worth about \$2500. If however, any situation arises which might make Berkeley financially feasible, I would be most grateful to hear of it

from you." [I am somewhat relieved to hear this news because it has become apparent that Brightsen would require too many exceptions to our general rules to make his operation here as a graduate student very feasible.]

Also in the mail were a couple of aerogrammes from John Rasmussen from Birmingham, England. John expressed his appreciation for the promotion to assistant professor, especially in view of his rather brief service as instructor, and said they will return to Stockholm and then be back in Berkeley sometime around the 15th of September. John said they have concluded that their 1-hour activity is due to fission product yttrium and not element 99 or 100. He also wrote about his teaching, the work in Stockholm, the Yugoslav (Draganich) who would like to come to Berkeley, and the Birmingham conference on "Low Energy Levels and Light Nuclei" that he is attending.

Dick Diamond wrote from Harvard to send me a copy of his paper "An Ion Exchange Study of Possible Hybridized 5f Bonding in the Actinides." This paper, with Ken Street and me as co-authors, is based on his declassified thesis. While Dick and I were at the Gordon Conference, we discussed some changes to be made before it is submitted for publication in Journal of the American Chemical Society. Dick said that the paper seems a bit long and asked about dropping some of the figures or cutting in other places. Finally, he added that their baby is growing bigger every day and again thanked us for the dress Helen sent them [Cathy Diamond was born [REDACTED]]. I put the paper aside to study later.

Another note arrived from Shizuo Fujiwara, saying his boat "Hikawa-maru" is scheduled to get to Seattle on August 12 and that he would be able to visit on August 17. If this isn't convenient, he asked that I let him know at an address in Seattle. I routed this to Iz and Earl.

D. F. Sundberg (Personnel, Los Alamos) requested an evaluation of David D. Cudaback. My reply was, "CUDABACK WAS WITH ME AT TOO EARLY A STAGE OF DEVELOPMENT TO ENABLE ME TO MAKE A FAIR EVALUATION OF HIS ABILITY. HAS CYCLOTRON EXPERIENCE, VERY NICE PERSONALITY, AND SEEMED TO SHOW INITIATIVE."

I received a copy of a memo from Pat Moyer in Personnel and addressed to me, Street, and Orleman, which listed status of applicants for Project Whitney. She asked for comments and for the list to be returned to Personnel.

Awaiting Arrival

Bissell, Eugene R. Received QPW clearance 7-2-53.

Holder, Bert E. Received "P" clearance 6-11-53. To arrive about August 1.

Schoenfelder, Carl. Received "P" clearance 5-11-53. To start August 1.

Pending Security Clearance

Gutmacher, Ralph. Entered for "P" first clearance on 7-2-53.

Tobias, Charles W. Entered for "P" first clearance on 10-28-52. Do you wish us to withdraw this clearance request?

Accepted Offer

Bonner, Norman. Offer accepted on 7-2-53. Now waiting for current PSQ's.

Holtzman, Richard. Offer accepted on 7-15-53. Was entered for security clearance on July 20. However, we will probably have to wait for a full "Q" because of his membership in an organization.

Kay, Eric. Offer accepted 5-4. Now awaiting US citizenship which is supposed to be granted about August 22. We will then ask security to grant a "P" on review if it is possible.

Offers Out

Warren, Buddy. Offer made on 7-7.

Active Applicants

Fry, Helen N. Interviewed in Berkeley by Reg Robinson. She is to write us a letter regarding her interests and college transcripts. She was in the office on 7-14.

Goodman, Leon. He is an organic chemist and has received extremely favorable recommendation from Prof. Winstein of UCLA. Do you wish to keep this file in the active file?

Johnson, Howard. He was to contact us about July 1 to see if the job was still open. We have no further word concerning the man--and have not seen him. Is he to remain active?

Korst, William. We wrote him on 6-16 after receiving his AEC fellowship application, asking him to fill out a PSQ if he were interested in employment. Would you like another letter written to him?

LeClair, Hugh G. Situation exactly same as Korst above. Would you like another letter written?

Colby, Nancy G. Interviewed 5-18 by Nels Garden who had no openings at that time. She is to check back about August 1. (She was married this summer.) She had a major in physical chemistry, graduated from UC in June 1953.

Smith, A. C. Referred to Earl Hyde who considered him a good prospect for employment at Livermore. Mr. Smith was then referred to Dr. Orleman by Mr. Hyde for further interviewing on February 25. Is this file to remain active?

Enders, John W. We wrote him 5-12 enclosing a PSQ and asking for reference. He is from Los Alamos. No further word.

Fick, Miss Jean. Her file was sent to Dr. Street to see if he was interested in further investigation.

Kruger, Paul. File sent to Ken Street for his recommendation.

Greene, David F. We sent him PSQ on 6-16 and also asked for references. He, too, is an AEC fellowship applicant.

Naptali, Leonard M. He has accepted a position with another concern but if he can be released, he is very interested in employment with the Lab.

Goishi, Wataru. Another AEC fellowship applicant, we sent him a PSQ which he returned to us. At present the file is on Mr. Everson's desk, awaiting action.

Stevens, Wayne. Receiving his degree in Chemical Engineering at University of Utah in August. We sent him a PSQ to fill out and return. We have not heard from him. Would you like to have another PSQ sent.

I read a July 15 letter from Spof English, who said that Malcolm C. Henderson has resigned as Deputy Director of the AEC Office of Intelligence and plans to spend about two months in Berkeley on vacation. Spof said that Henderson has been made a consultant to the Division of Research and that he will probably contact me to discuss work on heavy isotope analysis from weapons debris, etc. I routed this letter to Hyde, Iz, Stan, and Al.

There was a July 15 letter from Cyril M. Slansky (American Cyanamid Co., Idaho Falls). Slansky said he would vacation in the Bay Region the last two weeks of July and would like to visit on July 21 (although the date could be changed) to discuss hot cell design. Doral had written a note on the letter saying that Slansky had visited and had talked with Bill Ruehle about the subject.

Doral had also returned a card to James H. Stack indicating that I will be in Chicago from September 5 to September 10. This is in connection with television coverage of the ACS meeting.

Ruth F. Bryans (Publication Manager, American Institute of Physics) wrote on July 16 and asked if we wanted the large typed tables (for the "Table of Isotopes") returned. After checking with Jack Hollander, I wrote and said that we would appreciate having the sheets returned.

I also wrote to Harry Althouse (Sandoz Pharmaceuticals, San Francisco) to acknowledge receipt of the chapter "Energieumsetzungen Im Lebenden Organismus," which I said that I am forwarding to Professor Calvin, who is more of an expert in these matters than I am.

After a few conversations with some of the men and lunch, I went over

to San Francisco (421 Powell St.) to represent the University at a 2:15 p.m. meeting of representatives of the Bay Area colleges with representatives of BAETA to discuss academic courses via TV. Present were Sisters Maria (Dean) and Madeleine (Chairman of English Department), Holy Names College; Edward J. Griffin (Professor of Education and Coordinator of TV Training), USF; Lieberman (Coordinator for Communication Media), Contra Costa Junior College District; Katz (Film Workshop), California School of Fine Arts; Clymer (Vice-President), College of Marin; Arnstein (Supervisor of Teacher Training), California College of Arts and Crafts; Eddy (Dean of Students), Golden Gate College; Leo F. Cain (Dean, Education School), San Francisco State; Lewis (Coordinator of TV Services), San Jose State; Liff (Director of Radio-TV), City College, San Francisco; Doyle (Division of Public Relations), San Francisco State College; Knight (Visiting Instructor), San Jose State; Marsh (Director of KCSM-FM), San Mateo Junior College; Jonathan Rice, BAETA; James Day, BAETA.

Then, after a round of self-introductions (with about half the people present) initiated by Rice, Dean Cain, who presided, suggested the following agenda (which seemed to be, in part, a carry-over from a previous meeting): 1. Possibilities of each college for offering extension TV courses; 2. Accreditation of such courses; 3. Number and length of TV presentations per unit of credit; 4. Standards for written assignments, group discussions, examinations, etc.; 5. Fees to be charged; 6. Services to be provided by the college concerned; 7. Services to be provided by the TV station.

With regard to Item 1, Rice stated that they expect to start broadcasting between November 15 and December 1, 1953 (no sooner). They plan to include at the beginning, or soon after, two 2-unit courses and one 1-unit course. Cain asked each representative present (a little more than half of those ultimately present) to individually state when their college could begin to present a course. Essentially all of them stated that they couldn't be ready before next January or February (I said that the University of California, if it found it would give such courses, probably wouldn't be ready before the second semester). Most representatives seemed to think that courses for credit might start later than those without credit. Since fees can be charged for credit courses, this seemed to be a factor of some importance because of financial needs of the station.

It was pointed out, with regard to Items 2 and 3, that most colleges or universities usually provide fifteen 50-minute periods per unit of credit. A number of institutions that already have given instruction via TV (for example, Western Reserve) find fifteen 30-minute periods per unit to be adequate. The discussion included consideration of "on" or "off set" student groups to ask questions, i.e., a "control group." We discussed the possibility of interspersing discussion periods in the lecture series, say every third, fourth, or fifth period. It was suggested that it might be best to coordinate the time with the college semester times. There was a lengthy discussion of how credit could be applied toward degrees at various universities, for example, University of California, which can be summarized by saying that present procedures automatically take care of this (i.e., student must meet admission requirements and then have transcript of credits evaluated in the regular

way).

For Item 4 it was noted that the standards would obviously have to be substantially equivalent to those for regular courses. One proposal was made that students should come to a central place to take the examinations; an alternate suggestion was that the examination and written assignments be given in the regular manner of the particular institution involved, and this was more generally accepted.

It was agreed that a group presentation should be made to the coming November meeting of the Western College Association giving minimum criteria for credit courses; this will be worked out by a committee consisting of Edward J. Griffin (chairman), Liff, Doyle, and a University of California man. The University of California should notify Griffin as to the identity of its representative.

Rice asked that each institution notify him by letter of the courses (credit and non-credit) that they can give. He also said he will call another meeting of this group in August or September. It was suggested that there be some instructional meeting for college representatives so that they may learn about the techniques of TV instruction. Chairman Cain left the meeting at about 3:50 p.m., and I left about 4:00 p.m. while a rump session was still in progress.

Tuesday, July 28, 1953

This morning I dictated a report on the meeting I attended yesterday afternoon with representative of Bay Area colleges and representatives of BAETA, which I will give to Donald Coney, chairman of the Television Committee, for distribution to the members of the Committee.

Later I got together with Greg Engelhard to talk about some ASUC and athletic matters.

Wednesday, July 29, 1953

In rereading my chapter 3, "The Actinide Series," I noticed an error, so I wrote Professor Sneed a letter when I got to my office to ask if it could still be corrected, using a phrase of about equal length. I explained that it would be well worth making if it is not too late for us to do so.

About 9:30 this morning I telephoned Donald Coney about my TV meeting on Monday and made arrangements to see him tomorrow to give him my report.

Then, at 10 a.m. I met with the Radiation Laboratory Salary Committee.

A July 22 letter arrived for me from Sterling Cole (Chairman, Joint Committee on Atomic Energy), asking for written comments on some of the problems incident to definition of a Federal policy on industrial atomic power development. Cole emphasized that, since it may prove desirable to publish my comments, it will be necessary to exercise caution in the expression of my views in order to insure that "no restricted data" or "security information" is revealed in or compromised by my statement.

I also received a letter from Donald Lane, in which he included the following memorandum giving a preliminary grouping of the claims. He suggested that perhaps Wahl can advise him in some way the relative importance of the various groups, as well as some chronology or time schedule, and if the several groups were not developed simultaneously, indicate which were believed to be accomplished prior to April 1st, 1941. Lane said that, after he knows which groups are considered important, he will select a reasonable number of typical claims, and then perhaps Wahl can examine the notebooks to find the dates of the typical developments selected. He added that, if I have any better suggestions on how to proceed, to let him know. In answer to my query, he said that Capt. Lavender has given up his downtown office because of poor health and works a few hours a day at home, adding that Lavender's memory on details is not good. He also suggested the Scott I mentioned may be Stuart W. Scott, P. O. Box E, Oak Ridge.

MEMORANDUM

Case 52-A

cls. 1-6 Production of element by bombardment of material and aging of the bombarded material.  
cls. 7-23 Over-all combination processes for the production and recovery of element.

Case 52-B

cls. 1-28 Control of the oxidation state of element.  
cls. 29-34 Separation of material from element by solvent extraction.  
cls. 35-61 Separation by precipitation with or without carriers.  
cls. 62-84 Separation of fission products by carriers and by oxidation-reduction carrier cycles.  
cls. 85-128 Inorganic compounds, metal, and methods for producing metal.

Case 52-C

cls. 1-6 Separation of material from element by solvent extraction.

Case 52-D

cls. 1-10 Separation of element by electrolysis.  
cls. 11-33 Alternate carrier and cross-over procedures.

Case 208 - (div. of Case 52-B)

cls. 1-7 Inorganic compounds.  
cls. 8-9 Purification, concentration, isolating, by precipitation.

Later I visited some of the labs to talk about the research with the students.

Thursday, July 30, 1953

The meeting of our research group was attended by Behman, Carr, Clark, Grover, Harvey, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, La Salle, Lessler, Levy, Michel, Nervik, Passell, Perlman, Seaborg, Shudde, Smith, and Templeton.

Hulet reported that originally two alpha groups had been reported arising from the alpha decay of  $Cm^{245}$ , the electron capture daughter of  $Bk^{245}$ : 5.48 Mev (77%) and 5.34 Mev (23%). On the basis of the amount of alpha radioactivity in the two groups and the disintegration rate of  $Bk^{245}$ , the half-life of  $Cm^{245}$  was calculated to be about 7,000 years. They recently removed the radioactivity from the sample plate and performed another column separation. Alpha pulse analysis showed that only the 5.34 Mev group belongs to an isotope of curium while the 5.48 Mev alpha particle group is most likely due to  $Pu^{239}$ . From this the alpha half-life of  $Cm^{245}$  is calculated as about 28,000 years. He added that the amount of alpha radioactivity at 5.48 Mev due to  $Cm^{245}$  must be less than 50% of the  $Cm^{245}$  alpha activity in the 5.34 Mev group.

Jaffe reported on his measurements of the low intensity gamma rays from  $Am^{241}$ , using a scintillation counter: 60 kev (100): 102 kev (0.14): 128 kev (0.014): 168 kev (0.0016): 207 kev (0.0019). He noted that Passell observes L conversion lines of 102 kev radiation on the beta-ray spectrometer; the L conversion coefficient was calculated as 1.7. Using Gellman's theoretical values, he said this may indicate that the 102 kev gamma ray may arise from two levels (160 to 59 and 101 to 0) with one transition M1 or E2 and the other E1, while the 128 kev gamma ray may arise from transitions between the 160 and 33 levels. On the basis of previous experimental work, it appears that parity of the first four levels in  $Np^{237}$  is as follows: +101, +59, -33, -0.

Passell told the group that, in order to determine experimentally the absolute transmission of the double focusing beta spectrometer, they decided to make measurements on the electron lines due to the completely converted 44 kev gamma ray following the alpha decay of  $Cm^{242}$ ; from this they determined the transmission efficiency as 0.3%. They made a determination of the absolute number of conversion electrons due to the 59.6 kev gamma ray of  $Am^{241}$  from a run on  $Am^{241}$  made in the same manner as with  $Cm^{242}$ . This gave a conversion coefficient  $\alpha_{total}$  for the 59.6 kev gamma ray of  $0.92 \pm 0.1$ ;  $\alpha_L$  total =  $0.72 \pm 0.08$ ;  $\alpha_L$  III =  $0.145 \pm 0.015$ , agreeing exactly with values predicted by Gellman. He said they used the above  $\alpha_{total}$  and calculated a conversion coefficient of  $4.75 \pm 1.5$  for the 26 kev gamma ray of  $Am^{241}$ . Passell presented a level scheme indicated from these data. He also said that electrons of higher energy have been noted and gave their assignment and abundances.

\* \* \* \* \*

At 10:30 a.m. I met with Donald Coney in his office to give him my report for the Television Committee on the meeting in San Francisco Monday afternoon.

Back on the hill I made a telephone call to Herb York (Director of Project Whitney) to discuss our progress in staffing the chemistry group on the Project.

In today's mail was a letter from Stan McCaffrey, requesting that I speak at the annual Alumni Institute to be held at the Ambassador Hotel in Los Angeles on Sunday, September 28. In particular, he asked that I speak on new developments in intercollegiate athletics. I returned the card stating that I would speak.

I responded to the May 29 letter from Professor G. Schwarzenbach, saying that it seems to me that international support for the Gmelin Institute is worthwhile, at least until the 8th edition is completed, and that I believe the method he suggested for organizing such support is promising. I added that the most logical person in the United States to organize such participation is Dr. A. V. Grosse, who is serving as Chairman of the American Chemical Society's Committee on Foreign Compendia.

I also replied to a July 11 letter from F. Cap (Institut für Theoretische Physik, Innsbruck, Austria), who sent me a paper dealing with the placing of the heaviest elements in the periodic system. I explained my views about the matter and said that I feel the best solution is to place the entire series in the periodic system analogously to the rare earth elements.

Dr. D. A. Keys (Vice-President and Chairman of the Project Coordinating Committee of the Atomic Energy Council of Canada) visited today, and I toured Bldg. 5 with him, Nelson Garden, and Ashton O'Donnell. He was particularly interested in seeing the cave area with its various closed boxes and the "baby cave" installations with remote control equipment. Keys also talked with Burris Cunningham and Jim Wallmann about density determinations of some the rare elements.

Friday, July 31, 1953

I took a day off from the lab, as vacation time, in order to work in the yard, play with the kids, etc..

Saturday, August 1, 1953

Off and on recently I have been working on a talk to be given before the Division of Chemical Education on September 8 (Chicago ACS meeting). Although much of my day was spent working outdoors, I did prepare an abstract for the talk, which is titled "Oxidation States of the Actinide Elements."

Sunday, August 2, 1953

During part of the day I worked in my study on the page proof of PPR, Volume 14A, which just arrived at the lab this past week.

Monday, August 3, 1953

This morning I made the usual number of phone calls: one, for example, went to George Briggs about some athletic matters. I also telephoned Van Atta (Chester M.) and made arrangements to see him tomorrow.

After talking with some of the students about their research, I met with the senior staff in my office at noon for our regular brown-bag luncheon meeting.

I returned the page proof of Chapter 1 ("Introduction" by G. T. Seaborg) of PPR, Volume 14A to Joe Katz, noting that I have made a few corrections in red. I have page proof for a number of other chapters to read in my "spare" time.

In the mail today was a note from M. C. Sneed, saying that he has sent my correction to the editor although it may be impossible to have the change made for the volume is supposed to appear about the 1st of September.

From Donald Coney came a copy of the signed Agreement, dated June 9, between the Regents and BAETA about the use of the recording studio in Dwinelle Hall.

I also received notification from Personnel that Richard Hoff, who has completed his thesis work, transferred to the Project Whitney payroll as of August 1. [Hoff won't receive his degree until January.]

Tuesday, August 4, 1953

After a few phone calls, I wrote a note to Bill (W. H.) Johnston at Purdue University, suggesting the date of October 16 for my long-promised visit. I asked him to let me know if the date is satisfactory.

Page proof of Chapter 2 of PPR, Volume 14A ("The Chemistry of Actinium" by F. T. Hagemann) was returned to Joe Katz. I mentioned that Figure 2.1 is not too clear and perhaps should be redrawn. I said that I have attempted to correct the outdated energy values and mentioned the general problem of referring to electron capture in the tables, asking whether this should be uniformly done throughout the chapters.

At 10:30 a.m. Van Atta and I got together and talked about the preparation of the proposal to be submitted to the AEC for funding of the heavy ion accelerator. Actually, John Rasmussen prepared a draft last year of some of the material I need to include.

I also received a notice from Foster York about Patent Case S-5625 "Preparation of Neptunium Hexafluoride" that was executed on September 29, 1947. York said that a Notice of Allowability was received from the U. S. Patent Office about this patent application, which normally means that whenever the application is declassified, the application will issue as a patent.

After talking with some of the researchers, I continued reading page proof of the PPR, Volume 14A chapters. Although we are reading page proof for much of the volume, Chapter 20 ("Slow-Neutron and Spontaneous-Fission Properties of Heavy Nuclei" by J. R. Huizenga, W. M. Manning, and G. T. Seaborg) has yet to be declassified since its revision, in which we included the spontaneous fission rates of  $Pu^{240}$  and  $U^{236}$ . These we felt justified in adding to the chapter since these values were recommended for declassification by the Sixth International Declassification Conference held in Canada, April 8-10, 1953. Unfortunately, declassification has not yet been approved, and Volume 14A may be delayed in publication or the values must be eliminated from the chapter. This, I believe, would be foolish, and I have told Katz that I shall try to contact some people in Washington and produce some action.

Wednesday, August 5, 1953

The page proof of Chapter 3 of PPR, Volume 14A ("Nuclear Properties of Uranium, Protactinium, and Thorium Isotopes" by L. I. Katzin) contained a lot of corrections, and today I wrote Katz that I am sure the editors will be unhappy but the changes are necessary to make the chapter consistent with other chapters on nuclear properties and to bring it up to date. I wrote that this is the only chapter where so many changes are necessary and said, "You might tell the editors that I haven't proceeded like a drunken sailor and put in changes willy-nilly but have exercised great care in seeing to it that new material replaces old material in essentially the same number of spaces..." I pointed out some of my specific suggestions and then returned it, along with Chapter 6 ("The Chemistry of Uranium" by H. R. Hoekstra and J. J. Katz).

Much of the rest of the day was spent reading proof on some of the other Plutonium Project Record chapters although I did have a 1:30 p.m. appointment for a chest x-ray with Dr. E. Schulze Heald (2560 Bancroft).

Late in the afternoon I was able to send off the proof to Chapter 4 ("The Chemistry of Thorium" by L. I. Katzin) and Chapter 5 ("The Chemistry of Protactinium" by R. E. Elson) to Katz. I wrote, "To your probable surprise and certain relief, I have made no corrections and presume that you will answer the queries and make such other corrections as you deem appropriate." I also enclosed a copy of a patent disclosing the Ames method for making uranium, explaining that it may have escaped his attention and may be worth mentioning in our proposed book on The Chemistry of the Actinide Elements to be published by The Methuen Co.

Thursday, August 6, 1953

Present at this "first of the month" research group meeting were Asaro, Carr, Choppin, Clark, Cobble, Dunlavey, Glass, Gray, Grover, Gunn, Harvey, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, La Salle, Levy, Mathur, Michel, Nervik, Passell, Perlman, Raby, Seaborg, Slater, Stephens, Surls, and Wallmann.

Mathur reviewed the past work on  $Cs^{127}$  and then told about his experiments with several samples, plus some mass-separated samples. They redetermined the half-life and found it to be 6 hours 15 minutes. He said there are three gamma rays at 62, 125, 420 kev, and an x-ray of 30 kev with relative intensity ratio of 125 kev (1) (K/L conversion ratio of 7.8), 420 kev (6) (K/L conversion ratio of 6.3), 30 kev x-ray (20). The positron spectrum is definitely of two components, one being about 0.75 Mev and the other 1.2 Mev (this latter is uncertain by about 150 kev). Mathur said the decay of  $Cs^{127}$  has been shown to branch to the 75-second  $Xe^{127m}$  isomer (about one in  $10^4$  takes this path). He then described the counting chamber. Gamma rays, he said, were observed from the xenon decay of energy 175 kev and 125 kev, and an x-ray of 30 kev, all of which decay with a 75-second half-life. Mathur presented a list of energies and assignments of the conversion electron lines of  $Cs^{127}$ .

Gunn discussed his experiments to determine the heat of reduction of Am(VI) to Am(III); the average value is -99.4 kcal, and the calculated value of the Am(III)-Am(VI) potential is -1.70, v. He presented a corrected value for the heat of reduction of Am(v) as  $\Delta H = 67.7 \pm 1.8$  kcal and  $E = -1.70 \pm 0.04$  v.

Cobble discussed the entropy values of neptunium ions and then went on to review an article by Wilson of Rice Institute [Phys. Rev. 77, 516 (1950)] describing a rotational model of the nucleus.

\* \* \* \* \*

Up on the hill I telephoned Wally Reynolds to ask him to find out when the proposal for the linear accelerator would have to be submitted to be included in the fiscal year 1955 budget.

I then replied to James H. Stack's request of July 28 for an abstract of my upcoming talk on "Oxidation States of the Actinide Elements" to the Division of Chemical Education in September. I sent him the abstract and made a few comments about the material I hope to present.

Mignon Gill (Chairman, Speakers Bureau, Chicago Section, ACS) wrote on August 3 to ask if I am willing to be a speaker at local meetings of non-technical groups such as Rotary, Kiwanis, etc. during the time of the September ACS convention. In my reply today I said that I feel that my schedule will be such that it won't be possible. I added that, although I have given quite a number of talks of the type she suggests, I have never succeeded in finding a method of presentation which has interested such groups.

Friday, August 7, 1953

This morning Nels Garden gave me a serious memorandum, "Indifference of William Crane to Radioactive Hazards":

It has long been obvious that to perform chemical, physical, and mechanical operations on material containing radioisotopes requires time for detailed prior planning and the application of special techniques and skills that would not otherwise be required if radioisotopes were not involved. These special techniques and skills are as necessary and peculiar to operations involving radioisotopes as aseptic techniques are to the medical profession.

Special techniques and skills are required in order to insure primarily that no health hazard results from the work and secondarily, that the lifetime of laboratories and facilities are not shortened nor the value of radioisotopes as a research tool lessened by the indiscriminate spread of radioactive contamination.

Much has been developed in the way of equipment to help control the spread of radioactive contamination, but the lack of good prior planning and the omission of good techniques will lower its value considerably.

The initial lack of such techniques and skills and the gradual acquisition of them is most apparent in the graduate students. However, once in a while, individuals enter this field, who do not appear capable of acquiring these special techniques or skills or who never seem to understand or appreciate their need for acquiring them. This point is recognized in Handbook #42, published by the National Bureau of Standards, entitled "Safe Handling of Radioactive Isotopes" wherein it is stated:

"In a radioisotope laboratory, skill in radiation protection is as necessary as skill in chemical or biological manipulation. Persons failing to develop such skills should be advised to transfer to other occupations."

With full recognition of his ability, industry, and enthusiasm, the opinion still persists with those endeavoring to protect him over the last several years that William Crane appears to be an individual who falls in this category. Continued observation of his work has shown a general disregard for careful operations, and there has been no indication of improvement over the last several years. The last demonstration of his abilities was during the recent americium purification operations. While, in general, his methods and techniques have involved primarily a health hazard only to himself, his methods during the americium purification run endangered others. It is strongly felt that his continuing radioactive work at UCRL may lead to more serious hazards.

It is with regret that this memorandum is written, but it is strongly felt that we would be in dereliction of our duty to protect property and health if we did not bring this to your attention.

I routed Garden's letter to Iz, Earl, Stan Thompson, and Ken Street.

I continued working on my proofing job, but I also went over a paper prepared by Dean Dunlavey entitled "Alpha Activity of  $\text{Sm}^{146}$  as Detected with Nuclear Emulsions," which we plan to submit to The Physical Review next week.

After lunch I met with Alton L. Wilson to discuss the plans for Bldg. 101 in Livermore.

Saturday, August 8, 1953

This was a rather quiet day at home with Helen and the kids although part of the day was taken up with some writing projects.

Sunday, August 9, 1953

I again spent the day at home working on my various projects.

Monday, August 10, 1953

I made a few phone calls, including one to Charlie (C. L.) Marshall (Deputy Director of Classification, AEC, Washington) about declassification of the values for the spontaneous fission rates of  $\text{Pu}^{240}$  and  $\text{U}^{236}$  in Chapter 20 of PPR, Volume 14A. I feel quite strongly about this matter; however, Marshall suggested that this is something I should take up directly with Lewis Strauss, Chairman of the Atomic Energy Commission.

I continued proofing chapters for Volume 14A; in addition, I looked over the typed manuscript, "Alpha Activity of  $\text{Sm}^{146}$  as Detected with Nuclear Emulsions" (Dunlavey and Seaborg).

The senior staff met in my office for our usual Monday brown-bag luncheon meeting. There was some discussion about the Garden memo about Crane.

Afterwards I made the rounds of the labs to check on the students' research.

Tuesday, August 11, 1953

Again I made a number of phone calls; unfortunately, I was unable to reach Lewis Strauss this morning, but I shall try tomorrow. I did, however, talk with Alberto F. Thompson, to whom I described my attempts to solve the declassification problem.

Later I had "Alpha Activity of  $\text{Sm}^{146}$  as Detected with Nuclear Emulsions" by Dean C. Dunlavey and Glenn T. Seaborg mailed to S. A. Goudsmit (Editor, The Physical Review) for publication as a "Letter to the Editor."

At 10 a.m. Alton Wilson and I met with W. B. Reynolds in his office to discuss the chemistry building at Livermore.

A new stenographer, Shirley J. Daney, began working in our group today.

Shirley Daney



Much of the rest of the day was spent reading proof, journals, and reports.

Wednesday, August 12, 1953

One of the first things I did today was to telephone Lewis Strauss (now Chairman of the Atomic Energy Commission) to discuss the matter of the declassification of values of the spontaneous fission rates of  $Pu^{240}$  and  $U^{236}$ . I discussed the background of the case, the status of the book (PPR, Volume 14A), and said that their declassification was recommended by the U. S. delegation to and agreed to by the Sixth International Declassification Conference held in Canada, April 8-10, 1953. Strauss seemed rather agreeable and asked me to write a letter detailing the situation. I then wrote him the following:

The book to which I referred in my phone conversation this morning is Volume 14A of the Plutonium Project Record of the National Nuclear Energy Series entitled "The Actinide Elements," which Dr. J. J. Katz (Argonne National Laboratory) and I have had under editorship since the end of the war. This book, now in the page proof stage, gives comprehensive coverage of all the declassified and published information on the chemical and nuclear properties of all the transradium elements. Actually AEC declassification policy has progressed to such a stage, I am pleased to say, that essentially no basic scientific information is missing (which is one of the reasons that publication has been delayed until now) so I feel that one of the useful functions of the book will be to dispel the erroneous impression among many non-AEC scientists that much fundamental science is unnecessarily being kept secret.

Although everything else in the book has been already declassified, this is not true for the included actual values for the spontaneous rates of  $Pu^{240}$  and  $U^{236}$ , for which only the existence of spontaneous fission has been hitherto declassified and published. These values have been included tentatively up to the page proof stage, pending their possible declassification, for the sake of completeness because their declassification was recommended by the U. S. delegation to and agreed upon by the Sixth International Declassification Conference (held in Canada, April 8-10, 1953).

More fundamentally, of course, these values should be declassified if there is no longer any need to keep them secret and if the advantages

to our country of making the information available outweigh the disadvantages. I believe that this is now the case.

I believe that we can regard it as certain that the Russians know the spontaneous fission rate of  $\text{Pu}^{240}$ . We know that they have succeeded in producing plutonium and that they have exploded plutonium in atomic bombs. They are very familiar with spontaneous fission measurement techniques because they discovered this phenomenon (in natural uranium). The measurement of this property is quite simple when reactor-produced  $\text{Pu}^{239}$  (which always contains  $\text{Pu}^{240}$ ) is available and was accomplished by our Los Alamos scientists in 1944 immediately upon the availability of the first few milligrams. In the case of  $\text{U}^{236}$ , it is highly likely that they know the actual value, and I would think it certain that they at least have established an upper limit to the rate which shows that it is not a serious effect here.

All other measured spontaneous fission rates have been declassified and any group treatment of this phenomenon makes  $\text{Pu}^{240}$  and  $\text{U}^{236}$  conspicuous by their absence. The declassified values have been systemized in several different ways, all of which lead to fairly accurate predictions of the rates for such even proton-even neutron species, and this is especially true for  $\text{Pu}^{240}$ .

It seems to me that declassification of this constant for  $\text{Pu}^{240}$  would be helpful in the clarification of the difference between plutonium for weapon and for reactor use, which should be of value to the general impending program of greater industrial participation in the development of nuclear power.

These constants are, of course, of basic scientific interest and their release will contribute to the general advance of fundamental knowledge. I feel that in the tradition of free science and the general advancement of scientific theory that such basic information as this should ultimately be released and the question is essentially one as to the proper time.

I hope that I have succeeded in this hasty attempt in summarizing the sort of information on this question that you may find useful.

I sent a carbon of this letter to Joe Katz and then wrote a similar letter to Charlie Marshall (Deputy Director of Classification, Washington), saying that I have spoken and written to Lewis Strauss about the matter, and emphasizing that we can regard it as certain that the Russians know the spontaneous fission rate of  $\text{Pu}^{240}$ .

The page proof of Chapter 7 ("Nuclear Properties of the Plutonium Isotopes" by G. T. Seaborg) of PPR, Volume 14A, was mailed to Joe Katz with a few changes marked in red.

I then wrote to Donald Lane in answer to his letter of July 27 and to Joe Kennedy's letter of August 7, 1953. I said:

I agree in general with the comments of Kennedy and Wahl. I think that in the claims that refer to specific oxidizing agents, which of

course are not as important as the general claims, we are probably limited to the peroxydisulfate-silver ion combination as the only one that was used prior to April 1, 1941. Since no work with macroscopic amounts of plutonium was done prior to this date none of the claims involving inorganic compounds or the metal or the precipitation of nearly pure plutonium compounds apply to work accomplished prior April 1.

With respect to Captain Lavender, I feel that it would probably be better not to "sound him out" at the present time. We thought that with the knowledge that you know how to reach him one of us together with you might call on him during a visit to Washington.

For several days I have been trying to reach Joe Hamilton to discuss the proposal for the heavy ion linear accelerator and our bombardment schedule at the 60-inch cyclotron; today, I finally telephoned him at his home in San Francisco.

An August 10 response arrived from Winston Manning to my letter of July 3. Manning explained that their discussion about it had been interrupted by vacations. He suggested talking about some of the points the next time we get together, perhaps in early September if I am planning to come to the Chicago ACS meeting. I routed the letter to Stan, Bernie, and Al.

Since I am leaving town tomorrow, I made the rounds of the labs to check on the students' research and plans for the next week or so.

Then, at 4 p.m., I had another meeting with Wally Reynolds about our proposal for the building and financing of the heavy ion linear accelerator.

Thursday, August 13, 1953

Helen, the kids, and I left for Los Angeles by automobile in order to spend a week or so vacationing with my parents in South Gate.

[In Berkeley the research group had its usual meeting, attended by Cobble, Choppin, Grover, Hoff, Hollander, Huffman, Hyde, Jaffe, La Salle, Levy, Nervik, Passell, Perlman, Shudde, Smith, Stephens, Templeton, and Wallmann.

Smith talked in some detail about his work over the past year on the neutron-deficient isotopes of platinum and iridium produced in 32 Mev proton bombardments of iridium, a problem suggested by John Rasmussen. In the platinum fractions he found a 20-minute activity (possibly due to  $N^{13}$  and  $C^{11}$  produced by 0.3% carbon in the iridium target); an 11-hour activity (which on the basis of milking experiments and excitation functions could be assigned to  $Pt^{189}$ ); a 10-day iridium activity ( $Ir^{189}$ , daughter of the 11-hour activity); and a 3-day  $Pt^{191}$ . Smith described the experiments and related his work to that of T. C. Chu, who bombarded rhenium with 20-40 Mev helium ions to produce iridium isotopes. There was considerable discussion about the work.

Stephens talked about the alpha spectrograph that has been set up in connection with the 50-channel gamma analyzer to do alpha-gamma coincidence work. They tested the setup with a test run with  $\text{Am}^{241}$ , and Stephens went over the work in some detail.

Nervik described some 340 Mev proton bombardments of tantalum, in which he unexpectedly found a 2 hour to 2 hour 16 minute rhodium activity. Richard Borg, who has been working on uranium targets bombarded with 7 Mev neutrons on the 60-inch cyclotron, did not see this in the rhodium fraction he separated from the target, but they then bombarded  $\text{U}^{238}$  with 340 Mev protons, followed a rather elaborate slow chemical separation and found a 2 hour 2 minute rhodium activity, representing about 10% of the total rhodium activity. This has a beta particle of about 800 kev, along with gamma rays of 500 kev, 680 kev, and 1.1-1.2 Mev. Nervik suggested that it possibly should be assigned as an isomer of  $\text{Rh}^{107}$ .

\* \* \* \* \*

Friday, August 14, 1953

In South Gate with the family. My parents were very pleased to see their grandchildren, and today we celebrated Stephen's second birthday with a cake with two candles.

[In Berkeley the following letter arrived from Winston Manning, which Iz read and routed to Stan and Al:

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Argonne National Laboratory  
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TELEGRAM WUX 18 LEMONT, ILL.

Classification changed to **DECLASSIFIED**  
by authority of D. J. O'Flaherty August 11, 1953  
on 5/5/61 R. Howard  
Person making change  
Date.

Dr. G. T. Seaborg  
Radiation Laboratory  
University of California  
Berkeley 4, California

Dear Glenn:

Last winter you raised with me the question of how the publication, authorship, and credit for Berkeley and Argonne work on the heavy element fraction of Mike should be handled. As you may remember, I expressed the view that it was something which should be worked out by the scientists directly involved at Argonne and Berkeley. More recently I understand that you have indicated in conversations with Sherman Fried and Paul Fields that the position of the Berkeley workers is a willingness to treat the element 99 work as a joint effort of Berkeley and Argonne, but a conviction that the entire credit for element 100 should accrue to Berkeley.

My purpose in writing you now is to set forth the consensus of feeling of the Argonne workers on this question, one with which I as a non-participant am in complete agreement. The people here strongly favor handling the work on elements 98, 99, and 100 and perhaps some other aspects of the work as a joint accomplishment of the three laboratories principally involved -- Los Alamos, Berkeley, and Argonne -- without attempting to resolve further, for publication purposes, questions of more detailed credit for various aspects of the work. If Berkeley and Los Alamos were to agree to this proposal, the exact area to be included in the joint announcement could be decided by conference between those involved. It seems to us that there are many advantages in this approach to the question of publication and credit. Among these are:

1. The question of credit would be made definitely secondary to the technical accomplishments, something I feel is always desirable and particularly so where workers at more than one laboratory are involved.
2. A series of decisions on individual points as to who first established or disclosed various facts would be avoided.
3. This method of treatment recognizes that time differences are slight, that in some aspects the work of the various laboratories was independent, in others mutually inter-dependent.

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4. This approach includes with respect to Los Alamos a recognition of that laboratory's overwhelmingly important role in producing and collecting the source material, as well as the Los Alamos contribution to analysis of the debris.

If, instead of taking this approach, the alternative is used of considering individual points, some of them relatively fine points, in deciding priority questions for separate phases of the work, the matter of assigning proper credit to Los Alamos becomes more difficult, at least in some phases of the work. But if this individualistic approach were to be used, and excluding the question of credit to Los Alamos, the Argonne participants and I feel that the record definitely indicates that credit for the discovery of element 99 and for the spontaneously fissionable isotope of element 98 would go to Argonne, with the picture for element 100 at this time still not clear.

If you and the others involved at Berkeley still favor your earlier proposal and do not now consider acceptable our preference for a three-laboratory joint announcement, the people at Argonne feel that the matter should be submitted to some disinterested person or preferably a committee.

Sincerely yours,

*Winston*

W. M. Manning, Director  
Chemistry Division

WMM:ss

cc: W. G. Johnson  
W. H. Zinn

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Saturday, August 15, 1953

In South Gate with the family.

Sunday, August 16, 1953

In South Gate with the family.

Monday, August 17, 1953

In South Gate with the family.

Tuesday, August 18, 1953

In South Gate with the family.

Wednesday, August 19, 1953

In South Gate with the family.

Thursday, August 20, 1953

In South Gate. Our family drove to Santa Ana, where I played golf at the Willowick Golf Course (West Fifth Street at Santa Ana River) with some fellows named Gary, Skip, and Steve. I scored 52 on nine holes and then played an additional three holes. Helen and the kids visited some of her relatives and friends, including her aunt, Jesse Klever, in a rest home and a friend, Georgia Jennings.

[In Berkeley the following attended the meeting of the research group: Asaro, Behman, Carniglia, Clark, Dunlavey, Feay, Grover, Hoff, Hollander, Jaffe, La Salle, Levy, Michel, Nervik, Passell, Perlman, Ruben, Shudde, Smith, Stephens, Templeton, and Wallmann.

Clark described his method for preparing  $\text{BiBr}_3$  and his measurements of the lattice constants as  $a = 6.57 \text{ \AA}$ ,  $b = 9.82 \text{ \AA}$ ,  $c = 8.41 \text{ \AA}$ ,  $\beta = 106^\circ$ , and  $Z = 4$ . He has been unsuccessful so far in finding a structure to fit the data. In a similar manner he prepared  $\text{AuCl}_3$  and found lattice constants of  $a = 6.57 \text{ \AA}$ ,  $b = 11.00 \text{ \AA}$ ,  $c = 6.44 \text{ \AA}$ ,  $\beta = 113.3^\circ$ , and  $Z = 4$ .

Carniglia talked about his vapor pressure measurements on solid  $\text{AmF}_3$ , from which he obtained  $\Delta H_{\text{vap}} (1273^\circ\text{K}) = 97 \pm 1 \text{ kcal}$  and said a calculation of  $\Delta C_p$  of vaporization gave the value  $-20 \pm 15 \text{ cal/deg-mole}$ .

Levy reviewed the previous data on the long-lived state of  $\text{Bi}^{210}$  and then reported on his search for this in uranium ores. Levy said that the bismuth fraction was removed from a sample of uranium ore and allowed to stand until all of the RaE had decayed. The  $\text{Po}^{210}$  was removed, and the bismuth fraction was examined for the presence of the  $10^6$  year  $\text{Bi}^{210}$ . None was found. An upper limit of one part in  $10^6$  was set for the fraction of RaD that decays to the long-lived  $\text{Bi}^{210}$ . Since the half-life for the decay of RaD to RaE is 22 years, the partial half-life for the decay of RaD to the long-lived  $\text{Bi}^{210}$  must be greater than  $22 \times 10^6$  years. Levy presented a decay scheme for RaD, RaE, and  $\text{Bi}^{210}$  that he has worked out.]

Friday, August 21, 1953

In South Gate with the family.

[In Berkeley an August 19 response arrived from R. L. Doan (Phillips Petroleum Company, Idaho Falls) to my July 14 letter to him suggesting a program of nuclear research on heavy isotopes using the MTR. Doan said, in part, that most of the investigations seem feasible to undertake and, if the AEC is agreeable to their pursuing such work, they would not be adverse to undertaking at least part of the program I outlined. He pointed out, however, that the number of technical personnel at the MTR is quite limited and all of their men are presently engaged on other investigations, so that progress probably would not be too great unless they can acquire another man or two. Doan said that they will be glad to discuss some of the problems with Stan Thompson at the next opportunity.]

Saturday, August 22, 1953

The Seaborg clan drove back to Lafayette.

Sunday, August 23, 1953

Iz had brought home some of my mail from the office, and I spent some time looking it over. I noted a letter from Bill Johnston (Purdue University), who said that the date of October 16th is extremely satisfactory to them for my visit. Bill suggested I come in time to give two talks, a general one to their chapter of Sigma Xi and a more technical one sponsored by the Department of Chemistry. He also mentioned that many people in Karl Lark-Horovitz' physics department are looking forward to seeing me.

Monday, August 24, 1953

I checked some of the mail on my desk, most of which does not require an immediate response. However, I did read carefully the August 11 letter from Winston Manning and then got together with Stan and Al for a lengthy discussion about their reaction to it. They were rather outraged.

Later on I returned a phone call from Wes Fry (Backfield Coach), who wanted to talk about an eligibility problem that has arisen.

The senior staff had its usual luncheon meeting in my office; again much of the discussion was about Manning's proposal. I also checked on items for the meeting of the Laboratory Directors.

Alex E. S. Green (Florida State University) asked on August 10 for permission to use figures from some of our articles in an introductory text book in nuclear physics that he is preparing. I agreed to his request and sent the reprints he desired, noting that we shall send him a reprint of the "Table of Isotopes" as soon as they are received. In response to his request for the ionization energies of transuranium elements, I said that I know of no published source and suggested he contact O. C. Simpson at Argonne.

Much of my day was spent preparing for a report for the meeting of

the AEC Laboratory Directors, which will take place here on Thursday and Friday.

Tuesday, August 25, 1953

The document, "Proposal for Construction and Research Use of a Heavy Ion Accelerator," was completed today. Our part of the report described the research part of the program while Van Atta wrote about the machine itself. I began:

This is a proposal for a research program for the Radiation Laboratory using high energy (10 Mev per nucleon) heavy ions (C to Ne), together with a description and request for funds for an accelerator to be constructed in order to carry out such a program. The first section summarizes the proposed research program of a number of groups in the Radiation Laboratory and the second section is concerned with the description of a proposed machine and its estimated cost.

Under the research section I covered:

#### 1. Production and Identification of Transcalifornium Elements

The bombardment of uranium and transuranium nuclides with heavy ions seems to be the most feasible and economical way to synthesize isotopes of transcalifornium elements with sufficient neutron content to be long-lived enough for detection. The potential barrier in this region is such that an energy of about 10 Mev per nucleon for the accelerated heavy ion is required in order that nuclear contact cross sections comparable to geometrical cross sections might be realized. As large beams as possible are needed because of the severe competition of the fission process with the hoped for spallation reaction (e.g., C,xn reaction).

These new elements are of interest from both a nuclear and a chemical or atomic standpoint. From a nuclear standpoint there is great interest in ascertaining whether or not neutron or proton closed shells or subshells are being approached giving a region of relative stability. In this region there might be nuclides of special interest. The alpha decay energies afford a sensitive check on the binding energies of such nuclides. The relation between alpha energy and Z and A affords an excellent test for closed shells and subshells. Also, alpha decay rate dependence on energy for even-even nuclides generally exhibits retardation near closed shells, and the first excited states of even-even nuclei show increases near closed shells. The decay study of nuclides in this region will add much to the understanding of radioactivity, especially alpha activity, through the contribution of such data to the systemization and general understanding of nuclear structure in general. Spontaneous fission will rapidly attain a greater importance as a mode of decay and therefore a great deal can be learned about this process and the activation energy for fission in general.

The transcalifornium elements are interesting from an atomic or extra nuclear standpoint in a number of respects. Their chemical

properties should add significantly to our general understanding of the role of the 5f electronic shell in determining the chemical and physical properties of the actinide series of elements. Relativistic effects will play an increasingly greater part in influencing the binding energies and wave functions of the most tightly bound electrons in these high Z nuclides. The effects of finite nuclear size on x-ray energies and on internal conversion and electron capture decay processes should be rapidly increasing in importance due to the shrunken K electron wave functions and increasing nuclear size.

I then went on to cover 2. Nuclear Reaction Mechanisms and Structure, 3. Production and Study of Highly Neutron Deficient Nuclides of the Heaviest Elements, 4. General Study of Highly Neutron Deficient Nuclides, and 5. Study of Interactions of Heavy Ions with Matter. Van Atta then discussed the machine, saying in part that it would appear from past experience that adequate beam currents of the following ions could be produced with a reasonable developmental effort on the ion source:  $C_{12}^{+3}$ , 0.250 (e/m);  $N_{14}^{+4}$ , 0.286;  $O_{18}^{+5}$ , 0.278;  $F_{19}^{+5}$ , 0.263;  $Ne_{22}^{+6}$ , 0.273.

Among today's phone calls were a call to Jonathan Rice about BAETA, a call to Joe Katz about PPR, Volume 14A, and one to Ray Wakerling (Information Division) to set up a meeting for tomorrow. I also telephoned Rixford Snyder at Stanford about some athletic matters. In the course of that discussion I said I would arrange to get him tickets for some football games this fall.

The June issue of Reactor Science & Technology arrived today; it contains the revision of the editorial I wrote some time ago, which was considered too classified to be published. The editorial did include my suggestion about routine recovery of pile by-products, especially some important isotopes of transuranium elements, such as  $Np^{237}$  and  $Am^{241}$ .

Wednesday, August 26, 1953

I talked with Bob Connick about corrections in the page proof of his PPR, Chapter 8 ("Oxidation States, Potentials, Equilibria, and Oxidation-Reduction Reactions of Plutonium") for Volume 14A and also telephoned George Briggs about some tickets for Rix Snyder before meeting with Ray Wakerling.

I then sent Joe Katz the corrected page proofs of Chapter 8 (Connick's), 9 ("Ionic and Molecular Species of Plutonium in Solution" by J. C. Hindman), 10 ("Preparation and Properties of the Compounds of Plutonium" by B. B. Cunningham), 11 ("Nuclear Properties of Neptunium Isotopes" by G. T. Seaborg), and 12 ("The Chemistry of Neptunium" by B. B. Cunningham and J. C. Hindman). I pointed out to Katz a grammatical error in Chapter 7 and made a few other comments on the chapters.

Later I wrote to Dick Diamond at Harvard to send him a copy of our paper ("An Ion-Exchange Study of Possible Hybridized 5f Bonding in the Actinides"), which I had shortened to some extent. I told Dick that I am having some of the figures redrawn and that, since it will probably have to be retyped before going to the journal, he should feel free to make

changes. I also suggested that the data on the alkaline earths (which also show a reversal in elution order with concentrated HCl) should be brought back into it, emphasizing that we think an alternate explanation involving hydration shells is worth considering in addition to 5f bond hybridization.

A note arrived in the mail from Norman Bonner that his proposed trip to Sweden has not materialized and that he expects to arrive in Livermore within a week, one way or the other, of October 15. I told Doral to route it to Ken Street, Earl, and Iz.

At 3:15 p.m. I attended a meeting about security in Bldg. 64.

Al Ghiorso prepared the following information for me, primarily for use at the meeting of the Laboratory Directors tomorrow:

8/26/53 AG

## HEAVY ELEMENT DATA

VALUES SUBJECT TO CHANGE WITHOUT NOTICE

[ ] Indicates calculated or guesstimated item.

ISOTOPE	RADIATION	E Mev	T <sub>1/2</sub>	T <sub>9</sub> burns	T <sub>1/2</sub> burns	REMARKS
Cm <sup>245</sup>	$\alpha$	5.36 (75%) 5.46 (25%)	$\sim 1/5000$ y		[?]	Energy abundance from cyclotron expts $T_{1/2}$ from Mike expts
Cm <sup>246</sup>	$\alpha$	5.36	[6000 y]		[0]	
	SF		$3 \times 10^7$ y			
Cm <sup>247</sup>	[ $\alpha$ ]				[?]	$\beta - T_{1/2} > 10^3$ y
Cm <sup>248</sup>	[ $\alpha$ ]			2	[0]	
Cm <sup>250</sup>	[ $\alpha$ ]			150 (?)	[0]	$\alpha$ Highly dependent on $T_{1/2}$ Cf <sup>251</sup>
Bk <sup>249</sup>	$\beta^-$	$\sim 0.05$	1 y	730	[~0]	
	$\alpha$	5.42	$10^5$ y			
Cf <sup>249</sup>	$\alpha$	5.81 (90%) 6.00 (10%)	500 y	1000	300 (?)	$\alpha$ based on destruction of Cf <sup>249</sup> Cf dependent on $T_{1/2}$ Cf <sup>250</sup>
Cf <sup>250</sup>	$\alpha$	6.04	[20 y]	LARGE (?)	[0]	
	SF		1100 y			
Cf <sup>251</sup>	$\alpha$	6.04 (?)	[200 y]	100 (?)	[small]	
Cf <sup>252</sup>	$\alpha$	6.14	$\sim 6$ y	10	[0]	
	SF		230 y			
Cf <sup>253</sup>	$\beta^-$		20 d		[~0]	
Cf <sup>254</sup>	[ $\alpha$ ]				[0]	
	SF		60 d			
99 <sup>253</sup>	$\alpha$	6.64	20 d			
99 <sup>255</sup>	$\beta^-$		35 d			$\alpha T_{1/2} > 1$ yr
100 <sup>255</sup>	$\alpha$	7.12	16 h			
100 <sup>258</sup> (?)	[ $\alpha$ ]					
	SF (?)		15 d (?)			

Thursday, August 27, 1953

Present at the meeting of the research group this morning were Asaro, Cobble, Feay, Gray, Gunn, Hoff, Hollander, Jaffe, La Salle, Levy, Michel, Nervik, Passell, Raby, Seaborg, Slater, Smith, Stephens, Surls, and Templeton.

Asaro spoke about another revision in the decay scheme of  $\text{Am}^{241}$ , saying that from their original work  $\text{Am}^{241}$  was reported to have six alpha groups with the energy levels above the  $\text{Np}^{237}$  ground state as 169 kev, 1.4%; 114 kev, 13%; 71 kev, 84%; 43 kev, 0.2%; 11 kev, 0.2%; and 0 kev, 0.2%. He said that gamma rays of 60 and 26 kev have been confirmed several times; however, no one has seen a transition from the 11 kev level to the ground state. Their evaluation of the results of checking the machine with  $\text{Po}^{210}$  and another sample of  $\text{Am}^{241}$  has led them to conclude that low intensity instrumental peaks on the high energy side of the main peak appear when the main peak is on the high energy side of the plate, possibly because of a poor beam baffling system. Asaro said they now believe that the formerly assigned 11 kev level is actually the ground state. There was considerable discussion about the work.

Smith again reported on his iridium and platinum activities, saying that they have had a 100 Mev proton bombardment of platinum to form  $\text{Au}^{189}$ . Three platinum milkings of the purified gold fraction showed a mixture of 6-hour and 11-hour activities and a large amount of 3-day activity, from which a plot of the yield of the 3-day activity as a function of time of milking gave a 3.3-hour value for the half-life of the gold parent. The gross decay of the gold fraction, he said, indicates the presence of ~40-minute, 3-hour, ~6-hour, 1-day, and 3-day activities. The milking experiments indicate the 3.3-hour activity belongs to  $\text{Au}^{191}$ .

\* \* \* \*

Much of the rest of my day was spent at the meeting of the Laboratory Directors. This was attended by such people at Clarence E. Larson (Oak Ridge), Walter H. Zinn (Argonne), Leland J. Haworth (Brookhaven), Frank H. Spedding (Iowa State College), Thomas H. Johnson (Director of Research, AEC).

I had asked various staff members to outline the work of their groups for me and so was able to present a rather complete picture of our research at the meeting. I first described the chemistry of the heavy elements and the lanthanide elements, based on microcalorimetry; the structural determination of curium compounds using x-ray methods (including comments on our new x-ray equipment); vapor pressure measurements by the effusion method (comparing our measurements on  $\text{PuF}_3$  and plutonium metal with those of O. C. Simpson several years ago and our new measurements on  $\text{AmF}_3$  and americium metal). I covered our magnetic susceptibility studies, paramagnetic resonance studies (with Arthur F. Kip), and thermodynamic data for transuranium elements, including Cobble's experiments to determine the entropy of  $\text{NpO}_2^{++}(\text{aq})$ .

Next followed a description of our spallation and general cyclotron

studies, including the use of the time-of-flight mass spectrometer to aid in mass assignments of cyclotron-produced activities. I also talked about our experiments to determine the relative yield of isomer pairs in cyclotron bombardments as a function of particle bombardment energy.

The third portion of my talk covered instrumentation, including alpha energy analysis, pulse height analysis, the Cauchois bent crystal x-ray spectrometer, the alpha-particle spectrograph, the time-of-flight mass spectrograph, the beta-ray spectrometer, the mass spectrometer, Consolidated Engineering Corporation Commercial model mass spectrograph (Newton's instrument), and the analog computer for pile bombardment calculations.

I reviewed the history and described our activities in carbon ion acceleration and bombardments, and then went on to discuss our future plans, including the new accelerator.

A large portion of my talk was on our heavy element isotope research, including such things as nuclear systematics of the heavy region, alpha spectrum analysis, regularities already developed in even-even nuclei, nuclear emulsion studies, decay schemes of electron capture isotopes in the heavy region, new studies of berkelium and californium isotopes, atomic spectroscopy of heavy elements, isotopic shifts, etc.

I also described our present radiochemical procedures and techniques and concluded with a few comments about a number of miscellaneous programs, such as Newton's radiation chemistry work, liquid scintillators, Templeton's general x-ray program, Huffman's analytical program, and finally the health aspects of research in nuclear chemistry.

Later I telephoned John Francis Neylan to ask him about the visit of the Regents' Committee on Atomic Energy to Livermore. Neylan suggested that I telephone him next week.

#### Friday, August 28, 1953

The meeting of the Laboratory Directors continued today.

Later, in another meeting today it was decided that Bldg. 5 will be closed from 12 midnight until 8 a.m.

#### Saturday, August 29, 1953

I returned the page proof of Chapters 13 ("Nuclear Properties of the Transplutonium Nuclides" by G. T. Seaborg), 14 ("The Chemistry of the Transplutonium Elements" by I. Perlman and K. Street, Jr.), 15 ("Radiochemical Separation of the Actinide Elements" by E. K. Hyde), and 16 ("Radiochemical Assay by Alpha and Fission Measurements" by A. H. Jaffey) of PPR, Volume 14A, to Joe Katz. I pointed out that Hyde is including two versions of a Table of Contents for Chapter 15, which he has prepared and which he feels would be useful. In addition, I asked Joe to check on a couple of other things.

Sunday, August 30, 1953

Although I spent considerable time with the kids, I also worked in my study, in particular on my talk before the Division of Chemical Education.

Monday, August 31, 1953

This morning I made a number of phone calls as usual, including a call to Luis Alvarez to discuss plans for our heavy ion linear accelerator, and then worked on some of the paper work that has accumulated on my desk.

We had our regular senior staff luncheon meeting in my office at noon, and later I got together with Stan and Al to discuss the reply I should prepare to Winston Manning's letter of August 11.

In today's mail was an acknowledgement from Chairman Lewis L. Strauss of my letter of August 12 about the declassification of the spontaneous fission values of  $Pu^{240}$  and  $U^{236}$ . Strauss said he will reply when his colleagues come together again (they are scattered on vacations or errands).

In my rounds of the labs I noticed that Winifred Heppler, Burris Cunningham's technician, is again working. She was terminated some time ago because of maternity activities.

Tuesday, September 1, 1953

The first thing I did this morning was to return more page proofs of PPR, Volume 14A, to Joe Katz--Chapter 17 ("Correlation of Properties as Actinide Transition Series" by G. T. Seaborg), Chapter 18 ("Crystal Chemistry of the 5f Elements" by W. H. Zachariasen), Chapter 19 ("Optical Properties of Some Compounds of Uranium, Plutonium, and Related Elements" by E. Staritzky and A. L. Truitt), and the Preface. I made some comments on these chapters and then added that I have been looking over the Index, which I will probably bring to Chicago with me for it is a tough job to check and I am puzzled as to what to do.

Included in today's phone calls was one to George Briggs to remind him about the football tickets for Rix Snyder, one to Luis Alvarez, and one to John Francis Neylan. Neylan, who had requested I call him after he met with the Regents' Committee on Atomic Energy last week, suggested that he write AEC Chairman Strauss about our patent matter. I agreed that this would be useful.

Doral pointed out to me that W. George Parks had returned my check for \$39.80, dated July 6, because the Bank of America had returned it to him marked "Signature Missing." This was payment for my Gordon Conference bill. I signed, and Doral returned it to Parks.

Considerable time today was spent discussing the proper response to Winston Manning's letter of August 11 with Stan, Al, and Gary Higgins; we also talked about the current status of the debris program. Then, at about 4 p.m., I telephoned Rod Spence. I told him about the status of the 3000 pounds of debris, which has four to five times the activity of the previous batch and for which the chemical separation will be finished in a week or two. I asked if we could keep the entire element 100 fraction to look at the spontaneous fission activity before sending them their one-third. Spence agreed to this proposal. He told me that the Mike report is in press and that he will ask Smith (Ralph Carlisle) about declassification of the Radiation Laboratory version. After reviewing the difficulties in publishing together with Argonne (especially on element 100), I asked if Los Alamos would like to go in with Berkeley on open publication. Spence said that Louise Smith and Jack Barnes are working with Charles Browne. I mentioned all three, with Spence, as possible co-authors. Rod said he will talk it over with Browne and call back, possibly by Friday.

Today another of our new Ph.D. graduates, Harris B. Levy, joined the chemistry group of Project Whitney.

Wednesday, September 2, 1953

The first thing I did this morning was to reply to Winston Manning's letter of August 11:

DECLASSIFIED

~~XXXXXXXXXX~~  
SECURITY INFORMATION

~~XXXXXXXXXXXXXX~~ A September 2, 1953

This document contains restricted data as  
defined in the Atomic Energy Act of 1946.

Dr. W. M. Manning  
Argonne National Laboratory  
P. O. Box 299  
Lemont, Illinois

MB-GTS No. 129

This document consists of 1  
pages and 0 figures.

No. 3 of 3 copies. Series A

Dear Winston:

B  
P  
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This is in reply to your letter of August 11, 1953. I have gone over  
your proposal very carefully with Stan and Al and Gary, who had already  
studied it during my absence on vacation.

They feel that the proposal is so unfair with respect to our group that  
they cannot agree to it, and I agree with them so far as element 100 is con-  
cerned. Aside from the equities in the situation so far as this element is  
concerned, and I have gone into this so much in conversation and previous  
correspondence as to make it unnecessary to review it here, there is the  
apparent impossibility of publishing contradictory versions together.  
Either you are still maintaining your position on the 6.6 Mev 100 alpha,  
which we are absolutely certain is erroneous, or you have withdrawn from this  
position, in which case the question of publishing together doesn't arise.

Classification changed to  
by authority of  
Date  
on

Your remarks concerning element 99 deserve some comment. Not only are  
you wrong in suggesting priority on the initial December dates, as I have  
previously mentioned, but the majority of the group here feel that you have  
not yet completed the identification of this element due to the confusion in  
your distribution of its alpha particles between two atomic numbers, as I have  
also repeatedly suggested.

I should add that for the first time I am beginning to give up hope that  
we can come to an agreement on this matter. My own proposal went so far in  
your direction that I hadn't yet been able to sell it to Stan, Al and Gary  
(who feel my loyalties to Argonne are excessive), but I hadn't made too great  
an effort pending possible agreement from Argonne and perhaps could have  
succeeded in that event. I felt that this plan had about the right degree of  
concession on each side and met on the middle ground at an equitable place.  
However, even if we have failed, I feel that my many visits to Argonne  
primarily for the purpose of settling this matter were worthwhile, because,  
in my own opinion, such settlement was so desirable.

I notice that I haven't commented on your proposal of a neutral arbi-  
trator or arbitrating committee. I think that it would be difficult to find a  
neutral body with the prerequisites knowledge of the radioactivity, especially  
alpha radioactivity, of the heaviest elements. If we pursue our separate  
courses we will have subsequent scientific developments and the composite  
judgment of the scientific world as the ultimate judge, and this will in the  
long run be accurate with complete certainty.

I shall be glad to discuss this further with you and any of the members  
of your group during my visit to Chicago to attend the A.G.S. meeting next  
week if you think that it would be worthwhile.

DECLASSIFIED

Cordially yours,

GTS/db  
cc W.H.Zinn

~~XXXXXXXXXX~~ Glenn T. Seaborg

Yesterday a wire was received by the Graduate Division from John C. Cothran (Chairman, Scientific Division, Duluth Branch, University of Minnesota), requesting a transcript and evaluations from three men for Henry M. Neumann, who is being considered for an assistant professorship. This was referred to Iz and me, and today we wired, "HAVE HIGH OPINION OF SCHOLARSHIP, RESEARCH ABILITY, AND GENERAL PROFESSIONAL COMPETENCE OF DR. HENRY NEUMANN. HE HAS A GREAT INTEREST IN TEACHING AS HE HAS RECENTLY TURNED DOWN AT LEAST ONE HIGH PAYING RESEARCH POSITION TO STAY IN ACADEMIC WORK. HE WAS COOPERATIVE IN OUR GROUP EFFORTS AND IN HELPING NEWER STUDENTS."

Jonathan Rice and I met and had lunch together; he talked at length about plans for KQED, the Bay Area's educational television station, which is scheduled to begin broadcasting in December with a minimum schedule. Actually, Terrence O'Flaherty devoted his column in yesterday's San Francisco Chronicle to the station, suggesting that the name educational television should be dropped as being stuffy (he prefers the adjective community). O'Flaherty, an enthusiast of the idea, pointed out that Channel Nine programs must be professionally produced to be effective for amateur blackboard television can ruin the project.

Back on the hill I found a telegram from Frank Holloway (Chicago ACS) reading: "PLEASE CONSIDER GIVING A BRIEF INTERVIEW OVER RADIO STATION WLS DURING ACS WEEK WILL CONTACT YOU AT EARLIEST POSSIBLE DATE IN CHICAGO." I then dictated a number of short responses to letters that have accumulated on my desk. Robert E. Robinson (Chairman, Committee on Drama, Lectures, and Music, Santa Barbara College) wrote on August 27 to ask if I have been able to fit a talk into their Lecture Series during the fall semester of 1953-54. I replied today that I have been planning to try do this, saying that any date I suggest at this early time will be subject to some uncertainty. With this in mind, I said that I should like to suggest the second Wednesday in January, that is, January 13; I offered the title of "Atomic Energy."

Alberto F. Thompson wrote on August 18 with questions about the role of the Subcommittee on Publications and Information of the National Research Councils' Committee on Nuclear Science. In my response I wrote that I am a member of the Joint Commission on Radioactivity of the International Council of Scientific Unions, and, although I was unable to attend the Stockholm meeting last month, I was asked for my opinion on the matter of nuclear symbol terminology. I sent Thompson a copy of the letter I wrote to F. A. Paneth, noting that he will see that I am pretty much in the process of trying to find a position on this matter and, therefore, I shall be interested to learn the results of their survey.

W. Albert Noyes (Editor, Journal of the American Chemical Society) sent me on August 28 a manuscript by John R. Ferraro, Leonard I. Katzin, and George Gibson ("The System Thorium Nitrate-Water-Nitric Acid..."), along with the negative referee report by Don Yost. Noyes asked for my opinion. I wrote that I agree that some revisions are necessary, but I do not agree with the main thesis of the referee that the paper is not worth publishing. I said, "A number of the general conclusions of Referee I are not accurate. Thus I know from a personal knowledge of the situation that this work does not represent engineering design data and is not connected with any engineering process at all...."

W. Sterling Cole (Chairman, Joint Committee on Atomic Energy) wrote on July 22 and August 1 for recommendations about the matter of industrial participation in the atomic power development. In my response today I apologized for not being able to have a reply in his hands by August 10 as he suggested and said that I don't have any definite recommendations to make on this complicated question. I wrote, "I do feel, however, that we should be very careful not to impede our progress due to unnecessary secrecy limitations, and I believe that careful consideration should be given to the possibility of releasing all information which would be useful and helpful in a program of industrial participation consistent with our national security."

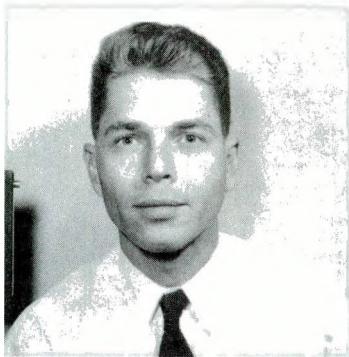
I also wrote to W. H. Johnston (Purdue University) about my talks there in October, suggesting that we discuss the arrangements at the ACS meeting in Chicago. I explained that I shall be in Philadelphia earlier in the week of October 11 and might be able come to Lafayette on Thursday afternoon, October 15; on the other hand, I said, I should like very much to take the 1:45 a.m. flight out of Chicago early Saturday morning.

A note also went to Donald A. McPherson (Row, Peterson & Company), saying that it looks as if I shall be in Chicago the morning of September 10 so that we should be able to get together for breakfast. (McPherson suggested this a few weeks ago.)

At 6 p.m. I attended a dinner meeting of the Executive Committee of the California Section of the ACS; this was held at Spenger's. I agreed to represent the California Section at the meeting of the Council of the ACS at the upcoming meeting in Chicago.

Thursday, September 3, 1953

This open meeting of the research group was attended by Asaro, Cunningham, Alfred Chetham-Strode, Jr. (a new graduate student, who obtained his bachelor's degree from Rice Institute and has been employed at Hanford), Grover, Gunn, Hoff, Hollander, Huffman, Jaffe, Kalkstein, La Salle, Mathur, Passell, Perlman, Ruben, Seaborg, Shudde, Smith, Stephens, George V. Shalimoff (with Huffman's group), Templeton, and Wallmann.



A. Chetham-Strode



G. Shalimoff

Hoff reported on experiments with  $\text{Am}^{242m}$ , produced by a five-day irradiation of  $\text{Am}^{241}$  in the MTR in Arco, Idaho. They observed the x-rays from the mixture on the bent-crystal spectrometer and set an upper limit of 5% for the isomeric transition from the observed abundances of curium,

plutonium, and americium x-rays. Hoff said that  $Q_{EC}$  calculated from closed cycles is 0.65 Mev and that the  $ft$  values for the beta decay and electron capture of  $Am^{242m}$  indicate a first forbidden decay, being 6.9 and 6.7 respectively;  $I = 0, 1$  with parity change. He noted that Jaffe reports the observation of K x-rays of approximately 100 kev in a sample of long-lived  $Am^{242}$ , which would indicate a K electron capture branching of 10%. Hoff presented a decay scheme.

Asaro talked about his work on the alpha spectrum of  $U^{230}$  (half-life = 20.8 days). In discussing the decay scheme, he said that it was found that the major fraction of the alpha group corresponding to the supposed decay to the 205 kev level was due to a  $Ra^{224}$  impurity caused by the presence of  $Pa^{228}$  in the original  $Pa^{230}$  sample. Another bombardment was performed in which the 22-hour  $Pa^{228}$  was allowed to decay before the protactinium fraction was purified. Asaro said the alpha spectrum of the  $Pa^{230}$  series was again observed at low resolution and it appears that the upper group is still a doublet with the lower gamma peak at 210 kev and the upper peak at 230 kev. The abundance ratio 230/210 is about 3; they will perform another experiment at high resolution to resolve the doublet and discover its origin. Asaro went on to say they observed gamma rays of 230 and 160 kev, in addition to the previously reported gamma ray of 72 kev, adding that these agree with the expected characteristics of a 4+ state (at the 230 kev level) but not with the expectations for a 1- state. He said the conversion coefficients of these gamma rays have not been accurately determined.

\* \* \* \*

I stopped in the office of the Department of Chemistry to speak with Miss Kittredge and some of my colleagues before going up to the hill, where I made certain I had my slides for my trip and then talked with some of the fellows.

Friday, September 4, 1953

I made a few phone calls and then got together for a while with Iz and Earl to discuss our material for the Semi-Annual Report, which is due September 16.

I also talked with Roger Batzel about the  $Pa^{231}$  question (source of  $U^{232}$  for use as an initiator in nuclear weapons).

Clyde A. Hutchison, Jr., sent me on August 26 a copy of a paper with Dieter Gruen on the magnetic susceptibilities of neptunium and asked for criticism. I wrote that I think it is a good piece of work and said that I am enclosing a few comments by Professor Cunningham who also read the manuscript.

A memorandum went to W. S. Bigelow in Personnel, requesting that Dr. Theodore Vermeulen be granted a leave without pay at the Radiation Laboratory, starting September 16 and extending to about July 1, 1954. Vermeulen, I said, has a sabbatical leave from the University.

After checking with some of the students, I went down to campus and had lunch with Brutus Hamilton. Much of our conversation centered on my

Alumni Day talk about athletics in Los Angeles on September 27.

Later, back on the hill, I visited some of the labs to talk with some of the fellows, including Frank Asaro.

Saturday, September 5, 1953

A lab driver picked me up at 7 a.m. and drove me to the airport in San Francisco in time to catch the 8:45 a.m. United Flight 602 to Chicago. There was a little excitement as I was about to board the plane when a lady, already on the ramp, sank to the floor. I couldn't really see what happened--whether she tripped, fainted, or died. However, some man from the boarding area, whom I presume was a doctor, came and examined her. The plane arrived in Chicago about 5:30 p.m., and I took a taxi to the Conrad Hilton Hotel.

Later Joe Katz came to my hotel room; we talked about a number of things, including PPR, Volume 14A, specifically the matter of how to handle the Index. Joe also told me that he has decided that he really would like an academic position. I said that I will "ask around" at this ACS meeting about a suitable one.

Sunday, September 6, 1953

In Chicago. In the afternoon I registered for the 124th National Meeting of the American Chemical Society and then, at 3 p.m., went to Roger Adams' room for a meeting of the Research Subcommittee of the ACS Committee on Chemical Warfare.

Later I saw and talked with many old acquaintances and colleagues, including Paul O'Connor, to whom I mentioned that Joe Katz had recently told me about his desire for an academic position. Paul said that he will speak to Reyerson (Lloyd H.), Assistant Dean of the School of Chemistry there at the University of Minnesota about it and possibly talk with Katz himself.

Monday, September 7, 1953

In Chicago. At 9:30 a.m. I attended a closed meeting of the ACS Council Publications Committee in Room 6 of the Conrad Hilton. Also present were Arthur Rose (chairman), Leigh C. Anderson, Otis C. Dermer, Melvin C. Molstad, and Vincent Sauchelli. Dermer was elected to serve as secretary. There was some discussion of the perennial problem of reduction in bulk of Society publications, and Audrieth (Ludwig F.) was named to head a subcommittee to study and make recommendations on the several alternative forms of microreproduction of such publications. We also discussed the history and function of this committee.

At 5:15 p.m. I went to a social hour for University of California alumni in Room 440 of the Hotel Morrison, where I saw and talked with many old friends and students.

The meeting of the Executive Committee of the Division of Physical and Inorganic Chemistry was held at 7:45 p.m. in Parlor C of the LaSalle Hotel, which I attended. Among the items discussed were plans for the

Executive Committee to meet during the second week of the Kansas City meeting. Particular attention was paid to future symposia, not only for the Kansas City meeting next spring, but also for the fall meeting in Atlantic City in 1954. Suggestions included: 1. High Temperature Chemical Reactions (Leo Brewer); 2. Activation Energies in Elementary Reactions (James N. Pitts, Jr. of Northwestern); 3. Crystal Structure by Diffraction (William N. Lipscomb, Jr. of the University of Minnesota or someone else suggested by Lipscomb). J. O. Hirschfelder brought up the question of the desirability of the American Chemical Society co-sponsoring the Journal of Chemical Physics. Our Executive Committee moved to favor the co-sponsorship, if it seems feasible after further exploration.

After the meeting I spoke with Joe Kennedy (Chairman-Elect) for a while about our patent problem; I also mentioned Joe Katz' desire for an academic position.

I missed most of Farrington Daniels' Presidential Address, "A Challenge to Chemists." This took place in the Ballroom of the Conrad Hilton and was followed by the usual social hour.

Tuesday, September 8, 1953

In Chicago. There was an open meeting of the ACS Council Committee on Publications at 9 a.m.; instead of attending it, I went to the meeting of the Council of the ACS as a representative of the California Section. It was held in the North Ballroom of the Conrad Hilton at 9:30 a.m. After the meeting Charles L. Thomas (Chairman, Committee on Constitution and Bylaws) pointed out to me a couple of sections in the California Section bylaws that are inconsistent with the National bylaws. These I shall point out to the California Section when I return home.

At noon I attended an official luncheon of the Division of Industrial and Engineering Chemistry.

Manson Benedict lectured at 3 p.m. in the Ballroom of the Palmer House on "The Chemical Engineering Side of Nuclear Power," and I found his talk to be very interesting.

At 4:35 p.m. I spoke on "Oxidation States of the Actinide Elements" at a symposium on recent advances in inorganic chemistry, sponsored by the Division of Chemical Education. This was held in Parlors B, C, and D of the Hotel Morrison, and was presided over by Jacob Kleinberg. An abstract of my talk is as follows:

The recent work on the heaviest elements, and particularly the transuranium elements, points to the beginning of a transition series with actinium in the same sense that the rare earth series begins with lanthanum. However, there is the important difference that the first elements in the heavy series exhibit the property of oxidation above the tripositive state with much greater ease than is the case for corresponding elements in the rare earth series. A very important criterion for this classification is the probable presence of seven 5f electrons in tripositive curium (ekagadolinium) and the presumed presence of fourteen 5f electrons in element 103, which

apparently establish the general position of elements 90-103 and the following elements in the periodic system. There might not be any such electrons in thorium with, for example, their first appearance in protactinium (two 5f electrons) or in uranium (three 5f electrons). An important aspect of these considerations is the probability that the 5f and 6d shells of the elements in this heaviest region lie so close together that the energy necessary for the shift from one shell to the other may in some cases be within the range of chemical binding energies.

The earliest element in this series which clearly exhibits the characteristic oxidation state of III is uranium, the third element in the series. In going up the series from uranium to curium, each of the successive elements exhibits a more stable III oxidation state than the preceding element. For the elements americium and curium the III state is the predominant one, and, in fact, seems to be the only state which is thermodynamically stable in acidic aqueous solution. Experiments indicate that berkelium exhibits stable III and IV oxidation states and californium exists solely in the III state, which behavior is in entire conformity with their expected analogy with their rare earth homologues, terbium and dysprosium, respectively. These considerations may be summarized with the standard oxidation-reduction potentials in volts for the III-IV change, which are as follows: U, 0.63; Np, -0.14; Pu, -0.98; Am, ~-2.4; Cm, <-2; and Bk, -1.6.

Perhaps the most striking chemical evidence as to the course taken in the filling of the 5f electron shell in this region is that offered by the work on the separation of the tripositive actinides and lanthanides by the ion exchange column method. A remarkable analogy in the spacing is seen between the group californium-berkelium-curium-americium and their rare earth homologues dysprosium-terbium-gadolinium-europium. Since the spacings here reflect the relative changes in ionic radii, the same sequence of changes in the ionic radius is encountered on filling in the 5f electrons as occurs on filling the 4f shell, offering clear evidence that curium represents the midway point in the actinide series of elements in view of its position analogous to gadolinium.

Thus it can be predicted with confidence that elements 99 to 103 inclusive will be almost exclusively tripositive, homologues of holmium to lutetium respectively, with analogous positions in the ion exchange adsorption method of separation. The chemical properties of the following elements may also be predicted, with element 104 as ekahafnium, element 105 as ekatantalum, element 106 as ekatungsten, etc.

I illustrated the talk with 18 slides.

The dinner meeting of the Editorial Board of the Journal of the American Chemical Society was held at 6:30 p.m. in Room 1 of the Conrad Hilton, and I attended. The question of a replacement for Pitzer, whose term is expiring, was discussed.

During the day I talked with a number of people, including Raymond R.

Edwards (University of Arkansas), who said he has proved that RaE has three isomeric states--5 days (on top), 30 hours, and 6 hours (below--this transition is low energy, M converted). Darrell Osborne told me that he is planning to visit Livermore and Berkeley from Wednesday, September 16 until Friday, September 18. He will be in Berkeley on Friday morning. I also talked with Gallie Bogel (Charleston, West Virginia), who is with Union Carbide--he knows Ann Carlson, a childhood friend of mine from Ishpeming who now lives in Charleston. Earl T. McBee (Purdue) approached me to discuss my talks there next month, and afterwards I mentioned Joe Katz' desire for an academic position. (I also discussed this with Lyle Dawson of the University of Kentucky.)

Wednesday, September 9, 1953

In Chicago. From 9 a.m. until noon I met with the ACS Committee Advisory to the Chemical Corps.

Again I chatted during the day with many acquaintances and collected a variety of messages and news items. I learned about a proposed Burris Cunningham-Anton Burg chapter on the actinides for an Interscience book. Russell R. Williams is interested in leaving Notre Dame. Truman Kohman asked that I send the UCRL report on  $\text{Sm}^{146}$  to him; he is now looking for  $\text{Gd}^{152}$  alpha particles. I learned that next year's Gordon Research Conference on Nuclear Chemistry is scheduled for the week of June 28 through July 2. Thomas R. P. Gibb told me that his consulting contract was arranged and is satisfactory. He said that he has made  $\text{FeH}_6$  with  $\text{Al}/\text{Fe} = 2$ , but the aluminum is metallic and he hopes to remove it. He also told me that beryllium hydride should be thermodynamically stable; he may work on it.

As Frank Holloway (Chicago Section) requested before I left Berkeley, I was interviewed for five minutes on the subject of atomic energy. This will be broadcast on the 10:15 a.m. news tomorrow on radio station WLS.

I attended the dinner meeting of the Division of Physical and Inorganic Chemistry at 6 p.m.

Thursday, September 10, 1953

In Chicago. As we arranged, Donald A. McPherson of Row & Peterson contacted me and we had breakfast together. McPherson, who is an old acquaintance, is most enthusiastic about his company and described their policy, which has the authors participating in the profits. McPherson said they are now in the elementary and high school field, but they plan to enter the college field. He said that he would like me to edit the texts of the physical sciences, starting on an informal, non-time consuming basis at about \$1000 per year. My response was that I will think about it since my time is so limited.

Always on the outlook for men for Project Whitney, I interviewed Thomas B. Douglas (with U. S. Bureau of Standards). Douglas said that he is in Brickwedde's (Ferdinand G.) Division, has one man working for him, and earns \$7240/year. I said we would contact him if anything develops for a man with his abilities.

I also talked for a while with George Watt--I mentioned Joe Katz' interest in an academic position. Watt told me about a biochemical position at Louisiana State University, possibly a good position for Greg Choppin.

I saw Dr. Pietsch and the American representative of the Gmelin Institute, and we discussed various policies. I told them that it is a good idea to have the English version of Gmelin volumes on uranium, thorium, and rare earths frequently revised, and I learned that they have given up the Schwarzenbach idea of international support (about which I have had some correspondence recently). The price for the volumes, I learned, will probably increase by a factor of 2.5.

Again I picked up a lot of interesting information from a number of people. Art Jaffey told me he found three spontaneous fission counts in  $U^{232}$  in approximately 100 hours of counting, corresponding to a "half-life" of  $8 \times 10^{13}$  years, a lower limit for the half-life. G. Pyle gave me some analyses of the Gumshoe test, which showed 1.00-1.32%  $U^{235}$  (4% by mass spectrometer); there were 20 c/m of plutonium with the ratio of  $Pu^{240}/Pu^{239} = 3.7/96.1$  (uncorrected for 8%  $Np^{239}$ );  $Pu^{241} = 0.21\% + 0.02$ , -0.1;  $Pu^{242} = <0.03\%$ ; 0.16%  $U^{236}$ ; 0.27%  $U^{237}$  (mass spectrometer). Old Hat gave 0.77%  $U^{235}$ , 3.6%  $Pu^{240}$ , 0.14%  $Pu^{241}$ . Charles Stevens made some measurements with the mass spectrometer. I learned that Sherman Fried, William D. Urry, Enrico Fermi, Hans Bethe met in Los Alamos to try to make sense out of the data but didn't succeed because the data obtained by ordinary means was not sufficient.

Larry Glendenin said that fission product analysis of Gumshoe shows the emission of about 8 to 9 Mev neutrons;  $Be^7/fission\ products = 10^{-3}$ . John Huizenga said that Marty Studier is now working on a napkin ring. Larry Asprey unsuccessfully tried to react  $CmF_3 + F_2$ ; he found only  $Cm_2O_3$ , certainly no higher than  $Cm_4O_5$ , when he tried to oxidize  $Cm_2O_3$ .

Later I took a cab to the airport, where I caught United Flight 623 at 3:45 p.m., bound for San Francisco. A lab driver picked me up when I arrived (9:30 p.m.) and drove me home to Lafayette.

[In Berkeley the research group met as usual with the following people in attendance: Asaro, Carr, Cunningham, Feay, Hoff, Hollander, Huffman, Hulet, Hyde, Kalkstein, Kingsley, La Salle, Robert Fletcher Nickerson (a new graduate student from Tufts College),



Robert F. Nickerson

Perlman, Raby, Ruben, Smith, Stephens, Templeton, and Wallmann.

Hollander reported on carbon ion bombardments, using the 60-inch cyclotron, of tantalum. They looked at gold and platinum fractions, and he said they were able to isolate only a few thousand counts of a  $42 \pm 3$  minute gold activity when the chemical separation was lengthy. This was probably due to  $\text{Au}^{190}$  since they found a few counts of a 10-hour tail (platinum impurity) after several half-lives. With rapid chemistry they saw four different activities: the 40-minute activity and another gold activity with a  $9 \pm 2$  minute half-life. They also saw a 2-hour and a 10-hour activity, probably platinum impurities or daughters, and a 4-minute gold alpha activity. In response to a question from Perlman, he said the mass number of the gold alpha emitter has an upper limit of 187. Hollander reported the platinum fraction showed the 2-hour and the 10-hour activities with the 2-hour platinum appearing to be genetically related to the 9-minute gold activity. Since the energy of the carbon ion bombardments is not well defined, he has no excitation functions. In response to Perlman's request, Hollander reviewed the knowledge about the energy of the carbon ion beam, adding that they have had negative results when they looked for astatine activities produced by a nitrogen ion beam on platinum.

Smith talked about preliminary results on a number of new mercury, gold, platinum, and iridium activities produced from gold bombarded with 140 Mev protons, iridium bombarded with 50 Mev protons, and gold bombarded with 100 Mev protons.

Templeton then discussed the variation of atomic size in the lanthanides. He said that one of the reasons there is not complete agreement on ionic radii for the lanthanides is that the value depends on the method of calculation; however, he is interested in the relative change of the ionic radii, so almost any crystal constant can be used. He went on to describe some work he has done using the molar volume. There was considerable discussion about the work.

Hyde described some work on the gamma-ray spectrum of  $\text{Fr}^{223}$  (Ack), which he did on the 50-channel analyzer with a scintillation counter. He saw L x-rays, 50 kev gamma rays (36 photons/100 disintegrations), 80-85 kev gamma rays (25 photons/100 disintegrations), 224 kev gamma rays (3 photons/100 disintegrations), and possibly a 310 kev gamma ray. He pointed out that the 80-85 kev peak could be due to K x-rays from a highly converted high energy gamma ray; however, Perlman questioned this interpretation. Hyde found  $1.15 \pm 0.05$  Mev as the beta-particle energy using an anthracene crystal.

\* \* \* \* \*

Friday, September 11, 1953

Most of my day was spent at a nuclear engineering conference at the lab. The discussion was about nuclear reactors, with emphasis on breeder reactors. George L. Weil projected a breeder reactor that could produce electricity at 0.013 mil/KWH using uranium at \$35/lb. It was pointed out that a good non-breeder reactor is nearly as good as a poor breeder.

At the meeting I was presented with a letter of introduction from Bertrand Goldschmidt, a friend since Met Lab days, for two of the leaders

of the French Atomic Energy Commission--Pierre Taranger (industrial director in charge of building their large production piles) and Jacques Yvon (head of pile studies). Both of these men were attending this conference, and Bert wanted them to meet Iz Perlman and me.

Back in my office I looked over some of the mail and found a September 9 note from Lewis Strauss, saying that they have decided to declassify the spontaneous fission rate of  $U^{236}$  and  $Pu^{240}$ . Strauss said I will receive this information formally in due course. Hurray, at last! I immediately sent a note to Joe Katz to inform him. I also wrote to Emilio Segrè (presently at Brookhaven), apprising him of this fact and saying that I don't know whether he is anxious that his own publication (about the discovery of  $Pu^{240}$ ) appear before Volume 14A comes out (sometime in the fall), but if so, he will probably need to submit his write-up for declassification and publication soon. I emphasized that he has been given full credit in PPR, Volume 14A. In addition, I told Segrè that I conferred with Kennedy last week at the ACS meeting and we are rather anxious to learn more about his award. [Through a New York lawyer, Gianinni, the Italian group (E. Fermi, E. Amaldi, B. Pontecorvo, F. Rasetti, and E. Segrè) submitted a claim for compensation for their original work, starting back in 1934, on neutron activation of the elements.]

John Rasmussen stopped in to talk briefly with me; he and his family have returned from Sweden.

John P. Howe, who came to Berkeley to attend the nuclear engineering conference, is an old friend; Helen and I invited him and his family to stay with us during the meeting--we had a pleasant evening together (after the kids went to bed). John, who is with the Atomic Energy Research Department of North American Aviation, Inc. in Downey, California, discussed various problems facing his company and suggested a consulting arrangement for me to visit their laboratory in the near future. I agreed to do this at some mutually convenient time, and John said he will speak to their Administrative Services Section to arrange for a contract.

#### Saturday, September 12, 1953

I spent most of the day with the kids and Helen. Pete and Lynne told me about school and their new teachers. All of the kids are quite excited about a lively little toy fox terrier (part chihuahua) named Cricket that they and their mother just picked up at the Oakland pound.

Later I tried to catch up on some of my writing, reading, proofing, etc.

#### Sunday, September 13, 1953

I again spent the day with my family. I am also in the process of preparing a talk on the athletics for the Alumni Day in Los Angeles in a couple of weeks.

Since the new students have arrived and people are back from vacations, Helen and I plan to have our annual open house next Sunday.

Tomorrow I plan to have Doral arrange to have oral invitations extended to members of the group and written ones sent to those whom she is unable to contact.

Monday, September 14, 1953

Our students go on our half-time school year pay schedule today. I spent some of my morning on campus, talking with colleagues and keeping office hours.

I had lunch with Chancellor Kerr in the Faculty Club to report on a number of things, such as athletics and television.

Back on the hill I telephoned Wally Reynolds to emphasize Ed Orlemann's key role in the Whitney project.

Later the Ambassador to the United States from the West German Republic, Heinz Krekeler, and Vice-Consul, Dr. Stahlberg, visited the Radiation Laboratory. I spent about an hour giving them a tour--we visited the synchrotron, the 184-inch cyclotron, the linear accelerator, and the bevatron. I took them to Bldg. 50 for a short introductory visit with Ernest Lawrence and then gave them a quick inspection of the laboratory equipment in Bldg. 4. We also stopped briefly in Bldg. 5 and they were shown the declassified wall chart of isotopes in my office, the cave room, and a quick glance from the corridor into a couple of the labs.

I also looked over the three-page report that Earl and Iz prepared for submission for possible use in the Semi-Annual AEC report. They covered Chemistry of the Rare Earths and Heavy Elements; Time-of Flight Mass Spectrometer; Alpha Particle Spectroscopy; and Nuclear Properties of the Heaviest Elements.

Tuesday, September 15, 1953

This morning I tackled some of my accumulated correspondence and made a few telephone calls.

A short letter went to Alva C. Byrns (California Section, ACS) about my attendance as a California representative to the ACS Council meeting on September 8. I reported that there were no actions that require special comment prior to the issuance of the minutes of the meeting; however, after the meeting Charles L. Thomas (Chairman, Committee on Constitution and Bylaws) pointed out a couple of sections in the California bylaws that are inconsistent with the National bylaws. I pointed out these sections to Byrns, noting that Dr. Thomas brought these out in a very nice way and gave me the impression that they were anxious that the interference with our Section activities be at a minimum; I suggested if the changes can be made without too much inconvenience, we should do so.

About 9:30 a.m. Richard L. Doan (Idaho Falls) telephoned me and described the chemical plant takeover by Phillips, scheduled for October 1. Doan described for me some personnel needs--someone to head the analytical control group of 50 to 60 men and someone to head the

technical development section (unit operators, process design, chemical engineering, and chemists). He said Slansky (Cyril M.) needs some senior men to take some responsibility. Doan also asked about the possibility of a consultant (Iz or me). I told Doan that I will talk with Iz and call him back tomorrow. I then went into Iz' office to discuss the problem, and he said that he will think about possibilities.

The corrected page proof for Chapter 20 ("Slow-Neutron and Spontaneous-Fission Properties of Heavy Nuclei" by J. R. Huizenga, W. M. Manning, and G. T. Seaborg) and the Appendix of PPR, Volume 14A, was returned to Joe Katz.

There was a September 8 letter on my desk from A. L. Thompson (McGill University), who asked about the progress on our linear radio frequency mass spectrometer that he had seen on his visit here during the summer. I told Doral to have Maynard Michel answer the letter.

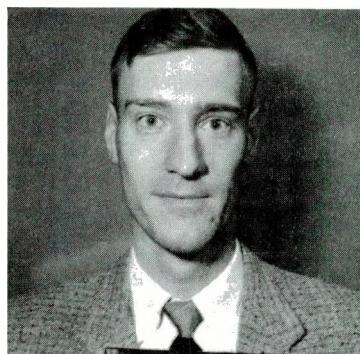
I wrote to Lewis L. Strauss (Chairman, AEC) to tell him that we are very pleased about his decision on the declassification question [re spontaneous fission rates of  $Pu^{240}$  and  $U^{236}$ ]. I suggested lunch or a chat during my visit to Washington on Thursday, September 24, or Friday, September 25, during the meeting of the Boner Panel.

I also wrote to Donald Lane to tell him about my forthcoming visit to Washington and to suggest we confer on the question of claim analysis and notebook notations, if his schedule permits.

A note went to my friend Clayton Sheldon in South Gate to tell him that I am scheduled to arrive in Los Angeles by plane from Chicago very early on Saturday, September 26. I suggested that, after a few hours sleep, I can start a golf game about noon, if his schedule permits.

In the middle of the afternoon Rex Shudde came in to talk with me about his 1A draft classification. I told him that I shall write my usual letter to his draft board (in Los Angeles); I mentioned that we have been quite successful in appeals when we describe the importance of the work.

Later I visited some of the labs and noted that Room 103 of Bldg. 5 is being cleaned out by Health Chemistry--it will be used by Harvey and Choppin for analytical work connected with the Paydirt sample. I also met some of the new graduate students, such as Allan W. Stoner from the University of Indiana and his NCAA championship basketball-playing friend, James F. Schooley.



James Schooley

Wednesday, September 16, 1953

One of my projects lately has been the preparation, with help from the data and calculations of Frank Asaro and Dean Dunlavey, of a short manuscript "Evidence for Subshell at  $Z = 96$ ." This I submitted today to S. A. Goudsmit (Editor, The Physical Review) for publication as a "Letter to the Editor."

I made a number of phone calls and then had a brief appointment at 10:30 a.m. with Donald Hanson about his work on the Whitney Project and his salary status.

Afterwards (10:50 a.m.) I telephoned Rod Spence in Los Alamos. Spence told me that he, Louise Smith, and Charlie Browne had discussed the matter of co-authorship on the initial open publication on elements 99 and 100 (as I offered to him in my call on September 1). Spence said they have decided against the idea on the basis that they feel they haven't contributed enough to deserve it; however, I urged them to reconsider and to discuss the matter with Norris Bradbury--I said that we will not consider the matter closed yet.

After this call, I telephoned Richard Doan, who said he will show up here on Monday morning to discuss the personnel matters he mentioned yesterday and to interview any applicants we have found. He asked that I make reservations for Sunday night for him at the Durant Hotel, and I offered to pick him up at the hotel sometime between 9 and 9:30 Monday morning. I then told Doral to make the hotel reservation and to arrange for a pass to Bldg. 5 for September 21, 22, and 23.

Another call this morning went to Luis Alvarez and, after the usual rounds of the labs, Luis, Al Ghiorso, and I went out to Mira Vista for nine holes of golf (LWA-48, AG-45, GTS-51).

Thursday, September 17, 1953

The research group meeting this morning was attended by Asaro, Batzel, Borg, William Phelan Bryan (a new graduate student from UCLA), Carr, Clark, Cobble, Cunningham, Grover, Gunn, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Levy, Stanley Jerome Lokken (a graduate student from North Dakota Agricultural College), Michel, Darrell Osborne (visiting from Argonne), Perlman, Passell, Reynolds, Ruben, Seaborg, Shudde, Smith, Stoner, Street, Stephens, and Wallmann.



W. Bryan



S. Lokken

Asaro gave a report on his work with Frank Stephens on gamma-gamma coincidence experiments. He described the apparatus, talked about the results, and the advantages and disadvantages of the setup.

Nervik again talked about work on a 2.2-hour rhodium(?) activity produced in the spallation of tantalum with 340 Mev protons. He described an experiment that showed the activity does not grow in from the ruthenium fraction, consistent with the fact that it was not observed in slow neutron fission of uranium. Nervik said the activity has a 1.0 Mev  $\beta$  activity and gamma rays (kev) of  $195 \pm 5$ , 225, 510, 630, 715, 1060, 1200(?), 1260(?), and 1500. From work with the time-of-flight isotope separator, it appears that the 2.2-hour rhodium does not have mass number 107, 108, nor 109.

\* \* \* \*

On the hill I looked over a copy of the general announcement for the California Alumni Day in Los Angeles on September 27, which Cliff Dochterman (Field Secretary, California Alumni Association) sent me. Dochterman mentioned that there would be a reception at the conclusion of the seminars (4:30 until 5 p.m.) at the Ambassador. He also said that Mr. and Mrs. John Mock of Los Angeles have invited all of the guest speakers to their home after the program for cocktails and supper. Dochterman mentioned that Mrs. Mock, a member of the California Alumni Association, asked him to specifically extend me an invitation.

I spent part of the day with Darrell Osborne, who is also visiting Livermore during this trip. Then at 4:30 p.m. I went to a meeting in the Faculty Club.

#### Friday, September 18, 1953

There were the usual number of phone calls this morning, including one to Tom (T. O.) Jones at Haverford College to discuss my visit and talks there next month. I said that my topic for Wednesday night, October 14, will be "Present Status of the Transuranium Elements." I am scheduled to speak at 11 a.m. on Tuesday morning (October 13) to the student body--I will telephone him with the topic for this talk. Jones also wants me to speak at 8 a.m. on Monday and Wednesday to his class in nuclear chemistry. I suggested that one of these lectures be on alpha decay.

Then at noon I attended the pre-game luncheon honoring the Berkeley Football Festival Queens at the Elks Club as a guest of the Berkeley Bear Backers' and the Berkeley Elks Club. It was a most enthusiastic group.

#### Saturday, September 19, 1953

I worked in my study for a while--later I went to Berkeley and saw Baylor defeat Cal by a score of 25 to 0 (I sat with some of the members of the Athletic Department). The captains of the Bears this year are Tom Dutton and Lloyd Talley.

Sunday, September 20, 1953

Helen and I had our annual open house for both Berkeley and Project Whitney people. Pictured are some of the attendees:



Iz Perlman, Margaret and Bernie  
Harvey, Jack Hollander



Walter and Marg Nervik



Lilly Goda, Velma Robinson, Margie  
Hollander, Roberta Garrett, Howard  
Hornig, Bobby Mohler



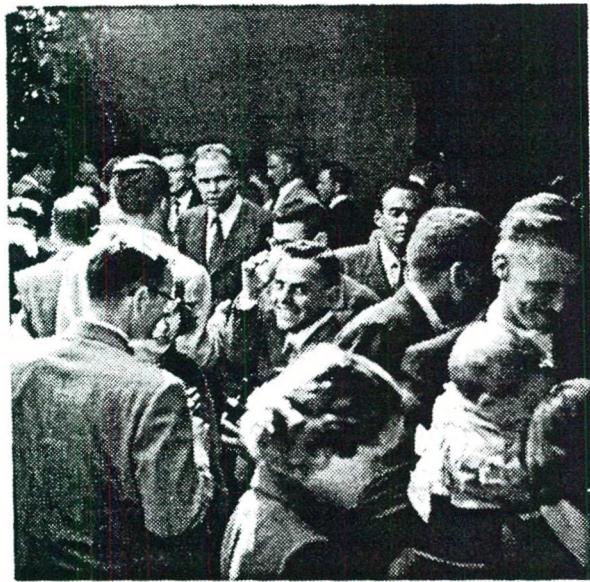
Ann and Stuart Gunn



Stephen and Helen Seaborg



Polly Perlman and Jack Hollander



Glenn Seaborg, Allan Zalkin, Gary Higgins, Louise and John Rasmussen (holding one twin daughter)



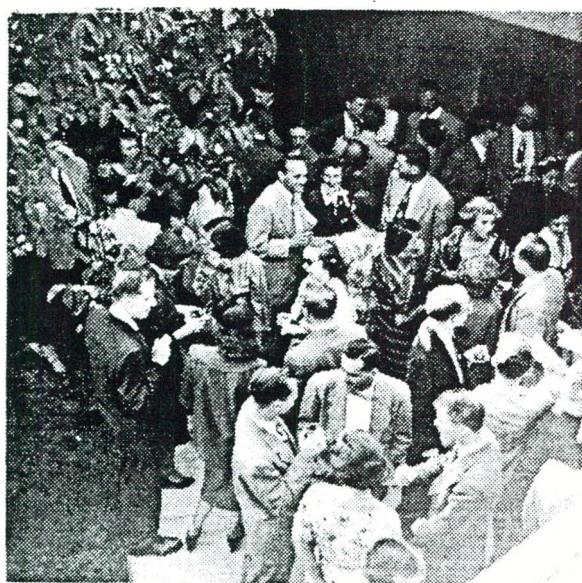
Robert Carr, Peter Gray



Edda Kalkstein, Mary Ellen and  
Richard Glass



Jack Hollander and Marvin  
Kalkstein



Crowd including Roger Batzel, Manny  
Lindner, Natalie Barton, et al.



Edwina Batzel, Margie Hollander  
in Seaborg kitchen

Young David created a bit of a stir in the kitchen by dropping a small garter snake that he had caught in Margie's apron pocket. She promptly took it out and handed it back to him--his explanation to her was that I had told him to do it!

Monday, September 21, 1953

I have made travel and a number of other arrangements for my trip east to attend the Boner Panel meeting on September 24-25. This morning I received a call from Jack A. Gibbs (Deputy Chief, AFOAT-1, Office for Atomic Energy), saying that due to the urgency of the current workload, the meeting is being postponed. This necessitated cancellation of my reservations and a number of letters--to Donald Lane, saying that it now appears I shall be in Washington during the day on October 15; to Hoylande D. Young, telling her that I shall be unable to attend the dinner in Chicago on the occasion of the award of the Gibbs Medal to Joel Hildebrand because of the meeting postponement; a similar letter went to Marvin C. Rogers (Chairman, Chicago Section, who invited me on August 28 to attend the Gibbs Medal ceremony); and finally I wrote to Joel Hildebrand (c/o Marvin C. Rogers). This latter letter read, "I am very sorry that circumstances make it impossible for me to be with you at the Willard Gibbs dinner as I had planned. May I extend to you my heartiest congratulations for this highly deserved honor. I think that the Chicago Section should also be congratulated on the excellence of their choice, for yours is a name well worth adding to the list of distinguished recipients."

Richard Doan (Idaho Falls) is visiting to interview possible personnel for their group; I talked with him for a while and also invited him to come out to the house to have dinner with Helen, the kids, and me.

Later, after a conversation with Iz, I sent a memo to E. E. Coleman in the laboratory business office, requesting that he make every effort to expedite the placement for resale in the student store by Thursday, September 24, of 50 copies of the "Table of Isotopes." We wish these to be available to students taking Chemistry 123.

In today's mail was a note from Robert E. Robinson (Santa Barbara), thanking me for accepting their invitation and saying that he is noting Wednesday, January 13, 1954 as the day on which I will speak there.

The senior staff met with me for lunch in my office at noon. A number of items were discussed--I agreed to talk with Wally Reynolds about some changes in the scheduling of the 60-inch cyclotron (after Berni Rossi, Joe Hamilton, and Al Ghiorso have a meeting to decide what they want). I also need to make arrangements for salary changes for Ed Orlemann and Donald Hanson (retroactive to September 1). It was also suggested, and I concurred, that two men be borrowed from Eugene Huffman's analytical group for Project Whitney work.

About 2:15 this afternoon I received a call from John Mackenzie, Jr. (Chairman Strauss' assistant), informing me that the Defense Department must formally concur on the declassification of the spontaneous fission rates of  $Pu^{240}$  and  $U^{236}$ , and then United Kingdom and Canada. I mentioned that I shall be in Washington on Thursday, October 15; Mackenzie said

that Strauss' calendar is clear for that date.

Tuesday, September 22, 1953

Much of my morning was spent on campus, where I talked with colleagues in the Department of Chemistry office, kept office hours, and gave the introductory lecture to the Chemistry 123 class. Members of the class this semester are Philip Anderson, Tom Blake, Gilbert Brink (auditor), William Bryan, David Buck, Kenneth Chapman, Alfred Chetham-Strode, Gregory Choppin (auditor), Ray Collett, Clifford Colvin, Robert Du Bois, Lois Durham, Eugene Evans, Mitchell Florence, Bruce Foreman, Arnold Fritsch, Stephen Kahn (auditor), James Kliegel, Mary La Salle, Zit-Zei Leung, Stanley Lokken, John Maier, Richard Marsh, Mehdi Mehr-Ali, Roger Millikan, Mitsugi Mukai, M. Papadopoulos, Russell Parsons, Dennis Patton, Richard Paulson, John Ragle, Robert Rinehart, Robert Scherrer, James Schooley, Willie Smith, Simon Soloman, Robert Spenger, Martin Steinberg, Paula Stiling, Allan Stoner, Donald Strominger, Michael Sweeney, John Worthington, and Ernest Zeller.

Yesterday I received a letter from Jean Elliott with details about the Alumni Day in Los Angeles on September 27. Miss Elliott wrote that Mr. Shirley E. Meserve, a prominent Los Angeles lawyer and one of their most loyal alumni, will be chairman of my session on athletics; she asked that I meet him about 10:30 a.m. Sunday in the lobby of the Ambassador Hotel. I sent her a brief note today, agreeing to the arrangements and giving her my parents' phone number.

I also spoke with Ken Street today about the possibility of his using Dick Glass on Project Whitney for a while.

Wednesday, September 23, 1953

I finally talked with Wally Reynolds this morning about some of the things brought up at Monday's staff meeting; then I answered some of my accumulated correspondence.

Miles J. Martin (Manager, Research Publication Services Section, General Electric, Schenectady) wrote on September 16 to ask for an evaluation of James H. Stack, explaining that they are considering Stack as Supervisor of Research Publicity. I replied in part, "I can recommend Mr. Stack with great enthusiasm and without any reservations whatsoever..."

Eugene L. Church (Argonne), in a letter dated September 16, asked about a reference in the "Table of Isotopes" to a paper to be published in the Journal of the American Chemical Society by R. A. Glass, S. G. Thompson, and me concerning an estimate of QEC of  $\text{Am}^{242m}$ . I responded that the article has not yet been prepared for publication but that I am enclosing a copy of a preliminary write-up, which will serve as the basis for this article when it is eventually published. In addition, I explained that the decay scheme of  $\text{Am}^{242m}$  is still under investigation and has not been completely worked out; however, I am asking our Information Division to send him a copy of the thesis by Richard W. Hoff, who summarizes the latest work.

I responded to a September 15 letter from Earl T. McBee (Purdue University) about my talks at Purdue in October. McBee mentioned that the weekend of October 16 is Homecoming and that my talk is the major attraction on Friday evening. He offered me some flight information. I wrote that I believe a good title for my talk before the Chemistry Department there on October 16 would be "The Actinide Elements" and for the Sigma Xi talk in the Purdue Hall of Music, "The Present Status of the Transuranium Elements." I told McBee that I shall try to make that talk quite general. In addition, I gave him a slight change in my traveling plans.

A note went to Ralph James at UCLA, who had pointed out on September 17 that he had not received any reprints of the article "L-Electron Capture and Alpha-Decay in  $\text{Np}^{235}$ ." I told Ralph that we are sending him 10 copies although our records show that a batch of these reprints had been mailed previously but apparently were somehow lost. I also mentioned that I will be in Los Angeles this weekend to speak on the athletic situation at the California Alumni Day on Saturday at the Ambassador Hotel; however, my schedule will not permit a visit at UCLA.

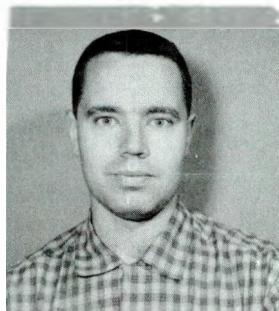
I also wrote to thank Donald McPherson (Row, Peterson & Company) for the set of science unitexts he sent us. [I met with McPherson during the ACS meetings in Chicago, during which time we talked about a possible consulting-type editing position.] In my letter I said that I shall look forward to seeing him in California and discussing further the matter which we explored at our pleasant breakfast talk in Chicago.

Later I visited some of the labs, particularly to talk with some of the new graduate students.

I also met with Doyle Northrup of the DOD long-range detection staff in Washington. We discussed the evidence on the recent (first) Soviet thermonuclear explosion that took place on August 12 (200-300 kilotons).

Thursday, September 24, 1953

The following people attended the meeting of the research group this morning: Asaro, Bryan, Carr, Clark, Cobble, Cunningham, Glass, Gray, Grover, Hoff, Jaffe, Herbert R. Johnston (an electrical engineering graduate student working with John R. Woodyard), Kalkstein, La Salle, John LeRoy Maier (new graduate student from Michigan School of Mining and Technology; recently an assistant chemist at Argonne), Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Reynolds, Seaborg, Schooley, Shudde, Slater, Smith, Stephens, Stoner, Surls, Michael Patrick Sweeney (an Air Force man from Loyola University in Los Angeles, who will do graduate research with our group), and Templeton.



Michael Sweeney



Herbert Johnston



John Maier

Jaffe reported on his work on the gamma spectrum of  $\text{Ir}^{192}$ , which Cork had studied a couple of years ago with a permanent magnet spectrograph and had found it to have electron-capture branching and 17 gamma rays; Cork proposed a decay scheme. Jaffe said that he obtained a sample of  $\text{Ir}^{192}$  from Oak Ridge and studied it with the bent crystal spectrometer and the 50-channel gamma analyzer. In spite of poor resolution Jaffe used the bent crystal spectrometer to estimate that  $\text{Pt:Os}$  K x-rays = 2:1. Combining this with the absolute beta count on the 50-channel gamma analyzer, he obtained, correcting for fluorescent yield: total K x-ray/ $\beta$  = 0.14; platinum K x-ray/ $\beta$  = ~0.09; osmium K x-ray/ $\beta$  = ~0.05. Then, using Cork's relative intensities and his total platinum x-ray count, he found that 1 Cork intensity unit =  $2.5 \times 10^{-3}$  conversion electrons/ $\beta^-$ ; correcting the K x-rays for contribution from conversion, he obtained K capture/ $\beta^-$  = 0.02. Jaffe went over his other calculations, the results of which indicated for the more intense gamma rays, E-2 transitions. From his work he concluded that Cork's EC decay scheme should be inverted. Perlman explained that the original purpose of the experiment was just calibration of the instrument, and Jaffe mentioned other nuclides that he plans to examine.

Passell described some work he has done on the decay of  $\text{Cm}^{242}$ , saying they previously reported a  $44.1 \pm 0.5$  kev gamma ray that is ~100% converted and a 102 kev gamma ray that is largely converted. He said that in order to see the L peaks of the 102 kev gamma ray (~70 kev), so much activity is needed that there is a large background from low energy electrons being directionally scattered into the higher energy beam. They have observed an L<sub>III</sub> peak corresponding to a 102 kev gamma ray and an L<sub>I</sub> + L<sub>II</sub> peak corresponding to a 102.1 kev gamma ray, confirming the assignment. The transition is E2, but abundances are uncertain. In the discussion it was pointed out that spontaneous fission is negligible.

Reynolds gave a review of the various mass spectrometers that our group has and their characteristics.

\* \* \* \* \*

In Chemistry 123 I presented an historical background of the subject, and then stopped in the Department office before going up to the hill.

Between 5 p.m. and 6:30 p.m. I met with Don Cooksey to go over a large range of current activities involving our work at the Radiation Laboratory and also the Whitney Project.

Friday, September 25, 1952

This morning I completed an evaluation of John Huizenga, who is applying for a Fulbright grant. I said in part, "I would rank him as one of the most outstanding young men in the country in the nuclear chemistry field of research. There is no question that he would profit from his proposed visit to the Netherlands, that he would do valuable work on his proposed problem, and that he would be a worthy representative of his scientific colleagues in the United States..."

Earl prepared an evaluation of Thomas E. Hicks, who has applied for a position with National Carbon Research Laboratories in Cleveland, for me to sign. I described work that Hicks has done under my administrative direction and his responsible position for California Research and Development Company. I concluded with, "I am confident that he would prove a worthy addition to your staff."

Much of the day was spent catching up on my report and journal reading and talking with the new students.

A lab driver picked me up at home at 7:30 p.m., drove me to the San Francisco Airport, where I caught United Flight 678 to Los Angeles. I arrived in Los Angeles at 10:40 p.m. and took a cab to my parents' home in South Gate.

Saturday, September 26, 1953

In South Gate. After spending the morning with my parents and Jeanette, I got together with Clayton Sheldon for some golf.

[Cal defeated Oregon State, 26 to 0, in a game played in Corvallis today.]

Sunday, September 27, 1953

In South Gate. Jeanette and I went to the Ambassador Hotel in time for me to meet Mr. Shirley E. Meserve in the lobby at about 10:30 a.m. Alumni had the choice of two informal seminars from 11 a.m. until noon, with the first entitled "Taking the 'Wonders' Out of the Wonder Drugs." Meserve, chairman of my seminar "The Pacific Coast Conference Athletic Situation," introduced me. An outline of my talk follows:

## PACIFIC COAST CONFERENCE ATHLETIC SITUATION

by Glenn T. Seaborg  
Faculty Athletic Representative  
University of California, Berkeley, California

### I. Organization of the Pacific Coast Intercollegiate Athletic Conference (PCIAC or "PCC").

#### A. Members

UC  
UCLA  
Idaho  
Oregon  
Oregon State College  
USC  
Stanford  
Washington  
Washington State

#### B. Representation in the Conference

1. The faculty representatives shall have jurisdiction in all matters touching the purposes of the Conference.

My freshman year as faculty representative.

PCIAC meetings twice a year (NCAA meets once a year).

2. Director's Association takes care of the business and managerial matters governed by the Conference and connected with the holding of athletic contests, i.e., make schedules. Also to make recommendations to the faculty representative.

Cal's director -- Brutus Hamilton.

A leading figure in the world of sports. Coach of the 1952 U.S. Olympic track team, this year's coach of the U.S. team participating in the Maccabiah World Games, voted Missouri's all-time athlete according to Helm's Foundation, in addition, a fine person and excellent example of good sportsmanship.

Assistant Director -- George Briggs.

C. Purpose

1. To govern all branches of athletic sports in which a student represents a member of this Conference in competition with any other institution, organization, club or team.

Means that non-Conference opponents in the Pacific Coast area must conduct their athletic programs in compliance with PCC rules or else they must be part of a Conference whose rules are acceptable to the PCC.

2. To establish and maintain high standards of scholarship and sportsmanship in the institutions which are members of the Conference, and in those coming under its influence; to promote intercollegiate athletics; to maintain such regulations and supervision of athletic sports governed by it as to keep athletic activities the incidental and not principal feature of university and intercollegiate life.
3. To encourage any system of intramural athletics which has for its object the regular participation of all students in some form of healthful sport.

II. Over-all Athletic Activity at California

Last year, the university had its biggest sports year of all time. Over one million people watched nearly 500 athletic contests involving 37 teams in 22 sports -- the greatest intercollegiate athletic program west of the Mississippi. About one thousand students participated, and 554 of them won Big or Circle "C's". Three thousand more participated in twenty intramural sports. In addition to fielding teams in the major sports of football, baseball, basketball, crew, track, tennis and swimming, there were also teams in such varied sports as cross country, fencing, golf, gymnastics, handball, rifling, rugby, sailing, soccer, skiing, volleyball, waterpolo, weight lifting, wrestling and boxing.

III. Educational Aspect

As is readily evident, California is doing its share in the promotion of intercollegiate athletics. However, we are also mindful of another purpose of the PCC, which is to keep athletic activities as an incidental and not principal feature of university and intercollegiate life.

Conference rules:

A. Academic Progress

Quantitative Rules

1. Ten-unit rule. Must have passed ten units in previous semester.
2. Twelve-unit rule. Must be carrying at least twelve hours at all times during period of competition.

3. Twenty-four unit rule. Must have earned twenty-four units since previous season.
4. One-fifth rule. Must pass five times as much as failed on cumulative record.

Qualitative Rule

Consideration of adoption by PCC of a "C" average requirement for eligibility for competition.

California athletes' scholastic record and record after graduation.

- a. Percent of athletes falling below "C" average (only 12.7%) is only about half of the percent of all freshman students. Larger percent of athletes in "C" to "B" group than for non-athletes, however not as high a percentage of athletes with better than "B" average.
- b. California and Stanford have lowest percentage of athletes in PCC who do not finish for their degree (16.6 and 16.8, respectively). Cal with 8.3% has smallest proportion quitting after completion of athletic eligibility.
- c. Differential in grade point average of athletes and those of all males is only -0.11.
- d. Recent athletes with higher than "B" average.

Bob Karpe  
Don Robison  
Bob Cummings  
Ed Hart  
Bob Minahen  
Carl Van Heuit  
George Witter  
John Ralston

e. Successful athletic alumni:

Dr. Matt Hazeltine, M.D., '16. Rugby, football.  
Physician and surgeon. Member of Alumni Council.  
Son is present center of team.

Cort Majors, '21. Captain of "Wonder Team".  
V.P. in charge of sales for Fibreboard Products.  
V.P. Calif. Alumni Association.

Dr. Harold P. "Brick" Muller, M.D., '24.  
Orthopedic surgeon, assistant team physician.

Dr. Brodie Stephens, M.D., '24. "Muller to Stephens". Distinguished brain surgeon in San Francisco.

Benny Lom, '30. Famous halfback. Very successful in merchandising field.

Eugene McAteer, '37. Varsity football captain. Tarantino's Restaurant, attorney, member Board of Supervisors, San Francisco City and County.

Stanley E. McCaffrey, '38. Varsity football. Executive Manager, California Alumni Association.

Vic Bottari, '39. All-American, member Alumni Council, member Berkeley Board of Education, partner in own insurance firm.

James Corley, '26. Varsity track. V.P. - Business Affairs, U. of C.

Maynard J. Toll, '27. Varsity crew. Distinguished attorney, past president Cal. Alum. Assoc., Los Angeles civic leader.

Kent Pursel, '25. Basketball. Pharmacist, Alameda Supervisor.

Chester G. Carlisle, '38. All-American basketball. Superintendent of Schools, Marin County.

And many others.

B. Time spent on athletics.

For average athletic student - per week:

Approximately 15 hours of classes  
30 hours of study  
10 hours practice - not including games  
12-15 hours work  
Approximately 70 hours.

New rules formulated with such a "work" week for athletes in mind and to give athletes more time for activities other than athletics (an athlete may make up to \$75.00 per month, but he should do the work to earn it.)

Major revisions:

1. Varsity practice in football and basketball shall be limited to 10 hours per week.

2. Limitations of playing season.

a. Football

Five weeks and two games for freshmen.

Effective in 1956, regular season to start on last Friday night or Saturday in September (presently next to last Friday night or Saturday), limit of ten varsity games, practice season to start eighteen days before first game.

b. Basketball

Opening practice date - November 1. Closing date, last Conference game, except for NCAA tournament Dec. 1 or Nov. 30 as first competition, exclusive of playoffs and NCAA tournament, only 27 varsity games, 27 J.V. games, 10 freshmen.

c. Baseball

30 varsity, 30 J.V., 16 freshmen games.

3. Spring practice.

Eliminated in basketball.

To be eliminated in football, effective 1956. Already no spring practice in Ivy league.

It is hope of PCC that the Big Ten and other conferences will follow suit.

IV. Conference Balance.

The situation within the conference is a very healthy one. In recent years the tendency has been toward a greater balance throughout the Conference. This was never more evident than in last year's competition for the various Conference and Divisional championships. The winner of the Conference football crown was not decided until the last day of the season. In baseball, Stanford and USC wound up the regular season in a tie for the Southern Division title, with Cal just a few games back. The Southern Division basketball race really was a race until the last weeks of the season, with three teams fighting it out most of the way. In tennis, the Southern Division had the three top teams in the nation.

This year promises a repetition of many of these races. UCLA and USC are favored to fight it out again for Conference honors, though California still cannot be counted out and should become a more important factor as the reason progresses. All three schools will have pretty much the same basketball teams as last year and there will be a large number of lettermen turning out for baseball and tennis.

Further action was taken at the June meeting of the Conference, towards furthering this balance within the Conference.

A. Round-Robin schedule, effective 1956 (Idaho excepted).

B. Television

Tie-in with NCAA program.

1. Member institutions may not televise by live television any home games during the 1953 football season except the following:

- a. Such games as are approved by the NCAA as part of the NCAA so-called network TV program.
- b. Such "sell-out" games as may be permitted under the NCAA TV plan.
- c. Such night games as may be permitted under the NCAA TV plan.

2. TV receipts. (For 1953 season only.)

Competing member institution - 20%  
Of remaining 80%:

Idaho -	5%
Other eight members -	11.875%

C. Rose Bowl.

No team may appear in two consecutive years. In case of tie, team that has not played in a Rose Bowl game for longest time would be favored.

Division of Rose Bowl receipts:

Idaho	5%
Other eight members	11.875%

V. Conference standing nationally.

General discussion of PCC accomplishments in the national sports picture.

V. Conference Standing Nationally:

Neither California's nor the Pacific Coast Conference's position in the national sports picture is any the worse for their operation within the rules of the Athletic Code.

For about the past six years, California has been rated among the top teams in the country. More recently UCLA and USC have also been accorded top ratings. Recent All-American teams have included such conference stars as Rod Franz, Jackie Jensen, Les Richter, and John Olszewski of California; Donn Moomaw and Paul Cameron of UCLA; Norm van Brocklin of Oregon; Paul Cleary, Pat Cannamela, Elmer Wilhoite and Jim Sears of USC; Bill McColl of Stanford; and Don Heinrich and Hugh McElhenny of Washington.

Washington and UCLA have had the PCC's most successful basketball teams in the last few years and as of last year, California is now moving in to join them. Such stars as Chuck Hangar, Andy Wolfe, Bill Hagler, and Bob Matheny of California; Don Barksdale and George Stanich of UCLA; Bill Sharman of USC; Jack Nichols and Bob Houbregs of Washington were just some of the Pacific Coast Conference basketball players to win All-American honors lately.

In track, USC has won the last twelve NCAA meets, taking five first places in 1953. California's John Roseme won NCAA champion in the javelin in 1952. With just a three--man team of George, Spurrier and Adams, California took fourth place in the 1952 IC4A competition with George beating Remigino (who later won the Olympics) in the 100. On the last Olympic team the Pacific Coast Conference was well represented by such

former and present stars as Cy Young (UCLA), Bud Held (Stanford), Sim Iness (USC), Perry O'Brien (USC), George Brown (UCLA), Jack Davis (USC), Price King (Cal - walker), and Bob Mathias (Stanford). A very large number of World, Olympic, and American records were set by PCC stars. One of the oldest still on the books is the Collegiate One Mile record of 3 min. 9.4 sec. set in 1941 by the California team of Froom, Reese, Barnes, and Klemmer.

Between California and Washington the Pacific Coast Conference has what many experts would rate as the two most prolific producers of championship crews. A look at the Olympic records will show California with three championships and Washington with one. In addition, California has won the Intercollegiate Rowing Association Championship six times and Washington has won nine times.

The Pacific Coast Conference has established a virtual monopoly on the NCAA tennis championship with UCLA winning in 1950, 1952, 1953 and USC in 1951. Last year the top three places went to UCLA, USC, and Cal. Recent singles champions have been Herb Flam, UCLA, 1950 and Hugh Stewart, USC, in 1952. In doubles, championships have been won by Jim Brink and Fred Fisher, Washington in 1949; Herb Flam and Gene Garrett, UCLA in 1950; Earl Cochell and Hugh Stewart, USC in 1951; Cliff Mayne and Hugh Ditzler, UC in 1952; and Bob Perry and Larry Heubner, UCLA in 1953. In addition, Cal took the team, singles and doubles championships in this year's Eastern Intercollegiate tournament.

The Pacific Coast Conference has also been well represented in baseball, swimming, golf and most of the other intercollegiate athletics.

VI. Outline of Closing Remarks.

Sports are supposed to help develop the character of those who participate and also of those who have an interest in them. When an athletic contest becomes a fight for survival, where winning at any cost becomes the primary objective, more harm than good is done. The spirit of clean and friendly competition is forgotten, and the only character traits liable to be developed are not very desirable ones. It is to be remembered that the athletic program of a university is intended to contribute to a fuller and more rounded life for its participants and its community. The student is in school primarily to get an education. If properly conducted, athletics is part of that education. It is not the principal objective of college life, just as it is not in later life.

Len Casanova (Oregon football coach):

"One thing I think we should all remember about college or high school football -- it must be an avocation, not a vocation, for the players. The most important job for a young man in school is to get an education which will prepare him for a career of his choosing. Football can help. It cannot do the complete job."

The maintenance of a high level of sportsmanship is a primary concern of the Conference. Wherever possible measures have been adopted to ensure this. The Southwest Conference has successfully initiated and used a plan by which the reports of the officials which list the number and types of fouls committed in football, would also list the number and name of the players committing any of the fouls (clipping, piling on, roughing kicker, illegal use of hands, unnecessary roughness, holding, unsportsmanlike

conduct). A chart is kept of all players committing such fouls, and whenever one man accumulates three fouls of this type his coach is notified. It is planned to inaugurate a similar system in the PCC this season in which any player guilty of a personal foul of the type requiring that he be reported to the Commissioner's office by name be immediately and clearly identified by the official who calls the foul. This procedure should make the officials, coaches, and players more conscious of the serious type of foul and also more aware of their responsibility to prevent it.

The responsibility for good sportsmanship does not end with the officials and teams. It is the responsibility of everyone in the university community. An unsportsmanlike act by any one of us reflects on the university as a whole. As many institutional violations of the athletic code are due to the actions of alumni and other members of the university community as are due to the teams and athletes that represent them. Just as the athletic code provides a set of rules for the activity of the athletes, it also contains rules for what an alumnus can and cannot do to contribute to the athletic program of his university. It might be worth mentioning them here.

1. Entertainment of high school and junior college athletes is forbidden unless the groups entertained are chosen on the basis of some interest other than athletics and provided, in the case of such entertainment, that the occasion and character of the entertainment has been first approved in writing by the faculty representative of the member institution. (Includes meals, banquets, outings, weekend

excursions, complimentary tickets, and similar means of influencing prospective students.) The furnishing of transportation to the campus of a member institution directly by alumni shall not be considered as entertainment.

2. A student or prospective student of any member institution who, except as provided in the Code, has been the beneficiary of an inducement of financial value to attend that institution, or who by cooperation on his part becomes the prospective beneficiary of any such inducement, shall be ineligible for athletic competition at any member institution.

California's record with regard to violations of the athletic code is as clean as any university's. It is up to all of us to see that it is kept that way.

It should be remembered that sports are supposed to be played for the fun of it. There have to be as many losers as there are winners and it is more important and better to be good losers rather than unsportsmanlike winners. Perhaps one of the most encouraging observations on the Baylor game was the one made by Wayne Hopkins, one of Baylor's star ends: "Cal was a good team in defeat -- they talked real fine to us after the game -- they're a fine bunch of guys."

Speakers at the luncheon (12:15 p.m. until 2:15 p.m.), which was held in the Embassy Room, were Dr. Raymond B. Allen (Chancellor, UCLA), Dr. Clark Kerr (Chancellor, UCB), and President Robert Gordon Sproul. This was followed by two sessions of informal seminars: 2:30 p.m. until 3:20 p.m. ["Across the Space Frontier" by Joseph W. Kaplan or "What's Being Done About Juvenile Delinquency?" by Milton Chernin] and 3:30 p.m. until 4:20 p.m. ["The Fabulous World of the Sea" by Roger Revelle or "Your Money and the New Administration" by Frank L. Kidner]. At 4:30 p.m. there was an informal reception for the speakers.

Later I was driven from the Ambassador Hotel to the home of Mr. and Mrs. John Mock (10807 Bellagio Road, Bel Air) for cocktails and supper. Afterwards I was taken to the airport in time to catch the 9:30 p.m. United Flight 667 to San Francisco. The flight arrived at 11:10 p.m., and I was met and driven home.

[Today the 49ers defeated Philadelphia by a score of 31 to 21 in Kezar Stadium.]

Monday, September 28, 1953

This morning I telephoned T. O. Jones to make final arrangements for my talks at Haverford College. We agreed that on Tuesday morning, October 13, I will speak to the Student Body on some reminiscences about the early work on atomic energy, including something about the shortage of scientists. The second lecture to his class (or possibly the first) will be on modes of radioactive decay. I will speak on nuclear reactions to the physics class at 9 a.m. on Monday. Other things on my schedule will be a couple of lunches and dinner Monday night with students. They will give me a tour of the area on Tuesday afternoon. Jones gave me his home phone number so that I can call him when I get in.

After a few other phone calls and conversations, I dictated a couple of letters. Chairman Lewis L. Strauss wrote on September 17 that he was going to take the Atomic Energy Commission out of town when I expected to be there (September 24). Today I wrote that the Boner Panel meeting had been postponed; in response to his query about what the Boner Panel is, I wrote that this is the Panel on Technical Intelligence Operations of the USAF Atomic Energy Detection Program on which Dr. C. P. Boner of the University of Texas serves as chairman. I mentioned to Strauss that I am coming East next month to fill some very long-standing lecture engagements and plan to come through Washington on Thursday, October 15, en route from Haverford College to Purdue University.

Z. D. Welch (Secretary, Purdue Chapter of Sigma Xi) requested a publicity photograph of me in connection with my talk there in October; I mailed this to him today.

Cyrus S. Fleck (Assistant Business Manager, Journal of Chemical Education) asked me in a letter dated September 24 about BAETA. Today I responded with:

In reply to your letter of September 24, 1953, the Bay Area Educational Television Association is a bona fide organization which was formed to coordinate the efforts of various schools and colleges

in the Bay Area in presenting interesting and worthwhile programs on Channel 9, Station KQED, which has been reserved for non-commercial educational use and which is expected to begin operations in December, 1953. Each college in the Bay Area, as well as the Superintendent of Schools in the various counties involved, is represented in the Association. The University of California is taking an active part in BAETA by donating studio space and equipment.

Plans are being made by various colleges to offer courses for credit, as well as science programs, lectures, etc. It is indeed a praiseworthy undertaking and one the Bay Area can be proud of. Mr. Rice has been doing a fine job as Program Manager.

I have prepared a script outline on the discovery and history of the chemical elements and Mr. Rice is preparing some of the visual material for this program, on which I plan to participate.

In today's mail was a nice thank-your note from Ambassador Heinz L. Krekeler (Federal Republic of Germany) for the tour of the lab I gave him recently. I also received a thank-you note from John P. Howe (North American Aviation, Inc., Downey, California) for Helen's and my hospitality during his visit. John suggested I spend a half-day consulting with them toward the end of October.

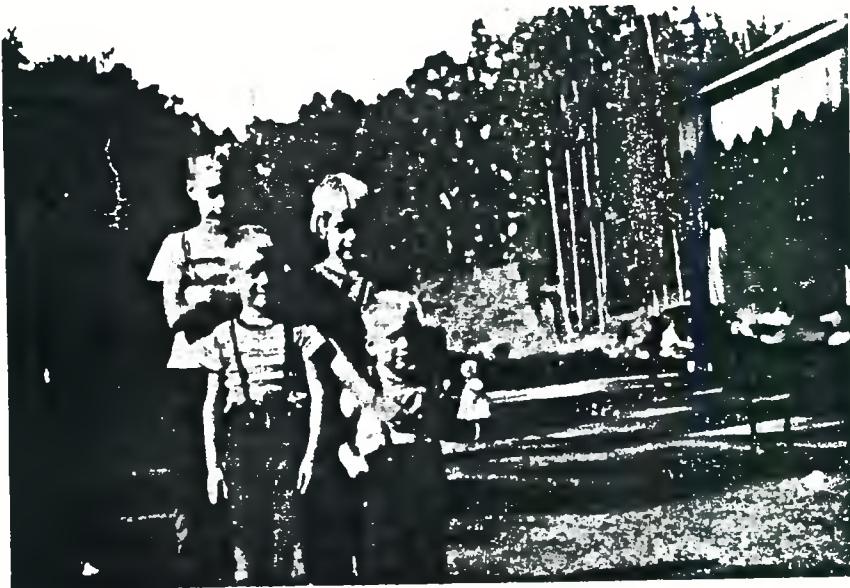
The senior staff had its usual noontime luncheon meeting.

Tuesday, September 29, 1953

I spent most of the morning on campus, including giving the lecture to the Chemistry 123 class. Today I began discussing fundamental nuclear concepts.

Back on the hill in the afternoon I visited some of the labs and talked with the researchers about their work.

At home Helen showed me a Kodacolor print of the kids at the Tilden Park merry-go-round; this was taken by Helen's friend Esther Bacon when she visited Helen earlier in the month.



Peter, Dave, Lynne, and Stephen Seaborg, September 1953

Wednesday, September 30, 1953

I made a few phone calls this morning and then went over my slides, etc. for a talk this evening. Later I visited some of the labs to talk with the students and look over the research.

I went down to campus in time to attend the Physics Department meeting at 4 p.m. in Room 375, Le Conte Hall. Then, after a 6:30 p.m. dinner in the Faculty Club, I spoke at 8 p.m. in Lewis Hall on "Present Status of the Transuranium Elements" to SAACS (Student Affiliates of the American Chemical Society).

Thursday, October 1, 1953

Present at today's "open" meeting of our research group were Ingmar Bergström (visiting from the Nobel Institute in Stockholm), Bryan, Carr, Chetham-Strode, Clark, Cobble, Cunningham, Gray, Grover, Jaffe, Herbert Johnston, Kalkstein, Koch, La Salle, Lokken, Maier, Mathur, Michel, Nervik, Passell, Rasmussen, Reynolds, Ruben, Schooley, Seaborg, Shudde, Slater, Smith, Stephens, Stoner, Sweeney, Templeton, and Wallmann.

Johnston gave a lengthy talk about beta-ray spectrometers, noting that there are two classes--the magnetic and the electrostatic field types. He went into some detail about the one on which he is working, an electrostatic field type with two retarding fields.

Bergström noted that separated isotopes from Oak Ridge are not very pure and contain considerable carrier. Since this is a disadvantage in beta-ray spectroscopy, they have avoided the problem by electromagnetic separation of the radioactive isotopes. He described their setup.

Mathur talked about his study of the nuclear isomerism of  $\text{Nb}^{89}$ , produced by 100 Mev helium ions on  $\text{Y}^{89}$ . By milking experiments he found the half-life of  $\text{Nb}^{87}$  to be 45 minutes and that of  $\text{Nb}^{89}$  to be 4 hours. Mathur said they ran the niobium fraction on a 50-channel gamma pulse analyzer; the presence of annihilation radiation decaying into a half-life of ~50 minutes, tailing into 4 hours, and the 15-hour activity, showed that both  $\text{Nb}^{87}$  and  $\text{Nb}^{89}$  are positron emitters. He presented a postulated decay scheme for  $\text{Nb}^{89}$  and  $\text{Zr}^{89}$ , but preliminary attempts to isolate a postulated niobium isomer ( $\text{Nb}^{89m}$ ) have been unsuccessful.

\* \* \* \*

In Chemistry 123 I continued lecturing on fundamental nuclear concepts. I stopped in the Department office before going up to the hill.

In my office I made a few phone calls, including one to Miles Hudson (Rugby Coach) about an eligibility problem, and then looked over my mail.

A nice note arrived from Stan McCaffrey about my talk at the Los Angeles Alumni Day last Sunday. Stan wrote, "Your discussion concerning athletics was of the greatest of interest to the many alumni present and your review of the situation was comprehensive and pertinent..."

I received a tentative schedule for my talks at Haverford College from Tom Jones.

I also received a note from Donald McPherson (Row, Peterson & Company), who said that he will be in Berkeley November 5 to 9 inclusive; however, if I will not be around at that time, there is a possibility that he can make the trip a week later. I immediately replied that I think the dates will be all right.

My old friend Paul Aebersold, now at Oak Ridge, requested some pictures in a letter dated September 27, and these were mailed to him today.

I declined an invitation from Michael Kasha (The Florida State University) to speak to their Sigma Xi group this academic year, pleading an impossibly full schedule.

In a conversation with Ingmar Bergström I learned that my friend Lars Melander will soon come to the United States to lecture at Notre Dame. I wrote to Melander in Stockholm and said that I was pleased to find out that he is coming to the United States this fall to give the Reilly Lectures and that I hope he will be able to visit us in California and have dinner with Mrs. Seaborg and me.

Ken Hulet and Stuart Gunn, two of our graduate students who have completed work for their Ph.D.'s, transferred to the Project Whitney payroll today.

Friday, October 2, 1953

After the usual phone calls, including one to George Briggs about football tickets for Rix Snyder, I answered some of the correspondence that has accumulated on my desk.

Dan (Daniel R.) Miller (Division of Research, AEC) wrote on September 30 to ask my opinion about a research proposal submitted by Wayne Meinke (University of Michigan) for support to bring up to date and expand his compilation "Chemical Procedures Used in Bombardment Work at Berkeley." I wrote Miller that I enthusiastically support his proposal, saying that the material would be very useful to nuclear chemistry research in general and the procedures would certainly be very much used in many of the AEC laboratories. I added that we found Meinke had a knack for getting things done and that he probably covered more ground than any graduate student whom we have ever had. I wrote, "All in all, it seems to me that it would be difficult for the AEC to get more for its money than it will receive on a project like this, and therefore, that the project is very well worth supporting." I then wrote to Wayne and said that it has occurred to me that it might be worthwhile to publish his material in book form in a few years. I wrote that, if he thinks this is feasible, I should appreciate it if he made no commitments until we have had a chance to discuss it further.

Sol Silverman, an attorney in San Francisco, wrote on September 25 to invite Helen and me to a cocktail party in his home (980 Baileyana Road, Hillsborough) from 2:00 to 5:00 p.m. on Sunday afternoon, October 18, to welcome home Brutus Hamilton from the World Maccabiah Games in Israel. I wrote Silverman that Mrs. Seaborg and I shall try to be there, but I cannot be certain whether it will be possible for I will be returning from a long trip East on either October 17 or 18.

In response to his September 30 request, I sent some publicity material and a photograph to Tom Jones (Haverford College) for my talks there beginning on October 12.

I also wrote to Tom Johnson (Thomas H.) (Director, Division of Research, AEC) to tell him that I am planning to be in Washington on Thursday, October 15, and would like to drop in to chat with him and some of the other people in his Division if he will be available at that time.

In today's mail was a memo from Alden H. Miller, requesting that I speak before the Sigma Xi Society on campus this year. I telephoned Miller and agreed to speak on November 11 on "Present Status of Transuranium Elements."

Also today I received a thank-you note from C. A. VanderWerf of the University of Kansas (Division of Chemical Education) for my talk at their symposium during the Chicago ACS meetings in September.

I visited a number of the labs and talked with some of the graduate students about their research during the afternoon; then, in the late afternoon, I went over to San Francisco and the Palace Hotel to go to a cocktail party in the Italian Room.

Saturday, October 3, 1953

Although I played with the kids for a while this morning, I later went to Memorial Stadium, where I had lunch in the Concessionaire's Room (located right from North Tunnel, two sections beyond the dressing room) with some of the Berkeley people and guests. This pre-game lunch of Southern fried chicken, sandwiches, fruit, etc. is regularly served by Mary Swafford of the Swafford Co., concessionaires for Memorial Stadium, to such people as Greg Engelhard, Miles Hudson, Stan McCaffrey, Joan Hauser (tickets), and members of the visiting athletic department (athletic directors, faculty athletic representatives, et al.), beginning about 11 a.m. and finishing in time for us to be in our seats by 1 p.m. Today we then watched Ohio State defeat Cal by a score of 33 to 0.

Sunday, October 4, 1953

Ed Pauley (Edwin W.) Pauley sent me four complimentary tickets to today's Rams-49ers game, and so I was able to invite Greg Engelhard, Allan Zalkin, and a couple of others to attend the game in Kezar Stadium with season ticket holders Kalkstein, Hollander, and me. The 49ers beat the Rams by a score of 31 to 30 in an exciting finish.

Monday, October 5, 1953

Since I am leaving town tomorrow, I tried to clear my desk top this morning. I made a number of telephone calls, including one to Emilio Segrè, who has just returned from the East, to discuss the status of our patent compensation application.

I wrote to Ed Pauley and thanked him for the tickets to yesterday's football game in Kezar Stadium.

L. M. K. Boelter (UCLA) wrote on September 25 to ask for an evaluation of Tom Hicks, who is being considered for a position on the staff of the Department of Engineering there. I described in considerable detail the work Hicks has done here, pointing out that most of the reports of the work of his group are classified secret. I concluded by saying, "He was a cooperative and congenial co-worker with his fellow supervisors and a capable and effective leader of graduate students."

I also signed a letter of recommendation for Vera Kistiakowsky Fischer, similar to those I have written in the past. Vera is applying for a Fulbright Grant to study in Denmark.

Iz mentioned on his return from a consulting trip to du Pont in Wilmington that Bill Jenkins had told him that he may get to Philadelphia for the California-Pennsylvania game next Saturday. I wrote to Bill today that I shall be staying in the Warwick Hotel, arriving Thursday night. I suggested he call me there and perhaps we can work out the details of getting together.

During today's staff luncheon meeting we talked about graduate student problems and assignments. I also told the group about the memo I received from C. E. Andressen, Jr. this morning, in which he said that the AEC has acted favorably upon our request for a Heavy Ion Accelerator to be constructed in Berkeley. The Commission is at present attempting to obtain the release of some funds being held in reserve, and we should start thinking about projects to be started as soon as the funds are made available. I asked that Al and Earl handle this during my absence, and I routed the letter to them, Iz, Stan, and Bernie.

Another item discussed at the meeting was a possible visit of Lord Cherwell, head of the British Atomic Energy program,--Don Cooksey recently received a proposal from Roy B. Snapp (Secretary to AEC) about this.

This morning I received a note from Earl McBee about meeting me in Indianapolis at 5:19 p.m. on Thursday, October 15. Doral pointed out that the correct time is 9:14 p.m., so I immediately sent this information on to him. A friendly note also arrived from AEC Chairman Lewis L. Strauss, saying he will be happy to meet with me during my visit to Washington on October 15.

At 2:45 p.m. I had a conference with Mrs. Jeanne Minge, who has been hired as Television Director on the Berkeley campus. Mrs. Minge formerly was with the Tulane University Television Program.

I then spent most of the rest of the day talking with some of the students.

#### Tuesday, October 6, 1953

I was picked up this morning at 7:15 a.m. and driven to San Francisco, where I caught United Flight 602 for Chicago at 9 a.m. The flight arrived in Chicago at 5:30 p.m. During this visit I am staying at the Shoreland Hotel.

#### Wednesday, October 7, 1953

In Chicago. The AEC Reactor Information Meeting began at 11 a.m. in the Museum of Science and Industry with about 400 people in attendance. Zinn (Walter H.) (Director of Argonne) made a few introductory remarks and read a telegram from Representative Sterling Cole (Chairman, Joint Committee on Atomic Energy [JCAE]), expressing his regrets about being unable to attend.

First to speak was Hafstad (Lawrence R.), Director of the AEC Division of Reactor Development, who spoke on several questions: (1) Where are we now technically, (2) How did we get here, and (3) Where do we go from here. He talked about operation in a period of financial retrenchment, saying that we must summarize the accomplishments over the last five years and offer a plan for the next five years. Hafstad pointed out that for fiscal 1955 \$88,200,000 has been budgeted for reactor development operating costs and \$73,200,000 for plant and equipment, with nearly half of this being non-military spending. Hafstad said that we need both short and long range ideas for this five-year plan, pointing out that the need for nuclear power is not great in the United States now. He stated the cost of electric power in the U. S. is 4 to 8 mils/kilowatt-hour while in Great Britain the cost is twice this and nuclear power may be more important there. Hafstad said we need to present tangible progress each year in order to get budget support.

On specific reactors Hafstad spoke about the pressurized water reactor (light or heavy water) and Zinn's new boiling water reactor, adding that the AEC will finance one of these. Other reactors under consideration include an experimental reactor, maybe an homogeneous type financed at about \$2 million, a possible sodium-graphite reactor (Monsanto seems interested in this one), and a plutonium producing reactor. Hafstad added that technically we are in good shape for our reactors are built on schedule. The key problem, he said, is the financing of new reactors, and therefore we must pick the right ones.

C. R. [C(ampbell) Rogers] McCullough explained that the Reactor Safety Committee of which he is chairman is a new committee and has had only one meeting. He noted the problem of toxicity of plutonium--the tolerance level is  $30 \times 10^{-9}$  mg/m<sup>3</sup> of air while, by comparison, phosgene is about 1 mg/m<sup>3</sup> of air. McCullough emphasized that the safety rests with the designer and the operator; he feels that the public should be informed of the hazard.

L. A. (Lawrence Avison) Hyland (Vice-President of Bendix), the next speaker, is the organizer for a public group involved with the production of atomic power. He said that we need to get the public behind us in our fight to develop atomic power for our survival may depend on it. He cited Russian atomic-powered submarines as a potential hazard. Not only that, he said, but industry is not ready to develop atomic power alone, but we know that military applications do lead to civilian applications. Hyland said that he thinks the present AEC leadership is good and believes a broad program should be laid down. He noted that the Atomic Energy Act of 1946 provides no barriers to development of atomic energy but there needs to be a more liberal attitude on patents. He suggested that work be done with cleared people and that the Act be interpreted broadly; however, he wants more secrecy. Hyland sees no advantage in committing private funds to the development of atomic power.

After this session I spoke with Charter Heslep (with the AEC Division of Information Services) about educational television and BAETA.

In the afternoon session (2 p.m.) Weinberg (Alvin M.) reported on the outlook after four years of work on the liquid fuel (homogeneous)

reactor. They have found the catalyst  $Cu^{++}$  to be too unpredictable for use. Eta for  $U^{233}$  was been determined to be 2.27 instead of 2.37;  $d\eta/dt$  Pu =  $-0.02/C^\circ$ . The  $n,\gamma$  cross section for  $Pa^{233}$  is 150 barns. All of this is not good for the chances for a successful homogeneous breeder reactor using the Th-U<sup>233</sup> cycle. However, they have operated 5000 hours at 1000 pounds pressure over a period of four months without a leak. Weinberg said that the question "Where is the fuel?" has been important. He described a proposed Homogeneous Reactor Experiment (HRE) to be operated at 1000 kilowatts. It will be important to investigate the effect of radiation on corrosion. Weinberg estimated a cost of \$180 per installed kilowatt. He mentioned corrosion experiments with fused salts that show 5-10 mils of corrosion in 1000 hours. These systems, and U-Bi fuel systems, are for use at high temperature and low specific power while aqueous systems are better for use at low temperature and high specific power.

Zinn asked if we can find customers who can afford the power if it is economically feasible. He said that cooling with NaK is feasible and the differences in cost are minor. He reported that EBR (Experimental Breeder Reactor) in Idaho percentage "on line" is very good and is running on its own produced electricity. He went on to describe a proposed 800 Mw 9% Pu-U<sup>238</sup> fast reactor plant (800 liters, 450 kg Pu) operating at 1700°F, 30% efficiency, \$90/KW cost of reactor plant, producing electricity at 5.6 mil/kilowatt-hour, counting all costs.

Untermyer (Samuel, II) discussed "boiling water reactors" and mentioned surface boiling. He also said that heterogeneous boiling water reactors offer promise.

Rear Admiral Hyman G. Rickover, an impressive fellow, spoke on the PWR (pressurized light water reactor) that Westinghouse has at the Bevis Plant; this will produce 50,000 to 60,000 kilowatts of useful electrical power; 600 pounds/in<sup>2</sup> steam. It will operate 3000 hours between chemical processings. It will have concrete shielding and be constructed at a minimum cost estimated at \$55,000,000. The naval part of the project will be completed by September of 1957.

Grebe (John J.), describing his reactor project, said their first objective is to produce radioactive isotopes for use in radioactive warfare, their second objective is the better use of uranium in breeding, and their third objective is to make reactors pay. They have found the best reactor fuel to be bismuth-uranium slurry with zirconium or chromium coated graphite as a structural element. This reactor has standard steam power cycles. He added that a fast breeder is best for power--a compromise between two approaches.

After the afternoon session, I spoke with John Huizenga, who told me about using the  $Bi^{209}(p,2n)Po^{208}$  reaction to get 4.19 Mev for the  $Bi^{209}(n,\gamma)$  reaction raising the  $Pb^{209}$   $\beta^-$ -decay energy to 1.09 Mev. Maybe the cause of this discrepancy can be apportioned among many places.

I also met R. P. Peterson, who will be Head of Industrial Development, AEC Division of Reactors, for one year. He has spent four years as Director of Research at Sandia, Albuquerque, got his Ph.D. in physics with Bob Brode in Berkeley.

Thursday, October 8, 1953

In Chicago. I had breakfast and a ride to Argonne with Robert F. LeBaron, Chairman of the Military Liaison Committee (MLC). At Argonne I talked with Charles M. Stevens, the outstanding mass spectroscopist, who described a triple filament instrument (a description of this was published with Mark Inghram in Reviews of Scientific Instruments last month). He possibly detected  $Cm^{249}$ ,  $10^{-14}$  g,  $10^8$  atoms, on occasion.

I also talked with Art Jaffey, Paul Fields, and Marty Studier; they told me that Joe Shot #6 used high  $gt$  ( $\sim 800$ ) plutonium and  $U^{235}$ . They may have detected some  $Be^7$ .

I learned that John Huizenga has a brother in graduate school; unfortunately he has very bad eyes.

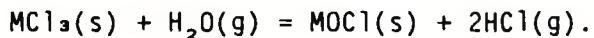
Larry Magnussen told me that he is studying  $Np^{237}$ .

Eventually, Bob LeBaron and I went out to the airport, where I caught United Flight 2500. This left Chicago at 5 p.m. and arrived in Philadelphia at 7:30 p.m. I spent the night at the Warwick Hotel.

[In Berkeley the research group held its regular Thursday morning meeting with the following in attendance: Bryan, Carr, Choppin, Cobble, Cunningham, Feay, Gray, Grover, Hollander, Jaffe, Kahn, Koch, La Salle, Lokken, Maier, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Shudde, Smith, Stephens, Stoner, Sweeney, and Templeton.

Rasmussen spoke on the decay scheme of  $Np^{238}$ , which has been studied by several workers. He and H. Slatis also studied  $Np^{238}$ , produced by 12.5 Mev protons on  $U^{238}$ , with two spectrometers while John was at the Nobel Institute. Rasmussen described the chemistry and said they determined the energy values for the four main gamma rays to be 1030, 986, 102.0, and 44.1 kev. Rasmussen talked about the work and said the most significant thing to come from the studies is that in the region of the high energy K conversion lines, they saw other K lines, which give a 939 kev gamma ray as definite, 925 kev as probable, and possibly a third gamma. He said they do not have a definite decay scheme but this certainly indicates a cluster of levels around 1 Mev, which is predicted by the Bohr-Mottelson theory.

Koch reported on their study of the following reaction in the temperature range  $700^{\circ}$ - $900^{\circ}$ K:



He described the apparatus, told about checking the measurements against some known standards, and then presented results for gadolinium and neodymium. Koch discussed the work and presented data of other workers.

\* \* \* \* \*

Friday, October 9, 1953

In Philadelphia. I made general remarks on the University of

California athletic situation (based on my recent talk in Los Angeles) at a luncheon meeting of about 50 Cal alumni at the Warwick Hotel. Brutus Hamilton and Brick Muller also spoke. Later I met with other members of the UC Berkeley Athletic Department and the football team who had flown in to Philadelphia from the San Francisco Bay Area.

I telephoned University of Pennsylvania President Gaylord P. Harnwell to inform him of my presence on his campus. When he learned that I was phoning from the Athletic Department, he could not believe it and assumed that I had been misdirected. Finally, I succeeded in explaining my role with athletics and the reason for my visit--to accompany the Berkeley football team scheduled to play his Pennsylvania team tomorrow afternoon. Harnwell insisted that I come to his office for a visit. [Harnwell, a physicist friend, was co-author with my long-time co-worker Jack Livingood of a physics text on experimental methods.]

There was also a dinner meeting of about 100 UC alumni at the Warwick Hotel at which President Robert Gordon Sproul was the featured speaker, and I again made a few remarks.

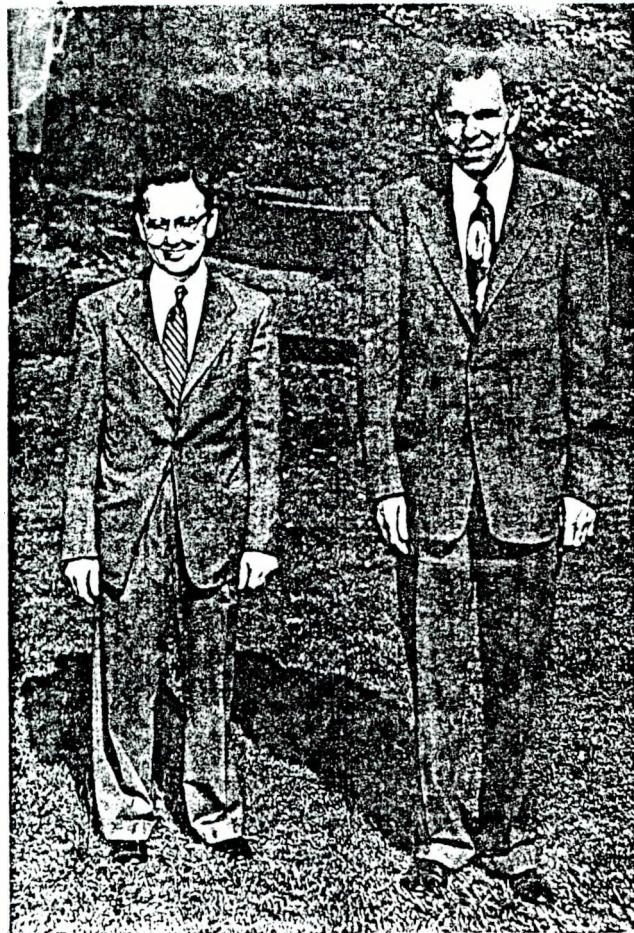
During the evening Bill Jenkins phoned and got together with me. Among the things he asked me to do was to ask Dave O'Kelley if he is interested in a job with du Pont (for example, Savannah River); if so, he should write to Bill.

#### Saturday, October 10, 1953

In Philadelphia. After a pre-game luncheon and rally attended by such notables as U. S. Chief Justice Earl Warren ('12), Bill and I sat on the bench with the Cal team and watched the team defeat Pennsylvania by a score of 40 to 0. There were over a thousand people in the Cal rooting section. Pennsylvania is even more Ivy League than Berkeley in its football program. Chief Justice Warren, Pappy Waldorf, and others spoke at the post-game reception in the Warwick.

#### Sunday, October 11, 1953

In Philadelphia. I traveled to Haverford, where I am scheduled to serve as a Philips lecturer, and was met by my old friend Tom Jones, who took me to his home for dinner. Later he drove me to my room in a dormitory.



Tom Jones and Glenn Seaborg, Haverford College, October 11, 1953

Monday, October 12, 1953

In Haverford. A tall, personable undergraduate student, T. Darrah Thomas, was assigned as my guide for my stay here. Thomas picked me up at my room this morning and escorted me to a 7:30 a.m. breakfast with students in a cafeteria. After breakfast he took me to Tom Jones' 8 a.m. Chem 61 (nuclear chemistry) class, where I spoke on "Modes of Radioactive Decay." Then, at 9 a.m., I addressed the Physics 13 class on "Nuclear Reactions."

From 10 a.m. until 12:30 p.m. I visited a chemistry lab, under the guidance of Dr. (William Buell) Meldrum (Chairman of the Chemistry Department) and Dr. (Otto Theodor) Benfey. Luncheon for twelve was served in the Faculty Dining Room at 12:30 p.m., during which Meldrum told me that T. Darrah Thomas may be interested in attending the University of California as a graduate student. Meldrum added that Thomas is the best undergraduate student he has ever known.

I met other faculty members and their wives at a 4 p.m. tea, and the students honored me with a 6 p.m. dinner (for 12), again in the Faculty Dining Room.

[In Berkeley Crane, Higgins, and Hoff began a cleanup recovery of americium in Bldg. 5A.]

Tuesday, October 13, 1953

In Haverford. At 11:10 a.m. President Gilbert F. White introduced me as the speaker for the Collection Address (talk to the student body). I reminisced about the early days of atomic energy and spoke of my concern about the current lack of student interest in science.

Then, there was a luncheon for 12 at 12:30 p.m. in the Faculty Dining Room.

Jones gave me a nice tour of the surrounding area during the afternoon--we visited Bryn Mawr, Swarthmore, and the Bartol Research Foundation. At Bartol I spoke with an old friend, Charles E. Mandeville, who told me about work they are doing on  $Pb^{203}$ ,  $Mo^{99}$ , and  $Ge^{71}$ . We discussed Dave O'Kelley's thesis work on beta radioactivities, and I told him that he has been sent a copy of the thesis. We also talked about inelastic scattering of fast neutrons (an interest of mine since my graduate student days), their 5 Mev van de Graaff machine, etc. I also met Charles O. Metzger, Stanley C. Snowden, and three Indian researchers.

There was a 7 p.m. buffet supper for me at the Jones' home, and I met a number of people. Guests included W. B. Meldrum (chemistry, Haverford), Theodore (Ted) Benfrey (chemistry, Haverford), W. E. Cadbury, Jr. (chemistry, Haverford), Richard M. Sutton (physics, Haverford), Cletus O. Oakley (math), Thomas Elkinton (President of Philadelphia Quartz and a trustee of the college), George L. Zimmerman (physical chemistry, Bryn Mawr--formerly with Bill Libby), Walter B. Keighton (physical chemistry, Swarthmore), Dr. Margaret N. Lewis (physics, Haverford), and Loren C. Hurd (Export Division, Rohm & Haas Co.). Hurd said that he will send us some material on zirconium.

Wednesday, October 14, 1953

In Haverford. After my 7:30 a.m. breakfast, I again spoke to the 8 a.m. Chemistry 61 class, this time on "Alpha Radioactivity." The rest of the morning was free for conversations with students who dropped by to speak with me. I talked with a student named Hoffman (class of '55), who may be interested in nuclear work at Berkeley and said he will get in touch with me in about a year. [Later Tom Jones told me that Hoffman is only an average student.] John Hershey (class of '56) also discussed study at Berkeley; I learned later that he is a good student. Meldrum chatted with me for a while and mentioned that he considers Thomas among the top 10 for all time there at Haverford. Meldrum also told me they are interested in hiring a physical chemist as an instructor or assistant professor--\$4200-\$4500 for nine months. He said this person would also teach instrumental quantitative analysis. Jones again arranged for a faculty luncheon.

At 6:30 p.m. President Gilbert White hosted a small dinner party for me at his home (1 College Circle).

At 8:15 p.m. I spoke in the Common Room, Founders Hall, on "Present

Status of the Transuranium Elements" as the first public lecture this year in The William Pyle Philips Lecture Series. [Other speakers in this series, all scheduled to be held in the Common Room, Founders Hall, will include Enrico Fermi, Raymond E. Zirkle, Harrison Brown, Walter H. Zinn, Martin Schwarzschild, and W. Sterling Cole.] My talk was similar to others I have given to the public on this subject.

Thursday, October 15, 1953

I travelled to Washington, where I met with Donald Lane and went over our claims and notebooks for from three to four hours. I helped Lane locate some of the material in the old notebooks--the March 28, 1941 pages on the demonstration of the slow neutron fission of  $^{49}$  (plutonium-239) and the February 23, 1941 pages on the first oxidation of plutonium by  $S_2O_3^-$ . Lane said he will make auxiliary claims in Case 61 and ask for assignments to the inventors.

In addition, I visited the AEC and talked with Thomas Johnson, Spoff English, George Kavanagh, and Dan Miller--and picked up a lot of information. They told me that they want us to see if our 17 tons of Mike debris has any 10-year  $Kr^{85}$  associated with it. [Measurement of the atmospheric concentration of  $Kr^{85}$  is used to assess Soviet plutonium production, as suggested by Tony Turkevich. This could be calibrated by comparing its yield to that of another long-lived fission product.] I learned that our chemistry budget for 1955 will be \$178,000 above that for 1954 (Berkeley couldn't know this yet). With regard to a MTR plutonium rod, I was told we should request this by memo from Dan Miller. Brookhaven, I was informed, has requested a 25 Bev machine, costing \$20,000,000. They mentioned that our heavy ion accelerator will come to the attention of the GAC on November 4. I made a note to myself to call some GAC members--Bill Libby, Jake Warner, Eugene Wigner--to prepare them for this. I learned that our heavy ion accelerator is now in the FY 1954, rather than FY 1955, budget and that Yale's heavy ion accelerator will be just like ours.

In discussing international control, I was told about the present methods of checking Soviet plutonium production, e.g., through the measurement of  $C^{14}$ . Doyle Northrup mentioned that the Boner Panel has been reconstituted--there will be a meeting next week (Tuesday and Wednesday) in Berkeley or Livermore with new members such as Ken Street, Luis Alvarez, et al.

I also learned that 25 g of  $Am^{241}$  will be recovered from 80 kg of Hanford plutonium at Oak Ridge. Three grams will also be recovered from the Los Alamos reactor Clementine. The incremental cost for recovering the  $Am^{241}$  will be \$100,000.

At the AEC I also met with Lewis Strauss--we discussed the role of Kenneth D. Nichols as General Manager, the export of radioactive tracers such as  $Fe^{59}$ , the  $Am^{241}$  recovery program, and the declassification of the spontaneous fission rates of  $Pu^{240}$  and  $U^{236}$  (which I believe is now accomplished), and the slow progress in our patent claim (I told him that I believe Roland Anderson is fighting it). Strauss said he had had a letter from Jack (John Francis) Neylan, suggesting that he hurry it up. Strauss asked that I inform him when I want him to intervene. He also

suggested that I write when I am coming to Washington so that we can get together.

It was a full, but reasonably profitable, day. I left Washington at 6:20 p.m. via American Flight 569 and arrived in Indianapolis at 9:14 p.m. Earl T. McBee picked me up at the airport and drove me to Lafayette, where I have a room at Purdue University.

[In Berkeley the research group met as usual with the following people attending the meeting: Asaro, Bryan, Carniglia, Carr, Chetham-Strode, Clark, Cunningham, Dauben, Glass, Gray, Grover, Hollander, Hyde, Jaffe, Kalkstein, Koch, La Salle, Lokken, Maier, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Shudde, Smith, Stephens, Stoner, Templeton, and Wallmann.

Carniglia went over in some detail his vapor pressure measurements on americium metal. He described the apparatus, the experiment, and then presented his data for americium, comparing them with those for neighboring elements.

Stephens concluded the meeting by giving a literature report on "Alpha-Gamma Correlation in the Transition of  $Bi^{211}$  to  $Tl^{207}$ " from Compt. rend. 237, 245 (1953).

\* \* \* \* \*

Friday, October 16, 1953

In Lafayette, Indiana. I met and talked with a number of people--Stanley R. Mickelsen (Director, Purdue Research Foundation), George A. Hawkins (Dean of Engineering), et al.

At 4 p.m. I spoke on "The Actinide Elements" to the Chemistry Department of Purdue University.

Then, at 8 p.m., I gave the Sigma Xi Lecture on "Radioactivity and the Man-Made Elements--Recent Developments" to a huge audience of a thousand people in the famous Hall of Music of Purdue University. This was recorded in full for later radio broadcast; I am to receive a copy.

Later I was driven to the Chicago Airport, where I ran into Jim (James R.) Arnold of the University of Chicago. We talked from about 1 until 2 a.m., and he mentioned that he found  $Be^7$  in atmospheric rainwater produced from heavy nuclides plus cosmic rays.

Saturday, October 17, 1953

I left Chicago at 2:30 a.m. via United Flight 601 and arrived in San Francisco at 8:10 a.m. A laboratory driver picked me up and drove me home.

I rested for a while and then drove into Berkeley to watch Cal beat San Jose, 34 to 14, in a game played in Memorial Stadium.

Sunday, October 18, 1953

The kids brought me up to date on their latest activities. I also looked over some of my mail that Iz had brought home.

The 49ers played in Chicago today, where they beat the Bears by a score of 35 to 28.

Monday, October 19, 1953

I had the usual phone calls and conferences and then looked over the accumulated mail. I noted a thank-you letter from Wayne Meinke for my letter of support for his revision of his compilation on chemical separations.

I also read an October 6 letter from Robert Naumann at Princeton, who asked for some of my slides on alpha emitters for use in his graduate lectures on nuclear chemistry. Earl is handling this request. Naumann also updated us on his research.

William H Sullivan wrote on October 7 from Oak Ridge about the second edition of his Trilinear Chart. Sullivan complained about the referencing system of our "Table of Isotopes."

Richard Doan wrote on October 7 from Phillips Petroleum Co. in Idaho Falls to describe his recruiting efforts on his trip to Berkeley. Doan made a special point of discussing Ken Hulet. He said that, if Hulet is on their staff, they will be in a better position to get AEC support on a major heavy isotopes program under the leadership of Berkeley than they would otherwise. Doan also mentioned preliminary talks with AEC Idaho Operations Office about a consulting agreement with Iz and me.

A rather long letter had arrived from Alberto Thompson, dated October 5, about various nuclear notations, the problems U. S. delegates have representing U. S. scientists at international conferences because of inadequate information of the U. S. position, etc. Thompson also mentioned that it has been reported that some American scientists want to drop the second "e" from berkelium; he was surprised that I have not indicated a preference and asked who favors the change.

C. E. Mandeville (Bartol Research Foundation) wrote on October 13 (before my visit) to thank me for arranging for him to get a copy of Dave O'Kelley's thesis. A similar letter, dated October 15, arrived from Eugene L. Church (Physics Division, Argonne), thanking me for a copy of Dick Hoff's thesis. Church made a few comments about his research on conversion electrons following  $\text{Am}^{242m}$  decay and his considerations concerning L subshell conversion of electric multipole radiations.

Frank R. Ward (Editor, Reactor Science & Technology) wrote on October 19 to describe changes in the editorial staff of the journal--James A. Lane of Oak Ridge will be the new editor and will appoint his own advisory group. Ward thanked me for my part in helping him bring the Journal to its present flourishing and healthy state.

Earl had prepared a response to Shizuo Fujiwara's recent letter, in

which he asked to spend some time here next summer, saying that we are in the process of attempting to obtain approval for a visit. [Fujiwara is spending a sabbatical with Herb Gutowsky at the University of Illinois.] Earl explained that Fujiwara must fill out a Personal History Statement, which he will receive from our Personnel Office, and emphasized that we have no funds available for living expenses here. I signed this, in addition to a memo to Donald Cooksey explaining our contacts, etc. with Fujiwara.

At noon we had our regular senior staff meeting in my office. In addition to the usual football critique, I brought up the subject of a technical means for verifying Soviet plutonium production figures in connection with an international control scheme, which Spof English discussed with me in Washington. The fellows had a few ideas, and I told them to think about the problem.

In addition, I asked Earl to help me draft an evaluation of Edwin Orlemann to be used in support of his promotion to rank of full professor.

Tuesday, October 20, 1953

Much of my morning was spent on campus; I talked with various people and then gave the Chemistry 123 lecture.

Back on the hill a short note went to R. K. Holbrook (North American Aviation) to supply him with my "Q" clearance number; this is in connection with a consulting position that John Howe is arranging.

I looked over the mail and then visited some of the labs to talk with some of the new graduate students.

Stan Thompson and I have had several talks with our Swedish visitor, Ingmar Bergström, about some sort of exchange deal whereby Stan could spend a semester in Sweden a couple of years from now. Bergström told us of another Swede, Sigvard Thulin, who wants to visit Berkeley, and one suggestion that we intend to explore is for us to continue to pay Thompson's salary while he is in Sweden and the Swedish group (Nobelinstitutet för Fysik) to pay Thulin. Bergström will talk with Manne Siegbahn about the idea when he (Bergström) returns to Sweden.

Wednesday, October 21, 1953

After the usual number of phone calls (including a call from Dick Erickson who asked me to speak to a group of insurance men who are Bear enthusiasts on November 23), I took care of some of my correspondence. Clarence Stover (former graduate student here and now an administrator at the College of Medicine, University of Utah) wrote on October 17 to ask permission for Marvin A. Van Dilla to visit the lab. I replied that Herman Robinson, Dave O'Kelley (now working with Ken Street's Project Whitney group), and Al Ghiors will be happy to see Van Dilla the week of November 2, as he requested.

I also wrote a letter supporting the promotion of Dr. John W. Gofman to a professorship in the Department of Medical Physics; this was requested by John Lawrence. The letter was similar to those I have

written for Gofman in the past--I wrote that my opinion of Dr. Gofman is very high and that I rate him at about the top of the many young chemists who were associated with me during and since the war.

On October 5 Mr. Charles H. Schauer (Director, Division of Grants, Research Corporation) requested that I serve as a member of the jury to choose the recipient of the Research Corporation Award for 1953. Today I replied, saying that I shall be glad to serve.

I then visited some of the labs and talked with some of the fellows about their work; I also tried to catch up on some of my journal and report reading.

At home I found a letter from Henry L. Jaffe, M. D. (Director, Department of Radiation Therapy, Cedars of Lebanon Hospital, Los Angeles), saying that he has rechecked my mother and is happy to report that she is doing exceptionally well. He wrote, "Her recent thyrogram now shows that the previously described hyperplastic nodule which took up so much of the tracer in the lower pole of the left lobe is no longer present and the function of the right lobe which previously was impaired is now normal." I am particularly pleased to hear this news. Iodine-131 obviously has done a good job!

Thursday, October 22, 1953

The research group meeting this morning was attended by Bryan, Carr, Chetham-Strode, Clark, Cobble, Cunningham, Dauben, Glass, Grover, Gunn, Jaffe, Kalkstein, Koch, La Salle, Lessler, Levy, Maier, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Seaborg, Shudde, Slater, Smith, Stephens, Stoner, Sweeney, Templeton, and Wallmann.

La Salle reported on her work with Cobble on the entropy of  $M0_2^+$  and  $M0_2^{++}$  ions. Using Hindman's cell values for neptunium couples, combined with work of Powell and Latimer on calculating entropy values for simple ions, they determined the entropy for  $NpO_2^+$  ( $S^\circ = 0$ ) and  $NpO_2^{++}$  ( $S^\circ = -10$ ). La Salle also described a determination of the entropy of  $VO_2^+$  ( $S^\circ = -5$ ). There was considerable discussion about the work, and Cobble said that this work substantiates Hindman's values.

Slater discussed his work on excitation functions for the reaction  $Pd^{110}(d,p)Pd^{111g}, Pd^{111m}$ . He described the experiments, presented cross section curves, and energy levels calculated for capture of a neutron in a well of infinite depth. Again the work brought forth much discussion.

Lessler talked about his experiments, using beta-gamma coincidence measurements, to show that the 73-kev gamma ray following the  $\beta^-$ -decay (1.2 Mev) of  $U^{239}$  is an  $E1$  transition. The  $U^{239}$  was produced by a  $d,p$  reaction on  $U^{238}$  in the 60-inch cyclotron.

\* \* \* \*

My attendance at the meeting was interrupted at 9:20 a.m. by a telephone call from Chancellor Clark Kerr about a matter that has concerned us--fraternity drinking. Kerr told me that he has learned from Vetterlein that the fraternities are organized. The State Board of

Equalization has informed Vice-President Corley (James H.), who contacted Kerr about the fact of big deliveries of liquor. I mentioned to Kerr that I shall see Victor Schmidt tomorrow.

Up on the hill I told Doral to send a \$20 check in response to the October 1 plea from S. M. Cohodas (General Chairman, Francis A. Bell Memorial Hospital Building Fund Campaign, Ishpeming); I then talked with some of the fellows and read reports and journals.

Friday, October 23, 1953

One of the first things I did this morning was to write to Spoff English about the possibilities for assessing Soviet plutonium production, a matter which we discussed when I was in Washington and which I have discussed with our local scientists:

I shall make a hasty attempt to answer your request for our possible thoughts on technical means for verifying past production figures in connection with an international control scheme. I have discussed this problem with most of the senior people in our Chemistry Group here, and I am rather doubtful that we have come up with anything that isn't already obvious to you. I am not too sure on the basis of which it is expected that the control scheme will operate, but we have assumed that samples would be rather freely available.

The total nvt to which a neutron moderator has been exposed can, of course, be measured for graphite by determining the amount of  $C^{14}$  and for heavy water by determining the amount of  $H^3$ . I don't have quantitative figures readily available, but these shouldn't be difficult to develop. Similarly the amount of  $Si^{28}$  in any aluminum used as cladding or construction material would be an indication of the nvt to which that aluminum had been exposed, and quantitative figures could be developed here through the use of the known neutron absorption cross section of  $Al^{27}$ .

If samples of the waste solutions from a plutonium production plant are available, there are several interesting checks that can be made. First of all, there is the very obvious one of determining the concentration of a very long-lived fission product such as 33-year  $Cs^{137}$ , or any of a number of others; in this case the total volume of the waste solution would, of course, also have to be known. The number of grams of plutonium per ton of uranium, which information would be useful in conjunction with a knowledge of the total uranium which had been used, could be determined from the percentages of  $Pu^{240}$  and  $Pu^{241}$  in the plutonium and there should be enough plutonium left in the waste solution from the first step to make this determination possible. (The same information would be available from a fission product if there happened to be a case where a long-lived fission product is partially converted by neutron absorption into another long-lived isotope not formed in appreciable yield in fission; I haven't had a chance to study all the possibilities carefully enough to be sure whether there is any such combination of circumstances that could be exploited.) The determination of the ratio of  $U^{236}$  to  $U^{235}$  in the uranium which had been used in the pile would also give information with regard to

the amount of plutonium which had been produced per unit weight of that uranium. Also, if  $U^{236}$  is found in  $U^{235}$  separated in a diffusion plant, one would know that the uranium had previously been used for plutonium production, that is, some kind of "combined operation" was in existence. The quantitative numbers which would be used here are readily available; for example, in the case of the variation of  $Pu^{240}$  and  $Pu^{241}$  with grams per ton level in the Hanford reactor, data are available in report ANL-4667 and similar data for the  $U^{236}$  to  $U^{235}$  ratio could readily be developed.

As you know, the age of any particular sample of plutonium metal can be readily ascertained from the percentage of  $Pu^{241}$  and its  $Am^{241}$  content. This would be very interesting information because if they had a lot of very old plutonium and very little of young plutonium, it would be a very unnatural situation.

I have the feeling that we haven't uncovered anything very startling, but I thought that I would send on such elementary thoughts as we have had. If we get any really good ideas, we will hasten to tell you about them.

About 10 this morning I went down to campus for a prelim. After that, I met with Vic Schmidt for much conversation and lunch. Vic also plans to attend Saturday's USC-Cal game on Saturday.

Sylvia Mae Bailey, a graduate student, had made an appointment to see me and discuss possibly doing her research in nuclear chemistry. Miss Bailey is from the University of Missouri, seems quite intense about school but is certainly no Miss America.

In today's mail was a response from my Swedish friend Lars Melander to my letter of October 1. Lars, who is presently visiting Notre Dame, said that it is impossible for him to visit Berkeley on this trip because of his schedule. He thanked me for the invitation and asked to be remembered to Helen and other friends here.

#### Saturday, October 24, 1953

I spent some time with the kids before Helen and I went to campus for the Southern California-California game in Memorial Stadium. USC won by a score of 32 to 20. We have not beaten USC since 1950, our problems starting with the infamous Pat Cannamela incident in the Cal-USC game two years ago.

#### Sunday, October 25, 1953

I worked in the yard and in my study for a while before going over to Kezar Stadium with Kalkstein and Hollander to watch the Detroit Lions defeat the 49ers by a score of 14 to 10. Before going home and in order to avoid the traffic on the bridge, we stopped at Grisons on Van Ness for a steak sandwich.

#### Monday, October 26, 1953

There were a number of phone calls this morning as usual, including

one to Rex Shudde. Shudde will take his prelims on Friday. Iz and I got together and prepared a midterm for Chemistry 123, which will be given on Thursday.

One of the more important things I did this morning was to sign a strong letter of support addressed to Kenneth Pitzer for the promotion of Edwin F. Orlemann to rank of full professor:

I am writing to give you my appraisal of the ability and demonstrated competence of Professor Edwin F. Orlemann in support of his advancement to the rank of full professor. I have known Professor Orlemann since he first joined the staff of the University of California in 1941, and during most of this time I have been in a position to make close observation of his career.

I became particularly impressed with his all-round abilities when he joined me on the staff of the Metallurgical Laboratory of the University of Chicago in 1943. Dr. Orlemann was in charge of a group concerned with the purification of plutonium and the production of the metal. The purity specifications set for this program were far in excess of anything ever before set for a production job. Both the purification methods and the analytical tests to determine whether the specifications were being achieved required the development, in many cases, of radically new methods. This task obviously required a thoroughgoing knowledge of analytical and inorganic chemistry. Orlemann clearly demonstrated that he had this knowledge and, equally important, the ability to lead and inspire the group leaders and chemists who numbered about thirty research men.

Unfortunately for our program in Chicago the reputation which Orlemann had made caused us to lose him late in 1944 to the Tennessee Eastman Corporation in Oak Ridge. This company which was involved in the separation of U<sup>235</sup> by the electromagnetic process, was in an extremely serious production crisis because the analytical program was not keeping pace with the demands made upon it. Dr. Orlemann had been singled out by a board of consultants comprised of several prominent professors of chemistry as the person to take charge in this crisis. We reluctantly agreed to his departure from Chicago. At Tennessee Eastman, Orlemann led his group in the development of new and rapid methods, particularly solvent extraction methods for the analysis of uranium from a variety of plant materials and instituted an analysis program for production runs which saw the project through the difficulties.

At the end of the war I was delighted that Orlemann returned to the staff here rather than accept any of his attractive offers from prominent midwestern universities. It has not been possible for him to devote himself entirely to academic pursuits since his return. You will recall that a few years later he was asked to assist the Atomic Energy Commission by serving on a board of consultants whose purpose it was to travel throughout the country and examine the perplexing analytical problems which were plaguing the numerous applied programs for the recovery of uranium from low grade ores.

Again in 1952 Dr. Orlemann was asked to assist in an urgent A.E.C. program. The Whitney Project of the Radiation Laboratory was being set up at Livermore, California. This project envisaged a Los Alamos type program for the study of urgent classified problems. A large staff was to be assembled in as short a time as organizational and space problems could be solved. Among the important branches of this project was to be a chemistry section of considerable size with key responsibilities to the whole program. Fortunately for us, Dr. Kenneth Street and a number of our recent Ph.D.'s in nuclear chemistry were available to handle the nuclear phases of the chemistry program. However, an important part of the chemistry program lay in the general fields of inorganic and analytical chemistry. Since no scientist of the caliber required for the leadership of this program was available locally nor could one be recruited elsewhere even though strenuous efforts were made to do so, Orlemann was called upon again. For the academic year 1952-53 he was on leave of absence status from the Department to help the Whitney Project in the crucial formative stages of its development. Currently he is continuing his service to the project on a half-time basis. The difficulty of securing a suitable replacement for Dr. Orlemann is a tribute to his possession of the combination of professional and personal qualities which made him so valuable in his previous assignments. He has a mastery of the theoretical and experimental aspects of his specialty of analytical chemistry. He has the ability to master enough of the principles of fields in which he has had no formal training, e.g., nuclear physics, so that he can integrate intelligently the work of his own group into that of the project as a whole. He has the research ability to attack and solve perplexing and difficult chemical problems. In addition, he has the administrative and leadership ability to organize a group of large size and to lead it to the successful completion of an assigned task. His humble, unassuming and humorous outlook make him a pleasant person as a co-worker or supervisor.

It may perhaps be worth emphasizing that the classified nature of these wartime and recent projects on which Professor Orlemann has spent such a large fraction of his professional career have not allowed him the opportunities for publication that he would have had otherwise. Nevertheless, a good part of this classified work was of as great a scientific value as material which is published in the first class scientific journals and when these accomplishments are taken into account, as I believe they should be, he has actually had a prolific output of sound, fundamental scientific accomplishment.

In conclusion, I might add a bit of evidence that my evaluation of Dr. Orlemann as one of the leading analytical chemists of the United States and worthy of professorial rank is correct: within the last year Professor Kolthoff of the University of Minnesota has strongly urged Dr. Orlemann to return there as a full professor to take charge of their analytical program.

Sincerely yours,

Glenn T. Seaborg

S/H/B

The Journal of the American Chemical Society referees for the Diamond paper ("An Ion Exchange Study of Possible Hybridized 5f Bonding in the Actinides") made several suggestions; today I sent their comments to Dick at Harvard and asked that he study the comments of referee II and make changes, if he thinks they should be made.

I went down the hill for an 11 a.m. appointment with my dentist, Dr. Kent Kohler, and then had lunch with the chemical engineers and Ken Pitzer.

I had another conference with Mrs. Jeanne Minge (TV Director) at 4 p.m. at which she gave me a copy of a paper she has prepared for our TV Committee on "Analysis of Television as a Technique for Disseminating Knowledge," which we will discuss at our next meeting.

At 6:30 p.m. Chancellor Clark Kerr and I had dinner with the football team. Kerr and I had a long conversation about the academic performance of athletes, and I offered to send him copies of some of the information I have on this subject plus a copy of the notes for my alumni talk last month.

Tuesday, October 27, 1953

The first thing I did was to have Doral copy a number of memos and lists about academic performance of athletes to send to Chancellor Clark Kerr, as I promised him last evening.

Included in my phone calls today was a call to Lloyd Smith about our heavy ion accelerator and one to Greg Engelhard about current problems in student activities.

Gilbert Brink, a graduate of COP who has been working in Herman Robinson's group, has spoken with me about his desire to enter graduate school in chemistry in February. In my conversation with Brink I learned that his main interest does not seem to be in the construction of gadgets per se, but in their ultimate use in physical chemistry. I wrote today to Pitzer, told him of my observations and Herman's comments, and said I would favor his admission to do graduate work in chemistry.



Gilbert O. Brink

At 1 p.m. I went out to Mira Vista for a golf game with Harvey White (104), Luis Alvarez (96), Al Ghiorso (86), and Bob Riddell (84). My score was 95.

Wednesday, October 28, 1953

This morning after the usual phone calls, etc., I got together at about 9:30 a.m. with Alton Wilson in Wally Reynolds' office to discuss some of our building problems.

Back in my office I found an October 23 letter from William Frankel (Managing Editor, The Family Library, Inc.), requesting that I review a staff article for an encyclopedia they are preparing. I told Doral to write a letter to Frankel agreeing to do it.

After talking with some of the students about their research, I went down to campus for an 11 a.m. Chemistry Department meeting.

The Television Committee met for its sixth meeting in the Librarian's office from 3:30 p.m. until 5:20 p.m. Present were Dufrenoy, Minge, Black, Coney, Emerson, Gifford, Harper, Hicks, Jones, Pickerell, and I. After the reading of the minutes of the previous meeting, Chairman Coney suggested that an executive subcommittee be formed to deal with matters requiring early action, etc. since it has been difficult to arrange meeting times for a committee of this size. We agreed to do this. Also discussed was Mrs. Minge's paper "Analysis of television as a Technique for Disseminating Knowledge from the University of California." We talked about opportunities offered by commercial stations and the types of programs that might be presented. In discussing BAEIA, it was brought out that it seems likely that BAEIA will need a good deal of support from the University and this should be a broad support. Commercial stations, such as KPIX, can offer an early opportunity to the University to gain experience. On a specific matter Harper moved that the executive subcommittee be given power to take action leading toward the acceptance of KPIX' offer of thirteen Sunday half hours following "Omnibus," beginning January 1, 1954. The motion carried, but I did not vote on the motion since I feel that work on programs for commercial stations would possibly conflict with our interest in BAEIA. In a discussion of the kind of program we might consider for production by the University on a commercial channel, we considered the recommendations in Minge's paper -- 1. "Spotlight on Learning," 2. "Object Lesson," 3. "Studies in Crime," 4. "Teen Talk." We expressed preference for the Studies in Crime program or her oral suggestion of a medical program. It was agreed that the executive committee will develop a budget proposal, decide on the type of program and, if possible, meet with representatives of KPIX and report progress at the next meeting.

Back on the hill I learned that Rex Shudde passed his prelims today with the comment "good showing." His committee was composed of Gerhard K. Rollefson, Leo Brewer, Henry Rapoport, John O. Rasmussen, Roderick Craig, and Robert J. Riddell, Jr.

Thursday, October 29, 1953

The research group meeting this morning was attended by Bryan, Carr, Chetham-Strode, Grover, Hollander, Hyde, Jaffe, Kalkstein, La Salle, Levy, Maier, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Seaborg, Shudde, Slater, Smith, Stephens, Stoner, and Templeton.

Smith referred to the  $8 \pm 2$  minute activity and 40 minute activity ( $\text{Pt}^{290}?$ ) seen by Hollander in carbon ion bombardments of tantalum; he said that he bombarded gold with 140-Mev protons, from which he studied the platinum milkings from the gold fraction. He saw a 2.6 hour activity with an  $8 \pm 1$  minute parent (mixed with a 5 1/2 hour activity due to  $\text{Pt}^{188}$ ). In addition, there was an 11-hour activity, probably a mixture of  $\text{Ir}^{187}$  and  $\text{Pt}^{189}$ ; this was resolved into an 11-hour activity with a 7 1/2  $\pm$  1 minute parent and an 11 hour activity with an  $\sim 50$ -minute parent. There was discussion about the difficulties with the assignments. Smith said that he also bombarded gold with 100 Mev protons, separated a gold fraction and then milked gold fractions at 90-minute intervals. From this he saw a 3-hour daughter ( $\text{Au}^{191}$ ) with a  $55 \pm 10$  minute parent; a 5 3/4 hour daughter ( $\text{Au}^{192}$ ) with a  $5 \pm 1$  hour parent; and a 13-hour daughter ( $\text{Au}^{193}$ ) with a  $150 \pm 40$  minute parent. Templeton commented that this illustrates that the parent mercury isotopes are not in very good shape.

Nervik spoke of the fission and spallation product data on tantalum bombarded with 340 Mev protons. In the last series he looked at cerium, bromine, cesium, tellurium, indium, cadmium, silver, palladium, and rhodium; he measured production cross sections for 17 isotopes with the extremes being  $\text{Cd}^{115} = 0.87 \mu\text{b}$ ;  $\text{Rh}^{105} = 19 \mu\text{b}$ . Nervik presented a plot of  $Z$  vs  $N$  and yields. The work was discussed by the group.

Carr reported on two bombardments of plutonium with protons to produce  $\text{Am}^{239,238}$  in one and  $\text{Am}^{239,238,237}$  in the other. He noted that  $\text{Am}^{238}$  has a 2.1 hour electron capture half-life, and no alpha particles have been reported. Carr looked at the gamma spectrum on the snapper and saw 960 kev and 570 kev gamma rays (with poor measurements), whose decay he followed; the average half-life was  $1.8 \pm 0.0$ , hours. This work and its relationship to Rasmussen's work with Släts was discussed. Carr also looked at  $\text{Am}^{237}$  but did not see anything above 300 kev--that examination is incomplete. He looked at  $\text{Am}^{239}$  but said more definitive work can be done using the 60-inch cyclotron--he saw  $\sim 230$  and  $\sim 280$  kev gamma rays, presumably corresponding to levels in  $\text{Pu}^{239}$ .

Jaffe talked for a while about calculations he recently made on data collected a year ago when he looked at  $\text{Np}^{238}$  on the bent crystal spectrometer. He finds a limit of 4% on the decay of  $\text{Np}^{238}$  by L electron capture. He related this to work done by Freedman and Rasmussen and presented a possible level scheme.

\* \* \* \*

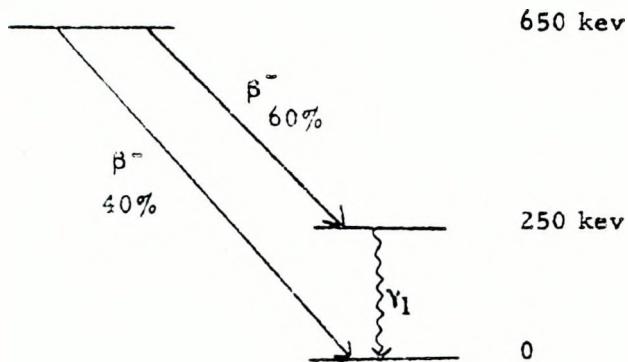
The Chemistry 123 class was given the following midterm this morning:

CHEMISTRY 123  
Midterm Examination

October 29, 1953

I (25)

Consider a radioactive nucleus with the following decay scheme:



Note:  $\gamma_1$  is 50% internally converted in the K shell. The atomic number of the product nucleus is 50, for which the K electron binding energy is 29.2 kev.

- Sketch the spectrum that would be seen with a beta ray spectrometer using a thin radioactive source without any covering.
- List by name the different photons which would be emitted from the sample.

II (20)

Suppose a derrick can lift a 10,000 kg weight and the material lifted is a sphere of condensed nuclear matter. What would be the radius of the sphere?

III (20)

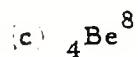
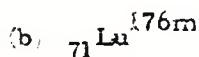
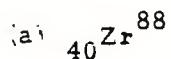
We have an even-even nucleus  $(Z^A)$  to which we add either a proton or a neutron to give an odd-even or even-odd nucleus, respectively. Suppose that the proton and neutron have exactly the same binding energies. Which isobar,  $(Z+1)^{A+1}$  or  $Z^{A+1}$ , could possibly be beta stable? Explain.

IV {20}

Calculate the alpha particle energy just capable of producing neutrons in fluorine. This is the  $\alpha, n$  threshold for the reaction  $F^{19} + \alpha, n, Na^{22}$ . (Neglect coulomb barrier considerations which might affect the yield of neutrons but not the energy threshold for the reaction.)

V (15)

List the possible modes for radioactive decay for the following whether or not each has been observed. (Qualify any answers if required.)



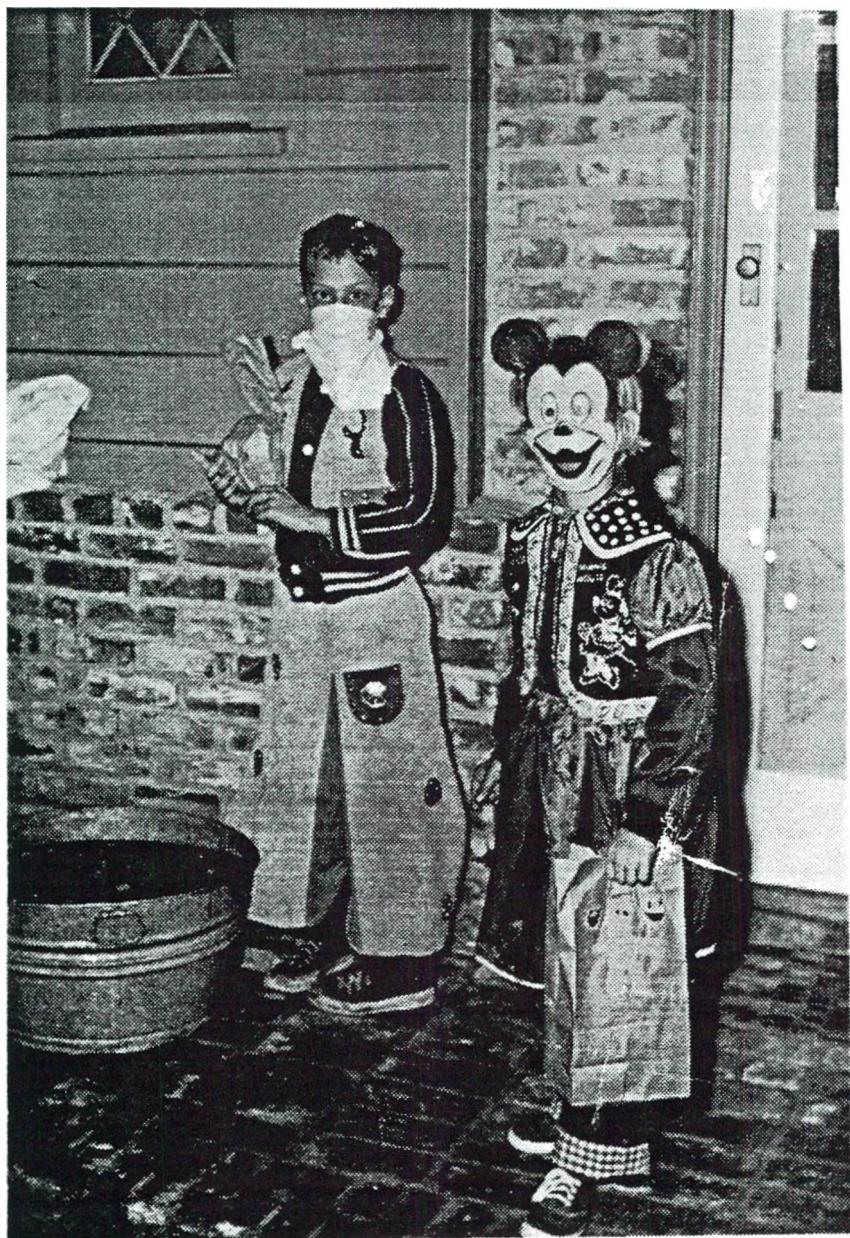
Friday, October 30, 1953

I travelled to Los Angeles with the football team and then went to my parents' home to visit and spend the night. My mother told me that her health has improved tremendously, something I was very pleased to hear.

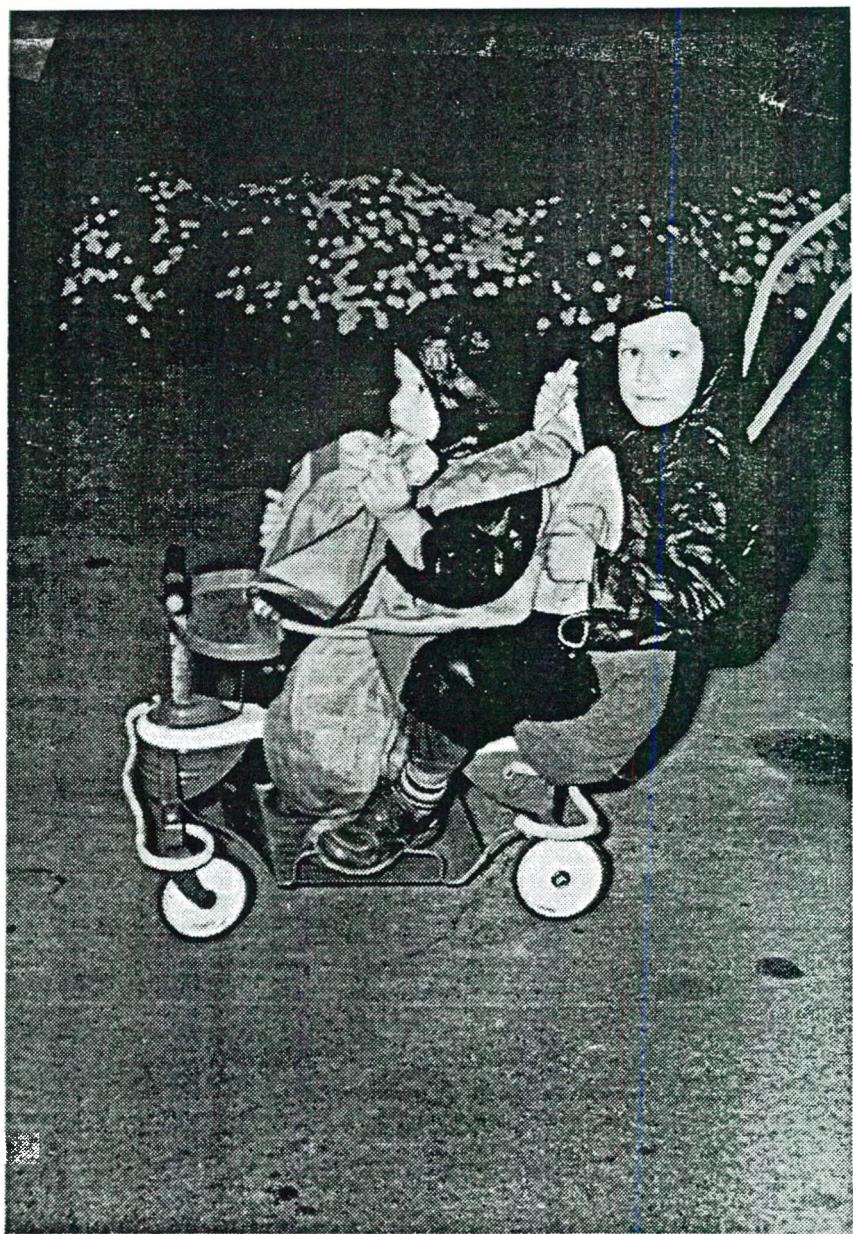
Saturday, October 31, 1953

In Los Angeles. UCLA defeated California today by a score of 20 to 7. We haven't beaten UCLA since 1950. After the game I attended a gathering of the "Southern C's"; I talked with them, but it was difficult to say anything cheerful. Later I joined the team to take the "Lark" back to the Bay Area.

[In Lafayette, Helen took the kids out trick or treating. One of the neighbors took the following photographs of them in their costumes:



Peter and Lynne Seaborg, October 31, 1953



Stephen and David Seaborg, October 31, 1953]

Sunday, November 1, 1953

The California Bears, with attending personnel, and I returned to the Bay Area from Los Angeles in ample time for me to see the 49ers beat the Chicago Bears, 24 to 14, in Kezar Stadium. Kalkstein and Hollander went with me to the game as usual.

Monday, November 2, 1953

After I returned from my recent trip to the East, I spoke with Dean Pitzer about my conversation with the Haverford student, Darrah Thomas, and the enthusiasm of his professors. Today I received a formal letter of recommendation for Thomas to the Graduate School from Tom Jones; I sent this on to Pitzer along with a supporting memorandum. Included with the letter about Thomas from Tom Jones was a note thanking me for my visit to Haverford. Jones said, "...Your visit was good for all of us because it helped to show some of the other members of our staff that scientists know something besides electrons..."

At 3 p.m. in Room 202 Lewis Hall, I took part in the prelim exam of chemistry graduate student, Edwin D. Becker, Jr. Others on the committee were Rollie J. Myers, Jr. (chairman), Donald S. McClure, Henry Rapoport, Charles Kittel, and Paul L. Chambré. Although we felt that the candidate had poor topics, we did pass him.

Tuesday, November 3, 1953

In addition to some phone calls this morning, I acknowledged a recent letter from Richard Doan (Idaho Falls) about his recruiting trip here. In response to his questions about Ken Hulet, whom Doan was particularly interested in hiring, I wrote, "I have talked with Ken Hulet and think that there may still be a chance since I have succeeded in getting a feeling for the reason behind his hesitancy. Perhaps we can talk further about this when I come to visit you at Arco." I also told Doan about another prospect, who is getting his Ph.D. next June. I wrote that this is Thomas O. Passell, a very able man and who seems interested in the possibility of living in the Idaho area. I suggested that perhaps Passell can visit at the time that Stan Thompson and Al Ghiorso come on one of their working trips.

I wrote a note to Dr. Henry L. Jaffe (Cedars of Lebanon Hospital) to express my appreciation for what he has done for my mother, saying that she feels the treatments have definitely helped. I wrote, "Although I have mentioned the subject in a general way in a number of my speeches concerning the uses of radioactive substances, I certainly now have a much better impression of the breadth of therapeutic value of such radioactive substances as  $I^{131}$ ."

Before going to campus to give the Chemistry 123 lecture, which today was on spontaneous nuclear reactions, I looked over the midterms to be returned. A number of students received grades above 90: Gilbert Brink (94), Kenneth Chapman (93), Alfred Chetham-Strode (94), Ray Collett (93), Stanley Lokken (90), Dennis Patton (93), John Ragle (92), Allan Stoner (94), and Donald Strominger (100).

I had lunch with Jay Withrow, my agent representing Northwestern Insurance Co., with whom I have my life insurance. Jay continues to work on me to increase the amount of my coverage.

Recently I was asked to supervise another "Science in Action" TV program (Ben Draper), and I asked Bernie Harvey to become involved with the program. Much of our afternoon was spent going over the program with the writers, et al.

Wednesday, November 4, 1953

Among today's phone calls was one to Ben Draper to discuss plans for the "Science in Action" program.

I made my usual tour through the labs to discuss research and other problems with the staff and students. Al Ghiorso told me about the progress being made at the 60-inch cyclotron toward the acceleration of nitrogen-14 ( $+6$ ) ions by Berni (G. Bernard) Rossi and Bart (William B.) Jones. [Previous attempts to accelerate  $N^{+7}$  have been unsuccessful here; however,  $N^{+6}$  beams have been produced in England.]

Recently, at home I received an October 29 letter from Victor T. Johnson (Vice President and Chairman of Program Committee, Acalanes High School Parents' Club). Johnson wrote to ask me to introduce a program "Science in Action" in the high school auditorium, scheduled for the evening of January 18, 1954, and to give an informal talk of about fifteen minutes in length on the subject "Careers in Science for Young People." This evening Johnson telephoned, and I agreed to participate in the program.

Thursday, November 5, 1953

Before the research group meeting, I returned a phone call to Donald McPherson (Row, Peterson & Company) at his hotel and made arrangements to see him this evening. This "first of the month" open meeting of the research group was attended by Bryan, Carr, Clark, Cobble, Conway, Cunningham, Dauben, Glass, Gray, Grover, Hollander, Hyde, Jaffe, Robert Edward Jones, Jr. (a graduate student from Columbia University),

Robert Jones



Kalkstein, La Salle, Maier, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Seaborg, Shudde, Smith, Stephens, Stoner, Templeton, and Wallmann.

Jaffe talked about his run on the bent crystal spectrometer of a

sample of  $Tl^{204}$ , which was obtained from Oak Ridge. This has been reported to have a K-capture branching ratio of approximately 1.5%. Jaffe saw  $K\alpha_1$ ,  $K\alpha_2$ , and indications of  $K\beta$  x-rays of mercury with good energy checks. He said that he is presently looking for L x-ray lines of mercury, which are in the range of 9 to 13 kev. Jaffe pointed out that the discrepancy in the reported half-life values for  $Tl^{204}$  (2.5 to 4.5 years) is probably due to the presence of an isomer of  $Tl^{204}$ ,  $Tl^{205}$ , or  $Tl^{206}$  with comparable half-life. He said  $Tl^{206}$  looks like the best bet because it is an odd-odd nuclide and has a short half-life. There was a lot of discussion about the work, and Perlman mentioned that, if there is really a  $Tl^{206}$  isomer, the isomers of  $Bi^{210}$  do not decay to it--they give only the 5-minute  $Tl^{206}$ .

Passell talked about work on  $Tl^{198m}$ , in which Bergström and co-workers at the University of Illinois assigned some electron lines to three gamma rays. Passell said they made  $Tl^{198m}$  by helium ions on  $Au^{198}$  and definitely confirmed the assignment of the three gamma rays (260 kev, 282 kev, and 48 kev). He presented the decay scheme that they postulate for the isotope.

I talked for the rest of the meeting on Pais' theory of elementary particles, which is based on selection rules for isotopic spin. I went over a number of points, explaining that I have heard of this by word of mouth.

\* \* \* \*

After giving the Chemistry 123 lecture, which again was on spontaneous nuclear reactions, I stopped in the Department office and then went up to the hill, where I talked with a number of the fellows and then looked over material for several talks I am scheduled to give in the near future.

At 6 p.m. I met Donald McPherson for dinner and conversation. McPherson went into considerable detail about a series of chemistry books that his company would like me to edit for Row, Peterson & Company. It is an interesting prospect, but I do not know if I have time for such a project.

Friday, November 6, 1953

After a phone call to Louis Werner this morning at the Naval Radiological Defense Laboratory at Hunters Point, I made the rounds of the labs to check on the research and to talk with various people.

Then at 2:15 p.m. I sat on Frank S. Stephens' prelim committee. Other committee members were Gerhard K. Rollefson, David H. Templeton, Donald S. Noyce, Roy Overstreet, and Stephen P. Diliberto. Stephens, one of Perlman's students, passed his examination.

Saturday, November 7, 1953

I worked in my study for a while and then went into Berkeley and watched Cal defeat the University of Washington by a score of 52 to 25. The score was rather an embarrassment to me since I was sitting next to

Dick Everest, Faculty Athletic Representative at the University of Washington.

Sunday, November 8, 1953

Much of my day was spent with the kids and on various writing projects in my study. The 49ers played the Los Angeles Rams in the Los Angeles Coliseum today. They won by a score of 31 to 27.

Monday, November 9, 1953

Included in this morning's calls was one to Rix Snyder and one to Miles Hudson, both about athletic matters. Robert White of McGraw-Hill also called to talk with me about writing a book for them.

I received a November 7 note from Z(arah) D. Welch (Secretary, Purdue Chapter of Sigma Xi), in which he said they arranged to have my Sigma Xi lecture on October 16 dubbed off the tape, which radio station WBAA recorded. Welch reported that the tape copy has been mailed to me and then asked me to return it when I am finished with it. I plan to have a copy made for my personal file here.

After the regular noontime senior staff luncheon meeting, I attended a meeting of the lab's salary committee.

Tuesday, November 10, 1953

There were a few phone calls this morning, including one from Dick (Richard R.) Smith in Idaho Falls about his and Reeder's clearances to visit here, I talked with Stan and Al and then called Phil Schiedermayer (head of UCRL's Security) to ask for his help. I also signed our usual letter to Darrah Thomas (Dean Pitzer has already sent him a letter assuring him of admittance as a graduate student) about our employment setup here at the laboratory and the security clearance.

I had a 10:30 a.m. appointment in my campus office with Rene Zentner about doing graduate research with our group; I then gave the Chemistry 123 lecture, concluding my discussion of spontaneous nuclear reactions.

Joe Hamilton and I had lunch together--most of the conversation was about the acceleration of  $N^{+6}$  ions in the 60-inch cyclotron. [Ghiorso, Rossi, Harvey, and Thompson have bombarded uranium with nitrogen ions and have found evidence for a chemically identified 7-minute, alpha-particle-emitting isotope of element 99.]

After lunch, I went to San Francisco with Bernie Harvey for a filming with Earl Herald of our "Science in Action" program on the cyclotron and nuclear reactions.

Wednesday, November 11, 1953

This morning I telephoned Tom Johnson (Director of Research) at the AEC in Washington to discuss progress on our proposed heavy ion accelerator, checked over my slides for this evening's talk, looked at my mail, and then prepared some notes for tomorrow's meeting.

There was a King of Hellenes ceremony in the Greek Theater at 2 p.m., which I attended. Then, at 3:15 p.m., I went to the seventh meeting of the Television Committee, again held in Coney's office. Also present today were Dufrenoy, Barrows, Black, Coney, Emerson, Gifford, Harper, Jones, Pickerell, and Minge. Coney announced that the executive subcommittee (Emerson, Jones, Pickerell, and Coney) met on November 9 with Mrs. Minge and Messrs. Dempsey and Matheson of KPIX to discuss Mrs. Minge's proposal for a 13-week cross section program tentatively entitled "Inquiry" to commence January 10, 1954. The subcommittee approved the proposal but made no commitments. We again discussed the effect of this project on the success of BAETA and concluded that the principal hazard to BAETA would be the possibility of the diversion of the University's production staff to a commercial show with no time left for assistance on BAETA productions. The following motion was carried unanimously:

MOVED (Barrows, Black) the reaffirmation of the KPIX project, "Inquiry," and approval of the format of the show (as described in pages 1 and 2 of Appendix A to these minutes), provided adequate funds are available for the production of "Inquiry" and also of the Seaborg [a proposed program on the chemical elements], Griller, and Extension programs in which BAETA is interested, and provided further that the "Inquiry" project does not conflict with BAETA's objectives and plans.

The chairman was instructed to find out from Jonathan Rice if "Inquiry" conflicts with BAETA's objectives and plans; if BAETA would like some kind of tie-in with "Inquiry" provided this is feasible; if BAETA wishes to have a kinescope of "Inquiry" and, if so, whether it will seek funds from the Ford Foundation for this purpose. We agreed that, if Rice saw no conflict, the Chairman should transmit the recommendation to the Chancellor. Other items discussed today was the budget (\$10,000), compensation for participants, and BAETA use of commercial studios.

The Sigma Xi lecture was held at 8:15 p.m. in Room 145, Dwinelle Hall. I again spoke on "Present Status of the Transuranium Elements," illustrating the talk with about 30 slides. Unfortunately, it was very poorly attended for the sponsors neglected to publicize the talk. I appreciated the attendance of Lincoln Constance, whose presence helped augment the small group that was present.

Thursday, November 12, 1953

The research group meeting this morning was attended by Bryan, Carr, Chetham-Strode, Clark, Cobble, Cunningham, Dauben, Glass, Grover, Hollander, Jaffe, Kalkstein, La Salle, Levy, Maier, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Seaborg, Shudde, Smith, Stephens, Sweeney, Tembleton, Wallmann. -

Glass spoke for the entire meeting on some work he is beginning on the mechanism of low energy helium ion spallation and fission reactions. In the first bombardment, they bombarded  $Pu^{239}$  with 37-Mev helium ions and said that the object of the research is to determine the extent that fission competes with spallation in the transuranium region. He first described the equipment and the techniques. In this bombardment they determined the absolute cross sections for the formation of  $Cm^{240}$  (1.20

mb),  $Cm^{241}$  (4.1 mb),  $Cm^{242}$  (0.65 mb),  $Am^{242m}$  (<0.25 mb),  $Am^{240}$  (7.7 mb), and fission product  $Ba^{140}$  (5.4 mb). Glass added that the total fission cross section, assuming 3% for the yield of  $Ba^{140}$ , would be about 360 mb. He also noted that the yield of  $Am^{240}$  is surprisingly high but this may be due to deuteron contamination of the helium ion beam. There was a lot of discussion about the work, which will continue.

\* \* \* \*

I stayed at the meeting for only a very short time for I was scheduled to report at the Radiation Laboratory to the AEC General Manager Kenneth D. Nichols, Commissioner Joseph Campbell, Brigadier General Kenneth E. Fields (Director, Division of Military Application), Lawrence R. Hafstad (Director, Division of Engineering), et al. I talked about the general organizational tie between the UC Chemistry Department and our nuclear chemistry group at the Radiation Laboratory, mentioning Dean Kenneth Pitzer and Professor Wendell Latimer. I spoke of the work of those on campus, such as that of Leo Brewer and Le Roy Bromley (high temperature); Robert Connick (basic chemistry); Melvin Calvin, Andrew A. Benson, and Bert M. Tolbert (bio-organic); and Bromley, Donald Hanson, Ted Vermeulen (chemical engineering). In speaking about the Nuclear Chemistry group, I named our academic people, our permanent staff, mentioned our few postdoctoral men, our 25 to 30 graduate students, and the chemical group of Project Whitney under Street, Orlemann, and Hanson. This was followed by a discussion of our research: I. Chemistry of Heavy Elements (Actinides) and Lanthanide Elements; II. Spallation, Fission, and General Transmutation Studies; III. Instrumentation; IV. Radiochemical Procedures and Techniques; V. Heavy Element Isotope Research; VI. Alpha Radioactivity, Energy Levels, etc.; VII. Miscellaneous, Radiation Chemistry, Health; VIII. Heavy Ion Acceleration and Bombardment; IX. Isotopes from Multiple Neutron Capture (Long Range Detection with Stan Thompson, Al Ghiorso, Los Alamos and Argonne people); and X. Molecular Atomic Beams (Low temperature nuclear alignment). Bob Thornton, Burt Moyer, and John Lawrence also made presentations.

I then stopped in my office to look over the mail. Dick Diamond has returned our paper, "An Ion Exchange Study of Possible Hybridized 5f Bonding in the Actinides," along with some slight changes to conform with the referee's comments. I returned the corrected paper to W. Albert Noyes--I also routed Dick's letter to Jim Cobble for his information. Diamond mentioned that Professor Eugene G. Rochow (Harvard) will spend Monday, November 16 until Wednesday, November 18, in the Bay Area and may telephone me on Tuesday to see if he can visit me. Dick himself, his wife Marian, and baby daughter Catherine will visit around Christmas time.

The AEC group, along with a number of Berkeley people, left for Livermore at about 11 a.m., where we remained through dinner.

Friday, November 13, 1953

I made a few phone calls this morning, including one to Winston Manning, during which I told him that the combined Ivy report did not include a description by his people of their work on the thermal neutron fission cross section of  $U^{237}$ ; Manning said that he will check into the matter. On other matters I spoke with Marv Kalkstein to ask him to help

give a tour of the lab to the Student Affiliates of the American Chemical Society tomorrow. I also talked with Herb York and David Judd.

In connection with a conversation I had with John Huizenga while I was in Chicago, I wrote him a note to refer him to Elmer L. Kelly's thesis (UCRL-1044) about the Bi(p,xn) excitation functions.

After making the rounds of the labs, I had a long chat with Al, Stan, and Bernie about their work. They showed me their data from the bombardment of  $U^{238}$  with  $N^{14}(+6)$  ions, indicating they have produced a 7.3-minute 99 isotope (probably mass number 247) decaying probably by electron capture and a 7.35 Mev alpha particle and also  $99^{246}$  with a half-life of minutes, decaying by electron capture, and observed through the growth of its 1.5-day  $Cf^{246}$  daughter. The fellows described their chemistry, the other nuclides they saw, and said they plan to write a report to submit to The Physical Review at the same time Hamilton submits his "Letter" about the acceleration of nitrogen (+6) ions.

I looked over the mail, had lunch, and then took the afternoon off for a much needed golf game at the Sequoyah Country Club in Oakland.

Saturday, November 14, 1953

I drove into the lab in Berkeley this morning to give the tour (with Kalkstein) to the Student Affiliates of the ACS and then went to Memorial Stadium for one of Mary Swafford's lunches with various members of the Athletic Department. Although wives often attend, Helen has been too busy with the children (Steve and Dave presently have the mumps, which they caught from neighbor children, Steve and Dave Dibble) to be able to attend. Today Cal and the University of Oregon played to a 0 to 0 tie.

Sunday, November 15, 1953

I worked in my study off and on during the day, interrupted frequently by one child or another. The 49ers played in Cleveland today; the Browns beat them by a score of 23 to 21. The Cleveland Browns have been a sort of nemesis to the 49ers dating back to the beginning of their competition in 1946.

Since I haven't written a report on my meeting with our attorney Donald Lane on October 15, I telephoned Joe Kennedy in St. Louis today and gave him a verbal report--Joe said that he has written to ask me about the meeting. I also described my meeting with Chairman Strauss on the same day and about recent conversations with Emilio. It was a rather lengthy conversation--Joe is going to Washington next week and wants to be knowledgeable about our latest thoughts when he sees Lane.

Monday, November 16, 1953

Again I made a number of phone calls. I talked with Alberto Thompson about some publishing matters. I also telephoned Chancellor Kerr to discuss the upcoming PCC meeting and its agenda. In addition, I tried to phone Rixford Snyder to talk about items for the PCC meeting, but he was out of his office today.

In today's mail was the letter Joe Kennedy told me about yesterday. I also received a copy of ANL-WMM-1113, "The Thermal Neutron Fission Cross Section of  $U^{237}$ " by G. L. Pyle, Sherman Fried, W. C. Bentley, and J. L. Lerner. Manning's covering letter said that he checked after my phone call on Friday and found that the report was not included in the Ivy report.

A November 13 memo also arrived from P. L. Schiedermayer (Security), confirming our conversation last week that Smith (Richard) and Reeder of Idaho Falls should be advised to proceed with the extension of their clearance, preparatory to their proposed visit here. I routed the memo to Stan and Al.

After looking over some reports and journals, talking with the students, and holding the noontime senior staff meeting, I went down to campus where I sat in on the prelim examination for Harold Ivan Weingarten at 2:15 p.m. in Room 202, Lewis Hall. Weingarten discussed the propositions (1) The effect of fluoro and perfluoro groups on the reactivity of adjacent functional groups, and proposals concerning the chemistry of certain compounds not yet studied and (2) A review of the structure of gliotoxin. Chairman of his committee was Henry Rapoport, and other members were Berni J. Alder, Andrew Streitwieser, Jr., Frederick H. Carpenter, and Roy Overstreet.

Tuesday, November 17, 1953

This morning I returned a telephone call to Don Hull, a roommate during my graduate school days. I talked with Emilio Segrè, and we decided that I should send a night letter to Joe Kennedy: "DON'T HESITATE TO PHONE US FROM WASHINGTON FOR ANY REASON EVEN MINOR THAT WOULD SAVE TIME." I also looked over a November 14 letter from Stanley V. Castner (former graduate student now at Convair) who wrote about his work involving the determination of several reaction cross sections. Castner said he would like to visit the UCRL between December 23 and December 30 to talk with some of us about his work. I asked Earl to respond to the letter and to work out the dates with Iz since I will probably be out of town.

I then went down to campus, where I had a 10:30 a.m. appointment with Harvey White before the Chemistry 123 lecture, which today was a continuation of the discussion about accelerators, neutrons, etc.

At 1:30 p.m. I attended the prelim examination of Russell H. Sanborn. The committee was composed of Donald S. McClure, chairman; Rollie J. Meyers; Donald Noyce; William B. Fretter; Raymond H. Sciobereti; and me. His performance on the examination was not too good, and Sanborn just passed.

Wednesday, November 18, 1953

This morning I telephoned Rix Snyder about some athletic matters, including the upcoming PCC meeting.

C. P. Boner (Dean of the University, Office of Government Sponsored Research, The University of Texas, whom I know as chairman of the Boner

Panel) wrote on November 6 to ask for a character and loyalty evaluation of Leon O. Morgan (old Met Lab colleague and former graduate student), who will assume a position involving access to classified material in a Department of Defense research project. Earl prepared a response for me to sign, describing my contact with Morgan and saying, "There is no question in my mind that he is a reliable, trustworthy, and loyal person. He has a mature stable personality, and his character, as far as I know, is beyond reproach."

I signed a reply Earl prepared, addressed to Stanley Castner, who wrote on November 14 that he wants to visit during the holidays, particularly to talk about reaction cross sections of aluminum. I said that we will be pleased to have him visit although I may be away. Others, including Drs. Perlman and Hyde, will be here on the 29th and 30th of December to discuss his program.

A lengthy letter arrived from Donald A. McPherson (Row, Peterson & Company), outlining his proposal to me on November 5 for editing a series of chemistry books. McPherson went into considerable detail about the offer and said that he wants me to take all the time I need to reach a decision. The offer sounds reasonable enough, but I doubt that I have time for such a project.

I spent considerable time in the labs looking over the research and talking with the fellows.

At 2 p.m. I went down to campus for the prelim examination of Miss Armine Paul in Room 202, Lewis Hall. In addition to me, her committee was composed of Rollie J. Myers (chairman), Donald S. McClure, Charles W. Tobias, Roderick Craig, and Owen Chamberlain. Miss Paul passed her examination with the comment "Good exam."

Today I read an interesting report, which arrived yesterday from Argonne, by Charles M. Stevens, Martin H. Studier, Paul R. Fields, and Herbert Diamond entitled "Identification of  $Cm^{248}$ ." This new isotope was identified in a pure curium fraction from "Mike" debris, using their 12-inch, 60° mass spectrometer, which was built by Stevens.

The Intercollegiate Athletic Advisory Board had its dinner meeting at 6:30 p.m. in the French Room of the Durant Hotel, which I attended. Other members of the Board include Dr. W. W. Middlekauff, Dean H. E. Stone, C. G. Carlisle, O. Cort Majors, C. S. Wheeler, Jr., John P. Engvall, Don Hartley, Tevis Martin, W. J. Norton, and Brutus Hamilton. A number of subjects were discussed, but the meeting was over by about 10:30 p.m.

#### Thursday, November 19, 1953

The meeting of the research group this morning was attended by Asaro, Carr, Clark, Cobble, Glass, Gray, Grover, Hollander, Hyde, Jaffe, Kalkstein, La Salle, Michel, Nervik, Perlman, Rasmussen, Schooley, Seaborg, Smith, Stephens, and Stoner.

Asaro spoke on his determination of absolute intensities of gamma rays using sodium iodide crystals, saying that the chief problem in the

low energy (less than 100 kev) region is correction for the escape peak (which is lower than the main peak by the energy of the iodine K x-rays). He presented a curve with points for  $\text{Am}^{241}$ ,  $\text{Th}^{228}$ ,  $\text{At}^{211}$ ,  $\text{Cd}^{109}$ , and  $\text{Th}^{230}$ , showing the theoretical calculation on the abundance of escape peaks compared with experimental observations. Perlman suggested that the amount of surface of the crystal exposed to the sample might have a marked effect on the escape peak, and Asaro said that this may be the source of the discrepancy between their experimental values and the theoretical ones.

Michel described his progress in attempting to determine the correct half-life for  $\text{Tl}^{204}$ --there are several old reported values. He said he is now collecting on the time-of-flight mass separator samples corresponding to mass numbers 204, 205, and 206 from a recently bombarded sample of normal thallium. He has about 200 counts per minute per day at mass number 204 but no counts at mass number 205 nor 206 have been observed yet.

Stoner talked about alpha-gamma and gamma-gamma coincidence studies with  $\text{Em}^{211}$  produced in a high energy proton bombardment of  $\text{Th}^{232}$ . They found the 70-kev radiation in coincidence with alpha particles. In addition, they saw radiations of 157 and 224 kev in coincidence with alpha particles. Stoner suggested a decay scheme. He also looked at 19.3-minute  $\text{Fr}^{212}$ . In this case only the 70-kev radiation was seen in coincidence with alpha particles, presumably because both the 48 and 22-kev radiations are highly converted. In addition, Stoner said they are studying the  $\text{At}^{208}$  daughter of  $\text{Fr}^{212}$  and found 170-kev and 150-kev gamma rays, which are both in coincidence with the 662-kev radiation. Stoner said that the first excited state of  $\text{Po}^{208}$  is undoubtedly at least as high as 660 kev since they know it is within two nucleons of both proton (82) and neutron (126) closed shells. X-rays of polonium were also seen in coincidence with the 662-kev radiation.

Cobble spoke extensively on Peter Fong's new theory of fission, which Fong presented at the Gordon Conference on Nuclear Chemistry last summer; he noted that Fong's is a statistical approach, not primarily dependent on shell theory, and uses the compound nucleus as the starting point. There was quite a lot of conversation about the work.

\* \* \* \* \*

After the Chemistry 123 lecture, I went up to the hill, looked over my mail and talked with some of the students. Warren Smith, who will be taking his qualifying examination shortly came in to talk with me (from 1:30 until 2 p.m.) about his propositions. I also got together with Joe Surls, who is having some problems with Physics 115.

Friday, November 20, 1953

I looked over a couple of reports, dated November 18, 1953, this morning. It is expected that these will be declassified without delay and submitted to The Physical Review as "Letters to the Editor" shortly. The first, UCRL-2410 "The Acceleration of Nitrogen-14 (+6) Ions in a 60-Inch Cyclotron" by G. Bernard Rossi, William B. Jones, Jack M. Hollander, and Joseph G. Hamilton, described the acceleration of  $\text{N}^{14}$  (+6) ions, verified by the production of astatine isotopes from platinum. The

second report, UCRL-2411 "Reactions of  $U^{238}$  with Cyclotron-Produced Nitrogen Ions" by Albert Ghiorso, G. Bernard Rossi, Bernard G. Harvey, and Stanley G. Thompson, reads:

The acceleration of  $N^{14}(+6)$  ions with the Berkeley Crocker Laboratory 60-inch cyclotron has made it possible to study nuclear reactions of these ions with  $U^{238}$ .

The following transmutation products have been observed:  $99^{247}(?)$ ,  $99^{246}$ ,  $Cf^{244}$ ,  $Cf^{246}$ ,  $Cf^{247}(?)$ ,  $Cf^{248}$ ,  $Bk^{243}$ , and other berkelium isotopes not yet identified. The identification of these elements was definitely established by their carrying on lanthanum fluoride precipitates and by their order of elution from a Dowex-50 ion exchange column.

The observed nuclear properties of these nuclides are summarized in Table I.

The nuclides  $Cf^{244}$ ,  $Cf^{246}$ ,  $Cf^{248}$ ,  $Bk^{243}$ , and  $Bk^{245}$  have previously been observed in this laboratory.

The yields of the transcurium nuclides were low even though bombardment currents of 0.1 microampere of  $N^{14}(+6)$  ions of energy greater than 100 Mev were available. In three separate experiments a total of 40 alpha-emitting atoms of the 7.3-minute isotope of element 99 were observed to decay in the ion exchange column fraction immediately preceding californium, namely the eka-holmium position. Thus, the element identification is certain though the mass number can only be inferred on the basis of nuclear systematics. By observations of the abundant fission product activity, it was found that almost all of the nuclear reactions of nitrogen ions with  $U^{238}$  resulted in fission much as in the case of carbon-ion bombardment of the same nucleus.

It is a pleasure to acknowledge the continued help and encouragement of Professor Joseph G. Hamilton, Director of the Crocker Laboratory. Our grateful thanks are extended to William B. Jones and the members of the 60-inch cyclotron operating crew for their cooperation in making the many bombardments necessary for this work. Special thanks are due Dr. Gregory Choppin for his valuable assistance with some of the chemical separations. It is a privilege to acknowledge that this work was accomplished with the always helpful guidance of Professor Glenn T. Seaborg. The continued interest and encouragement of Professor Ernest O. Lawrence is gratefully acknowledged.

Table I covered the decay properties of these nuclides.

Among my phone calls this morning was one to Rollie Myers with whom I discussed his student, David Meschi--Meschi's qualifying examination was scheduled for today. Myers told me that Meschi is doing quite well in research, that he started from scratch to build his apparatus and is about half finished with it. Myers reported that Meschi is a chemical physicist who is not terribly original. I also learned that the examination has been rescheduled for December 14.

Rene Prestwood, who was an undergraduate student here at Berkeley before the war, is visiting the laboratory from Los Alamos. He stopped in to see me, and I offered to get him a ticket to tomorrow's football game between Cal and the University of Oregon. Prestwood accepted, and so I then telephoned George Briggs to arrange for the ticket.

I went to hear Emilio Segrè speak in the Building 50 Auditorium this morning.

Helen and I attended the ACS Award Dinner that began with a social hour at 6:30 p.m. in the Alameda Hotel.

Saturday, November 21, 1953

Helen and I drove to Stanford and stopped at the Rix Snyders' for a pre-game reception and buffet luncheon. The "Big Game" ended in a 21 to 21 tie--unfortunately, Cal was unsuccessful in a final attempted field goal.

Sunday, November 22, 1953

I worked in my study much of the day. The 49ers played Green Bay in Milwaukee today and won by a score of 37 to 7.

Monday, November 23, 1953

Among this morning's phone calls was one to Joe Hamilton, who said they are mailing the two "Letters" to The Physical Review today.

Later in the morning Mr. Haral H. Cobb (Central Bank Bldg., Oakland), an ardent "Californian," picked me up and drove me to the Leopard Cafe (140 Front St., San Francisco), where I addressed the San Francisco Pond, Order of Blue Goose, International, a group of insurance executives, at noon. The talk, arranged by Dick Erickson, was similar to the one I gave in Los Angeles to the Alumni Association on "The University and Intercollegiate Athletics." Cobb drove me back to the campus after the talk.

I received and read the following November 20 letter from Donald Lane:

LEE B. KEMON  
DONALD E. LANE  
SOLON B. KEMON

TELEPHONE  
DISTRICT 7-3870

LAW OFFICES OF  
LEE B. KEMON  
AND  
DONALD E. LANE  
PATENTS AND TRADE-MARKS  
1331 G STREET, N.W. WASHINGTON 5, D.C.

November 20, 1953

Filed 11/20/53	Action _____
Rec'd NOV 23 1953 GTS	
Ans. _____	File _____

Dr. Glenn T. Seaborg  
University of California  
Radiation Laboratory  
Berkeley 4, California

Re: AEC Docket No. 7

Dear Dr. Seaborg:

Since your visit here October 15, a selection of about twenty of the two hundred patent application claims has been made, but only minor progress on tying these in with the workbook entries has been possible. Dr. Kennedy was here yesterday, and it was decided that the selection of typical claims and certain workbooks would be forwarded to St. Louis for checking by him and Dr. Wahl.

A Supplemental Application to add cases S-61, S-202, and S-5617 to the subject docket, to double the amount of the compensation or award sought, and to seek reassignment of all cases to the applicants in case of no award, has been prepared, and is enclosed for the signatures of you and Dr. Segre, and for you to forward to St. Louis. Dr. Kennedy advises me that he and Dr. Wahl agree on the amount now to be sought.

I have talked with Mr. Anderson this morning, and have filed a letter request to him for certain copies. He agreed to furnish same as promptly as possible. My examination of files at his office this morning shows:

- (1) The assignment in S-61 does not refer to eng-30.
- (2) Claim 21 of S-52d was transferred to an application in the names of Seaborg and Perleman, Case S-3155, S.N. 712,243, which case is not a true division of S-52d, and hence should not be added to the subject docket.

(3) Claims 1-10 of S-52d were transferred to a divisional application now identified as S-5617, S.N. 755,178 filed June 17, 1947. The assignment in this case also refers to eng-36.

Mr. Anderson also read to me parts of a letter he received yesterday from Mr. Ooms, Chairman of the Patent Compensation Board, admonishing Anderson to cooperate with me in getting issues defined so that this case can proceed to trial. Perhaps your visit with a member of the Commission resulted in word being passed to the Patent Compensation Board as well as to the Patent Branch.

I mentioned to Dr. Kennedy that it seemed wise to give the Patent Branch one more chance to define issues by submitting our documented summary of typical claims for approval or disapproval. If that secures no progress, then a memo detailing the numerous instances and points at which General Counsel has refused to take any position might well be passed by you to the Commission for action. Possibly that time would be appropriate for inquiring about a settlement.

A copy of the typical claims will be forwarded to you in a few days.

Very truly yours,

*Donald E. Lane*

DEL.mB  
AIRMAIL  
cc: Drs. Kennedy, Segre, Wahl.

After looking over Lane's letter, I telephoned Emilio to discuss it with him.

Tuesday, November 24, 1953

I drove to San Francisco this morning for a 9 a.m. breakfast at the Clift Hotel to discuss with Berkeley and Stanford representatives some of our strategy for the December PCC meeting here. Stanford's Rix Snyder will introduce and Berkeley will second a motion with respect to the illegal play by Pat Cannamela in the 1951 Berkeley-USC football game. We also went over details for the PCC dinner.

Afterwards I returned to Berkeley, and went with Rene Prestwood, Herman Spindt (Director of Admissions), and Al Ghiorso to Contra Costa Golf Club for a 1 p.m. game of golf. Spindt played only 12 holes: (RP-88, GTS-95, AG-101).

Wednesday, November 25, 1953

I telephoned a number of people--Wally Reynolds, Dan Wilkes, John Flaherty (San Francisco AEC office), Mary Swafford (concessionaire for Memorial Stadium), Betty Dyer, Ernest Lawrence, Harris Levy, et al.

In between phone calls I dictated--to Joe Kennedy I wrote:

We have signed the enclosed Supplemental Application in order that the matter might proceed without delay, although Emilio has raised a question which we should consider before this document is actually submitted. We feel that careful consideration should be given to the question of whether we are on firm ground in including a claim to case S-61, because we might suffer a setback if we are not. We must be sure that our assignment of case S-61 was part of the consideration for contract eng-30. On checking the wording of eng-30, it appears that this is the case, but we feel that in order to be sure we should find out whether the assignment of case S-61 was made after the date of signing of eng-30, and hence, obviously as a consequence of eng-30. If case S-61 was assigned at some time previous to the signing of eng-30, we should probably want to give careful consideration to whether the inventors gave something to the Government which they might not easily be able to, or want to, take back. I am sending a carbon copy of this letter to Mr. Lane with the hope that he will be able to obtain information on the date of our assignment of case S-61 and inform us.

I informed Jim Norton that G. Don Paxson of his electronics department approached us about joining our group. Since we are losing one of our men (Gilbert O. Brink), Paxson's request is timely, and we would like him to make a full transfer effective December 1.

Paxson



I read a November letter from Darrah Thomas, who described his financial problems and said that Dean Pitzer suggested he might win a National Science Foundation Fellowship, which would be better from the financial point of view. Thomas asked for my approval or any other ideas. I replied that I am in complete agreement with his plan, but our previous offer will remain open if he should not get the fellowship. I suggested that he should proceed with the security clearance.

Later I reported on "Nuclear Chemistry at the Radiation Laboratory" to the Audit Subcommittee of the California Legislature at a lab meeting.

About 4 p.m. I telephoned Rod Spence to tell him that some of our fellows had definitely identified an element 99 isotope from  $U^{238} + N^{14}$ , declassified it, and sent a note to The Physical Review. I said the article was played down, definitely not a discovery paper with no name for element 99 suggested, and that the authors are not the same as the discovery authors. I cited the precedent in the case of americium.

Spence agreed with the procedure and thought that this would react to speed up the possibility of the declassification of the discovery work. He said that he will talk with Ralph Carlisle Smith about it. I mentioned that Beckerley was now anxious to proceed with the declassification of the discovery work.

Thursday, November 26, 1953 (Thanksgiving Day)

Helen prepared a traditional turkey dinner with trimmings for the family today. The kids seem free of the mumps, so Helen invited the Thompsons (Stan and Alice and daughters, Ruthie and Joyce), to join us.

Friday, November 27, 1953

Before I left for the lab this morning, Helen announced to me that she is afraid that she is getting the mumps.

At the lab I phoned Spof English, Dan Wilkes, et al. and then took care of some of my correspondence. I mailed Winston Manning a copy of UCRL-2411 ("Reactions of  $U^{238}$  with Cyclotron-Produced Nitrogen Ions" by Albert Ghiorso, G. Bernard Rossi, Bernard G. Harvey, and Stanley G. Thompson). I wrote that this is written in this de-emphasized manner because the authors are, of course, not the discoverers of element 99, whose discovery we date as December 19, 1952.

On November 11 Francis (F. E.) Blacet, Chairman of the Department of Chemistry at UCLA, wrote:

The time has come when the older staff members must make a decision concerning the future of Ralph James in the Department. In most categories we have no difficulty in making an evaluation. It would be helpful, however, if you could give us your impression as to how well his contributions, made while here, are generally regarded in the field. You probably know all the young men who have gone into academic work after the war and have started more or less from scratch in nuclear chemistry research. How does his contribution compare with others of his age group, both in quantity and quality? Compared to six years ago, how do you feel concerning his probable future contributions in the field?

I know this sort of puts you on the spot but, at the same time, anything you could tell us would be helpful.

I have given much thought to the performance of our Ph.D.'s in the academic world, and today I sent the following to Blacet:

November 27, 1953

Professor Francis Blacet, Chairman  
Department of Chemistry  
University of California  
Los Angeles 24, California

Dear Francis:

Your letter of November 11, 1953 asking for my evaluation of Ralph A. James does, as you say, put me on the spot. My answer, in full justice to Ralph, will be rather lengthy, and probably from your point of view, not entirely clear cut.

I shall begin by saying that he has not done as much as I expected he would when I recommended him to you about six years ago. However, I expected a great deal, and I think that we should try to analyze his output in terms of the potentialities at U. C. L. A., and by comparison with other people in similar situations.

To begin with I feel that it is worthwhile to summarize his output of publications. Actually, his total number of publications is not at all bad for his stage, but only one of them, from my hasty survey, is based on work done at U. C. L. A. He was co-author of five exceptional papers (Nos. 19.1, 22.1, 22.2, 22.8, and 22.16) published in the National Nuclear Energy Series, Plutonium Project Record, Volume 14B, which appeared late in 1949. This was followed by another notable contribution in the PHYSICAL REVIEW (78, 472) early in 1950. Then he had another good publication as the result of work here at the Radiation Laboratory in the PHYSICAL REVIEW (85, 369) early in 1952. His article in the PHYSICAL REVIEW with Tewes on "Proton Induced Reactions of Thorium-Fission Yield Curves" is his major contribution based on work performed at U. C. L. A. This is a very good piece of work, typical of Ralph. It represents a great deal of effort both in experimental work and theoretical interpretation. I am very pleased with it, and I have heard many favorable comments throughout the country concerning it. In his theoretical treatment involving reaction rate analogies he came very close to anticipating an explanation for the fission process later made by another scientist which is attracting a great deal of attention. As you know, the theoretical explanation of the detailed course of the fission process has been very difficult to arrive at and Ralph made a remarkable contribution here considering the facilities at his disposal and the fact that it was practically a one man job.

As you suggest, a proper evaluation can be made only by comparison with other young men in the field. This I feel can be done properly by making a comparison with others of the same

general age group in universities with similar facilities who are attempting to do their research work in what I would call straight nuclear chemistry. There aren't many such places, and, therefore, such a comparison is not too difficult. (Incidentally, I might add that I very much approve of U. C. L. A. having a program in straight nuclear chemistry in conjunction with the cyclotron.)

On the West Coast the University of Washington has a cyclotron comparable to that of U. C. L. A. but has not yet succeeded in getting a nuclear chemistry program underway; their cyclotron has not yet been used by the chemists. (Incidentally, I might add that none of the other major universities on the West Coast who might be comparable but perhaps do not have quite equal facilities, namely Stanford, Cal Tech, and U. S. C., has a nuclear chemistry program.) Going on to the Midwest we have Washington University in Saint Louis where Kennedy and Wahl have used the cyclotron very little, bending their efforts more in the direction of those of Cliff Garner, that is, to the use of tracers to solve chemical problems. Cornell hired a good nuclear chemist about five years ago, and when he didn't seem to come up to their expectations they hired another man several years ago to replace him; he in turn has now left them, presumably due to the mutual difficulty of working out a satisfactory nuclear chemistry program. Princeton hired a good nuclear chemist about five years ago, and he left about a year ago due to the difficulty of getting a program underway there that suited him. Harvard hired Dick Diamond a couple of years ago who is getting a program in nuclear chemistry underway there, but his start has been rather slow and they have asked me to give an evaluation and prognostication in his case exactly in the manner that you have asked me to do for Ralph James.

Since these were all promising young men, the question arises as to why the satisfactory program in nuclear chemistry did not materialize in each case or at least as to why the departments did not feel that such a program had come about. Apparently, it is very difficult for these men to go into a chemistry department and start essentially from scratch in nuclear chemistry research with limited facilities and with very little opportunity for the continuous exchange of ideas in as fast a moving field as nuclear chemistry. The equipment is rather complicated and is probably often somewhat minimal so that the man is usually at quite a disadvantage with respect to what was available at the place where he received his training. However, I believe that the importance of nuclear chemistry is such that any first class university should attempt to carry on a program in spite of these obstacles.

The foregoing bears on the comparison of the contributions of Ralph with others of his age group. I think that he is doing as well as anybody in a strictly comparable position who is confining his research efforts to the nuclear chemistry field. If we stay with the universities, one has to go to the larger nuclear laboratories, say Chicago, Columbia, and California, before you get better performance by men in very broadly the same age group and this in many cases is due to the better facilities, including larger groups for the exchange of ideas, and, of

course, in some cases, due to the fact that these universities have the very top men in the country who cannot be moved. If you go on to the national laboratories for a comparison, I can only think of a couple of men in the same general age group whom I would rate above Ralph. If it should come to the point where you would be interested in these names, I should be glad to supply them, although one of them would probably not be available even for a full professorship. I might add that I would be very happy to make as unbiased a comparison as possible between Ralph and any other young man who might come to your attention, if you should wish me to do so.

Among the important factors, with which of course you are familiar, and which should come into any overall evaluation, are the questions of whether Ralph's teaching loads have been heavy and whether he has been teaching courses which vary from year to year, since these certainly can detract from a man's research program. Also important is the matter of support via equipment, electronic assistance, etc., and the general availability of graduate students.

Your question as to his probable future contributions is exceptionally difficult to answer because I have not had a chance to talk with Ralph for such a long time. I saw him briefly during my visit to the UCLA campus last March, but we didn't get around to discussing his research plans. I feel confident that he will continue to make future contributions, but it is difficult to guess whether the quantity will be up to your expectations. I somehow feel that we will probably never have to worry about the quality.

I know that I have given you some tangential answers here, but as I said at the beginning, it is a complicated situation and I don't see how I could do full justice to Ralph in a much shorter communication. If you were to ask me directly whether I would recommend that you should keep James by advancing him to an Associate Professorship, or whether you should replace him by attempting to find a better man, I would say that my recommendation would be, in the balance, to keep him. If you decide to do this, possibly you can find ways and means of increasing the support for his work, increasing his outside contacts, arranging for him to have more graduate students, decreasing his teaching load and in general encouraging him to a greater quantity of output. For example, we would be happy and consider it profitable to us to have Ralph visit us several times during the year and perhaps spend an occasional summer here.

Cordially yours,

Glenn T. Seaborg

GTS/lmh

In my rounds of the labs I noted that Bernie Harvey and Chetham-Strode, with the assistance of a couple of health chemists, are working up a plutonium bombardment from Hanford.

At home I received a November 23 letter from North American Aviation, Inc. (Downey, California) outlining the terms of a consulting contract with the company.

Helen definitely has the mumps--she told me that she had phoned Alice Thompson with the news so that Stan, who is uncertain about his medical background, can get a shot of gamma gobulin.

Saturday, November 28, 1953

Helen was so miserable that she collapsed into bed in the room (bedroom #2, Stephen's room, see sketch following November 30 entry) next to the playroom--I was a bit desperate until Helen suggested that I call Miss Emma Stelzer (baby nurse for Lynne and Dave) and arranged for her to help us out for a few days. I drove to Berkeley to pick her up.

Sunday, November 29, 1953

While Miss Stelzer took care of the kids, I read in my study. The 49ers played the Colts in Baltimore, winning by a score of 38 to 21.

Monday, November 30, 1953

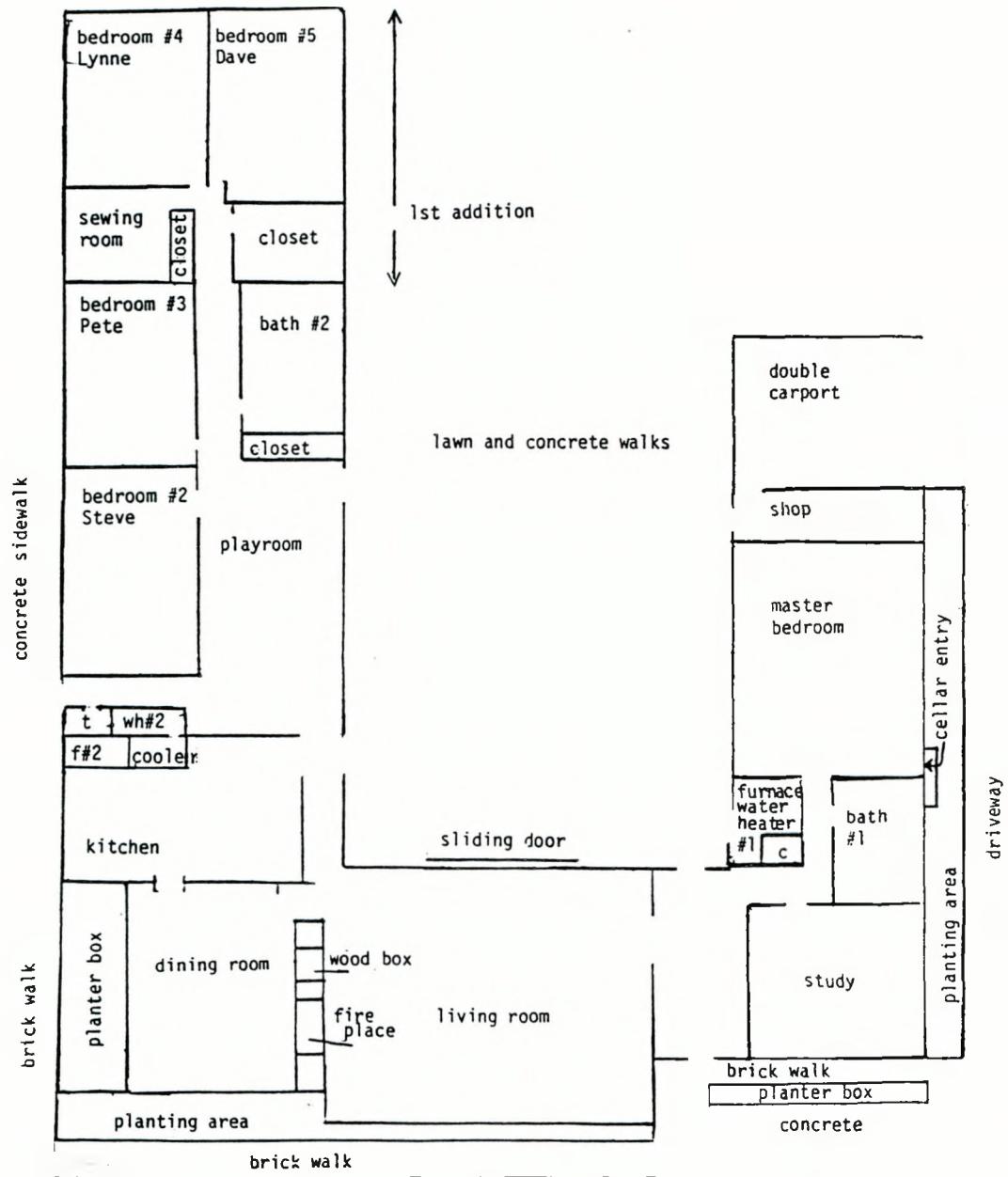
Included in this morning's phone calls was one to Hurford Stone (Dean of Students) about some eligibility matters, one to Bob Thornton, another to Ernest Lawrence, one to Dan Wilkes about some publicity items, and one to J. Flaherty (AEC, San Francisco).

John Huizenga wrote on November 25 to thank me for my letter of November 25 in which I referred him to Elmer L. Kelly's thesis. John noted that he wrote Kelly early in the summer for a copy but didn't get it until a couple of weeks prior to my letter since Kelly had been away from Berkeley. John added that they have been concentrating on the proton excitation functions of bismuth with low energy protons, a region in which Kelly's results were very preliminary.

A James R. Griffith, a senior at Birmingham-Southern College in Birmingham, Alabama, wrote on November 23 to request information about graduate work here. Griffith mentioned that one of his chemistry professors, Dr. Harold E. Wilcox, is presently studying here, and so I asked Doral to check to see if she could locate Wilcox for me.

I received the following letter from Donald Lane:

Sketch of floor plan of home at 1154 Glen Road, Lafayette, California:



LEE B. KEMON  
DONALD E. LANE  
SOLON B. KEMON

TELEPHONE  
DISTRICT 7-3870

LAW OFFICES OF  
LEE B. KEMON  
AND  
DONALD E. LANE  
PATENTS AND TRADE-MARKS  
1331 G STREET, N. W. WASHINGTON 5, D. C.

November 27, 1953

19138 11/30/53

Action	_____
Recd. NOV 5 1953 GTS	_____
Ans.	_____
File	_____

Dr. Glenn T. Seaborg  
University of California  
Radiation Laboratory  
Berkeley 4, California

Re: AEC Docket No. 7

Dear Dr. Seaborg:

My copy of your letter of November 25, 1953 to  
Dr. Kennedy has been received.

Case S-61, application S.N. 634,311, filed  
December 11, 1945, was assigned to the Government of the  
United States by individual assignments executed on  
26 October 1945 by Kennedy, Segré, and Seaborg. These  
assignments apparently made on mimeograph forms do not  
make any reference to eng-30. These assignments were exe-  
cuted subsequent to the date on which eng-30 was signed,  
17 September 1945, and I believe that we should contend  
that S-61 was assigned "pursuant to eng-30". You have  
already noted that eng-30, paragraph 3, recites the agree-  
ment to assign rights "including the subject matter of  
Case S-61".

The copies of assignments furnished to me by

Mr. Anderson for S-61 identify the application only by title, Production of Energy, and by the execution date of the application, 26 October 1945, and said title and execution date agree with that of the copy of the S-61 application also furnished by Mr. Anderson.

The assignment in S-5617, the division of S-52d, includes the recital of eng-30.

I expect to be able to forward at least to Dr. Kennedy early next week all claims or typical claims of the S-61 and S-5617 for inclusion in the claim summary sent November 24, 1953.

Very truly yours,

*Donald E. Lane*

DEL.mb

cc: Drs. Segre, Kennedy  
and Wahl

AIRMAIL

At our noon senior staff meeting a number of matters were discussed--Surls' grades, the NSF fellowships, plutonium metal (I asked Earl to check into this), the heavy ion linear accelerator, etc. I visited some of the labs and talked with some of the students, such as Chetham-Strode, about their research.

Eventually, I went down to campus for a 4:30 p.m. meeting in the Faculty Club of a group of golf enthusiasts, the University of California Faculty Golf Club. Members include M. W. Allan, Luis W. Alvarez, Lloyd B. Bernard, George F. Break, Norman Buchanan, A. J. Carlson, W. G. Dauben, Robert M. Drake (President), H. W. Edwards, Leonard Farber, Walter Galenson, Clyne Garland, Luther Gilbert, Harry Kingman, M. M. Knight, Mike Koll, Sherman Maisel, Stan McCaffrey, Capt. McGregor, John Michaelis, James Miller, M. P. O'Brien, George Pettitt, Robert J. Riddell, Jack Rogers, Earl R. Rolph, A. H. Schaff, Sam A. Schaaf (Chairman Handicap Committee), Glenn T. Seaborg, O. Guinn Smith, Herman A. Spindt, Laurence Talbot, Ian Turner, Harvey White, C. R. Wilke, Scott Wilson, and Ray Wood. Members will be provided with a list of golfers, addresses and phone numbers, days on which golfer can play, and handicaps.

Tuesday, December 1, 1953

After the usual number of phone calls, including one to Segre about our patent compensation case, I wrote to Donald Lane and said that Segre and I now agree to his inclusion of Case S-61 in the Supplement Application.

I also completed a recommendation for Geoffrey Wilkinson, who is applying for a Guggenheim fellowship in order to study with Professor Bjerrum in Copenhagen. I wrote, in part,

Since leaving us Dr. Wilkinson has returned to the field of inorganic chemistry and has carried out some rather interesting and significant contributions on complex compounds of the transition elements. In particular, his work on the metallo-organic compounds of cyclopentadiene has been of considerable importance. This apparently is the study Dr. Wilkinson has in mind to continue and to extend during his stay in Professor Bjerrum's laboratory in Copenhagen. I am certain the contact with Professor Bjerrum's group would prove stimulating and useful to Wilkinson.

Dr. Wilkinson is a thoroughly trained scientist with an impressive variety of accomplishments as demonstrated by his past publications. He is capable of initiating and completing a research program of considerable importance.

On the personal side, he is a splendid person who would be a good representative of his academic and professional colleagues.

I then went down to campus to give the Chemistry 123 lecture.

Al Ghiorso and I spent the afternoon in Tilden Park for golf (AG-97, GTS-93) and conversation.

Wednesday, December 2, 1953

Stan Thompson decided to apply for a Guggenheim fellowship in order to cover his travel expenses to Sweden, and my report to the Committee today read:

I have known the candidate Dr. Stanley G. Thompson during his entire research career and have been in a position to observe his performance and accomplishments firsthand during this period. I can recommend him with the greatest enthusiasm and absolutely without reservation. I believe that Dr. Thompson's case is an excellent example of just the type of man and career that the Guggenheim Foundation should support.

Dr. Thompson's research career effectively began when he joined my section of the wartime Metallurgical Laboratory of the University of Chicago during the summer of 1942. The chief responsibility of this section was to conceive and explore in the laboratory methods for the chemical separation and isolation of plutonium from uranium and tremendous fission product radioactivity produced in the chain reacting pile. Thompson conceived the process which was eventually

adopted for this separation and first installed at the pilot plant in Oak Ridge, Tennessee, and then later used for the manufacture of plutonium at the Hanford Engineer works in the state of Washington. After his conception and early laboratory demonstration of the process, working essentially by himself, the importance of his idea became clear to those charged with the responsibility for the development of this plutonium separation and decontamination process, and he was placed in charge of the laboratory program for its development. This entailed the direction of a group of about thirty-five scientists, a responsibility which Thompson carried on with the greatest efficiency. I believe that Thompson's achievement in conceiving and carrying to laboratory completion this plutonium chemical process was one of the outstanding scientific achievements of World War II and an achievement which has been largely unheralded due to secrecy requirements and the innate modesty of Dr. Thompson. Following the completion of the laboratory investigation at the Metallurgical Laboratory, Dr. Thompson was chosen by the du Pont Company to head up the most essential portion of the chemical development research at the Hanford Engineer Works during the critical startup time of the plant in 1944-45.

Following the completion of his assignment at Hanford in 1945, Thompson returned to the Metallurgical Laboratory to begin the basic scientific aspect of his career. The first problem that he tackled was the hitherto unsolved one of separating the newly discovered elements americium (atomic No. 95) and curium (No. 96) from the rare earth elements, which he accomplished as the result of his extraordinary chemical ingenuity. When I selected a number of the outstanding members of the group to move back to the University of California in 1946, Thompson was of course included. Although he had directed the research of many Ph.D. chemists, he had not yet obtained his Ph.D. so he undertook his graduate work under my direction at this time. He undertook the extremely difficult assignment of searching for the then undiscovered element No. 97 as his doctorate thesis problem. It is characteristic of his energy and determination that he should have been willing to undertake a problem of this magnitude for his thesis work. Actually, this problem was not completed in time to serve as a thesis, which was written in 1948 on his fine work on the chemical properties of americium and curium. However, he continued to solve the many difficulties attendant with the general problem of identifying transcurium elements. This culminated in the discovery of element 97 (berkelium) in December 1949 and element 98 (californium) in January 1950, and he was the key man in this work. This, of course, is the high point in his research career, a discovery of great scientific and historical importance.

Thompson, who received a permanent staff appointment with our nuclear chemistry group in the Radiation Laboratory following the receipt of his Ph.D. degree, has numerous other important research accomplishments to his credit as his list of publications shows. Among these are the discovery of numerous heavy isotopes in the transuranium region and detailed studies of the chemical properties of berkelium and californium. Worth special mention is his key work on the discovery of artificially produced alpha radioactivity in the rare earth region relating to the closed shell at 82 neutrons. This

was the first production of alpha radioactivity below the heavy region; in fact, the only natural alpha radioactivity previously known in this region was that of  $\text{Sm}^{147}$ .

I believe that Thompson has proposed a very worthwhile project for his study at the Nobel Institute for Physics at Stockholm. This should make it possible to bring together for the first time his great chemical ability and ingenuity and the advanced instrumentation and techniques in beta, gamma and, more recently, alpha ray spectroscopy which have been developed at the Nobel Institute. This should yield research findings which otherwise would be impossible for some time to come, greatly advancing the whole area of heavy isotope systematics, as well as furnishing details of energy levels, closed shells, and general nuclear structure in the region of the heaviest isotopes. Dr. Thompson should profit very much personally by working in an outside laboratory because all of his scientific career so far has been confined to working within our group.

In conclusion, I want to emphasize that Dr. Thompson is a chemist with extraordinary manipulative ability and ingenuity with a good theoretical understanding of the meaning of his experimental results. He must be rated as one of the few top nuclear chemists in the world. He has a very pleasant personality, gets along well with people, and I believe he would be an excellent ambassador of good will for the United States. The problem which he has proposed is very worthwhile and one which can be uniquely accomplished with his training and the facilities which will be available. He will personally profit very much from the experience. I believe that it would be difficult for the Guggenheim Foundation to find a more worthwhile candidate for its support.

Other letters this morning went to Professor Eugene G. Rochow (Harvard University), who had asked for an evaluation of Richard Diamond in order to determine whether Diamond should be retained at Harvard. I asked Rochow if I could defer my response until after I speak with Diamond during his visit to Berkeley next month. If this is not satisfactory, I wrote, I shall attempt to do the best I can without an interview.

I also declined an invitation from Professor W. E. Groth (presently visiting Harvard) to visit Germany (Bayreuth) in May, 1954. I wrote that I am planning to attend a meeting of the International Union of Pure and Applied Chemistry in Switzerland during the summer of 1955, and I should like to visit Germany at that time if it can be arranged.

At 10:30 a.m. Jonathan Rice of BAETA/KQED and I went to see a film. We then had lunch at Larry Blake's, along with a discussion about plans for KQED. In the afternoon I had been scheduled for Warren Smith's prelim examination, which was postponed; so I went out to Livermore with a number of other people from Berkeley.

Thursday, December 3, 1953

Today's open meeting of the research group was attended by Behman, Bryan, Carr, Chetham-Strode, Cobble, Gray, Grover, Hollander, Huffman,

Hyde, Jaffe, Kalkstein, La Salle, Lokken, Maier, Mathur, Michel, Passell, Perlman, Rasmussen, Schooley, Seaborg, Michael Edward Senko (a graduate student from Purdue University), Slater, Smith, Stephens, Stoner, Sweeney, and Templeton.



Michael Senko

Mathur described his discovery of the 55-second isomer of  $Xe^{125}$  in the recoils of  $Cs^{125}$ , studied in the 60-channel pulse analyzer. The  $Cs^{125}$  was prepared by the bombardment of  $I^{127}$  with helium ions. Mathur said that decay showed that a good part of the gamma radiation at 30 kev (x-rays), at 75 kev, and at 110 kev was due to  $Xe^{125m}$  while other peaks are due to 18-hour  $Xe^{125}$ . He showed an energy level scheme for  $Xe^{125m}$  and said that, assuming one gamma ray of 185 kev per disintegration of 18-hour  $Xe^{125}$ , the branching ratio of the decay of  $Cs^{125}$  into 55-second and 18-hour isomers has been determined to be  $10^{-3}:1$ . Mathur went on to talk about the 4-hour isomer of  $Nb^{89}$ , previously seen by Kofstad and by Hyde. Mathur produced it by the helium ion bombardment of  $Y^{89}$ . He milked the 4.4-minute  $Zr^{89m}$  and the 78-hour  $Zr^{89}$  from the purified niobium fraction of a bombardment of niobium with protons and obtained evidence of a 55-minute  $Nb^{89m}$  and a 4-hour  $Nb^{89}$ . Mathur said that  $Nb^{89}$  decays to  $Zr^{89}$  and that  $Nb^{89m}$  decays to  $Zr^{89m}$  with  $Nb^{89}$  being formed in greater yield by both methods; he added that when other target elements are used, they were unable to observe the  $Nb^{89m}$ . He presented a decay scheme.

Stephens discussed his and Asaro's work on  $Th^{231}$ , previously studied by F. Wagner, Jr., et al. at Argonne, and whose decay scheme is questioned when one considers other odd-even nuclides in the region. Stephens presented the results of their investigation of the gamma-ray spectrum and coincidence work and provided a corrected decay scheme.

Rasmussen talked for a while on the problem of spheroidal distortions of nuclei (Hill and Wheeler) and the effect on alpha decay.

\* \* \* \* \*

I spent some time on campus before going up to the hill.

Friday, December 4, 1953

Since I will be busy next week, I tried to clear some of my pending actions off my desk. I made a number of phone calls and talked, for example, to Lloyd Smith about the heavy ion linear accelerator plans, to Robert E. Robinson (Santa Barbara) about arrangements for my talk there on January 13, to Cornelius Tobias about heavy ion biological effects, and also to George Briggs about some athletic scouting problems.

One of the girls in the office managed to locate Harold E. Wilson, mentioned by potential graduate student James R. Griffith as a reference. Wilson is here as a Ford Fellow in the Virus Laboratory--and he recommended Griffith to Iz in a phone conversation. As a result I wrote to Griffith and described the process for admittance to graduate school--explaining that he should write to Professor K. S. Pitzer if he is interested in nuclear chemistry or to Professor E. M. McMillan if his interest is nuclear physics. I then told him about the research program at the Radiation Laboratory, including security clearance, etc.

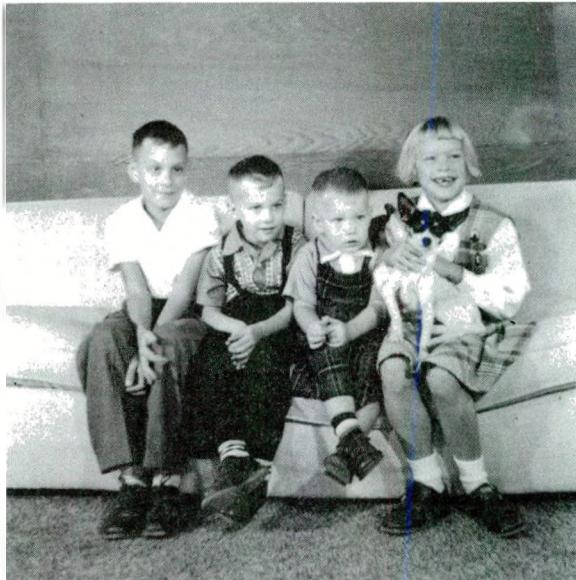
Today a notice arrived from W. S. Bigelow in Personnel, notifying us of a Q clearance, dated November 30, for Arnold Rudolph Fritsch. Fritsch is a graduate student from the University of Rochester and has elected to do his graduate research in nuclear chemistry (with Perlman).

As usual, I went through the labs to talk with the various researchers and to look over the work. In Room 105 of Bldg. 5 Crane, Higgins, and Hulet were involved with opening some curium capsules containing gas. I was told that the amount of curium is about  $10^{13}$  c/m and there is about 1 R (per hour) coming through the box.

Saturday, December 5, 1953

Much of my day was taken up with the family since I shall be away from home much of this coming week.

Helen recovered quickly from the mumps and has resumed her maternal duties (Miss Stelzer has returned to Berkeley). Presently Helen is addressing Christmas cards; she includes a picture of the kids for our out-of-town friends.



Peter, David, Steve, and Lynne (with Cricket), 1953

Sunday, December 6, 1953

After spending much of the day with my family, I went to the Clift Hotel in San Francisco for the Winter Meeting of the Pacific Coast Athletic Conference. The member institutions were represented as follows: University

of California--Professor Glenn T. Seaborg (Faculty Athletic Representative, F), Brutus Hamilton (Athletic Director, D), George Briggs; University of California at Los Angeles--Professor Claude Jones (F), Wilbur C. Johns (D), William Putnam, Harry Morris; University of Idaho--Dean Thomas S. Kerr (F), Gale L. Mix (D); University of Oregon--Dean Orlando John Hollis (F), Leo A. Harris (D); Oregon State College--Professor Glenn W. Holcomb (F), R. S. Keene (D); University of Southern California--Professor Hugh C. Willett (F), Willis O. Hunter (D), Alan Ewenk; Stanford University--Professor Rixford K. Snyder (F), Alfred R. Masters (D); University of Washington--Professor H. P. Everest (F), Harvey Cassill (D); and State College of Washington--Professor Emmett B. Moore (F), Dean Golden Romney (D). Also present were Commissioner Victor O. Schmidt, Supervisor of Officials Frank G. McCormick, and Director, Service Bureau, Bernard Hammerbeck.

President Dean Kerr called the first session, a joint meeting of Faculty Representatives and Athletic Directors, to order at 8:05 p.m. Harry Morris, Director of Publications for UCLA and 1954 Rose Bowl Game Manager, was attending the meeting for the first time and was introduced. Commissioner Schmidt read his report, and the chairman of the Commissioner's Advisory Committee announced the schedules of the various sessions, along with the assignment of agenda items. We approved a motion authorizing the Advisory Committee to designate the items requiring early attention and permitting the Faculty Representatives to add other agenda items to the preferential list. Before we adjourned at 9:30 p.m., President Kerr extended the best wishes of the Conference to UCLA in the Rose Bowl game on New Year's Day. (UCLA qualified for the Rose Bowl with a 13-0 win over USC, helped by Berkeley's elimination of Stanford by achieving a tie.)

[Although I did not attend, the 49ers defeated the Green Bay Packers in Kezar Stadium today by a score of 48 to 14.]

Monday, December 7, 1953

In San Francisco. The Faculty Representatives met at 9:05 a.m. and discussed student petitions and eligibility cases. We also heard the report on the USC case, which had been referred to the President of USC. In this case we were presented with a letter, dated December 1, 1953, from Jess Hill (Head Football Coach) and addressed to Dr. Fred D. Fagg (President of Southern California):

I should like to take this opportunity of presenting my philosophy regarding athletics, and in particular concerning football here at the University of Southern California.

First and foremost, I should like to make it very clear that at no time, either as a player or as a coach, have I ever condoned unfair, illegal, or unsportsmanlike playing on the part of the men who have represented the teams that I have played on or coached. In fact, I have always insisted upon ethical conduct according to the letter of the rules as well as the spirit of the rules.

I understand that I have been accused of making certain unguarded statements from the sideline during the progress of our California game in 1951 and our Stanford game of 1952, advising our players to "go out and get certain players" of the California and Stanford football teams.

I resent such allegations and deny ever having made such statements either on the sidelines or elsewhere to that effect, because such illegal tactics are not a part of my coaching philosophy. Furthermore, I know that all of my assistant coaches have the same respect for members of other football squads as we do our own.

My entire coaching staff and I have done everything possible to instill in our players a sense of hard, aggressive, legal, and sportsmanlike fair play. As long as I am employed by the University of Southern California in the capacity of head football coach I intend to maintain such ethical standards of integrity, and if at any time I feel that I have lost the proper perspective regarding aggressive and ethical football I shall immediately tender my resignation.

I believe in playing football as hard, as aggressively and as cleanly as possible and I insist upon our players giving their utmost at all times. My own philosophy regarding athletics, and in particular football, has been sustained as a player and as a coach for over thirty years. I shall always do my utmost to see that it never changes as far as the ethical side of football is concerned.

We adjourned at 11:00 a.m. and then reconvened at 11:05 a.m. with the Directors, who were directed to present the 1957 football schedule to this meeting. We again considered special petitions--the petition of UC student Nathan Short that his competition in baseball for the 1952-53 season be disregarded was denied by unanimous vote. Also defeated was a motion to amend Section 5.17 of the Code to permit the Conference representative to the Rose Bowl to give two complimentary tickets to members of their junior varsity football squad not included in that squad selected for preparation for the Rose Bowl game and to members of their freshman football squad. The session adjourned for lunch at 12:10 p.m.

The Faculty Representatives reconvened at 2:10 p.m. and again considered the matter of illegal football play (USC), which had been referred to the President of USC. A number of other alleged illegal activities were also considered. At 4:10 p.m. we again met with the Athletic Directors. First there was an appearance of representatives of the Tournament of Roses Association. President Kerr introduced Lathrop Leishman, who in turn introduced William Nicholas and John Biggar of the Football Committee, Elmer Wilson (Vice President) and Harry Hurry, who as President of the Association, extended an invitation to all members of the Conference to attend the New Year's Day festivities. In other matters we defeated a motion to use five officials in freshman football games (UCLA, USC, Oregon voted for), and we unanimously carried a motion that the home institution in all Conference football games allow the visiting institution 150 mutual complimentary tickets. It was decided that the 1954 PCC Track Meet will be held in Seattle on May 28 and 29, 1954. The Commissioner was instructed to communicate with the Big Ten Conference and establish the date and place of the Big Ten-PCC Track Meet after the site of the NCAA Championships has been determined in January 1954 at the time of the NCAA National Convention. Other motions that carried unanimously were to hold the 1954 North-South Basketball play-off at the site of the Southern champions on March 5 and 6, and March 8, if necessary, and to hold the 1954 baseball play-off on May 28, 29, and 31 at the site of the winner of the Northern Division. After a few other decisions, we adjourned at 5:10 p.m.

We held our usual Monday social hour of the Faculty Athletic Representatives (Vat 69 Club) at which no business is discussed so as not to interfere with the ritual of cocktail consumption.

Tuesday, December 8, 1953

In San Francisco. The Faculty Representatives met for the sixth session at 9 a.m. and again considered the reports from the member institutions on matters referred to the Presidents for discipline, attention, and report. In addition, there was further consideration of the USC case of illegal football play. Also discussed were various amendments to the Athletic Code. At 11:05 a.m. we met with the Directors. Rix Snyder moved, and I seconded, that the case of possible illegal football play by the team of a member institution (USC) be closed. This carried unanimously. (I do not feel that Jess Hill's letter to President Fagg to be sufficient response to the Cannamela incident; however, at least the incident and Hill's attitude has the disapproval of most of the Conference members.) USC Faculty Athletic Representative Hugh Willett, speaking with emotion, thanked me for agreeing to close the case.

A number of cases of alleged rule violations by member institutions and referred for action to the Presidents were closed. Other matters at this session included unanimous approval of a motion that, in the event that the Commissioner is not in attendance, the President of the Conference and the Secretary of the Conference, in order, be designated as official alternates to the NCAA Convention. Final action was taken on several proposals for amendments to the Athletic Code. We carried (UCLA, USC, Idaho against) a motion to incorporate into the Code the privilege of members of the Conference to cancel all athletic schedule commitments with a member institution placed on probation because of violation of rules and regulations of the Code. Included in this motion was a provision requiring members of the Conference to cancel all athletic schedule commitments and to make no future commitments with a member institution expelled from the Conference until the expelled institution is reinstated as a member of the Conference. The motion also requires members of the Conference to cancel all athletic schedule commitments with a member institution who withdraws from the Conference. We adjourned at 12:00 noon for lunch.

The Faculty Representatives met for the eighth session at 2:05 p.m. and then with the Directors at 4:10 p.m. for the ninth session. At this session a motion carried unanimously to approve the Gillette Razor Company as sponsor of the 1955, 1956, and 1957 Rose Bowl radio and television broadcasts. There was also final unanimous approval on instructions to our delegates to the NCAA Convention: (1) oppose the adoption of the amendment which would change eligibility requirements for institutional membership (designated as I in Proposed Amendments), (2) oppose proposed amendment having to do with playing and practice seasons insofar as it introduces new legislation, and (3) instructed our delegates, except as specifically instructed, to use their own best judgment. In other action the Code was amended to permit the issuing of not to exceed 30 non-mutual complimentary tickets to prospective athletes who have been invited to visit the campus for home athletic contests (Oregon voted against this amendment, and I abstained).

On our way to dinner I noticed newspaper headlines proclaiming President Dwight D. Eisenhower's announcement of an international Atoms for Peace program.

Wednesday, December 9, 1953

In San Francisco. This morning's session of Faculty Representatives began at 9:05 a.m., and consideration was given to a number of amendments to the Code. At 11:05 a.m. the Directors joined us, and final action was taken on a number of the proposed amendments to the Code, such as approval to the amendment of Section 9.07, which adds the sentence, "No student, eligible for varsity competition, shall participate in more than a total of 27 basketball games, regardless of whether they be varsity, junior varsity, or a combination of both,--Conference and NCAA playoffs excepted" and approval of an addition to Section 9.08--"No student, eligible for varsity competition, shall participate in more than a total of 30 baseball games, regardless of whether they be varsity, junior varsity, or a combination of both,-- Conference and NCAA playoffs excepted." There was also unanimous approval for the recommendations of the Directors regarding the television policy for 1954, which included such points as: (a) That the member institutions be permitted to cooperate with the NCAA in such a controlled television program as may be approved by its (NCAA) membership for the 1954 football season, (b) That member institutions be permitted to televise sold-out games on such bases as may be prescribed by the NCAA TV program as adopted for 1954, (c) That member institutions be permitted to dispose of rights to live or post television for all sports except football for the academic year 1954-55 on the same basis as permitted by last year's rules and likewise in conformity with such regulations as may be included in the next year's NCAA television program. Final action of the morning was to approve, in principle, a resolution to the NCAA to urge the continuance of the present football rules.

The afternoon session began at 2:05 p.m., and we considered and studied a number of items. Then at 4:10 p.m. we again met with the Directors. During this session a motion carried that the Commissioner be instructed to study the matter of touch football further, consult with the coaches, and bring to the Spring meeting a further report on the matter, thus postponing Agenda Item 22 until the Spring 1954 Meeting. There were other actions on the Code; again some of the matters were postponed until the Spring 1954 Meeting, such as action on (a) Section 3.09, (b) Section 3.11, (c) The possibility of eliminating correspondence units from consideration for Conference purposes, and (d) Possible use of UC Extension Division work as permissible for use by Stanford and USC in applying Conference rules. The thirteenth session adjourned at 5:05 p.m.

Thursday, December 10, 1953

In San Francisco. At 9:00 a.m. the Faculty Representatives and the Directors were called together for the fourteenth session by President Kerr, again with all members represented. At this session we heard and approved the report of the special committee, which was chaired by Professor Willett, on problems in basketball. Two sections of the Code were amended: (1) Section 10.05 was amended to read "10.05-- Coaches--Criticism of Officials. No representative of a member institution shall criticize in any way the work of any official of an athletic contest, or the officiating of athletic contests in general, except to staff members of his own institution and to the Commissioner" and (2) A new section 10.06 was added to the Code as follows:

"10.06--Coaches--Contact with Officials. No coach or anyone connected with the athletic department of a member institution shall have any contact whatsoever either before, during, or after an athletic contest, with any official of the athletic contest except when requested by the official, or when permitted by the playing rules of the particular contest, or when necessary for the sole purpose of extending the usual courtesies expected of a host institution." In other matters we approved a motion giving the Commissioner sole authority and responsibility in the selection and appointment of football officials. We also approved a report of the committee, chaired by Professor Everest, on a meeting with the Big Ten, which culminated in the signing of a new contract for Rose Bowl games to be played in 1955, 1956, and 1957. Everest also reported on efforts to obtain more tickets for the Big Ten Conference. This session adjourned at 10:45 a.m., and the Faculty Representatives met alone at 10:55 a.m. to consider a number of items. We adjourned at 12:00 noon for lunch.

The Faculty Representatives met at 2:05 p.m. with Professor Willett (USC) absent. Professor Dean Hollis (Chairman of the Nominating Committee) presented the following nominations for officers for next year, all of whom were unanimously elected: President--Dean T. S. Kerr; Secretary--Professor Emmett B. Moore; Member of Commissioner's Advisory Committee--Professor Glenn T. Seaborg. At this point in the session Professor Everest announced the untimely death of Mr. Jeff Cravath, former head football coach at the University of Southern California, and said that a telegram of condolence had been sent to his widow and family in the name of the Conference. A few other items were considered, and the session adjourned at 2:45 p.m.

At 2:55 p.m. the Faculty Representatives met with the Directors. Although action on a number of items was postponed until the Spring 1954 Meeting, final action was taken on items including, for example, a paragraph added to Section 9.02 of the Code reading: "No student, eligible for varsity competition, shall participate in more football games in any football season than the number of varsity football games regularly scheduled by his institution, regardless of whether the games in which he participates be varsity, junior varsity, or a combination of both, allowable postseason games excepted." We agreed to authorize the Commissioner to destroy some old documents--interview statements of athletes interviewed by Ed Atherton in 1938 and 1939 and all correspondence up to and including December 31, 1952 dealing with schedules of trips, hotel accommodations, appointments of officials, etc. The Directors reported they had elected the following officers for next year: President--Mr. Wilbur C. Johns, Secretary--Mr. Alfred R. Masters, Member of Commissioner's Advisory Committee--Mr. Harvey Cassill. Finally, Professor Jones moved that the Conference extend its sincere thanks to the host institutions, Stanford University and the University of California, for their masterly arrangements in handling this meeting, and especially for planning the Conference dinner and the most enjoyable entertainment thereat, and that particular thanks be extended to the Faculty Representatives and Directors of the host institutions for the many courtesies. This motion carried unanimously. The 1953 Winter Meeting of the Conference adjourned "sine die" at 4:00 p.m.

[In Berkeley the research group had its usual Thursday morning meeting with the following people attending: Sylvia Mae Bailey (our new graduate student),

Sylvia Bailey



Bryan, Carr, Chetham-Strode, Clark, Cunningham, Dauben, Feay, Gray, Hollander, Hyde, Jaffe, Kalkstein, La Salle, Maier, Nervik, Passell, Perlman, Rasmussen, Ruben, Schooley, Stephens, Stoner, and Wallmann.

Maier went into some detail about the equipment he is building to determine the high temperature equilibrium between  $\text{NpBr}_3$  and  $\text{NpBr}_4$ ; this, he said, will be similar to that used by C. W. Koch for determining the equilibria between various lanthanide trichlorides and oxychlorides.

Clark described his work, technique and equipment, to determine the unique structure of auric trichloride, explaining to the group that this involves the determination of crystal parameters for all the atoms in one unit cell, in this case four gold and twelve chloride atoms.

Jaffe talked about the decay scheme of 270-day  $\text{Ag}^{110m}$ , which Kai Siegbahn reported as an isomeric transition because the K-L difference of the conversion electrons from the 0.12 Mev transition corresponded to silver rather than cadmium. Jaffe said that, since  $\text{Pd}^{110}$  is stable, one might observe palladium K x-rays on the bent crystal spectrometer; previous work set an upper limit of 3% for electron capture with both silver and cadmium K x-rays being seen. The intensity ratio of silver to cadmium lines is 2:1. Jaffe, using a  $4\pi$  counter and the proportional counter attached to a 50-channel analyzer, determined the absolute intensity of K x-rays as 0.037/disintegration and calculated the ratio of IT to  $\beta^-$  as 0.055, assuming the gamma ray is essentially completely converted. In response to a question from Perlman, Jaffe said that electron capture takes place in less than 0.5% of the disintegrations.

Hollander told the group that Ghiorso will report on some of the recent successful nitrogen ion work next week. He then described two bombardments of natural indium ( $Z = 49$ ) with nitrogen ions. In the barium fraction he found a 95-minute and a 12-minute (not  $\text{Ba}^{127}$ ) activity. The cesium daughter of the 95-minute barium is a 1.4-minute activity; the chain is probably due to mass number 126. The 12-minute barium may be  $\text{Ba}^{124}$ . He said that, since they didn't see any  $\text{Ba}^{127}$ , the ratio of the yields of the reactions  $\text{N}_2\text{n}/\text{N}, \text{N}_3\text{n}/\text{N}, \text{N}_5\text{n}$  could be approximately  $\leq 0.1/\sim 1/\sim 1$ . The bombardments were short in duration since indium has a low melting point.

\* \* \* \* \*

Friday, December 11, 1953

Although I have tried to keep up with the activities at the lab by phone this week, I am eager to talk with some of the fellows about President Eisenhower's announcement a couple of days ago about his "Atoms for Peace" program, promoting the peaceful uses of atomic energy. Hopefully, this will help counteract some of the publicity involving weapons.

Charles Wilke telephoned to tell me about a possible person to head the Project Whitney chemical engineering program. He is James C. Bresee, a chemical engineer who is head of the practice school (which usually has about 12 students) for general engineering at K-25 at Oak Ridge. Wilke said Bresee is about 30 years old and may be on leave from MIT. He added that Joe Burn of Union Oil is trying to hire him; Wilke volunteered to look Bresee over while he is on a trip East next week.

In addition to talking with some of the students and men about the research, I looked over some of my mail. Francis Blacet wrote on December 2 from UCLA to thank me for my letter about Ralph James and to say that it gives them a perspective of the nuclear field which they badly need and which should prove most helpful.

Iz and I also spent a bit of time preparing a midterm for next Tuesday's Chemistry 123 class.

Saturday, December 12, 1953

I spent considerable time with the kids today--Pete and Lynne are now sharing mumps.

Sunday, December 13, 1953

Although this is Lucia Day in Sweden, the custom of being served coffee and Lucia rolls in bed by beautiful girls has not followed us to Lafayette; Helen and I were not served rolls in bed this morning.

I spent some time with the kids; then Kalkstein, Hollander, and I went to Kezar Stadium to watch the 49ers defeat the Baltimore Colts by a score of 45 to 14. We stopped at Grisons for a steak sandwich before returning home.

Monday, December 14, 1953

This was a rather busy day with lots of phone calls, including calls to Chancellor Kerr and President Sproul about the PCC meeting, to Nello Pace about a couple of chemistry students, to Dan Wilkes with information about our heavy ion bombardment to produce isotopes of element 99 to enable him to prepare a press release, to Ken Street about the possibility of men such as Bresee for Project Whitney, etc.

Also, in today's mail was a response from Winston Manning about the manuscript I sent him on November 27 (paper by Ghiorso, et al. on nitrogen ion bombardment of uranium to produce isotopes of element 99). Manning wrote, "As you point out in your letter, the element 99 aspect is

somewhat de-emphasized in the the manuscript. As a matter of fact, without further explanation, this de-emphasis would probably be quite puzzling to many readers who follow the field reasonably closely. We recommend that some explanation be appended, perhaps as a footnote. The wording might approximate the following:

The work reported here is not the first resulting in the production of isotopes of element 99. Earlier work, not yet declassified, resulting in the characterization of isotopes of element 99, has been done at Berkeley and Argonne [alternatively, Berkeley, Argonne, and Los Alamos].

Such a statement would be noncommittal about the other questions of priority on element 99 and at the same time serve to keep the record straight until declassification and publication of the prior work. Jim Beckerley informs me that addition of such a statement to your manuscript would pose no classification problems...." I took the letter around to Al and Stan to read, discuss, and make a decision.

Today's mail also brought airline tickets for a round trip from San Francisco to Santa Barbara from Robert E. Robinson for my talk there on January 13, 1954.

In addition to Manning's letter, we discussed a number of other items at the brown bag luncheon meeting of the senior staff this noon, including a visit tomorrow by Maurice Goldhaber, Brink's acceptance into graduate school, O'Kelley's interest in a position at Arco, and Cunningham's plans to make measurements on 200 mg of neptunium in a  $\text{DNO}_3$  solution.

I also received a note from Charles D. Coryell, informing me that he is sending me under separate cover a report, "A Radiochemical Study of Fission Yields in the Region of Shell Perturbations and the Effect of Closed Shells in Fission" by Alexis C. Pappas, and a summary of a thesis by Donald R. Wiles entitled "A Study of Fission-Yield Fine Structure." Coryell used the reports to emphasize the opportunities in nuclear chemistry and to encourage the applications of graduate students at MIT.

Later in the afternoon I went through the labs to look over the research.

Tuesday, December 15, 1953

I responded to a couple of letters this morning: I declined an invitation from H. Marjorie Crawford to lecture at Vassar College later this year, pleading an overextended schedule. D. E. Porter (The Portman Mineral Company, Saint Augustine, Florida), wrote on December 8 about a mineral evaluation of a tract of land along the East Coast. The letter was not particularly clear as to what Porter has in mind, but he referred to Dr. Clifford K. Beck of the University of North Carolina; so I sent a copy of Porter's letter to Beck and asked that he advise me about the nature of his project and what he has in mind.

At 10 a.m. Maurice Goldhaber stopped in to talk with me and to visit with some of the fellows; then at 10:30 a.m. Dan Wilkes came by in order

to get some material for publicity about the nitrogen ion work.

I then went down on campus, where the Chemistry 123 class had the following midterm:

CHEMISTRY 123

Midterm Examination

December 15, 1953

I (25)

If we consider an alpha emitter,  $Z^A$ , whose alpha energy and half-life are known:

- (a) What would be the expected relationship of the alpha energy of  $Z^{A+2}$  to that of  $Z^A$ ? Give reasons. Would deviations be expected, and if so, where?
- (b) How would the half-lives be expected to compare?
- (c) What, if anything, could be said about the energy and half-life of  $(Z+2)^A$ ? Give reasons.

II (25)

Suppose the following data are taken on a Geiger counter:

No absorber	$\sim 10^6$ c/m
500 mg/cm <sup>2</sup> Al	$\sim 10^4$ c/m
500 mg/cm <sup>2</sup> Pb	$\sim 3 \times 10^3$ c/m
500 mg/cm <sup>2</sup> Au	$\sim 2 \times 10^2$ c/m.

What radiations would you suspect were associated with the sample. Be as specific as possible.

III (25)

The thermal neutron capture cross section of  $I^{127}$  is 6.1 barns. If 1 g of iodine ( $I^{127}$ ) is placed in a thermal neutron flux at  $10^{12}$  neutrons/cm<sup>2</sup>-sec, what will be the resulting activity, (a) after 20 minutes? (b) after several hours?

IV (25)

The uranium content of an iron meteorite is too low for direct analysis but can be detected by sensitive measurement of the radon content. What is the percent uranium in a 100 g sample which gave 10 disintegrations per minute of radon ( $Em^{222}$ )? (This disintegration rate is that of radon alone.)

For several years John Lawrence has invited me to join him and some colleagues at a stag duck hunting party at the Delta Ranch. I have never accepted the invitation in the past, but this year I decided to join the group. We drove to the ranch at Los Banos, where we had a barbecue dinner and much comradery.

Wednesday, December 16, 1953

In Los Banos. My colleagues and I arose before daylight, dressed in raingear, and proceeded to go out duck hunting. I have never shot a gun and do not like the idea of killing; fortunately, the ducks were in no danger from me. I returned home to Lafayette in the afternoon.

Thursday, December 17, 1953

The research group meeting this morning was attended by Asaro, Choppin, Clark, Dauben, Ghiorso, Gray, Grover, Harvey, Hollander, Hyde, Jaffe, Kalkstein, La Salle, Larsh, Maier, Michel, Nervik, Passell, Perlman, Ruben, Schooley, Seaborg, Stephens, Senko, Stoner, Sweeney, Templeton, Thompson, and Wallmann.

Ghiorso (who is often to busy to appear at these meetings) reported on the recent successful acceleration of  $N^{+6}$  ions, stating that previous attempts to accelerate  $N^{+7}$  were unsuccessful. In the present experiments they exposed uranium foils to the  $N^{+6}$  beam for about 20 minutes, followed by more or less traditional chemistry, including Dowex-50 column separation. Alpha pulse analysis showed 1.6-day  $Cf^{246}$  (6.75 Mev), 30-minute  $Cf^{244}$  (7.1 Mev), and 7.4-minute  $99^{247}$  (7.3 Mev) in the californium and element 99 chemical fractions. They found a total of 40 alpha events for the new element in the element 99 (eka-holmium) fraction and 150 alpha events in an unseparated actinide fraction, which was used to determine the half-life. The mass number assignment (247) was made on the basis that a  $N^{14},5n$  reaction is reasonable, but another mass number is certainly possible. Ghiorso emphasized that this does not constitute the discovery of element 99 for other classified information exists on that subject. He stated that the observed californium activities are probably formed by electron capture in element 99 parents.

Thompson talked about the irradiation of  $Pu^{239}$  in the MTR at a flux of  $1.5 \times 10^{14}$  n/sec/cm<sup>2</sup>. Prior to this, the highest possible flux was  $3.8 \times 10^{13}$  at Chalk River. So far they have identified  $Am^{241}$ ,  $Am^{243}$ ,  $Cm^{242}$ , and  $Cm^{244}$ . They also found alpha activity in the californium fraction and in the fraction corresponding to element 99. In the californium chemical fraction they saw alpha particles of energies 5.8, 6.05, 6.15 Mev and in the 99 fraction, 6.6 Mev. Thompson said they do not know if the three alpha groups observed represent three isotopes of californium or fine structure of one or more isotopes, adding that the berkelium fraction from this target contained a soft  $\beta^-$  emitter, which is likely to be  $Bk^{249}$ . Thompson gave a probable sequence of reactions for the formation of the heavy actinides and his reasoning for suggesting the mass number of the 99 alpha activity is 253 (at least the lower limit), corresponding to the capture of 14 neutrons. Thompson added the spontaneous fission rate in the californium fraction is approximately 5% of the alpha decay events and said they observed no spontaneous fissions in the 99 fraction.

Kalkstein spoke about the two new barium activities produced by  $In(N, xn)$  reactions reported by Hollander last week, whose half-lives are 12 minutes and 95 minutes; the 95-minute activity had a 1.5-minute cesium daughter. He said that, although a 12-minute  $Ba^{127}$  is known, they did not observe its known daughter activity (5-hour  $Cs^{127}$ ), and so they conclude both isotopes are new. Kalkstein said they have tentatively assigned the 1.5-minute cesium activity and the 95-minute barium activity to  $A = 126$ . The 12-minute activity is very likely due to  $Ba^{124}$ , based only on the assumption that this is reasonable for such a half-life. Kalkstein examined the gamma-ray spectrum of the 95-minute activity on a sodium iodide scintillation counter and found the following peaks: 30 (kev), relative intensity ~0.6; 64? (weak); 110 (weak); 150 (possibly due to  $180^\circ$  backscattering of 350 and 510 kev gamma rays); 170 (also possibly due to backscattering); 230 (~0.3); 390 (~0.5); 510 (1); 710? (weak). Kalkstein discussed the data in some detail.

\* \* \* \*

The midterms were explained and returned to the students in Chemistry 123. Highest grades were obtained by William Bryan (82), Alfred Chetham-Strode (88), Arnold Fritsch (84), and Roger Millikan (86). Today's lecture was on the preparation and identification of radionuclides. I stopped in the Department of Chemistry office before going up to the hill and spoke with Ken Pitzer for a while.

I took care of some of my correspondence--Stan McCaffrey asked on December 7 for nominations for the "Alumnus of the Year" Award for 1953. In a short note I suggested that Chancellor Clark Kerr and once again Mrs. Lillian E. Gilbreth are two worthy candidates. In the case of Mrs. Gilbreth I attached a summary rather than the nomination form (I nominated her last year). I also noted that Clark Kerr's record is well known to us all and speaks for itself.

On the advice of my old friend Professor Roy E. Heath (Michigan College of Mining and Technology), E. Victor Luoma wrote from Houghton, Michigan, on December 12, for information about graduate study in nuclear chemistry here. I responded with my usual letter, suggesting he write Professor K. S. Pitzer for application forms and saying that I am giving Dean Pitzer his transcript and photograph. In addition, I described our program at the Radiation Laboratory (Luoma mentioned his financial need).

After conversations with Dave Templeton and Richard Lessler, I wrote to the Commandant (USAF Institute of Technology, Dayton, Ohio) to request an extension of one year for Richard M. Lessler's assignment here, noting that he will probably be able to complete the requirements for a Ph.D. degree, rather than just the M.S. degree, with the additional time. Again, I emphasized the shortage of Ph.D.'s in nuclear chemistry in government laboratories, universities, industry, and in the armed forces.

Russell Ball (local AEC office) came by at 2 p.m. to talk with me about our recruiting for the Whitney program and a number of matters.

I made a tour of some of the labs to check on the research and later went to the 6:30 p.m. Athletic Advisory Board dinner meeting at the Durant Hotel. After the meeting Helen met me and went with me to the

Chemistry Department's Christmas party.

Friday, December 18, 1953

Stan, Al, Bernie Harvey, and Greg Choppin have written up the work about the production of the transcurium isotopes produced in the neutron irradiation of  $Pu^{239}$  in the MTR, which Stan described at yesterday's research meeting. This has been declassified and sent as a "Letter to the Editor" to The Physical Review today; it included the mention of an isotope of element 99 that emits 6.6 Mev alpha particles. The document included a statement to the effect that there is other unpublished information relevant to element 99, and in the covering letter to S. A. Goudsmit (Editor, The Physical Review), they asked if it is still possible to add a similar statement as a footnote to the paper, "Reactions of  $U^{238}$  with Cyclotron-Produced Nitrogen Ions" before it appears in print.

Included in this morning's phone calls was one to Wendell Latimer and one to Richard Doan (Idaho). I told Doan that Bill McVey and Dave O'Kelley have expressed some interest in Arco. O'Kelley, I said, would be interested in a program to determine cross sections in neutron-induced reactions if he were to go with Phillips, adding that he now makes about \$700/month. Doan said that he hopes to have a fundamental research program there. I also mentioned to Doan that I might visit Arco during the last half of January, and he suggested that I come prepared to give a talk to their technical group.

When I visited the labs, I stopped in and talked with Lou Slater (Bldg. 50) to try to determine his future plans.

At 2:15 p.m. I chaired the prelim examination of Warren G. Smith. Other members of the committee included Henry Rapoport, Rollie J. Myers, Leo Brewer, Chaim Richman, and C. A. Dekker. Smith's propositions were: I. The attenuation of anisotropic gamma radiation when an RF field is imposed on a sample of  $Nb^{91m}$  in which the nuclei have been partially aligned with a magnetic field and low temperature should permit the determination of the magnetic moment of  $Nb^{91m}$ . The RF field will cause all nuclear orientational energy levels to be equally populated; II. The angular distribution of the protons produced in d,p reaction will permit assignment of nuclear spectroscopic state of daughter nuclei if the spectroscopic state of the parent is known. Smith passed his examination.

Saturday, December 19, 1953

I read journals and accumulated papers and did some writing during the day.

Sunday, December 20, 1953

Again I worked in my study, reading journals, etc. Helen and I plan to drive to South Gate on Wednesday in order that all of us can spend Christmas with my parents and that Helen and I can attend the Rose Bowl game.

Monday, December 21, 1953

Christmas Recess for the University begins today.

At 10 a.m. I met with Philip Schiedermayer (Security) to discuss the letter designations on Livermore badges for our Berkeley people.

I called Bill McVey and told him about my conversation with Dick Doan last Friday. Also I phoned Dave O'Kelley about the same matter and suggested that he write Doan and suggest that he (O'Kelley) visit Arco. Dave mentioned that Dick Gilbert and Bob (Robert N.) Osborne might also be interested in positions there.

A note went to John P. Howe (North American, Downey), saying that it seems likely that we shall be spending the holidays in South Gate, and hence I should like to suggest Monday, December 28th, or possibly Tuesday, December 29th, for my visit to his place. I added, "I presume that you have tickets to the Rose Bowl game, but if you haven't, I think that I can fix you up."

I also wrote to R. M. Warren (Assistant Secretary, American Chemical Society) to inform him that, because of my schedule, I cannot accept reappointment to the Council Standing Committee on Publications.

I made my usual rounds of the labs to check on the experiments.

Tuesday, December 22, 1953

This morning I attempted to clear pending material off my desk.

I telephoned Dave Beckerley in Washington about adding a disclaimer about discovery to the paper describing the observation of isotopes of element 99 produced by the bombardment of uranium with nitrogen ions, as Manning suggested. He posed no objection. We also talked about the paper describing the observation of an isotope of element 99 ( $^{99m}$ ) by the bombardment of plutonium with neutrons.

I wrote to Manne Siegbahn (Nobelinstitutet för Fysik) to confirm the conversation I recently had with Ingmar Bergström about Thompson's possible visit to his (Siegbahn's) laboratory and Sigvard Thulin's visit with us. I said our thought is that Thompson go to Sweden during the spring semester of 1955 and, in exchange, Sigvard Thulin might come with us at approximately the same time, if possible. I said that our laboratory would continue to pay the salary of Thompson and his laboratory would pay the salary of Thulin with an arrangement between Thompson and Thulin for the exchange of equivalent amounts of American and Swedish moneys to the extent of Thulin's salary in order that Thulin might have the American money he would need. I suggested that each man's travel expenses would be taken care of one way or another by the man himself, possibly through a fellowship or his laboratory. I wrote that, if this arrangement seems generally satisfactory, I shall begin to make the arrangements with our Atomic Energy Commission.

A note went to Donald Lane to inform him that I plan to attend a meeting in Washington on January 7, 1954. I said that I will phone to

see whether there is anything that we might profitably talk about at that time.

Wayne Meinke wrote on November 30 about an index they have made for information contained in the nuclear chemical data file at Berkeley--he described the setup and then asked about changes made in the "Table of Isotopes" in UCRL-1928 before it was presented in final form in the Reviews of Modern Physics. I explained in today's response that we made very few changes because of the method of reproduction, noting that we added a reference to Ray Sheline's work on Mg<sup>28</sup> (which should not affect his file) and a new entry listing Si<sup>32</sup>.

I also filled out a National Science Foundation evaluation form for Dwight Conway, a former undergraduate student who did some research with us. I wrote in part, ..."He made an excellent record in his course work and showed an aptitude for research in this field. I am certain that he is a better than average student deserving of fellowship support but I must emphasize that I am not well enough acquainted with his potentialities to evaluate this application properly...."

Dick Diamond stopped in to talk with Iz and me about the uncertainty of the future of nuclear chemistry at Harvard and, in particular, of the uncertainty of his position. (Professor Rochow recently wrote to ask for an evaluation of Diamond's work, and I asked for a postponement of my response until after I talked with Diamond.)

About 2:30 p.m. I reached Rod Spence in Los Alamos and told him of our success in producing element 99 from plutonium plus neutrons in the MTR and our plans to publish the work in The Physical Review. Spence said their security people are going over the heavy element part of the Mike report to prepare it for project-wide distribution; he expressed optimism of the group there with respect to ultimate declassification. I also told Spence that we are gathering together their share of the samples from the Paydirt operation to send to them. Later I telephoned Dan Wilkes and told him about our plans to publish the work about element 99 from plutonium plus neutrons. Also today I phoned Ken Street and told him that I had talked with Richard Doan on Friday about Dave O'Kelley (O'Kelley is presently working with Street's Project Whitney group).

#### Wednesday, December 23, 1953

Helen and I packed the kids, etc. into the car and drove to South Gate. My parents were particularly pleased to see their grandchildren.

#### Thursday, December 24, 1953

In South Gate. The Seaborgs had a pleasant reunion and later a traditional Swedish Christmas Eve supper of lutefisk, herring, rice pudding, saffron bread, etc., followed by the opening of Christmas gifts.

#### Friday, December 25, 1953 (Christmas)

In South Gate. The kids got up early to check on Santa Claus' gifts--it was a reasonably active family day. In the afternoon we had

the traditional Christmas dinner of turkey and trimmings.

Saturday, December 26, 1953

In South Gate. Today I got together with my friend Clayton Sheldon and a couple of other fellows for some golf at Rio Hondo. For nine holes Clayton scored 57 and I, 46. We then played an additional five holes.

John Howe phoned me during the day to confirm my consultation visit to North American in Downey on Monday.

Sunday, December 27, 1953

In South Gate. The Seaborgs visited the Sheldons--Rita, Clayton, Ronnie, and Gayle--at their home (10005 Hildreth Avenue, South Gate) and had dinner with them.

Monday, December 28, 1953

In South Gate and Downey. The day was spent consulting at North American Aviation in Downey, where I talked with John P. Howe, Chauncey Starr, E. E. (Ercole Enrico) Motta, Sidney Siegel, and others. They described the present situation with regard to their program in high temperature chemistry separations methods, whose support is to be independent of the Atomic Energy Commission by the end of the fiscal year 1956, and told me about their results. They are working on Compact Chemical Processes (CCP), using metallurgical techniques for the separation of plutonium and fission products from metallic uranium fuel elements--these avoid the dissolution of the uranium but retain it in metallic form throughout. I learned that they want to decide whether, from the point of view of possible commercial uses, it is wise to attempt to get private funds to invest in their work. They also want to evaluate the work in order to give the Reactor and Research Divisions of the AEC authoritative and objective bases for support of the work.

Some of the points I made included the fact that I consider their results interesting and encouraging and that I have confidence that the need for work in this field will continue. I suggested that patent advantages may be obtained on developments of apparatus and processes, that some unique combinations of unit steps and material of construction may be found that will provide optimum processes, and that since their processes make it possible to avoid oxidation of uranium to a salt and subsequent reduction back to metal, they should provide a real saving in cost of processing. I did, however, mention some possible negative points: 1. Additional basic work is needed before specific processes can be outlined; 2. Detailed cost analysis of specific, thoroughly engineered processes are needed before economic superiority of this general approach can be demonstrated, noting that many workers in the field are skeptical that any process can be made compact; 3. It is probably not possible to obtain a patent position on the basic chemistry involving high temperature separation processes; 4. Even if an economical process could be worked out, would North American Aviation want to enter the chemical industry in competition with other large experienced operating companies; 5. It will be most difficult to get private funds to compete in establishing a unique patent position since national laboratories are

already operating on sizable budgets in this field; 6. Specific reactor systems and objectives must be used in specifying and judging a separations process; and 7. I suggested a conservative way of maintaining some position in this field would be to employ a few senior persons making studies and keeping up contacts with other workers.

Later Helen and I had dinner with the Howes in their Long Beach home.

Tuesday, December 29, 1953

In South Gate. I again went out to Rio Hondo with Clayton. We were joined by Ronnie and Tony: CES-107, GTS-88, Tony-87, Ronnie-74.

Wednesday, December 30, 1953

In South Gate.

Thursday, December 31, 1953

In South Gate. Clayton and I played the last nine holes at Rio Hondo today (GTS-44, CES-51). Later the Seaborgs spent a quiet New Year's Eve at home with my parents.

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