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Seaborg, G.T.

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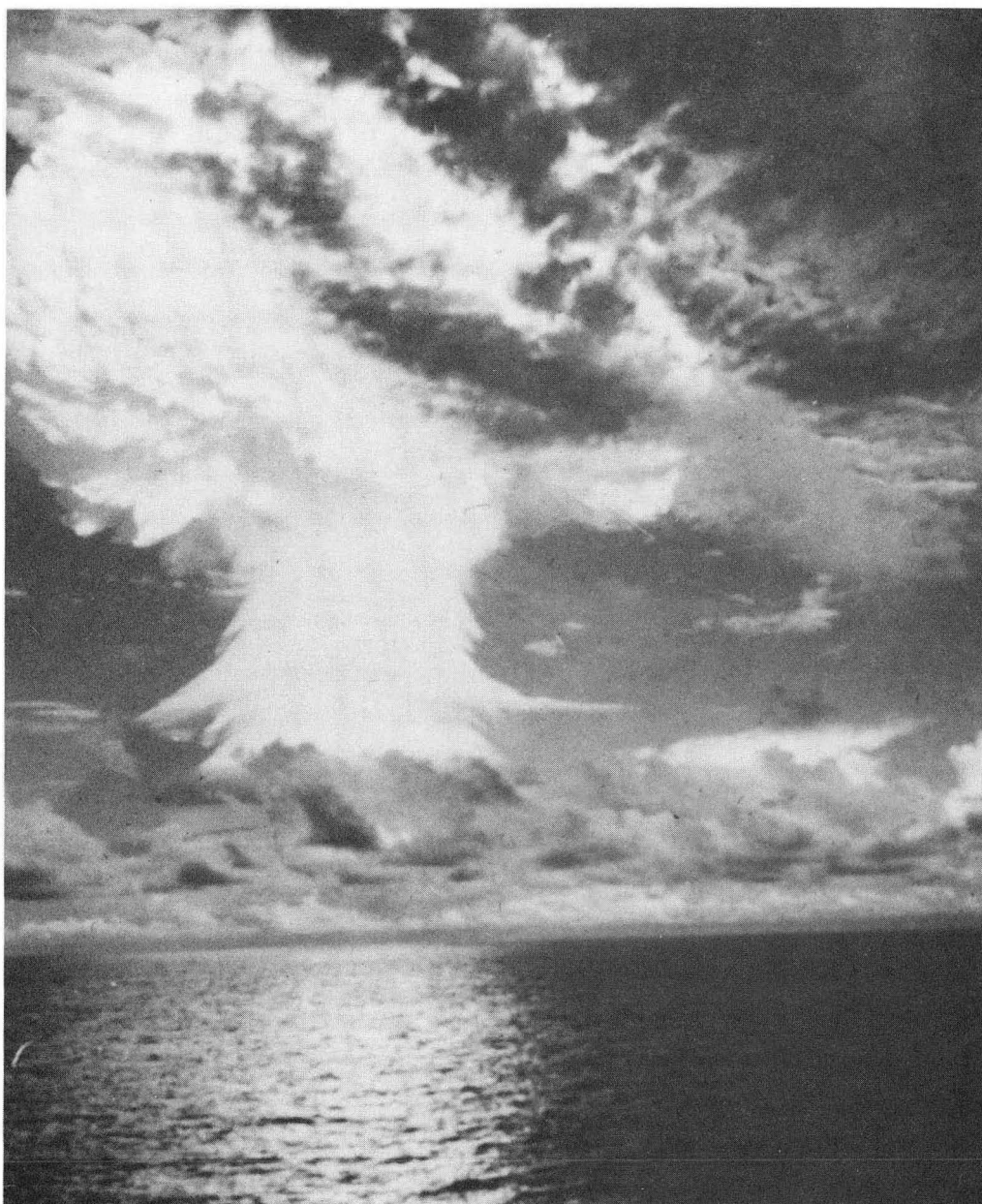
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Journal
of
GLENN T. SEABORG

1946-1958

VOLUME 6

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under Contract DE-AC03-76SF00098



XBB 791-243

Mushroom cloud produced by the Mike explosion, Eniwetok, November 1952, in which the elements einsteinium (atomic number 99) and fermium (atomic number 100) were discovered

Preface

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

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

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

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

Introduction

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

- Fischer, Vera Kistiakowsky (Autumn 1948-January 1952), Seaborg, "A Study of the Isotopes of Promethium."
- Flamm, Eileen Joy (Fall 1957-September 1960), Perlman, "Perturbation of Alpha-Gamma Angular Correlations in Transuranium Isotopes."
- Fleming, Edward Homer, Jr. (Autumn 1949-June 1952), Cunningham, "The Specific Alpha Activities and Half-lives of U^{234} , U^{235} , and U^{236} ."
- Florence, Lt. Mitchell Garth (Summer 1953-January 1955), Perlman, no thesis [M.S.].
- Folger, Robert Lancaster (Autumn 1948-June 1951), Seaborg, "High Energy Proton Fission-Spallation of Uranium."
- Foreman, Bruce Milburn (Fall 1953-June 1958), Seaborg, "Spallation and Fission in Th^{232} and the Masses of the Heaviest Elements."
- Fritsch, Arnold Rudolph (Autumn 1953-January 1957), Perlman, "Energy Levels of Neutron Deficient Lead Isotopes."
- Fung, Si-Chang (Autumn 1949-January 1952), Perlman, "Nuclear Excitation and Recoil by High Energy Particles."
- Futrell, Jean (September 1955-September 1958), Templeton, "The Radiation Chemistry of the Symmetrical Dichloroethylene."
- Gallagher, Charles Joseph, Jr. (Autumn 1954-January 1958), Rasmussen, "Electron-Spectroscopic Studies of Neutron-Deficient Rhenium Isotopes."
- Gibson, Lt. Walter Maxwell (February 1954-June 1957), Seaborg, "Fission and Spallation Competition from the Intermediate Nuclei Americium-241 and Neptunium-235."
- Gilmore, John (Fall 1956-September 1960), Perlman, "The Effect of Angular Momentum on Fission Probability."
- Glass, Richard Alois (Autumn 1950-June 1954), Seaborg, "Studies in the Nuclear Chemistry of Pu, Am, and Cm, and the Masses of the Heaviest Elements."
- Glenn, William Ellis, Jr. (Fall 1947-January 1952), L. C. Marshall, "Time of Flight Mass Spectrograph" (Ph.D. in Engineering).
- Goeckermann, Robert Herman (Autumn 1946-January 1949), Perlman, "Characteristics of Bismuth Fission with High Energy Particles."
- Gonzalez-Vidal, Jose (Autumn 1955-September 1958), Seaborg, "Survey of Tritium-Producing Nuclear Reactions."
- Gordon, Glen Everett (Fall 1956-September 1960), Seaborg, "Fission and Spallation in Nuclear Reactions Induced by Heavy Ions."

- Gray, Peter Rygaard (Fall 1952-January 1956), Seaborg, "Electron Capture and the Auger Effect in the Heaviest Elements."
- Grover, James Robb (Autumn 1952-January 1958), Seaborg, "The Reactions of Tantalum with 5.7-Bev Protons."
- Gruber, John Balsbaugh (Fall 1957-June 1961), Cunningham, "An Analysis of the Absorption Spectra of Tm(IV) and Am(VI)."
- Gunn, Stuart Richard (Autumn 1950-June 1954), Cunningham, "Thermodynamics of Aqueous Ions of Americium."
- Hall, Kenneth Lynn (Autumn 1949-July 1951), Templeton, "Counting Efficiency of Bismuth 205" [M.S.].
- Hardgrove, George Lind (Autumn 1956-September 1959), Templeton, "The Crystal Structure of Several Cis-1,2 Dihalobenzocyclobutenes."
- Hicks, Harry Gross (Autumn 1946-June 1949), Seaborg, "New Neutron-Deficient Radioactive Isotopes of the Rare Earth Region."
- Higgins, Gary Hoyt (Autumn 1949-June 1952), Seaborg, "An Investigation of the Isotopes of Americium and Curium."
- Hill, Max Wilmer (Fall 1955-January 1959), Perlman, "Nuclear Decay Studies of Protactinium Isotopes."
- Hoff, Richard William (Autumn 1950-January 1954), Seaborg, "Orbital Electron Capture in the Heaviest Elements."
- Hollander, Jack Marvin (Autumn 1948-September 1951), Perlman, "Nuclear Transformations Using Accelerated Carbon Ions."
- Hopkins, Horace Herbert, Jr. (Autumn 1946-June 1949), Cunningham, "Spallation Products of Arsenic with 190 Mev Deuterons."
- Hulet, Ervin Kenneth (Fall 1949-September 1953), Seaborg, "An Investigation of the Isotopes of Berkelium and Californium."
- Hummel, John Philip (Autumn 1953-September 1956), Perlman, "Alpha Decay Studies in the Heavy Element Region."
- Jaffe, Harold (Autumn 1951-June 1954), Perlman, "Electron Capture Studies in Shielded Nuclei."
- James, Ralph Arthur (Fall 1946-June 1948), Seaborg, "Isotopes of the New Element Curium (Atomic Number 96)."
- Johnson, Quintin Calvin (Fall 1957-January 1961), Templeton, "Some Problems in Crystallography."
- Jones, Merle Eugene (Autumn 1949-September 1951), Cunningham, "Vapor Pressure of Americium Trifluoride."

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Thomas, Thomas Darrah (Autumn 1954-September 1957), Seaborg, "Spallation-Fission Competition from the Compound System U^{239} 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 Stragglings of Heavy Recoil Nuclei" [M.S.].

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

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

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

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

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

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

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

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

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

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

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

Some of the other graduate students associated with our group during this period included Howard W. Anderson (1956–1958, Chem E), Robert S. Brown (1954–1959, Chem E), Richard P. Burns (1954–1955), Amado Y. Cabezas (1957–1961), William B. Carter (1957–1960, Chem E), Lung-wen (Linda) Chiao (1957–1961, Rasmussen), Yung-Yee Chu (1956–1960, Templeton), Henry Cheung (1953–1958, Chem E), Denny L. Condotta (1952–1953, Chem E), Harry L. Conley (1957–1960), Marshall W. Cook (1950–1954, Chem E), Alberto Cortes (1956–1962 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. Lavinger (1951), Victor L. Ledesma (1957-1963, Chem E), Hugh R. Lehman (1948-1952, Chem E), Nian-Tze (Norman) Li (1957-1960, Chem E), David Z. Lippmann (1948-1953), Robert E. Lundin (1950-1955), Edward J. Lynch (1949-1953, Chem E), John L. Maier (fall 1953), Arturo Maimoni (1950-1956, Chem E), Richard Marrus (1957-1959, Physics), Robert E. Meredith (1956-1959, Chem E), Eugene I. Motte (1952-1954, Chem E), Robert F. Nickerson (1953-1958), Ronald Odum (1954, Chem E), Alfred W. Petersen (1952-1954, Chem E), John E. Powers (1951-1954, Chem E), Homer E. Rea, Jr. (1949-1953, Chem E), Albert J. Rothman (1950-1954, Chem E), Barney Rubin (1947-1951, Chem E), Russell H. Sanborn (1952-1955), Sidney D. Skirvin (1951-1952), Harry E. Spencer (1950-1954/Connick), Peter O. Strom, Jr. (1955-1956), Carol C. Sweeney (1957-1958), Archie B. Treadwell (1954-1955), John H. Vanderveen (1954-1961, Chem E), William R. Wilcox (1956-1960, Chem E), Joseph Winocur (1955-1960, Chem E), Peter Yankwich (1948), Thomas J. Ypsilantis (Physics, 1949-1950), Rene D. Zentner (1954-/law).

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

In addition to students and staff, we frequently had participating guests, including postdoctoral appointments, for varying periods of time. These included, for example: John M. Alexander (1957-1958), Saadia Amiel (1956-1957), Nathan E. Ballou (1947-1948), Christiane Baltzinger (1957-1958), Ingmar Bergström (September-October 1953), Norman A. Bonner (summer 1948), Fred L. Canavan (1955-1956), Albert A. Caretto, Jr. (1956-57), Andre Chesne (1957-1958), Gregory F. Choppin (1953-1956, summer 1957), Maung Cho Cho (June-July 1957), T. C. Chu, James W. Cobble (1952-1954), Alexander Cosmatos (1958), Ugo Croatto (1951-1952), Lloyd Currie (1957), Richard M. Diamond (summer 1957), Harold W. Dodgen (summer 1949, summer 1950) Peter Fong (summer 1956), Kenneth W. Ford (December 1957), Wilhelm Forsling (1955), Sherman Fried (July 1952), Gerhart Friedlander (September 1952), Bernard Fries (1947), Jean Fuger (1956-1957), Tor Ragnar Gerholm (June 1958), Lawrence Glendenin (1952), Richard C. Hoff (1958), Peter Graf (1955-1956), Dieter Gruen (1955-1956), Barun C. Halder (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. Grahlman (1946-1947), Lorraine Hanna, Al Hartzell, Virginia C. Hempel, Winifred Heppler (1946-), Margie (Schnarr) Hollander (1948-1955), Pat Howard (1956-), Glen I. Iddings, Alice Israel, Edward Jeung (1956), Frances Jewell, Hildred Jensen, Stephen Kahn, Louise Kalm, George W. Kilian, Charles W. Koch, Almon E. Larsh (1950-), 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 Pionteki, Elinor Potter (1952-), Jean Rees (1956-), Carolann Rossi (1957-), Helena Ruben (1950-), Jean Samson, Aldo F. Sciamanna, George V. Shalimoff (1953-), Donald Simkin, Dorothy (Bockhop) Stewart, Doris (Heisig) Terwilliger, Yoshiko Uchida, Helene Voyer (1947), Jane Waite, Lawrence A. Williams, Jane Wulf, Mary Wyld, Al Wydler, Gifford Young, M. Charles Zeitz, et al.

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

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

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

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

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

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

Curium (96) was the first to be isolated in weighable amount in 1947, berkelium (97) and californium (98), in 1958. During this decade the chemical properties of berkelium, californium, einsteinium, fermium and mendelevium, as well as astatine, were defined by using the tracer technique; macroscopic quantities of neptunium, plutonium, americium and curium were used for the production and determination of the properties, including crystal structures (via x-ray diffraction), of numerous compounds and metallic forms of these elements. Such work was also done with many of the rare earth elements. The involvement of 5f electrons in bond 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 (May 31) as twins, Paulette Jeanne and Peter Glenn, of which only Peter Glenn survived.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

My first college level Commencement Address was given at Gustavus Adolphus College on May 30, 1954. I also spoke to a lay audience upon my visit to my hometown of Ishpeming, Michigan, in July 1954, on the occasion of its Centennial Celebration. I presented the awards to the winners of the 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.

Tuesday, January 1, 1952 - New Year's Day

Much of my day was occupied with the bowl games. A few students, in addition to Dick and Marian Diamond, watched the Rose Bowl with us in the afternoon. Dick is visiting Marian, who is still a student on the Berkeley campus, during the holidays, and we found time to talk about his work at Harvard, the declassification of his thesis, etc. In the Rose Bowl, Illinois beat Stanford by a score of 40 to 7 (Illinois ran for 361 yards against Stanford's 53 yards; however, Stanford passed for 180 yards while Illinois made only 73 yards in the air). Marvin Kalkstein, one of our guests, stayed and had supper with us--the kids were quite entertained by him.

In other bowl games, Maryland beat Tennessee, 28 to 13, in the Sugar Bowl; Kentucky beat Texas Christian, 20 to 7, in the Cotton Bowl; and Baylor was beaten by Georgia Tech, 17 to 14, in the Orange Bowl.

Wednesday, January 2, 1952

Helen and I recently sold our Ellsworth Street home to a young couple. We agreed to an unusually low down payment and are carrying the mortgage; however, Helen and I were rather surprised to receive a letter from the new owners (W. A. Lecaro and M. T. Navarro), requesting that the monthly payment be lowered (from \$200 to \$100) because he is earning less overtime and his wife is going to have to quit her job soon since she is pregnant. Today I wrote Lecaro:

I was surprised to learn that you expect a reduction in the amount of the payments at so early a date after making only a couple of payments on our contract. As you may not know, the major part of your unusually small down payment went to the real estate company and to cover other expenses connected with the transaction. A reduction in payments at this time would leave no tangible evidence to us that a sale had been made and, in fact, would make the transaction from our point of view essentially equivalent to merely renting the house. Furthermore, a reduction in payments would, in our view, be equivalent to reducing the price of the house since the cost of living is apparently increasing at a rate greater than 5% per year, the interest rate of our contract. For these reasons, I do not see how we can agree at this time to breaking our contract.

[Even though Lecaro's situation may not be good, I believe the Nobel publicity has left the impression that the Seaborgs are now wealthy.]

Several letters went out today in connection with the Gordon Research Conference on Nuclear Chemistry, which will take place the week of June 23, 1952. I replied to Kurt Kraus' letter of December 11, 1951, in which he suggested that we have someone else talk because they have no new work, by saying that we want to have a number of classical workers attend as focal points for discussion. It appears, I wrote, that Joe Katz will act as a sort of discussion leader; I suggested that he and Joe decide about whether he should give a paper. Finally, I thanked Kurt for his expression of congratulations.

I wrote to Nathan Sugarman about his participation in the Gordon

Research Conference and said that I believe his idea of including work on the radiochemical identification of meson-induced reactions seems to be a good one. I asked him to send Charles Coryell and me a suggested title and names of authors. A note went to Jack (J. M.) Miller to say that we are pleased that he is willing to deliver a paper on his high energy work.

A longer letter about the Gordon Conference went to Clark Hindman, who thanked me for the invitation to participate in a letter dated December 26 and then asked for more information. I explained to Hindman that we feel the papers should be broad discussions of reasonably new work and needn't be concerned with only his own work. I mentioned that Joe Katz will be a sort of coordinator or discussion leader for a half-day session on Heavy Element Chemistry, in which he, Burris, and possibly Kurt Kraus, will be the main speakers. I suggested to Hindman that perhaps he should get together with Joe to exchange ideas.

Edwin O. Wiig (University of Rochester) also wrote on December 26 to accept my invitation to take part in the Gordon Research Conference.

On another matter I commented on a December 19 letter addressed to Ken Street from John A. Wheeler (Princeton). Wheeler asked for a copy of UCRL-1185, "Slow Neutron Fission of $\text{Am}^{242\text{m}, 242, 243}$ " by Street, Ghiorso, and Thompson. This I mailed him, along with a copy of my short paper "The Mechanism of Fission." I pointed out that he will probably find my report to be rather naive, but since it will be published as a "Letter to the Editor" of The Physical Review in the near future, it couldn't escape his critical eye for very long.

I also signed and returned a couple of Secrecy Orders--for Case S-582 ("Method of Recovering Transuranic Elements of an Atomic Number Below 95" by Glenn T. Seaborg and Ralph A. James) and for Case S-834 ("Process of Purifying Uranium" by Glenn T. Seaborg, Edwin F. Orlemann, and Lyle H. Jensen).

I made the rounds of the labs and discussed, not only research, but also yesterday's football games.

Thursday, January 3, 1952

The group meeting this morning was attended by Dunlavey, Feay, Fischer, Glass, Gunn, Hollander, Hyde, Kalkstein, Larsh, Levy, Michel, Perlman, Rasmussen, Reynolds, Robinson, Ruben, Seaborg, and Worthington.

Dunlavey gave the breakdown on coincident electrons from the alpha decay of Np^{237} (using about 3500 total alpha counts) as: no e^- (16%), ≤ 20 keV (11%), 20-40 keV (40%), ≥ 40 keV (21%), doubles (11%), triples (1%). Dunlavey said that most of the electrons above 40 keV are probably around 60 to 80 keV energy. He also said that the exposure period used was three days. Since the daughter of Np^{237} , Pa^{233} , is a beta emitter of 27-day half-life, this would result in approximately 3-1/2% overabundance of electrons and nearly all of this 3% might be expected to fall in the group of energy ≥ 40 keV, unless they were of such high energy as to produce no visible track. In the discussion Perlman mentioned that Asaro found no definite evidence of fine structure in the Np^{237} alpha spectrum, which would be consistent with almost all of the alpha particles going to

an excited state of the daughter. I suggested two possible decay schemes and noted that, if other alpha groups were present in low abundance, they would not have been seen by Asaro.

Dunlavey also reported that he is investigating Pu²³⁹ and U²³⁶, adding that the former has a low abundance of soft electrons and the latter has some low-energy and some moderate-energy electrons. There followed some discussion.

Worthington talked about his work with Per Kofstad on Ni⁵⁶, which they produced by a bombardment of iron with helium ions. They separated the nickel fraction from the iron after one week; they followed the decay on a Geiger counter and ran one sample on the gamma scintillation pulse analyzer. They observed four peaks of 140 kev, 480 kev, 770 kev, and ≥ 1.4 Mev. Worthington said the second peak could include part of the annihilation radiation from Ni⁵⁷ initially present or from the Co⁵⁶ daughter. The best half-life value for Ni⁵⁶ (from following the decay of gamma-ray peaks) is 6.0 ± 0.5 days. We discussed his methods of determining the half-life. Worthington said they found only one positron component, using the bender, corresponding to Ni⁵⁷, immediately after the separation. Ten days later they saw a 1.5 Mev positron component belonging to the Co⁵⁶ daughter. In both cases they saw a small amount of negative electrons. In answer to a question I asked, Worthington said the maximum positron branching for the Ni⁵⁶ would appear to be about 24%. During the discussion I mentioned that Ray Sheline and Ray Stoughton of Oak Ridge are also working on Ni⁵⁶.

Fred Reynolds described eradication processes for emulsions used to detect electrons.

* * * * *

When I arrived on the hill, I checked with Doral about my phone messages and then answered some of my pending correspondence. Stan and Al offered some suggestions for my comments on David B. Langmuir's letter of December 12, 1951, in which he passed on Bernard Harvey's proposal of October 30 about the use of Pu²³⁹ alpha particles as an indicator for the Pu²⁴⁰ content of pile-produced plutonium. In my letter to F. T. Hobbs (AEC, Washington) I wrote that the method looks feasible with the limitation that there would have to be a separate calibration for each type of pile and probably, to a lesser extent, for each position in a given pile where the neutron distribution is different. I pointed out that the group in Chicago is in a much better position than we are to make accurate analyses. I also mentioned that the measurement of the beta particles of Pu²⁴¹ is another method, known by all concerned, for determining the gt level.

Ray Stoughton (Oak Ridge) wrote on December 13 to describe some openings for Ph.D.'s at Oak Ridge--one for someone with training in nuclear chemistry and one for someone with training in theoretical chemistry. He also mentioned a half-time teaching position in the Oak Ridge School of Reactor Technology. Since I had not yet answered, Ray called the other day and talked with Iz and me. He is quite anxious to fill the positions, and I mentioned that John Rasmussen is getting his degree and would be an excellent man. Ray also described his current

research (on Ni⁵⁶). Today I wrote Ray and said that I had talked with Rasmussen, who may visit Oak Ridge to talk with him. I pointed out, however, that Rasmussen is very interested in academic work and may look into such opportunities. I said that Iz and I will discuss the Oak Ridge positions with some of our men who will obtain their Ph.D. degrees in the near future. I also told Ray that we will get in touch with him later on in connection with the work on Ni⁵⁶. Finally, I thanked him for his congratulations in connection with my recent trip to Stockholm.

A rather long letter went to Dr. Moise Haissinsky (Paris) to thank him for the luncheon and discussion I had with him on the heaviest elements. I wrote,

I have thought a great deal about your point of view and have discussed it with several of my colleagues here. We do not feel that we agree with you and especially not on your point of view that U, Np, Pu, and Am constitute a group of analogous elements that can be singled out from the other transactinium elements. In order to point out our reasons for disagreement most succinctly, consider the following. Why limit yourself to the chemical properties in aqueous solution? If you consider solid compounds, and for definiteness let us consider the halides, you won't find any reason for listing U, Np, Pu, and Am together as a group. As a matter of fact, uranium is about the only member of the group that has any halides of oxidation state greater than 3.

Finally, I asked Haissinsky to send me a copy of his graph comparing the densities or atomic volumes of the elements.

I answered a December 7 letter from John Willard (University of Wisconsin) and thanked him for his congratulations. With regard to speaking at their local ACS section, I said that I have about given up accepting such invitations although I would like to work out something for his invitation. I suggested that perhaps I could speak in Madison before or after the ACS meeting in Atlantic City during the week of September 14. With regard to possibilities for an instructorship, I suggested Allan Zalkin, who did his graduate research with Dr. D. H. Templeton on the determination of crystal structures by x-ray diffraction. Zalkin, I wrote, is a very good physical and inorganic chemist with a thorough knowledge of nuclear chemistry. In addition, I mentioned that John Rasmussen is an excellent man, who is pretty strictly a nuclear chemist. Rasmussen, I said, will be taking a trip East for the purpose of job interviews in about two weeks and could take a side trip from Chicago to Madison.

Finally, I responded to an invitation from James R. Bercaw (President, Eta Chapter, Phi Lambda Upsilon, Ohio State University) to give a series of two or three lectures on their campus. (Previous lecturers were Peter Debye and Linus Pauling.) I wrote that I am not sure that I can work this into my schedule, but I asked if his invitation could be combined with the one I hope to accept sometime in the future from Professor and Mrs. Kurbatov. In addition, I pointed out that I would find it difficult to prepare a manuscript for publication within the three-week time period before the scheduled first lecture. I then said that the only date I am sure of at the present for a trip east is at

the time of the Buffalo ACS meeting, where the Division of Physical and Inorganic Chemistry is meeting. This would limit possible dates for the lectures to March 28 and 29, one of which is a Saturday; I asked whether these dates are suitable from his point of view. I then thanked Bercaw for the congratulations from him and the Eta Chapter.

Friday, January 4, 1952

In addition to talking with some of the people about their research, I also answered a few letters. I wrote to Wilfred J. Brewer (City Clerk, Ishpeming) to thank him for the heart warming resolution adopted in my honor by the Council of the city of Ishpeming on December 5, 1951. I wrote that Mrs. Seaborg and I had an enjoyable visit to Sweden and that we hope to visit Ishpeming with our children sometime in the future.

Recently I received letters from the fifth grade class of Elmer Avenue School describing the students' enthusiastic response to the book, You and Atomic Energy, that I helped John Lewellen prepare. I had the letters copied and sent the copies to Lewellen.

John Rasmussen carefully went over the manuscript that I received in November from Dr. Shizuo Fujiwara on melt extraction methods. Today I wrote Fujiwara to thank him for the beautiful fan and his congratulations. I included Rasmussen's comments on his paper.

In today's mail was a January 2 letter from Charles Coryell about his actions with regard to next summer's Gordon Research Conference on Nuclear Chemistry. I went over his suggestions and then immediately wrote that I agree with most of his thoughts. I added that I do want to invite Bob Penneman and Bernard Harvey but feel that the general letter will be sufficient. I also said that I have asked Joe Katz to give some thought to the session on Heavy Element Chemistry and then asked Coryell if it is necessary to have the actual titles and authors of the invited papers for the preliminary program.

Saturday, January 5, 1952

I went in to the lab for a while today to read reports and to spend some time with the students. While I was there, I received a telephone call from Bill (W. H.) Johnston of Purdue University, who had asked me earlier to speak to their chemistry department. I agreed to try to do this some time in the spring, combining it with a talk to their Sigma Xi chapter. In today's conversation I explained that my schedule has become extremely full; we agreed to postpone both talks until some time in the fall.

Sunday, January 6, 1952

In addition to playing with the kids, I gave considerable thought to some of my writing projects. I have to write an article for Encyclopaedia Britannica, and I must prepare my Nobel lecture for publication.

I did, however, look over the January issue of Popular Mechanics, which contains the Golden Anniversary Hall of Fame:

POPULAR MECHANICS
FIFTIETH ANNIVERSARY
HALL OF FAME

1902-1952

FROM THE VANTAGE POINT of a half century during which *Popular Mechanics Magazine* has observed and reported the noteworthy accomplishments in the fields of mechanics, the sciences and discovery, its editors pay tribute in this Golden Anniversary Hall of Fame to 50 Americans, native or naturalized. These are the men whose achievements in these fields have contributed in such high degree to the welfare of mankind as to deserve signal honor of their fellow men. There are other men of eminence whose lives reached into the 20th century but whose significant work occurred before 1902; these have been omitted from this roll of honor. Throughout the years, the pages of *Popular Mechanics* have reflected the accomplishments of every man on this distinguished roster.



Carl D. Anderson

For his discovery in 1932 of the positron, a part of the atom, which was a vital contribution to new research on the structure of matter. Faculty member, California Institute of Technology, Pasadena



Edwin H. Armstrong

For his inventions as an electrical engineer in the field of radio; FM static-free radio, 1939; super-regenerative circuit, 1920; superheterodyne, 1918, and others. Faculty member, Columbia University

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William Beebe

For his explorations in which he has given man a glimpse into new frontiers; for his descent into hitherto-unknown ocean depths in the first bathysphere; and many other contributions in zoology



Richard E. Byrd

For explorations in the Arctic and Antarctic; for flights over the North Pole (1926) and the South Pole (1929); for the discovery of great land areas in the Antarctic and his solitary 5-month vigil

Ewing Galloway



Hugh H. Bennett

For his work in soil conservation; for founding the U. S. Soil Conservation Service of which he was chief for many years. He was recently made a special assistant to the Secretary of Agriculture



George W. Carver

For his work as an agricultural chemist in helping revolutionize agriculture in the South; for the creation of new products from peanuts, soybeans, sweet potatoes and cotton stalks. Died in 1943



Hans A. Bethe

For basic research in theoretical physics; for his theory of atomic nuclei; for his discoveries on the energy production of stars; for his shock-wave theory. Faculty member, Cornell University, Ithaca



Arthur H. Compton

For directing the work at the University of Chicago which resulted in the first atomic chain reaction; for important discoveries on X rays and also on cosmic rays. Chancellor, Washington Univ., St. Louis, Mo.



Percy W. Bridgman

For discoveries on the physical effects of high pressures; for research on the electrical conduction of metals and the properties of crystals. A member of the faculty, Harvard Univ., Cambridge



Karl T. Compton

For his discoveries in the field of electrical discharges; for research on the harnessing of atomic energy; for leadership of development board, Nat'l Military Establishment. Chairman of Corp., M.I.T.



Luther Burbank

For the creation of new fruits, grains, grasses, vegetables, flowers and trees; for his work on the crossing and selection of plants which he conducted until shortly before his death in 1926



Glenn H. Curtiss

For pioneer work in the development of aircraft; for his Navy-Curtiss flying boat which made the first flight across the Atlantic Ocean in 1919; for building World War I planes. He died in 1930



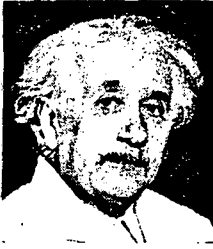
Vannevar Bush

For his work as director of the Office of Scientific Research and Development in coordinating war research for an electrical superpower system and many inventions. President, Carnegie Institution



Lee deForest

For the invention of the radio tube and 300 other inventions in wireless, telegraphy, radiotelephone, wire telephone, sound-on-film talking pictures, facsimile and the television. Los Angeles



Albert Einstein

For his theories of relativity, on which all atomic research is based; for revolutionizing mankind's ideas of time and space; for his law of photoelectric effect. Faculty member, Princeton University



George E. Hale

For research in astronomy on solar and stellar spectroscopy; depths of the universe and the stellar evolution, for his work as organizer and director of Mt. Wilson Observatory. Pasadena. Died in 1938



Lincoln Ellsworth

For polar exploration; for Amundsen - Ellsworth Nobile transpolar flight in 1926; Antarctic flight, 1935, when he claimed 300,000 square miles of new land for the United States. He died in 1951



Edwin P. Hubble

For his theory of the expanding universe and for other research on outer space; for work on the design of 200-inch Hale telescope at the Palomar Observatory. Cosmologist, Palomar, Mt. Wilson



Enrico Fermi

For discoveries in atomic research as pertaining to the magnetic moments of nuclei, beta-ray emission, artificial radioactivity as well as for devising the first atomic pile. Faculty, the University of Chicago



Edward C. Kendall

For research and discoveries in biochemistry; for isolation of thyroxine from thyroid gland and hormones of the adrenal cortex, also synthesis of the latter. The Mayo Clinic at Rochester, Minnesota



Henry Ford

For his pioneer activity in the automotive field; for his original assembly-line methods and mass production which made it possible for the average American to own an automobile. He died in 1947



Charles F. Kettering

For his pioneer work as automotive engineer and inventor; for the self-starter, lighting and ignition systems; for improved auto fuels; for nearly 50 years of vital research. Dayton, Ohio



James Franck

For research on energy exchange between electrons leading to discoveries in photochemical processes and in photosynthesis. A member of the faculty, chemistry, at the University of Chicago



Morris S. Kharasch

For work in organic chemistry, seed disinfectants and fungus diseases; for isolation of ergot; for his discoveries in antiseptics and hormones as well as in synthetic rubber. The University of Chicago



George W. Goethals

For his work as chief engineer and chairman of the commission which completed one of the greatest of engineering feats of all time — the Panama Canal. Colonel Goethals died in 1928



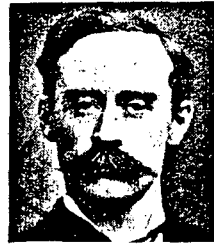
Irving Langmuir

For chemical research and molecular physics; for the development of the gas-filled tungsten lamp, electron-discharge apparatus, a high-vacuum pump, surface chemistry. General Electric Company

Dwight Galloway

**Ernest O. Lawrence**

For his invention of the cyclotron and other vital machines for nuclear physics; for important research into the nature of the atomic nucleus. Director of Radiation Laboratory, University of Calif.

**Robert E. Peary**

For the discovery of the North Pole in 1909 after years of Arctic exploration; for his courage in proceeding on foot with a small group when most of his party had turned back. Peary died in 1920

**Charles A. Lindbergh**

For planning and executing the first solo flight across the Atlantic Ocean on May 20-21, 1927, in The Spirit of St. Louis; he flew 3600 miles in 33½ hours; for his inspiration to aviation development

**I. I. Rabi**

For discoveries in physics pertaining to magnetism in regard to molecules resulting in much new information in the atomic and molecular structures; also for radar research. Faculty, Columbia Univ.

**Thomas Midgley, Jr.**

For his discovery of the antiknock properties of tetraethyl lead, principal ingredient of the ethyl and the high-test aviation gasolines and vital to powerful modern engines. He died in 1944

**John L. Savage**

For outstanding work in civil engineering; for directing design and the building of Grand Coulee, Hoover and other dams and irrigation projects throughout the world. The Bureau of Reclamation

**Robert A. Millikan**

For his work in physics, including determination of fundamental electrical charge on the electron; for his research on the properties of cosmic rays. Prof. emeritus and v.p., California Institute Tech.

**Glenn T. Seaborg**

For discoveries and continuing research in nuclear physics pertaining to the elements heavier than uranium; codiscoverer of plutonium and others. Member of faculty, University of California

**J. Robert Oppenheimer**

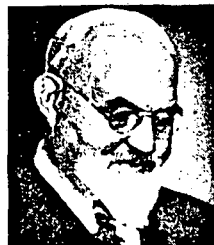
For extensive research in nuclear physics; for his direction of the Los Alamos laboratory in New Mexico during the perfection of the atomic bomb; for work on Atomic Energy Comm. Princeton, N.J.

**Alexander P. deSeversky**

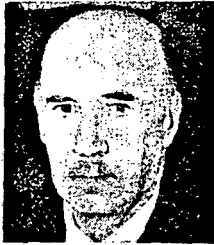
For the invention of the fully automatic bomb-sight, for airplane design, including a single-seat fighter plane, air-cooled engine with a turbo-supercharger for high altitude flight. New York City

**Linus C. Pauling**

For his resonance theory of chemical bonds; for his discoveries in the fields of physics, biology, chemistry and medicine; for atomic structures of protein molecules. California Institute of Technology

**George H. Shull**

For the development of hybrid corn, which has increased the annual crop by millions of bushels — believed to be one of greatest food boons of modern times. Faculty member, Princeton Univ.



Igor I. Sikorsky

For the development of first successful helicopter built in the United States; for building and flying the first multiengine planes; for work in developing many long-range planes. Sikorsky Aircraft



Nicola Tesla

For early research on electric current; for the substitution of alternating current for direct current; for a system of arc lighting; for a high-frequency generator; for the Tesla coil. Died in 1943



Elmer A. Sperry

For his contribution to aviation with early instruments for blind flying, the Sperry gyrocompass, airplane and ship stabilizers and scores of other important inventions. He died in 1930



Harold C. Urey

For the discovery of the hydrogen atom of atomic weight two and for the production of heavy water and uranium-235; for extensive research on the construction of atoms. University of Chicago



Charles P. Steinmetz

For his electrical inventions, including magnetite-arc lamps, induction regulators, transformers and measuring instruments; for protection against lightning. General Electric. He died in 1923



Frank Lloyd Wright

For the major role he played in the development of modern architecture; for his originality in arranging interior spaces and for his exteriors of functional design. Wisconsin and Arizona



Louis H. Sullivan

For his work in architecture; for applying many new principles to skeleton-steel buildings; for introducing "functionalism" as the basis of modern architecture. Chicago. Sullivan died in 1924



Orville Wright

For the invention, with his brother, Wilbur, of the first successful airplane; for being the first man to fly in the historic power-driven airplane at Kitty Hawk, N. C., Dec. 17, 1903. He died in 1948



Leo Szilard

For his work with Enrico Fermi in devising the first atomic-pile chain-reaction system composed of uranium and graphite used in the earliest atomic experiments at the University of Chicago



Wilbur Wright

For the invention, with his brother, of the first airplane; for the successful promotion of the plane to the United States government, for early glider research with Orville Wright. He died in 1912



A. Hoyt Taylor

For pioneer research in the development of radar during the years 1922-1945 at the U. S. Naval Research Laboratory; for research in radio; for his electrical measurements. Taylor retired in 1945



Vladimir Zworykin

For the development of the iconoscope, an electronic tube which made modern television possible; for the development of the electron microscope. R. C. A. Laboratories, Princeton, N. J.

Monday, January 7, 1952

In between phone calls and other interruptions, including a stop at First Aid for the regular blood test and urinalysis, I wrote a couple of dozen thank-you notes to people who have written congratulatory letters to me. Special letters went to my former secretaries, Ruth Rogers Brockett to whom I said that I had heard of her new marital status some time ago in connection with one of my visits to Argonne and Mary Bender to whom I wrote that she also made a contribution to the honor which has now come to the laboratory.

I acknowledged a congratulatory letter from Knowles Ryerson (Davis campus), who wrote to me in Stockholm from Italy on his way home after a year's leave in Thailand.

After replying to a congratulatory note (December 30) from John O. Viking of Ishpeming, who said he knew my father and Uncle Henry, I asked Doral to send a copy of Viking's letter to my father in South Gate.

In a November 30 letter German E. Villar (Montevideo, Uruguay) warmly congratulated me on the Nobel Prize in Chemistry, saying:

I have before me your letter of the 3rd of December, 1948, in which you answered one of mine which announced my proposition of presenting you to the Nobel Committee for Chemistry, as a candidate for the award for the year 1949. In reality the delay in receiving the laurel, only two years....is not much in this world where so many exceptional figures have been obliged to wait until their death for the recognition they merit....

To Villar I wrote, "I remember very well our exchange of correspondence in 1948 and feel sure that this may have contributed to the successful outcome."

Other letters of appreciation went to people such as Marshall Gates (Assistant Editor of Journal of the American Chemical Society), our former Indian visitor, B. C. Halder (now at Notre Dame), and Edwin O. Wiig (who visited Berkeley on December 10). To Wiig I also commented that I am glad he accepted our invitation to participate in the Gordon Research Conference on Nuclear Chemistry.

I returned to Professor Göran Liljestrand (Karolinska Institutet, Stockholm) the account of the 1950 Nobel ceremonies, written by Philip S. Hench. Hench received the 1950 Prize in Physiology and Medicine jointly with Edward C. Kendall and Tadeus Reichstein. One of the staff made a copy of the paper for my files.

At our regular Monday brown-bag luncheon staff meeting at noon in my office, we talked about a variety of subjects, including my trip to Stockholm and Paris and the bowl games.

Stan Thompson and I also had a conversation about presenting a paper at the Buffalo ACS meeting on the masses of translead nuclides. The values are quite good, and there has been considerable interest expressed in the work. A number of people have worked on this project, including

Truman Kohman, Dick Diamond, and Stan Thompson. Stan and I agreed that I should prepare an abstract for the meeting.

Tuesday, January 8, 1952

I spent much of the morning on campus, talking with colleagues in the Department of Chemistry office and later giving the Chemistry 123 lecture. Today I discussed radiochemistry.

Later, on the hill, I read a congratulatory note from Satnam Singh Hitkari (Income Tax Officer of Kanpur, India): "Kindly accept my humble congratulations on the occasion of the award of the Nobel Prize. Through this prize the world has expressed its gratitude to the services you are rendering to bring peace and prosperity to the world." Hitkari then asked for my autograph, which I sent.

On January 2 C. J. Lapp (NRC Fellowship Office) sent an evaluation sheet for Vera Kistiakowsky Fischer, who is applying for a Merck Postdoctoral Fellowship. Earl filled out the form, saying that although she does not rate as high as some of our postdoctoral people, she is a careful, orderly, conscientious, and productive worker with a firm background in theoretical principles and in the basic techniques of nuclear chemistry and that such a fellowship would allow her to continue to work here where she will have excellent facilities at her disposal. I signed and returned the evaluation to the National Research Council today. [Vera will receive her Ph.D. degree this semester. John Rasmussen will also receive his degree then, and we plan to offer him an instructorship in the Department of Chemistry for the coming semester or possibly longer.]

Wednesday, January 9, 1952

I had the usual number of phone calls and conversations today. I did find time to write to Charles Coryell about the Gordon Conference, saying that I made only a few minor changes on his general letter to prospective attendees and on his schedule. I suggested that he have the letter mimeographed since he is in a better position to have it cleared with Professor Parks. I also told Coryell that I checked the names on the list of recipients of those to whom I will mail the letters from Berkeley.

I also wrote an evaluation for Roy E. Heath to the American College Bureau (Chicago). [Heath was one of my group leaders at the Metallurgical Laboratory.]

Thursday, January 10, 1952

The morning group meeting was attended by Asaro, Bröwne, Carniglia, Carr, Carter, Cunningham, Dauben, Fischer, Glass, Glenn, Higgins, Hoff, Hollander, Huffman, Jaffe, Kalkstein, Levy, Nervik, Perlman, Rasmussen, Reynolds, Robinson, Seaborg, Skirvin, Slater, Street, Templeton, and Terwilliger (Doris Heisig).

Nervik reported first on his bombardment of copper with 340 Mev protons to form C^{11} , which gave a production cross section of 0.033 mb. In a bombardment of copper with 50 Mev protons, which should be below the

threshold for C^{11} formation, he found more counts of C^{11} than in the 340 Mev proton bombardment. There was considerable discussion about impurities causing the apparent discrepancy.

Dauben said they measured the lattice constants of some $CeOCl$ prepared by Charles Koch. She gave the lattice constants of $LaOCl$ ($a = 4.119$, $c = 6.882$), $CeOCl$ ($a = 4.080 \pm 0.004$, $c = 6.830 \pm 0.007$), and $PrOCl$ ($a = 4.053$, $c = 6.809$). Dauben said the structure is tetragonal, $PbFCl$ -type. Additional lattice constants for oxychlorides are for samarium ($a = 3.982$, $c = 6.721$), europium ($a = 3.955$, $c = 6.692$), gadolinium ($a = 3.958$, $c = 6.674$), terbium ($a = 3.919$, $c = 6.634$). Cunningham added that they are trying to correlate the free energy of the vapor phase hydrolysis of the trichlorides with calculated changes in crystal energies.

Asaro reported that they have determined the abundance of the second highest energy alpha group in the Ra^{226} alpha radiations with the alpha-particle spectrograph as $5.51 \pm 0.04\%$ of the total alpha radiations. He also said that they ran a sample of U^{230} , prepared by Slater, in the alpha-particle spectrograph and found two groups separated by 69 ± 3 kev. The higher energy group is 77% abundant and the lower energy group is 23% abundant. Asaro noted that the first daughter of U^{230} , Th^{226} , has an alpha spectrum consisting of two groups separated by 115 ± 3 kev, with the higher energy group being 78% abundant and the lower energy group being 22% abundant. The second and third daughters, Ra^{222} and Em^{218} , have no observable complex alpha structure.

Robinson presented a graph of constant error curves for his calculations of the geometry of low geometry counters.

Browne showed a chart of the observed x-rays of bismuth from Pb^{210} decay from experiments with his bent crystal spectrometer, which he compared with work of K. Siegbahn (Sweden) and M. Frilley (Mme. Joliot-Curie's laboratory). There was a discussion about why L_1 transitions are obtained here and not in the higher elements. Browne also presented a list of the gamma rays observed in Pb^{210} decay and related his work to that of San-Tsiang Tsien. He pointed out that Tsien's value of 42.6 kev is not inconsistent with his result since he (Browne) would not have observed such low intensities. Browne said that his 23.31 kev gamma ray is a second order reflection of his 46.69 kev gamma ray. In the discussion Perlman said that we may be using too high a decay energy for Pb^{210} (70 kev).

* * * * *

I stopped in and spoke with some of my colleagues in the Department office before giving the Chemistry 123 lecture, which today was a continuation of the subject of radiochemistry.

The following arrived from Robert M. Underhill (Secretary of the Regents):

THE REGENTS OF THE UNIVERSITY OF CALIFORNIA
OFFICE OF THE SECRETARY AND TREASURER

ROBERT M. UNDERHILL
Secretary and Treasurer
GEORGE D. MALLORY
*Assistant Secretary and
Assistant Treasurer*
MARJORIE J. WOOLMAN
Assistant Secretary

240 ADMINISTRATION BUILDING
BERKELEY 4, CALIFORNIA

January 7, 1952

Professor Glenn T. Seaborg
2808 Ellsworth Street
Berkeley 5, California.

NOV 19 11 10/52
RECD. JAN 10 1952
AMS.
FILE

Dear Professor Seaborg:

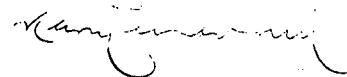
At their meeting held on December 14, The Regents of the University unanimously adopted the following resolution:

WHEREAS, Dr. Glenn T. Seaborg, Professor of Chemistry, and Dr. Edwin Mattison McMillan, Professor of Physics, through their cooperative and independent researches have created and identified six new chemical elements previously unknown on earth, for which achievement the Swedish Royal Academy of Sciences has conferred upon them jointly the Nobel Prize in Chemistry for the year 1951; and

WHEREAS, This international recognition of their pre-eminence in the field of Trans-Uranic elements, concerns only a part of the fundamentally important contributions they have made to science, to teaching, and to the military security of the United States and its allies, both during World War II and in the years that have followed; therefore, be it

RESOLVED, That The Regents of the University, by formal vote extend to Professor Seaborg and Professor McMillan their hearty congratulations on the honor which they have won, and their sincere appreciation of the part each has played in maintaining the eminence and extending the usefulness of the University of California as a teaching and research center serving the people of this State, of the Nation, and of the World.

Sincerely,



Another letter arrived from Charles Coryell about a couple of changes in the participants in the Gordon Research Conference this coming summer. I immediately replied that I agree with his letter. I also added that I was interested in his note with Suess on "Empirical Evaluation of Unpaired Spin Effects for Protons and Neutrons," adding that Kohman has recently made an interesting contribution in this connection in which he (Coryell) might be interested.

I recently wrote to J. W. Buchta (Editor, Reviews of Modern Physics) to ask if the journal is interested in publishing a revised version of the "Table of Isotopes." [Jack Hollander has been reviewing the literature and bringing the 1948 "Table" up to date.] Buchta wrote, in a January 7 letter,

We shall indeed be pleased to bring up-to-date the "Table of Isotopes." I believe we are justified in publishing a revised table although I presume the time will come when the expense of such revisions will not be justified in terms of the amount of new material. I shall consult with one or two of our associate editors, but I have no doubt but that they will agree with me in recommending publication. This is my first note to you since the recent high honor came your way. American scientists are pleased and believe the award fully merited.

Friday, January 11, 1952

Included in this morning's activities was a phone call to Joe Kennedy in St. Louis to discuss a number of matters, including the Gordon Conference and the Buffalo ACS meeting. Since there is a shortage of papers for the Buffalo meeting, I told Joe that Stan Thompson and I, along with Dick Diamond as co-author, have decided to present a paper "Masses of Translead Nuclides." I said that I will mail him two copies of the abstract, which I did. A copy of the abstract also went to Dick Diamond, along with a note explaining that we are considering presenting it at the Buffalo ACS meeting. I sent a copy of the abstract to Truman Kohman. I explained to Truman that Stan is an author because of his early work and that Diamond, because of the extensive calculations he made. I asked if he wants to be associated with the paper, saying there is plenty of time to change the authorship but I didn't want to do it without his permission.

I read a January 8 note from John Lewellen, thanking me for sending him the correspondence from the Elmer Avenue School fifth graders about the book You and Atomic Energy. John reported that he sent it on to the Children's Press publisher, who during a policy crisis, held out for continuing the You book series instead of turning the company exclusively into the more profitable line of publishing color books for the dime stores.

Also, from Robert M. Underhill (Secretary and Treasurer of the Regents) I received for inspection and return their only copy of our Contract No. NDCrc-201 (covering our early work on plutonium, which contains the long patent clause giving the Government an irrevocable option to purchase a non-exclusive license).

COPY

Contr. No. NDCrc-201

MEMORANDUM OF AGREEMENT made this 19 day of June 1941, effective as of the 1st day of April 1941, between the National Defense Research Committee of the Council of National Defense (hereinafter called "the Committee"), for and on behalf of the United States of America, and the Regents of the University of California, Berkeley, California (hereinafter referred to as "the Contractor").

WHEREAS, the Contractor conducts and maintains an experimental testing and research laboratory or laboratories and the Committee desires that the Contractor conduct studies and experimental investigations in connection with certain elements and report the results thereof to the Committee;

NOW, THEREFORE, THIS AGREEMENT WITNESSETH:

1. The Contractor agrees to supply the necessary laboratory facilities, materials, and skilled technicians and to undertake the necessary preparatory work for and to conduct, with the utmost secrecy and dispatch, in accordance with instructions heretofore or hereafter issued by the authorized representative of the Committee, studies and experimental investigations in connection with the production of elements No. 93 and 94 by means of the cyclotron. The Contractor shall furnish a full and complete final report of its findings and conclusions not later than October 1, 1941. The authorized representative of the Committee for the purposes of this paragraph is Dr. Lyman J. Briggs, in care of the National Bureau of Standards, Washington, D. C.

2. The Committee agrees to pay the Contractor for the services and materials necessary for performance of the work required under Par. No. 1 an amount, not exceeding a total of Seven Thousand Five Hundred Dollars (\$7,500.00), determined by the number of hours during which the Contractor operates the cyclotron in carrying out such work, at the rate of Twenty-five Dollars (\$25.00) per hour. Subject to the total limit above specified, payments shall be made on or about the first of each month at the foregoing rate for the number of hours during which the Contractor has operated the cyclotron in the preceding month in carrying out the work required under Par. No. 1, except that the final monthly payment shall not be made until after receipt of the final report required by Par. No. 1.

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3. (a) The contractor hereby grants to the Government of the United States an irrevocable option to purchase a non-exclusive, license or licenses, subject to the payment of royalties, to make, have made, and use, for military, naval, and national defense purposes, and to sell in accordance with law, material, and to use processes, under all United States patents and applications for patents owned or controlled by the contractor covering inventions heretofore developed and reduced to practice and concerned with the subject matter of this contract. Any such license shall be granted upon reasonable terms subject to negotiation at the time the Government may desire to exercise its option hereunder.

(b) The contractor agrees to and does hereby, in consideration of the premises and in consideration of payments to be made by the Government under this contract, grant unto the Government a non-exclusive, irrevocable, royalty-free license, to make, have made, and use, for military, naval, and national defense purposes, and to sell or otherwise dispose of in accordance with law, material, and to use processes, under all inventions made in carrying out the work contemplated by this contract, including all inventions which for the first time were reduced to practice as a result of the work contemplated by this contract, whether patented or unpatented. The contractor agrees to make to the Government, prior to the final settlement under this contract, a complete disclosure of all inventions made in carrying out the work contemplated by this contract and to designate in writing which of the said inventions have been or will be covered by applications for patents filed or caused to be filed by the contractor. The contractor shall have the right, upon notification by the Government, to elect whether it or the Government shall file applications for patents on inventions in addition to those designated by the contractor as aforesaid.

(c) As to all such inventions that are not covered by applications for patents as specified in subparagraph (b) the contractor agrees that the Government shall have the right, at the Government's expense, to file, prosecute, and act upon applications for patents thereon, and the contractor shall secure the execution of the necessary papers and do all things requisite to protect the Government's interest in prosecuting such applications to a final issue. When an application for patent is filed by the Government as aforesaid, all right, title, and interest in and under the patent shall be assigned to the Government by the contractor except that the contractor may retain a non-exclusive, non-transferable license.

(d) The contractor covenants that he has not entered into and will not enter into any arrangement to evade the intent of this Article for the Government to obtain without further payment a non-exclusive license to patents, applications for patents and inventions as called for in Paragraph (b) above.

(e) It is agreed that the execution of this contract shall not constitute a waiver of any rights the Government may have under patents or applications for patents.

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4. The Contractor agrees never to disclose any information concerning this contract or obtained as a result of the work called for in Par. No. 1 hereof to any person, except employees assigned to such work, without the written consent of the Committee or its authorized representative.

5. The Contractor agrees that it will immediately submit a confidential report to the Committee whenever, for any cause, it has reason to believe that an active danger of espionage or sabotage exists at the site of any of the work called for in Par. No. 1 hereof. This report shall contain complete information relative to the reasons which cause the Contractor to be apprehensive of such danger.

6. The Contractor agrees that it will, whenever requested by the Committee or its authorized representative, report to the Committee the citizenship, country of birth, or alien status of any or all of its employees at the site of, or having access to, any of the work called for in Par. No. 1 hereof.

7. The Contractor agrees that it will refuse to employ on, and will exclude from the site of, any of the work called for in Par. No. 1 hereof, any person or persons designated by the Committee or its authorized representative for cause as undesirable to have access to such work. The Contractor further agrees that it will, upon request of the Committee or its authorized representative, discharge or transfer, and thereafter exclude from the site of such work, any person or persons already employed, who may be designated by the Committee or its authorized representative for cause as undesirable to have access to such work.

8. The Contractor agrees at all reasonable times to permit representatives of the Committee to visit and inspect the work called for in Par. No. 1 hereof, and to report the progress of such work from time to time upon request of the Committee or its authorized representative.

9. No Member of or Delegate to Congress, or Resident Commissioner, shall be admitted to any share or part of this contract or to any benefit that may arise therefrom, but this provision shall not be construed to extend to this contract if made with a corporation for its general benefit.

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10. If, at any time, the Committee is of the opinion that the progress of the work called for in Par. No. 1 hereof indicates that such work cannot profitably be carried to conclusion, the Committee shall have the right to terminate this agreement upon thirty days' notice in writing to the contractor. In the event that this agreement shall be so terminated, the Committee agrees to indemnify the Contractor against loss upon any outstanding commitments, including those for personnel, which the Contractor may have made by reason of the work called for in Par. No. 1 hereof, and which the Contractor is unable to cancel, provided however, that in no event shall the maximum amount payable under this paragraph exceed the total amount payable under Par. No. 2 hereof, less any amounts actually paid to the Contractor under that paragraph prior to notice of termination. Upon receipt of the notice of termination herein provided, the Contractor agrees to exercise all reasonable diligence to obtain the cancellation of any outstanding commitments which it has.

IN WITNESS WHEREOF, the Committee and the Contractor have caused this agreement to be signed and sealed, intending to be legally bound thereby.

NATIONAL DEFENSE RESEARCH COMMITTEE
OF THE COUNCIL OF NATIONAL DEFENSE

Witnesses:

/s/ Samuel Callaway By Vannevar Bush (SEAL)
Chairman

The REGENTS OF THE UNIVERSITY OF CALIFORNIA

By /s/ Robert M. Underhill (SEAL)

APPROVED AS TO FORM:

s/ A. H. Conard
Associate Attorney for the Regents
of the University of California

I, Winifred I. Williams certify that I am the Assistant secretary of the corporation named as contractor herein; that Robert M. Underhill who signed this contract on behalf of the contractor, was then Secretary of said corporation; that said contract was duly signed for and in behalf of said corporation by authority of its governing body, and is within the scope of its corporate powers.

(Corporate)
/s/ Winifred I. Williams (Seal)

This is the contract made in June 1941, with the retroactive effective date of April 1, 1941, and of which my coworkers and I were unaware at the date of its execution (June 1941); it apparently did not relate to the measurement of the slow neutron fission cross section of Pu²³⁹. Underhill added that he has not been able to locate, either in his files or in the President's office, a letter of June 12, 1945, from Robert A. Lavender to Robert G. Sproul. He said he will make another search for it, but perhaps Lane may have to get the copy from the AEC files.

Saturday, January 12, 1952

I went to the laboratory for a while, read reports, and talked with some of the fellows.

Sunday, January 13, 1952

I again read and worked on my writing projects. The weather hasn't been good enough for outdoor work.

Helen and I have decided to use some of the Nobel money to add a couple of bedrooms to the children's wing of our house since Stephen's arrival has left the house overflowing with children. Bill Rice has discussed the addition with us and will prepare the drawings. We hope that the construction will begin some time in the spring.

Monday, January 14, 1952

Monday morning brought its usual number of phone calls, conferences, etc. I learned about another meeting of the IO-7 Panel on long-range detection to be held on January 30 and 31. I telephoned Manning and Katz in Chicago about visiting there on the way back from the meeting in Washington.

Another letter, dated January 11, arrived from Charles Coryell about details of the program for the Gordon Research Conference. Coryell also mentioned a sample of plutonium and other display material on new elements for the American Academy of Arts and Sciences. I replied to Coryell and suggested that, when he thinks of new names of people to invite to the Conference, he might send the general letter to them directly without contacting me, adding that I shall do the same and that we should send our own lists to each other for information. I reported that I have learned that Joe Kennedy is accepting our invitation and that he should do what he wishes about inviting Russell Williams. On the other matter, I wrote that I asked Burris Cunningham to look into the matter of samples for display by the American Academy of Arts and Sciences, and I believe Burris is proceeding as fast as possible, noting that we hope to receive permission for display of the americium and neptunium samples although one can never be sure.

In today's mail were also letters from both Margaret and Ivan Kurbatov and James R. Bercaw (Phi Lambda Upsilon), explaining that the invitation to speak at Ohio State University was a joint invitation. Bercaw said the printing of the manuscript could be modified in any way I like and the dates I suggested, March 28 and 29, would fit their schedule beautifully. The Kurbatovs wrote that they would like me to be their house guest while in Columbus and said they hope that I will not have to rush away immediately after my last lecture on Saturday morning but can stay through the afternoon and evening to meet some of the Ohio State staff.

One day last week Stan McCaffrey of the California Alumni Association asked me to join President Robert Gordon Sproul on a week's tour (February 10 to 17) of alumni groups in California. I agreed to do this, and today a confirmatory letter arrived from Sproul, who explained that

the purpose of these tours is to strengthen local alumni associations, etc. Sproul said the tours include the President, who speaks on University affairs, and three members of the faculty, who speak on interesting aspects of their specialities. The group travels by automobile, stopping for alumni luncheons or dinners in the principal towns and cities. He added that the trip sometimes becomes a bit wearing, but past participants have found it to be an enjoyable and interesting experience.

Tuesday, January 15, 1952

Iz is giving the Chemistry 123 lecture (on radiation chemistry) today, so I took the opportunity of a relatively free morning to catch up on some of my correspondence.

A memorandum went to Don Cooksey, requesting approval for the trip to Washington on January 30 and 31 in order to attend the meeting of the IO-7 Panel and to consult with Drs. W. M. Manning and J. J. Katz at Argonne National Laboratory in Chicago on February 1.

A rather long letter went to Colonel G. Glaser (Assistant DCS/O for Atomic Energy, Headquarters, USAF, Washington, D. C.), in which I described Major Charles I. Browne's graduate research and suggested the desirability of a postdoctoral year for Browne. In addition to such an appointment being of great value to our group, I wrote:

There is also a more specific reason why it would be of value for Major Browne to spend further time here. We are currently getting under way some important measurements pertinent to the function of the IO-7 Panel of the Research and Development Board's Committee on Atomic Energy. Early work on this problem was highly illuminating, and we are now in the process of improving the equipment. If Major Browne were to remain in this laboratory, we would suggest that he spend a major part of his time participating in this program.

Bill Libby wrote on January 8 and described Luis Marquez' (our former graduate student) work of the past two years. Libby asked if I would be willing to support Marquez for an assistant professorship at one of the schools with high energy accelerators since Marquez is extremely interested in the reactions of high energy proton beams. Libby suggested schools such as Rochester, Stanford, Cal Tech, and possibly Illinois. He expressed some concern about Marquez' accent as far as teaching is concerned. In today's response I wrote that we have a high regard for Marquez and will support his application at such places as he [Libby] decides are appropriate. I noted that, because of conversations I have had with people from Cal Tech, I doubt that they are interested in adding a nuclear chemist. I also pointed out that I have a little doubt about Stanford adding a nuclear chemist and I know that Albert Noyes (Rochester) will not start any young man above the instructor level.

I replied to a January 10 letter from Eric A. Lof (American Society of Swedish Engineers). Lof congratulated me on the Prize, mentioned that his home was in Grängesberg (my mother's home town in Sweden), and invited me to attend the Society's annual dinner on February 9, if I happen to be in the East. I thanked Lof for his letter, briefly

described our visit to Stockholm, and said that my schedule is such that I shall be unable to be in New York around February 9. I asked that he extend my congratulations to Dr. Richard Soderberg (Massachusetts Institute of Technology), the 1952 recipient of the John Ericsson Gold Medal.

The Svedborg wrote on January 4 to remind me that I promised to send him an autographed photograph for their library at the Gustav Werner Institut för Kärnkemi. In today's response I said that I will send him the photograph as soon as I have a chance to have one taken since those that I have are many years old and not suitable for such a position of honor. I also expressed my appreciation for the nice visit we had in Uppsala, saying that this was our first introduction to the Lucia ceremony. I wrote, "We were glad to find on our return that our children had survived our absence in good shape and were both happy and unhappy to learn that they had missed us very much."

In a January 6 letter William Gladis from Lakewood, Ohio, and someone who has written previously, gave me some Russian references to reports of the discovery of elements 99 and 100. I sent Gladis a translation of one of the articles, noting that there is no reference in it to element 99 or 100. I suggested that Gladis contact the Western Reserve University library for the journal. I then mentioned that I have learned that Professor Luis Alvarez mentioned our attempts to produce these elements in an address he gave in England the year before last. This, I said, was apparently misquoted by the British press as saying that we had produced these elements.

Homer F. Priest (Project Lincoln, Massachusetts Institute of Technology) wrote on January 7 to congratulate me on the Nobel Prize and to inform me that Project Lincoln, whose purpose is to carry out research and development for a satisfactory air defense of the United States, is interested in building up a staff of chemists for the program. Priest mentioned that the work primarily will be in the purification and production of various germanium modifications and will extend to other potential transistor materials. In my response I said that we will keep the positions in mind and, since other non-nuclear chemists may be interested, I shall send a copy of his letter to Dean K. S. Pitzer of the College of Chemistry.

I wrote to Donald Lane to inform him that I find his draft of the stipulation, as enclosed in his letter of January 7, 1952, very satisfactory. I told him that Mr. Underhill has been unable to find Lavender's letter of June 12, 1945 to Sproul and that perhaps he (Lane) can get this from the AEC files or wherever Lavender's correspondence is to be found. I mentioned that I can not recall if I received \$1.00 in this connection. I also enclosed a copy of Contract NDCrc-201, which I had had Doral copy, saying that it covers the work involving the date of April 1, 1941 in case it is of interest to him. I wrote, "You will recall that I told you that this contract was entered into, unknown to the inventors, in June, 1941 and was made retroactive to April 1, 1941; it was not specifically directed to the measurement of the slow neutron fission cross section of 94^{239} ."

To Professor Dr. E. Pietsch (Gmelin Institut, Schliessfach, Germany)

I sent a thank-you note for his congratulatory letter. I mentioned that we were interested to hear of the Gmelin volumes that were issued last year and plan to purchase them for our chemistry group here. I then told Doral to check on the volumes available and the price.

A thank-you note also went to P. Veeraraghavan (Madras, India) for his letter of congratulation.

Wednesday, January 16, 1952

When I made the rounds of some of the labs to talk with the researchers, Al Ghiorso told me that W. E. Urry of the U. S. Air Force headquarters in Washington, D. C. had telephoned him and requested that he attend the meeting of the IO-7 Panel. I told Al that I plan to stop in Chicago on the way back from the meeting of the Panel and suggested that he do the same.

I then wrote to Joe Katz to remind him that I shall need a reservation for some sleeping space near the Argonne Lab for the night of January 31 and to inform him that Al Ghiorso will be coming with me.

A letter went to Robert M. Underhill to express my appreciation for the resolution of the Regents that he sent me on January 7. I wrote:

I feel very fortunate to be the subject, with Dr. McMillan, of such a resolution by the Regents, and I want to say that research such as that which was cited is made possible only because of the contributions of many University staff members, the presence of excellent facilities, and a long-standing background of enlightened support from the University administration and Board of Regents.

I also replied to a nice congratulatory note from Mary and Vance Cooper:

We were very pleased to hear from you, especially in memory of the old Chicago and Oak Ridge days of collaboration. In fact, we think that you should be credited with most important 'assist' for your vital contributions towards making it possible for me to avoid the Oak Ridge canteen with its terrible consequences.

[Visitors to Oak Ridge during the war usually became ill from the food and water; however, the Coopers helped me avoid this by having me as a house guest.]

A January 11 note arrived from Jerome D. Luntz, saying that he is sending me a complementary copy of the newly expanded January issue of Nucleonics, whose front cover carries photographs of me and the three other Nobel Laureates in the field of nuclear science for the year of 1951.

I also received a January 14 letter from Carl B. Tupala, Promotion Manager, Station WJPD, Ishpeming Broadcasting Co., Ishpeming, Michigan. Tupala described the "March of Dimes" campaign, sponsored by the Ishpeming Junior Chamber of Commerce and Station WJPD, and asked if I would contribute a short tape or disk recording on my work, my trip to

Sweden, or whatever, closing with a plea to support the "March of Dimes" drive and thus combat infantile paralysis. Since this is a worthwhile cause and for the city of Ishpeming, I told Doral to check with Dan Wilkes about preparing a script and a recording for the program.

Later I worked on the draft of the article, "Plutonium," for the Encyclopaedia Britannica. Burris Cunningham has spent considerable time on this draft.

Thursday, January 17, 1952

Present at the meeting of our research group this morning were Asaro, Carniglia, Carr, Carter, Cunningham, Dauben, Dunlavey, Feay, Fischer, Fleming, Glass, Gunn, Higgins, Hoff, Hollander, Hulet, Jaffe, Jenkins, Kalkstein, Koch, Levy, Martin, Michel, Nervik, Perlman, Rasmussen, Ruben, Seaborg, Slater, Street, Templeton, and Worthington.

First to report was Higgins, who talked about a 40-Mev deuteron bombardment of Pu^{239} to produce americium isotopes. When he counted the americium samples on the Nucleometer and the alpha pulse analyzer, he saw an activity of about 1.7-hour half-life in the Nucleometer decay curve, probably due to a mixture of Am^{237} and Am^{238} . Assuming this short-lived activity is due to equal amounts of Am^{237} and Am^{238} electron capture activity, he found a branching ratio of $K/\alpha = 2.5 \times 10^3$ for Am^{237} or a partial alpha half-life of about 0.4 years. The alpha-particle energy was 6.07 ± 0.1 Mev. Higgins also observed 12-hour Am^{239} (alpha particle energy of 5.77 Mev). He calculated for this the ratio $K/\alpha = 1.2 \times 10^4$, corresponding to a partial alpha half-life of 16.4 years. A previous lower energy deuteron bombardment gave a partial alpha half-life of about 30 years. Also on the previous lower energy bombardment, the half-life of Am^{238} was determined as 2.1 hours with no alpha activity associated with the electron capture decay. Higgins said the 1.7-hour component in the 40-Mev deuteron bombardment is presumably an average of the half-lives of Am^{238} and Am^{237} , so the half-life of Am^{237} is probably about 1.3 hours. I brought up Ghiorso's suggestion that the activity presently assigned to Cm^{238} is really Cm^{239} , based on carbon ion bombardments of thorium, and said that this is a problem that should be unraveled.

Slater announced that he now has 5×10^6 c/m of U^{232} separated, and he and Asaro plan to look at the alpha spectrum in the magnetic spectrograph. Dunlavey will also look for electron-alpha coincidences in his electron-sensitive photographic emulsions.

Koch said that he has been continuing the equilibrium studies on the hydrolysis reaction: $\text{RECl}_3(\text{s}) + \text{H}_2\text{O} = \text{REOCl}(\text{g}) + 2 \text{HCl}$, where H_2O and HCl are in the vapor phase. He presented tables giving their percentage disagreement with the International Critical Tables on the equilibrium vapor pressures of HCl over aqueous hydrochloric acid, noting that the errors in I.C.T. values do not affect their latest results on the hydrolysis studies since they measured HCl and H_2O partial pressures separately. Koch also described a radiation shield installed in their reaction chamber, pointing out that lack of the shield apparently did not contribute to error. He said he has completed work on the hydrolysis of SmCl_3 and then presented his results, along with those for lanthanum and

gadolinium chlorides. Koch said he plans to do terbium chloride next. There was a general discussion about the expected effects of change in crystal structure.

Martin talked about the radiative electron capture process, referring to early work by Schiff. He said that Bell also studied the simple K capture decay of Fe^{55} and found essential agreement with the Swiss work. The ratio of radiative to non-radiative electron capture decay is about 10^{-4} for a decay energy of 200 kev, and the ratio increases with energy. Martin, using a sample of Fe^{55} , free of Fe^{59} , found the ratio of radiative to non-radiative capture is of the same order of magnitude as Schiff's prediction; however, Martin said his sample had a considerable amount of iron metal, which made absorption corrections uncertain. I remarked that this is an interesting study. I also mentioned a note in the last Physical Review about the effects of chemical binding on the half-lives of nuclear isomers. Another interesting note, I pointed out, concerns the study of the pi meson reactions on nitrogen to produce C^{11} . Iz told the group about Browne's work that shows that two L x-rays of uranium following the complex beta decay of Pa^{232} are some 100 ev more energetic than accepted values. Browne doesn't think the discrepancy is due to instrumental effects.

* * * * *

After the meeting I stopped in at the Department of Chemistry office to talk with Miss Kittredge and some of my colleagues. Bill Giauque has spoken with me several times about seconding his nomination of Samuel Jackson Barnett (one of my physics professors at UCLA) for the 1952 Nobel Prize in Physics. I agreed to do so and said that I will speak with Joseph Kaplan (also at UCLA) about back-up material.

In the final lecture of the semester in Chemistry 123, I discussed heavy elements and atomic energy.

A request for an evaluation of Dean Dunlavey arrived from Helen G. Doty (Personnel Assistant, Arthur D. Little, Inc.). Doral composed a response, using my previous letters, which I signed and mailed today.

I wrote an additional memo to Don Cooksey to tell him that Al Ghiorso has been requested to attend the meeting of the IO-7 Panel of the Committee on Atomic Energy of the Research and Development Board in Washington on January 30 and 31. I asked for approval for Ghiorso to stop over at Argonne with me on February 1, pointing out that we shall try to get the Radiation Laboratory reimbursed for our travel expenses except for those incurred during our Chicago visit.

Friday, January 18, 1952

Again, I had a number of phone calls and conversations with various people about their work.

My correspondence was rather light today. I wrote to the Kurbatovs to tell them that I am accepting the Phi Lambda Upsilon invitation to come to Columbus on March 28 and 29. I wrote that I shall be happy to be their house guest during my visit, that I plan to arrive from Buffalo

during the evening of March 27, and that I shall let them know my arrival and departure times.

In a letter that arrived today, M. Cannon Sneed (University of Minnesota) asked if I want to revise my chapter on the actinide series for Comprehensive Inorganic Chemistry. Sneed explained that Volume I, in which my chapter is to appear, is now ready to be sent to press. I responded that I believe a number of changes are now in order, which I will try to make if he returns the manuscript to me (I wrote this chapter about five years ago). I noted that my schedule is such that it will probably be impossible to do as comprehensive a job as I should like; however, even with the minimum changes, the chapter will be lengthened considerably.

I also received a January 15 letter from Nathan Sugarman, saying they have decided to have Tony Turkevich alone report on the Chicago work at the Gordon Conference. His title will be "Report on Meson Radiochemistry at the University of Chicago Cyclotron." He suggested that Marquez report on his work on high energy reactions. Sugarman also asked about possible new instructors for the Institute for Nuclear Studies of the University of Chicago.

A Francis X. Carr (Washington, D. C.) wrote on January 7 to the University of California, inquiring if I were the Seaborg, a chemist, who was employed as a guide by the 1933 Chicago World's Fair. The letter was routed to me, and today I wrote Carr to suggest that Earl W. Seaborg, whom I do not know but who is a chemist and resides at 5424 University Avenue, Chicago, might possibly be the man he knew.

My contribution to the Encyclopaedia Britannica on "Plutonium" is being typed; I hope to mail it next week.

Saturday, January 19, 1952

In addition to spending some time with the children, I did some reading and writing.

Sunday, January 20, 1952

I again read journals and worked on some of my writing projects.

Monday, January 21, 1952

After checking on some of the research, I looked over the morning's mail and answered some of my correspondence.

In response to my recent telephone request I received a letter from Joseph Kaplan with some back-up information about Samuel Barnett and his work. Kaplan explained Barnett's work with relationship to that of Richardson and also to the work of S. A. Goudsmit and G. E. Uhlenbeck. I phoned Giauque and read the letter to him, then asked Dorai to have a copy made and taken down to Giauque on campus.

Edgar Westrum sent a paper, "The Heat of Solution of Neptunium Metal and the Heats of Formation of Some Neptunium Chlorides. A Microcalorimeter

for Heat of Solution Measurements" by Westrum and Eyring, to us to be processed for publication. He explained the reasons for the delay in publication. I told Doral to handle the details after getting an ok from Burris Cunningham. Westrum also asked about procurement of reprints for the Department of Chemistry and the Radiation Laboratory for both this paper and another one, "The Heat of Formation of Thorium Sesquisulfide" by Eyring and Westrum, which will be ready for submission about February 1.

Another letter arrived from John D. Voelker (Ishpeming), who said that one of the reasons he has not written the article about me is that he has been up to his hips writing a book, which should be finished in about a month. Voelker said that he believes that he needs to cover the work on which I am now engaged and asked if I would be able to cooperate with him on the project.

Truman Kohman wrote on January 14 to say that he will attend the Gordon Conference although his family will be unable to attend. Truman offered some suggestions for the programs and said that he would like to present some ideas on a new type of statistical mass equation on which he is working. He also offered some topics for future conferences or symposia: (1) radioactive measurements of geologic times and (2) yields of nuclear reactions.

As usual we discussed a number of research and administrative matters at our noon senior staff lunch meeting.

In the afternoon I proofread my manuscript on plutonium for the Encyclopaedia Britannica.

Tuesday, January 22, 1952

I again wrote to Charles Coryell about changes in the Gordon Conference programs, suggesting that he strike Kohman and Sugarman off the program on "High Energy Nuclear Reactions" and put Turkevich and Marquez on, and possibly add Kohman to the program on "Decay Energetics and Systematics." I made a few other suggestions and then asked Coryell to decide which alternatives to follow. [Coryell has been doing an excellent job on the programming.]

I then wrote to Truman Kohman, in response to his letter of January 14, to inform him that we will put him down for the program on "Decay Energetics and Systematics," saying that we do not need a definite title now. I added that I shall write him on the other matters separately.

I also replied to Sugarman's letter of January 15 about the Gordon Conference. I agreed that it would be worthwhile to add Marquez to the program and said that I shall write separately if it appears we have any good prospective men for his instructorship.

On another matter I wrote an evaluation of Per Kofstad for the Berkeley Committee on Fellowships and Graduate Scholarships. I concluded my report by saying that I believe he is deserving of fellowship or scholarship support.

Wednesday, January 23, 1952

After additional phone conversations with Bill Giauque, today I wrote a seconding letter to his nomination of Samuel Jackson Barnett for the 1952 Nobel Prize in Physics:

I have discussed carefully with Professor Giauque Dr. Barnett's research record, and we feel that it is such as to merit serious consideration. Although Professor Giauque summarizes and analyzes Dr. Barnett's research contributions in some detail in his nominating letter, I should like to reiterate in summary, that Dr. Barnett's first experimental finding of the anomalous ratio of the electronic magnetic to mechanical moments is an outstanding contribution to physics since it opened the way to the formulation of the electron spin, which had a tremendous impact on the entire area of atomic structure. It seems to me that S. A. Goudsmit and G. E. Uhlenbeck are also worthy of consideration for their outstanding contribution in this connection.

I went on to describe my personal knowledge of Barnett and to say I believe he is a worthy candidate for consideration as recipient of the 1952 Nobel Prize in Physics. I mailed this letter to the Nobel Committee for Physics, Stockholm (with a blind carbon to Giauque).

Hari Sharma, our former graduate student who has returned to India, wrote a congratulatory letter on January 14 and then asked that I write a letter of recommendation for him for a position as a lecturer in the Chemistry Defence Academy, Dehradun and for a fellowship in Canada. Hari mentioned that the Atomic Energy of India has not offered him a job as yet and that it will be hard to get a teaching position in a university in India. He said that he is looking into a possibility of going to Sweden (his wife is Swedish). Finally, he added that he has mailed a bound copy of his thesis to me by sea. Doral prepared these letters, using the others we have written for him as a pattern, for me to sign.

Today I received a reminder from Dr. Arne Holmberg (Editor of "Les Prix Nobel en 1951") to send him (Kungl. Vetenskapsakademiens Bibliotek, Stockholm) a number of items, including the text of the speech I gave at the Town Hall banquet, my photograph, and a short autobiographical note.

Anna May Schenck (Science Editor--Yearbook, The American Peoples Encyclopedia) requested that I prepare an article on isotopes, for a survey article covering important developments in atomic energy in 1951. She offered a \$20 honorarium for a 500-word article, which is due February 22. Since my time is so limited, I asked Dave Templeton, who agreed to prepare the article if Schenck agrees. I then transmitted this information to Miss Schenck, explaining that Professor Templeton is eminently qualified to prepare such an article.

Iz and I spent some time together preparing a final examination for Chemistry 123.

Our supply of letters of invitation to The Gordon Research Conference on Nuclear Chemistry arrived from Charles Coryell, and Doral had one of the girls mail them out to our list of prospective attendees.

UNIVERSITY OF CALIFORNIA, RADIATION LABORATORY
BERKELEY 4, CALIFORNIA

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
LABORATORY FOR NUCLEAR SCIENCE AND ENGINEERING
CAMBRIDGE 39, MASSACHUSETTS

January 22, 1952

The Gordon Research Conference on Nuclear Chemistry

New Hampton, N.H., June 23-27, 1952

Dear Colleague:

The authors of this letter have been asked by the Program Committee of the GORDON RESEARCH CONFERENCES to arrange a program in Nuclear Chemistry for the 1952 Conference series, to be held June 23-27, 1952 at the New Hampton School, New Hampton, N.H. We are writing this informal letter to several hundred people to explain the conference aims, since this is the first conference planned for Nuclear Chemistry and if it proves to be as successful as most of the others, it is likely that further meetings will be arranged in later years.

These conferences, under the sponsorship of the American Association for the Advancement of Science, are designed to bring together for a leisurely and friendly week specialists in the field from the universities, from industry, and from government, to cover in detail the problems at the front of the field of science. Sessions are held for 4.5 days mornings and evenings, with an invited discussion leader and several speakers, but all those in attendance participate in discussion in the informal sessions and in the free periods. The discussions are based on ideas and developments in progress, and neither abstracts nor written reports are required. A tentative and rough outline of the program is listed below, including the names of some of the people who have been specifically asked to report. Informal reports by representatives of any foreign laboratories will be especially welcome.

New Hampton is in the middle of the vacation state of New Hampshire in a good place to use the afternoons for swimming, hiking, golf, or bull sessions. It is about 110 miles north of Boston, and is served by trains from North Station, Boston, to Franklin, N.H. Accommodations are available for wives and for children over six, and the food is excellent.

The whole list of Gordon Conferences will be announced February 18 in Science and in Chemical and Engineering News. Applications for attendance are to be submitted to Professor W. George Parks, Department of Chemistry, University of Rhode Island, Kingston, R.I. Attendance is regulated by committee action to keep the number present within limits of accommodations and to a figure consistent with maximal friendly interchange of ideas.

We hope very much that you will give this conference consideration, and that you will discuss it with other nuclear chemists who might not have heard of it.

Glenn T. Seaborg
Glenn T. Seaborg
Charles D. Coryell
Charles D. Coryell

Thursday, January 24, 1952

The group meeting this morning was attended by Asaro, Bertelli, Carr, Dauben, Dunlavey, Fischer, Glenn, Gunn, Higgins, Hollander, Hyde, Jaffe, Jenkins, Kalkstein, Rasmussen, Reynolds, Robinson, Ruben, Seaborg, Slater, Templeton, and Thompson.

Jenkins said that he has continued milking emanation from Ra^{228} with the object of extending the half-life limits of Em^{224} beyond those previously reported. He performed a continuous flow experiment in order to build up the amount of Ra^{224} that would grow from the beta decay of Em^{224} and, after ten hours, no Ra^{224} was seen. Assuming the alpha branching of Ra^{228} to be 5×10^{-7} , the lower half-life limit for Em^{224} may be extended to less than 0.5 minute. I added that, if Fr^{224} were extremely forbidden and its half-life long, the results would be explained.

Thompson said that Am^{243} was bombarded at Chalk River to produce $\text{Cm}^{242,3,4}$. This curium was purified and rebombarded to enrich the heavy curium isotopes. Asaro looked at a fraction of the enriched curium in the alpha particle spectrograph and saw six alpha groups. Two were the known Cm^{242} groups of 6.110 and 6.065 Mev. There was another group at 5.965 Mev and three together at 5.741, 5.761, and 5.783 Mev. From the pulse analyzer results and the examination of Cm^{243} , obtained from the decay of Bk^{243} , the 5.965 and 5.761 Mev groups were tentatively assigned to Cm^{243} and the 5.741 and 5.783 Mev groups to Cm^{244} . Asaro said the ratio of the 5.965 to 5.761 Mev groups was about 1:2. Asaro also said they will examine a sample containing Cm^{242} and Cm^{244} to confirm these conclusions, adding that there is still an unexplained bump in the tail of the Cm^{242} group. I suggested they should also examine the unbombarded fraction of $\text{Cm}^{242,3,4}$ to obtain the change in ratios of the peaks and thus calculate the neutron absorption cross sections and also examine the low energy region for evidence of Cm^{245} . Thompson said the Pu^{238} growing in from Cm^{242} would be near the expected energy for Cm^{245} and would make this examination difficult.

Hyde reported on bombardments of thorium with 350 Mev protons in which the resulting francium isotopes were mass separated on the time of flight mass spectrometer: Fr^{212} --initial pulse analysis showed a 6.36 Mev alpha group and the 6.23 Mev group of Em^{212} growing in with a 20-minute decay period. Fr^{211} --pulse analysis showed a single 6.23 Mev alpha particle with a 27-minute half-life. Fr^{210} --pulse analysis showed a 6.23 Mev alpha group decaying with a 25-minute half-life. Earl added these conclusions could all be reasonable since the alpha energies change slowly below the closed shell, but the electron capture half-life should shorten with decreasing mass number. He said Fr^{211} should branch by electron capture to Em^{211} , which is a 16-hour alpha, electron capture activity and, by Momyer's work, has an EC/α ratio of 6 so that the alpha daughters through At^{211} should have been observed. They were not, leading to the conclusion that the EC/α of Fr^{211} of 10/1 previously reported must be in error; calculations indicate something like 10^{-3} instead. Hyde went on to say that, if an astatine fraction is removed from an initially pure francium fraction after a few hours, much At^{208} , alpha daughter of Fr^{212} , is seen but no At^{207} or At^{206} from Fr^{211} and Fr^{210} . The K x-rays decay with the 1.8-hour half-life of

At²⁰⁸ and do not tail to longer half-lives indicating less than 0.0008 of any longer activity (Po²⁰⁷ or Po²⁰⁶). He also said they believe the results are valid but are puzzled how to explain them without postulating isomers for some of the astatine isotopes.

Dunlavey said Sm¹⁴⁷ alpha particles have been rechecked for electron coincidences by examination of the tracks produced in photographic emulsions; none were found. No tracks were seen on a preliminary examination of a sample that should have contained Sm¹⁴⁶. Rasmussen said that the Sm¹⁴⁶ was chemically separated from enriched Nd^{144,5} isotopes bombarded with helium ions and decay tracks should have been observed if its half-life is less than 10⁷ years.

Fischer reported on a 9-Mev proton bombardment of mass-enriched Sm¹⁵⁰. The Eu¹⁵⁰ produced decayed with a 12.5-hour half-life. Examination in the beta-particle spectrometer showed two groups of 1.76 ± 0.04 Mev (7.3%) and 0.98 ± 0.03 Mev (92.7%). There was also a 0.60 Mev group with a half-life shorter than three hours, which is not accounted for. She said the target had small amounts of Sm^{149,7} present, which led to the production of 25-day Eu¹⁴⁷ and 150-day Eu¹⁴⁹.

Rasmussen presented a formula for the calculation of multiple order neutron capture yields but explained the calculation is tedious and time consuming; the solutions will be made on the analogue computer in Bldg. 50. He also presented a possible explanation for the differences in x-ray energies observed by Browne in the beta transition of Pa²³² to U²³².

* * * * *

After the meeting I went to the final public examination for the degree of Doctor of Philosophy for William Ellis Glenn, Jr. (Electrical Engineering), held at 10 a.m. in Room 125, Cory Hall. Bill's committee consisted of Lauriston C. Marshall (chairman), Lester E. Reukema, David H. Sloan, Glenn T. Seaborg, and John R. Woodyard. Bill performed very well.

I then got together with Dan Wilkes and prepared a disk recording for the "March of Dimes" drive in Ishpeming, Michigan. I spoke briefly about my childhood in Ishpeming and my recent trip to Sweden (including the crowning of Stockholm's Lucia). I concluded by mentioning that in Sweden I was asked to speak at a huge gathering in behalf of the Salvation Army and that I believe it is important to give financial aid to organizations of this type of which the "March of Dimes" is an excellent example. I went on to say that the "March of Dimes" campaign is of vital importance and that funds obtained in this manner are desperately needed to combat and relieve the suffering due to infantile paralysis. Back on the hill I wrote a covering letter to Carl B. Tupala (Production Manager, Ishpeming Broadcasting Company), saying that I hope the recording arrives in time and thanking him for his congratulations about the Prize. I asked Doral to make certain the recording is sent air mail, special delivery and to send a night letter to Tupala to inform him that the recording was mailed.

I also mailed my 15-page article, "Plutonium," to John V. Dodge (Managing Editor, Encyclopaedia Britannica), noting that there are a

number of places where cross references to other articles could be used to advantage, but I am leaving this for him to do since I am not sufficiently familiar with the Encyclopaedia's content.

A rather long letter (of January 20) about a variety of subjects arrived from Dick Diamond (now back at Harvard), which I read and routed to the appropriate people. He said that Geoffrey Wilkinson agreed to send us a gram of scandium oxide and that Professor Kistiakowsky has a secret repository that he may use for documents. Dick asked if he would be able to get small samples of alpha-particle emitting tracers, such as some Ra²²⁶ and Am²⁴¹ left in his old lab in Berkeley. Finally, Dick pointed out that, if the paper on nuclear masses is to be given, it would be a good idea to recalculate the masses in the transplutonium region.

Since Al Ghiorso has become involved with special analysis work (with Los Alamos, the Air Force, and others interested in the analysis of debris from Soviet nuclear weapons tests), I have had several conversations with our business manager, Wally (W. B.) Reynolds, about the funding for Al's work. Reynolds learned that Los Alamos will not recommend support of the program from the AEC, so I said that I will try to get support from AFOAT. Today I received a breakdown of costs from C. E. Andressen, Jr., also of our business office: room alterations, \$1,000; steel shielding, \$1,000; electronic equipment, \$5,000; research labor, \$7,000; total, \$14,000. To this was added \$2,000 contingency, making the direct cost of \$16,000. Andressen added \$14,000 for indirect cost, making a total of \$30,000 for fiscal year 1952. He estimated that \$20,000 would be adequate for fiscal year 1953.

Friday, January 25, 1952

Mario Rollier wrote from Milan on January 18 to thank me for making possible his stay in Berkeley and to describe his return to Italy. He reminded me that he had talked with me in Berkeley about writing a supporting letter for his appointment as a representative of Italy on the Advisory Committee of the Joint Commission on Standards, Units, and Constants of Radioactivity. Today I wrote to Professor Antonio Nasini (Director of the Chemical Institute of the University, Corso Massimo d'Azeglio, Turin, Italy) and said that we were very much impressed by Professor Rollier during his recent visit to the Radiation Laboratory and feel that he would be admirably suited to serve in such a capacity.

In a letter to Professor George B. Kistiakowsky (Harvard) I said that we are arranging to send him two copies of a secret write-up of Dick Diamond's thesis for safeguard in his repository for secret documents in order to make them available to Dick for his work. I explained that the manuscript has been submitted for declassification but must be treated as secret material until it is declassified. I added that his daughter Vera has completed her thesis work, and we think that it was a good job.

I also wrote to Joe Katz, describing the situation with my chapter on the actinide elements for Sneed's Comprehensive Inorganic Chemistry and asking him to read the enclosed rough draft of my updated chapter. I told Joe that Ghiorso and I still plan to see him next Thursday and are planning to arrive on American Flight 71 at 10:25 p.m.

A memo went to Ray Wakerling about Charles Levine's thesis, "A Study of Naturally Occurring Plutonium" (UCRL-1040), to describe the differences between it and UCRL-1025 and the deletions necessary to issue it as an unclassified report.

Doral had the abstract of the remarks I plan to use on the Cal Alumni Lecture Tour with President Sproul typed, and I proofed them today. The talk will be titled simply "Atomic Energy."

Saturday, January 26, 1952

I went to the lab for a while in order to talk with some of the graduate students and other research workers. I also read reports and journals.

Sunday, January 27, 1952

Although I did some writing and reading, the kids took up quite a bit of my time today.

Monday, January 28, 1952

I tried to clear the mail off my desk this morning since I will be out of town the rest of the week.

Truman Kohman wrote on January 14 that he has enough uncommitted funds to support a postdoctoral man for the rest of the year. He wrote that he would prefer John Rasmussen, if he is interested, otherwise he would like to take Si-Chang Fung. In my response I wrote that Fung accepted a position with Tony Turkevich in Chicago, and I am sorry about this because I think his (Kohman's) position would have had a higher stature. I then described a couple of other possibilities--Gary Higgins and Luis Marquez. Finally, I wrote that John Rasmussen believes he should have academic standing in the position that he accepts and that we are putting him on as an instructor in the Chemistry Department here next semester. He will probably be offered an extension in that capacity next year if he doesn't find another academic position in the meantime.

To Lektor Erling Kofstad, who asked on January 19 about the progress of his son Per, I wrote that I believe Per is doing well in his graduate work towards the Ph.D. here. I reported that thus far he has received all A's in his class work, is held in good regard by his instructors, and is just now getting underway on his laboratory work.

A short note went to Joe Kennedy to inform him that I believe the program for the Buffalo meeting, which I just received, looks ok. I asked Joe if he, Frank (Long), or Milton (Burton) have any suggestions for a dinner speaker at the Buffalo meeting. I mentioned that one suggestion might be Bill Libby on his radiocarbon dating work.

I replied to a letter from President Edgar M. Carlson (Gustavus Adolphus College, St. Peter, Minnesota), who congratulated me on my recent trip to Sweden and invited me to participate in the United Nations Institute and Commencement Program at the College this year as the commencement speaker. I explained that my schedule is so heavy this year

that I couldn't arrange it so as to definitely be there on June 1st, and that I have never undertaken such a venture and thus have great doubts that I would be a very good performer. I said that I think it would be quite likely that I could undertake such an assignment another year when I have more time. I said that, if it should be possible to rearrange my travel schedule this year so as to place me in the East about that time, I should be more than happy to come up to St. Peter as one of their sponsors and possibly participate in the Institute, if it is not too late to make such arrangements. However, I will not know until after I have taken a trip East near the end of March.

A note went to Bernard Harvey to tell him that we are apparently making some progress on the question of his coming to work with us, and I am trying to follow the problem of getting permission and clearance through the authorities both here and in Washington. I apologized for the number of errors in the article in the recent issue of Chemical and Engineering News about the work on the neptunium series. Finally, I said that I hope he and others from Chalk River will be able to attend the Gordon Research Conference on Nuclear Chemistry in New Hampshire the week of June 23, 1952, and that Stan Thompson and Al Ghiorso send their greetings.

Doral prepared an evaluation form, based on previous material, for Alan Frank Clifford, who is applying for a fellowship administered by the National Research Council, which I signed today.

I replied to a January 22 letter from James Cobble (Oak Ridge), who accepted our offer of a postdoctoral appointment. Since Oak Ridge is considering granting Cobble a leave of absence, he asked if our offer was for a staff position or a postdoctoral fellowship. I explained that it is not easy to answer the question since it is a staff position that we treat as a postdoctoral fellowship because it is necessary for our scientists to be on the Radiation Laboratory payroll. I added that it seems to be that his coming will be consistent with a leave-of-absence arrangement with Oak Ridge, if this is mutually desirable with him and his superiors. I also agreed to Cobble's proposal that he arrive here in October or November and said that I look forward to seeing him at the Buffalo ACS meeting.

Ken Pitzer routed me a letter from Lloyd R. Zumwalt (Director, Tracerlab, Inc., Western Division), expressing interest in recent or potential graduates to fill positions as radiochemists at their Berkeley laboratory. I routed the letter to Iz and Earl with a note that Skirvin should be notified. [Skirvin's background has proven to be too poor for graduate school at Berkeley.]

I also wrote to Lars Gunnar Sillén to thank him for his help with the Swedish trip and to say that I am sorry to have learned indirectly from Kraus that he won't be able to attend the Gordon Conference this year. I sent my regards to Birgit and asked him to tell her that Helen and I are sorry that we didn't get to see her during our visit to Sweden.

A note also went to Ray Stoughton, in which I said that Worthington is completing his work on his Master's degree this week and leaving us, so it will be some time before anything further of a very serious nature

will be done on Ni⁵⁶. I wrote that we have summarized the present work in a "Letter to the Editor" form, which I am enclosing for his perusal and possible simultaneous publication with something from his group.

A response, dated January 23, arrived from Douglas C. Polhamus (Colonel, USAF) to my request that Charles I. Browne be retained at the University of California for postdoctoral training. Polhamus said that the existing policies within the Air Force preclude such an assignment. He noted, however, that it is contemplated that Browne will be assigned to the Special Weapons Command for duty at Los Alamos. They are sending a query to that group about my proposed utilization of Major Browne, and he will inform me of the final decision.

Margie Hollander reported to me that she has made arrangements for the shipment of the tracer samples of Ra²²⁶ and Am²⁴¹ to Dick Diamond at Harvard.

I managed, in spite of all the correspondence, to talk for a while with George Watt, who is spending a few days in Berkeley.

Tuesday, January 29, 1952

Art Mitchell, the laboratory driver, picked me up at home at 6:50 a.m. and drove me to the Oakland Airport, where Al Ghiorso and I were scheduled to take American Flight 918 to Dallas. Unfortunately, the Oakland Airport was fogged in, and we were bussed to San Francisco for the flight. George Watt, who was returning to Austin after a short visit in Berkeley, was also on the flight. This problem resulted in our arrival in Dallas being late; we missed our connecting flight to Washington and had to fly via Chicago in order to get to Washington. During our brief stop in Chicago, I wired the Mayflower Hotel in Washington about our late arrival.

[In Berkeley, the Chemistry 123 class was given its final examination:

CHEMISTRY 123

Final Examination
January 29, 1952

1. (15)

- (7) (a) Estimate the alpha energy of Ra^{228} .
- (8) (b) Using this value determine the percent alpha branching of Ra^{228} .
(Show methods used since there may be some variation in answers expected.)

2. (10)

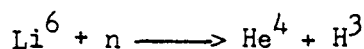
- (7) (a) Ag^{106} and Ag^{111} have almost the same half-life and would be difficult to distinguish using that property. If one had a sample of one of the two, discuss all measurements that might be made to identify it and distinguish it from the other.
- (3) (b) Suppose you had a long-lived mercury isotope suspected of being Hg^{206} ; what specific experiment could you do to make certain of the mass number assignment?

3. (10)

Construct the decay curve that would be obtained over a 30 hour period if one started with a mixture of equal activities of two nuclides which have half-lives of 3 hours and 10 hours.

4. (15)

- (10)(a) What will be the energy of He^4 and of H^3 formed in the reaction



with thermal neutrons?

- (5) (b) If the range-energy relationship for alpha particles is given by the expression

$$R = aE_{\alpha}^r,$$

what will be the range of tritium in this particular case in terms of the constants a and r?

5. (15)

Calculate the maximum amount of Na^{24} activity which can be made by the (d,p) reaction on a thin sodium target weighing 100 mg if the cross section for the particular energy deuterons used is 0.1 barn. The deuteron beam is 10 microamperes.

6. (35)

- (3) (a) Explain why the experimental fact that all nuclei with even A (whether Z is odd or even) have integral spin numbers is compatible with the neutron-proton concept of nuclear composition and not with the older proton-electron concept.
- (3) (b) Draw the most likely decay scheme for a β^- -emitter having two beta groups with end points at 1.20 Mev and 0.65 Mev and two gamma rays of 0.35 and 0.55 Mev if it is known in addition that all beta particles are in coincidence with gamma radiation.
- (2) (c) C^{11} is listed in the Table of Isotopes as having "No γ " yet an absorption curve clearly shows electromagnetic radiation. Explain.
- (2) (d) Predict the possible mode or modes of decay for $_{40}Zr^{93}$ and $_{57}La^{138}$.
- (3) (e) In a beta ray spectrograph a particular gold isotope shows among other electron lines one of 400 kev energy. If a gold absorber of thickness 300 mg/cm^2 is placed before a more intense source, an electron line of the same energy is seen. Explain.
- (3) (f) What weight of $MsTh_1$ (Ra^{228}) would be found in thorium ore containing 1000 kg of Th^{232} ?
- (2) (g) Of which radioactive series would Em^{221} be a member?
- (3) (h) Why do we think of deuterium and beryllium as target materials for photoneutron sources and not other elements such as carbon as an example?
- (3) (i) Explain why slow neutron cross sections can be much larger than the geometric cross section but in general proton induced reactions have smaller than geometric cross section.
- (2) (j) Why does one find $_{48}Cd^{117}$ in low yield among the fission products of slow neutrons on uranium and $_{48}Cd^{107}$ not at all?
- (3) (k) An isotope of the odd element Z is made by irradiating Z^A with fast neutrons but not with slow neutrons. What is its probable isotopic assignment?
- (2) (l) A radioactivity suspected of being an isotope of iron is precipitated quantitatively with ferric hydroxide. Why is this not very good evidence that that activity actually is an iron isotope?
- (4) (m) Write all reactions for making U^{232} from Th^{232} in a high flux pile.

Iz and our readers, Richard Glass and Gary Higgins, corrected the examinations.]

Wednesday, January 30, 1952

Al and I arrived in Washington after midnight and took a cab to the Mayflower Hotel, where we checked in and had a few hours sleep before having to get up in order to attend the 9:30 a.m. meeting of the IO-7 Panel at the Pentagon (Room 3E 1060).

I found time during the day to telephone Charles Coryell at MIT to straighten out a few things about the Gordon Conference.

Thursday, January 31, 1952

In Washington. Al Ghiorso and I again spent the day at the IO-7 Panel meeting at the Pentagon. I had hoped to visit the AEC, but the Panel meetings lasted so long that I was unable to do so. Al and I took a cab to the airport in time to catch American Flight 71 at 8:30 p.m. We were met in Chicago at 10:25 p.m. and driven to the Argonne Guest House.

[In Berkeley, the research group met as usual with the following in attendance: Asaro, Bertelli, Biller, Carniglia, Carr, Carter, Conway, Cunningham, Dauben, Dunlavey, Fischer, Fleming, Glass, Glenn, Gunn, Higgins, Hoff, Hollander, Hulet, Hyde, Jaffe, Jenkins, Levy, Michel, Nervik, Perlman, Rasmussen, Ruben, Slater, Street, Templeton, and Worthington.

First to speak was Carter, who spoke extensively on the crystal structure of yttrium trichloride. There was considerable discussion among the attendees.

Asaro reported that they have been looking at the alpha particles from Pu^{239} in an attempt to identify the transition to the ground state of the U^{235} nucleus. Asaro showed the curve he obtained with a very long run with a very steady current, in which it is seen that the transition at highest energy is in the highest abundance. He reported that this was assigned to the ground state transition because only 0.01% gamma/alpha was observed. Perlman added that, if the main group was 10 keV above the ground state, they could not be differentiated. Asaro observed that, if the group in highest abundance went to a metastable state of U^{235} with a very long half-life, the absence of the large abundance of gamma rays would be explained. He said that the difficulty lies in that experimentally with most even-odd and odd-even nuclides, the highest energy alpha group is not in the highest abundance.

Hyde reviewed the work he discussed last week on the preliminary evidence on $\text{Fr}^{211,210}$. He said they repeated the experiment except that a plate was also taken for $A = 209$, which again proved to be an approximately 25-minute activity, but this was actually found to be Em^{212} (K-capture daughter of Fr^{212}), which had found its way to this position. This had been ruled out previously because of the high resolution of the machine, but Glenn suggested that the Em^{212} may be multiply charged and be collected on harmonics smearing out over the region, which suggested that the work be pursued in a better vacuum system. Hyde described the equipment designed and built by Bill Glenn to ionize and collect emanation gas. They found they were able to collect about 8% of all the emanation in the tube although the emanation collects

on both sides of the plate. Glenn hopes to correct this by putting a magnet around the tube, keeping the glow discharge on one side only. Hyde said they hope to take the emanation fraction directly from a thorium bombardment and attempt to collect it. Glenn spoke briefly on the multiple ionization smear.

* * * * *

Friday, February 1, 1952

In Chicago. Al and I spent most of the day at Argonne talking about a variety of subjects with a number of people. With Joe Katz and Clark Hindman I went over the program for the Gordon Conference. One non-scientific matter I discussed with Winston Manning was his invitation to me to spend an extended period of time at Argonne consulting. I have talked about this with the staff at the Rad Lab, the Department of Chemistry, and with Helen; today I offered to come about the middle of May and stay for about two months. Manning will arrange for someone to find a suitable house for us.

Al and I left Chicago at 3:55 p.m. on United Flight 623. Although it was a stormy night, there was no difficulty flying and we arrived in San Francisco at about 10 p.m. Art Mitchell, the lab driver, picked us up and drove us home.

Saturday, February 2, 1952

After I greeted the children this morning, I looked over some mail that Iz had brought home from the lab for me.

I had received and I looked over the atomic density curve from Moise Haissinsky, who obviously does not agree with my actinide theory yet; he again set forth his arguments in his covering letter. Finally, he asked for a set of reprints from us on the transuranides.

I also read a January 23 letter from C. B. Marquand (ACS Committee Advisory to the Chemical Corps), who said that Lt. Col. Loyd E. Harris would like to come to the west coast to visit industrial concerns and laboratories with reference to the "search for new agents" program (chemical warfare). Marquand asked for suggestions.

It has been so wet lately that I have been exempt from yard work, but I did go out this afternoon and prune the trees and the grape vines.

Sunday, February 3, 1952.

I read and played with the kids today. Helen mentioned to me that life is a lot simpler since Pete began attending kindergarten in the morning. The bus comes at 8:05 a.m., and he is home by about 11:15 a.m. This allows for a leisurely lunch, and then Pete is able to take a nap along with Steve, David, and Lynne.

Monday, February 4, 1952

One of the first things I did this morning was to look over the final examinations and the final grades, which Iz assigned, for Chemistry 123. The highest total was received by Harold Jaffe (our graduate student) (91.8), then Stephen Carniglia (also our student) (85.8), followed by William Ruehle (a health chemist who audited the course): Ruben M. Aboody, E; Joseph H. Abraham, C; Allen H. Anderson, B; Richard J. Borg, B; Dan F. Bradley, A; Russell G. Brauer, D; Kenneth E. Broadhead, B; Edwin A. Butenhof, B; Stephen C. Carniglia, A; Robert J. Carr, A; Ken Tang Chow, C; Wayne J. Christensen, B; Warren E. Clifford, A; Frank P.

Conant, Jr., B; Dwight C. Conway, B; Melvin S. Coops, C; James M. Craven, B; James M. Cripps, B; George E. Danald, A; Don R. de Halas, B; Wallace J. Dodson, B; Henry Dolezal, C; David A. Dows, B; Raymond C. Gatti, C; Bruno Gentili, C; Roscoe H. Goeke, B; LeRoy G. Green, B; Alfred H. Higgs, B; William M. Home, A; Joseph I. Irani, C; Harold Jaffe, A; William W. Jones, B.; Dwight H. Kouns, C; Robert F. Kubins, C; Louis Kovich, C; Melvin Look, C; Robert H. McCrea, E; Michael G. McKee, C; Earl W. McLaughlin, B; Mark F. Mitchell, A; Richard J. Nagle, B; Helen E. Nelson, C; Joseph D. O'Connor, C; Thomas O. Passell, B; Howard L. Peterson, B; David A. Pichler, B; Richard F. Porter, B; Tawfik Raby, C; James O. Rice, C; William G. Ruehle, A; Daniel A. Seedman, E; George Shalimoff, B; John L. Shehi, B; Buren R. Shields, B; Sidney D. Skirvin, C; Lloyd R. Snyder, B; John H. Stern, B; Edward C. Sudan, C; Robert L. Tellefsen, C; Hans C. Vetter, B; Lawrence A. Williams, B; Alexander T. Wilson, A, and Albin J. Zielen, A.

I made a number of phone calls and then dictated several letters. I wrote to Ed Westrum and told him that the paper he sent with his letter of January 17 seems ok to me (I had Burris check it over) and that Doral will write him about the reprints of this and the other paper. Then, as a result of my conversations with Katz and Hindman in Chicago last week, I described and invited him to attend the Gordon Conference, scheduled for the week of June 23-27.

I also wrote to Charles Coryell to tell him about the latest developments connected with the Gordon Conference, including my talks with the people in Chicago and my letter to Westrum. While I was dictating the letter to Coryell, a call came in from Ed Tompkins (now at Hunter's Point), who asked that Nate Ballou be allowed to give a paper on his work on the analysis of the composite decay curve of the uranium fission products. I agreed and added this information to Coryell's letter. Finally, I asked Coryell to have his secretary send me time schedules of various ways to go to New Hampton from Boston.

I stopped in Burris Cunningham's lab to inquire about some information that Joe Katz requested. I then promptly mailed the data on to Katz.

Sigfred Peterson wrote on January 28 that he has become increasingly dissatisfied with his position at the University of Louisville and has been investigating possibilities of positions elsewhere. He asked that I fill out an evaluation form for Carbide and Carbon Chemicals Co., which I promptly did. Sig also congratulated me on my "international achievement." In my response today I told Sig that I shall be glad to have him use my name and that I am sorry things are turning out unfavorably in Louisville.

Ralph E. Dyar (Spokane, Washington) wrote on January 25 to offer his congratulations about the Nobel Prize and to promote his book News for an Empire, which he said contained a well-rounded story of Hanford and mentioned the part I played in that project. In my reply I said I shall try to find a copy of his book to read. I thanked Dyar for his congratulations.

A note also went to AEC Commissioner T. Keith Glennan to explain that

I was unable to get to the AEC Building last Thursday, but I hope we shall have a chance to discuss the subject of his commencement address in the not too distant future.

The senior staff had its regular Monday noon meeting in my office. In addition to talking about some of the things Al and I learned on our recent trip, I showed the fellows the recent correspondence between Jim McCormack and Luis Alvarez. (McCormack inadvertently wrote to congratulate Alvarez on receiving the Nobel Prize.)

Dear Jim:

Thank you very much for your recent note of congratulation. Since I wasn't awarded the Nobel Prize of the Swedish Academy, I am extremely happy to be the 1952 recipient of the Nobel Prize of the James McCormack Foundation. I trust that the honorarium is of the same generous proportions, and I am eagerly looking forward to receiving the check. Probably through some oversight on the part of the secretary of your Foundation, the check was not enclosed with your note.

I am closing in on Glenn Seaborg's golf score these days, but for the moment he leads me in that department, as he does in the business of Nobel Prizes (Swedish).

With many thanks again for your kind thoughts,

Sincerely,

Luis W. Alvarez

cc: E. McMillan
Dept. of Utter Confusion



Today the following arrived from McCormack:

MEMORANDUM FOR Dr. Luis W. Alvarez

Dr. Edwin M. McMillan

Prof. Glenn T. Seaborg

This explanation is going to seem as incredible as the event itself. However, feeling I have no better choice than to make a clean breast of the business, here is generally what happened.

a. One day, having a few spare moments, I decided to correct a long standing dereliction and write a note to Seaborg and McMillan saying that it was a good thing about the Nobel Prize, making a "witty?" remark about McMillan's status as a chemist.

b. I thought also of writing Alvarez about the current condition of Pacific Coast football, with humorous remarks, of course.

c. Following a trend of thought and action much like that of a Mr. Riegles (I believe his name was), I proceeded to mesh everything together, with results that are now familiar to you.

Obviously, it is now "back to the drawing board" for me. I have evolved the following new plan of action:

a. Forget the whole thing.

b. Never go close to Berkeley again.

Your erstwhile friend.

/s/ Jim

James McCormack, Jr.
Brig General, USAF
Assistant, DCS/D

Tuesday, February 5, 1952

I again took care of some phone calls and some correspondence. I wrote to George Watt and told him that Al and I made our connection in Chicago all right. I wrote that I invited Ken Pitzer to attend his symposium in Buffalo and that I learned that George Jura's European trip fell through and he, therefore, will go to the Buffalo meeting. In addition, I wrote that we are interested in Gregory Choppin or Colby D. Hall, Jr., for a postdoctoral position with us, noting that we realize they will be easy for him to place and that there are advantages to us, as well as to them, in their coming with us for a time.

Last November T. S. Chapman (Technical Staff, Dow Chemical Company, Rocky Flats Plant, Denver) wrote to ask about chemists suitable for work with them. At that time I told Chapman that I was routing his letter to Professor T. D. Stewart, who knew the bachelor men. Today I again wrote Chapman and said that ordinarily I have little contact with bachelor of science men but recently a person came to mind. I then went on to describe Sidney D. Skirvin, explaining that Skirvin's background has proved to be too poor for graduate work in Berkeley. Skirvin, I wrote, has had considerable acquaintance with the techniques of working with radioactive materials.

I responded to a January 28 letter from Wendell M. Stanley, in which he included a formal invitation to participate in the next Conference on Science, Philosophy, and Religion to be held in New York City from September 2 to 5, 1952. I wrote that I realize the value of such an undertaking and have admiration for his participation in it; however, I feel I simply would be unable to prepare anything of value in the time which I have available.

I also wrote to Erik Braunerhielm (Royal Ministry for Foreign Affairs, Stockholm) to thank him for his letter of December 29 and to tell him that we received a notice that our furniture left Göteborg by boat on January 18th. I also thanked Braunerhielm for all he did to make our stay in Stockholm so pleasant and profitable.

Walter Yust (Editor, Encyclopaedia Britannica) wrote on January 28 to ask me to prepare an article on "Transuranic Elements" (300 words) and one on "Promethium" (250 words) for the Britannica. The rate is two cents per word; the "Promethium" article is due April 1 and the "Transuranic Elements" article is due May 1. In my answer today I said that I should be glad to undertake the article on "Transuranium Elements," but I suggested he contact Charles Coryell at MIT for the article on "Promethium" for he is much better qualified than I to write such an article.

I supplied the answers to the questions in the January 26 letter from Bertil R. Gustring (The Bulletin of the American Society of Swedish Engineers) and added some more material for my article "The Transuranium Elements" that I sent him in November. I also explained to Gustring that I am unable to attend the February 9 annual dinner in New York.

Recently I have received a couple of invitations to speak at The University of Wisconsin, one from John Willard to speak before the Wisconsin Section of the ACS and one from Dr. Charles Heidelberger of The Medical School of The University of Wisconsin. Willard wrote on January 31 about conflicts with my proposed date, so today I wrote to say that I shall let him know when it seems possible for such a talk. On another matter Willard mentioned that they are looking for a good health physicist (Doral checked with Nels Garden, who knows of no one), 2/5/52-3 and so I told Willard that I will let him know if anyone worthwhile turns up. I also wrote to Heidelberger to explain why I am postponing acceptance of his January 9 invitation.

Bill Orr (William C.), now at the University of Connecticut, wrote on January 30 to say that he and Steve Friedland have become interested in some of the recent work being done on fission yields with the mass spectrometer. He asked if we knew of anyone studying such yields in fission introduced by neutrons of several Mev or more. I routed the letter to Iz, Stan, Al Ghiorso, and Herman Robinson (to whom Orr sent his greetings and asked for comments). In today's response I told Orr that no one knows of similar work being done although it is possible that such a program is underway somewhere. I wrote, "It strikes me as a worthwhile program, and it is certainly one in which we here would be interested in the results."

Wednesday, February 6, 1952

In between phone calls I answered some of my mail. One letter went to Tony (Anthony) Turkevich in Chicago. Tony wrote on January 18 about a position for one of his students, James B. Niday. I wrote that I have nothing definite to offer but mentioned the program in which the Radiation Laboratory is collaborating with the California Research and Development Company. [This is a program to build a high intensity proton accelerator, MTA, for the production of high neutron flux to produce Pu^{239} by bombarding U^{238} (in natural uranium); thus eliminating the need for U^{235} to run a chain reaction.] I suggested that, if Niday wants to explore the possibility, he should contact Dr. Kenneth Street, who is in charge of the radiochemical program. I also wrote that, although there is no money in the budget for the purpose, eventually we may want to add a permanent man to our heavy isotope program. I wrote that we would want to talk with Niday before deciding and said that I might do this by passing through Chicago either on my way to or back from the Buffalo meeting of the ACS near the end of next month.

On a recent weekend Dorr Etzler, my neighbor and old friend, asked if I knew of a radiochemist for their La Habra laboratory. (Dorr has a position with California Research Corporation in Richmond.) At the time I couldn't think of anyone, but today I wrote him and mentioned Edward H. Fleming, saying that Fleming will be getting his degree next June, is qualified, and shows some interest. I apologized to Dorr for the formal way of handling this query but said that I seem to forget it when I see him around home.

I also answered the January 17 letter from John Voelker (Ishpeming) about the biographical article he wants to do on me. Voelker seemed concerned about the coverage of my work, to which I suggested that the general way in which he referred to my work in his previous write-up was probably the most desirable, and about the "now" element of my work. To the latter my response was that the interest in atomic energy is so much a continuing thing that the time element may not be so important. I also wrote that I will be happy to cooperate on such an article at any time by furnishing material, checking an outline, and reading and making suggestions in the draft. (Dorr has already sent Voelker reprints of my publications.) I also suggested that he might get some help on the work by discussing it with a professor of physical chemistry or a professor of physics at either Northern Michigan State in Marquette or Michigan Tech in Houghton. Finally, I mentioned that I understand that we were on a radio program together in Ishpeming (my recording for the March of Dimes), or at least it was scheduled.

I made my usual rounds of the labs to talk about the research with the various people

Thursday, February 7, 1952

The following attended this "first of the month" open meeting of the research group: Asaro, Browne, Bertelli, Carniglia, Carr, Carter, Edward Shannon Clark, Jr. (a graduate student from Union College, Schenectady, who will do his research with Templeton), Conway, Cunningham, Dauben, Feay, Glass, Gunn, Higgins, Hoff, Hollander, Hulet, Jaffe, Jenkins,

Kalkstein, Koch, Kofstad, Larsh, Levy, Hirdaya Behari Mathur (our new graduate student from India), Michel, Nervik, Thomas Oliver Passell (a graduate student from Oklahoma A & M, who will do his research with me), Perlman, Ruben, Seaborg, Skirvin, Slater, Street, Templeton, Terwilliger, Thompson, and Tyrén.



Edward Clark



Hirdaya Mathur



Thomas Passell

Per Kofstad reported that he has determined a production cross section of 1.5×10^{-3} mb for the production of 20-minute C^{11} from silver plus full-energy (340 Mev) protons. He noted that the cross section could be off by a factor of two. Perlman suggested that Kofstad look at the manuscript by Marquez to see if his value is consistent with Marquez' curves. Kofstad went on to say that he found a production cross section of 32 mb for Cd^{107} . He saw an activity with a half-life of 65 minutes, which decayed with a 2.6 Mev positron--the daughter had a half-life of ~40 days. He noted that Ag^{105} has a 40-day half-life, so the 65-minute activity was assigned to Cd^{105} although its value in the tables (questionable) is given as 38 minutes. There was a discussion about whether he has a mixture and whether the amount of 40-day silver is correct.

Tyrén talked for the rest of the meeting about his work. This included the study of excitation functions of the type $A(p, xnypz\alpha)B$ for elements having more than one isotope present. He bombarded niobium with high energy protons and studied first the elements near niobium: zirconium, yttrium, strontium, gallium, etc. He said that it was rather hard to resolve the decay curves, and he is not sure that he obtained independent yields. However, he presented a curve showing the relative

yields of Cu^{64} ; Cu^{61} ; Zn^{69} ; Ni^{65} ; and Cu^{67} , adding that near the threshold (about 200 Mev) there may be some contribution due to fission. There was some discussion about the threshold energy, which Tyrén said he calculated by using the binding energy and the coulomb energy from the Fermi book. He also said that he is carrying out experiments to determine the yield of Na^{24} to see if the contribution from fission is high or low. Tyrén also discussed some work with the linear accelerator (32 Mev protons) and described the techniques (brush recorder and two counters in coincidence) he used to measure short half-lives. He bombarded nickel with the 32 Mev protons, saw an ~3-second half-life (probably Cu^{58}). This half-life was determined as 2.6 seconds and 2.8 seconds by two methods. In this case he also did an excitation curve at different energies and made some energy measurements. The work evoked much discussion.

* * * * *

After stopping in the Chemistry Department office to talk with a number of people, I went up the hill and took care of some of my correspondence.

Tuesday I talked with Bill Dauben and asked him if he could supply me with a list of chemical companies and academic institutions who might be able to supply Colonel Loyd Harris (letter from Marquand of January 23) with chemicals for the "search for new agents" program. Today a very comprehensive list arrived from Bill, and I promptly wrote to Marquand to send him the names. I also wrote that, if Harris feels that these companies should be contacted before Colonel Harris gets in touch with them, Dauben will be glad to help. I explained that my schedule is so heavy at present that I could not do it personally.

I looked over the paper on Ni^{56} by Ray Sheline, which Ray Stoughton sent me. Stoughton said they are submitting this to Sam Goudsmit (Editor, The Physical Review) in order to get the refereeing started, pointing out that he understood from his conversation with me a few weeks ago that our publication on Ni^{56} (with Worthington) would be delayed longer. [Worthington completed his research for his master's degree and, as a member of the Air Force, has gone to McClellan Field in Sacramento. Since we have no one to continue this research, we decided to submit it as a "Letter to the Editor" immediately; I mailed Stoughton a copy.] Stoughton mentioned that Worthington's results are in substantial agreement with Sheline's, adding that he thinks Sheline's half-life figure is quite good. He also noted that Sheline has returned to Florida State University to begin his teaching career; he will continue his work on some isotopes that he started in Oak Ridge, including V^{53} , Cr^{55} , and Mn^{57} .

As a member of the ACS Council on Publication I recently received a critique of the Journal of Organic Chemistry from A. L. Marshall. This I sent down to Jim Cason to ask that he determine the reaction of the organic chemists in the Department of Chemistry. Yesterday a well-thought-out letter arrived from Cason, the joint effort of Bill Dauben, Henry Rapoport, and Donald Noyce. They began, "At the outset, let us say that our interest stems from the fact that we regard the Journal of Organic Chemistry (JOC) as very important, indeed, to the

healthy functioning of organic chemistry in this country. Without it, the Journal of the American Chemical Society would hold a monopoly on publication in this country in the field of organic chemistry. This is a particularly insidious type of monopoly, for it actually means that one or two men may virtually dictate what may and may not be published, and how it must be written...." In my response to Marshall today, I pointed out that our organic chemists seem to feel that the survival of the Journal of Organic Chemistry is vital to the future of organic chemistry and noted that I have asked one of our chemists to summarize their views, which I will send to him if it is forthcoming. I also suggested to Marshall that we invite Professor James Cason, Jr. of the Chemistry Department here to the next meeting of our Council Committee on Publications so that he may express his views, in the event that the Journal of Organic Chemistry is scheduled for discussion at that time.

I answered a letter from Tom (L. O.) Morgan that arrived today. Tom said he hopes to attend our Gordon Conference on Nuclear Chemistry and asked if he needs authorization from me in order to attend. In my reply I said he should let me know right away if he doesn't get an acceptance so that I can take care of it. I also told Tom that we enjoyed seeing Martha [Boyd, Betty Morgan's sister] on her recent visit and thanked him for his congratulations in connection with the Nobel award, saying that this should be regarded as honoring his own contribution to the area which was being recognized by the award.

I made a tour through some of the labs to watch the research.

Friday, February 8, 1952

Since I will be gone next week on the tour with President Sproul, I made an effort to clear my desk of pending correspondence.

In response to the request of January 19 from Dr. Arne Holmberg (Kung. Vetenskapsakademiens Bibliotek, Stockholm), I mailed him my photograph, a copy of the speech that I delivered at the Nobel banquet, and a short biography. I explained that I have not yet written my Nobel lecture, but I will send it to him as soon as this has been done. I also thanked Holmberg for the fine treatment Mrs. Seaborg and I received during our stay in Stockholm and said that we have very fond memories of the whole occasion and look forward to seeing again the many new friends whom we found.

Yesterday I received a package of ore samples from a relative, Lawrence O. Risem (Vista, California), who asked that I have them analyzed. I wrote to Risem today and said that I am returning the package since there are no facilities here for analytical work. I pointed out that we receive a number of samples for analysis so it is apparently not well-known that the state university has not set up facilities for such purposes, adding that I am not sure where he can get the material analyzed but presumably some commercial outfit is the best bet.

Doral sent some publicity material to Hubert J. Pedersen (Huntington Park Signal, Huntington Park), whose February 6 request for information about my youth in South Gate, was transmitted to me yesterday by Verne

Stadtman of California Monthly. This is apparently in connection with Alumni Day in Los Angeles on February 17. Doral suggested that Pedersen contact my parents in South Gate for any further information.

On January 25 Nils G. Sahlin (Minneapolis) invited me to be the principal speaker at Svenskarnas Dag in Minnesota. In my response to Sahlin today I wrote that I am quite interested in the possibility of attending and participating, but I should like to learn more about it before deciding. I told Sahlin that I have a commitment to attend a scientific meeting in New Hampshire beginning on Monday, June 23rd, which might compete with his date of Sunday, June 22nd. However, I wrote that, if I can catch Northwest Airlines Flight 212 leaving Minneapolis at 5:05 p.m. for New York, the two engagements might be consistent. I then asked about the subject of my talk, noting that I do not feel that I would care to talk on a political or a general sociological topic but would feel qualified to talk on something like atomic energy. I also pointed out that I doubt I will have time to prepare a manuscript for him.

When I was in Chicago recently, I talked with Dieter Gruen about measurements of the magnetic susceptibilities of some neptunium compounds and the results of which were interpreted on the basis of 5f electrons using essentially the crystalline electric field model proposed by Penny and Schlapp. As I requested, Gruen sent me a reference to a summary of the work (pages 17 to 31 in the quarterly report of Section C-I of the Chemistry Division of Argonne Laboratory for July, August, and September 1951 - ANL-4740). I wrote Gruen, thanked him for his letter, and said that I will read the section in ANL-4740 when the report comes in to our library.

I also wrote to Mel (M. S.) Freedman at Argonne to tell him that we are bringing our table of masses up to date and are interested in learning his latest beta disintegration energies for those instances where they haven't been published as yet, noting that the latest summary report we have is ANL-4613. I added that we are particularly interested in Ra^{225} but are also interested in any other of his latest values.

In connection with my role as Chairman of the Division of Physical and Inorganic Chemistry of the ACS, I also wrote a couple of letters. One went to Dr. Alan T. Waterman (Director, National Science Foundation), inviting him to be the after dinner speaker at the dinner meeting of the Division on the occasion of the spring 1952 meeting of the American Chemical Society in Buffalo. I wrote that the dinner will be on Wednesday evening, March 26, and said we have in mind a talk of some thirty minutes in length on some phase concerning the National Science Foundation, possibly emphasizing its present status and future plans. I explained that we do not have funds to pay the expenses of our speakers and usually invite one of the chemists to speak, but there is a strong feeling that we should have some talks of a broader nature.

I also wrote to Joe Kennedy about a number of items concerning the Division, such as the matter of the Division acting as a co-sponsor with the Division of Polymer Chemistry for a symposium on "Inorganic Polymers and their Application" next fall at Atlantic City. I agreed that a breakfast in Buffalo on Monday, March 24, for all the chairmen of the symposia is a good idea. I also suggested that we should settle the time

of our Executive Committee meeting in Buffalo, possibly following previous custom by assembling at the registration desk of the headquarters hotel at about 6 p.m. on Monday, March 24. I mentioned that one item on the agenda should be future symposia.

Joe Kaplan (UCLA) recently sent me a list of publications of Samuel Barnett, along with a copy of a letter that Barnett wrote to Kaplan. I sent this material down to Bill Giauque. (Giauque and I recently nominated Barnett for the 1952 Nobel Prize in Chemistry.)

I also acknowledged a January 31 letter of congratulations from Lt. Colonel A. W. Brewer (Divisional Commander, The Salvation Army, Oakland), who read of my participation in the Lucia Day Festival in Sweden in the February 2 issue of The War Cry. Brewer sent me a copy of the magazine, for which I thanked him.

Then in today's mail I received a copy of the report by Nate Ballou from Ed Tompkins (Hunter's Point). Tompkins recently called me about the work, saying that he would like to see the work reported at the Gordon Conference. Ed mentioned that Paul Tompkins would also like to see the work reported. I told Doris to have someone copy the report and send it to Coryell.

I received and read a letter from James R. Bercaw, describing the arrangements for my Phi Lambda Upsilon talks at Ohio State on March 28 and 29. He mentioned that the Third Annual Lecture Series for 1952 will hereafter be known as the Seaborg Lecture Series and said the lectures are scheduled for 8 p.m. on Friday and 10 a.m. on Saturday. I immediately acknowledged the letter, saying the times seem satisfactory and that I would enjoy a small informal dinner on Friday. In answer to his query about the possibility of talks elsewhere, I suggested that we make no effort in that direction but see what develops naturally.

George Everson sent me a copy of his letter to Harold A. Fidler (Office of the AEC Area Manager), accompanying the Personnel Security Questionnaire, the fingerprints, and other documents of Bernard George Harvey. Everson pointed out to Fidler that we desire that Harvey be cleared for employment at the Radiation Laboratory to begin work here by midsummer, 1952.

The rest of my day was spent talking with some of the students.

Saturday, February 9, 1952

Since I am leaving early tomorrow morning for the speaking tour with President Robert Gordon Sproul, I spent much of the day with the children and Helen. I also did some reading and writing.

Sunday, February 10, 1952

Cliff Dochterman (Field Secretary, California Alumni Association) picked up Ed McMillan and me at our homes this morning and drove us (via the Bayshore Freeway - Route 101 Bypass) to the Benjamin Franklin Hotel in San Mateo. We arrived about 9:30 a.m. Others in the 1952 President's Tour include President and Mrs. Robert G. Sproul, driven by Stanley E.

McCaffrey (Executive Director, California Alumni Association) and his wife Beth and Austin H. MacCormick (Professor of Criminology), driven by Varsity Yell Leader Eric Carlyle.

The Alumni Breakfast was held at 10 a.m. in the main dining room of the hotel. The program consisted of Songs and a Yell by Eric Carlyle, introductions by Chairman Robert Tuck (Class of 1935 and Manager of Atlas Heating Company, San Francisco), remarks on the Alumni Association by Stan McCaffrey, about 10 minutes of remarks by me on "What's Next in Atomic Energy," a brief talk "Truth about Crime" by Austin MacCormick, a brief talk by Edwin McMillan on "The Nobel Prize," and then President Sproul spoke for about 20 minutes on "A Report from the University." The program concluded with the singing of "All Hail." I met a number of active alumni, including Dan Koshland (class of 1913, an executive in Levi-Strauss and father of my friend Daniel Koshland, Jr.). I also met Assemblyman and Mrs. Richard Dolwig of South San Francisco.

The group left San Mateo about 12:30 p.m. and drove via Route 101 Bypass, Route 156, and Route 1 to Carmel. There, at about 3 p.m., we checked into the La Playa Hotel. There was no scheduled luncheon, but we met at 5:45 p.m. in the lobby to drive to the Monterey County Fairgrounds for an Alumni dinner, which was held in the Exhibit Room. Dr. Harry Brownell (a dentist who is active in the local "Grid Club") was chairman for the 6:30 p.m. dinner, and the program was roughly similar to the breakfast program. State Senator and Mrs. H. R. Judah (Santa Cruz) and Assemblyman and Mrs. Donald L. Grunsky (Watsonville) attended the dinner. We returned to the hotel at about 10:30 p.m.

Monday, February 11, 1952

In Carmel. After an informal breakfast at the La Playa Hotel in Carmel, the group drove to Salinas and stopped for a "coffee stop" with James (Jim) Young, President of the Salinas Alumni Club. At 10:30 a.m. we continued our drive south on Highway 101 to King City and the El Camino Real Hotel. After washing up, we met in the Spanish Dining Room for a luncheon. (Lew Tully, a social science teacher at the King City High School was chairman.) We were asked to make our remarks brief at this program because of time limitations. State Senator Fred Weybret of Salinas attended the luncheon as did William Dochterman, Cliff's older brother.

At 2:45 p.m. the group left King City, proceeded south on Highway 101 to Paso Robles and the Paso Robles Inn. We met in the lobby at 5:45 p.m. to go to a pre-dinner reception at the home of Paul Snyder (Paul and his wife Jean are organizers of the new alumni group; Paul is a traveling representative for Livingston Construction & Building Equipment and Supplies). This went on for about 45 minutes, and then the group drove to the U.S.O. Building for a 7 p.m. dinner, chaired by E. Conant Livingston. (Livingston is owner of the Livingston Construction Co., a classmate of President Sproul and father of Mrs. Paul Snyder.) The program was essentially the same as yesterday evening's dinner. Again, we met active alumni and also State Senator and Mrs. A. A. Erhart of Pismo Beach. We returned to the Paso Robles Inn about 10:45 p.m.

Tuesday, February 12, 1952

In Paso Robles. At 9:45 a.m., after breakfast at the Paso Robles Inn, we left for a drive to Hanford via Route 41, which eventually joins Route 198. In Hanford we had lunch at Paden's Cafe with Lee Rice and several local alumni.

We left Hanford at 1:30 p.m., proceeding north on North Irwin Street and then following the directional signs to Fresno, going through Laton and Fowler, where we turned onto Highway 99 and approached Fresno from the south. We arrived in Fresno about 2:15 p.m. and went to the Hotel Californian.

At 6 p.m. there was an informal reception with some of the Fresno alumni leaders in Beth and Stan McCaffrey's room. The Valley Bear Backers and the Alumni Scholarship Committee are quite active in Fresno. This was followed by dinner at 7 p.m. in the main ballroom on the mezzanine floor. Jim Waller (class of 1942 and president of the Valley Bear Backers) handled the arrangements for the dinner. Again, we met many active alumni. State Assemblymen Wallace Henderson (and his wife) and William Hansen, both from Fresno, were present along with UC Regent Earl Fenston. John Paul, Secretary of Valley Bear Backers, handled reservations for the dinner. The program followed the format of our other dinners. My remarks seem to be always well received. I usually cover the peacetime uses of atomic energy, describing the energy (equivalent to the burning of 2000 tons of coal) in my 1-inch cube of metallic uranium (which I show to the group) and then going on to talk about the use of tracers in industry, agriculture, and medicine. Professor MacCormick has an extraordinary engaging and humorous delivery for his description of his experiences in prison work.

Wednesday, February 13, 1952

In Fresno. After breakfast our group drove south on Highway 99 to Tulare, turned in the center of town to Porterville, where we stopped at the Porterville Motel to wash up prior to the 12 noon buffet luncheon and reception at the Legion Hall. There was no organized program, but the local alumni chairman, Boyd Eckard (class of 1940), called on President Sproul for a few remarks and introduced MacCormick, McMillan, and me.

The group left Porterville at about 2:30 p.m. to drive to Bakersfield and the Bakersfield Inn. We arrived about 4 p.m. and at about 6 p.m. there was a reception prior to the 7 p.m. dinner in the Palm Room of the Inn. This was followed by our usual program. Arrangements for the dinner were handled by Joe Shea (class of 1926, president of the local alumni club and an engineer for the Richfield Oil Company). Also in attendance at tonight's dinner were Dr. Robert Sheldon, head of the Stanford Alumni Club, and State Assemblyman H. W. Kelly (and his wife) from Shafter.

Thursday, February 14, 1952

In Bakersfield. Today our group travelled from Bakersfield, via Castaic Junction (coffee stop) and Ventura (lunch at the Pierpont Inn) to Santa Barbara. We drove past the UC campus to the El Encanto Hotel.

Since it was only about 4 p.m., some of us drove to the new Goleta campus that is about eight miles north of Santa Barbara to look around before the reception at the El Paseo Restaurant. [We are staying in the El Encanto Hotel.] Dinner began at 7 p.m. with the same program as we have had at our other dinners. Toastmaster for the evening was Russell Bell (class of 1917, member of the "Big C" on campus, President of the Federal Building and Loan, and Past President of the Rotary Club). The chairman for the dinner was Dr. Jack Rathbone (a dentist and a member of the class of 1940; his father, Albert J. Rathbone, was a member of the class of 1913). State Senator James J. McBride of Ventura was present. [At the dinner everyone helped Eric Carlyle, the varsity yell leader, celebrate his 21st birthday with a cake with lighted candles.]

[In Berkeley, the research group met as usual with the following in attendance: Asaro, Clark, Conway, Dauben, Dunlavey, Feay, Fischer, Hoff, Hollander, Hulet, Jaffe, Jenkins, Kalkstein, Koch, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Skirvin, Slater, Street, Templeton, Terwilliger, and Thompson.

First to speak was Passell, who described running the beta spectrum of Pa^{232} . He said that four of the five conversion electrons he saw were not too prominent but appear to indicate five gamma rays of energies (in Mev) of 0.142, 0.157, 0.206 (highest abundance), 0.216, and 0.387. He said that it seems unlikely that any two peaks arise from the K and L conversion of the same gamma ray (from energy summation considerations). Passell said that previous work (March 1951) gave beta particle energies of 0.991 Mev, 0.639 Mev, and 0.265 Mev. The recent work gave data sufficient only to confirm the two lower energies, giving values of 0.655 Mev and 0.276 Mev. He presented a tentative decay scheme.

Dunlavey said that he investigated the fine structure of U^{236} using the recording of conversion electron-alpha coincidences in nuclear emulsions as the criterion. He said that this was a 98% pure sample from Fleming (U^{235} was the remainder). He said that out of the 4350 alpha particles counted, 68% had no conversion electron, 27% had a conversion electron of 25 ± 5 kev, and 5% had a conversion electron of 40 ± 5 kev. This is consistent with the isotope having one excited level of about 45 ± 5 kev with the gamma ray being converted in the L and M shells.

Rasmussen, who recently visited Oak Ridge where he talked with Rose (Morris E.), reported on a correction to the theory previously proposed as a possible explanation of the anomolous x-ray shift that Browne observed in the decay of Pa^{232} .

Terwilliger described her Dowex 50 ion exchange separation of Hg^{206} from lead and bismuth.

Finally, Slater talked about his method of electroplating U^{232} , using 0.33 M $(\text{NH}_4)_2\text{SiF}_6$ at pH 3.57. He reported that this seems to work better than the oxalate method and the plates may be good for the spectrograph.

* * * * *

Friday, February 15, 1952

In Santa Barbara. After breakfast at the El Encanto Hotel, our group left Santa Barbara and drove south (Highway 101) through Oxnard, Santa Monica, the Beach Cities, and Long Beach. We had lunch with local alumni, including Robert Peacock, in Laguna Beach at the Victor Hugo Inn. We then drove on to San Diego, arriving about 3:30 p.m., where we made our headquarters at the El Cortez Hotel at 7th and Ash Streets. At 4:45 p.m. we assembled in the lobby to go to a reception at the home of Dick (Richard) McCune (a member of the class of 1927, member of the Alumni Council and owner of McCune Motors; McCune was associated with the Bank of America in Los Angeles for a number of years). At 6:30 p.m. we had dinner at the Cuyamaca Club, where Anderson Bostwick (class of 1924) was chairman and toastmaster. Bostwick is President of the First National Bank of San Diego. Other leading alumni who were present are Roger R. Revelle (class of 1936), who is Director of Scripps Institution on the La Jolla campus, and James Archer (class of 1930), former member of the Alumni Council and an attorney in La Jolla. Assemblyman Frank Luckel and Assemblywoman Kathryn Niehouse attended the dinner. Again, the program was similar to our previous programs.

Saturday, February 16, 1952

In San Diego. We checked out of the El Cortez Hotel at 8:20 a.m., assembled in the lobby in order to go to General Leo "Dutch" Hermle's home in El Cajon for breakfast. At about 10:30 a.m. General Hermle showed us a short cut to La Jolla and the Scripps Institution of Oceanography, where Roger Revelle gave us a short guided tour. We had lunch in La Jolla with Revelle, left La Jolla at 1:45 p.m. to drive on Highway 101, via Santa Ana, into Los Angeles. Instead of going to the Ambassador Hotel with the group, I went to my parents' home in South Gate. My parents were extremely interested in some letters and pictures Helen and I have recently received from Sweden. I also suggested to my parents that they visit us about Easter--not only are the children growing and developing rapidly, but there is a possibility that we may be in Chicago later in the spring and summer.

Sunday, February 17, 1952

In Los Angeles. Jeanette picked me up this morning and drove me to the Ambassador Hotel in time for the 30th Alumni Day (registration began at 10 a.m.). The schedule was as follows: 11:00 a.m. Seminar Sessions (attend one) with Austin MacCormick and Gordon Watkins; 12:15 p.m. Luncheon in Embassy Room with President Sproul as the speaker; 2:30 p.m. Seminar Sessions (attend one) with Raymond H. Fisher and with Glenn Seaborg and Edwin M. McMillan; 3:30 p.m. Seminar Sessions with Judson Landis and Stanley Freeborn. Jeanette and I left shortly after my talk, skipping the 4:30 p.m. reception in the Gold Room for President and Mrs. Sproul, to go back to South Gate. There a reporter from the South Gate Daily Press Tribune, Jack Boettner, interviewed my parents and me about my youth for a sort of "home town boy makes good" type article. A Press Tribune photographer took our picture. After the reporter and photographer left, Jeanette drove me to Whittier to show me her and Eino's new house. Later, after a nice dinner prepared by my mother, Jeanette took me to the airport for United Flight 457 back to the Bay

Area. I arrived in San Francisco at 10:25 p.m.

SEABORGS DUST OFF FAMILY ALBUM



DR. GLENN SEABORG (center), co-winner of the Nobel Prize for Chemistry in 1951, glances over family album with his parents Mr. and Mrs. H. Seaborg of 9237 San Antonio avenue, South Gate. Dr. Seaborg, a chemistry professor at the U. of California, visited here Saturday night.

—Press-Tribune photo engraving

Monday, February 18, 1952

Although I arrived home rather late, the kids made sure that I got up at the proper time. They were anxious to greet me and tell me of their activities.

Eventually I drove to campus, where I stopped in the Chemistry Department office for the news. This is the first day of the new semester.

When I got up to the hill, I looked over the mail that had arrived during my absence. R. K. Holbrook of North American Aviation requested by telegram on February 11 an evaluation of Sigfred Peterson. Doral had informed him that I was out of town. Today I wired, "SIGFRED PETERSON IS A MAN OF VERY GOOD TECHNICAL ABILITY, HAS FINE RESEARCH OUTPUT AND INITIATIVE AND PLEASANT PERSONALITY. I CAN RECOMMEND HIM HIGHLY."

T. S. Chapman (The Dow Chemical Company, Rocky Flats Plant, Denver) wrote on February 12 to thank me for my letter about Skirvin and to suggest I have Skirvin contact him since he (Chapman) will be in Berkeley on February 25 and 26 on a recruiting trip. Chapman thanked me for

referring his recruitment letter to Professor T. D. Stewart and said that he not only plans to interview applicants, but he also wants to talk with Nels Garden about their health safety program. Earl had already read the letter and had contacted Skirvin about an interview with Chapman.

I read a February 7 letter from Jim (J. O.) Maloney, whom I knew during the Met Lab days in Chicago. After congratulating me about receiving the Nobel Prize, he asked if our university is offering any work along the line of integrated course work in nuclear science and engineering. (Maloney is in the Department of Chemical Engineering at the University of Kansas.) I routed the letter to Earl, asking him to draft a reply.

Other letters that I looked over included one dated February 8 from Charles Coryell, updating the arrangements for the Gordon Conference. One minor problem he mentioned was about the participation of George Boyd. Coryell said that he had been unable to reach Boyd by phone, but he plans to suggest that, if Boyd does not want to talk in the field of tracers, he talk on technetium chemistry and technetium in nature. Coryell mentioned a couple of other minor matters about the program and said that he will have to give the program to Parks soon.

Franklin J. Meine (The American Peoples Encyclopedia) requested, in a February 13 letter, a glossy photograph of me to be used, along with photographs of other Nobel Prize winners, in advertising. I told Doral to write the covering letter and to send such a photograph.

I read a February 14 note from George E. Boyd (Oak Ridge) commenting that John Rasmussen made a very favorable impression on his recent visit. He wrote, "You are certainly giving your people an excellent training both in theory and practice of nuclear chemistry, so much, that even our hypercritical Dr. M. E. Rose spoke to me in a commendatory manner about his conversation with John." Boyd also sent his belated but sincere congratulations on the Nobel Prize. I routed this letter to Stan, Iz, Al, and John Rasmussen.

I looked over and signed a listing Doral had prepared for Miss Kittredge in the Chemistry Department office of the classified theses that she (Miss Kittredge) should be holding since they are not yet declassifiable. The list included the theses of A. C. Wahl (May 1942) "Induced Radioactivities in the Heaviest Elements"; S. G. English (February 1943) "The Measurement of Slow Neutron Absorption Cross Sections of Some Heavy Isotopes"; J. W. Gofman (February 1943) "The Properties of Certain Isotopes Produced by Deuteron and Neutron Bombardment"; W. W. Meinke (January 1950) "High Energy Bombardment Products of Thorium"; B. Rubin (June 1950) "Performance of Liquid-Liquid Extraction Equipment"; M. Davis (February 1951) "Liquid-Liquid Extraction"; R. M. Diamond "An Ion Exchange Study of Hybridized 5f Bonding in the Actinides"; E. M. Douthett (June 1951) "The Ranges of Fragments from High Energy Fission of Uranium"; R. L. Folger "High Energy Proton Fission-Spallation of Uranium"; and J. C. Wallmann (June 1951) "The Specific Activity and Half-Life of Various Isotopes of Plutonium."

Mark (C. M.) Olson (E. I. du Pont de Nemours & Co., Wilmington), whom I have known since the Met Lab years, wrote on February 6 to congratulate

me on the Nobel Prize and to thank me for the invitation to attend the Gordon Conference. He pointed out that, although he is not yet in the Atomic Energy Division, he may find it worthwhile to attend. He also briefly mentioned the work his group is doing.

I read a February 7 letter from Lee DuBridge (President, Cal Tech), asking if I would serve as one of the judges at a Southern California Science Fair on April 19 and 20 at the Los Angeles County Museum. DuBridge described the event, for high school students, which the sponsors hope will become an annual event.

Tom Rice and Mark McDermott (Walla Walla, Washington) wrote on February 9 to suggest that element 100 be named eurekium. They said, "What better means than to embody in its name a suggestion of the triumphant nature of this discovery?" I routed the amusing note to Stan, Al, Iz, and Earl.

I also looked at a February 11 note from Wendell Stanley, who said that he understood the position I stated in my letter of February 5 regarding the conference on science, philosophy, and religion in New York in September. [I declined his invitation.]

In a letter of February 12, John Voelker thanked me for the material I sent and my letter of February 6. He said that he thinks he will be able to do an interesting article for a lay audience now, adding that he has just finished his book.

On January 29 George W. Muhleman (St. Paul) sent me a copy of "A Tabulated History of the 98 Chemical Elements," for which I had supplied him with some information. Muhleman pointed out that the chart still lacks data and then asked if Ida Tacke were the Jewish chemist who first brought the Allies the information that the Germans were also on the trail of the A-bomb. I thanked Muhleman for his chart and said that I believe it is popularly assumed that Lise Meitner brought the Allies the information although I do not know whether this is actually true.

As usual, after such a trip, my day was interrupted with numerous phone calls. The senior staff also had its regular brown-bag lunch meeting in my office at noon at which time I described my recent trip with President Sproul.

Tuesday, February 19, 1952

Before going to the campus, I dictated a couple of letters. One went to Alan T. Waterman (National Science Foundation) who accepted my invitation to speak at the dinner meeting of the Division of Physical and Inorganic Chemistry at the ACS Buffalo meeting. I said the date will probably be Wednesday, March 26 and the place will probably be one of the large hotels in Buffalo.

I also wrote to Joe Kennedy, saying that I liked Frank Long's suggestion that we have Alan T. Waterman as a speaker for our dinner of the Physical and Inorganic Chemistry Division in Buffalo. Waterman replied affirmatively to my invitation, I wrote, and will speak on "Present Status and Future Plans of the National Science Foundation." I

told Joe that I will write and give Waterman the place and time of the dinner as soon as we know, assuming that the date will be Wednesday, March 26.

I declined a February 11 invitation from Dean H. P. Hansen to address an evening session of the AAAS, Pacific Division, at its annual meeting at Oregon State (Corvallis) during the period of June 16-21. I explained that I already have a previous commitment in the East for that week.

A note also went to Ed Tompkins (Hunter's Point) to thank him for his letter of February 6 with the enclosure of the report by Hunter and Ballou. I said that we will reserve about 15 minutes at the Gordon Conference for Ballou's report and suggested that he send a copy of the report to Coryell. I also mentioned that we have our copy of Paul Tompkins' letter to Professor Parks and are looking forward to seeing Werner and Ballou at the Gordon Conference.

On campus I gave the introductory lecture to the Chemistry 223 class at 11 a.m. Members of the class include Myron G. Andrews (audit), Stephen C. Carniglia, Robert J. Carr, Glen M. Iddings (audit), Harold Jaffe, Emily B. King (audit), Hirdaya B. Mathur (audit), Walter E. Nervik, Thomas O. Passell, Tawfik M. Raby (audit), Roland Rivest (audit), Monte H. Rowell, William G. Ruehle (audit), Rex H. Shudde, Sidney D. Skirvin, Helge E. Tyrén (audit), and Alexander T. Wilson.

Before returning to the hill, I had lunch and heard Arthur H. Compton speak.

Today's mail brought me a February 12 letter from Antonio Nasini (Istituto Chimico della Universita di Torino) who thanked me for my letter of January 25 and agreed that Dr. Mario A. Rollier is quite well suited to join the Advisory Committee of the Joint Commission on Radioactivity of the ICSU. He suggested that, if I need an official designation from the Italian body member of the IUPAC, I address their National Research Council in Rome.

Wednesday, February 20, 1952

In between phone calls and other interruptions this morning, I dictated a couple of letters.

I wrote to Mark Olson (du Pont) and A. G. Maddock (Cambridge University), acknowledging their letters expressing interest in attending the Gordon Conference. To both men I expressed my appreciation for their congratulations about the Nobel Prize. I then sent a copy of Maddock's and a copy of Olson's letter to Coryell, along with a copy of my response. I also told Coryell that I received the copy of his letter to W. G. Parks with the enclosed proposed schedule. The schedule, I wrote, looks all right to me. On another matter I told Coryell that we are making constant use of the Appendices B and C of his PPR, Volume 9 for our updating of our "Table of Isotopes" and asked if he has reprints of these that he can send us so that we won't wear out our copies of the volume.

In today's mail was a rather long handwritten letter from Dick

Diamond (Harvard) in which he wrote that he had received from us both the tracers and his thesis. Dick mentioned that he plans to talk with Jack Schubert (Argonne), who will be visiting MIT, about his thesis. He also said that he heard from Ray Sheline, who is interested in the closed shell at 126 neutrons and 82 protons. Dick asked that we send a copy of our summary of atomic masses to Ray Sheline and, if a revised version is now available, a copy of that to him.

A note arrived from Bill (age 12) and John Sharp of Monte Vista, saying that they are interested in science and that both of them would like my autograph. The autographs went out immediately.

I also received a letter from John V. Dodge (Encyclopaedia Britannica), thanking me for agreeing to prepare a 3000-word article on "Transuranium Elements" and reminding me that the deadline for the material is May 1.

After lunch, I had an appointment for a chest x-ray with Dr. E. Schulze Heald (2560 Bancroft).

In addition to visiting some of the labs, I spent some time reading reports and journals. I noted that the Meinke, Ghiorso, and Seaborg article, "Further Work on Heavy Collateral Radioactive Chains," appeared in the February 1 issue of The Physical Review [Phys. Rev. 85, 429 (1952)]. In it we report the half-life of Ac^{222} , set an upper limit for the half-life of Pa^{225} , and report that we have partially identified an additional short-lived chain collateral to the actinium ($4n + 3$) natural radioactive family.

Thursday, February 21, 1952

I awoke with a migraine this morning and was too sick to go to the campus.

The research group met as usual this morning with the following people attending: Asaro, Carr, Carter, Clark, Dunlavey, Feay, Glass, Gunn, Higgins, Hoff, Hollander, Jaffe, Kalkstein, Levy, Martin, Momyer, Nervik, Passell, Perlman, Rasmussen, Ruben, Skirvin, Slater, Templeton, and Thompson. The first to report was Clark who told about his attempts to prepare crystals of $\text{LaCl}_3 \cdot n\text{H}_2\text{O}$. He reported that he found the value for n to be 7.16, but the determination has little significance since the crystals are deliquescent. He noted that the crystals are stable at relative humidities from about 11 to about 58% and are unstable on heating. At relative humidity less than 11%, the crystals decompose to a powder that is probably a lower hydrate as indicated by x-ray analysis. Templeton explained that this investigation was started when it was found that hydrolysis of the anhydrous chloride yielded an unidentifiable phase. Vapor pressure measurements on PuCl_3 indicate the existence of several hydrates. He said that removal of water from a hydrate sometimes leaves a similar structure, but this similarity is not immediately apparent on examination of the x-ray data as the symmetry changes.

Martin reported on his investigation of the characteristics of thallium-activated CsBr for use as a crystal in scintillation counting.

He pointed out that the advantages of CsBr are that its crystals are cubic, clear, non-hygroscopic and may be polished by optical means; however, it is only about one-fourth as efficient as NaI as regards pulse output.

Momyer said that they have been using the technique of ionizing emanation molecules in a glow discharge and then collecting them on a platinum plate (negative electrode) for the isolation and study of the characteristics of emanation isotopes produced in 184-inch cyclotron bombardments. He described bombardments of thorium with 340-Mev protons, in which the gaseous products were taken directly from the target. So far they have determined that Em^{210} has a 2.7-hour half-life and decays by electron capture and a 6.02 Mev alpha particle (90%). Em^{211} has a 16.0-hour half-life, decays by electron capture and a 5.82 Mev alpha particle. He described the determinations of the mass number assignments and the branching ratios. Finally, Momyer talked about the production of emanation from a thorium target bombarded with 100 Mev protons and the observation, as decay products of Em^{221} , of Fr^{221} , At^{217} , and Po^{213} alpha peaks in the pulse analyzer. The half-life of Em^{221} appears to be about 20 minutes.

Levy reported that, since an organic compound of bismuth was desired for use in a Szilard-Chalmers reaction in an attempt to get samples of Bi^{210} of higher specific activity than previously possible, he prepared a sample of tri- α -naphthyl bismuthine. He described his preparation, noting that the analysis checked quite well. They plan to bombard the sample for one day in the Oak Ridge pile and then check the enrichment factor with the RaE produced. If the results are promising, they may try a one-year irradiation in the Hanford pile or the MTR.

Asaro talked about his runs in the alpha ray spectrograph to attempt to identify the alpha groups reported a few weeks ago in a sample of $\text{Cm}^{242,3,4}$. Using three different samples (sample 1 was mostly Cm^{242} with between 0.1 and 0.3% of Cm^{243} and Cm^{244} in about equal abundance, sample 2 was about 50% Cm^{242} and 50% Cm^{244} , and sample 3 was pure Cm^{242}), he found the two large peaks of the highest energy are definitely assigned to Cm^{242} . The higher energy Cm^{243} group cannot be so definitely assigned as the background in the pure Cm^{242} sample is too high in this region. Thus, the peak may be due to either Cm^{242} or Cm^{243} . Pulse analysis indicates an alpha particle group in this energy region. He went on to note that the Cm^{243} group between the Cm^{244} groups is definitely not Cm^{242} as indicated by its absence in pure Cm^{242} . He said the assignment of the alpha groups of Cm^{244} was made by comparing the results of sample 2 with sample 1.

Dunlavey reported on his observation of 5582 alpha tracks from Pu^{239} in photographic emulsion. He said that 16% had electrons associated with them and 84% had no electrons. He said the electrons were in energy groups of 18-20 kev, 50 ± 5 kev, and a predominant group of 30 ± 5 kev, noting that these could result respectively from L and M conversion of the 51 and 37 kev gamma rays. He also observed electrons of 100 ± 20 kev in 0.1-1% abundance. Asaro commented on the work.

The last speaker was Browne who talked about his study of the x-ray spectrum of Np^{237} from the decay of U^{237} and compared this with that

produced by the decay of Am^{241} .

* * * * *



Friday, February 22, 1952

Since this is Washington's Birthday and a holiday, I spent the day at home. However, I have a couple of writing projects to keep me busy: the article for the Encyclopaedia Britannica on the transuranium elements, the revision of my article on the actinides for Sneed's Comprehensive Inorganic Chemistry, and my Nobel lecture.

Saturday, February 23, 1952

I again worked on my writing projects and played with the kids.

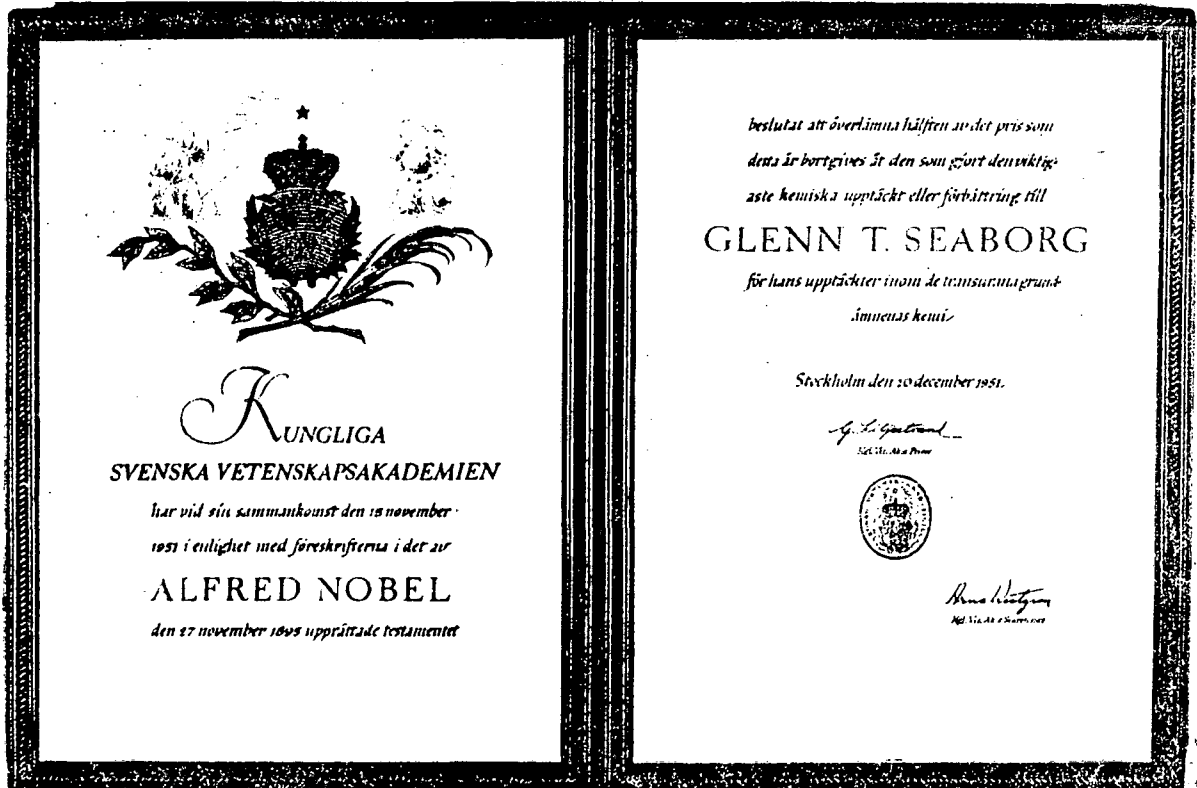
The Radiation Laboratory personnel recently received a copy of the following invitation to an Open House tomorrow in honor of Ed McMillan and me:

	<p>THE RADIATION LABORATORY UNIVERSITY OF CALIFORNIA will have an OPEN HOUSE Sunday Afternoon from 2 to 5 o'clock February 24, 1952</p>	
<p><i>The Latin inscription: "It is thru the inventive spirit of the arts that life is made more abundant" appears on Nobel medal.</i></p>	<p><i>Honoring</i> DR. EDWIN M. McMILLAN and DR. GLENN T. SEABORG <i>1951 Nobel Prize Winners in Chemistry</i></p>	<p><i>The reverse side carries a likeness of Alfred Nobel, who established these In- ternational Awards in his will. Medal is gold, 2½ inches in diameter.</i></p>
<p><i>For Radiation Laboratory personnel and their immediate families</i></p>		

Sunday, February 24, 1952

I played with the kids and worked on some of my writing projects during the morning. Then Helen and I went in to the lab for an Open House, honoring Ed McMillan and me. Jim Sheridan of Lawrence's office made many of the arrangements for the reception.

It was a well-attended reception featuring attractive photographs of McMillan's and my gold medals and certificates:



Burris Cunningham had prepared a display of some of the heavy elements, and a recording I had made of a few remarks about the transuranium elements in connection with the exhibit was played during the reception. Ed and I were presented with certificates signed by members of the Radiation Laboratory staff.

Glenn Theodore Seaborg

We are proud to congratulate you on the achievements which brought you the Nobel Chemistry Award for 1951.

Members of the Radiation Laboratory Staff
UNIVERSITY OF CALIFORNIA
February 24, 1952 - Berkeley, California

[The following text is extremely faint and illegible, appearing as a series of vertical columns of characters.]

Monday, February 25, 1952

There was quite a bit of correspondence to handle this morning since I was ill on Thursday. I wrote to George Boyd to thank him for his February 14 remarks about John Rasmussen. I wrote, "We think very highly of John and forecast a brilliant future for him wherever he decides to

carry on his work." I also thanked Boyd for his congratulations in connection with my recent trip to Stockholm.

I acknowledged a February 8 letter from Willie Osburn (President, Salvation Army Advisory Board, Oakland) who congratulated me on the Nobel Prize, which he read about in the War Cry. Osburn mentioned that he is of Swedish descent and visited Sweden last summer. I declined, however, his invitation to attend the Salvation Army Banquet on March 7 in Oakland, explaining that our schedule is so full that it is impossible to add to it. I added that I have a very high regard for the humanitarian contributions of the Salvation Army.

Dan Wilkes sent me an acknowledgement that he wrote for his book, Atomic Discovery, requesting my approval for mention of my name. I looked it over and then sent him a brief note saying that I believe it is ok.

I asked Doral to have copies made of a February 8 letter from F. S. Dainton (Chemistry Department, The University, Leeds) for Charles Coryell. Dainton explained that he would like to attend the Gordon Research Conference on Nuclear Chemistry but since he will be coming to the United States in the early fall, he feels he cannot make this additional visit. However, he wrote, he hopes to see some of the speakers in Berkeley.

Professor J. W. T. Spinks (University of Saskatchewan, Saskatoon, Saskatchewan) wrote on February 11 to say that he wants to attend the Gordon Conference and to say that he is willing to present a short paper on applied nuclear chemistry. He listed a number of projects in progress at his school. In today's reply I said that, although the program is becoming rather full, it seems to me that it will be possible to make room for a 10- or 15-minute account describing the general nature and progress of the work. I mentioned that I am sending his suggestion to Professor J. W. Kennedy, who will be the discussion leader of the session on "The Use of Tracers." Copies of my letter went to Kennedy and Coryell.

I declined a January 29 invitation from Dr. Frisch and Dr. med. Hein (Bavaria, Germany) to attend and lecture at a European meeting of Nobel prize winners in Lindau this coming June 23-27 because of previous commitments.

James Stokley (General Electric, Schenectady) sent me an \$8.00 check as my share of the royalties from Science Marches On. In a P.S. he thanked me for my kind letters to his youngster's school class. [I always attempt to answer all questions I receive from school children.]

A nice note arrived from Stanley E. McCaffrey, who thanked me for going on the President's Tour. Stan wrote, "Your presentation at the meetings was superb and your presence certainly added to the interest and enjoyment of the trip for all of us in the touring group."

I answered some questions from Miss Frances Ritzinger (Joe Kennedy's office at Washington University) about matters connected with the Division of Physical and Inorganic Chemistry and the Buffalo ACS meeting next month.

After the senior staff luncheon meeting at noon, I got together with Emilio to discuss the long and rather important letter that arrived from Donald Lane today:

LEE B. KEMON
DONALD E. LANE
SOLON B. KEMQAN

4/30/52
Copy sent to R. M. Underhill

LAW OFFICES OF
LEE B. KEMON
AND
DONALD E. LANE
PATENTS AND TRADE-MARKS
1331 G STREET, N. W. WASHINGTON 5, D. C.

Noted	2/25/52
Action	
Recd.	FEB 23 1952 GTS
Ans.	
File	

February 20, 1952

Dr. Glenn T. Seaborg
Radiation Laboratory
University of California
Berkeley 4, California

Re: AEC Docket No. 7

Dear Dr. Seaborg:

Yesterday the Patent Compensation Board met to consider AEC Docket No. 5, so I requested and had another pretrial conference for the purpose of considering the Regents' letter of October 8, 1951, and its effect on our proceeding. We discussed this matter at length off the record, and then a memorandum summary was given to the reporter by the Board to go in the record of this proceeding. I will send copies of the summary as soon as the court reporter can furnish same.

The gist of the conference may be summed up by stating the Board is not satisfied by the Regents' letter of October 8, 1951.

The Board states that in paragraph 1, the Regents "claim an interest", and that it would be impossible for the Board to properly evaluate applicants' interest, and

hence any dollar value of compensation, unless the nature of the Regents' claimed interest is in some way defined.

For example, if the Regents' "interest" should include the right for the Regents to authorize others to make, use and sell the inventions, the applicants' interest would have considerably less value.

The Board states that in paragraph 5, the Regents' modified disclaimer proposal is not applicable to contract eng-30 because it disclaims "compensation for" inventions, rather than making no claim "to any inventions", etc.

Various methods of getting the Regents to clear up these two vital points were suggested and discussed, including the old suggestion that applicants sue the Regents in the California state courts to perfect applicants' title to these inventions. I believe, of course, that such a suit should be unnecessary and that it would merely create stubbornness, hard feeling, and delay.

It is my inference from talking to the Board that if we can get the Regents to define their claimed interest and to execute the eng-30 disclaimer without changing its wording, then the Board will hold that it can proceed to evaluate applicants' commercial rights as set forth in eng-30, and which were restricted by the Atomic Energy Act.

It was suggested by Mr. Harter that applicants try once again to negotiate with the Regents to clear up the

title dispute, and that perhaps an offer by the applicants to give the Regents a non-assignable, non-exclusive, royalty free license to use the inventions, would satisfy the Regents, and would justify the Regents in making a new statement "claiming no interest other than the attached license to use" and "executing the eng-30 certificate of disclaimer without change".

While such a free license may seem to give the Regents more than you think they are entitled to, it is a suggestion worth considering if it provides any chance to untie the present title problem without resorting to local litigation. I think that the Regents would desire to avoid such litigation, and might be willing to accept something concrete, like a free license, in exchange for their current undefined but claimed "interest". Anyway, that is the Board's suggestion that they requested me to present to the applicants and to the Regents to get this proceeding moving.

Dr. Seaborg might try the suggestion out on Mr. Underhill, and if acceptance appears possible, I would be glad to prepare the license papers and to propose a new Regents' statement to AEC.

The Board is agreed that this title problem should be concluded before proceeding with a determination of the inventions and of the value of the commercial rights therein taken over by the Atomic Energy Act on August 1, 1946.

I will be glad to have your comments and suggestions. If you want me to correspond with Mr. Underhill and/or Mr. Neylan, please advise.

At the conference, applicants' committee membership status was briefly mentioned, and the Board indicated it did not consider this point important at this time. However, I was asked if applicants were on any AEC committees or panels as of now. Since the conference, I note in the July-December 1951 AEC report, page 140, that Dr. Seaborg is listed on the Advisory Committee on Chemistry. Is that 1949 appointment still effective?

Enclosed are copies of Capt. Lavender's letter of June 12, 1945, to Mr. Underhill, which Mr. Anderson's office has furnished. I am told that AEC wrote Mr. Underhill on February 6, 1951 for certain correspondence, but that it has not yet been furnished, although over a year has passed.

Very truly yours,

Ronald E. Lane

DEL.mb
AIRMAIL
cc: Segrè, Kennedy, Wahl

P.S. I believe we could make progress with the AEC if the Regents' letter of October 3, 1951 to AEC could be modified as follows:

In paragraph designated 1., after "interest" insert

-- , said interest being defined in the accompanying non-exclusive, non-assignable license, --

In paragraph designated 5., reword to read -- That the Regents of the University of California hereby ratify the agreement dated September 17, 1945, identified as Contract No. W-28-094-eng-30, by executing nunc pro tunc the Certificate of Disclaimer accompanying said Contract Agreement. --

DEL.

Emilio and I prepared a draft of a letter to John Francis Neylan, which we sent on to Kennedy and Wahl, saying that we thought perhaps the thing to do is to get in touch with Neylan again, since this will have to be done eventually anyhow. I said that we propose sending the enclosed draft. I also wrote that the reference to the non-assignable, non-exclusive royalty free license is deliberately omitted with the thought that this might be brought up in a conference with Neylan which would presumably follow sometime after his receipt of this letter. We see, I wrote, no objection to such a license if it should somehow satisfy the Regents, but we have no way of judging whether or not this will be the case. Our draft to Neylan read:

Our attorney Mr. Lane has finally succeeded in having an informal talk with the AEC Patent Compensation Board in connection with the Regents' letter of October 8, 1951 and has reported the following results.

The Patent Compensation Board states that in paragraph 1, the Regents "claim an interest", and that it would be impossible for the Board to properly evaluate applicants' interest, and hence any dollar value of compensation, unless the nature of the Regents' claimed interest is in some way defined.

The Patent Compensation Board also states that in paragraph 5, the Regents' modified disclaimer proposal is not applicable to contract eng-30 because it disclaims "compensation for" inventions, rather than making no claim "to any inventions", etc.

It is apparent that this title problem has not been concluded and that further discussions between the inventors and the Regents are necessary in order to take advantage of the Regents' friendly intentions.

Tuesday, February 26, 1952

I spent most of the morning on campus: checking in the Department of Chemistry office, keeping office hours in Room 23, Lewis Hall, and then giving the Chemistry 223 lecture.

Up on the hill I looked over the mail and replied to C. B. Marquand's letter of February 16. I told him that I shall plan to attend the meeting of the ACS Committee Advisory to the Chemical Corps in Buffalo on March 27, 1952.

Later I made the rounds of the labs and talked with some of the students.

Wednesday, February 27, 1952

Aside from phone calls and conversations with various people, this was a rather quiet day.

The mail brought a note from William J. Norton (Business Manager, Radiation Laboratory), who said he was sorry to have missed the Open House at the Radiation Laboratory on Sunday. He wrote, "It is indeed a

pleasure to note that the University continues to grow in the field of education and research led by such men as yourself."

Doral finally found time to go over the receipts, etc. of Helen's and my Swedish trip and to prepare an itemization of the expenses covered by my travel advance. This she sent to Eleanor Irvine, Ernest Lawrence's secretary.

Thursday, February 28, 1952

This morning the research group meeting was attended by Asaro, Biller, Bertelli, Carr, Clark, Dwight Conway (an undergraduate student), J. Conway, Dauben, Dunlavey, Feay, Fleming, Higgins, Hoff, Hollander, Hulet, Hyde, Jaffe, Jenkins, Kalkstein, Koch, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Seaborg, Rex Harrison Shudde (graduate student from UCLA who will do his graduate research with our group),

Rex Shudde



Skirvin, Slater, Street, Templeton, and Thompson.

Thompson reported a new pile neutron capture cross section for Pu^{238} , obtained by irradiating a mixture of $\text{Pu}^{238,242}$ containing small quantities of intermediate plutonium isotopes in the Chalk River pile. The value is changed from 218 barns to 420 barns. He noted a recent Brookhaven table gave 425 ± 75 barns. Thompson then gave our best values for the capture cross sections for pile (Chalk River and Hanford) neutrons (in barns) Pu^{238} , 420; Pu^{239} , 325; Pu^{240} , 410; Pu^{241} , 307; Pu^{242} , 50. He noted that the values obtained in Chalk River and Hanford irradiations are quite similar, adding that these cross sections were obtained by mass spectrometric analysis before and after irradiations in the pile. There was considerable discussion about neutron capture cross sections during which time Rasmussen said that the neutron capture cross sections are supposedly determined in a random manner involving how close a nuclear resonance level is to the thermal neutron capture excitation of the nucleus. I added that when the neutron binding energy is high, the levels are close together, and there is a greater chance of neutron capture. I also mentioned that we have calculated, with the help of Dick Glass, the isotopic masses quite accurately, and the neutron binding energies do not appear to vary uniformly with the mass number. Again there was much discussion.

Dunlavey reported that there is either fine structure in Th^{232} or there is contamination of 3-5% U^{238} in his sample. He is now preparing

some cleaner thorium. I noted that Thompson has heard from Bernard Harvey of Chalk River about new alpha energy values: Th²³², 4.05 Mev; Cm²⁴², 6.118 ± 0.003 Mev.

Asaro reported that they have been attempting to trace down reports that certain even-even alpha emitters have three alpha groups. So far he has found, at most, two alpha groups in such nuclides. He has looked at Ra²²⁶ from 500 to 700 kev below the main group and can state there is no group of abundance greater than 0.1%. He has looked at U²³⁰ and will look at U²³²

Michel talked about the changes incorporated into the new time of flight mass spectrometer: Berkeley boxes have been added for high level activity work and flexibility has been increased to enable the collection of four isotopes consecutively on a platinum plate. He mentioned a few other proposed changes.

Rasmussen then described the circuit improvements they have made in the nucleometer (Dwight Conway is working with him). He presented curves of some of the runs they have made on isotopes such as Sr⁹⁰-Y⁹⁰, W¹⁸⁵, Gd¹⁴⁸, and Pm¹⁴⁷. He gave a quite complete report on work they intend to do.

Rasmussen also talked extensively on some proposed electron screening corrections for alpha decay calculations. Again there was much conversation about the proposal.

* * * * *

After the Chemistry 223 lecture, I returned to my office on the hill and looked over the mail and my messages.

Earl prepared an evaluation letter about Giles F. Carter in reply to a February 13 request from Bruce J. Miller (Personnel Administrator, Union Carbide and Carbon Corp., Tonawanda, New York), which I signed and mailed today. I explained that Carter's research work was under the supervision of Professor David H. Templeton on the determination of inorganic crystal structures by the x-ray method, and I feel he has performed well at our weekly research group meetings and appears to understand the work of the group. I said that Carter is young and has had limited laboratory experience, but this handicap should be overcome with a moderate amount of supervision in the first year or two on his first job.

A short letter went to James R. Bercaw (Ohio State) to accompany a manuscript, "Nuclear Thermodynamics of the Heaviest Elements." I plan to speak on this subject in one of my Phi Lambda Upsilon lectures there in March. I also gave Bercaw my travel schedule (I will arrive on March 27 at 7:46 p.m. on American Flight 773 from Buffalo and leave on TWA Flight 221 at 7:35 p.m. on March 29). I asked Bercaw to give the information to the Kurbatovs.

L. Jackson Laslett (Iowa State College) wrote on February 16 that his colleagues, Dr. D. Martin and Dr. A. Voigt, will probably attend the Gordon Conference and, since some of the topics are related to important aspects of his nuclear physics program, he also expects to attend. I

acknowledged his letter and said that I think it will add a lot to have the work at Iowa State represented. In addition, I thanked him for his congratulations about the Nobel Prize.

I accepted a February 19 invitation from Arthur I. Goldberg (Bureau of Public Relations, The University of Buffalo) to participate in a radio and television round table on Saturday, March 22 (7:30 p.m. to 8 p.m.) on the subject "Chemistry Looks to the Future." I told Goldberg that I plan to arrive in Buffalo late Friday night and shall stay at the Statler Hotel if my reservations come through. [I have also received a letter, dated February 20, from Edward S. Shanley (Chairman, Publicity Committee for the Buffalo ACS Meeting, describing the program and saying that he had suggested my name as a participant.)]

Today's mail brought a lengthy letter from Milton Burton (University of Notre Dame), who wrote that most of their applicants for graduate appointments are from small Catholic institutions while those from non-Catholic institutions are not up to the standards that he expects for graduate students. Burton emphasized that non-Catholic students are subjected to no missionary influences at Notre Dame. He also reported that, with regard to political atmosphere, he believes the characteristic that marks the campus is a tolerant acceptance of expression of almost any shade of opinion. He went on to say that the point to his introductory explanation is that any good students interested in their work if it were a non-Catholic institution, should not hesitate for that reason. He enclosed announcements of their program. I routed the letter to Iz.

Later I wandered through the labs to check on the research.

Friday, February 29, 1952

Yesterday a paper, "Certain Criteria of Nuclear Stability and their Relation to the Mechanism of Radioactive Disintegration and Nuclear Fission" by C. A. Muses, arrived from W. Albert Noyes, who said that the author objected to the decision of the referee rejecting the publication of the manuscript in Journal of the American Chemical Society. Noyes explained that, in such a case, his policy is to refer the manuscript to a member of the Editorial Board, and he asked my recommendation. I looked over the paper and today wrote Noyes, "The article is, in my opinion, definitely not worth publishing. In fact, it is quite obvious that this represents an outstanding example of scientific illiteracy."

I also sent a note to Professor M. Cannon Sneed (University of Minnesota) to inform him that the revision of my chapter on the actinide series for his book Comprehensive Inorganic Chemistry is well along and he can expect it in a couple of weeks. I said that it now appears that it will run about 100 typewritten pages.

A February 27 letter arrived from Leo Yaffe (Chalk River), who said that he is leaving Chalk River to take up a position directing nuclear chemistry research at McGill University. Yaffe said that he would like to visit Berkeley to discuss the work he might do at McGill. He asked that, if all this can be arranged, if a visit near the end of April will be satisfactory. Yaffe mentioned that he has definitely decided to

attend the Gordon Conference.

I made my usual rounds of the labs and worked on some of my writing projects.

Saturday, March 1, 1952

I spent the day at home with Helen and the kids and working on my various writing projects.

Sunday, March 2, 1952

Again, I worked on my writing projects. Later we honored Helen, whose birthday is today, with gifts and a birthday cake. She also received a birthday gift box, which contained an apron, refrigerator dishes, saffron bread, etc., from my parents.

Monday, March 3, 1952

At the lab this morning I asked Doral to have a copy made of a February 12 letter from Joseph Weiss (University of Durham) and send it to Coryell. Weiss reported that he will be unable to attend the Gordon Conference because he spent eight weeks in the United States last year; however, he is interested in the problems in nuclear chemistry and hopes to attend one of our later conferences.

At the noontime senior staff lunch meeting Stan Thompson showed me the response he had written to a recent letter from Bernard Harvey (Chalk River). Stan told Harvey that we are very interested in his recent alpha energy measurements and mentioned that Perlman had asked him to report that our best alpha energy value for Cm^{242} from the alpha ray spectrometer is 6.110 Mev. Stan also reported that Hyde, Glenn, and Ghorso have cracked the Em^{221} problem and described the work. Thompson discussed the arrangements for processing the plutonium samples and mentioned plans to attend the Gordon Conference.

Later I signed a carefully worded memorandum, addressed to Don Cooksey and prepared by Earl Hyde, about our desire to purchase with the laboratory's equipment fund a new optical spectrograph and spectrograph magnet. John Conway and Burris Cunningham will be most closely associated with the work, I wrote.

I also signed a memorandum to Don Cooksey, requesting authorization for my upcoming trip to Chicago, Buffalo, New York, and Columbus, March 20th to 30th. As is required for such authorizations, I listed my plans for each visit.

In today's mail was a list, from Joe Kennedy as Secretary-Treasurer, of topics to be discussed at the Executive Committee Meeting of the Division of Physical and Inorganic Chemistry on Monday, March 24, in Buffalo.

Tuesday, March 4, 1952

I spent most of the morning on campus: checking the department office and talking with various colleagues, keeping office hours, and delivering the Chemistry 223 lecture.

Back on the hill, in my position as Chairman of the Division of Physical and Inorganic Chemistry, I wrote to Milton Burton, Isadore Perlman, and John E. Willard to ask them to serve as a nominating

committee. I explained the duties of the committee, saying it is the duty of the committee to suggest a candidate for Chairman-Elect (by tradition this should be Joseph W. Kennedy), a candidate for Secretary-Treasurer, a candidate for Councillor (tradition suggests the same as the candidate for Chairman-Elect), a candidate for Alternate Councillor (traditionally the same as the candidate for Secretary-Treasurer), and two candidates for membership on the Executive Committee for a three-year term (the candidates this year will replace G. K. Rollefson and George Glockler, whose terms expire at the close of the fall meeting). I concluded by saying that I shall assume they are willing to serve unless I hear to the contrary, adding that it would be convenient to have the slate of candidates by the time of the Executive Committee meeting in Buffalo, scheduled for Monday, March 24.

The rest of my day was spent reading, writing, and talking with the students.

Wednesday, March 5, 1952

I had the usual number of phone calls and interruptions this morning, but I did manage to dictate a few letters. One went to Lee A. DuBridge in reply to his letter of February 7, 1952, in which he invited me to serve as a judge at the first Southern California Science Fair. I wrote, "I have delayed accepting your invitation because my traveling schedule has been so uncertain that I could not be sure that I could make it on April 18. I still have one trip for which a date has not yet been decided, but I feel that it is very unlikely that this will come at exactly that time."

I also wrote to Leo Yaffe (Chalk River), who wrote on February 27 about his desire to visit Berkeley, saying that we shall be glad to have him visit provided the proper official arrangements are made. I informed Yaffe that Al Ghiorso and Stan Thompson will be away during the last week of April and the first week of May, and I shall be gone during May and June. I suggested that perhaps he should come around the middle of April if he wants to discuss his work with members of this group, pointing out that we can make satisfactory arrangements to handle his requirements through discussions with others if his time schedule makes this necessary. I added that I am glad to hear that he is planning to attend the Gordon Research Conference on Nuclear Chemistry.

James H. Stack (ACS News Service) asked on February 26 for an advance copy of my address on "Masses of Translead Nuclides" at the Buffalo meeting. Today I sent him some material, used as a basis for the talk, but explained that I am not writing up this talk. Stack uses such papers for publicity purposes.

The mail brought a first draft of John Voelker's proposed new piece on me called "The Man Who Sparked the Atom Bomb." He wrote, "In many ways it is not (and deliberately so) as dignified or detailed a piece as my first one on you. The reason is that I am now sadly convinced that no magazine of general popular interest would otherwise consider it; and my fear is that even now it may be too esoteric." Voelker added that he hopes he hasn't committed too many blunders of fact. I shall take this home to discuss it with Helen.

Last month I received a critique of the Journal of Organic Chemistry from A. L. Marshall (Chairman, Council Committee on Publications and a member of the General Electric Research Laboratory in Schenectady), which implied that the journal was of little use. I consulted with my campus colleagues about the letter and received rather irate comments from our organic men. Jim Cason wrote to Marshall, expressing the opinion of our organic men that the journal was invaluable. Today a February 29 letter arrived for me from Marshall, stating that Cason misunderstood his attitude, which was that General Electric could get by with one copy rather than the extra copies of the journal that they usually order, not that the journal itself should be eliminated. Marshall also mentioned that the subject of the meeting in Buffalo will be Chemical Abstracts. I had a copy of Marshall's letter sent down to Jim Cason.

Thursday, March 6, 1952

The group meeting this morning was attended by Asaro, Browne, Bertelli, Carniglia, Carr, Carter, Clark, D. Conway, J. Conway, Cunningham, Dauben, Dunlavey, Feay, Glass, Higgins, Hollander, Huffman, Hulet, Hyde, Jaffe, Jenkins, Kalkstein, Koch, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Seaborg, Shudde, Slater, Street, Templeton, and Thompson.

Rasmussen talked for much of the meeting on the subject of the electron screening correction to alpha decay rate calculations. This subject, which he reported on last week, evoked much discussion among the senior members of the group.

Higgins reported on their studies of the decay properties of Cm^{238} and Cm^{239} produced from helium ions on Pu^{239} . They separated the curium from the americium, also formed in the bombardment, and found, in the nucleometer, activities of 2.3 hours, about 8-9 hours, and a tail of about 50 hours. He said the americium fraction showed a 12-hour activity (Am^{239}) followed by a 50-hour activity (Am^{240}). Higgins added that the alpha half-life of the 2.3-hour activity was calculated to be about 2.8 days. He went on to say that the two attempts to milk the daughter of the 6.5 Mev alpha emitter have been partially successful in that the 8 hour, 6.15 Mev alpha particle of Pu^{234} was seen, showing the presence of Cm^{238} as an alpha emitter. The ratio of the 8-10 hour, EC activity to the 50-hour tail was six times larger than the ratio of 12-hour to 50-hour tail in the pure americium indicating, perhaps, the presence of an ~8-hour activity that might be assigned to Cm^{239} . Higgins added that the Cm^{238} activity includes the daughters, Pu^{234} and Am^{238} in equilibrium, and they plan to try to milk the americium daughters that grow from the separated curium in order to determine more precisely the existence of Cm^{239} . I suggested that he enlist Dunlavey's aid in the application of the photographic emulsion technique to the problem for the 6-pronged decay in the U^{230} granddaughter of Cm^{238} should be quite easy to identify.

Cunningham announced that there is an important correction to the published ultraviolet absorption spectra of aqueous curium compounds, and J. Conway presented the qualitative absorption curves. This work was discussed by Perlman and Cunningham.

Slater said that they are investigating the excitation function for the reaction $U^{238}(d,p)U^{239}$. They have looked into the matter of interference and concluded that it will be possible to do the work. I suggested that he start at the lowest energies first, using the 60-inch cyclotron.

Dunlavey spoke briefly about his photographic emulsion studies on U^{232} in which conversion electron coincidences were used to investigate the fine structure. He said that roughly 50% of all the U^{232} alpha particles had such coincident conversion electrons, indicating that a large amount of decay goes to excited levels of the daughter. The electron energies were mainly 25 ± 5 kev, but definite groups were observed with energies of 40 ± 5 kev and 60 ± 5 kev. Hence, Dunlavey said, it appears quite likely that more than one excited level exists in Th^{228} .

* * * * *

After my usual campus activities, including giving the Chemistry 223 lecture, I went up to the hill.

In connection with the Gordon Conference, Charles Coryell sent me a copy of a letter he received from G. Kayas, now at the Ecole Polytechnique with Professor L. Leprince-Ringuet. Kayas asked, since he does not know anyone at Berkeley, if Coryell could help him get some Kodak or Ilford nuclear plates irradiated with various particles at Berkeley for use as standards. Perlman suggested that I talk with Walter Barkas, which I did. Today I wrote Coryell to tell him that Barkas agreed to get in touch with Kayas to work out some sort of bombardment schedule.

In today's mail was a memo from Don Cooksey about the fine show of heavy elements that Burris Cunningham prepared for the Open House. Cooksey suggested that I go out and get another Nobel Prize so we can have another party.

Much of the afternoon was spent on various writing efforts.

Friday, March 7, 1952

Much of my day was spent on the usual phone calls, checking on the research, and working on various writing projects.

In today's mail was a March 5 letter from Arthur I. Goldberg (The University of Buffalo), thanking me for accepting his invitation to participate in the University of Buffalo Round Table on Saturday evening, March 22. He announced the other participants: Dr. Edgar C. Britton (President, American Chemical Society), Dr. Henry M. Woodburn (Chairman, Chemistry Department, University of Buffalo), and Dr. Carleton F. Scofield (Chairman of the Psychology Department and moderator). The subject for discussion will be "Chemistry Looks to the Future." Goldberg went on to describe some of the details.

I also received a response from Mel Freedman (Argonne) to my recent query about the disintegration energy of Ra^{225} . Freedman explained that their value of 300 ± 50 kev is very rough and they plan to run another

sample in a couple of weeks on the beta and gamma scintillation spectrometer. Mel went on to give me data for other nuclides, describing each as either "A" finished and declassified, "B" finished but not declassified, and "C" unfinished but suitable for rough estimates. He offered to send more information when he gets it. I routed the letter to Dick Glass.

Saturday, March 8, 1952

I continued to work on my writing projects today, interrupted occasionally by one child or another desiring some fatherly attention. Young Steve now has two teeth--he and Pete are great pals, and Pete can make Steve smile even when Steve is grumpy with the rest of us. Helen (and Pete) have reported to me that David is now getting into more mischief.

Sunday, March 9, 1952

Again, I spent the day with the family and on my writing projects.

Carmel ("Candy") Way, our live-in helper, has been ill much of the time in the last few months. In fact, recently she was so ill that, for about three days, she was barely able to get out of bed to go to the bathroom; so Helen called her family to ask them to come and get her in order that she could get the care she needed. I told Helen that we should not have her come back for she would just get sick and give something to the kids again. When Candy came to pick up her possessions, she said that she has decided to go back to Illinois and live with her sister. So young Pete has moved into Candy's room--he is delighted with all the extra room for his toys.

Monday, March 10, 1952

After numerous phone calls and taking care of other administrative duties, I worked on my writing projects.

In today's mail was a letter from Margaret H. Kurbatov (Ohio State), stating that her husband has been ill and it appears that his recovery may take several more weeks. She said she feels that their home will not be a pleasant place for me to spend my stay in Columbus and they will make other arrangements for my accommodations (my talks there are scheduled for March 28 and March 29). Mrs. Kurbatov also thanked me for a recent batch of reprints.

Also, in today's mail was a memorandum from Francis A. Jenkins (Associate Dean of the Graduate Division), saying that Warren Heckrotte was advanced to candidacy for the degree of Doctor of Philosophy in the field of physics and his thesis committee is composed of Robert J. Riddell, Jr., Glenn T. Seaborg, and Joseph V. Lepore.

At noon, in our lunchtime staff meeting in my office, we discussed our continuing budget problems, including funds for research equipment, off-site special runs and experiments, and postdoctoral appointments. I told the group about recent conversations I have had with Wally (W. B.) Reynolds (Radiation Laboratory Business Manager), who has asked that I

discuss these items in a memorandum to C. E. Andressen, Jr. of Reynolds' office.

In the afternoon I made the usual rounds of the labs to look in on the research.

Tuesday, March 11, 1952

There were the usual phone calls, etc., then I went down to campus. I checked in the Department office, held office hours in Room 23, Lewis Hall, and then gave the Chemistry 223 lecture.

Back on the hill I read a letter from W. George Parks (Director of the Gordon Conferences), suggesting that our conference be represented at a meeting of the Advisory Board of the Gordon Research Conference at 6:00 p.m. on Monday, March 24, 1952, in the Georgian Room of the Hotel Statler in Buffalo. I had Doral copy the memorandum and send it to Coryell, along with a covering letter from me explaining that I won't be able to attend this meeting because that is the time for the meeting of the Executive Committee of the ACS Division of Physical and Inorganic Chemistry. I wrote that perhaps he (Coryell) will be willing to attend and, if so, he should get in touch with Parks directly.

I also received a note from James B. Plate (Associate Editor, Prentice-Hall, Inc.), notifying me that he is sending me, under separate cover, a copy of Sillén's Problems in Physical Chemistry. He asked for my reactions to the text.

Wednesday, March 12, 1952

Today I dictated and signed a memorandum to C. E. Andressen, Jr. about our difficulty in securing funds for certain research problems and equipment. I listed some specific items costing in excess of \$1,000 that we would like to purchase within the next few years: an x-ray spectrometer (x-ray crystallography) with Geiger tube recording (\$20,000), a Carey recording spectrophotometer (\$10,000), a Cauchois type x-ray spectrometer (\$5-10,000), an alpha-particle spectrometer (\$5-30,000), and hydrogen leak detector (Annie) (\$4,500-5,000). I gave some details about the equipment, saying that I have not listed small items costing less than \$1,000, and suggesting that a yearly increase in our budget of \$30,000-\$40,000 would be suitable. I detailed the need for an additional \$50,000 yearly for our off-site runs and experiments at Hanford, the new Arco reactor, Oak Ridge, Chicago, and Chalk River. Finally, I wrote that we should like to have funds to allow the addition of two postdoctoral men, in addition to those we now have.

I also wrote a brief note to James Stack (ACS News Service) to inform him that the dinner speaker for the Division of Physical and Inorganic Chemistry in Buffalo on Wednesday, March 26, will be Dr. Alan T. Waterman, Director of the National Science Foundation, who will speak on "Present Status and Future Plans of the National Science Foundation."

My schedule and the weather has not been particularly conducive for golfing lately, but this afternoon Al Ghiorso, Stan Thompson, Bill Jenkins, Luis Alvarez, and I went out to Mira Vista Country Club. My

score reflected my lack of practice (AG-94, SGT-86, GTS-104, WAJ-89, LWA-103).

Thursday, March 13, 1952

The following attended the research group meeting this morning: Asaro, Browne, Carniglia, Clark, Cunningham, Dauben, Dunlavy, Glass, Glenn, Gunn, Hoff, Hollander, Hyde, Jaffe, Kalkstein, Koch, Levy, Martin, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Seaborg, Templeton, and Thompson.

First to report was Martin, who described experiments with Hoff in which they bombarded niobium with carbon ions in an attempt to find alpha activities in the 50-neutron region. Both silver and palladium beta and electron capture activities were produced, but they found no conclusive data on alpha emitters although there was some evidence of a short-lived (about 1 minute) 3 Mev alpha emitter in low abundance. They unsuccessfully tried bombarding palladium to find this activity. Martin said they saw a two-hour silver activity with both carbon ions (on niobium) and protons (on palladium). In their last bombardment (carbon ions on niobium) they found a 4 Mev alpha activity with a half-life less than two days; this may be due to a rare earth contamination. I suggested they run down the 4 Mev alpha activity even though it seems doubtful for this region.

Martin also reported that he had seen a 76 kev gamma ray, ascribed to Am^{243} , in the americium fraction from the last Chalk River neutron irradiation of Pu^{242} , on the scintillation pulse analyzer. This was present in 90% of the alpha disintegrations. Perlman commented that it seems very similar to Am^{241} , in that the ground state alpha transition must be in very low abundance if the gamma rays are so abundant. There was quite a bit of discussion about the work.

Browne announced that he has data on x-rays and gamma rays in the decay of Pa^{232} and Pa^{233} , but it will take at least a half hour to report. He has not formulated a decay scheme.

Cunningham announced that a group at Ames has determined that the heats of solution of cerium and neodymium are like those of lanthanum and praseodymium which we determined some time back, and found them also to differ from the older values by about 10 Kcal. He said it would seem that the values in the NBS tables are in error by this amount.

I then told the group that Glass has calculated isotopic masses from closed cycles and calculated chemical atomic weights from the results; these differ sharply from accepted values in the heavy region. This led us to suspect that there were errors in the chemical atomic weights in the lighter elements; Kalkstein, in examining the entire periodic table critically, found several which need changing. I listed the more correct values he found for thorium, uranium, gold, ruthenium, thulium, iridium, terbium, palladium from isotopic masses and compared these with the hitherto accepted chemical atomic weights.

Templeton spoke about some Swedish work on the crystal structure of tungsten nitride, in which they assumed that the nitride was W_2N ; the

workers were forced to postulate 256 tungsten atoms per unit cell in order to explain the intensities. The intensities are well explained if one assumes the compound to be W_3N .

Asaro spoke briefly on a preliminary run on U^{233} with the alpha ray spectrometer. He found two alpha groups separated by 40 kev with the high energy group in about 90% abundance and the low energy group in about 10% abundance.

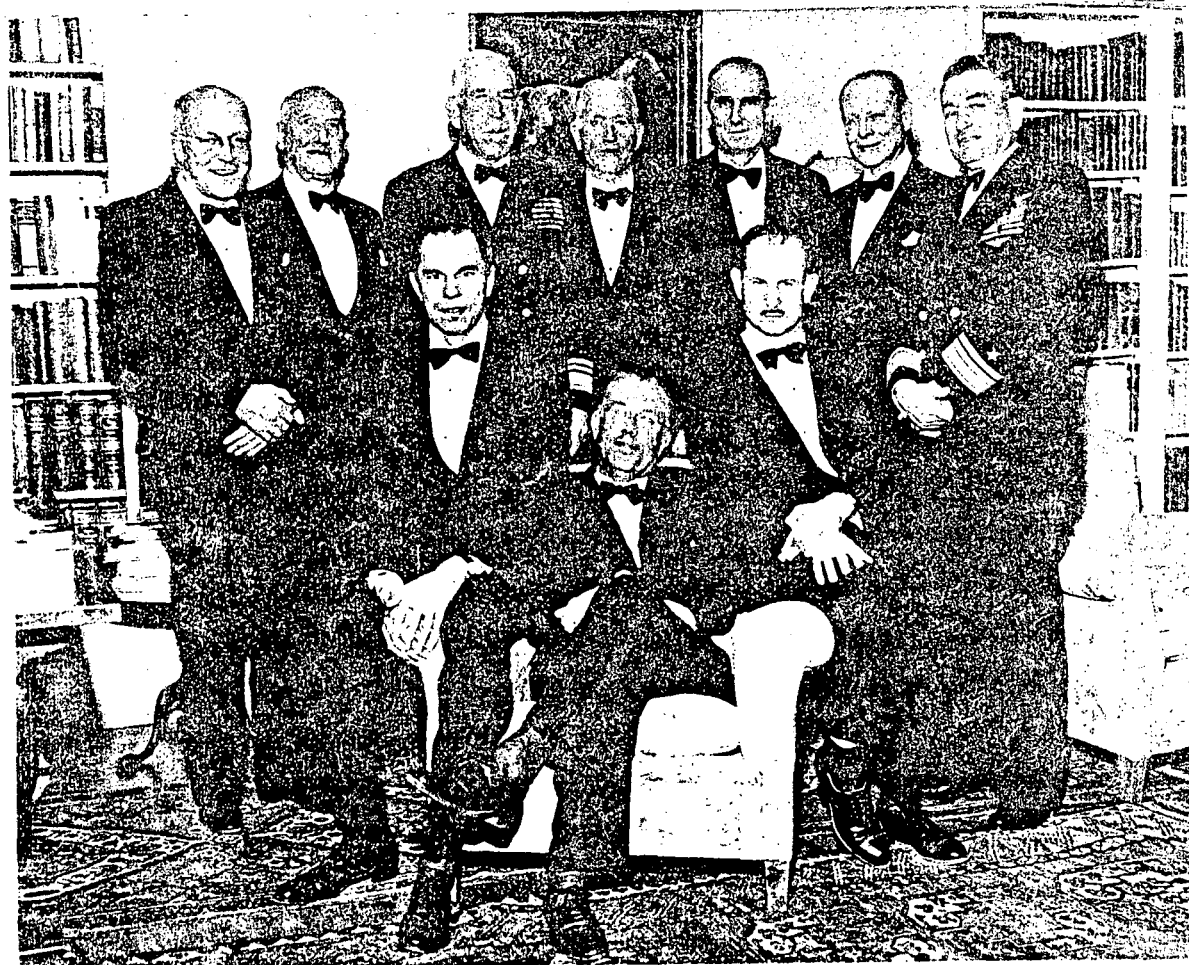
Dunlavey then talked about alpha-electron coincidences from U^{238} in photographic emulsions. He found 27.5% of the alpha particles have electrons in coincidence, while 72.5% do not. After making corrections, he concludes that less than or equal to 21.5% of the alpha particles have electrons in coincidence and 78.5% do not. The electrons are L and M conversion electrons of 25 ± 5 kev and 40 ± 5 kev gamma rays. Dunlavey also found in Th^{232} an electron activity of about 35 ± 5 kev in coincidence with 20-30% of the alpha tracks.

* * * * *

After the Chemistry 223 lecture, I went up to the hill and checked the mail and my messages.

I read a February 15 letter from Raphael Attard (Director, Nova et Vetera, Malta), who had solicited an article from me some time ago. Attard wrote, "It is with great regret that we beg to inform you we have decided to call off for the present our efforts at publishing the above quarterly. A number of factors have forced us to this decision, the most important being the greatly increased cost of paper, printing, and postage...." Finally, Attard congratulated me on the Nobel Prize.

I again worked on my writing during the afternoon, but I left early in order that Helen and I could attend a black tie dinner given by the Swedish Consul and Mrs. Manne Lindholm in honor of Edwin McMillan and me. Also invited to this black tie affair were Wendell M. Stanley (Nobel Prize chemistry, 1946), William F. Giaque (Nobel Prize chemistry, 1949), and Ernest O. Lawrence (Nobel Prize, physics 1939) with their wives, and others (see picture).



Nobel Laureates at Swedish Consulate, San Francisco, March 13, 1952
Seated (left to right): Glenn T. Seaborg, Ernest O. Lawrence, Edwin M. McMillan; Standing: Wendell M. Stanley, Norwegian Consul General Jörge Galbe, Vice Admiral J. L. Hall, Jr., Swedish Consul General Manne Lindholm, William F. Giauque, Matson Line Director William P. Roth, Rear Admiral Bertram J. Rodgers.

Friday, March 14, 1952

In addition to the draft of Voelker's article, "The Man Who Sparked the Atom Bomb," which I received last week, I also received a March 4 letter from him, in which he asked me to return the second copy of his draft with my criticism. I returned the draft this morning with my comments: "Looked upon as a literary effort, I think that it is good, but as the subject of the discourse I am worried about its flattering nature. I realize that this increases its popular appeal, but it can be quite embarrassing; perhaps it is possible to tone it down in this regard. In particular, I think that it is of paramount importance to mention the names of my co-workers on the discovery of plutonium, as I have noted on the second page. I also wonder whether a little something

more on the personal side wouldn't counterbalance the laudatory description of my work."...

I replied to a February 5 letter from Walther Seeburg Kaufmann (Saarbrücken, Saarland), who inquired if we might be related. He described briefly his father's family, some of whom emigrated to England. I wrote that my ancestors came directly from Sweden some time ago and have never been settled in England.

Among the usual numerous phone calls this morning was one from a Donald Jones, who wanted to meet with me to discuss a science television show. We made an appointment for him to visit me at home in Lafayette tomorrow evening.

In the mail today was a note from James R. Bercaw (Ohio State), reporting that he received my manuscript (for my Phi Lambda Upsilon talk), "Nuclear Thermodynamics of the Heaviest Elements." He noted that the printer was somewhat concerned about the reproduction of the glossy print of the energy surface figure. Bercaw said that they have made reservations for me at the Deshler-Wallick Hotel in Columbus. I then dictated a note, addressed to Mrs. Kurbatov, saying that I was very sorry to hear about her husband's illness and that Mr. Bercaw has written me that I have reservations at the Deshler-Wallick Hotel.

In connection with the Gordon Conference I wrote a note to Professor F. S. Dainton (The University, Leeds, England). I said that I am sorry that he will be unable to attend but I am glad to learn that he will visit the United States in the fall and that we may say him in Berkeley.

The rest of the day was spent in my usual manner--reading, writing, and checking the research.

Saturday, March 15, 1952

As usual, I spent time with the kids and with my various writing projects, which I hope to have in reasonable shape before leaving for the ACS meeting next week.

Bill Jenkins, as he often does, stopped out at the house in the afternoon to talk and play with the kids. He had dinner with us and was still visiting when Donald Jones (N. W. Ayer & Son, Inc.) arrived for his appointment with me. Jones talked about a science TV program that he wants to produce for AT&T. Although he hopes to present the program this fall on CBS or NBC, the program is still in the planning stage.

Sunday, March 16, 1952

Again I spent the day around the house with the kids and my various projects.

Bill Rice is completing the drawings for the addition to our home, in which we plan to add two bedrooms, a sewing room, and a large closet, to accommodate our growing family.

Monday, March 17, 1952

Included in this morning's phone calls was one from Joe Kennedy about a few matters connected with the ACS meeting in Buffalo. I also brought up the draft of our letter to John Francis Neylan, which Emilio and I wrote and sent to him on February 25. Joe said that he and Art agree with the draft, so I had Doral retype the letter for me to sign and send to Neylan.

I looked over a draft paper from D. B. Langmuir (U. S. Liaison Officer, Chalk River) to H. A. Fidler (Berkeley AEC Area Manager) about the visit of Leo Yaffe to our lab. Langmuir asked that corrections to the paper be sent directly to Fred Hobbs in Washington, along with a list of names of people with whom Yaffe might have classified discussions. I looked over the paper, which covered the material Yaffe may want to see and discuss, and told Doral to inform Fidler that I find it satisfactory.

Burris Cunningham asked me to approve a letter to Ralph W. Burhoe (Executive Officer, American Academy of Arts and Sciences). Burris wrote that the AEC has not granted approval for the display of compounds of plutonium but that samples of americium and neptunium will be sent to him. Burris also wrote that we have prepared a chart showing the colors of representative compounds and aqueous ions of the heavy elements, which should be ready for shipment about April 1.

I went down to campus for the Charter Day ceremonies. The speaker was Arthur L. Goodhart, Master of University College, Oxford University.

Later I made the rounds of the labs to talk with some of the students and fellows; I then did some reading.

Tuesday, March 18, 1952

I took care of a few phone calls and administrative matters before going down to campus to give the Chemistry 223 lecture.

Ken Pitzer told me about a graduate student, Lawrence L. Altman, from the University of Cincinnati whom he is accepting for graduate work. Altman seems to be interested in our type of work. When I got up to the hill, I asked Doral to prepare the usual letter to Altman, offering him employment at the laboratory on our student basis.

A group of science students from Klamath Falls High School in Oregon visited the lab this morning. Doral arranged for a couple of our graduate students to act as guides. [Tours of the lab, which are normally discouraged, are arranged through Jim Sheridan in Ernest Lawrence's office.]

Later I declined a March 11 invitation from Dr. Robert B. Fischer (Chemistry Department, Indiana University) to participate in a speaking tour in the fall of 1952 or spring of 1953 on behalf of the Midwest Cooperating Sections of the American Chemical Society. I explained that my schedule is so full that I find it impossible to add to it and carry on my teaching duties and research program.

A short note went to Joe Kaplan (UCLA), requesting a set of Samuel Barnett's reprints, which I could send on in support of the nomination of him for the Nobel Prize in physics. [Barnett was one of my professors at UCLA and did early research work at Cal Tech that gave evidence for electron spin.]

Wednesday, March 19, 1952

This morning I tried to clear my desk of various administrative duties since I will be leaving tomorrow morning.

I declined the annual invitation from Miss Frances V. Benner (Special Assistant, Office of the Executive Secretary, American Chemical Society) to engage in speaking tours for various local sections. Again, I pleaded a heavy schedule.

A few days ago I wrote Arthur I. Goldberg (The University of Buffalo) that the ACS Housing Bureau had given me a room at the Richford, rather than at my first choice, the Statler. Then, this morning I received a wire that the Bureau had found a room at the Statler for me; another note then went to Goldberg to explain that he can reach me at the Statler Hotel after all.

A note from Leo Yaffe arrived in today's mail. Yaffe wrote that he plans to arrive early in the evening of April 13 and will stay until April 19, saying that he changed the time according to my suggestion since he wouldn't want to miss Ghiorso and Thompson. I told Doral to show the letter to Al, Stan, Iz, and Earl and to send copies to Fidler and Everson.

I also received a letter from the office of John Francis Neylan, acknowledging my letter of March 17 and saying that a copy of the letter was sent to Neylan, who is on his annual vacation in Arizona until some time in April. I asked Doral to send copies to Kennedy, Wahl, and Segrè.

When I made my rounds of the labs, Stan showed me a copy of a letter he wrote to Art Jaffey at Argonne. Stan described the various experiments they (Stan, Al, Ken Hulet, and John Rasmussen) plan to do when they visit Argonne from April 28 to May 9 to use the Argonne pile. He added that he hopes some of the Argonne people (possibly John Huizenga, Larry Magnussen, Marty Studier) might be interested in working with them.

Thursday, March 20, 1952

I was picked up this morning about 6:30 a.m. by lab car and driven to the San Francisco airport in time to catch TWA Flight 36, which left at 8:30 a.m. The flight arrived in Chicago at 5:15 p.m. A lab car met me and drove me to the Guest House at Argonne. I made a few phone calls and ate dinner before retiring.

[In Berkeley, the research group met as usual. Present were Asaro, Bertelli, Biller, Carniglia, Carr, Clark, Conway, Cunningham, Dauben, Fischer, Gunn, Higgins, Hoff, Hollander, Jaffe, Kalkstein, Koch, Levy, Michel, Passell, Rasmussen, Ruben, Shudde, Slater, Templeton, and

Thompson.

First to report was Higgins, who described the helium ion bombardment of Pu^{239} . In the curium fraction from a citrate column they saw 2.3-hour Cm^{238} . The decay curve had an initial slope of 2.3 hours curving into a longer half-life. From their experiments they concluded that a 10 to 12-hour parent was growing a 12-hour daughter (Am^{239}). The work evoked some discussion. Higgins added that the experiment was done with the plan of placing some of the Cm^{238} in a photographic emulsion in the hope of finding 2-prong tracks from the decay sequences.

Shudde talked about his work with Dunlavey, in which they observed the electrons from U^{232} in a photographic emulsion. He showed the decay scheme suggested by the work, saying that Dunlavey observed some approximately 20 kev electrons and thought this might indicate a transition between a 55 kev excited state and the ground state. There was some discussion about the preparation of the U^{232} .

John Conway described his various attempts to increase the sensitivity of spectrographic detection, saying that the chief difficulty in increasing the observed intensity is the affect of the background radiation from the spark.

Dauben reported that a preliminary study of the oxybromides of lanthanum, cerium, praseodymium, and neodymium indicates they are similar in structure to LaOCl , that is, of the PbFCl type. She said they have also taken a number of pictures of holmium, yttrium, thulium, and lutetium compounds. These look similar to those of lanthanum but may have a different structure. In addition, a picture of the oxychloride of samarium was taken with a sample of only 5 to 10 micrograms.

Thompson talked about a sample of curium, now containing 75% Cm^{242} , 15% Cm^{243} , and 10% Cm^{244} . He described the history of the sample and said the analysis is very tentative because of the difficulty in running the sample on the mass spectrograph. However, from the data they calculated the half-life of Cm^{243} as roughly 35 years and that of Cm^{244} as roughly 20 years. The reason the half-life of Cm^{244} is shorter is that the Cm^{243} alpha decay is a hindered transition, typical of even-odd mass types. He reported that Fred Reynolds also found Cm^{245} in about 0.1% abundance, but they have seen no alpha particles from its decay. Preliminary estimates now give a value of about 10 to 15 barns for the neutron capture cross section of Cm^{242} . Thompson mentioned they plan to irradiate highly purified heavy curium isotopes in the Argonne pile to determine neutron fission cross sections.

* * * * *

Friday, March 21, 1952

In Chicago. I talked with a number of people at Argonne today, including Deputy Director Norman Hilberry, with whom I discussed the financial arrangements for my two-month visit beginning some time in May. Hilberry offered two possibilities: (1) no pay from the University of California but Argonne would pay me my present salary plus the expenses for the entire family and (2) two-thirds of my University of

California pay and consultantship fees from Argonne of around \$50 to \$100 per day with only my expenses being paid. I shall have to consider which proposition is the most favorable.

John Huizenga told me about his work on Pu^{243} . He said he saw two gamma rays in cascade following the beta particle, which may be more energetic than our value of 0.39 Mev. His value for the disintegration energy is probably greater than 0.6 Mev.

I made a point of speaking with James B. Niday, who is interested in a position in the Bay Area. Niday told me immediately that, with his experience, he expects a salary of \$6,000 a year or more. He has three children and helps support his parents. I described the MTA (Material Testing Accelerator) work, saying that this is done under a joint contract with Standard Oil and UCRL. I told Niday that the two subsidiaries of Standard Oil involved are California Research and Development Company at Livermore and the California Research Corporation of Richmond, California. The possible position we have in mind for him is with the Livermore group, whose chemistry group is headed by Kenneth Street. The other possible position, I said, is working with Stan Thompson, and Stan can tell him about this when he visits Argonne at the end of April.

I learned that Volume 17A of the PPR has been distributed, and so I looked up Hoylande Young to tell her that I had not received a copy. Hoylande said she would check on the whereabouts of my copy. We also talked about the progress of the other volumes of the Plutonium Project Record, such as Volume 14A.

On a less scientific matter I asked Don Stewart if he would look into the matter of a car for us during our visit in May and June, either to rent (which Don said he thought was probably not feasible) or to buy. I described our requirements and desires.

I was asked to write a letter of recommendation, addressed to Paul A. Dana (Personnel Officer, Argonne), for Robert H. Goeckermann. This I dictated to one of the secretaries, saying that Goeckermann, a former Met Lab employee, has been working at Hanford since he earned his Ph.D. at Berkeley. I wrote that Goeckermann's work at the Metallurgical Laboratory was outstanding, as it has been at Hanford. I said that he made a very good record at Berkeley, where his research work on the high energy fission of bismuth and neighboring elements was directed by Professor I. Perlman.

I left Chicago at 6:30 p.m. on American Flight 738, arrived in Buffalo at 10:06 p.m., and took a limo to the Hotel Statler.

Saturday, March 22, 1952

In Buffalo. This was a rather relaxing day, spent mostly going over various notes and papers. Arthur Goldberg contacted me, and I got together with the other participants of this evening's Round Table for a preliminary discussion of tonight's program, "Chemistry Looks at the Future." We were given an outline of the points to be covered: I. Industry and agriculture, II. Atomic Energy (what can be expected and

how soon), III. Biological developments.

The Round Table was aired from 7:30 until 8 p.m. The other participants were Edgar C. Britton (President of the American Chemical Society), Henry M. Woodburn (Chairman, Department of Chemistry, The University of Buffalo), and Carleton F. Scofield (Chairman, Psychology Department).

Sunday, March 23, 1952

In Buffalo. I registered for the 121st National American Chemical Society Meeting on the mezzanine of the Hotel Statler, where I saw and talked with a number of acquaintances.

Monday, March 24, 1952

In Buffalo. At 9 a.m. I spoke on "Masses of Translead Nuclides" (co-authored by S. G. Thompson and R. M. Diamond) at a session of the Division of Physical and Inorganic Chemistry in the Orchard Room of the Hotel Lafayette. In summary I said:

The amount and the accuracy of the data on the energy of radioactive decay associated with the nuclides above lead is now beginning to be such as to make feasible its use for the calculation of rather good values for the relative masses of these nuclides. One of the programs of this laboratory for the last several years has been the accumulation of new data, the establishment of methods for estimating the values when the data are missing, and the evaluation of all available data for this purpose.

The radioactive decay energy along a radioactive series and its collateral chains is summed so that the total decay energy for each nuclide in the series is known with respect to its position above the bottom of the series, that is, with respect to its terminal lead (or bismuth) isotope. This can be done for each of the four mass types, and these can then be related to each other through the experimentally determined neutron binding energies.

Relative masses have been calculated for all known nuclides above lead and these have been related to absolute measurements of the masses of several of the nuclides, they have been used to construct energy surfaces, and are being studied from the standpoint of determining the characteristics of nucleon binding in this region.

I illustrated the talk with seven slides. This session was presided over by John C. Bailer, and my talk was followed by a paper by Charles Coryell and Ronald A. Brightsen on "Evaluation of Shell Effects in the Nuclear Energy Surface."

I skipped the rest of the session in order to attend the 10 a.m. symposium on "Thermodynamics of Irreversible Systems" held in the Ballroom of the Hotel Lafayette. J. O. Hirschfelder presided, and papers were given by John G. Kirkwood and Bryce L. Crawford ("The Macroscopic Equations of Transport") and R. T. Cox ("The Statistical Method of Gibbs in Irreversible Thermodynamics"). This symposium continued after lunch.

At 6 p.m. I attended a dinner meeting of the Executive Committee of the Division of Physical and Inorganic Chemistry in the Hotel Statler. We discussed a number of items, including arrangements for various upcoming symposia and meetings.

The General Meeting was held at 8:30 p.m. in the Grand Ballroom of the Hotel Statler with President Edgar Britton presiding. My old friend and colleague, Harrison S. Brown, was presented with the ACS Award in Pure Chemistry. Among the other awards was the Scientific Apparatus Makers Award in Chemical Education presented to Joel H. Hildebrand. Edward U. Condon delivered the address of the evening and spoke on "Scientists and the Federal Government," covering such things as lack of funding for basic research--he pointed out that research receives about \$3,000,000 while the military receives about \$1,500,000,000 per year. Condon also discussed the loyalty investigations.

I saw and talked with many friends at the general mixer following this talk.

Tuesday, March 25, 1952

In Buffalo. This morning I attended the Howard Adler "Symposium on Recent Developments in Phosphorus Chemistry" in the Ballroom of the Hotel Lafayette for a while.

Later I had conversations with a number of people. Ron Brightsen approached me and asked if I would send him some decay energies for some nuclei with $Z > 40$, $N < 50$, such as Mo^{91} .

John Huizenga also talked with me again, this time about some thoughts he has had about the Pu^{241} - Am^{241} - U^{237} - Np^{237} decay cycle.

At 3 p.m. I stopped in at the open meeting of the Council Committee on Publications in Room 308 of the Statler Hotel. Helen (Mrs. Watson) Davis (Science Service) met with me at 3:30 p.m. and asked that I send them a list of the transuranium elements, a reprint of my article on alpha radioactivity, and a review of the discovery of the transuranium elements. I agreed to do so.

The Editorial Board of the Journal of the American Chemical Society had a dinner meeting in the Statler at 6 p.m., which I attended.

Wednesday, March 26, 1952

In Buffalo. At 9 a.m. I went to the symposium on "Structure and Properties of Surfaces," held in the Ballroom of the Hotel Lafayette and presided over by Paul H. Emmett. George W. Watt gave the introductory remarks, and John E. Willard spoke on "Applications of Radiotracers to the Study of Surfaces..." Other speakers were Dietrich E. Beischer; Allan T. Gwathmey and L. B. Johnson; and Floyd E. Bartell and J. T. Smith.

At 2 p.m. in the Orchard Room of the Hotel Lafayette I heard Harrison Brown present his acceptance speech for the ACS Award in Pure Chemistry. The talk was entitled "Chemical Aspects of the Origin and Evolution of Planets."

I then went to Room 308 of the Statler Hotel for the closed meeting of the ACS Council Committee on Publications (it began at 2 p.m.). Present were Ludwig F. Audrieth, Harrison, A. L. Marshall (chairman), William T. Miller, Jr., Howard S. Nutting, Arthur Rose (acting secretary), R. Norris Shreve, and I. James W. Perry and Evan J. Crane were present by invitation. The meeting was confined to an extended discussion of the problems of Chemical Abstracts, particularly long-range problems arising from the continual increase in the volume of chemical literature. Nutting presented a report on the possibilities of mechanical devices for expediting the preparation of abstracts and indices.

At 6:30 p.m. I presided at the dinner meeting of the Division of Physical and Inorganic Chemistry. This was held in the Lafayette Hotel, and Alan T. Waterman of the National Science Foundation spoke on "Present Status and Future Plans of the National Science Foundation."

Thursday, March 27, 1952

In Buffalo. Bob (Robert E.) Connick presided over the general session of the Division of Physical and Inorganic Chemistry at 9 this morning. I stayed to hear the second paper by Connick and Z. Z. Hugus on "Participation of Orbitals in Bonding in Uranium and the Transuranium Elements." I had to miss the other papers in the session because of a meeting of the ACS Committee Advisory to the Chemical Corps (Room 326, Hotel Statler). The program included such items as introductory remarks by H. F. Johnstone, a report of the committee on publicity and public relations by Sidney Kirkpatrick, a report relative to the search for new agents by Lt. Colonel Loyd E. Harris, etc.

I left the meeting in time to hear a paper by Thomas C. Hoering and Joseph W. Kennedy called "Oxygen Exchange between Sulfuric Acid and Water." Joe and I then had lunch and conversation together.

In the afternoon I went to hear a paper by James W. Cobble, William T. Smith, Jr., and G. E. Boyd, "Thermodynamic Properties of Technetium Compounds I." [Cobble is scheduled to come to Berkeley as a postdoctoral student.]

I left Buffalo at 5:10 p.m. to fly, via American Flight 773, to Columbus, Ohio. This has been quite a successful meeting with about 3000 people attending. The weather has been brisk (25 to 51°F) but no snow.

My flight arrived in Columbus about 7:45 p.m. James Bercaw met me and drove me to the Deshler-Wallick Hotel in downtown Columbus.

[In Berkeley. The research group held its usual meeting with the following people attending: Asaro, Carr, Carter, Clark, Dauben, Dunlavey, Fleming, Higgins, Hollander, Huffman, Hulet, Kalkstein, Koch, Levy, Michel, Nervik, Perlman, Rasmussen, Ruben, Slater, and Terwilliger.

Levy talked about the isomeric states of Bi^{210} , pointing out that it is not yet known which is the ground state; for purposes of discussion, he called the isomeric state that decays predominately with a 5-day beta particle RaE and the state that decays predominately by alpha emission

with a 10^6 year half-life Bi^{210} . He went on to say that, after Bi^{210} was formed by irradiation of Bi^{209} with neutrons, all the RaE was allowed to decay out and the remaining Bi^{210} was purified of polonium. The Bi^{210} was then allowed to stand about six months to allow Po^{210} to grow in. The Po^{210} was milked from the mixture, and he calculated the branching ratio of the bismuth isotope of beta particle + isomeric transition/alpha particle = $2.8 \times 10^{-3} \pm 30\%$. In the discussion Rasmussen asked if Bi^{210} has been looked for from the decay of RaD, and Levy said that he is doing that by milking the ores. However, if he finds it in the ores, one can't say definitely that it is the ground state.

Hyde reported on his study of Em^{221} , saying they bombarded Th^{232} with low energy protons. He used his closed system to collect the sample, which was pulse analyzed. Em^{212} was present as a contaminant. Hyde showed the pulse analyzer curve and pointed out that it seems impossible to produce Em^{221} in this way without making a lot of Em^{212} , which hides the Em^{221} alpha peak.

Higgins announced that the alpha half-life of Cm^{238} was erroneous in his previous report. They now calculate 41 ± 2 days as the alpha half-life.

Dunlavey reported that, in the observation of 5000 disintegrations from Th^{232} in their photo emulsions, 24.5% have conversion electrons of about 50 kev and about 30 kev, indicating that the gamma energy corresponding to the excited level is about 50 to 55 kev, while 75.5% have no conversion electrons. He showed the decay scheme, which checks with alpha ray spectrographic measurements.

Fleming described his measurement of the specific activity of U^{234} , from which he determined the half-life as $2.543 (\pm 0.012) \times 10^5$ years. He compared his value with others in the literature.

Slater said that he has been determining the cross section at various deuteron energies for the reaction $\text{U}^{238}(\text{d},\text{p})\text{U}^{239}$. He presented values for deuteron energies of 12.7 to 14.4 Mev in which he found the cross sections to increase from 132 mb to 279 mb. Slater said he believes the peak of the reaction yield to be quite near the value given for 14.4 Mev. The work is continuing.

* * * * *

Friday, March 28, 1952

In Columbus. James Bercaw picked me up at the Deshler-Wallick Hotel and drove me to the Ohio State University campus, where I spent much of the day meeting and talking with members of the staff of the Chemistry Department. Mrs. Kurbatov was most apologetic about not being able to entertain me because of the illness of her husband; I expressed my sympathy, adding that I hope he recovers soon. After dinner with some of the staff and Phi Lambda Upsilon people, I gave the first of two talks of the Third Annual Lecture Series of the Eta Chapter of Phi Lambda Upsilon on "Present Status of the Transuranium Elements." This was my usual talk on this subject, illustrated with about 25 slides.

Saturday, March 29, 1952

In Columbus. This morning's 10 a.m. lecture was entitled "Nuclear Thermodynamics of the Heaviest Elements." In the lecture I covered such points as accumulative decay energy, table of masses, neutron binding energies, and energy surfaces. The talk evoked much discussion and was well received.

I was scheduled to leave Columbus at 7:35 p.m., but fortunately I learned about TWA Flight 591, which was forced to land in Columbus rather than in Pittsburgh, and I was able to get on this flight. This arrived in Chicago at 3:15 p.m., allowing me to catch an earlier flight (3:55 p.m., United Flight 623) out of Chicago. Thus, I got into San Francisco at 9:50 p.m., about 2 1/2 hours earlier than I expected to arrive. Art Mitchell, the lab driver, picked me up and drove me home. I arrived about 11 p.m.

During the flight I made notes to remind me of some of things I must do when I return to my office. One thing is to talk with our local administration and decide which of Norman Hilberry's offers to accept for our visit there in May.

Sunday, March 30, 1952

I am most happy that I was able to catch the earlier flight yesterday because I was able to get a decent night's sleep in my own bed. The kids brought me up to date on their problems, activities, etc.

Monday, March 31, 1952

In the stack of mail I tackled this morning was a check for \$42.24 from Walter Yust (Encyclopaedia Britannica) for the article "Plutonium." Since Burris Cunningham spent so much time drafting the article, I endorsed the check and gave it to him. Yust also requested that I sign and return the copyright release form, which I did.

I had also received a March 28 letter from Donald Lane. Lane enclosed a copy of the reporter's transcript of the February 19th conference with the Patent Compensation Board. Among other things he said that the present status of our claim is that the Board is waiting for him to advise them if the suggested further negotiations with the Regents result in further modification of their position. He wrote that Roland Anderson advised him on March 26 that his office is still analyzing the patent applications to summarize the technical subject matter, and that he (Anderson) hopes to be able to get together with Lane in a few weeks to discuss acceptable fact stipulations which will simplify the subject matter for the Board's consideration.

Another draft of his biographical sketch, along with a March 20th cover letter, had arrived from John Voelker. I put the draft in my briefcase to study at home, but I noted some of Voelker's comments--a question about my age when I was in Chicago, remarks about adding personal touches, and a question about what I think of my work and its implications for the future. Voelker suggested that we wait for the pictures until we see what happens to the piece but he asked for a

picture of me for his den.

Other letters included a March 21 letter from Cyril Stanley Smith (Director, Institute for the Study of Metals, The University of Chicago), thanking me for my December 11 letter of recommendation for Dr. Allan Zalkin but explaining that they found it impossible to offer him a position. I routed the letter to Zalkin.

I also read a brief note of appreciation from Donald Jones (who met with Bill Jenkins and me on the evening of March 15 to discuss a television science show that his company is preparing for American Telephone and Telegraph Company). Jones said he hopes to talk with me more when the project begins to take more definite shape.

Carleton F. Scofield had written a nice note on March 24, thanking me for participating in the Round Table discussion on March 23.

I signed the response that John Rasmussen had drafted to the latest letter from Shizuo Fugiwara. Fugiwara recently sent me an advance copy of his article on the correlation of melting points of inorganic compounds, and John had gently questioned some of Fugiwara's equations and had offered some suggestions.

A March 25 letter had arrived from L. Leprince-Ringuet, thanking me for material I recently sent him and for arranging with Barkas for the irradiation of photographic emulsion. He went on to say that he was happy to have met Mrs. Seaborg (on our Nobel trip) and to say that he hopes to visit next spring if the question of visas can be resolved. Finally, he wrote that he is sending me a report of their work on the disintegration of a κ (kappa) meson.

The senior staff had its regular lunch meeting in my office at noon, and I related the various bits of information that I picked up on my trip. A frequent topic at these staff meetings is the progress of the design for Building 70, our new central chemistry building. Earl Hyde is following this.

Later I visited the various labs to check on the status of the students' research. I learned that Stephen Charles Carniglia has passed his prelims with the comment, "good originality but verbose." His committee consisted of Isadore Perlman, Henry Silsbee, Leo Brewer, William D. Gwinn, F. Campbell Williams, and Erman A. Pearson.

Tuesday, April 1, 1952

Before going to campus this morning, I wrote a brief note to Spof English (U.S. AEC, Washington), saying that I noticed in the last semi-annual report to Congress that I am still considered to be a member of the Advisory Committee on Chemistry of the Division of Research. Since the purpose of this assignment, I wrote, has been accomplished, I should like to resign from this formal connection. I added that I am always glad to discuss any problem of the Research Division with him informally.

I looked over documents from Harold Fidler that Donald Cooksey transmitted to me on March 26 about the visit of Leo Yaffe. Cooksey asked for evidence that the men who will talk with Yaffe read the documents. Since Iz, Earl, Burris, Al, and Stan had already read the documents, I asked Doral to return the papers to Cooksey.

After the Chemistry 223 class, I returned to the hill and looked over more mail.

I noted a March 26 invitation from Stan McCaffrey for my family and me to be guests at Pinecrest Lair of the Golden Bear again this summer. I called Stan and told him that we shall be spending a couple of months in Chicago and will have to skip Pinecrest this year.

An apologetic note arrived today from Alberto F. Thompson, who had learned from Hoylande Young that I did not receive my copy of Volume 17A of the Plutonium Project Record (Production and Preparation of U²³³: Survey Volume, edited by Glenn T. Seaborg and Leonard I. Katzin). Thompson wrote that this is being sent to me (it also arrived in today's mail) and that Volume 17B is now being bound and will be ready for distribution in about two weeks.

I worked for a while on my multitude of writing projects.

Wednesday, April 2, 1952

After the usual phone calls and miscellaneous administrative duties, I dictated a few letters.

I replied to the March 17 letter from Nils G. Sahlin, who withdrew his invitation for me to speak at this year's Svenskarnas Dag, Inc. since the committee learned that the governor of Varmland would be attending their ceremonies and presenting gifts. The committee decided that it was necessary to invite the governor of Minnesota to be this year's speaker and suggested that I speak next year. I wrote that I think this is a good solution to the problem of my tight schedule for I had serious doubts as to the feasibility of coming, noting that I accept few invitations to speak at occasions of a non-scientific nature. The question of my coming next year, I wrote, will probably have to be decided in relation to my schedule at that time. At present, I wrote, I have a tentative engagement for the week of June 21, 1953, which would make it impossible for me to come at a time corresponding to that which was chosen for this year's event.

A note went to James B. Plate (Prentice-Hall, Inc.) to acknowledge receipt of Sillen's Problems in Physical Chemistry. I wrote that I think this is an outstanding book and the best of its kind which has appeared to date.

I also wrote to comment on the periodic chart that Paul H. Nisley (A. D. Mackay, Inc.) sent me and said that I doubt that I like this form of the chart as much as his previous one. I went on to mention several points of confusion about his chart.

As a result of talking with James Cobble on my recent trip, I sent a memorandum to George Everson. I wrote, "As we have discussed in the past, we should like to have James W. Cobble replace W. A. Jenkins. A starting rate of \$525/month would seem reasonable. Dr. Cobble has indicated that he can come in late October or early November. It probably would be worthwhile to start the hiring procedure now so that it can be completed in ample time." [Bill has accepted a position with the E. I. du Pont Company and is about to leave for Wilmington in his Golden Goose. We are going to miss his inspirational golfing.]

Thursday, April 3, 1952

The following attended this month's open research group meeting: Asaro, Carniglia, Carr, Clark, Conway, Dauben, Dunlavey, Fleming, Higgins, Hoff, Hollander, Hyde, Kalkstein, Larsh, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Seaborg, Shudde, Templeton, Terwilliger, and Tyrén.

Fleming presented his final values for the half-lives of U^{234} (2.475×10^5 years $\pm 0.65\%$); U^{235} (7.13×10^8 years $\pm 1.9\%$); U^{236} (2.391×10^7 years $\pm 0.76\%$). He stated that these are overall errors and pointed out that the main error in the U^{235} determination was in the alpha-ray pulse analysis. Fleming went over some of the details of the determination.

Shudde talked about the study of Cm^{238} decay in photographic emulsions in which they looked for decay of double alpha events in order to verify the chain $Cm^{238} \rightarrow Pu^{234} \rightarrow U^{230}$. They found five such decays with the appropriate energies. Shudde said the half-life for Cm^{238} is 2.3 hours and the half-life for Pu^{234} is 8.5 hours; the plate was exposed for 43 hours. He pointed out the interesting observation that four of the double alpha events have an angle of $50^\circ \pm 1^\circ$ and the fifth, almost 100° .

Asaro said they have been interested in looking at a 4 Mev alpha emitter in the spectrograph to check the agreement with the energy determined in the pulse analyzer since they have no standards in this region. They exposed 4-hour Tb^{149} , prepared by John Rasmussen, for 12 hours. The value they obtained for Tb^{149} , using Ra^{226} as a standard, was 3.97 ± 0.02 Mev. There was quite a bit of discussion on the work. Asaro also talked about his work with Tyrén, in which they looked at U^{232} on the alpha-ray spectrograph. The sample was prepared by Slater and was not as large as they expected, but it was adequate. They saw two alpha groups separated by 58 ± 3 kev, in agreement with Dunlavey and Shudde's work. The spectrograph gave $26 \pm 3\%$, corresponding to the

decay to the upper energy state of the daughter; however, only 300 events were counted. There was considerable discussion, and Asaro said they plan a longer exposure. He also spoke briefly about Th^{232} and U^{234} , saying that Th^{232} has a 55 keV gamma ray and U^{234} has a 48 keV gamma ray.

Rasmussen spoke to the group about the first excited states of even-even nuclei.

* * * * *

After the Chemistry 223 class, I went up to the hill and found a teletype from Frederick T. Hobbs (Washington, D. C.) about Leo Yaffe's visit:

RE AEC 43/527 PROPOSED VISIT BY DR LEO YAFFE (CHALK RIVER) TO UCRL FOR CLASSIFIED DISCUSSIONS UNDER ISOTOPES AND EXTRACTION CHEMISTRY AREAS APPROVED BY COMMISSION TODAY, NECESSARY STATEMENT OF NO OBJECTION HAVING BEEN RECEIVED. REQUEST THAT DETAILED ARRANGEMENTS BE MADE WITH D. B. LANGMUIR, USAEC LIAISON OFFICER, CHALK RIVER, INFORMING THIS OFFICE FOR RECORD. ALSO REQUEST REPORT IN TRIPPLICATE, OF DISCUSSIONS INCLUDING BRIEF SUMMARY OF INFORMATION DISCLOSED AND CANADIAN INFORMATION RECEIVED WHICH WAS OF PARTICULAR INTEREST, BE SUBMITTED THIS OFFICE WITHIN THIRTY DAYS AFTER CONCLUSION OF VISIT.

I wrote a short note to Mrs. Watson Davis to send her the material on the transuranium elements, which I promised in Buffalo.

After working on some of my writing projects for a while, Iz and I got together with Don Cooksey and went out to Diablo Country Club for conversation and nine holes of golf (IP-52, DC-51, GTS-49).

Friday, April 4, 1952

I have received from Joe Katz some of the chapters of PPR, Volume 14A, which have to be checked because of the deletions that were made in order to declassify the material. I sent Chapter 8, "Oxidation States, Potentials, Equilibria, and Oxidation-Reduction Reactions of Plutonium," by Bob Connick down to him today to read; I asked him to send any corrections he wants to make to Joe Katz within a couple of weeks. Earl Hyde is reading Chapter 15, "Radiochemical Separation of the Actinide Elements," and I worked on Chapter 1, "Introduction."

I made my usual rounds of the labs to check on the research. I learned that my student Dick Glass passed his prelims yesterday, receiving the comment "decidedly one of the better examinations." His committee was composed of David H. Templeton (chairman), George E. Gibson, Donald S. Noyce, William F. Giauque, Arthur B. Pardee, and John H. Reynolds.

Saturday, April 5, 1952

The kids wanted a little attention from me today, but I still found time to work on some of my writing projects.

Sunday, April 6, 1952

I did some reading and writing and acted fatherly today.

Monday, April 7, 1952

This morning I returned Chapter 1, "Introduction," of PPR, Volume 14A to Joe Katz, saying that my corrections are in red and that I believe it is now ready to go to the printers. I asked Joe to give it one last reading before sending it to Oak Ridge and to call me if there are any important changes.

Last Friday I received a letter from Bill Libby, who announced that he would be in Berkeley today and tomorrow and would like to talk with me. We spent considerable time together today talking about a variety of subjects, including declassification problems.

In today's mail was an April 3 letter from T. F. Bewley (Vice-Chairman, Los Angeles Chapter, American Institute of Chemists). Bewley wrote about the honorary award I am to receive from AIC and asked if I can be prepared to give a 30-minute speech on any subject of my choosing at the AIC meeting, which will be set up in June or September. I telephone Fred (Frederick G.) Sawyer at Stanford and spoke with him about this, explaining that I shall not be available in June.

The senior staff held its regular brown-bag luncheon meeting in my office this noon.

Tuesday, April 8, 1952

Arne Holmberg wrote on March 31 to ask if he can have my manuscript for my Nobel Lecture before the end of April; this morning I dictated a short note, addressed to Holmberg, saying that I believe I can have the manuscript in his hands by that time.

Betsy J. Stover (one of our Ph.D. graduates now at the University of Utah) wrote on April 4 that she and Red (her husband, Clarence, and also a former student here) plan to be in Berkeley during the week of April 14 and would like to visit the lab, particularly to talk with Burris Cunningham and John Conway. I replied that we shall be happy to see them, and I advised her that our Security Division would like her to contact Idaho Operations requesting the visit. I also told Betsy that, in any event, we shall have passes waiting at the Gate Office if she lets us know when they expect to arrive.

After giving the Chemistry 223 lecture on campus and checking in the Department office, I returned to the hill, where I did some reading and writing.

Also, in today's mail was a not unexpected letter from Douglas C. Polhamus (Colonel, USAF), saying that he regrets to inform me that it will not be possible to permit Major Charles I. Browne to remain at our laboratory after his graduation in June. Browne will be sent to the Special Weapons Center, Albuquerque, New Mexico, as soon as he has completed his current studies. I routed the letter to Iz and to Charlie.

Wednesday, April 9, 1952

I just learned that the titles of Larry Magnusson's and John Huizenga's talks for the Gordon Conference have not been received so I telephoned Winston Manning this morning for the information. Larry's talk will be entitled "Decay Schemes of $4n + 1$ Series and Relation to Neutron Binding Energies" and John's will be "Branching Ratio between Neutron Emission and Fission as a Function of Gamma Excitation Energy on U^{238} ."

A note also went to Milton Burton to send him some material connected with the Division of Physical and Inorganic Chemistry. Burton had written me from London, where he was attending a meeting, about a couple of items he thought we had neglected to answer during our Executive Committee meeting in Buffalo, but I pointed out that I believe they have all been taken care of as he will see in his copy of the minutes.

A few weeks ago I asked Joe Katz to go over the draft of my revisions of the chapter on the actinide elements for Sneed's Comprehensive Inorganic Chemistry, which he agreed to do. Then, when I visited Chicago recently, we talked about the possibility of converting the document into a short monograph. The manuscript came back from Joe today; in the covering letter he wrote that he is enthusiastic about converting it into a monograph and will get underway when I furnish him with a copy of the manuscript.

After checking in at some of the labs and talking with some of the people, I worked on various writing projects.

Thursday, April 10, 1952

Present at the morning research group meeting was Carniglia, Carr, Carter, Feay, Fischer, Fleming, Glass, Higgins, Hollander, Hyde, Jaffe, Kalkstein, Levy, Michel, Passell, Perlman, Rasmussen, Reynolds, Robinson, Seaborg, Shudde, Skirvin, Slater, Street, and Templeton.

Fischer announced that there is now a gamma-ray scintillation counter in Room 330, Bldg. 50. She noted that it is poor for determining energies because of the high noise level and the fact that the linear amplifier cuts off the tops of peaks at a fairly low level but, she said, that it is satisfactory for following decay of a particular electromagnetic radiation.

Slater said that he now has a few more points on the $U^{238}(d,p)U^{239}$ excitation function, using the 60-inch cyclotron. These include five points from about 20.2 Mev (95 mb) to 17.6 Mev (115 mb).

I asked Street to tell about his experiments on uranium cross sections, and he said that Bill Crane, who is working with him, has been working on the $d,p2n$ cross section of U^{238} . He noted that the U^{237} is produced from two reactions: $U^{238}(d,p2n)U^{237}$ and $U^{238}(d,2pn)Pa^{237}$, after which Pa^{237} decays by beta emission to U^{237} . Street said the cross sections for the production of U^{237} rise from their starting point of about 10 Mev to a maximum short of 40 Mev and then level off. The cross section is then almost level at about 250 mb.

There was considerable discussion about the cross sections for these types of reactions, and I suggested that Street have one of his fellows summarize the data. Perlman added that they should look at thorium, and Street remarked that actinium chemistry is more difficult and fast chemistry would be necessary. I pointed out that I believe Meinke has published a method for a rapid separation of protactinium from thorium.

Reynolds concluded the meeting by giving a rather lengthy report on the present status of the work on the low energy beta particle spectrometer, which is of electrostatic design.

* * * * *

I stopped in and talked with Miss Kittredge and some of my colleagues in the Department of Chemistry office before giving the Chemistry 223 lecture. Later on the hill I tackled my writing projects, which I want to complete before the family and I leave for Chicago the beginning of May.

Iz and I also worked out a midterm for Chemistry 223, which will be given next Tuesday.

Friday, April 11, 1952

In between phone calls I answered some of the mail that has been sitting on my desk.

I completed and mailed a loyalty evaluation form for Robert H. Goeckermann, who has applied for a position at North American Aviation, Inc. The form included the questions: List the organizations with which he has been affiliated to your knowledge, Do you consider him reliable, Has he ever shown any subversive or Un-American tendencies.

While I was in Buffalo attending the ACS meeting, Waldo Cohn, whom I have known since my graduate school days in Berkeley, approached me and described his present "loyalty" problem. Cohn, who works as a biochemist at Oak Ridge, said he has been invited to attend the Second International Congress of Biochemistry in Paris this coming July to present a paper on the work of his group in the field of nucleic acid structure. The laboratory directors and the AEC program directors, both in the local and the Washington offices, generally agreed he should be sent by Oak Ridge to attend the meeting and then to visit laboratories in England, Scandinavia, and France. At the time we talked Waldo was most concerned that he was going to be turned down for trip approval and possibly a passport in view of the length of time that had elapsed since he had made the formal request. I told Waldo then that I would attempt to help in any way he asked. Wednesday I received a note from him, saying that things developed very much as he indicated they might and, on the advice of Shields Warren, was asking me and a few others to write to S. R. Sapirie (Oak Ridge Operations Office) about his loyalty. In an enclosed statement of the problem Cohn wrote:

...To this nebulous but most effective bar to what I consider my rights as a citizen and my value as a reference to my friends and associates, Dr. Shields Warren has added essentially this: In the

present situation, which derives from the present international tension, the appearance that I was once (1938-39) musically and scientifically associated with Martin Kamen and have since corresponded with him and seen him occasionally (principally at scientific meetings), added to the opinion, held in some quarters, that Kamen is a sort of latter-day Typhoid Mary, leads to a fear on the part of certain AEC officials that my record is not one which would stand scrutiny by an antagonistic Congressman or Senator, bent on embarrassing the AEC. Thus the negative decision....

This morning I wrote an appropriate letter to Sapirie, describing my relationship to Cohn and saying that I believe his loyalty to the United States is beyond question. I wrote, "I have not seen any evidence that would indicate to me that he is or ever was a poor security risk, and I feel that I have been sufficiently close to him that I could have detected any undesirable traits in this connection." In my covering letter to Waldo, I listed the governmental positions I have held for use in establishing my credibility.

Yesterday a short note arrived from Leo Yaffe, saying that he will arrive early Sunday evening and will be staying at the Durant Hotel. I dictated a response to be left at the Durant to give him my home telephone number and to say that I shall pick him up at the Durant Hotel to drive him to the lab between 9:00 and 9:30 a.m. Monday morning unless I hear from him to the contrary.

A few days ago an invitation arrived for me and Ed McMillan from Ralph E. Dyar to be his guests at the Books and Authors luncheon at the St. Francis Hotel on April 15. Dyar will be on the program and discuss his book, News for an Empire. I sent a copy of the invitation over to McMillan. After determining that McMillan did not want to go, I wrote Dyar and explained that neither of us will be able to attend.

I also declined an April 3 invitation from Frank L. Lambert (Chairman-Elect, Southern California Section, Pacific Southwest Association of Chemistry Teachers, Occidental College) to address their group in Los Angeles on May 23. I explained that I shall be in Chicago then but perhaps I can speak some other time; for example, I have a scheduled trip to Los Angeles in late September or early October.

I prepared, with Earl's assistance, a lengthy response to a request some time ago from J. O. Maloney (Department of Chemical Engineering, University of Kansas and a wartime Metallurgical Laboratory colleague) about the teaching of nuclear science at the University of California. This was mailed today.

Since I am going to Los Angeles next weekend for the Science Fair judging, and, after the conversations I have had recently with Segrè and Joe Kennedy, I decided to try to talk with Edwin Pauley (Board of Regents), particularly since I have been unable to talk with John Francis Neylan who is in Arizona recovering from a case of shingles. I sent him a short note, explaining that I shall be in Los Angeles on April 18 and would like the opportunity to talk with him in his office that afternoon. I asked that he let me know whether he would be available for

a short conference at that time.

I went through some of the labs to check on the research and then again worked on my writing.

Saturday, April 12, 1952

Bill Rice has completed the drawings for the addition to our house; Helen and I have yet to sign the contract for its construction, but we hope to do this soon for we would like the addition ready for occupancy when we return from Chicago.

My parents arrived from South Gate a few days ago to spend the Easter holiday with our family. This has been particularly helpful to Helen since Lynne and Dave have come down with cases of chicken pox. Dave is extremely miserable because he also has a case of giant hives, and my mother has been spending quite a bit of time rocking him.

Sunday, April 13, 1952 - Easter Sunday

Dave and Lynne are still miserable with the chicken pox, but the Easter baskets with candy and gifts helped divert their attention.

My day, other than for family duties, was again taken up with writing projects.

Monday, April 14, 1952

This morning I picked up Leo Yaffe at the Durant Hotel and drove him to the hill for conversations with me and many of our staff members.

Leo Yaffe



I skipped most correspondence today because of Yaffe's visit, but I did make a decision on the question of time for a symposium at the fall ACS meeting, which Miss Frances Ritzinger (Secretary to Joe Kennedy) posed. The Division of Physical and Inorganic Chemistry generates much correspondence, but fortunately Joe Kennedy has to handle most of the paper work now since he is the Secretary-Treasurer.

Ed Pauley telephoned me from Los Angeles and said that he would be very happy to talk with me on Friday; however, he would prefer a morning appointment. When I told him my schedule, he offered to pick me up at the airport. [Later in today's mail, a letter arrived stating that the judging for the Science Fair had been cut to just one day, Friday afternoon; this made a morning appointment with Pauley essential.]

I read a letter that came in Saturday's mail from Lars Melander, who thanked me for the letter about the Gordon Research Conference this coming June. He explained that travelling expenses will prevent them from coming. Lars also thanked me for the reprints they receive from our group. He went on to say that he is spending most of his time at the Nobel Institute on problems of organic nuclear chemistry and added that Forsling has a rather interesting interpretation of the results by Huffman and Lilly on the complex formation of hafnium and zirconium in $\text{HCl} + \text{HF}$. Forsling will send his paper when it is ready. Melander also stated that they were glad to have me there last December and he is sending a small reprint (in Swedish) dealing with the Nobel Prize in Chemistry 1951, written for their "Svensk Kemisk Tidskrift." I told Doral to call McMillan and read that part of the letter to him.

During my rounds of the labs, I learned that the men are beginning the extraction of americium from a plutonium cow. This operation is taking place in the Annex of Bldg. 5.

Betsy and Red Stover, who are visiting the Bay Area, stopped in to greet me. Betsy is particularly interested in talking with John Conway and Burris Cunningham on this visit.

Yaffe came to our regular Monday noon senior staff meeting. In addition to conducting some business, the meeting was also a bit of a social hour.

Dr. Harry Brynielsson, whom I met on my first trip to Sweden in 1949, is visiting the Bay Area with his wife. I met and talked with them this afternoon and learned that they also plan to be in Berkeley tomorrow.

Later Helen and I got together with Bill Rice and Hans Rodde to sign the contract for the construction of our addition. Although there are still a few unsatisfactory things about the original house, we are generally very pleased with Rodde's workmanship and want him to build the addition.

Tuesday, April 15, 1952

I signed a letter that Earl prepared in response to a request from Stuart L. Adelman (South Orange, New Jersey), who asked if he could obtain microcurie quantities of cobalt-62 from the Berkeley cyclotron for "an experiment dealing with the higher isotopic forms of cobalt, including cobalt-62." The response pointed out that I am not directly connected with the operation of the cyclotron and referred him to Dr. Joseph G. Hamilton. It read: "I might state in advance, however, that the experiment you contemplate with cobalt-62 is quite impossible because of the shortness of the half-lives of the isomeric states. Furthermore, it will probably be necessary for you to submit evidence of your experience with radioactivity and its safe handling because of the health hazards involved." [Adelman has written to me on previous occasions.]

A short note went to Waldo Cohn to tell him I overlooked one governmental position that he should perhaps add to the list I gave him in the letter of April 11: Member of the 10-7 Panel (AFOAT-1) to the Committee on Atomic Energy of the Research and Development Board, U. S.

Department of Defense.

I wrote a covering letter to The Svedberg (Uppsala) for the autographed photograph he requested. I asked if he would send me one of himself. In addition, I said that we shall be sorry to see Helge Tyrén leave although we understand that their cyclotron is now working.

Noting that our visitor, Leo Yaffe, is safe in the hands of Stan and Al, I went down to campus and gave a midterm to the Chemistry 223 class.

CHEMISTRY 223

Midterm Examination
April 15, 1952

- 25
1. A cyclotron accelerates deuterons which have an energy of 16 Mev and a range of 200 mg of aluminum per cm^2 . What is the range in air of the carbon ions which would be accelerated in the same cyclotron without any change in its fundamental characteristics?
 - 25
 2. Calculate the mass difference between Po^{220} and Em^{220} through the use of the data in the Table of Isotopes and estimations from the alpha systematics.
 - 20
 3. Compare the ratio of π^- to π^+ production from the bombardment of deuterium with very high energy (~150 Mev) neutrons and also with protons. Show reasoning.
 - 0
 4. The nuclide ${}_{23}\text{V}^{50}$, which is not shown in the Table of Isotopes, would be expected to decay by both negatron and positron emission.
 - 10
 - (a) Sketch the Bohr-Wheeler parabola(s) for $A = 50$ indicating the decay of ${}_{23}\text{V}^{50}$.
 - (b) Assuming that the β^+ transition to the ground state is allowed (which is actually not true), what can you say about the corresponding β^- transition? What would be the spin (I) and parity of ${}_{23}\text{V}^{50}$ in this case?
 - (c) Using the Bohr-Wheeler considerations, the fifth power law (which is actually a poor approximation), and the assumption of part (b); calculate the expected half-life and β^-/β^+ branching ratio for ${}_{23}\text{V}^{50}$.

Later, in the afternoon, Al Ghiorso and I took Leo Yaffe out to Mira Vista, where we played the last nine holes of the course (AG-44, LY-63, GTS-50).

Wednesday, April 16, 1952

Iz and I looked over the Chemistry 223 midterm, which Dick Glass had graded. Stephen Carniglia obtained the highest score (71).

J. Guéron wrote from Paris on April 7 about a friend of his, Professor M. Prettre, who will be visiting the United States and Canada under the auspices of the O.E.E.C. to make a study of applied research. Guéron said that he has taken the liberty of advising Prettre to call me when he is in Berkeley, which should be from May 2 until May 5. Guéron asked me to thank Mrs. Seaborg for her note and said that it was a pleasure to have us in Paris. I wrote Guéron today and said that it would be better for Prettre to call me on May 2 because I shall be leaving town on May 5. I added that I shall be glad to show him some of our laboratories and introduce him to a number of colleagues. Finally, I thanked him again for the fine time which he and Mrs. Guéron showed us during our visit in Paris.

I answered an April 11 letter from Jerry Howland (Brookhaven), who questioned me about the enrollment limitation for the Gordon Conference. He said that a couple of girls at their laboratory, Elizabeth D. Wilson and Joan P. Welker, have had their hearts set on going and are being scared off after learning the limit is 100. I wrote Howland that the attendees will be chosen by Parks and, therefore, Elizabeth Wilson and Joan Welker should get their applications in as soon as possible. I added that I believe their work sounds like it is worth a brief report, and we will arrange this if they should be able to attend. I added that I hope he will find it possible to attend. I told Doris to send a copy of Howland's letter and a carbon of mine to Coryell.

Much of my afternoon was taken up with my writing projects. I am keeping the office staff quite busy with the typing. Iz and I have found that his secretary, Mildred J. Davis, is an extremely fine typist; we probably will have her type the "Table of Isotopes" when the revision is completed later this year.

At home Helen is quite busy coping with the chicken pox and getting ready to move our family to Chicago for two months.

Thursday, April 17, 1952

The research group meeting this morning was attended by Asaro, Bertelli, Carr, D. Conway, Dauben, Dunlavey, Gunn, Hoff, Hollander, Huffman, Hulet, Hyde, Jaffe, Kalkstein, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Seaborg, Shudde, Slater, Templeton.

Asaro gave a lengthy report on alpha spectrometer measurements and energy levels in several heavy nuclei. He reviewed recent work reported in the literature, which he attempted to reconcile with Berkeley work. The subject evoked much discussion.

Shudde corrected his previous report on U^{236} , saying that the previously reported conversion electrons in 32% abundance corresponding to a 45 kev gamma ray should be corrected to electrons in 27% abundance corresponding to a 50 kev gamma ray. He also said they investigated U^{232} and found conversion electrons in 30% abundance corresponding to a 55-60 kev gamma ray. Shudde also talked about work with Pu^{236} , but they plan to recheck the experiment. I added that the sample, which came from Gary Higgins, probably has Pu^{238} contamination.

Slater mentioned that he is still running an excitation function for the reaction $U^{238}(d,p)U^{239}$, but the curves of his data (60-inch cyclotron) taken with a 0.5 mil foil and a 1 mil foil do not coincide. He was offered several suggestions.

* * * * *

The midterms were returned to the members of the Chemistry 223 class today. After checking in the Chemistry Department office and talking with some of my colleagues, I went up to the hill, where I again worked on my manuscripts, etc.

Friday, April 18, 1952

Art Mitchell picked me up at 6:00 a.m. and drove me to the San Francisco airport in time to catch Western Flight 301 at 8 a.m. I arrived in Los Angeles at 9:45 a.m.

Edwin W. Pauley met me and drove me to his office (717 North Highland Avenue, Los Angeles), where we began our meeting at about 10:45 a.m. I mentioned our inability to get in touch with John Francis Neylan and, since I was in Los Angeles as a judge for the Science Fair, I thought I should take the opportunity to talk with him about a difficulty that has arisen in our patent compensation matter. I began by bringing him up to date on the entire patent matter, recounting a description of the inventions and the time of their occurrence, unsuccessful attempts during the war to get a three-cornered agreement between the Government, the inventors, and the Regents, the final signing of the patent applications by the inventors in the fall of 1945 with the accompanying agreement eng-30, the Atomic Energy Act of 1946 that took away such rights, the final creation of the Patent Compensation Board to compensate for such occurrences and their invitation of a claim from us, the submission of our claim (through our attorney Mr. Lane) of January 1950, the Regents' letter of November 17, 1950 claiming sole ownership, our negotiations with Mr. Neylan and the Committee on Atomic Energy Contracts of the University of California Board of Regents, culminating in Neylan's summarizing letter to us of October 8, 1951 and in Underhill's letter of October 8, 1951 to the Patent Compensation Board modifying the Regents' stand.

We then discussed the various ways of defining the Regents' claimed interest. Pauley seemed quite willing to cooperate and seemed to understand the Patent Board's position. I brought up the suggestion Mr. I. Harter had passed on to us through Mr. Lane that the Regents be offered a non-assignable, non-exclusive, royal-free license to use the inventions. I had the impression that this seemed reasonable to Pauley

and that he wanted to help resolve the difficulty that was blocking us and felt that this should be possible. At the end of the conference he asked permission to call Brodie E. Ahlport, another member of the Regents Committee on Atomic Energy Contracts and an attorney, which he did. Ahlport suggested that I come over to see him, which I did when my conference with Pauley concluded (between 11:15 and 11:30 a.m.)

I went to Ahlport's office (5657 Wilshire Boulevard, Los Angeles) and began my discussion with him some time between 11:30 and 11:45 a.m. Again I described the situation and reviewed the patent matter in essentially the same way as I had done for Mr. Pauley. Ahlport said that he understood completely what was bothering the Patent Compensation Board and that it was clear to him that they were and should be interested in the extent of the Regents' claimed interest. He used, as an example, the possibility that the Patent Compensation Board might find, after they had settled our claim, that it represented only 1% of the ownership of the inventions. Ahlport agreed that it should be possible to define the Regents' interest in a way that would further the claim of the inventors and make possible the resolution of the difficulty before the Patent Compensation Board. He spent some time trying to figure out a method of doing this. I mentioned Mr. Harter's suggestion of a non-assignable, non-exclusive, royalty-free license for the Regents.

Ahlport thought, and he gave this as a "horseback" opinion that he might be forced to modify upon more careful consideration, that he had a better idea as follows: The Regents should assign the entire rights to the inventors as an internal University matter that would not particularly concern the Patent Compensation Board; in turn the inventors, in view of the fact that the University has a public trust and must protect its interest, would arrange to give the Regents a non-assignable, non-exclusive, royalty-free license to use the inventions. The inventors would agree that the administration of the research portion of the funds would be joint between the inventors and the Regents. Then the inventors could confront the Patent Board with a clear, complete ownership of the inventions. With respect to the research funds, it would be understood that the funds to be assigned to Washington University would be under joint ownership of the inventors and the university administration there. I think this would make very little difference with respect to the administration of the research funds compared with the original plan. I told Ahlport that, from my point of view, this would be a good plan but that I was not so sure about Neylan nor could I be sure that the other inventors would understand or agree to the matter of the administration of the research funds.

Ahlport said that he would write to Neylan to explain the plan this afternoon, and I should wait to hear from Neylan for the next step. Ahlport also said that the Regents will meet in Davis on Friday, April 25, and he hopes the Committee on Atomic Energy Contracts can meet for consideration of his plan at that time. I pointed out that I am leaving on May 5 for a two-months' stay in Chicago, and he added that he is hopeful that the matter can be cleared up before then. Ahlport then telephoned Pauley, who is apparently in agreement with the plan. I gave Ahlport Lane's name and address and Harter's name. Both conferences were extremely friendly, and both men said they thought the matter of the Regents' interest can be satisfactorily defined or resolved in some such

way. My conference with Ahlport ended a few minutes before 1 p.m.

At 2 p.m. I went to the Los Angeles County Museum in Exposition Park and met with other judges of the first Southern California Science Fair. Included among the judges were such people as Carl D. Anderson, Edwin M. McMillan, Ira S. Bowen (Director of Mount Wilson and Palomar Observatories), Robert F. Bacher, Philip A. Munz (Director of the Rancho Santa Ana Botanic Garden), R. F. Faulk (California Research Corporation), John K. Northrup (Northrup Aircraft, Inc.), Frank Capra of the motion picture industry, and Robert D. Vold (Chairman of the University of Southern California Chemistry Department). A "team" plan of judging was set up, and the judging took place between 2 and 4 p.m. It was a full, interesting afternoon. At 7 p.m., after dinner and some preparations, there was a television broadcast by KTTV from the Museum exhibition area, involving exhibitors, teachers, judges, etc. The four top awards were made to the winners who will go to the National Science Fair in Washington, D. C. (This is operated by Science Service, directed by Watson Davis.) The reception that followed was scheduled to last until 10 p.m., but I left relatively early to spend the night with my parents. (They returned to South Gate earlier this week.) The exhibits are scheduled to remain at the Museum through Sunday.

Saturday, April 19, 1952

In South Gate. My parents, particularly my mother, was delighted to have me spend at least part of my 40th birthday with them. I brought them up to date on the family's activities before catching the noon plane (United Flight 659) to San Francisco.

My flight arrived in San Francisco at 1:40 p.m., and the lab driver picked me up and drove me to Lafayette. There my birthday was celebrated with the usual festivities.

Sunday, April 20, 1952

I worked on some of my writing projects and helped entertain the kids so that Helen could have some extra time for preparations for our trip. Our departure time is somewhat in question because of our unexpected bout with chicken pox.

Monday, April 21, 1952

One of the first things I did this morning was to give a final check to the manuscript for my Nobel Lecture and then mail it to Dr. Arne Holmberg in Sweden.

Included in the mail awaiting me was a letter from T. Fraser Young (The University of Chicago), requesting that I try to attend their physical chemistry seminars and possibly speak at one of them when I come to Chicago. I replied that I shall try to attend the seminar a few times and to speak at one of them, adding that I remember with pleasure attending a few and speaking at one of them when I was in Chicago during the war.

Helen and I have had several conversations with a desperate W. A.

Lecaro, the purchaser of our Ellsworth Street house, about his financial situation. We finally agreed to lower the payments for a short time (his wife is about to have a baby). Today I wrote an explanatory note to the Collection Department, Berkeley Branch, Bank of America. I stated, "He will pay \$150 per month rather than \$200 per month for the five months beginning in June, that is for the months of June, July, August, September, and October; the payments will then revert to \$200 beginning with the November payment. During this period of five months the interest rate on the whole loan will be 6% per annum rather than 5%, and this should be included in the \$150 payments with the remainder to apply on the principal. The interest rate will revert to 5% at the end of the five-month period."

In today's mail was a letter from Don Stewart (Argonne) giving me information about cars for our upcoming visit. Don stated that he learned that it is difficult to find a car to rent (the rate is \$80-\$85 per month). He then supplied information about buying Plymouths and Chevrolets. For example, he wrote that one can buy a two-door Plymouth "Cambridge" Club Coupe, delivered with heater and including 2% Illinois Sales Tax from Essermann Motor Sales, Inc. for \$1780. Don included some prices from other dealers. I shall take the letter home to discuss with Helen.

Also in the mail that arrived today was a letter from John Francis Neylan. Neylan said that he found my letter of March 17 upon his return to his office and that my office advised him that I was in Los Angeles. He suggested I call him on my return so that he can arrange a meeting of the Committee. Neylan then wrote, "As presently advised, I find it impossible to find anything of substance in the objections of the Board. In fact, the recitals confirm a suspicion which I have had and which I voiced to you, - that notwithstanding anything The Regents would or could do you were going to have a most difficult time."

After our lunchtime staff meeting at which we talked about a variety of topics, I got together with Emilio to tell him about my meetings in Los Angeles with Pauley and Ahlport and the letter I received today from Neylan. We discussed some strategy and decided that I shall have to call Neylan.

I made a tour of some of the labs and then worked on my writing projects.

Tuesday, April 22, 1952

I took care of a number of phone calls and other administrative duties before going to campus for the Chemistry 223 lecture. While on campus I also spoke with some of my colleagues in the Department.

Back on the hill I wrote a note to James R. Bercaw (Phi Lambda Upsilon, Ohio State) to thank him for his complimentary letter of April 9 about my talks in Columbus and the nice letter and check for my hotel expenses from D. W. Magee. I also wrote, "I wonder whether you could send me some reprints of my lecture on 'Nuclear Thermodynamics of the Heaviest Elements' since we would find it useful to have a few copies here."

Doral has been collecting notes and slides of my speeches to ship to Chicago for my use there. This has been a rather big job.

The rest of the afternoon was spent in my usual manner of reading, writing, and talking with the students and scientists.

At home we celebrated young David's third birthday with cake and gifts.

Wednesday, April 23, 1952

Earl prepared a letter of recommendation for me to sign for Dean C. Dunlavey, who is applying for admission to the UC School of Law. The letter, addressed to Dean William L. Prosser, read in part, "Dunlavey has a fine personality and has a spirit of friendliness which enabled him to fit smoothly with his fellow students...I am not entirely clear as to his aims and purposes in entering law school and I have not explored this in detail with him. If I know of any fault in connection with Mr. Dunlavey, it is perhaps a slight indecisiveness as to his plans and ultimate goals once he completes his education...I am reluctant to see a man of his scientific ability lost to research at a time when there is such a serious shortage of scientists."

About 10:30 a.m. I telephoned John Francis Neylan in order to discuss with him the general question of the Patent Compensation Board's reaction to the Regents' letter of October 8, 1951. Neylan reiterated the belief he expressed to me in his letter of April 18, 1952, that the Patent Board was asking for something that was not necessary. He said he thought one of the aims of the Patent Compensation Board was to get the University to disclaim all of its rights, but I told him that I felt that this was definitely not the case. He then asked if I had any suggestions as to how to define the Regents' interest, and I mentioned the non-exclusive, non-assignable, royalty-free license mentioned in Lane's letter of February 20, 1952. Neylan's reaction was very definitely against this suggestion; he claimed that this would be giving away the University's rights or essentially saying that it has none. I told Neylan that the other inventors' patience would be extended by such a negative reaction and there would certainly arise doubts as to his intentions. Neylan interpreted this as a threat that the Regents should accept this suggestion or else; I reiterated that Harter's suggestion was only a suggestion and that the main point was to define the Regents' interest.

Neylan appeared to be quite aroused at this point and suggested that he had gone as far as he could and that he would probably withdraw personally at this point since he could not give away the University's rights with a clear conscience and this appeared to be called for. I reiterated that this was not in my opinion being asked for and that the central point was a definition of the Regents' "interest." Neylan said he thought that a careful recital by the inventors of the inventions for the Patent Compensation Board, together with the absence of any adverse testimony from the University, should be enough to enable the Patent Board to determine the amount of any award or compensation. Neylan then said that he did not want to have the excellent relationship between him and me to come to a breaking point over a matter like this because of his respect for me. He said that he will propose at the meeting of the Board

of Regents on Friday (April 25) that he has gone as far as he can and that someone else should be appointed to carry on.

Since no mention of a letter from Ahlport had been made up to this point, I asked if he had heard from Ahlport on this matter. He said that he had not found any such letter in his correspondence, and so I told him that I had seen Ahlport and Pauley on my trip to Los Angeles (I had previously described my reason for the trip) and that I had the impression that they felt that the Regents' interest could be defined. By this time in the conversation Neylan seemed to agree that the Regents' interest could somehow be defined but that it would have to be a substantive definition and not just a write-off of their interests. He repeated that he doubted that he could carry on amicably and that he would ask for the appointment of someone else for this purpose. I then told Neylan that I am leaving for Chicago on May 5th and that the inventors are anxious to carry this on as fast as possible. Neylan said that he will try to arrange for a meeting with the new representative or representatives of the Regents for this coming weekend. He will get in touch with me Saturday in order to give me the details. This conversation lasted until about 11 a.m.

In today's mail was a copy of a memorandum from W. B. Reynolds to R. P. Connell, explaining that arrangements have been made for me to work at Argonne for approximately two months and that the Radiation Laboratory will be reimbursed for my salary and the travel expenses of me and my family. Reynolds asked Connell to make out travel pre-approval for the trip, which will begin about the first part of May. Reynolds requested to have me put on the Laboratory pay roll full time on May 1.

I also received an invitation from John E. Christian of Purdue University to speak at the Purdue Chapter of Sigma Xi some time in the next school year, starting next October and continuing through May of 1953.

M. L. Rushmore (Merck & Co., Inc.) sent me a copy of their Periodic Chart, which he spoke to me about in Buffalo, and asked for criticism or suggestions.

Thursday, April 24, 1952

The research group meeting this morning was attended by Clark, D. Conway, Dauben, Dunlavy, Fleming, Higgins, Hollander, Hyde, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Seaborg, Shudde, and Templeton.

My undergraduate student, Dwight Conway, spoke first on some work they have done with the 4π proportional counter. He described an experiment in which they determined that the 4π counter is good for absolute counting of beta emitters to at least 4% accuracy. With samples of At^{211} , they determined the alpha decay branching ratio using the pulse analyzer as varying from 41.0 to 42.0%. Then, assuming that the percent that goes by K-capture as 58.0%, they determined the K-capture counting efficiencies in the 4π counter. Conway said the six values they obtained varied quite a bit, seemingly dependent upon the amount of gold evaporated on the sample. In the discussion John Rasmussen said they did

not expect to find the counting efficiency so sensitive to the thickness of the gold film and they intend to check on backscattering.

Dauben reported that they found a sample of Am_2O_3 , prepared by Cunningham, to be hexagonal, the so-called A form. This is the first time this form has been prepared, and it has the following parameters:

$a = 3.817 \pm 0.005 \text{ \AA}$; $c = 5.971 \pm 0.010 \text{ \AA}$;

and for Pu_2O_3 , $a = 3.840 \text{ \AA}$; $c = 5.957 \text{ \AA}$.

Dauben said the values for Pu_2O_3 are quite different from those predicted by Zachariasen, indicating the sample we have contains some Pu^{+4} .

Rasmussen announced that he and Dunlavey are preparing samples of odd neutron isotopes of light elements to put in the Argonne pile to study n, α reactions. They are preparing samples of Mg^{25} , Si^{29} , S^{33} , Cr^{53} , Zr^{91} (51 n), Nd^{143} (83 n). They will have two sets of samples, one to be surrounded by cadmium to cut out the thermal neutrons. In response to a question from Templeton, Rasmussen said they can tell the alpha particles from the protons in the photographic plate since an alpha particle and a proton of the same range should have different grain density, and Dunlavey said they plan to use a plate that is proton insensitive. Perlman asked about trying some of the elements in the heavy region, and I explained that most of the elements in the heavier region are difficult cases since one would want an isotope with an odd number of neutrons but which doesn't alpha decay and capture neutrons, as in the case of Am^{241} . U^{235} was suggested as a possibility.

Passell reported they have run the beta spectrum of Np^{239} but they have yet to correlate the data with the x-ray and low energy gamma-ray data of Browne. They also plan to run Np^{238} . This evoked some discussion about decay energies, and I suggested that someone should work out the decay scheme of Pa^{234} .

* * * * *

I spent some time on campus before going up to the hill. There I took care of some phone calls and other administrative matters.

Earl Hyde prepared a statement for me to sign about Dwight C. Conway, who is applying for admission to The University of Chicago. The letter, addressed to Miss Valerie C. Wickhem (Director of Admissions), read in part: "Mr. Conway was accepted as an undergraduate member of our predominantly graduate school research group in nuclear chemistry here at the Radiation Laboratory of the University of California in order to get some elementary training in research techniques in this field. This in itself is evidence that we had a high regard for his potentialities as a scholar and as a research scientist..."

Friday, April 25, 1952

Don Stewart has been doing an extremely diligent job locating the best deal on a car for us in Chicago. I have talked the matter over with Helen and this morning I wrote to Don to go ahead with Dorchester Motors, if they have a Plymouth Cambridge Club Coupe in the gray color. If they don't, he should buy it from Essermann Motor Sales. I added that if neither has a model in gray and can't deliver by the time I get there, perhaps he will phone me again and we can talk it over again in terms of

what colors are available. I sent Don a check made out to him for \$1900 to cover the cost of the car, an Illinois license, and public liability (\$10,00/\$20,000) and property damage (\$5,000) insurance. I asked that the insurance be with a company that has offices in California so that it can be transferred, adding that Sears Roebuck and Company is probably as good as any. Finally, I asked that he give some thought as to the most convenient way for me to pick up the car as soon as possible after we arrive in Chicago.

Earl Hyde prepared another letter of recommendation for Dean Dunlavey for me to sign, which I did today. This was addressed to the Graduate Division to support Dunlavey's application for a Jake Gimbel Scholarship Loan.

I finally mailed Chapter XXXVII entitled "The Actinide Series" to Professor M. C. Sneed (University of Minnesota) for his volume Comprehensive Inorganic Chemistry. A carbon of the manuscript went to Joe Katz at Argonne.

A few days ago P. R. Constantinoff (President, Museum and Archives of Russian Culture, San Francisco) sent me a periodic chart prepared by Vladimir V. Romanoff (Paris) for evaluation. John Rasmussen looked it over; today I wrote Constantinoff (with a carbon to Romanoff), saying that it is based on sound principles and emphasizes some points different from a number of other charts in use. I said that it is not possible to say which type is best since this depends on the varying viewpoints of the users. I pointed out, however, the currently accepted chemical symbols for several of the elements that are different from the ones on his 1945 chart (43, Tc; 61, Pm; 62, Sm; 69, Tm; 72, Hf; 85, At; 86, Em; 87, Fr; 91, Pa; and 94, Pu). I mentioned that four additional elements have now been made and identified (95, Am; 96, Cm; 97, Bk; and 98, Cf).

Charles Newton (Assistant to the President of California Institute of Technology) wrote, in a letter I received today, to ask the cost of my trip to Pasadena to help judge the Science Fair. He gave the attendance figures for the Fair as 1,670 for Friday night; 5,475 for Saturday; and 13,148 for Sunday. The total, he pointed out, is about double the normal weekly total for the Los Angeles County Museum. I immediately replied, saying the cost of my plane ticket (my total expenses) was \$40.00 and adding that I am glad the Science Fair was such a success and that it was very worthwhile from my own point of view.

I made a tour of the labs to check on the research.

Saturday, April 26, 1952

The construction of the addition to our house is proceeding amazingly rapidly, and it is helping keep the kids' attention diverted from the itching of the chicken pox.

John Francis Neylan telephoned about 12:30 p.m., as he promised. He said that he took the matter up with the Committee on Atomic Energy Contracts at yesterday's Regents' meeting. Neylan said he told the Committee that he was going to withdraw and suggested that he be replaced in the negotiations. Later the matter was taken up in the full meeting

of the Board of Regents when he made the same suggestion that someone, possibly another member of the Committee on Atomic Energy Contracts, replace him. Neylan said that no one wanted to undertake the responsibility and that the Regents are going to request us to state (presumably in writing) what we want them to do. He also told me that Mr. Underhill will get in touch with me in a few days to talk about this.

Sunday, April 27, 1952

I gave a final reading to my article on the transuranium elements for the Encyclopaedia Britannica and then spent the rest of the day with the family. Pete and Steve have now come down with the chicken pox, so our May 1 departure date for Chicago is definitely postponed.

Monday, April 28, 1952

The spring recess for the University is this week, so most of the students will be free for research.

I was told that the extraction of the americium from the plutonium cow was completed on Saturday. The Health Chemists tore up the somewhat contaminated linoleum in the Annex preparatory to replacing it.

This morning a telegram arrived from W. A. Noyes, Jr. and J. H. Hildebrand in Washington, D. C. reading, "HEARTIEST CONGRATULATIONS ON YOUR ELECTION WITH MCMILLAN AMERICAN PHILOSOPHICAL SOCIETY."

I mailed to Walter Yust (Encyclopaedia Britannica) my article, "The Transuranium Elements," which he requested in a letter of January 28. I pointed out that there are a number of places where cross references to other articles could be used to advantage but I said that I was leaving this up to him since I am not familiar enough with the Encyclopaedia's content.

In addition to various phone calls, etc., I got together with Segre and talked about my conversation with Neylan last Saturday. We prepared a very rough draft of a letter to the Board of Regents, in response to their request, to tell them that we want them to furnish the information requested by the Patent Compensation Board at the Prehearing Conference on February 19, 1952 in Washington, D. C. We explained that apparently the Board needs a definition of the Regents' interest before the Board can proceed. We wrote, "A suggested method for defining the Regents' claimed interest, satisfactory to the inventors, put forward by Mr. I. Harter of the Patent Compensation Board on the assumption that the Regents are not seeking any monetary compensation, is that this interest be defined as a non-assignable, non-exclusive, royalty-free license to use the inventions." I then wrote Joe Kennedy, enclosing my notes on my telephone conversation on Saturday with Neylan and sending Kennedy and Wahl our rough draft of the letter desired by the Regents. I told Joe that I shall call him Wednesday afternoon to see whether we can agree on the plan of action and to modify the draft, in case such a letter is part of the plan.

The senior staff had its brown-bag luncheon meeting in my office at noon. We talked about a number of things, including the plans for

Building 70 (our planned chemistry building).

Most of the rest of the day was spent making additional preparations for my extended visit at the Argonne Laboratory and talking with the students about their future research.

Tuesday, April 29, 1952

Most of the day was spent on administrative matters, more details about my Chicago visit, and talking with the students.

I did write to Professor George W. Parks to explain that one of our new Ph.D.'s, Dr. Edward H. Fleming, is going to work with California Research Corporation to set up a radiochemical laboratory in their La Habra Laboratories. I said that they would like to send Fleming to the Gordon Research Conference on Nuclear Chemistry; I am writing independently of his application to indicate my agreement with the desirability of his attending and to express the hope that the lists are not yet full.

Today's mail brought another letter from J. Guéron, giving another time for Professor Prettre's arrival in Berkeley. (Guéron wrote about this a couple of weeks ago.) Since the time is after my scheduled departure for Chicago (although at the moment I am not certain when we will be able to leave because of the chicken pox), I routed the letter and its information to Perlman. [Perlman will handle most of my duties and make decisions while I am in Chicago.]

Wednesday, April 30, 1952

Again, among this morning's phone calls, were some connected with our departure for Chicago. Hopefully, we shall be able to leave in about a week.

Neylan told me Saturday that I would hear from Robert Underhill but since I have yet to do so, I telephoned Underhill about 10:15 this morning. Underhill sounded as though he were rusty concerning the case and asked me to send him some information about the case. He gave me the impression that little is done by the whole Board of Regents and that they work only through the committees, which in this case means the Committee on Atomic Energy Contracts of which Neylan is still chairman. Underhill told me that the Board of Regents will meet on May 23, June 27, July 25, August 22, and September 19.

I also returned John Voelker's manuscript to him with a few suggestions, such as requesting that he mention Stanley Thompson in connection with the discovery of elements 97 and 98. I also said I want to go easy on any direct quote along the lines he suggested due to the possibility of misuse by newsmen, but I did enclose a quote from one of my articles, which I said, may suit his purpose. After answering some questions he posed and including a couple of photographs for his den, I told Voelker that my family and I are going to spend a couple of months, probably May and June, in Chicago where I shall spend some time at Argonne National Laboratory, adding that we are going to try to come up to Ishpeming at the end of our stay. Later, in today's mail, another

letter arrived from Voelker, expressing concern because I had not answered his letter of March 20.

Also, in today's mail was an April 29 letter from Art Wahl, saying that he and Joe approve of my recent actions in negotiating with the Regents. He wrote, "Mr. Ahlport's suggestion looks reasonable to us, and we would go along with it if, as you suggest, in practice the joint administration would leave things pretty much as they are and would not take control of the research funds from the inventors." Art added that, because of travel, Joe has not drafted the letters to Lane and the Patent Compensation Board that Joe and I discussed and will probably now wait until after May 5, for further developments in California before proceeding.

Another letter that arrived today was from Arne Holmberg, who said he has sent the manuscript of my Nobel lecture to the printers. I was notified by Thomas B. Steel (Secretary, Faculty of the College of Chemistry) that I have been elected Vice-Chairman of the Faculty of the College of Chemistry for the year 1952-53.

Luther P. Eisenhart (American Philosophical Society) sent me the following notification of my election to the Society (I had heard of this yesterday by a telegram from Noyes and Hildebrand). Eisenhart said a newly elected member is expected to attend one of the general meetings as soon as possible to sign the Laws and to be formally inducted. He gave me the dates of the next General Meetings.

American Philosophical Society,
Independence Square,

Philadelphia, April 26, 1952

Sir:

I have the honour of informing you
that you have been this day elected a Member of
the American Philosophical Society, held
at Philadelphia for promoting useful knowledge.

Yours,

Sir:

Your obedient Servant,

Leah A. Eschmeyer
Secretary

Executive Officer

Professor Glenn Theodore Seaborg
University of California
Berkeley 4, California

About 2:25 p.m. I telephoned Joe Kennedy in St. Louis to talk about my latest encounters with Neylan and Underhill, and the draft of the letter to the Regents that Emilio and I prepared. Joe suggested an expansion of the letter with more explanation, and I agreed to this plan. Joe agreed to drafting the letter, which he and Wahl will sign and then send on to Emilio and me for our signatures. I told Joe what I proposed to send to Underhill, and he agreed to this. I mentioned that I will to be in Washington on May 12 and plan to try to see and talk with Lane. Joe thought this was a good idea and, since he has not done anything about contacting Lane about refiling our claim, agreed that I should take it up with Lane if I see him.

I then wrote Underhill and sent him (1) a copy of the memorandum summary of the prehearing conference hearing in Washington, D. C. on February 19, 1952 before the Patent Compensation Board in which the Regents' letter of October 8, 1951 was considered, (2) a copy of our Attorney Lane's letter of February 20, 1952 describing this prehearing conference in more detail (for his personal use), and (3) a copy of our Agreement Contract No. W-28-094-eng-30 between the inventors and the U. S. Government, including the Certificate of Disclaimer in its original form. I went on to tell Underhill that the inventors will send him the requested letter outlining their desires and suggestions on the matter within a few days.

Finally, I wrote Lane, saying that we have delayed writing him because the situation sofar as the Regents is concerned has been rather slow in developing. I said that it appears I shall be in Washington on Monday, May 12, and asked if it would be possible for us to have dinner together that evening so that we can spend an hour or two going over the whole matter.

Doral has had the room and phone number list of members of our group updated and distributed:

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg</u>
Abrams, Dorothy	396	102	4
	378	339	50
Asaro, Frank	397	107	4
Baker, Joan	250	201B	4
Batzel, Roger E.	250	201B	4
Biller, William F.	380	346	50
Browne, Charles I.	397	109	4
Brutschy, Fred	466	203E	4
Buchholz, Doral	246	114	5
Carlson, Margot M.	246	114	5
Carniglia, Stephen C.	349	105	5
Carr, Robert J.	349	104	5
Carter, Giles F.	7-8159	318	Lewis
Chan, Jannie	376	332	50
Clark, Edward S., Jr.	7-8159	318	Lewis
Coleman, George	250	201B	4
Conway, Dwight C.	380	350	50
Conway, John G.	374	316	50

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg.</u>
Cook, Marshall W.	466	203E	4
	7-472	213	Gilman
Crane, William W. T.	250	201B	4
Cunningham, Burris B.	349	105	5
Dauben, Carol H.	7-8159	318	Lewis
Davis, Mildred J.	246	114	4
DiGrazia, Herbert X.	470	355	50
Dunlavey, Dean C.	378	341	50
Feay, Darrell C.	349	105	5
Fick, J. Leonard	466	203E	4
Fischer, Vera K.	379	345	50
Fleming, Edward H.	349	105	5
Gallagher, Fran	393	203F	4
Gede, Victor	470	355	50
Ghiorso, Albert	358	111	5
Gilbert, Richard	393	203F	4
Glass, Richard A.	349	104	5
Goda, Lilly	395	103	4
Guenther, Lloyd	466	203E	4
Gunn, Stuart R.	250	202	4
Hanson, Donald N.	7-473	120	Gilman
Hartzell, Alfred J.	395	103C	4
Hempel, Virginia O.	380	346A	50
Heppler, Winifred	349	105	5
	250	202	4
Hicks, Harry G.	393	203F	4
Hicks, Thomas E.	467	203E	4
Higgins, Gary H.	349	104	5
Hoff, Richard W.	380	350	50
	260	102	5
Hollander, Jack M.	246	113	5
Hollander, Margie J.	246	114	5
Huffman, Eugene H.	470	340B	50
Hulet, E. Kenneth	260	102	5
Hunt, Charles D'A.	7-472	213	Gilman
	466	203E	4
Hutchin, William	393	203F	4
Hyde, Earl K.	345	110	4
Iddings, Glen M.	250	201B	4
Jaffe, Harold	397	109	4
Jost, John	466	203E	4
Kalkstein, Marvin I.	358	108	5
King, Emily	250	201B	4
Koch, Charles W.	7-8207	27	Lewis
Kofstad, Per	378	344	50
Lane, Albert F.	7-473	101	Gilman
Larsh, Almon E., Jr.	358	111	5
Latell, Frances	466	102	4
Levy, Harris B.	7-8109	21	Lewis
Lindner, Manfred	393	203F	4
Lippmann, David Z.	7-8159	310	Lewis
Litz, Lawrence	393	203B	4
Lundin, Robert E.	7-8159	318	Lewis

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg.</u>
Marlowe, James	395	103C	4
Martin, Donald F.	395	103	4
Mathur, Hirdaya B.	380	344	50
Mattern, Kenneth L.	466	203E	4
McKennon, Docia	260	102	5
Michel, Maynard C.	395	103	4
	372	311	50
Mohler, Bobby Ann	381	353-4	50
Momyer, Floyd F.	345	110	4
Nervik, Walter E.	378	343	50
Newton, Amos S.	381	353-4	50
O'Kelley, G. Davis	250	201A	4
	397	108	4
Ord, Mary L.	395	103B	4
Osborne, Robert N.	393	203F	4
Oswalt, Robert L.	470	355	50
Passell, Thomas O.	397	109	4
Perlman, Isadore	246	115	5
Petzold, Dora A.	395	103	4
Powers, John E.	7-472	221	Gilman
Rasmussen, John O., Jr.	380	350	50
	375	330	50
Rea, Homer E.	7-473	101	Gilman
Reynolds, Fred L.	397	108	4
Ring, Stanley A.	393	203B	4
Robinson, Herman P.	395	103	4
Robinson, Raymond W.	466	203E	4
Ruben, Helena W.	7-8159	318	Lewis
Rubin, Barney	466	203E	4
Scarborough, James	470	340	50
Seaborg, Glenn T.	246	114A	5
Shudde, Rex H.	378	341	50
Skirvin, Sidney D.	345	110	4
Slater, Louis M.	380	350	50
Spencer, Harry E	7-8159	306	Lewis
Stevenson, Peter C.	393	203F	4
Street, Kenneth, Jr.	250	201B	4
Templeton, David H.	7-8159	318	Lewis
	7-649	106	Gilman
Terwilliger, Doris H.	466	203E	4
Thompson, Stanley G.	260	102	5
Turner, Beverly	397	107	4
Tuttle, William N.	374	316	50
Tyrén, Helge	380	346	50
Vermeulen, Theodore	7-473	104	Gilman
Waite, Jane A.	396	102A	4
Williams, Lawrence	470	355	50

Thursday, May 1, 1952

Present at this first of the month group meeting were Asaro, Carniglia, Carr, Clark, Cunningham, Dauben, Dunlavey, Feay, Glass, Gunn, Hoff, Hollander, Hulet, Jaffe, Levy, Michel, Momyer, Nervik, Passell, Perlman, Rasmussen, Reynolds, Ruben, Seaborg, Shudde, Slater, Templeton, and Tyrén.

Tyrén gave an extensive report on work with Walter Barkas and Robert W. Deutsch on the direct study of secondary emitted particles. He reported that the apparatus was similar to that used in meson experiments with the target placed to intercept ~330 Mev protons; the secondary particles spiral inwards towards the cyclotron center. He said that the procedure is to bombard for 15 seconds and obtain tracks from the emitted particles in photographic plates. Then, he said, one can determine the charge of the particles from the grain density of the tracks and the radius of curvature. Thus, from range energy relations, one can determine the mass and energy of the emitted particle. Tyrén said they have bombarded copper, gold, uranium, carbon, beryllium, and aluminum; the experiment takes only a few hours, but it takes months to calculate and count the tracks. The work evoked much discussion, particularly with Dunlavey and me.

Nervik presented spallation yields from bombardments of tantalum with 340 Mev protons: Ni⁶⁵ (cross section in mb, 0.038, 0.043, 0.033); Ni⁶⁶ (0.017, 0.014); Cu⁶⁴ (0.057, 0.069, 0.067); Cu⁶⁷ (0.048, 0.055, 0.059); Ga⁷⁶ ([0.062], 0.065); As⁷⁶ (0.033, 0.056); As⁷⁷ (0.022, 0.030); Sr⁹¹ (0.016); Ru¹⁰³ (0.0033). I asked if he had looked for isomers, and Nervik said he hadn't and that only one or two isotopes are observed for each element isolated so that the choice of suitable isomers is very limited. We discussed the choice of tantalum as a target material.

Asaro talked about the gamma radiation of Pu²³⁹, stating that parity and spin changes can be determined by conversion coefficients. He referred to the work of other experimenters. Asaro said they determined the electron energy (kev) in the beta ray spectrometer and then corrected for the assumed transition:

$L_{II} = 31.4/52.5$; $L_{III} = 35.0/52.2$; $M_{II} = 47.1/52.3$; $M_I = 50.2/51.6$. He went on to say that the English workers D. West and J. K. Dawson found 8% L vacancy per alpha particle and therefore half of the electron transitions must go by conversion in the L shells. He added that, since most electrons are in the 30 to 35 kev range, then L conversion of the 50 kev transition should be the most predominant effect; the conversion coefficient was calculated to be 200, which is about the order expected for quadupole transitions. Asaro pointed out that only L_{II} and L_{III} transitions are found in electric quadupole radiation of this energy. The work evoked much discussion.

* * * * *

Up on the hill I mailed to Joe Katz my corrected copies of Chapters 7 ("Nuclear Properties of the Plutonium Isotopes"), Chapter 11 ("Nuclear Properties of the Neptunium Isotopes"), and Chapter 13 ("Nuclear Properties of the Transplutonium Nuclides") of PPR, Volume 14A. These chapters are declassified and are now ready to be sent to Oak Ridge.

In today's mail was a handwritten letter from S. J. Barnett, who thanked Joe Kaplan, Professor Giauque, and me for our efforts in his behalf (nominating him for the Nobel Prize in physics). He sent copies of his reprints but noted that two of the most important are in poor condition. Barnett also included a nearly complete listing of all of his papers. He also mentioned a couple of points that he thought might be helpful even though they had never been mentioned outside his family. He said that when he received the Comstock Prize (2nd award) in 1918, the certificate of award stated that the results for which it was given were "of the utmost significance in the theory of magnetism and far reaching in their bearings upon our views of the ultimate structure of matter." He added that the chairman of the committee said that, in the future, whenever Ampere's work is mentioned, this work will have to be mentioned with it. Barnett went on to say, "You were with me as a student for so short a time that I am afraid I can't claim much credit for the brilliant work you have done. But I have always been greatly pleased to hear about it, and especially so when you received a Nobel Prize. I was also greatly pleased when a Nobel Prize went to Professor Giauque. Though I have not written you before, I have always felt hearty congratulations to you both." I asked Doral to have a copy of the letter sent down to Giauque.

An abstract of a paper for the Gordon Conference arrived from Stephen E. Stephanou, and I asked that a copy be sent to Coryell.

I read a memo from W. B. Reynolds about my leave. Travel will be reimbursed for both me and my family, and I shall receive a per diem allowance of \$9.00 from the time I leave here until I return unless I take time out for vacation travel. Travel expenses for business trips will be allowed but not meals nor hotel costs.

Bill Crane and Pete Stevenson stopped in to talk with me for a while. Bill told me about their bombardment of U^{238} with 190 Mev deuterons and gave me their production cross sections for a number of protactinium isotopes. Pete gave me a formula they have worked out for determining the true disintegration rate from the counting rate by incorporating various absorption and scattering corrections.

Doral has had a revised home address list of our group prepared:

RADIATION LABORATORY CHEMISTRY DIVISION
HOME PHONE AND ADDRESS DIRECTORY
May 1952

Abrams, Dorothy 5938 Marshall St Oakland 8 Humboldt 3-8017	Carr, Robert J 2511 Telegraph Ave Berkeley 4 ASHberry 3-9310	Dauben, Carol H 1322 La Loma Ave Berkeley 8 BERkeley 7-6250R
Asaro, Frank 1904 University Ave Berkeley 4 ASHberry 3-8982	Carter, Giles F 2512 Etna St Berkeley 4 BERkeley 7-8490	Davis, Mildred J 985 Gill Court Albany 6 Landscape 4-5262
Baker, Joan E 47 Sotelo Ave San Francisco 16 SEabright 1-1343	Chan, Jannie 2322 Haste St Berkeley 4 ASHberry 3-0080	Di Grazia, Herbert X 2131 North Point St San Francisco 23 Fillmore 6-4788
Balkwell, William R 2045 Del Norte St Berkeley 7 Landscape 4-3142	Clark, Edward S, Jr International House Berkeley 4 ASHberry 3-6600	Dunlavey, Dean C 1133 Spruce St Berkeley 7 Landscape 4-1019 (emergency)
Batzel, Roger E 251 Leland Way Concord Concord 2414	Coleman, George H 2147 Parker St Berkeley 4 ASHberry 3-7283	Feay, Darrell C 2214 Durant Ave, Apt 6 Berkeley 4 ASHberry 3-7509 (emergency)
Biller, William F 611 - 30th St Oakland 9 GLencourt 1-2282	Conway, Dwight C 2617 Haste St Berkeley 4 BERkeley 7-8872	Fick, J Leonard 1116 Blake St Berkeley 2
Browne, Charles I, Jr 215 Stanford Ave Berkeley 8 Landscape 6-1411	Conway, John G 1153 King Drive El Cerrito 7 Landscape 5-7545	Gallagher, Fran 2818 Telegraph Ave Berkeley 5 ASHberry 3-4987
Buchholz, Doral 2790 Shasta Rd Berkeley 8 ASHberry 3-5493	Cook, Marshall W 2817 Regent St Berkeley 5 ASHberry 3-6153	Gede, Victor 213 Yale Ave Berkeley 8 Landscape 6-5782
Carlson, Margot M 1890 Arch St, Apt 304 Berkeley 9 ASHberry 3-7147	Crane, William W T 2121 Young's Court Walnut Creek	Ghiorso, Albert 687 Vincente Ave Berkeley 7 Landscape 5-4677
Carniglia, Stephen C 1301 El Curtola Blvd Walnut Creek Walnut Creek 5161	Cunningham, Burris B 144 Bret Harte Rd Berkeley 8 THornwall 3-3834	Gilbert, Richard S 260 Gloria Drive Concord

Glass, Richard A
2629 Etna St
Berkeley 4
BERkeley 7-4626

Goda, Lilly Y
2332 Grove St, Apt C
Berkeley 4
THornwall 3-2468

Gunn, Stuart R
2744 Regent St
Berkeley 5
BERkeley 7-6358J

Hanson, Donald N
1117 Miller Ave
Berkeley 8
ASHberry 3-2934

Hartzell, Alfred J
1634 Hopkins St
Berkeley 7
Landscape 6-8147

Heppler, Winifred B
18 Columbia Circle
Berkeley 8
BERkeley 7-8356W

Hicks, Harry G
5710 Ayala Ave
Oakland 9
HUMboldt 3-9688

Hicks, Thomas E
2626 Hilgard St
Berkeley 9
BERkeley 7-0663W

Higgins, Gary H
2235 Hearst Ave
Berkeley 9
ASHberry 3-8173

Hoff, Richard W
International House
Berkeley 4
ASHberry 3-6600

Hollander, Jack M
431 - 63rd St
Oakland 9
HUMboldt. 3-9446

Hollander, Margie J
431 - 63rd St
Oakland 9
HUMboldt 3-9446

Huffman, Eugene H
Faculty Club
Berkeley 4
ASHberry 3-5678

Hulet, E Kenneth
50 Maple Lane
Walnut Creek

Hunt, Charles d'Ancona
14 Berkeley Ave
Orinda
Orinda 8665

Hutchin, William
1184 Oxford St
Hayward

Hyde, Earl K
975 Miller Ave
Berkeley 8
Landscape 4-1956

Iddings, Glen M
912 Jeffry Drive
Concord
Concord 2009

Jaffe, Harold
1725 Oxford St, Apt 9
Berkeley 9
ASHberry 3-6730

Jensen, Hildred
2526 College Ave
Berkeley 4

Jost, John, Jr.
1737 - 9th Ave, Apt D
San Francisco 5
MONTrose 4-5675

Kalkstein, Marvin I
1672 Oxford St
Berkeley 9
BERkeley 7-0549J

King, Emily
2000 Virginia St
Berkeley 9
THornwall 3-10852

Koch, Charles W
1451 Marin Ave
Albany 6
LANDscape 5-4131

Kofstad, Per
2430 College Ave
Berkeley 4
BERkeley 7-7993

Larsh, Almon E, Jr
2620 Hillegass Ave
Berkeley 4
ASHberry 3-9351

Latell, Frances
2306 Howe St
Berkeley 5
BERkeley 7-4657J

Levy, Harris B
2501 Benvenue Ave, Apt 27
Berkeley 4
ASHberry 3-9172

Lindner, Manfred
308 Verona Ave
Danville
VERnon 7-4444

Litz, Lawrence M
1175 Glendora Circle
Danville
VERnon 7-4241

Lundin, Robert E
2512 Etna St
Berkeley 4
BERkeley 7-8490

Maguire, Patricia W
21 Entrada Ave
Oakland 11
OLympic 3-2140

Marlowe, James R
172 Sonora Ave
Danville

Martin, Donald F
148 Cleopatra Drive
Sherman Acres, Concord
Concord 9106

Mathur, Hirdaya B
International House
Berkeley 4
ASHberry 3-6600

McKennon, Docia B
2815 Acacia Rd
Walnut Creek
Walnut Creek 3164

Michel, Maynard C
12 Yale Circle
Berkeley 8
Landscape 6-9454

Mohler, Bobby Ann
2531 Ellsworth St
Berkeley 4
BERkeley 7-0178R

Momyer, Floyd
362 Clifton St
Oakland 9
PIedmont 5-5205M

Nervik, Margaret R
2717 Haste St
Berkeley 4
BERkeley 7-9043

Nervik, Walter E
10 Prospect Rd
Piedmont 10
GLencourt 1-7692

Newton, Amos S
833 Ramona Ave
Albany 6
Landscape 6-8308

O'Kelley, G Davis
168 Hill Road
Berkeley 8
ASHberry 3-5074

Olsen, James L
2330 Florida Ave, Apt 1A
Richmond

Ord, Mary Lou
2829 Prince St
Berkeley 5
OLympic 3-0134

Osborne, Robert N
213 Yale Ave
Berkeley 8
Landscape 6-5782

Oswalt, Robert L
2423 Blake St
Berkeley 4

Passell, Thomas O
2338 Grove St
Berkeley 4
BERkeley 7-4983W

Perlman, I
1158 Glen Road
Lafayette
Lafayette 3021

Petzold, Dora A
1810 University Ave
Berkeley 3
THornwall 3-2480

Powers, John E
2914 Ellsworth St
Berkeley 5
BERkeley 7-6069R

Rasmussen, John O, Jr
1945 Delaware St
Berkeley 9
ASHberry 3-1457

Rea, Homer Earl, Jr
International House
Berkeley 4
ASHberry 3-6600

Reynolds, Fred L
2539 - 38th Ave
San Francisco 16
SEabright 1-1236

Ring, Stanley A
8176 Mabel Ave
Castro Valley
LUcerne 2-8781

Robinson, Herman P
31 Diablo Circle
Lafayette
Lafayette 3831

Robinson, Raymond W
1392 Sunset Loop
Walnut Creek
Walnut Creek 7-480

Ruben, Helena W
651 Vincent Ave
Berkeley 7
Landscape 5-3187

Scarborough, James M
1745 Highland Place
Berkeley 9
ASHberry 3-3169

Seaborg, Glenn T
1154 Glen Road
Lafayette
Lafayette 2001

Shudde, Rex H
2627 Virginia St
Berkeley 9
BERkeley 7-9078

Skirvin, Sidney D
International House
Berkeley 4
ASHberry 3-6600

Slater, Louis M
International House
Berkeley 4
ASHberry 3-6600

Spencer, Harry E
2512 Etna St
Berkeley 4
BERkeley 7-8490

Stevenson, Peter C
101 Tamalpais Rd
Berkeley 8
ASHberry 3-6386

Street, Kenneth, Jr
5 Lincolnshire
Walnut Creek
Walnut Creek 9588

Surls, Joseph P. Jr
2644 Dwight Way
Berkeley 4

Tarrant, James R
2575 Le Conte Ave
Berkeley 9
THornwall 3-8494

Tellefsen, Robert L
1712 Marin Ave
Berkeley 7
LANDscape 6-5869

Templeton, David H
804 Vincente Ave
Berkeley 7
LANDscape 5-2788

Terwilliger, Doris H
1757 Oxford St, Apt 9
Berkeley 9
ASHberry 3-8822

Thompson, Stanley G
230 Elaine Drive
Concord
Concord 5562

Turner, Beverly A
2635 Dwight Way, Apt 17
Berkeley 4
THornwall 3-8547

Tuttle, William N
2511 Hearst Ave
Berkeley 9
ASHberry 3-6134

Tyrén, Helge
1240-A Milvia St
Berkeley 9
LANDscape 4-6053

Vermeulen, Theodore
725 Cragmont Ave
Berkeley 8
LANDscape 4-0374

Waite, Jane A
1169 Miller Ave
Berkeley 8
ASHberry 3-5006

Williams, Lawrence A
3144 College Ave
Berkeley 5

Friday, May 2, 1952

Helen has consulted with the kids' pediatrician about the date he thinks they can travel (without giving chicken pox to every susceptible person on the airplane) and was told that, barring complications, we can probably leave next Friday for they should be no longer contagious. This morning I made several phone calls apprising various people of this. I talked with Wally Reynolds in our business office, suggesting that perhaps the date of my leave and being placed on the Radiation Laboratory fulltime payroll should be changed to May 16 since I have been asked to attend a meeting of the IO-7 Panel of the Committee on Atomic Energy of the Research and Development Board in Washington on May 12 and May 13.

I then dictated a number of letters. One went to Joe Kennedy about a question regarding the Division of Physical and Inorganic Chemistry. I wrote that I would like the opinions of Frank Long and Milton Burton (to whom I sent carbons) on the matter and suggested that Long and Burton address their opinions directly to Joe (with carbon to me at Argonne) since I shall be there during the last half of May and the month of June. I told Joe that we can consult by phone about the matter after he hears from Long and Burton.

A letter also went to John Willard about the talk he asked me to give in Madison for the final meeting of the year before the Wisconsin Section of the ACS. I said that I shall be glad to come for Friday evening, June 6, provided I can do it by not missing more than Friday afternoon from the laboratory. I wrote, "Therefore, if there is an afternoon train leaving around 2 p.m., put me down for Friday evening, June 6; otherwise, make it Saturday, June 7."

I thanked Professor S. J. Barnett for the reprints he sent me and said, "I do not know whether we can hope to be successful in this undertaking [nomination for Nobel Prize in physics], but it seems to me that we might be in the course of a few years. I have the impression that a name usually comes up a number of times before the selection is made." I also told him that I remember very well the course on electricity and magnetism that I took under him and that I was impressed at that time with the importance of the experiments he described.

A note went to Luther P. Eisenhart to say that I was pleased to receive his letter of April 26 and learn of my election as a member of the American Philosophical Society. I wrote, "I will certainly try to attend a meeting in order to be formally inducted as soon as possible."

I also wrote to C. B. Marquand (ACS Committee Advisory to the Chemical Corps) and said that I do feel that I should visit the Army Chemical Center as soon as my schedule possibly permits, but I feel that this cannot be during the month of June. I explained that, even though I am spending the last half of May and the month of June in the Middle West as a visiting consultant to Argonne National Laboratory, my schedule is such that I shouldn't leave for any appreciable length of time during that interval. I suggested possibly a visit toward the end of the Fall Meeting of the American Chemical Society. In addition, I wrote that, although I do not feel I can attend a June meeting of our Subcommittee at the Center, I might be able to get away for a meeting quite close to

Chicago.

I acknowledged the abstract for the Gordon Conference that I just received from Stephen E. Stephenou (Los Alamos), saying the results of his work interest me very much and noting that we look forward to hearing his report during the session on Heavy Element Chemistry. (A carbon of this went to Coryell.)

My final two corrected chapters for PPR, 14A were returned to Joe Katz. These are Chapter 17 ("Correlation of Properties as Actinide Transition Series") and Chapter 20 ("Slow Neutron and Spontaneous Fission Properties of Heavy Nuclei"). I pointed out that only declassified matter is involved in the corrections.

I declined an invitation, dated April 17, to address the Purdue Chapter of Sigma Xi, which I received from Dr. John E. Christian (School of Pharmacy, Purdue University). I noted that I have tentatively planned to come to Lafayette within the next year or two to address the Chemistry Department at the invitation of Drs. E. T. McBee and W. H. Johnston and perhaps something can be arranged for that time. I asked how soon he will need a definite date since, as I pointed out, such an event will have to be combined with some other trip and I don't have a definite schedule for next year.

Yesterday an amusing letter, dated April 29, arrived from Bill Jenkins (now with the Technical Division, E. I. du Pont, Wilmington). Bill wrote, "After my sincere effort to leave Berkeley with all the little jobs taken care of, I find that I left with a most important document. Of course, I am referring to the golf handicaps and, in case your memory fails, I am enclosing them." He then said that he made the state line with his 1951 license plates and also was able to leave Las Vegas with his Hudson although it was doubtful at one time. [Bill used to claim that he bought his car with earnings from gambling in Las Vegas on his way to Berkeley.] He went on to say that he hopes to spend a few days with us in Chicago; he sent his regards to everyone, including Helen and the "impi doos" (his and my nickname for our children). I acknowledged Bill's letter today and mentioned that Al and Stan and about five others are in Chicago trying to do their big job. I wrote that Al is banking on the gas sweeping method since the iron filings process ran into some difficulties; however, I haven't had any reports about their progress. I told Bill that our trip was delayed because of the chicken pox but we plan to leave next Friday. I also mentioned my committee meeting in Washington on May 12 and 13.

Saturday, May 3, 1952

Although I spent some time reading today, I made a sincere effort to help entertain the kids so that Helen could accomplish some of the tasks that need to be done before we leave for Chicago. Cases of chicken pox do not make very gracious children.

Our architect Bill Rice has had several conversations with us about the addition, which he will continue to oversee until the work is completed. Helen has already chosen the colors for the rooms. We agreed to his suggestion that we have the hardwood floors in the rest of the

house polished while the children are away, and he will take care of that matter. Bill convinced us that mugo pines would be the proper bushes to plant in the planter box near our front entrance; Bill, with the help of Jack and Margie Hollander (who will stay in the house while we are away) will do the planting.

Sunday, May 4, 1952

The children still took some of my attention. They, and Helen and I, are fascinated by the amazing progress on the construction of our addition.

Monday, May 5, 1952

I again took care of some administrative duties, read reports and journals, and talked with the men.

I sent Joe Katz an addition to add to reference 86 of Chapter 7 and to reference 1 of Chapter 11 of PPR, Volume 14A: D. A. Orth, Ph.D. thesis, University of California (January, 1951) and University of California Radiation Laboratory Declassified Report, UCRL-1059 Rev. (November, 1951).

A response arrived today from Donald Lane to my letter to him of April 30. Lane asked that I call when I arrive in Washington (May 12) in order that we can make plans to have dinner together.

I also received a letter from Lawrence Altman, who had been accepted into graduate school by Dean Pitzer and to whom I offered our type of research assistantship. Altman wrote that certain circumstances have developed that will prevent him from pursuing graduate work at the University of California. He added that he has informed Dr. Pitzer and Mr. Everson of this.

After our noontime senior staff luncheon meeting in my office, I again did some reading, etc.

Later Ken Street and Bill Crane stopped in to talk with me for a while and to ask that I find out from the Chicago people their schedule for the ionium separation at Y-12. They want 2 grams of ionium (after enrichment) for neutron bombardment at Arco, Idaho and hope that this can be done in June rather than in the October run. Street suggested that we could do cooperative experiments with Argonne in which we extract the Pa^{231} .

Tuesday, May 6, 1952

This morning I wrote to Charles Coryell to tell him that I have gotten out of the engagement in Minneapolis on June 22 (Svenskarnas Dag), so I will not need his help on the night driving adventure he offered me in order to get from Boston to New Hampton. I asked, however, if he would be willing to round up or cause to be rounded up a number of sets of golf clubs since practically the entire California contingent will be interested in this activity and the required implements are too bulky and heavy to transport all the way from California or Chicago. I added that I have the impression that our Gordon Conference is going to be

oversubscribed, on the basis of the numerous reports I have heard of people wishing to attend. I said I presume that the handling of the applications by Parks is going smoothly.

A note went to AEC Commissioner T. Keith Glennan to tell him that I plan to be in Washington next Monday and Tuesday (May 12 and 13) to attend another meeting of the AFOAT long-range detection committee. I said that I shall try to get over to the AEC to see whether he cares to discuss the subject of the GAC that he raised during my last visit.

At yesterday's staff meeting Burris Cunningham asked me to arrange for a leave of absence from June 16 to August 18 for his technician Winifred Heppler. I wrote a memo to W. S. Bigelow (Personnel Department) today, requesting such a leave. I explained that Mrs. Heppler wants to join her husband, who has been recalled to the Navy, during that time period. Since we are so interested in the resumption of her work here (she is a highly skilled assistant in the microchemistry laboratory), we recommend that she be granted a leave for this period rather than the alternative termination of employment with rehiring.

Winifred E. Heppler



I answered an April 29 letter from D. S. Le Beau, Chairman-Elect of the Division of Colloid Chemistry of the American Chemical Society. Le Beau asked a number of questions about the organization of our Division of Physical and Inorganic Chemistry, which I attempted to answer.

Emilio and I looked over and signed the revised letter to the Regents that Joe Kennedy prepared from our draft of April 28, and one of the girls in the office took it personally down to Mr. Underhill's office. I then wrote Joe, told him that we thought the letter was excellent, and explained the disposition of the copies. I added that Emilio will get in touch with Underhill around the middle of the month in order to encourage him to keep the matter moving. I mailed a copy of the letter to Lane and said that I shall explain the background of it when we get together on May 12.

A note arrived in today's mail from Bert Tolbert, thanking me for my support and saying that he received the U.S. Public Health Service special research fellowship and will go to Eidgenosche Technische Hochschule about September 1 on leave.

The rest of the day was spent talking with some of the men and students in their labs.

Wednesday, May 7, 1952

Yesterday I received a paper from W. Albert Noyes, Jr. (Editor, Journal of the American Chemical Society) asking for my comments as a member of the Editorial Board. Noyes said they do not believe they can afford to publish all of the details. He enclosed the paper "Studies of the Solvent Extraction Behavior of the Transition Elements" by Peppard, Faris, Gray, and Mason of the Argonne National Laboratory, along with the comments of two referees. I read the manuscript and this morning wrote Noyes that I agree with the comments of the referees and feel that Referee I (Garner) offers a good plan of action, especially in paragraphs 1, 2, and 3.

I also wrote to Dick Diamond (Harvard) about his corrected manuscript that he recently sent me. I said, "Street and I will go over them in the time between now and declassification (which unfortunately may allow us more than ample time)." I told Dick that we will send him a copy of the Hugus and Connick manuscript when it becomes available, noting that their ideas differ from those of Glueckauf. [Our declassification problems have certainly not eased yet.]

A note went to Fred G. Sawyer (Stanford Research Institute), saying that I have delayed replying to his request for a suggested date for me to receive the honorary award from the Los Angeles Chapter of the American Institute of Chemists in order to see whether I could fit it in to coincide with other business. I said that my schedule is not sufficiently crystallized to do this so that rather than delay further, I will suggest Friday, September 26. I asked Sawyer to let me know if this is satisfactory.

I also answered a letter from L. Jackson Laslett (Iowa State College) and said that I am sorry that he will be unable to attend the Gordon Research Conference on Nuclear Chemistry. I added that I should like to see Dr. D. S. Martin and Dr. A. F. Voigt attend, but they should get their applications in as soon as possible.

The final draft of the article, "The Man Who Sparked the Atom Bomb," arrived from John Voelker, who said he plans to submit it to Harper's Magazine. Voelker writes in a very readable manner, but I am afraid his article is still a bit excessively flattering. He began:

Today, at 40 he could play the part of Abe Lincoln without makeup. Like Lincoln, he is an authentic American genius. Like Lincoln, he is as modest as an old shoe. His accomplishments in the field of nuclear science are more fabulous than the wildest science-fiction flights of space ships and interplanetary rockets. He is today a Buck Rogers of the world of nuclear science. The scope of his research is timeless and spaceless: to discover the very guts of the universe itself; what force it is that makes the world tick. His name is Glenn T. Seaborg. Part of his story can now be told...

Voelker went on to say that he will be delighted to see me if my commitments allow me to visit Ishpeming this summer. He wrote, "In any case if you will drop me a timely line I will start saving brook trout so that you and your family can have at least a taste."

In today's mail was a letter from W. George Parks, saying that he has added Ed Fleming's name on the list of requests for attendance at the Conference on Nuclear Chemistry. Parks noted that he believes that an outstanding program has been arranged and that he is certain the Conference will be successful under my direction and supervision.

I received a nice note from Ed Mack, Jr. (Ohio State University). Mack wrote that he regretted very much that he had to be away when I was at Ohio State. He wrote, "I have heard many enthusiastic accounts of your visit and many wishes expressed that you could be here long and often!" Mack said that he was sending me 50 printed copies of my lecture ("Thermodynamic Properties.....") with the compliments of the Chemistry Department.

M. C. Sneed (University of Minnesota) wrote that my chapter, "The Actinide Series," for Comprehensive Inorganic Chemistry, which arrived a few days ago, contains exactly the material that they hoped to have presented. He then said, "Within a few days you will receive a small token of appreciation. It is hoped that your score will be improved by this evidence of our good wishes."

Also in today's mail was a lengthy letter (dated May 6) from Robert M. Underhill (Secretary and Treasurer of The Regents):

The Regents' Committee on Atomic Energy Commission Projects met at Davis on April 25 and discussed the matter you had taken up directly with Regent Neylan, Regent Pauley, and Regent Ahlport, namely, that you and your associates in the matter pending before the AEC Patent Compensation Board wanted the Regents to further modify their communications to the Commission and its Patent Compensation Board. In particular, the Committee was informed that your counsel, Mr. Lane, and your associates desire to reduce the Regents to the status of a licensee.

The Committee reviewed in detail the correspondence with the Patent Compensation Board and the background material which was considered when the letter of October 8, 1951 was very carefully prepared. The Regents' Committee felt that it could go no further in the matter, and decided to report the situation to the Regents at the full Board meeting that afternoon.

It was reported to the Board, and the letter of October 8, 1951 was read in its entirety. The position of the Regents as public trustees and the obligations inherent in this trust were brought out in the discussion. The Regents felt that in the letter of October 8, 1951 they had done everything possible to clear the way for the inventors in their claim for compensation. You, your associates, and your counsel undoubtedly have copies of that letter, and it seems only proper to summarize it:

In that letter the Regents claimed to have an interest, but did not claim to be the sole owner; they definitely stated that the inventors had made discoveries of value and owned legal title to the inventions; that the inventors had legal title and could assign with the consent of the Regents, which was given; they expressed the

opinion that, although they had an interest, which interest was not defined, the inventors were entitled to such compensation as would be provided under the Atomic Energy Act of 1946; and they expressly waived the right to any compensation for the University. The form of disclaimer, which the Regents indicated they would sign, clearly leaves the way open for the inventors to obtain compensation, and it precludes the Regents from ever directly entering into the matter or from making a claim against the Commission or the inventors.

With all this background, the Regents felt that they had taken no steps adverse to the interests of the inventors and that it would not be necessary for the Regents further to define such interest as they claim to have or to change the form of disclaimer.

To satisfy all the parties and the attorney, a number of whom live at a distance from Berkeley, is quite difficult by conversation with one, or possibly two, of the parties.

Since you are represented by counsel, and counsel has informed you of the situation, the Regents believe that he, after reading this letter explaining the position of the Regents, should attempt to set down in writing exactly what proposal you desire to bring before the Regents for consideration and the exact form of statement the parties wish made.

I asked Doral to have copies made of this letter for Segrè, Wahi, Kennedy, and Lane. Then I got together with Emilio to discuss it and our response. We wrote to The Regents, "We have received your letter of May 6, 1952 after we had sent you ours of May 2, 1952. Since our letter essentially covers the points raised by you, I and my associates respectfully request that you consider it as an answer to your letter."

Thursday, May 8, 1952

The research group meeting this morning was attended by Asaro, Carniglia, Carr, Clark, Cunningham, Dauben, Dunlavey, Feay, Glass, Gunn, Hoff, Hollander, Hulet, Jaffe, Levy, Michel, Momyer, Nervik, Passell, Perlman, Rasmussen, Reynolds, Ruben, Seaborg, Shudde, Slater, and Templeton.

First to speak was Asaro, who said they had taken another look at U^{232} on the alpha particle spectrograph and found the results to agree with previous work showing an energy separation of 58 kev for the two alpha groups. He said they found the abundance of the lower group to be 31% rather than 26% as previously reported and this agrees well with the value of 30% reported by Dunlavey from his determinations of alpha-electron coincidences in nuclear emulsions. There was some discussion about this and about planned future work.

Momyer reported on the study of Em^{208} and Em^{221} . In the case of Em^{208} they bombarded thorium with 340 Mev protons and found a small peak in the pulse analyzer data at 6.13 Mev (between the Em^{210} and the Em^{212} peaks), which appears real. He said they may be able to obtain evidence for the mass assignment by milking Po^{204} from the emanation fraction. Momyer then described experiments that gave the alpha

branching of Em^{221} as about 25% and thus an alpha half-life of about 90 minutes. He reported unsuccessful attempts to milk Po^{217} from Em^{221} by recoil and so to determine its half-life.

Clark described his experimental setup for his x-ray diffraction work on the higher hydrate of lanthanum trichloride, $\text{LaCl}_3 \cdot n\text{H}_2\text{O}$. In the discussion Dauben said that Clark has tried to investigate the decomposition of this hydrate and found two or three lower phases, one being the same as previously prepared and reported by Rollier.

Rasmussen reported their attempts to detect thermal neutron (n, α) reactions by the photographic plate method to have been unsuccessful. He explained that all of the plates, returned from the Argonne pile and developed, were completely blackened. Rasmussen suggested that the experiment might be successful if the plates were developed immediately after irradiation.

At my suggestion Rasmussen told the group that Thompson and Magnusson, while in Chicago, bombarded a sample of one microgram of americium (98% Am^{243} and 2% Am^{241}) in the Argonne pile, did extensive chemistry, and confirmed the original work of Thompson and Street that Am^{244} is a 25-minute negatron emitter. It took so long to perform the chemistry that they were unable to get a good beta-gamma spectrum in the scintillation spectrometer.

Michel gave a progress report on the new time-of-flight mass spectrometer, which is now working although the vacuum system is not satisfactory yet.

* * * * *

After I returned to the hill, I signed and mailed to Robert Underhill the letter that Emilio and I drafted yesterday. Later I decided to phone Underhill and see just what he had in mind. Underhill, in our phone conversation, said that he wanted a very detailed answer. I told him that I couldn't see what answer was required beyond our letter of May 6, 1952 and I doubt that we should try to draft an exact form of the statement defining the Regents' interest since such definition coming from us or Mr. Lane would result in criticism from the Regents. I then talked with Emilio, who said that he will try to see Regent D. H. McLaughlin (a member of the Regents' Committee on Atomic Energy Commission Projects) to explain that we didn't feel sufficiently competent and didn't sufficiently understand the Regents' interest to enable us to define it.

Later I wrote to Joe Kennedy and told him what had transpired and what I had done. I also sent a note to Lane, with the copy of Underhill's letter and my reply. I told Lane that I shall defer further comments until I have a chance to talk with him.

Also, in today's mail was a check for \$70.00 from Walter Yust (Encyclopaedia Britannica) for my article on the transuranium elements. I signed and returned the copyright release he included.

After Helen called and reported that the doctor proclaimed the

children non-contagious, I left work, with the best wishes of the office staff, and went home to help with the kids while Helen concluded our preparations for tomorrow's trip. Later my parents telephoned to wish us a good trip.

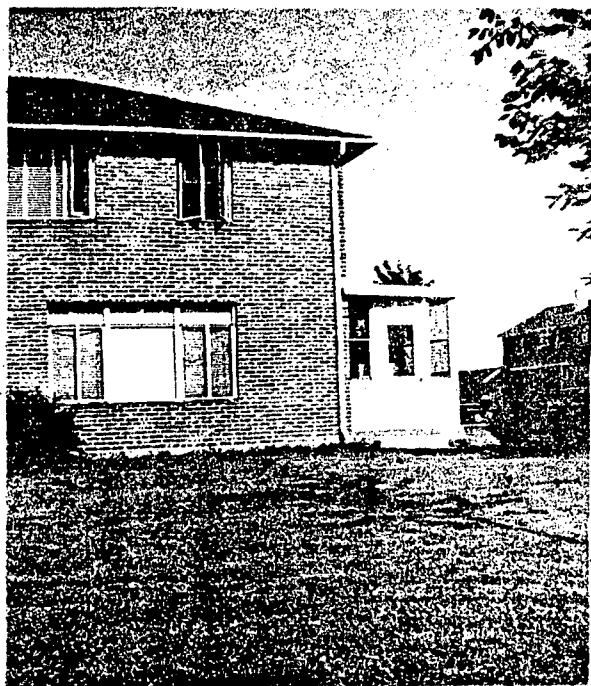
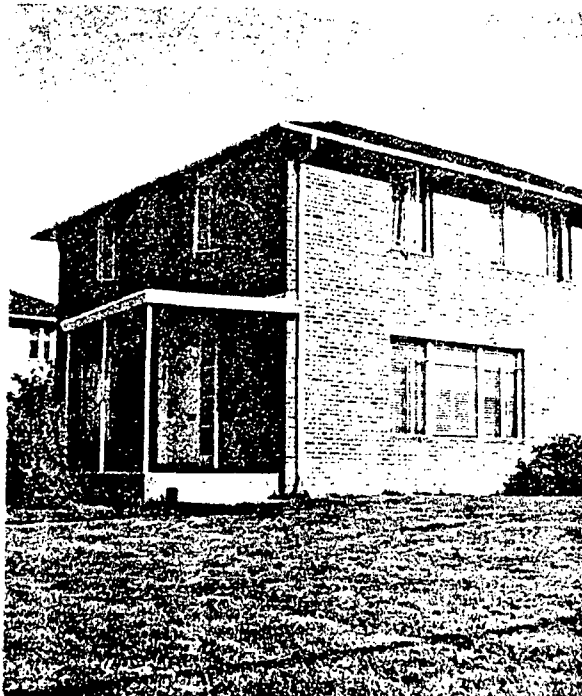
Friday, May 9, 1952

The Seaborg clan left Lafayette early, bag and baggage, to fly from San Francisco to Chicago for our approximately two-month stay.

The Berkeley group is being left in Iz Perlman's capable hands, Doral will send me any mail I need to handle, and the telephone is always available. The next two months promise to be fruitful for everybody.

Our trip to Chicago was as uneventful as such a trip can be with four young children. To add to the interest of the trip, Lynne managed to get airsick as we landed in Chicago. In Chicago we were picked up by Larry Magnusson and driven to our home at 2651 Western Avenue, Park Forest, Illinois (Phone: Skyline 5-5880). This is a housing development about 25 miles from the Argonne National Laboratory with a high concentration of Argonne employees; our home (two connecting apartments in a two-story building making it a six-bedroom, three-bath house) was formerly owned by the Philip Klutznicks. The laboratory furnished towels, bedding, dishes, and kitchen equipment. Someone from the lab left a few groceries in order that we could have a bite of supper and put the children to bed. While Helen took care of the children, Don Stewart came and took me to pick up more groceries and my new car.

The Housing Counselor at Argonne, Byron Kilbourne, arranged for the rental of this house and supplied us with details about the project, including such things as garbage collection, appliances, lawn maintenance, heating, no-pet rule, etc.



5651 Western Avenue, Park Forest, Illinois

Later Winston Manning telephoned to welcome us.

Saturday, May 10, 1952

In Chicago. The kids seem delighted with our new home and our new Plymouth.

My old friend, Steve Lawroski, stopped by to greet us and to invite Helen, the kids, and me to have a picnic supper with him, his wife Helen, and their children, Nancy (just a little older than David) and young Steve (a month older than our Stephen), at their home at 64 Elm Street, which is about 3 1/2 blocks from us.

Sunday, May 11, 1952 - Mother's Day

In Chicago. I spent the day with my family. Helen and I talked by phone with a number of old friends.

At 5:15 p.m. I left Chicago on American Flight 66, bound for Washington, D. C. This flight landed in Washington at 8:45 p.m., and I took a cab to the Hotel Statler and checked in.

Monday, May 12, 1952

In Washington. I took a taxi to the Pentagon in order to attend the 10-7 meetings all day.

When I saw French (Pete) Hagemann, who was also in Washington, I quizzed him about the limits of Mark Inghram's mass spectrographic analyses, which some of the Berkeley people had asked me to check on.

Donald Lane had dinner with me, and we went over in more detail my recent contacts with the Regents and Robert Underhill (I had already sent Lane correspondence on the matter). After a lengthy discussion, we decided that Lane should send a letter to Underhill, in view of the suggestion made in the last paragraph of Underhill's letter to me of May 6 (that our counsel should set down in writing exactly what proposal we desire to bring before the Regents for consideration and the exact form of statement the parties wish made). Lane said that he would draft a letter to Underhill and send it to the four of us for approval before mailing it to Underhill.

[In Berkeley a nice letter arrived for me from Jan Rydberg, who included a reprint of an article he had written for Elementa (in Swedish) about the Nobel Prize. Rydberg said the pictures he took were poor, but he sent the two best ones. He reported that he is continuing his work on organic complexes with heavy metals, which will eventually be published. Jan said that everyone there is waiting for a textbook on radiochemistry with a comprehensive part on techniques. He added his and Britta's best wishes to Mrs. Seaborg and the children.

I also received an acknowledgement from L. F. Eisenhart of my acceptance of election to membership in the American Philosophical Society. Eisenhart wrote that he is sending me under separate cover a copy of No. 1 of Volume 96 (1952) of the Proceedings.]

Tuesday, May 13, 1952

In Washington. Again I spent the day at the IO-7 meetings at the Pentagon. Later I caught American Flight 61 (4:30 p.m.) to Chicago. This arrived in Chicago at 6:10 p.m., and I was picked up by an Argonne driver and driven to our new home in Park Forest.

Helen said that she had telephoned a number of old friends and her cousin Francis while I was gone. Clarence and Francis Heppe, and their children--Butch, June, and Jimmy--are living in the Chicago area while Clarence is on temporary assignment as a pharmacist's mate in the U. S. Navy. Francis offered the family's services as baby sitters whenever needed. Helen also told me that she had made arrangements for a woman to come in to help her with the cleaning.

[In Berkeley a telegram arrived from Marvin Kalkstein, who has been visiting his parents in Brooklyn. It read, "STILL RUNNING HIGH FEVER CAUSE YET UNDETERMINED DO NOT KNOW HOW LONG BEFORE CAN RETURN PLEASE INFORM WHOMSOEVER CONCERNED."]

Wednesday, May 14, 1952

In Chicago. I spent my first full day at Argonne, talking with men about their work, etc.

I noted a carbon of an April 30 letter from W. B. Reynolds to J. H. McKinley (Business Manager, Argonne) about my consultantship here. Reynolds said, "In reply to your letter of April 23 regarding Dr. Seaborg's trip to Argonne during the period of May 1 to July 1, 1952, the arrangements as proposed are satisfactory, and we will bill you accordingly."

I made some arrangements for carpooling from Park Forest to Argonne with Harold M. Feder in the Chemical Engineering Division so that Helen may have the car part of the time. Feder and his wife Sally are close neighbors.

[In Berkeley a nice note, addressed to me, arrived from Leo Yaffe, thanking me and my colleagues for our hospitality during his recent visit. He made a special note to thank Perlman, Ghiorso, Hyde, and the Hollanders and said he hopes to soon be able to invite me to come to Montreal and that some of my group will also visit.

Iz responded to a May 7 letter from Errett C. Albritton (Editor of Handbook of Biological Data), who asked if we could prepare or suggest sources for tables for their Handbook on the biological effects of ionizing radiation and the physical properties of radioactive isotopes. Iz explained our only tabular material that might be pertinent is a new edition of our Table of Isotopes, which we expect to be ready by the end of the year for publication in Reviews of Modern Physics.]

Thursday, May 15, 1952

In Chicago. One of my obligations during this visit to Argonne is to give a number of lectures. I am reasonably prepared for this since I

sent a number of boxes of illustrative slides here in advance of my arrival and have made some notes. My first talk is scheduled for a week from Friday (May 23) on the general subject of alpha decay.

A note arrived from Doral, along with some correspondence that Iz asked her to send on to me. Doral also said that my paper with Ghiorso, Higgins, Larsh, and Thompson on "Spontaneous Fission of U^{234} , Pu^{236} , Cm^{240} , and Cm^{244} " was declassified and sent on to Goudsmit for publication in The Physical Review. Doral also mentioned that Jack and Margie planted our trees over the weekend (mugo pines in our front planter box). I wrote Doral a note and said we are pretty well settled, gave her my telephone numbers (home SKyline 4-7426, lab extension 2627), sent her my expenses for the Washington trip, said to tell everyone hello, and to give Jack and Margie special thanks for their agricultural efforts at our Lafayette home.

I replied to a May 8 letter from Donald F. Mastick (Scientific Adviser, Technical Information Service, AEC, Washington), who had asked some time ago about reprinting the "Table of Isotopes" in one of their documents. At the time I explained that it was in the process of being revised. Today I said that it should be possible to send him a copy of our material by about November 15. I asked if he wants only the section on the heavy isotopes or the entire table.

[In Berkeley, the research group met as usual with Perlman presiding and the following people in attendance: Asaro, Carr, D. Conway, Cunningham, Dauben, Dunlavey, Feay, Fischer, Glass, Higgins, Hoff, Hollander, Hulet, Hyde, Jaffe, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Street, and Templeton.

Asaro spoke first about some correspondence with West and Dawson, who claimed to have seen gamma rays of energies of 52, 37, 59, and 32 kev in the decay of Pu^{239} , in disagreement with Berkeley work. West and Dawson replied to Asaro's query, saying that they, in agreement with Berkeley work, have found that the 59 kev gamma ray is indeed due to Am^{241} . Both the 59 kev and the 32 kev gamma rays disappeared upon purification of their sample. They also found a 45 kev gamma ray due to plutonium but not to Pu^{239} . In the discussion Perlman mentioned work done by the Chicago people with high gt plutonium.

Perlman also talked about the decay scheme of Np^{238} , noting that it provides a check on a point in alpha decay theory because the alpha decay of Cm^{242} leads to excited states in Pu^{238} as does the beta decay of Np^{238} . He said that we want to know why, in even-even nuclides, only one excited state is seen. He presented the decay scheme of Chicago people, Freedman and Engelkemeir. Perlman said he believes the crucial experiment will be the beta-gamma coincidence work on the 1.23 Mev beta particle and the 100 kev gamma ray.

Templeton spoke to the group for a while on star tracks produced by cosmic rays in photographic emulsions.

Fischer said that, in order to check if the 4-hour positrons seen in the helium ion bombardments of praseodymium were due to deuteron contamination of the beam, she did a deuteron bombardment on praseodymium

but did not observe any 4-hour activity. She did see a 2.9 hour, ~0.9 Mev positron with probable mass assignment to Nd¹⁴¹; a 20.6 hour, ~2.5 Mev beta particle with probable mass assignment to Pr¹⁴²; and a 3.35 day activity probably assigned to Nd¹⁴⁰. Fischer also talked about the column separation of an old sample of rare earths presumably produced in Am²⁴¹ neutron-induced fission. She saw a 61.2 ± 3.0 day activity (presumably 61-day Y⁹¹); a 67.2 ± 3.5 day, 0.4 Mev beta activity (presumably 74-day Tb¹⁶⁰); and a 3.7 ± 1.0 year europium activity with 0.06 Mev, 0.3 Mev, and 1.9 Mev beta particles (these may be due to 5.3-year Eu¹⁵², 5.4-year Eu¹⁵⁴, 1.9-year Eu¹⁵⁵ or maybe a mixture of these).

Fischer also reported on a spectrographic analysis of a poor column separation from a bombardment of ytterbium (about a year ago) with 120 Mev deuterons. The probable assignments of the peaks were identified as follows: lutetium peak (4-day Yb¹⁶⁷, 9.5-day Lu¹⁷¹, 38-day Yb¹⁶⁹), ytterbium peak (4-day Yb¹⁶⁷, 9.5-day Lu¹⁷¹ or Tm¹⁶⁷, 33-day Yb¹⁶⁹), thulium peak (4-day ?, 9.5-day Tm¹⁶⁷, 120-day Tm¹⁷⁰). Perlman added that most of these isotope assignments were made by Wilkinson and Hicks.

Rasmussen continued his report on Am²⁴⁴ that he began last week. He said that the use of the anthracene scintillation pulse analyzer showed the beta end point to be 1.3 Mev although there may be a certain amount of higher energy beta particles (the latest decay cycle prediction is 1.48 Mev). He is still working on his calculations.

Levy talked about his attempts to find a compound that will undergo a Szilard-Chalmers reaction to give a high specific activity of Bi²¹⁰. His Oak Ridge bombardment of tri- α -naphthylbismuthine resulted in a tarry mess that was hard to dissolve and had no appreciable enrichment of the specific activity. Levy added that the compound should be stable at the temperature at which the pile runs, and Perlman suggested that it be run in the pile at a lower temperature.

* * * * *

Friday, May 16, 1952

In Chicago. Again I talked with the men at Argonne.

An invitation arrived for Helen and me from Hoylande D. Young to attend a Chicago Section ACS dinner at the Furniture Mart on May 23, at which time William C. Rose of the University of Illinois will receive the Willard Gibbs Medal Award. Hoylande said that she has invited Maurice D'hont, the visiting Belgian chemist, to attend and that the Winston Mannings and the Harold H. Strains plan to come.

I also received a carbon copy of a letter from Donald Lane, addressed to Joe Kennedy. Lane told Kennedy about my conversation with him on Monday evening (May 12); he included a draft of his proposed letter to Underhill and asked for approval, by return mail, from Kennedy, Wahl, Segrè, and me. I looked the letter over and immediately sent Lane my approval, mentioning that the next meeting of the Regents is Friday, May 23, and he will probably have to get the letter off on Monday for maximum effectiveness. I also sent a brief note to Segrè and said that I thought

he should call Underhill in order to be sure that he will bring up this matter at the Friday meeting of the Regents.

Saturday, May 17, 1952

In Chicago. The kids seem quite happy here and are fascinated by a train (the Belt Line Railroad) that runs a few blocks from our house. Other Argonne people living in Park Forest, which is on the site of the old Indian Wood Country Club, include John McKinley (Business Manager of Argonne), Milton Ader (a chemist whom I have known since Met Lab days), and Harold Etherington (a reactor engineer).

Later I got together with Larry Magnusson, and we went to Shady Lawn Golf Course for nine holes of golf and conversation (LBM-52, GTS-51). This is a public fee course in Beecher, Illinois (on Dixie Highway, 4 miles south of Lincoln Fields Race Track).

Sunday, May 18, 1952

In Chicago. Helen and I took the kids for a ride around the area in our new Plymouth this afternoon.

Monday, May 19, 1952

In Chicago. In my mail today was a letter from Joe Kennedy. Joe said that Winston Manning recently called his attention to the fact that Art Wahl has not been nominated for the 1953 ACS Award in Pure Chemistry and this is his last year of eligibility. Joe wrote that he has prepared the nomination and asked if I want to write a supporting letter and send it (eight copies) to the Executive Secretary of the American Chemical Society in Washington before June 1, 1952. He sent a similar letter to Emilio. I wrote a quick note to Doral and asked if she would send me a copy of the letter of recommendation to the professorship that I wrote for Arthur C. Wahl some time ago.

Richard C. Vogel talked with me for a while about some work with a dibromide ester, which they find dissolves nearly every metal (zirconium, tin, uranium, iron, etc.) although not tantalum, platinum, etc. The method came into use in the steel industry where it was used to dissolve iron.

[In Berkeley a May 16 letter arrived for me from Robert A. Naumann (now a graduate student at Princeton), describing some work they have done on the 6.75-hour molybdenum activity produced by bombarding Cb^{93} with protons. Bob asked if he could come to the Radiation Laboratory about July 1 to attempt to determine the mass assignment with the mass spectrograph. (This is the sort of matter that Iz will handle.)

Emilio Segrè met with Regent Donald H. McLaughlin in San Francisco to talk with him about our patent problem and to make certain that it be discussed at the Regents' meeting on Friday. Emilio explained the reasons why the undefined interest of the Regents was an impediment to a satisfactory settlement with the AEC, and McLaughlin intimated that the Regents were worried mainly about patent policies with respect to the faculty; they were anxious not to make a precedent. Emilio went on to

say that this referred to past things and was excluded as a precedent. McLaughlin suggested that the interest of the Regents might be the gifts we will make to them (sic) and that he thought they might put this in writing. He said that possibly the Regents could assign all their rights, whatever they might be, to us in consideration of our prospective gifts. (Emilio's thoughts on this were that he doubts this will be acceptable to any of us.) McLaughlin noted that he saw the desirability to remove the impasse and said that he will do his utmost to this effect, but he thought with so many lawyers things tended to get complicated. He also did not seem particularly receptive to the proposal of Mr. I. Harter. Emilio went on to tell McLaughlin that we do not care very much which particular definition of their interest the Regents give as long as they give one consistent with their letter of October 1951. McLaughlin's comment on the wording of the disclaimer was that it is too technical a question for us, and Emilio agreed. McLaughlin also said that he expects that the Regents' Committee on Atomic Energy Projects will take up the matter on Friday.]

Tuesday, May 20, 1952

In Chicago. In one of my conversations I learned that Art (Arthur B.) Shuck is making plutonium napkin rings to be put in the MTR for neutron bombardment. The needed material is arriving from Los Alamos today.

At home our telephone number has been changed to an unlisted number--Skyline 4-7426. Helen and I have talked with some of my relatives. Helen spoke with Betty (Seaborg) Rigby (Uncle Henry's daughter), who lives on the north side of Chicago--we live about 35 miles south of the downtown section. Betty told Helen that they will come to see us some Sunday. Then Edith and Rudolph Ericson (Edith is my father's first cousin, the daughter of a sister of my father's father) were in the Chicago area recently, and Edith and I talked by phone for a while (Helen and I were unable to see them). I told Edith that I want to take the whole family to visit Ishpeming, and we tentatively agreed on the 4th of July weekend--she said she will arrange a dinner or something and will check with Uncle Henry and Aunt Minnie to make certain that all of my father's relatives are included.

Wednesday, May 21, 1952

In Chicago. I received a copy of Lane's May 19 letter to Robert Underhill, which he prepared after obtaining the approval of his draft by the inventors.

LEE B. KEMON
DONALD E. LANE
—
DAVID E. VARNER
SOLON B. KEMON

TELEPHONES
DISTRICT 3870-3871

LAW OFFICES OF
LEE B. KEMON
AND
DONALD E. LANE
PATENTS AND TRADE-MARKS
1331 G STREET, N.W., WASHINGTON 5, D. C.

May 19, 1952

Robert M. Underhill, Esq.
Secretary and Treasurer
The Regents of the University of California
240 Administration Building
Berkeley 4, California

Re: AEC Docket No. 7 - Seaborg et al

Dear Mr. Underhill:

Your letter dated May 6, 1952 addressed to Dr. Seaborg and stating the position of the Regents in the above matter has been shown to me by Dr. Seaborg. A copy of the letter of May 2, 1952 addressed to you by Dr. Seaborg and his associates, has also been furnished to me. I have been requested to supplement that letter with a more definite proposal for consideration by the Regents at the next meeting, May 23rd.

Progress of the subject claim before the Patent Compensation Board of the Atomic Energy Commission is stalled by reason of the fact that the question of title to inventions has not been completely resolved to the Board's satisfaction by the last communication the Board received from the Regents, dated October 8, 1951.

At a prehearing conference held February 19, 1952, the Patent Compensation Board suggested that counsel for the applicants negotiate further with the Regents in an effort to secure documents that would resolve the apparent remaining questions of title in order that the cause might proceed to a hearing upon the merits.

Specifically, the remaining questions for resolution are:

1. What is the nature of the "interest" claimed by the Regents in their communication dated October 8, 1951?

2. Will the Regents execute now and without modification the Disclaimer attached to the Memorandum of Agreement made September 17, 1945, between Seaborg et al and the Government of the United States?

Although the Regents' communication dated October 8, 1951 states that the applicants made inventions, that applicants own legal title to inventions, that applicants are entitled to just compensation and/or an award, that the Regents waives its rights to apply for compensation and/or an award, and that the Regents disclaim "compensation" for any of the inventions, such statements, in the opinion of the Patent Compensation Board, do not "clearly leave the way open for the inventors to obtain compensation".

In determining a dollar value for compensation for inventions, it is necessary for the Patent Compensation Board to determine first who owns what interests in inventions. A full ownership of inventions involves ownership of entire right, title and interest in and to said inventions. Obviously, applicants' title is less than full if the Regents continue to claim an undefined interest in the inventions. The problem in the subject proceeding comes in evaluating applicants' remaining interest, and this can be ascertained only after the Regents' "interest" is defined.

The Patent Compensation Board, AEC counsel, the applicants, and their counsel, are not in a position to appraise the "interest" claimed by the Regents until the claimed interest is in some way defined by the Regents.

All parties to this proceeding appreciate the responsibility of the Regents in protecting the rights and interests of the taxpayers supporting the University of California. The Patent Compensation Board also has a responsibility to federal taxpayers to limit compensation to the applicants to cover only applicants' interest in the subject inventions. It thus becomes important to the Patent Compensation Board and to the applicants to ascertain the precise nature of "interests" claimed by others than the present applicants for compensation.

If one is considering the purchase of property and is advised that a third party has an "interest" in the property, it is logical that the would-be purchaser should determine whether the third party interest is a right of way, a right to use, a lien for services rendered or supplies furnished, a tax lien, or ownership of a fractional part of the property.

The Patent Compensation Board realizes that it may be difficult for the Regents to define their claimed interest. I am certain that Board Member Isaac Harter's suggestion, that applicants ask the Regents if they would accept as their interest, a non-assignable, non-exclusive, royalty-free license to use the inventions, was made in a spirit of helpfulness, and was not made with any thought of reducing the Regents to a status without value. Such a license is a tangible interest in the inventions, and informed taxpayers should consider it more valuable than a mere undefined interest. Even if Mr. Harter's suggestion does not meet with the Regents' approval, it may help the Regents in determining what their interest is not, and hence point the way to what is the claimed interest.

It is impossible for the Patent Compensation Board or applicants' counsel to define for the Regents the "interest" claimed by the Regents. I think Mr. Harter's license proposal was prompted by the fact that the Regents' communication of October 8, 1951 states in paragraph 3 "that applicants do own legal title to said inventions and discoveries." The license theory is legally compatible with the Regents' statement on record that legal title is in the applicants.

In response to the last paragraph of your letter of May 6 to Dr. Seaborg, and in view of the foregoing explanations, the applicants propose that the Regents modify the communication of October 8, 1951 to AEC as follows:

A. For the sentence designated "1.", substitute:

"That the Regents of the University of California, a corporation, claims as its interest a non-assignable, non-exclusive, royalty-free license to use any of the inventions described in the application entitled AEC Docket No. 7, and does not claim to be the owner of any of said inventions;"

B. In the Disclaimer following the sentence designated "5.", strike out the words "to compensation".

It is respectfully requested that the above specific proposals be considered by the Regents at their next meeting, and that applicants be advised promptly thereafter whether these proposals are satisfactory, or be advised of the exact nature of statements which the Regents will submit to the AEC Patent Compensation Board to clarify the present situation.

Very truly yours,

Donald F. Lane

DEL.nk
AIRMAIL
cc: Dr. Seaborg, et al

[In Berkeley a May 16 thank-you letter arrived from Waldo Cohn reading in part, "I filed for my passport April 21 and received word that it was completed and ready for me two days ago. We leave for New York tomorrow and sail May 21 on the Elizabeth as originally planned (although I must pay my own way, it seems). At some later date I expect to be informed on the details of new security status but, after seeing what you and the 34 others replying had to say about me, I have no worries on that score. Thanks again for your response. It was most encouraging to me to know that I had friends when they were needed."

William F. Meggers (Chief, Spectroscopy Section, National Bureau of Standards) wrote on May 20 with a number of questions about the chemical and physical properties of the transuranic elements and asked that I or one of my assistants supply the answers.]

Thursday, May 22, 1952

In Chicago. I received some mail that Doral forwarded to me from Berkeley. She included a return air ticket from Chicago to which she added the comment, "...Among them is your air ticket from Chicago--or do you like Chicago that well?"

Doral also sent me a letter from G. R. Martin (University of Durham), explaining that he is interested in the Gordon Conference but cannot attend because of financial reasons [Doral had sent a copy of this letter to Coryell.]

Another letter was from D. R. Stein with questions about the reintroduction of analytical chemistry information into the Gmelin Handbuch.

I also received a thank-you letter from Donald F. Mastick for my letter of May 15. Mastick said that, if possible, he would like to include the entire table.

[In Berkeley the research group held its usual Thursday morning meeting. In attendance was Asaro, Carr, Clark, Dauben, Feay, Hollander, Jaffe, Koch, D. Conway, Levy, Michel, Momyer, Nervik, Passell, Perlman, Rasmussen, Ruben, Shudde, Slater, Street, and Templeton.

First to speak was Slater, who presented a curve of the excitation function of $U^{238}(d,p)U^{239}$. The curve went from 300 mb at 14.4 Mev deuterons to 7.4 mb at 174 Mev deuterons. He stated that the curve is similar to that obtained with Bi^{209} but the bismuth yield falls off faster with increasing energy by a factor of five or ten. The bismuth peak, he said, is about 100 mb and falls off to approximately 0.2 mb.

Passell said they have made one run on Np^{238} and two runs on Np^{239} and both nuclides showed a large number of conversion lines. He presented a rough graph of the electron lines in Np^{238} and said that the L_1 , L_2 , and L_3 lines check Browne's gamma determinations. Passell showed a partial tentative decay scheme for Np^{238} .

Koch described his experimental setup for the preparation of rare earth trihalides and oxyhalides.

Finally, Rasmussen said that the 1.3 Mev endpoint for the beta decay of Am^{244} corresponds to an allowed unfavored transition.

* * * * *

The Chemistry 223 class turned in their term papers today. Some of this year's titles are "An Adjusted Mass Equation for the Trans-Lead Region" by Stephen C. Carniglia, "On Nuclear Electric Quadrupole Moments" by Robert Carr, "Proton Binding Energies and Nuclear Shell Structure" by Walter E. Nervik, "Scintillation Counter Spectrometry" by Thomas O. Passell, "Neutron Capture Gamma Rays" by Monte H. Rowell, and "Beta Decay Energetics near Closed Shells" by Alex T. Wilson.]

Friday, May 23, 1952

In Chicago. Some of my conversations here at Argonne are about cooperative work between the Berkeley people and the Chicago men. For example, I have talked with Paul Fields about a request of Stan Thompson. Stan wanted me to find out if Mark Inghram would run Stan's sample of Pu^{242} on his mass spectrometer. Other talks are about similar queries.

Today I gave my first lecture before the Physics and Chemistry Divisions at Argonne. I discussed the relation between alpha energy and A and Z; the talk was illustrated with the usual slides.

Helen and I attended the ACS dinner at the Furniture Mart at which William C. Rose (University of Illinois) received the Gibbs Medal. We met a number of people, including the Belgian chemist Maurice D'hont who may visit Berkeley before returning to Belgium; we also reminisced with a number of old friends. The Heppe family came to babysit with our kids.

[In Berkeley there was a bad spill in Room 107, Bldg. 5 when some plutonium got out of Carniglia's box.]

Saturday, May 24, 1952

In Chicago. The weather has been cloudy and rainy lately, so I spent some time today studying the set of architectural plans for Building 70 (our central chemistry laboratory in Berkeley), which I received yesterday from Earl.

Helen and I left the children with a baby sitter, an older woman, in order to have dinner with some friends. Steve woke up before it was dark, but he was not upset when he saw this stranger (something about which Helen was quite concerned). The baby sitter just changed him and put him back in bed.

Sunday, May 25, 1952

In Chicago. All of the Seaborgs were invited to have dinner with Steve and Helen Lawroski and their children. The Lawroskis are moving to a new home in Naperville in about a week.

Monday, May 26, 1952

In Chicago. One of the first things I did at the lab today was to write to Doral about a number of things. I asked her to tell Earl that, in the Building 70 building plans, I notice that my office is still small and there appears no space for the long wall chart of isotopes; I also believe that both Perlman and I should have doors to the corridor from our laboratories. I asked that she send me copies of the minutes of our Thursday morning meetings (not marked secret) and that she have Asaro send a copy of a Pu^{239} alpha energy-abundance curve as determined with his alpha-ray spectrometer and his analysis of the Wagner-Freedman-Engelkemeir data in which he reassigned some of their gamma rays. Among my other requests, I asked for a number of slides that I used during my Foster lectures for a talk on high energy nuclear reactions that I will also give here at Argonne, plus a slide giving the distribution of fission products from the high energy proton bombardment of uranium. I also asked Doral to check with Hildebrand or Pitzer to see whether Brewer has been nominated for the 1953 ACS Award in Pure Chemistry. [I suppose when Doral gets these requests, it will seem to her that I have not left Berkeley.]

I wrote to Dr. Edgar M. Carlson (President, Gustavus Adolphus College, St. Peter, Minnesota) to explain that I have been unable to rearrange my schedule to attend their ceremony on the weekend of June 1. I said that I do hope that it will be possible another year.

I also wrote two supportive letters, one for Arthur C. Wahl and the other for Sherman Fried, for the 1953 ACS Award in Pure Chemistry. These were sent to Alden H. Emery (Executive Secretary of the ACS, Washington). In both cases the letters included material about the men similar to that I have written for them in the past.

Tuesday, May 27, 1952

In Chicago. Again I spent the day consulting with various men at Argonne. I also looked over my notes for tomorrow's lecture.

Wednesday, May 28, 1952

In Chicago. Today I got together and talked with Dave Karraker (former Berkeley graduate student). Dave happened to mention that they have found that the half-life of Eu^{152} is 13 ± 2 years and that of Eu^{154} is 16 ± 4 years rather than the old values of about 5 years.

In addition to talking with various people about their work, I gave my second lecture to the Physics and Chemistry Divisions. This was a continuation of my discussion on alpha decay, and today I talked about the relation between half-life and alpha energy. Again the talk was illustrated with slides.

Thursday, May 29, 1952

In Chicago. It was more or less a usual day, spent talking with various men.

[In Berkeley the research group met with the following people attending: Asaro, Carniglia, Carr, Carter, Clark, Cunningham, Dauben, Dunlavey, Feay, Fischer, Glass, Hoff, Hollander, Hyde, Jaffe, Kalkstein, Levy, Nervik, Passell, Perlman, Rasmussen, Ruben, Shudde, Slater, Street, and Templeton.

Hyde told the group about Mathur's work, saying that he has been applying the glow discharge plating method (first used with emanation isotopes by Momyer and Hyde) for investigating xenon and krypton activities. Mathur bombarded I^{127} with ~100 Mev protons, producing Xe^{127} (and less). Mathur saw I^{121-3} (neutron deficient isotopes found by Marquez) and deduced the properties of Xe^{121-3} : Xe^{122} (half-life about 15 hours), Xe^{123} (half-life ~ 8 hours), Xe^{121} (half-life 1-2 hours).

Clark reported that the green color previously reported in a $LaCl_3$ solution was due to copper or nickel contamination; a pure solution was subsequently found to have a yellow color. In response to Cunningham's remark that their $LaCl_3$ solutions were colorless, Templeton said that Clark's sample is highly concentrated; Templeton believes the color is definite.

Dunlavey reported that he detected no coincident conversion electrons in 6200-alpha events of Gd^{148} with an Ilford G-5 emulsion.

Rasmussen spoke briefly on some correlations he has made about the first excited levels of some nuclides (Ce^{140} , Nd^{142} , Sm^{144}).

Passell reported that the approximately 100 keV gamma ray in Np^{238} is now known to be in coincidence with the hard beta particle. He said they still have a 200 keV gamma ray that is unreported elsewhere. He presented their present, very tentative, decay scheme. In response to a question as to the objection to the Freedman-Jaffey decay scheme, Perlman said that they earlier were reluctant to assign electrons to M and N conversions, but they now believe such conversions are fairly common.

Fischer spoke again about the 4-hour activity repeatedly seen in the bombardment of Pr^{141} with helium ions. With additional chemistry the activity came through after the first column volume. Templeton suggested that it may be due to Sc^{43} produced from a potassium impurity in the praseodymium; Fischer said she will check, but she is almost certain that the spectrographic analysis showed no appreciable potassium impurity.

Jaffe talked about their observance of a gamma ray of 57.60 keV in low abundance and a five times greater abundance gamma ray of 61.83 ± 0.3 keV gamma ray in Np^{239} decay. Jaffe said they saw two very weak lines that may be the second order K x-rays of plutonium. They saw an ~170 keV gamma ray (first order) in very high abundance (Pb absorption measurements). The sample has been given to Don Martin for observation.

Asaro compared the excited levels of Pu^{239} as found from the complex structure in the alpha decay of Cm^{243} and the beta decay of Np^{239} .

* * * * *

Friday, May 30, 1952

In Chicago. I spent this Memorial Day with the kids and Helen. We drove the children around some of the Chicago area, particularly our old haunts around the University of Chicago.

Winston and Dorothy Manning invited Helen and me to have dinner with them and their children, Stephen and Joan, in their home in Downers Grove. Again the Heppes babysat for us.

Saturday, May 31, 1952

In Chicago. This morning young Pete had a telephone call from Candy Way, who has been living with her sister in Illinois since she recovered from her illness. Pete was delighted to have Candy wish him a happy birthday. Helen then had a rather long conversation with her; it was apparent that Candy would like to return to California and eventually go to business school. Helen asked if she would like to work for us during the rest of our stay in Chicago and then return with us to California, and Candy agreed.

Fred and Edrey Albaugh (Fred was a UCLA and a Met Lab colleague who is now employed at Hanford, and Edrey was my secretary during the Met Lab days) are vacationing with Edrey's parents (Royal and Marie Smith, 9514 South Damen Street, Chicago), along with their two young sons, Jimmy and Jeff. The Seaborgs spent a fine day at the Smiths, reminiscing and celebrating Pete's sixth birthday and Jimmy's second birthday.



Glenn and Peter Seaborg and Jeff Albaugh, 9514 South Damen St., Chicago, May 31, 1952



Peter Seaborg and Jeff Albaugh, May 31, 1952



Stephen Seaborg and Jimmy Albaugh, May 31, 1952



Stephen Seaborg, May 31, 1952

Sunday, June 1, 1952

In Chicago. This was a family day, and Helen and I again took the children sightseeing.

Monday, June 2, 1952

In Chicago. Most of my day was spent at the Institute for Nuclear Studies and the Department of Chemistry of the University of Chicago, where I talked with such old friends and acquaintances as Nathan Sugarman, Tony Turkevich, Bill Libby, Thorfin Hogness, Warren Johnson, and Fraser Young. I also attended the seminar at the Department of Chemistry at which I am scheduled to speak next week.

Tuesday, June 3, 1952

In Chicago. Much of my day was spent consulting with various research people. In today's mail was a May 28 letter from Emilio Segrè:

I have talked over the phone with Mr. Underhill who has told me that the Regents are unwilling to do anything of the things we requested.

We will receive an official communication in which I think they will mention their unwillingness to take a license as representing their interest as proposed by Lane, omitting all the rest.

I would proceed under the assumption that the Regents will not add anything to their letter of October 1951 and I would make the point that they have an interest which obviously has no value because they disclaim any compensation, and being a body so solicitous of the public interest, they would never give away without compensation a thing which has some value. Moreover, they recognize that we have title to the applications etc., etc.

If a body of lawyers claims an interest of no value and undefinable, I do not see why other competent lawyers should take it too seriously.

This is of course a laymen reasoning, but I would follow a line of this type, and if necessary modify our request for compensation.

Underhill repeated to me the old story that Anderson is pushing Harter to make impossible requests, that the Regents are willing to do anything for their dear faculty and that the Patent Comp. Board and the AEC are only desirous to rob us.

I hope you have talked to Lane of the things to do in case the Regents refuse to answer and define their interest. For instance should we not file our request of a definition of their interest and their reply with the Patent Comp. Board. Could not this be a good method of showing their hand?

Let me know if you get any bright ideas, but I would proceed under the assumption that they will not write anything any more, that we have however enough to show that they have no claim and a comparison of their various letters (1950) will rather discredit them.

I also received a letter (May 29) from Doral with answers to some of the questions I posed in my last letter. She also passed on information from Earl about the financing of the new building (70). Earl said there will have to be commitments on funds within 30 days or lose the \$800,000 appropriations. The closest estimates of the architects and mechanical engineers, he said, are that the building can be built for about \$1,300,000, and the sum would leave no money for landscaping, moving of equipment, paving, furniture, etc. which is estimated to come to about \$250,000. Doral reported that Earl said that Al Wilson is worried about how to squeeze this amount out of the building cost--at present it looks impossible. She also wrote that the building plans are scheduled to be discussed with the AEC office in Chicago soon. On other matters Doral wrote that Jane (Waite) had sent off the slides I requested except for the one giving the distribution of fission products from the high energy proton bombardment of uranium, which will be ready for mailing on June 2. She reported that Pitzer sent off a letter last week nominating Leo Brewer for the 1953 ACS Award in Pure Chemistry and that they will send me copies (not marked SECRET) of the minutes of the Thursday morning meetings from May 15th on next week. Doral sent me some information about transportation from Boston to New Hampton. She included some old prediction data that Margie asked to be sent to me and said that Charles Browne will send information on Np^{238} on Monday. Finally, Doral asked if I want the Daily Cals sent to my home address.

Wednesday, June 4, 1952

In Chicago. Before visiting the labs, I wrote a couple of letters. One went to D. R. Stein (American Representative of The Gmelin Institute of Inorganic Chemistry). I told Stein that I agree with the suggestion of including analytical chemistry in the Gmelin volumes with a maximum space allocation of 4%.

I also replied to Emilio's May 28 letter. I said, "Mr. Lane and I discussed possible courses of action in the event the Regents refuse to define their interest, and our conclusions were along the same lines as suggested by you in your letter. However, it would be very helpful if they would write a letter in which they expressly state that they are unwilling to define their interest, and you might see whether you can get them to write such a letter. In other words, in the official communication which you said was forthcoming, it would be very helpful if they will comment on the general proposition of defining their interest and not merely on their unwillingness to take Mr. Harter's specific suggestion. In any case, I believe it would be better if this letter were addressed to Mr. Lane since he was requested by the Patent Compensation Board to communicate with the Regents."

Later in the day I gave my third seminar to the combined Chemistry and Physics Divisions on alpha decay. This time I discussed examples of even-even cases, the effect of odd nucleons, techniques, etc. Again the talk was illustrated with slides.

Thursday, June 5, 1952

In Chicago. In addition to consulting with various people at Argonne, I looked over my material and slides for my lecture in Madison

tomorrow.

[In Berkeley, the research group met as usual with the following people in attendance: Asaro, Carr, Carter, Clark, Cunningham, Dauben, Dunlavey, Feay, Lawrence E. Glendenin (visiting from Argonne), Hoff, Hollander, Huffman, Hyde, Jaffe, Kalkstein, Kofstad, Levy, Michel, Momyer, Nervik, Passell, Perlman, Rasmussen, Skirvin, Slater, Ellis P. Steinberg (visiting from Argonne), and Templeton.

Asaro talked about recent work in which they looked at the alpha spectrum of Ra^{223} in the hopes of finding an alpha group of energy higher than that presently known since alpha systematics predicts such a group. The last investigation was by the Frenchman, S. Rosenblum, in 1937. Asaro described their chemistry and said they found the same alpha particle groups as Rosenblum plus one of higher energy: 5.751 Mev (9%), 5.719 (51.5%), 5.607 (28%), 5.540 (9%), 5.433 (2-3%). He said that emanation activity that diffused through the machine during the run resulted in some random tracks on their photographic plate and leaves the relative abundances, not the energies, in some doubt. Asaro said they looked for and found the alpha groups of the emanation and bismuth daughters.

Clark announced that the green color of their $LaCl_3$ solutions was found to be due to copper impurity, but they found that concentrated solutions purified of copper still show a definite yellow tinge.

Skirvin talked about the determination of excitation functions for Ac^{225} and Ac^{226} produced in bombardments of thorium by protons ranging in energy from 100 Mev to 340 Mev. He described the bombardments and the chemistry (Meinke's procedure) and said the cross sections vary from 4 mb at 100 Mev to 14 mb at 340 Mev for Ac^{226} and from 6 mb to 20 mb for Ac^{225} .

At this point Perlman announced that the linac is now working consistently with very well-defined proton beams of 1/4 microamps.

Michel told the group that they now have a vacuum in the new time-of-flight mass spectrograph and are now checking transmission efficiency. They hope to use the machine for the separation of plutonium isotopes, separation of isotopes in connection with determinations of fission cross sections, and mass assignments of various active nuclides.

Passell talked about work on Np^{239} with the beta-ray spectrometer. Since they did not know where the beta spectrum was under all the hash, they made a Fermi plot from the raw data without attempting to subtract out the conversion electrons; this indicated a beta-particle energy of 0.200 Mev. Passell also presented a decay scheme worked out by Peter Stevenson and Harry Hicks for Cu^{67} from data obtained with the beta-ray spectrometer. Passell said that Stevenson and Hicks proved that the 0.580 Mev beta particle and the 96 kev gamma ray are in coincidence.

* * * * *

Friday, June 6, 1952

In Chicago. Today is Helen's and my tenth wedding anniversary, but we will not have a wild celebration since I am going to Madison this afternoon. I spent the morning in my office at Argonne, where I wrote to J. W. Perry (Massachusetts Institute of Technology) to decline an invitation to attend a symposium on "Machine Techniques for Information Selection."

I also returned a summary of information about the transuranium elements to William F. Meggers (National Bureau of Standards). I explained to Meggers that no summary listing the discoverer of all the different isotopes of the transuranium elements has yet appeared although such a summary will appear in the forthcoming Volume 14A of the National Nuclear Energy Series.

I received a couple of acknowledgments from R. M. Warren (Assistant Secretary, ACS) for my letters of support for the nomination of Sherman Fried and the nomination of Arthur C. Wahl for the 1953 ACS Award in Pure Chemistry.

A June 3 letter arrived from Vera Kistiakowsky Fischer, describing her latest work on the 4-hour activity observed in the helium ion bombardment of praseodymium. She mentioned that Earl Hyde suggested that it might be due to Sc^{43} or Sc^{44} produced from potassium contamination of the praseodymium and said she checked the spectroscopic data, which makes this seem unlikely. [Vera has terminated in Berkeley and has accepted employment at the Naval Radiological Defense Laboratory at Hunter's Point in San Francisco.]

I also received a letter from Doral. Since Doral brought up a number of items, I telephoned Berkeley and talked with her and Iz. Doral had included the proofs to my Nobel Lecture, which she and Millie had proofed, for me to look over before returning to Arne Holmberg. We discussed the number of reprints. With regard to the demand for the reprints of my Ohio State University lecture, I told her to have the talk multilithed for our use. Doral gave me the schedule for Perlman, Cunningham, Thompson, and Ghiorso to get to the Gordon Conference and said that Hyde plans to leave on the June 16th and may have his in-laws car to drive everyone (and golf clubs) to New Hampton. We also talked about arrangements for my flight to Boston. I gave Doral some instructions about bank notices for Margie to send from our house. Among the things I discussed with Iz were the original drawings for Chapter 17 "Correlation of Properties as Actinide Transition Series," which he will send me.

At 2 p.m. (Chicago time) I left Union Station on the Chicago, Milwaukee, and St. Paul Railroad train bound for Madison. I haven't been on a train in some time, and the trip was rather relaxing. John Willard met me in Madison at 3:48 p.m. (their time), and I spoke to the Wisconsin ACS Section at 4:30 p.m. on "Present Status of the Transuranium Elements." The talk was well attended (it had been publicized at The Medical School by Dr. Charles Heidelberger) and well received. At the dinner following the talk I talked with many old acquaintances (Farrington Daniels, et al.) and met a number of new people.

Saturday, June 7, 1952

I spent the morning talking with people in the Department of Chemistry of The University of Wisconsin and then took an afternoon train back to Chicago.

Sunday, June 8, 1952

In Chicago. The Seaborgs spent the day with my old friend, John Lewellen, and his family. John, who was the Executive Producer for the Quiz Kids radio program, lives with his wife, Wilma, and children, Luann and Tom, at 400 Hill Avenue, Glen Ellyn, Illinois. He is now the Executive Producer for the TV show "Down You Go" and today asked me to appear as a guest on the show. Today we had a picnic-type lunch, and John took Pete, Lynne, and me for a ride over the Chicago area in his small private airplane.

Monday, June 9, 1952

In Chicago. Today I spoke to members of the Chemistry Department at the University of Chicago on "Present Status of Research on the Transuranium Elements." This topic always evokes enthusiasm from the audience. I also visited people in the Institute for Nuclear Studies.

Tuesday, June 10, 1952

In Chicago. Much of the day was spent talking with the fellows in the lab about their research. I saw Dave Karraker who gave me a new value of 94 years for the half-life of Sm^{151} ; however, this has a large error of perhaps 30 years. Dave said that he will publish the work, along with R. J. Hayden and M. G. Inghram.

The following teletype arrived from Earl Hyde: "STREET, CRANE, THOMPSON, AND I HAVE NOT YET HEARD FROM PARKS REGARDING RESERVATIONS AT GORDON RESEARCH CONFERENCE. WE ARE DEFINITELY PLANNING ON COMING, BUT A CLARIFICATION WOULD BE DESIRABLE. WERNER, BALLOU, AND OTHERS IN BAY AREA ARE ALSO ANXIOUS."

At 3:30 p.m. I spoke in the Chemistry Building Auditorium on "Production and Separation of Transuranium Elements." This talk was of a more general nature than the ones I have presented in my seminars to the Physics and Chemistry Divisions since the entire laboratory was invited to attend. Such lectures of general interest to the entire laboratory are presented from time to time here at Argonne.

Wednesday, June 11, 1952

In Chicago. I made some telephone calls about the status of the Gordon Conference reservations and later telephoned Berkeley to let everyone know their reservations were ok. I was brought up to date on some of the things going on in Berkeley, and Doral described the mail, some of which she has forwarded to me.

I filled out and mailed an evaluation sheet for Vera Kistiakowsky Fischer to the Board of U. S. Civil Service Examiners for Scientists and

Engineers, generally noting that she rated as average or above average in knowledge and ability, etc. In a description of my observations of her relations with superiors, associates, and subordinates, I wrote that in general her relations have been quite good, qualified only by occasional flashes of temper and that she is an interesting person.

In the mail received at home was a short letter, dated June 9, from Emilio Segrè. He wrote, "I have your letter of June 4 and I have phoned Mr. Underhill today. He said that he will talk to the Chairman of the Atomic Energy Commission of the University and let me know the answer. I am fairly sure that he will refuse to write anything. However, it seems to me that for our purposes the letter he wrote some time ago, together with Lane's letter addressed to him and his official reply, which should by now be in your possession, constitute enough material amply to prove our thesis. Otherwise, Mr. Lane could write him another letter saying that the only communication we had had in reply to the letter of Lane was the official transcript from the Regents' meeting by Underhill and asking whether he would condescend to answer Lane's letter." Segrè went on to say that there can be no doubt that we have asked repeatedly for a definition of their interest and that they have refused to answer.

Thursday, June 12, 1952

In Chicago. I spent the day at the Summer Symposium of the Division of Physical and Inorganic Chemistry of the ACS at the University of Notre Dame. The topic was "Isotopic and Exchange Reactions."

[In Berkeley, the meeting of the research group was attended by Asaro, Biller, Carniglia, Carr, Carter, Dauben, Dunlavey, Gunn, Hollander, Hyde, Kalkstein, Levy, Rasmussen, Ruben, Shudde, and Slater.

Dunlavey said that he observed about 6700 alpha particles in an investigation of the complex structure of U^{234} . He reported that 25% had conversion electrons corresponding to a gamma ray of about 45 to 50 kev; 75% had no conversion electrons. He was not able to confirm the 117 kev gamma rays that Asaro saw previously.

Shudde described a photographic method for observation of short-lived alpha emitters.

Asaro talked about the use of Don Martin's gamma ray pulse analyzer to look at the gamma rays in the Ra^{223} chain. They are planning to see the gamma spectrum (4-second half-life) of Em^{219} . Asaro also mentioned the work they have been doing on the alpha and gamma spectrum of several even-even alpha emitters. He said that generally there have been two alpha groups, but they have observed two gamma rays in the case of U^{234} , U^{236} , and Pu^{238} , which is evidence for three alpha groups. He also gave evidence for two gamma rays (45 and 147 kev) accompanying Cm^{242} alpha decay.

Slater talked for a while about an experiment he proposes to do to determine the d,p and the d,n excitation functions of Th^{232} in order to circumvent the ordinary difficulties encountered in the chemical purification involved in such an experiment.

* * * * *

Friday, June 13, 1952

In Chicago. There were continued conversations and discussion with the research people at Argonne. One thing I am trying to encourage during this visit is continued collaboration between Berkeley and Argonne.

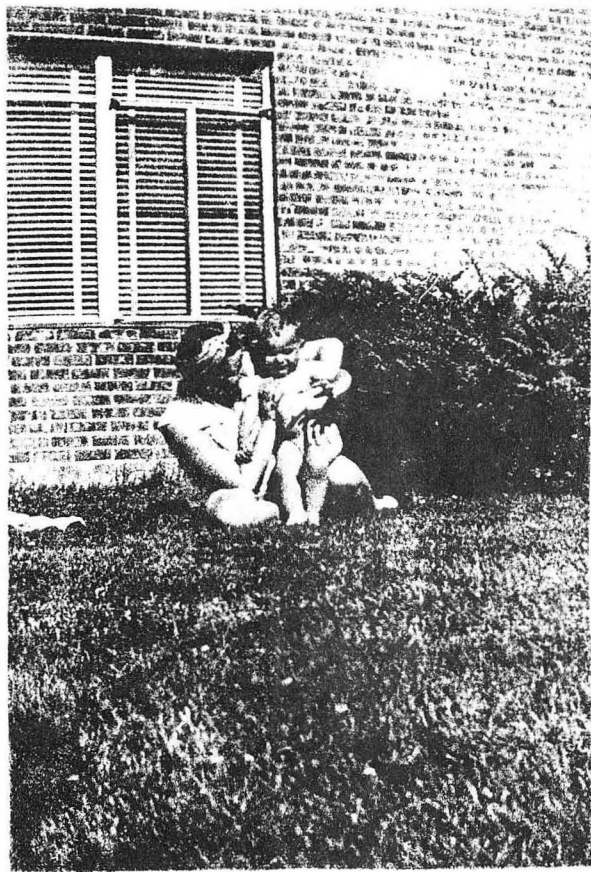
Saturday, June 14, 1952

In Chicago. This was a family day. Helen and I have contacted my Uncle Henry Seaborg in Ishpeming about a visit there sometime during our stay here in Chicago and have tentatively made arrangements to go up there during the 4th of July holiday--assuming that the kids are reasonably healthy.

Life has become much more relaxed for Helen since Candy Way has again come to live with us. The family adventures have included a trip to the Brookfield Zoo.



Peter and Stephen Seaborg, Brookfield Zoo, Summer 1952



Candy Way and Stephen Seaborg
at home at 2651 Western Avenue,
Park Forest, Illinois



Stephen Seaborg



Peter and Lynne Seaborg, 2651 Western Ave., Park Forest, Illinois

Sunday, June 15, 1952

In Chicago. Helen and I hosted a reunion of Argonne people who used to be at Berkeley. The group included such people as Don and Dorothy Stewart, George and Natalie Barton, and Larry and Betty Magnusson and those men being trained here for their jobs at the Savannah River Plant--Jim Wallmann, Don Orth, Bill McDonnell, Bob Folger, and Dave Karraker.

Monday, June 16, 1952

In Chicago. I again had conversations with the researchers about their work.

The mail brought me a June 13 letter from Brigadier General D. J. Keirn (Director AFOAT,1) and Doyle L. Northrup (Technical Director, AFOAT,1), stating that Volumes 2 and 3 of the final report on Project Crave (IO-7 Panel of the Research and Development Board) are being forwarded to me. The letter stated that the comprehensive nature of these volumes results in their being unusually sensitive from a security standpoint and that it is requested that the copies be returned unless a real need for a continuing reference to them is determined to exist.

I also received a letter from Charles Coryell regarding details about the Gordon Conference, particularly about the uncertainty of handling the expenses

of some of the people coming. Coryell suggested that final decisions be postponed until I come to New Hampton. Coryell also commented on his inability to locate golf sets and on the transportation problems from Boston to Franklin, New Hampshire on Sunday night. I immediately wrote back and said I agree with him about the expense money, mentioning that I believe that Dr. A. L. Thompson of McGill will also want some expense money. I told Coryell my travel schedule and said that he shouldn't worry about transportation since it seems convenient to come up on the 9:00 p.m. train. I wrote that we will probably be able to round up golf clubs after we get there and that I believe Hyde has decided not to drive so that the Berkeley group may also have to go up on the 9:00 p.m. train.

Today I gave a seminar before the Chemical Division and the Nuclear Chemistry Group. This one was entitled "Similarity of Nuclear Properties between Rare Earth and Actinide Nuclides."

At home I found a letter from Jerome D. Luntz (Executive Editor, Nucleonics), who sent me copies of correspondence about their tentative plans for reporting on the Gordon Research Conference on Nuclear Chemistry. Luntz said he plans to attend the first two days of the conference.

[In Berkeley a thank-you letter arrived from The Svedberg for the photograph I sent him. He said the picture has much character and they all love to have it. He also sent me a photograph of himself. He thanked me for our kindness to Helge Tyrén and said their cyclotron is now running fairly well, 190 Mev and about $1/2 \mu\text{A}$ at 100 pulses/second.

The Chemistry 223 class had the following final examination:

CHEMISTRY 223 FINAL
June 16, 1952

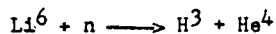
Masses may be obtained from Friedlander and Kennedy.

1. (10)

- (5) (a) The excitation functions for deuterons on bismuth show that the d,n reaction cross section never exceeds 50 millibarns while that for the d,2n reaction reaches 800 millibarns at the peak. Explain.
- (5) (b) In a light element the excitation function for the p,pn reaction is quite flat in the energy range well above the energy threshold. However, the p,2n excitation function goes through a definite maximum and decreases with increase in energy. Explain.

2. (15)

What will be the range in air of tritium ions formed in the reaction



with thermal neutrons? In estimating the range use the range energy curve for alpha particles in Halliday, p. 125.

3. (15)

- (8) (a) Show by calculations why ${}^9_5\text{B}$ cannot bind a proton. (Hint: It is a Wigner nucleus.)
- (7) (b) How can you use the allowed nature of beta decay processes involving Wigner nuclei for evidence that neutrons and protons have the same order in filling orbitals in the shell model. Explain fully.

4. (10)

- (5) (a) With a double-crystal x-ray spectrometer it is possible to obtain precise energy determinations and to measure the natural width of x-ray lines. The width at half-maximum for the K x-ray of silver is about 10 ev. What is the lifetime for the emission of the K x-ray?
- (5) (b) Suppose two successive K shell internal conversion processes were to take place in the same atom. Would you expect that the resulting K x-rays would be normal or abnormal in energy? Explain.

5. (15)

- (5) (a) In a beta decay of an odd mass number nuclide the transition from the odd neutron to the odd proton is an



transition. Classify this transition according to Gamow-Teller selection rules.

- (5) (b) Suppose the nuclide had an even mass number for which the shell model predicts $f_{5/2}$ and $d_{5/2}$ for odd neutron and odd proton. What would be the spin change in the beta decay? Explain.
- (5) (c) Explain briefly and without derivation how the interaction constant, g , in the Fermi equation for beta decay can be evaluated.

6. (15)

Predict the alpha-decay energy and half-life for Po^{220} (polonium).

7. (6)

Thorium is irradiated with 20 Mev protons and an alpha emitter is obtained in a fraction which follows rare earth chemistry. How could you deduce from what is known about the fission process that it is not a rare earth alpha emitter? How could you tell from its decay properties?

8. (8)

A boron trifluoride counter detects neutrons of 1 ev energy with 50% efficiency, that is, half of the neutrons are absorbed. What will be its efficiency for neutrons of 10 volts?

9. (6)

Pu(VI) in aqueous solution is slowly reduced. The rate is independent of the Pu(VI) concentration and depends only on the total plutonium concentration. Give a possible explanation for these kinetics.

Why is the resin column technic the most feasible method for separating curium and actinium?

Final grades for the semester were Harold Jaffe (84.4, A), Robert J. Carr (83.6, A), Walter E. Nervik (78.8, A), Stephen C. Carniglia (78.7, A), Alexander T. Wilson (77.7, A), Thomas O. Passell (71.2, A), Monte H. Rowell (46.3, C).

My student, Gary Higgins, is getting his Ph.D. this semester.

This is also the day that our academic people will go on full-time pay for the summer.]

Tuesday, June 17, 1952

In Chicago. This morning I dropped a note to John Voelker in Ishpeming and said that we are planning to bring the whole family up for a visit to Ishpeming over the 4th of July weekend and that perhaps we will have a chance to meet at that time. I added that he should not cancel anything important like a fishing trip in case he has one planned.

A note also went to Jerome Luntz (Nucleonics) about his plans for reporting on the Nuclear Chemistry Conference of the Gordon Research Conference. I wrote that it will be most important to get the agreement of the participants and that it is possible that their reaction may be negative.

I also wrote to Charles Heidelberger (University of Wisconsin) and said my expenses for the trip to Madison (June 6) amounted to about \$15.00. I added that I enjoyed coming up to give the talk and having a chance to see so many old friends again.

Today I talked to a group of Belgian scientific visitors on "History of Transuranium Elements, Discussion of Other Topics."

At home I received a letter, dated June 14, from Segrè. Emilio quoted a letter he had received that day from Robert Underhill:

I have gone into the record of the matter about which you phoned me on Monday.

Since the whole matter has been taken over by The Regents through its Atomic Energy Commission Projects, the only way that I could have consideration given to the matter would be to place it before that Committee. To do so I would suggest that you write me exactly the request that you or Dr. Seaborg wish to place before the Committee. I have a feeling that we may have a meeting of that Committee before the month is out, and would like to have copies made of your letter for each member so that they could be sent out in advance and each member of the committee thus advised of the request before the Committee meeting is held.

In view of the past history of this matter I believe it is the only way that I can present your request for action.

Emilio went on to say that it seems to him that the Regents want only to waste time. He reported that Underhill had said he had to consult with Mr. Neylan before answering his (Segrè's) phone call. Emilio wrote, "I would suggest that either we use the letter of Neylan preceding the letter of Lane (I do not have the file here) in which he states that they can not define their interest or that we write a letter asking only and very categorically that they define their interest. I think that since they have renounced compensation they must either refuse to define or define it in a very peculiar way. In either case we could go to the Patent Board and tell them that this is all that the Regents are willing to write after their letter of October 1951 and I hope this may put them in the right light. I think we should avoid to drag the thing forever because this is the tendency of both The Regents and Anderson, but it is

not to our advantage. Maybe we could have Mr. Lane write the request for definition of the 'interest'."

[In Berkeley a June 9 letter arrived from George M. Kavanagh (Chemistry Branch, Division of Research, AEC), asking for my opinion of a request for possible support of a research program in nuclear chemistry by Geoffrey Wilkinson of Harvard University. Kavanagh pointed out that the proposal is presented in a more general form than is usual.]

Wednesday, June 18, 1952

In Chicago. This morning I spoke to the Nuclear Chemistry Group of the Institute for Nuclear Studies at the University of Chicago on the "Mechanism of Fission and Nuclear Thermodynamics of Heaviest Elements." This was somewhat similar to the talk I gave at Ohio State recently.

After the talk I got together with T. Fraser Young for a conversation about his work. Young showed me his apparatus for measuring Raman spectra. This is presently the only one in the world of this type although a student of his, Lawrence A. Blatz, is setting up such apparatus at Los Alamos. Young mentioned that Bill Gwinn had seen the apparatus; he told me about his present graduate student, Aubrey C. Jones. I also saw his microcalorimeter for temperatures down to 10^{-6}°C .

I then drove from the University of Chicago to the Argonne National Laboratory. I responded to the letter I received from Emilio yesterday, saying that it seems to me that the only point we are trying to make is that the Regents should answer the letter of Lane one way or another or tell us that they don't intend to answer it. I said that I don't see that they need another letter from us to make this point but I am not sure from his (Segrè's) letter of June 9 whether there is some sort of an answer to Lane's letter that I haven't seen. I asked if the Regents considered Lane's letter at their meeting on May 23, if there were an official transcript by Underhill of the meeting, or if he (Segrè) were referring to the results of the April meeting. I also mentioned that I shall see Joe at the Gordon Research Conference in Nuclear Chemistry next week and will talk the whole matter over with him.

In the mail today was a note from George Boyd, who included a letter he had sent to Charles Coryell inquiring about the amount of time they might expect at the Gordon Conference. He said, as usual, Charlie seems to be as erratic as ever and has not answered his inquiry. Boyd said they are planning on roughly one hour and Jim Cobble will speak in his place for it is necessary that he (Boyd) stay at home this summer for he hopes to go abroad in the autumn.

In today's seminar before the Chemistry Division and Nuclear Chemistry Group at Argonne I covered "Ion Exchange Study of Possible 5f Bonding in the Actinides." In this talk I discussed a difference from the lanthanides--a possible type of covalent bonding in the actinides that occurs much less in the lanthanides. I went over the ion exchange data, equilibrium data, and column adsorption-elution data. The talk was based on Dick Diamond's research.

[In Berkeley an interesting letter, dated June 14, arrived from Alma

Luise Olson of Lindsborg, Kansas. Miss Olson described her background: her parents were confirmed at the Kopparberg Church and met and married in Ishpeming. She has cousins in Grängesberg. She spent a dozen years in Scandinavia as a special correspondent to The New York Times and met and had conversations with Dr. Robert Millikan in Stockholm. Miss Olson wondered if my grandfather might have known her father, John E. Olson, in Ishpeming some time before 1879. Doral had a copy made of the letter and sent it to me.]

Thursday, June 19, 1952

In Chicago. I wrote a couple of letters this morning: one went to George Boyd, saying that I think we will be able to give Cobble the time he requires (at the Gordon Conference). I added that I am sorry he won't be able to attend and that I hope he will have an enjoyable trip abroad this summer.

Another letter went to T. F. Bewley (Los Angeles Chapter, American Institute of Chemists) in reply to his letter of May 29, a copy of which was forwarded to me from Berkeley. I suggested Friday, September 26, 1952, as the date for my speech and said that I am asking my office in Berkeley to send him biographical material and three glossy photographs of me. A carbon of this went to Doral with a note to send the material.

I also wrote to Frank L. Lambert (Southern California Section, Pacific Southwest Association of Chemistry Teachers) to suggest the morning of Saturday, September 27, as the best time for me to talk to their group as I have another speaking engagement in Los Angeles for the evening of September 26. I told Lambert that I shall return to Berkeley around the middle of July and will send him the title of my talk soon after that.

At home I received a letter from Joe Kennedy about his conversations with Art Wahl about Emilio's recent correspondence. Joe said they agree in general with Emilio's view and believe that Lane should write to the Regents, pointing out that Underhill's letter of May 28 answers only a small part of our question. Joe went on, "If all these things fail and we must find some new approach to the Regents, this would best be done when you are back in Berkeley. Such a new approach might be an attempt to split the Regents, or a (well-considered) threat to sue in the state courts. We wonder if Mr. Lane has any opinion on this last course of action."

[In Berkeley the research group met as usual. The following were present: Asaro, Browne, Carniglia, Carr, Carter, Clark, Dauben, Feay, Gunn, Higgins, Hollander, Huffman, Hulet, D. Conway (Conway will enter graduate school at the University of Chicago in the fall but is spending the summer with our group), Levy, Newton, Passell, Perlman, Rasmussen, Slater, and Templeton.

Asaro described his examination of a volatilized sample of curium produced by neutron irradiation of Am^{241} at Chalk River in the 60° magnetic alpha-ray spectrograph. He verified the presence of a third alpha group of Cm^{242} , saying the group is highly hindered with respect to alpha decay theory and leaves the Pu^{238} daughter nucleus in an excited

state approximately 100 keV above the second observed level (i.e., above the first excited state of the product Pu^{238}). Asaro went on to say that this third level can be correlated with the conversion electrons found by Freedman of Argonne in the $\text{Np}^{238} \rightarrow \text{Pu}^{238}$ decay. He discussed the decay scheme of Freedman, which he said may be in error since the average values of the L and M binding energies were used to determine the low energy gamma transitions from the internal conversion electron spectra. Asaro presented a simpler decay scheme.

Carter compared the crystal structure of AlCl_3 , which other workers have recently studied, with his similar structure of YCl_3 . There was some discussion of the work.

Browne talked about the Am^{241} decay scheme and said a group of English workers recently reported work with a proportional counter. They did not see the 33.3 keV transition that we have used in our decay scheme. Browne said, in reexamining our data, he has seen the transition in two samples but not in a third. This throws considerable question, he noted, on our previously reported decay scheme.

Browne also discussed some work he has done on "beta systematics."

* * * * *

Friday, June 20, 1952

In Chicago. I had conversations with a number of men in the lab and assembled some material to take with me to the Gordon Conference.

A rather detailed letter arrived from Frank Asaro with some additional data on some curium isotopes. Frank said that the decay scheme of Cm^{242} has been shown previously to consist of at least two alpha groups and now he finds three groups with two gamma rays of energies 45 keV and 150 keV. If similar groups exist in U^{234} , U^{236} , and Pu^{238} , it would be helpful in explaining the presence of two gamma rays found in the decay of each of these nuclei. They determined the decay scheme of Cm^{243} by subtracting the known peaks of Cm^{242} and Cm^{244} from the spectra of a sample containing $\text{Cm}^{242, 243, 244}$. It is a useful letter, which I appreciated receiving.

I also received a check for my trip expenses to the University of Wisconsin from Charles Heidelberger. Charlie thanked me again on behalf of the Wisconsin Section for my very stimulating and interesting lecture. He said, "We had particularly wanted an exceptionally good one to finish up the season, and we certainly got it."

Later, at John Lewellen's request, I participated on a panel of "Down You Go" on WGN-TV (this is scheduled for rebroadcast on the radio tomorrow). This show is moderated by Dr. Bergen Evans, Professor of English at Northwestern University, and the participants compete by trying to fill in letters for words listed in outline form on a bulletin board. (I learned later that I won a set of the Encyclopaedia Britannica for submitting a question that the panel missed--this was a sort of compensation for my appearance on the show.)

Saturday, June 21, 1952

In Chicago. Since I will be gone this coming week, I spent a lot of time with the kids today. Helen and I took them for a drive and stopped to see Mrs. Vincenza Insoda in Lockport. Mrs. Insoda is the mother of my friend from UCLA and the Met Lab, Zene Jasaitis; Helen and I met her in December 1944 during our days in Chicago. Several of Mrs. Insoda's friends and relatives were also visiting her today.

Sunday, June 22, 1952

A lab driver took me to the Chicago airport in time to catch a noon flight, along with a couple of others from Argonne, to New York. In New York we took American Airlines Flight 388 at 5:15 p.m. to Boston. This arrived about 6:18 p.m., and we took a cab to the railroad station. The Boston and Main train left at 9 p.m. for Franklin--a number of Gordon Conference attendees were also on this train. In Franklin we went by bus to New Hampton, New Hampshire, where we greeted old friends and colleagues who are attending the first Gordon Conference on Nuclear Chemistry.

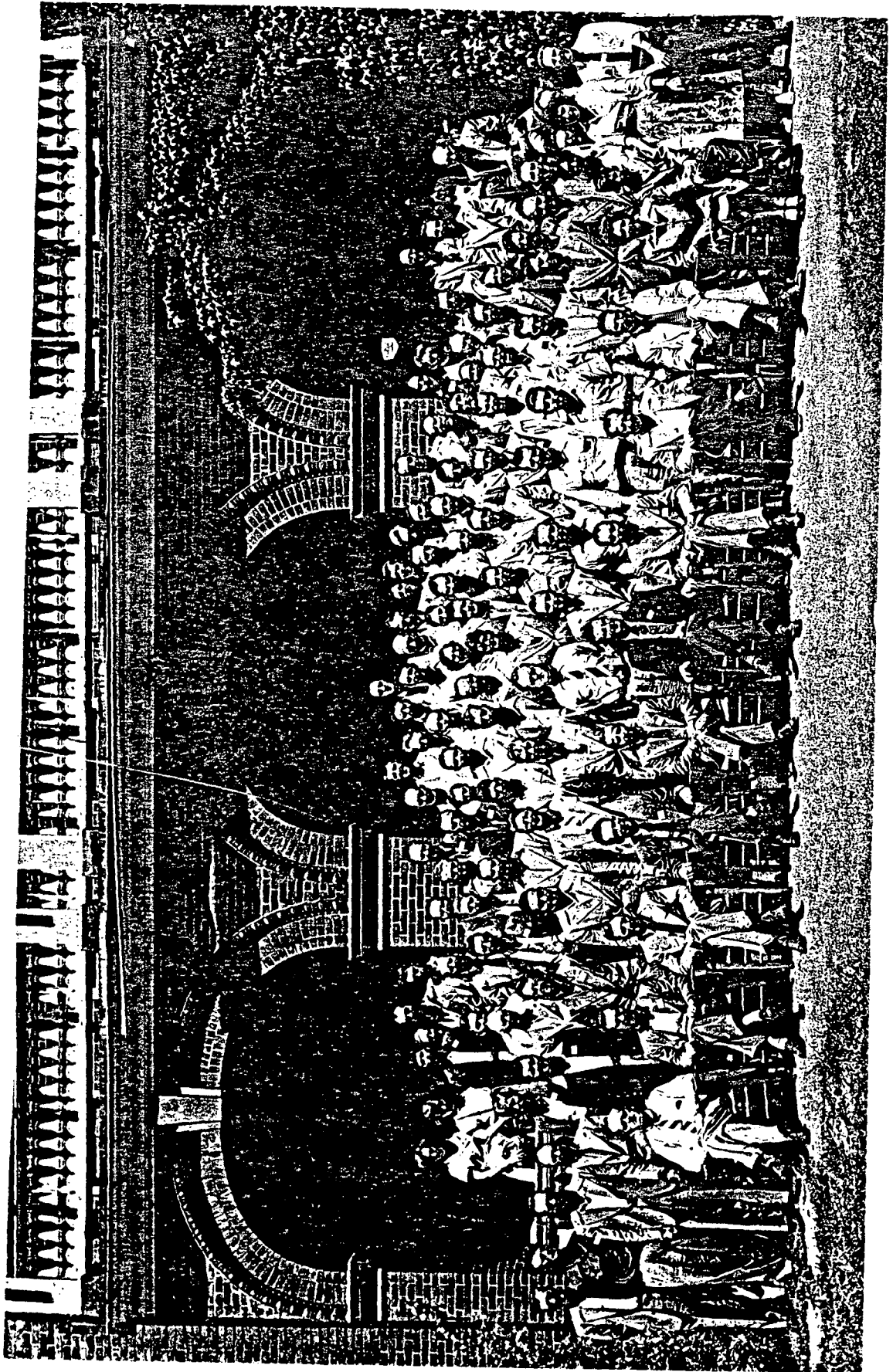
Monday, June 23, 1952

In New Hampton. This is the first Gordon Research Conference on Nuclear Chemistry, sponsored by AAAS and being held in the relaxing setting of the New Hampton School--I am Chairman, and Charles D. Coryell is Vice-Chairman. Meetings are held in Meservey Hall at 9:00 a.m. and 7:30 p.m., with the afternoon free for conversation or relaxation (swimming, golf, tennis, side trips, etc.).

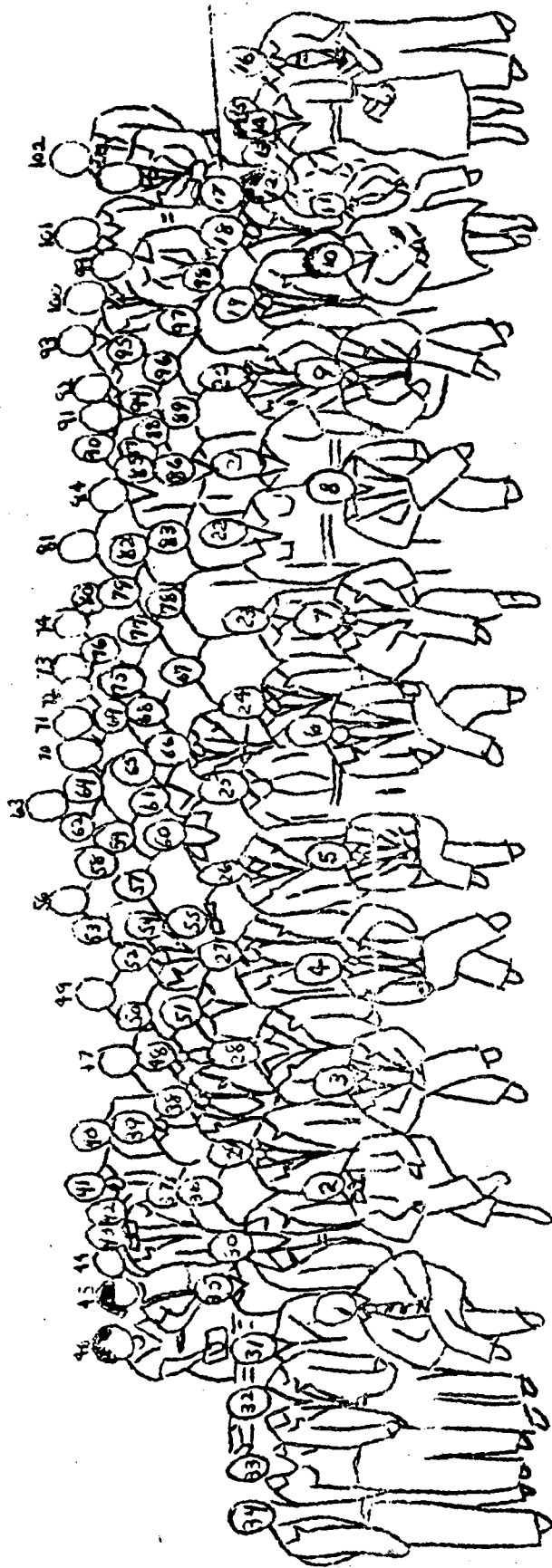
Maurice Goldhaber was the discussion leader at this morning's session on Nuclear Physics, and he and his wife Gertrude Scharff-Goldhaber spoke. Among the items he covered were K/L gamma ray conversion ratios, and she gave a summary of nuclear spin agreement with the shell model.

I talked with a number of people in the afternoon on a variety of subjects. Ray Sheline, Ray Stoughton, and Rod Spence asked for a copy of my Ohio State address.

Before the evening session the group was photographed.



AAAS = Nuclear Chemistry



June, 23, to June 27, 1952

- | | | |
|---------------------------------|------------------------|-----------------------|
| 1. Dr. Parks | 44. Marguerite Marquez | 87. H. M. DeAngelis |
| 2. N. Sugarman | 45. Jane Wagner | 88. B. C. Haldar |
| 3. J. E. Willard | 46. Marg. Parker | 89. Dale L. Milhelm |
| 4. M. Goldhaber | 47. Paul Fields | 90. L. E. Glendenin |
| 5. I. Perlman | 48. F. W. Melpolder | 91. A. DeHaan, Jr. |
| 6. C. Coryell | 49. T. B. Novvey | 92. ? |
| 7. G. E. Seaborg | 50. Rene J. Prestwood | 93. A. W. Adamson |
| 8. G. Friedlander | 51. Lionel S. Goldring | 94. H. Suess |
| 9. J. W. Kennedy | 52. J. Cobble | 95. J. P. Cali |
| 10. Gertrude Scharff-Goldhaber | 53. John W. Barnes | 96. R. P. Schuman |
| 11. Michael A. Goldhaber | 54. Kathryn Johnston | 97. E. J. Fang |
| 12. Joan Walker | 55. H. L. Finston | 98. John P. Butler |
| 13. Jerome Howland | 56. John R. HuiZENga | 99. R. Spence |
| 14. Elizabeth D. Wilson | 57. James W. Cobble | 100. Richard Wolfgang |
| 15. Clarence Heininger | 58. J. S. Gilmore | 101. P. R. O'Connor |
| 16. Carl R. Wilson | 59. W. H. Johnston | 102. R. P. Epple |
| 17. Bruce Dropesky | 60. R. H. Tomlinson | |
| 18. Alois Langer | 61. Frank G. Young | |
| 19. A. Ghiorso | 62. S. E. Stephanou | |
| 20. I. B. Whitney | 63. L. B. Werner | |
| 21. W. Crane | 64. S. Katcoff | |
| 22. K. Street | 65. Louise Clark | |
| 23. L. O. Morgan | 66. J. J. Mitchell | |
| 24. L. B. Magnusson | 67. E. H. Fleming, Jr. | |
| 25. Edgar F. Westrum, Jr. | 68. T. P. Kolman | |
| 26. O. F. Hill | 69. D. S. Salley | |
| 27. H. W. Alter | 70. F. C. Mead, Jr. | |
| 28. A. D. Kirshenbaum | 71. F. Albaugh | |
| 29. G. W. Parker | 72. R. R. Edwards | |
| 30. John R. Bradford | 73. R. A. Brightsen | |
| 31. L. S. Foster | 74. W. W. Martin | |
| 32. Nathan E. Ballou | 75. James Arnold | |
| 33. L. Zunsalt | 76. L. Yaffe | |
| 34. William H. Beamer | 77. John W. Jones | |
| 35. A. F. Voight | 78. W. W. Meinke | |
| 36. E. O. Wiig | 79. Philip Shapiro | |
| 37. R. W. Dodson | 80. J. M. Miller | |
| 38. L. G. Stang, Jr. | 81. J. C. Miskel | |
| 39. George Ford | 82. C. J. Borkowski | |
| 40. C. S. Fisher | 83. R. W. Fink | |
| 41. Raymond K. Sheline | 84. R. R. Williams | |
| 42. Yvonne Sheline (Mrs. R. K.) | 85. A. L. Thompson | |
| 43. Luis Marquez | 86. Geo. K. Schweitzer | |

Nathan Sugarman and I were the discussion leaders for the evening session for the topic High Energy Nuclear Reactions. Scheduled speakers included A. L. Thompson and Iz Perlman, Jack Miller and Ed Wiig, Luis Marquez and Tony Turkevich. The topic is so large that it will be continued tomorrow.

[In Berkeley a request for an evaluation of Allan Zalkin arrived from Ellison H. Taylor (Acting Assistant Research Director, Oak Ridge). Taylor commented, "We have now about as many people as we can afford and house for the next few years so that we would not be interested in him unless he were considerably above the average, or unless his qualifications were particularly well-suited for some of our applied programs." Doral sent Taylor a note saying that I am absent from the laboratory and will no doubt write to him about Zalkin when I return the middle of July.

My office received a copy of a June 20 memo, which went out under my name as Vice-Chairman of the Faculty of the College of Chemistry, to Thomas B. Steel (Office of the Registrar), listing committees of the Faculty of the College of Chemistry for 1952-53. Executive Committee: K. S. Pitzer, R. R. Hultgren, T. Vermeulen, H. E. White, G. T. Seaborg. Committee on Honors: E. F. Orlemann, Jr., chairman, L. A. Bromley, D. S. Noyce.]

Tuesday, June 24, 1952

In New Hampton. The morning session consisted of a continuation of the topic of High Energy Nuclear Reactions with the remainder of the scheduled speakers giving their talks.

The free afternoons allow time for a lot of discussion, both scientific and personal, among the participants. Among the people with whom I talked were Turkevich (on pi meson reactions), Kohman (he wants me to predict the alpha decay energy of Pb^{204} ; he also thinks that Ca^{48} and V^{50} are beta unstable), Miller (interested in the reaction products of cobalt + 340 Mev protons), Sugarman (on pi meson reactions), Coryell (on the mass equation), and M. Goldhaber (he questions the isotopic assignment of 2-hour Y^{88}).

I was able to have a long conversation with Joe Kennedy about our dealings with the Regents.

Bernard Harvey also talked with me for a while. Harvey, whom we hope will receive clearance to work with Stan Thompson in Berkeley, told me that he will go to Harwell for a while at the end of October. He mentioned that his family consists of three boys and a girl.

In a conversation with A. L. Thompson he told me that he hopes to visit Berkeley the week of July 20.

The discussion leader for the evening session was Isadore Perlman, and the topic was Decay Energetics and Systematics. I was the first speaker, Hans Suess then spoke, followed by Charles Coryell and Truman Kohman. In Kohman's talk he suggested that Pb^{204} should have an alpha half-life of 5×10^{16} years and wondered if the approximately 3 Mev alpha

particle could be detected by nuclear emulsions.

Wednesday, June 25, 1952

In New Hampton. Charles Coryell led this morning's discussion on Nuclear Fission with several of the speakers talking about π^- -induced fission. Speakers were Tony Turkevich, Larry Glendenin, Rod Spence, Nate Sugarman, Nate Ballou, John Huizenga, and Leo Yaffe.

During the afternoon I managed to get out on the New Hampton School Golf Course for nine holes of golf with Al Ghiorso, Rod Spence, and Iz Perlman (AG-41, RS-45, IP-42, GTS-39).

Joe Kennedy, who was the discussion leader for the evening session on Tracers and Exchange Reactions, gave a report of the recent ACS Symposium at Notre Dame. Speakers were Dick (R. W.) Dodson, Arthur W. Adamson, Elizabeth D. Wilson, and James Cobble.

After the evening session I chatted for a while with Jim Cobble, who will be coming to Berkeley this fall in a postdoctoral position.

Thursday, June 26, 1952

In New Hampton. John Willard led the morning session on Hot Atom Chemistry with speakers William H. Hamill and Russell R. Williams, Jr., Raymond R. Edwards, Arthur W. Adamson, and James Cobble.

Joe Kennedy and I received a letter from Emilio Segrè, saying that he again phoned Underhill after he heard from me. Underhill told him there is no transcript of the meeting of the Regents of May 23, no letter of the Regents besides the one of May 6 and the one of June 12, and the Regents considered Lane's letter at the meeting of May 23, and their answer is the statement reported by Underhill in his letter of May 12. Emilio said that Underhill said the Regents will not do anything else but they will answer if we want to write to them. However, he (Emilio) believes they will do exactly what they did with the letter of Lane, namely, reply on a small side issue in order to drag on the thing forever. Emilio said, "It seems to me that we have enough ammunition at present to go again to the Compensation Board and point out to them that all that the Regents answered to the letter of Lane was the communication of Underhill, which obviously is no answer, and in addition point out that their interest has no value, as the Regents themselves acknowledge and confirm in their letter of May 6th." Emilio said that he agrees with Joe's letter of June 17 and that if we write to the Regents, we should put an absolute precise question, asking them to define their interest and nothing else. He added that it is obvious that Neylan entirely runs that committee, and that Underhill checks with him before saying anything. Finally, he wrote "One last remark: Once Glenn knew my name, and Joe always used to misspell it. I see now that you have inverted the parts inasmuch as Joe writes it correctly and Glenn misspells it" (the fault of my Chicago secretary). Joe and I again went over our problems and discussed the next step.

The evening session on Techniques was led by Gert Friedlander and had as speakers C. S. Fisher, Casimer Borkowski, Maurice Goldhaber, Henry G. Thode, Bernard Harvey, Al Ghiorso, Earl Hyde, and others.

After the session I got together with Gert Friedlander and discussed the possibility of his visiting Berkeley for a couple of months to confer on work of mutual interest.

[In Berkeley the research group met as usual. This was a very small meeting, conducted by Dave Templeton and attended by Asaro, Carter, Clark, Dauben, Dunlavey, Higgins, Hoff, Hollander, Levy, Passell, Rasmussen, Shudde, Slater, and Templeton. Carter was the only speaker, and he presented and discussed preliminary data for cell constants of some rare earth trichlorides from indexing their powder patterns. There was some discussion on the work.

* * * * *

Friday, June 27, 1952

New Hampton. Joe Katz led the final session of the first Gordon Conference on Nuclear Chemistry this morning on the subject of Heavy Element Chemistry. Speakers were Burris Cunningham, Stan Thompson, Clark Hindman, Stephen E. Stephanou, and Ed Westrum.

Later I took the bus from New Hampton to Franklin, the Boston and Main train to Boston, a taxi to the airport, and a plane at 4:25 p.m. to Chicago. I arrived in Chicago about 8:45 p.m., was met by an Argonne driver, and driven home to Park Forest. I believe this has been a most successful conference with over a hundred scientists attending and with much free discussion in a very relaxed atmosphere.

[In Berkeley this was the last day for Helge Tyrén, who is returning to Uppsala, Sweden.

A June 25 letter arrived from W. H. Sullivan pointing out two discrepancies in my article "Some Comments on the Mechanism of Fission." Margie replied, saying that she believes the discrepancies he mentioned are caused by my correction of the half-life values for U^{232} and Pu^{238} from those reported in the original Segre article.]

Saturday, June 28, 1952

In Chicago. Helen and the kids were happy to have me home.

A June 27 letter arrived for me at home from Donald Lane who included a photostatic copy of a letter to him, dated June 24, 1952, from Mr. Underhill, stating in effect that the Regents refuse to define their "interest" and refuse to agree to execute the Disclaimer as originally presented. Lane wrote that, in his opinion, our next step should be for him to file a statement with the Patent Compensation Board advising the Board of the outcome of our current attempts to negotiate with the Regents and to accompany the statement with copies of his letters of May 19 and June 19 to Mr. Underhill, and of Mr. Underhill's reply dated June 24. Lane reminded me that the pretrial conference on February 19, 1952 specifies that Counsel for applicants will report to the Board the success or failure of negotiations to clarify title. Lane said he would ask the Board to schedule another pretrial conference on this undefined interest point in such a statement. He added, "With regard to the proposal to file a new claim solely on seeking an award based on Section

11(e)(2)(c) for reports filed, I believe that a new or separate claim is not necessary." He went on to say that at the next pretrial conference, he could ask the Board and General Counsel if they are willing to by-pass the claim for an award for reports and information furnished. Lane noted that this phase has not been touched on in the pretrial conferences to date.

Sunday, June 29, 1952

In Chicago. Again this was a day spent with my family.

Monday, June 30, 1952

In Chicago. At the lab I found a nice note from John Voelker, who said that he and his wife Grace would like to have me, my wife, and children over for a trout dinner when we visit Ishpeming but hesitate to set commitments since our time will be so short and my relatives and old friends are so many. Voelker suggested that if we can make it, I should call him; otherwise he will be happy to deliver some trout to any place I say.

One of the first things I did this morning was to write to Lane to reply to his letter of June 27 and to tell him that Joe Kennedy and I talked things over last week while we were attending a conference in New Hampshire. I wrote that I agree with the plan suggested in the second paragraph of his letter of June 27 and asked about the desirability of his sending another letter to Underhill in which he specifically asks whether the Regents will or will not define their interest since his letter of June 19 directed itself pretty specifically to Mr. Harter's proposal. I explained that I will leave it to his judgment as to whether he wants more documentation on the refusal of the Regents to define their interest. With respect to the matter of refileing, I explained our thought that the present single application would be replaced by two applications, one for an award and the other for compensation. I suggested that he continue to consider this possibility.

I also wrote a short note to Warren H. Crowell (President, UCLA Alumni Association), thanking him for his letter (which Doris forwarded), in which he said that I will be the recipient of the Dickson Achievement Award (UCLA Alumnus of the Year Award) for the year of 1952. I said that I plan to attend the banquets scheduled for the latter part of September or the first part of October, but I mentioned my commitment in Los Angeles for the evening of September 26.

Dieter Gruen and I had a long chat about his work on the analysis of $5f^2$ electrons from Th^{+2} and the correlation with PuO_2^{++} ; he plans to do the $6d^2$ electrons in the same manner. He mentioned that Lawrence Brockway, a physicist, will check to see if $5f$ electrons can be orbitally quenched (Charles Kittel in Berkeley should be good with this sort of work).

[In Berkeley a note arrived from Arne Holmberg, thanking me for the corrected proof of my Nobel Lecture and saying that I will receive 200 reprints free of charge. Holmberg asked if the address from which the proof was sent (P.O. Box 5207, Chicago) was a temporary address.

A letter also arrived from Roswell G. Ham (President, Mount Holyoke College) stating that I will receive a formal invitation early in September to attend a formal convocation on the third and fourth of October on the theme "Science and Human Values."

Frederick Sawyer wrote on September 25 to ask if it would be possible for me to stop off in Los Angeles on my return from the Atlantic City ACS Meeting in order to attend the meeting of the American Institute of Chemists. He said such an arrangement would avoid a somewhat delicate situation--that of the local chapter treasury being unable to pay travel expenses.

A formal memo arrived from Robert Underhill, stating that my salary was fixed by the Regents at \$8,800.00 (66.7% time) for the year ending June 30, 1953.

A notice of the filing, dated April 29, 1952, of patent application for Case S-2292 ("Recovery of Neptunium from Aqueous Solutions" by Glenn T. Seaborg, Roy C. Thompson, and Frederic W. Albaugh) arrived from Foster York.]

Tuesday, July 1, 1952

In Chicago. Before visiting the labs to talk with the researchers, I responded to the letter of June 9 from George Kavanagh (Chemistry Branch, Division of Research, AEC) about the research proposal of Wilkinson and Diamond. I wrote, "I believe the proposal is an excellent one and should have the support which is requested. I know both applicants very well and have been in a position to observe intimately their research capability and performance; they are very good men and can be counted on, practically with complete certainty, to carry on a very interesting, significant and productive research program."

Later in the morning, allowing for the difference in time, I telephoned Berkeley and talked with Doral and Iz about a number of matters. Iz read the memo he wrote to the salary committee about the merit pay raise for Al Ghiorso (from \$775/month to \$835/month): "We can only repeat our previous statements on the truly outstanding ability, ingenuity, and productiveness of Mr. Ghiorso and the key position he plays in our entire research program and graduate student training program. He continues his eager interest in his work and his willingness to spend extra hours when necessary to the experiments in progress. He deserves special consideration in the form of a salary increase."

Another item we discussed was something that came up at the Gordon Conference, a two-month visit from Gert Friedlander of Brookhaven. This is something we all agree will be mutually beneficial to both labs, and Iz agreed to talk with our administrative people about it. As an afterthought I later sent Iz a handwritten note, "Following our phone conversation today it occurred to me that Everson should probably write Haworth (cc to Dodson) pointing out our intention of borrowing Gert for 2 months if this hasn't been done. You could help with a proper opening paragraph."

My new consultant contract (\$40/day, 25 days/year) with the Department of the Army because of my position on the ACS Committee Advisory to the Chemical Corps begins today. For this position I am cleared for access to Secret, Confidential, and Top Secret material. The description of the position is rather awesome:

Serves as an expert consultant as a member of the American Chemical Society Committee Advisory to the Chief, utilizing an advanced knowledge of the principles of Nuclear Chemistry incident to review and verification of research principles and practical applications involved and/or in the determination of the nature of the problem or problems involved in the determining and/or attaining principles and practical application for Chemical Warfare use, on phases of research and development, such as weapons, munitions, and dissemination. In making decisions, takes into consideration advanced principles and consideration of implied relationships and principles, such as the principles involved in the dispersion of solids and liquids; basic studies on the formulation of particulates and aerosols; basic research concerning the conditions existing in HE chemical munitions and the dispersal of solids, liquids, and gases.

Wednesday, July 2, 1952

In Chicago. I spent a usual day at Argonne.

[In Berkeley Doral responded to a letter, dated June 25, from James Colvin (Director of Public Relations, Encyclopaedia Britannica), informing me that I had won a set of the Encyclopaedia Britannica and World Atlas for downing the panel of "Down You Go" on WGN-TV on June 20. Doral wrote that she is sure I will be delighted with the news (if I do not already know it) and that she is writing to let Colvin know that I recently moved to 1154 Glen Road, Lafayette, California, so that if he has not already shipped the Encyclopaedia to me at my old address, he should arrange to have it sent to me at my new address.]

Thursday, July 3, 1952

In Chicago. I spent a usual day at Argonne.

In the late afternoon all of the Seaborgs--Glenn, Helen, Pete, Lynne, David, and Stephen--left Chicago on the "400" of the Chicago and Northwestern Railroad bound for my hometown of Ishpeming, Michigan, the northern terminus of this rail line.

[In Berkeley the research group had its usual first of the month meeting with the following people attending: Carniglia, Carr, Clark, D. Conway, Dauben, Feay, Hoff, Hollander, Walter J. Laird (du Pont man), Levy, Nervik, Passell, Rasmussen, Shudde, Slater, Mark D. Snyder (du Pont man), Templeton, and Thompson.

Levy reported on his work on Bi^{210} , produced by irradiating natural bismuth with neutrons in the pile, and said that Bi^{210} decays by alpha emission to Tl^{206} as its main mode of decay but it also decays to Po^{210} . Levy went on to say that so far they have been unable to determine if Bi^{210} is an excited isomeric state or actually the ground state. Browne failed to see any isomeric transition gamma rays for RaE, but his low limit of detectability was so high as to make the experiment inconclusive. Levy said the only thing he can report is the ratio of Po^{210} formed to the number of alpha particles observed, which is 1:273. Thus the half-life for beta decay to Po^{210} is about 2.7×10^8 years. There was considerable discussion and advice about the work.

Rasmussen told the group about some work of de Benedetti at Carnegie Tech in which he used germanium diodes in a rapid coincidence counter run directly by photomultiplier tubes. Using his scintillation counters with Na^{22} as the source of annihilation radiation, de Benedetti obtained resolving times of about 10^{-9} seconds. Rasmussen showed the curve of coincidences vs delay time that Sergio de Benedetti obtained. He went on to say that he is using a similar coincidence counter for measurements of delayed coincidences, describing the coincidence counter for measurements of delayed coincidences and the various crystals that can be used, along with their characteristics.

* * * * *

Friday, July 4, 1952

The train was late, arriving in Ishpeming after 3:30 a.m. Despite the late hour a contingent of relatives met us at the train station--Uncle Henry and Aunt Minnie Seaborg, Helen Marie and Elmer Swanson, Elsie (Swanson) Sundlie and her husband George, and Margaret (Carlson) Gaboury and her husband Lawrence. Uncle Henry and Aunt Minnie have invited us to stay with them at their home at 1115 N. Fifth St., corner of Park St., during our visit. Also staying here will be Uncle Lawrence and Aunt Christine Seaborg, who will come up from Menominee. Their sons John (age 15) and Donald (age 13) are coming with them, but they will stay at the home of Gus Dahlstrom (515 N. First St.). [Henry and Lawrence are brothers of my father while Gus is one of my father's first cousins, son of a sister of my father's father.]

After sleeping as late as the kids would allow and having lunch, Uncle Henry drove us to Negaunee and Marquette to visit relatives on my father's side of the family. First we visited Aunt Lillie (Seaborg) Larson and Uncle Bill Larson in Negaunee (525 Elm St.) Aunt Lillie was very patient with our restless kids and diagnosed Dave's problem as hunger, which she alleviated with some cookies. She then served us the traditional coffee and rolls.

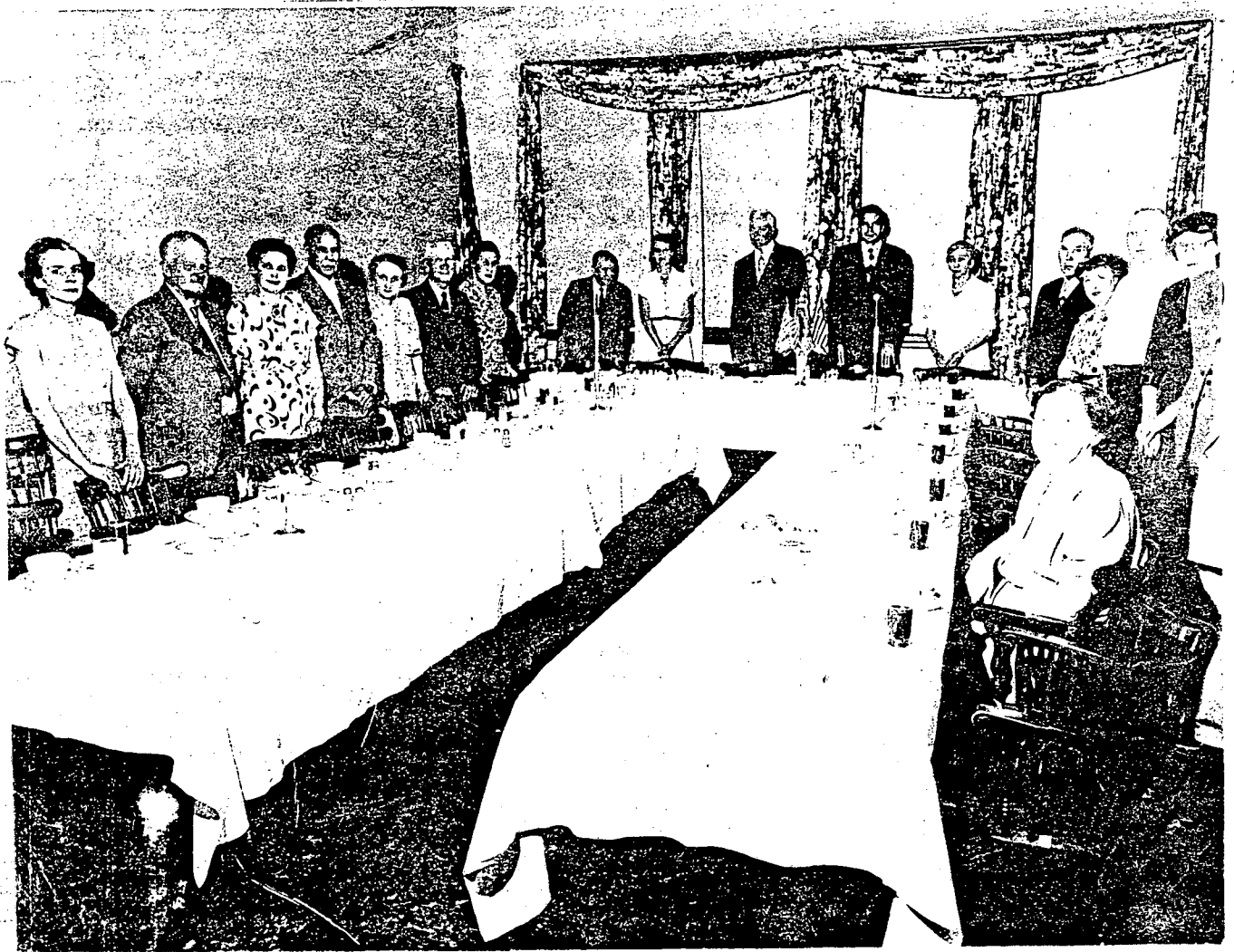
Uncle Henry then drove us to Marquette to see Pauline "Lala" Cuyler and her husband Ed. Lala is my first cousin, daughter of a sister of my father. We also visited with the Cuylers' daughter Jean Daugherty, her husband William, and their children, including the Cuylers' son Bill and his wife Rose Marie.

Uncle Henry took us by to see the Philip Larsons (my first cousin, son of Lillie and Bill), but they were not at home. We then drove back to Ishpeming, where the Lawrence Seaborgs joined us for a pasty dinner that Aunt Minnie had prepared.

Saturday, July 5, 1952

In Ishpeming. Helen, the kids, and I walked around town and saw our old houses--the house where Jeanette and I were born (231 New York St.), the house where we lived from 1915 until we moved to California in 1922 (802 E. Wabash St.), and the Seaborg family home (639 E. Division St.). We stopped to visit Ruth Engstrom's mother (Erica Olson, 827 Empire St., corner of 7th St.) and saw Ruth's brother (August Olson). In the Old Location we visited my mother's cousin (Hulda Swanberg, 605 N. 7th St.) and Aunt Ida Green (630 Vine St.).

Edith and Rudolph Ericson hosted a large family dinner in our honor at the Mathur Inn this evening.



Mathur Inn, Ishpeming, Michigan, July 5, 1952: (l to r) Viola De Gabriele, Edward Cuyler, Helen Marie Swanson, Lawrence Seaborg, Minnie Seaborg, George Sundlie, Lillian Larson, Gustaf Dahlstrom, Helen Seaborg, Rudolph Ericson, Glenn Seaborg, Edith Ericson, Henry Seaborg, Lala Cuyler, William Larson, Christine Seaborg, Elmer Swanson (hidden on right), Elsie Sundlie, Jerry De Gabriele (hidden on right)

Sunday, July 6, 1952

In Ishpeming. After breakfast with the Henry Seaborgs and the Lawrence Seaborgs, we all visited Gus Dahlstrom at the Dahlstrom family home (515 N. First St., site of their historic pop works). Gus showed us the large picture hanging on the wall of my great grandfather, Eric Sjöberg (Seaborg), who had changed his name from Olsson to Sjöberg. We also saw an old album of family pictures.

Later we joined a large group at the Sundlie camp, located on the Little Dead River, for a midday dinner prepared by Elsie. Present at this pleasant occasion were Lawrence, Christine, John, and Donald Seaborg; Henry and Minnie Seaborg; Elsie and George Sundlie; Helen Marie and Elmer Swanson; Edith and Rudolph Ericson; Gus Dahlstrom; and Lillie and Bill Larson, their son Philip and his children Don and Clarice. Edith, Rudolph, and Gus left in the middle of the afternoon, but the rest stayed for supper. Gordon and Helen Swanberg, with their son John, dropped by for a while in the afternoon. Don Larson had his fishing pole with him, and naturally our kids wanted to fish too. So Elmer helped them, and they caught a few small fish; this thrilled them very much. Don and Clarice also gave them a few of their fish. [As it turned out, we had to take these home to Park Forest for Helen to cook for dinner.]

By supper time the following people had arrived at the Sundlie camp: Harry and Lila Green and their family; Viola and Jerry De Gabriele and their children (Gregory, Caroline, and Christopher); Verna (Swanberg) Chinn, her husband (Percy), and their son (Gordy); Bill and Thelma Swanberg; my Ishpeming childhood friend Clarence G. Larson, his mother (Anna, Mrs. A. Gottfrid Larson) and his wife (Janet); George Green and his wife; and Ruth Engstrom's sister (Gladys Olson Ninnis), her husband (William Ninnis), and son (Paul Ninnis).



Ishpeming, Michigan, July 6, 1952, l to r: Glenn, Peter, Lynne Seaborg; Lawrence Seaborg (in back), Clarice Larson; David, Donald, John Seaborg; Donald Larson (in front)



front row: Christine, Minnie, Helen Seaborg; Lillian and Phil Larson;
back: Lawrence, Henry, Glenn Seaborg, Bill Larson, July 6, 1952



front: David, Peter, Glenn Seaborg, Bill Larson; middle: Lynne Seaborg,
Gus Dahlstrom, Edith Ericson, Lillian Larson, Elsie Sundlie, Helen Marie
Swanson; back row: Helen Seaborg, Rudolph Ericson, Elmer Swanson, George
Sundlie, July 6, 1952



front: Donald and Clarice Larson, David and John Seaborg, Helen Marie Swanson; middle: Gus Dahlstrom, Lillian Larson, Minnie Seaborg, Edith Ericson; back: George Sundlie, Rudolph Ericson, Lawrence Seaborg, Elsie Sundlie, Henry and Helen Seaborg, Bill Larson, July 6, 1952

Helen, the kids, and I travelled back to Chicago by night train. Henry, Minnie, and Gus were at the train station to see us off--Gus with a box of candy for the kids and tears in his eyes. He is close to 80 years old, and he seemed to sense that this is the last time we will see each other. Standing on the bank near her home Hulda Swanberg waved to us as the train went by. (In Ishpeming there is daylight until about 10 p.m. at this time of year.). At the train's first stop, neighboring Negaunee, we spotted Libby (Seaborg) Jaedecke (my father's first cousin), and her husband Clarence, who were seeing some friends off at the station. I hopped off the train to greet them, and Helen rushed the kids to the end of the car so they could say hello. We kept the kids up until we reached the station at Stevenson where I hoped that we could all greet my mother's brother, Oscar Erickson and his wife Selma, who live in nearby Wallace, Michigan. I had phoned him and expected to see them at the station, but I could find no sign of them; I am afraid their shyness prevented them from making this contact with us. Helen and I waited up until Menominee, where we got off the train to say goodbye again to Lawrence and Christine; again we hoped that Oscar and Selma might be there because Lawrence had said that he would drive them back to Wallace, but we were again disappointed.

Monday, July 7, 1952

The train arrived in Chicago early in the morning, and an Argonne driver met us and drove us to Park Forest. I rested a while and then drove to the laboratory to spend the rest of the day.

Tuesday, July 8, 1952

In Chicago. One of the conversations I had today was with a young fellow named Peter Rygaard Gray. Gray, who was born in Ishpeming, is a nephew of Dorice Gray, my secret love in the 3rd, 4th, and 5th grades in the Ishpeming High Street and the Old Grammar School. He did his undergraduate work and course work for his master's at Michigan College of Mining and Technology in Houghton, Michigan. His research for his master's was completed at Argonne, and he is presently working for du Pont here. Gray is interested in working for a Ph.D. in Berkeley, and so I described our setup and told him how to apply.

Wednesday, July 9, 1952

In Chicago. This morning I replied to the June 25 letter from Frederick Sawyer (Stanford Research Institute), which Doral forwarded to me. Sawyer had suggested that I speak to the American Institute of Chemists on my way back from the Atlantic City ACS meeting because they cannot afford to pay travel expenses, and in my response I pointed out that I had suggested the date of September 26 as a suitable date since I have another engagement in Los Angeles at that time. I also mentioned that I shall be back in Berkeley next week and he can reach me in Berkeley by phone if he feels this needs to be discussed further.

I also answered a June 28 letter from Sid Marks (Hotel Harvey, Hollywood), who asked about my experiences as a newspaper boy. I wrote, "I delivered the Los Angeles Express and the Los Angeles Herald back in 1923 at the age of eleven years. This was not only a financial aid but also an experience which I consider valuable for later life."

As usual I talked with various members of the Chemistry Division of the Argonne group.

Walborg and Walter Eggen and Walborg's sister, Esther Dahl, had dinner with us this evening. Walborg and Esther are daughters of Eric Dahl of Ishpeming, whose first wife was my father's older sister Sophia. Sophia, who died in 1900, was the mother of Pauline ("Lala") Cuyler.

Thursday, July 10, 1952

In Chicago. My day was taken up with a meeting of the Research Subcommittee of the American Chemical Society Committee Advisory to the Chemical Corps. Roger Adams (chairman) presided over the meeting, which was attended by Jonathan W. Williams, Arthur C. Cope, and me. Among others who attended were Major General Bullene of the Chemical Corps, Major Randolph (Subcommittee on Chemical Warfare), Harlan N. Worthley (Executive Director, Research and Development Board and Committee on Chemical Warfare), Thomas H. Whitehead (Chemistry Laboratory), William H. Summerson (Medical Laboratory), and Edward J. Schantz (Biological Division, Camp Detrick). On the agenda were such items as (I) The Army Research Program and its Relation to the Chemical Corps (including such things as facilities, personnel, budget, etc.); (II) Significant Items of Basic and Applied Research (chemical) within the Chemical Corps (in relation to the short-term and long-term development in chemical and radiological laboratories, medical laboratories, and biological

laboratories); (III) Review and Examination of Past and Present Research of the NRC, the NDRC, the OSRD, the National Science Foundation, and other groups; and (IV) Additional Ways and Means for Conducting Research (for the immediate future and in time of an all-out emergency). Discussed were the technical objectives of chemical warfare, agents (research and development), dissemination, protection. Bullene told us that he wants to emphasize (1) protection, (2) new ground weapons, and (3) micrometeorology. He brought up the question of tactical biological warfare agents and also mentioned the policy of retaliation only.

I learned that there are 600 to 800 draftees with scientific training (at least bachelor's degrees) assigned to the Chemical Corps. We went over the types of personnel assigned to various labs, including the medical and biological labs, and the types of problems on which they work.

[In Berkeley the research group met as usual with the following in attendance: Asaro, Carniglia, Carr, Clark, Dauben, Dunlavey, Feay, Glass, Higgins, Hollander, Huffman, Hulet, Jaffe, Kalkstein, Laird, Levy, Michel, Passell, Perlman, Rasmussen, Ruben, Shudde, Slater, and Snyder.

Jaffe talked about the work they have been doing to solve the mystery of the 33 kev gamma ray in Am^{241} , which Charlie Browne reported but the English failed to find. Jaffe said they reran Browne's original sample and again saw the line with the reported intensity relative to the 26 kev gamma ray. Jaffe said they investigated the possibility that the 33 kev line belongs to a long-lived isomeric state of the daughter, Np^{237} , which might be expected in Browne's aged sample but might be absent in others due to recent chemical separation. They found the 33 kev gamma ray in a new Am^{241} sample but in only 1/3 the abundance found by Browne, and the neptunium fraction showed no trace of the line. One possibility is a long-lived $\text{Am}^{241\text{m}}$. They plan to watch for growth of the 33 kev line intensity and take an entirely new x-ray spectrum, using a better source arrangement than Browne employed, with better resolution. There was considerable discussion about the problem.

Passell described the new beta ray spectrometer, designed not only for the investigations of beta energies, but also for beta-gamma coincidences. There was a discussion about various scintillators, and Perlman described M. Goldhaber's experiments in which he incorporates the source into the scintillator crystal.

Michel talked about the progress on the time-of-flight isotope separator, saying that their preliminary trials are giving encouraging results and they expect to be operating in the near future.

* * * * *

Friday, July 11, 1952

In Chicago. Yesterday a telegram arrived from Herbert N. Alyea: "THE AMERICAN CHEMICAL SOCIETY DIVISION OF CHEMICAL EDUCATION IS HOLDING SYMPOSIUM ON RECENT ADVANCES IN PURE CHEMISTRY AT THE ATLANTIC CITY MEETING THIS SEPTEMBER. WE ARE ANXIOUS TO SCHEDULE HALF HOUR TALKS EACH BY YOU, LIBBY, GLASSTONE, AND AEBERSOLD. WOULD YOU BE WILLING TO TALK ON TOPIC YOU SUGGEST SUCH AS NEW ELEMENTS OR TRANSURANIC ELEMENTS AT THE

SYMPOSIUM: IF YOU ARE NOT ATTENDING THAT MEETING WOULD YOU BE WILLING TO RECOMMEND AND INVITE ONE OF YOUR ASSISTANTS FOR THIS TOPIC. PLEASE TELEGRAPH REPLY COLLECT TO ME STILLWATER, OKLAHOMA WHERE I AM ATTENDING A CONFERENCE THIS WEEK." This morning I wired back, "GLAD TO TALK ON RECENT ADVANCES IN TRANSURANIUM ELEMENTS. ADVISE ME OF TIME WHEN AVAILABLE." After thinking about my plans for attending the meeting, I then wrote Alyea at Princeton to tell him that I plan to attend the Atlantic City ACS meeting only through Wednesday. I said, "If your symposium is to be scheduled for Thursday or Friday, please let me know as soon as possible in the event you would like me to suggest a substitute speaker."

I replied to a July 8 letter from W. H Sullivan, who inquired about the time for a visit with me here at Argonne to discuss his handbook project, explaining that this is my last day here. I said that we will find some other opportunity to discuss the questions he mentioned.

I returned Volumes 2 and 3 of the final report on Project Crave to Brigadier General D. J. Keirn and Dr. Doyle L. Northrup (Department of the Air Force, Washington), saying that I think that a thorough and excellent job has been done.

A note also went to Joe Kennedy to express my opinion about a date for the summer symposium (Division of Physical and Inorganic Chemistry) for 1953, which will be held at the Knolls Laboratory, Schenectady, New York. I said that, if it is true that it must fall within the period of July 5 and August 14, I suppose we might have to settle for the week of July 6. I wrote that I hope that Frank (Long) will write and give Joe his opinion.

Since this is my last day at Argonne, I took care of various red-tape matters involved with my leaving the lab. I have made arrangements with Don Stewart to have a driver take our car to Lafayette, and Candy, who is returning to Lafayette with us, has helped Helen pack our possessions and leave our Park Forest house in good order.

Saturday, July 12, 1952

In Chicago. All of the Seaborgs and Candy had a rather exhausting day at the annual Argonne picnic at the site of the lab. The program was well planned--the site has swings, slides, bars, and jumping pits for the kids. In addition, a merry-go-round and pony rides were rented for the occasion. There were games and races--young David ran very well in a race for his age group; however, he got rather confused about changing directions and did not win the contest. Some of the adults played horseshoes and baseball. Hot dogs, potato salad, baked beans, pop, milk, coffee, and ice cream were sold at 5¢ a serving. Helen and I particularly enjoyed seeing all our friends with their families.

Sunday, July 13, 1952

The Seaborg clan and Candy had an uneventful flight to San Francisco today. We left Chicago at 10:45 a.m. and arrived in San Francisco about 5 p.m., a little late. General Jimmy Doolittle was also on the flight, and the kids and especially I were thrilled to meet him. It has been an

interesting couple of months and a good experience for the kids, but I think they were happy to get home, find their old possessions, and explore their new rooms.

Monday, July 14, 1952

I stopped on campus to talk with colleagues there before going up to the hill. Then, on the hill I had an unusual number of phone calls and then talked with various people about their work. I also looked over the mail on my desk. Dorai had forwarded or sent copies of the most important items to me in Chicago, but there still was quite a stack of things to check.

Today's mail brought a nice note from Bertil R. Gustring (Editor, The Bulletin of The American Society of Swedish Engineers), expressing his appreciation to me for allowing them to publish my article "The Transuranium Elements." He returned the pictures and periodic table and said they received many favorable comments and compliments.

The noontime brown-bag senior staff meeting in my office covered a variety of subjects from the status of our new chemistry building to current experiments.

At home I found that Candy had helped Helen unpack and set up the arrangements for the kids' new rooms.

Tuesday, July 15, 1952

Again much of the day was spent in phone calls and conversations with people, not only within our group, but also with people in the Director's office.

I answered a letter from Peter Gray, whom I met in Chicago. Gray said that he would like to make formal application to study for a Ph.D. here, and I replied that he should send a transcript of his record at the Michigan College of Mining and Engineering and letters of reference to Dean K. S. Pitzer in the Department of Chemistry.

I also signed and returned a consulting contract to Argonne National Laboratory.

Wednesday, July 16, 1952

Again much of the day was spent on phone calls and administrative matters.

One administrative matter that has consumed some of my time lately is Project Whitney, a weapons project spearheaded by Edward Teller and Ernest Lawrence that they propose to set up in Livermore at the site of the MTA Project, which is being phased out. Herbert York is to be the Director. Of particular concern to me is suggesting well-qualified chemists for the project, preferably those who already have clearances.

I signed a letter of recommendation for Allan Zalkin, addressed to Dr. Ellison H. Taylor (Oak Ridge), which Earl drafted. It was very

similar to others that have been prepared for Zalkin.

Thursday, July 17, 1952

Present at this morning's meeting of the research group were Asaro, Biller, Carter, Clark, Dauben, Dunlavey, Feay, Higgins, Hollander, Hyde, Kalkstein, Laird, Levy, Michel, Rasmussen, Seaborg, Shudde, Slater, Snyder, and Templeton.

Slater presented a graph of preliminary values of the cross section, for the reaction $\text{Th}^{232}(\text{d},\text{p})\text{Th}^{233}$ for deuteron energies of 20 Mev and lower. He said that although the peak yield of this reaction seems to be about half an Mev higher than that of the corresponding reaction on U^{238} and does not fall off as rapidly from the peak towards higher energies as does that for U^{238} , the magnitudes of the cross sections at the peak yield differ by almost a factor of ten. The peak of the d,p reaction on U^{238} seems to be about one-third of a barn while the peak yield of the d,p reaction on Th^{232} appears to be about 34 mb. He finds this surprising and will repeat his work.

Carter, who recently reported that YbCl_3 and LuCl_3 have slightly different structures from the other rare earth trichlorides, said the difference is further brought out by comparing the ratios b/a and c/a of YbCl_3 and LuCl_3 with those of other rare earths: b/a $\text{YCl}_3 = 1.728$, $\text{YbCl}_3 = 1.704$, $\text{LuCl}_3 = 1.696$; c/a $\text{YCl}_3 = 0.930$, $\text{YbCl}_3 = 0.952$, $\text{LuCl}_3 = 0.949$. He and Templeton discussed these structures, and Carter said that he would like to prepare the trichlorides of gallium, indium, scandium, and thallium to compare them with those of the rare earths. He pointed out that TlCl_3 was found to have the YbCl_3 structure. In the discussion I asked about the actinides, and Carter said the actinide trichlorides have the same structure as UCl_3 . Templeton added that a change occurs with the bromides. Templeton discussed Zachariasen's work on other compounds with different but similar structures. I added that the reason for my question is that we explain the complex ions of the actinides in hydrofluoric acid as being due to 5f bond hybridization. I said that, if this covalent bonding which Zachariasen reports is real, it might be additional evidence for the use of 5f orbitals.

I told the group about research in other laboratories relating to 5f hybridization in the actinides. I also mentioned that Freedman, Wagner, Engelkemeir at Argonne observed a 140 kev gamma ray in the decay of Pu^{240} and showed their decay scheme. I said they do not trust the result since such a scheme is not common, but it is in good agreement with our observation of three alpha groups from Cm^{242} which correspond to the levels 0, 50, and 150 kev.

Hollander gave a report on some work of Martin Redlich and Eugene Wigner at Princeton that shows that, with many nuclei, the nuclear energy level separation may be expressed by a constant times a set of integers. The constant varies with the nucleus. He presented some examples. I then spoke on the correlation between beta stable isotopes in the rare earth series with those in the actinide series.

* * * * *

The rest of my day was spent in the labs and conferring with various people.

Friday, July 18, 1952

After the usual phone calls, I answered some of my correspondence. I declined the invitation from President Roswell G. Hamm (Mount Holyoke College) to attend their convocation next October because of my schedule. I did say, however, that I shall be interested to receive further information about the progress of their project and that I wish them a successful undertaking.

To my parents I mailed a copy of the June 14 letter from Alma Luise Olson about her relatives from Ishpeming and her years in Sweden.

A short note went to L. P. Eisenhart (American Philosophical Society), thanking him for the copy of the Proceedings. I wrote that it appears to be an interesting journal, but since I receive more journals than I can actually read, I feel I should not add this to the list I receive.

In reply to a July 1 letter from Stanley Parton (State Vice-Chairman, Distinguished Service Awards, California State Junior Chamber of Commerce, San Jose), I wrote that I shall be glad to serve as a member of the judges' panel in connection with their distinguished service awards this year.

L. Earle Arnow (Director of Research, Sharp & Dohme, West Point, Pennsylvania) asked on July 8 for a print of the 60-inch cyclotron picture that appeared in an article by William Q. Hull in the January 21 issue of C & E News. Today I sent him the print and, in answer to his request for a similar print with me and/or McMillan included, I said we have no such print. Arnow wants the print for a new edition of an elementary chemical textbook.

I wrote to Bernard Harvey to thank him for returning the copy of my lecture on nuclear thermodynamics (Ohio State talk). I wrote that it would be helpful if he could send us five or ten copies (Harvey was going to copy it); however, I said, if it is an appreciable expense, not to bother since we can go ahead and have it photostated here. I then wrote, "I have no further definite information on the matter of your coming here, but I know that it is being very actively worked on at the present time because I have had conferences with the security people both here and in Chicago since I last saw you."

In addition to reading reports and journals, I talked with Perlman and Hollander about the status of the revised "Table of Isotopes." The revision is going well, and we hope to have a draft copy finished by the end of the year.

Saturday, July 19, 1952

I spent the day with my family. The addition to our house means that I have some additional landscaping to do around that section.

Sunday, July 20, 1952

Again, I spent the day with my family and reading.

Monday, July 21, 1952

I had phone calls and conferences with a number of people during the morning.

A note arrived from Warren H. Crowell (President, UCLA Alumni Association), saying that he is happy I will be able to attend their fall banquet and receive the Dickson Achievement Award (Alumnus of the Year Award) in person. Crowell wrote they have not yet set an exact date for the banquet but he will get in touch with me.

I also received a letter from John E. Christian (Purdue University), who has written me before about speaking before the Purdue chapter of Sigma Xi sometime next year. Christian said that he learned from Dr. Johnston (William H.) that I plan to visit Lafayette some time next year and again asked about speaking to the Sigma Xi chapter.

The senior staff had their luncheon meeting in my office, and later I visited the labs to talk with the students.

Tuesday, July 22, 1952

In connection with my duties with the Division of Physical and Inorganic Chemistry (and with the approval of the other officers), I wrote to Dr. William Shockley (Bell Telephone Laboratories) to ask him to be the after-dinner speaker at the dinner meeting of the Division of Physical and Inorganic Chemistry on the occasion of the fall 1952 meeting of the ACS in Atlantic City. I gave him the details and said our Division will be glad to pay any expenses (but explaining that in a number of instances the organizations with which the speakers are connected have taken care of this), and asked that he speak for thirty of forty minutes on the subject of transistors.

While I was at Argonne, I talked with Sherman Fried and Clark Hindman about some experiments with protactinium (IV) in solution. Today a report arrived from Sherman, describing the experiments and saying the results indicate that they have observed protactinium (IV) and also suggest a large variety of new experiments. Sherman said he expects to be in Berkeley on July 25 and will call me so that we can see each other.

I learned, during my visits to the labs, that Hulet and Gunn are beginning to purify some americium. They are working in Bldg. 5A (the Annex), well supervised by monitors from Health Chemistry.

Wednesday, July 23, 1952

There have been a number of conferences about the staffing of the Project Whitney program. Today I sent a telegram to Allan Zaikin, who is vacationing with his mother in Haliburton, Ontario: "CAN YOU RETURN FAIRLY SOON TO DISCUSS IMPORTANT JOB POSSIBILITY HERE?"

Our Indian student, Hirdaya Mathur, contacted me and asked me to sign a progress report, which he must file with the Education Department, Embassy of India. This I did--Earl wrote the Supervisor's Report: "Mr. Mathur has initiated some research on the radioactive properties of some isotopes of xenon under the direct supervision of Dr. Earl K. Hyde. Dr. Hyde reports that he is making quite satisfactory progress in mastering the experimental techniques and in obtaining new and significant data."

Thursday, July 24, 1952

The group meeting this morning was attended by Asaro, George Barton (vacationing from Argonne), Carniglia, Carr, Clark, Dauben, Dunlavey, Gunn, Higgins, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Laird, Levy, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Seaborg, Shudde, Slater, Surls, and Snyder.

Nervik presented a summary of the fission and spallation of tantalum (340 Mev protons), saying cross sections were measured for isotopes of the following elements (normally one nuclide per chain): tungsten, cerium, silver, palladium, ruthenium, strontium, bromine, arsenic, gallium, copper, nickel, and iron. Nervik presented a curve of the cross section vs mass number, noting that the maximum in the fission part of the curve occurs below the point that one would expect for binary fission of the tantalum nucleus itself. He said that at least 15 neutrons are emitted first since the peak comes at about mass number 80. I added that one can assume that the nucleus must emit the maximum possible number of neutrons before fission becomes a competing process and this might require 15 or 20 neutrons to be emitted. There was much discussion about the work.

Asaro reported that he recalculated the curves of energy vs half-life for even-even alpha emitting nuclei, using the energy correction suggested by Georges Ambrosino and Henri Piatier (previously mentioned by Rasmussen). Asaro said he used Cm^{242} to calculate the radius constant (1.15×10^{-13}) to use in the calculation of theoretical curves for other heavy nuclei. Asaro found that the experimental points do not lie on the calculated curves. I told the group about conversations I had with Bernard Hamermesh, a theoretical physicist at Argonne, who was looking into a similar effect of electron binding energy in beta decay. This is, I said, the increase in binding energy of the electrons due to the higher Z and larger number of electrons in the daughter nucleus. There was considerable discussion about the problem. I mentioned that it would seem that closed subshells at $N = 148$ and at $Z = 96$ exist with a minor subshell at $Z = 90$. I said one supporting reason is connected with the analogous properties of actinides and lanthanides with differences in Z of 32 and in A of 90.

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After spending some time on campus, I went up to the hill.

Friday, July 25, 1952

Again I had phone calls, conversations, and conferences. Sherman

Fried telephoned and said that he and Jim Wallmann have arrived in the area and are ready to start work. Fried and Wallmann are going to take part in an Argonne-Berkeley cooperative program of extracting the americium from some nickel-clad plutonium slugs we have. Sherman said that he is staying at the Claremont Hotel, but Jim will stay with his wife Mary's parents.

Sherman Fried



A reply arrived from Allan Zalkin to the telegram I sent him on Wednesday, saying that he will be in to see me Tuesday morning.

Saturday, July 26, 1952

Most of the day was spent working in the yard and with my family.

Sunday, July 27, 1952

In addition to playing with the kids, I did some reading. I have been trying to catch up on my report and journal reading. For example, I noted that the Dunlavey and Seaborg "Letter to the Editor" entitled "Investigations of Complex Structure in Alpha-Emission with Nuclear Emulsions" appeared in the July 1, 1952 issue of The Physical Review (Phys. Rev. 87, 165 (1952)).

Monday, July 28, 1952

Much of the day was taken up with phone calls, appointments, and meetings.

One of the calls I received was from J. C. Dillon (Department of Engineering, UCLA), who told me about a course, "Modern Physics for the Engineer," that they are planning through Engineering Extension for the Fall, 1952 semester. Dillon said they would like to have me as one of the guest lecturers. I expressed some interest in the course, and Dillon said he will send me an outline and description of the course.

In today's mail was a carbon of a letter from Charles Coryell to W. George Parks (University of Rhode Island), thanking Parks for our opportunity to have the Gordon Research Conference on Nuclear Chemistry this June. Coryell mentioned that he was elected Chairman for next year's Conference and that Isadore Perlman was elected Vice-Chairman. Coryell went over some of the details for next year's Conference, mentioning that they hope to hold the Conference the same week next year at Colby Junior College.

I read a rather lengthy letter from Frank R. Ward (Reactor Science & Technology). Ward said that he has accepted the editorship of the journal, replacing George Weil who has resigned. He described a recent survey of project people about the kinds of articles they would like to have published. Ward asked for my comments and invited me to be a guest editorial writer during the coming year.

An acknowledgement arrived from Stanley Parton of San Jose to my acceptance of their invitation to participate in the Distinguished Service Award Panel and Banquet of the California State Junior Chamber of Commerce. Parton said details of the program will be sent to me in the near future.

The senior staff had its regular luncheon meeting in my office.

Dr. Arne Holmberg of the Swedish Academy of Sciences arranged to have an additional proof of my Nobel Lecture sent to me, and I looked it over this afternoon.

Tuesday, July 29, 1952

One of the first things I did this morning was to write a memorandum to George Everson about employing George Barton on our new Project Whitney at a salary of \$600/month beginning September 1. I told Everson to arrange to have Barton's Q clearance transferred and to reimburse him for travel and moving expenses from Chicago.

I also met with Allan Zalkin, described the Project Whitney program, and arranged with him to join the program.

A teletype went to Gerhart Friedlander at Brookhaven, "FAVORABLE LETTER FROM EVERSON IS ON WAY. WE WILL CONTACT OUR MRS. BARRIEAU REGARDING HOUSING."

Later I replied to a July 21 letter from T. F. Bewley (Los Angeles Chapter, American Institute of Chemists), in which he explained why they desire to hold their September meeting on a Thursday, preferably September 25. I wrote that I can change my schedule to be able to make it on September 25 and that I shall send information on my topic and an abstract within a few days.

I returned the additional proof of my Nobel Lecture to Dr. Arne Holmberg, thanking him for the additional chance to check the article and saying that we found that the printers correctly understood and entered all of our corrections.

During my rounds of the labs I learned that Hulet and Gunn have progressed to the column box in their americium purification project.

A. L. Thompson (McGill University) is spending a few days in the area and at the University. I talked with him for a while and invited him to attend our group meeting Thursday morning.

Wednesday, July 30, 1952

This morning I wrote a memorandum to George Everson to inform him that we want to employ Dr. Allan Zalkin, who is at present on Project XR-1, on Project Whitney starting August 1. I suggested a starting salary of \$550 and said that I have talked with Don Gow and Ed McMillan about this shift.

I also wrote to Bernard Harvey to tell him that the AEC has given an o.k. for the start of processing for his employment here. I explained that the next step will be the regular clearance procedure, which should be straightforward in his case and normally takes about ten weeks.

Today's mail brought a nice letter from Wayne Meinke (University of Michigan), who said he got a lot out of the Gordon Research Conference and suggested a session next year on "Nuclear Chemical Techniques." Wayne said he was writing to ask permission for Lynn Hall (a former student here) and Wayne Cassatt to visit the Radiation Laboratory. Wayne said they are interested in instruments, that Lynn still has a Q clearance, and that he has called Earl about the trip. I routed the letter to Earl and said that he should answer if he thinks it is necessary.

I also received a letter from Maurice D'hont, the Belgian scientist I met at Argonne, who said he has received news that his Belgian company and the government will send him to Berkeley the week of August 18 to August 24 to visit the Radiation Laboratory. I wrote to D'hont (c/o C. E. Larson, Oak Ridge) to say we shall be glad to see him that week and that he should make certain the necessary arrangements for his visit are made with the administration of the Radiation Laboratory.

I made the rounds of the labs to check on the research.

Thursday, July 31, 1952

The research group meeting this morning was attended by Asaro, Carniglia, Carr, Clark, Conway, Dauben, Dunlavey, Feay, Hollander, Hyde, Jaffe, Kalkstein, Levy, Martin, Michel, Nervik, Passell, Perlman, Rasmussen, Seaborg, Shudde, Slater, Templeton, and A. L. Thompson (visiting from McGill University).

Rasmussen described and spoke about the performance of a fast coincidence counter that is being set up in Building 50. He said they have been using a fast low level coincidence circuit with sharp cut-off germanium diodes and fast organic scintillators. The instrument showed a resolution of about 10^{-8} seconds with Co^{60} , which has two gamma rays of about 1 Mev in cascade. Rasmussen said they have looked at beta-gamma coincidences in both the Ra^{226} and the Ra^{223} natural radioactive series and the curves showed no evidence of asymmetry in either case, which indicates there is no delay in the gamma emissions ($<10^{-9}$ seconds). There were questions and suggestions about the work.

Asaro said they ran some At^{211} made by Dick Hoff last week in the alpha ray spectrograph and obtained an alpha particle energy of 5.863 ± 0.008 Mev (only one peak). Next week he plans to look for the isomeric

state of Po^{211} . Asaro also said they reanalyzed their data on $Cm^{242,243,244}$ and found an alpha peak at 6.01 Mev, which is the approximate place they would predict an alpha particle from Cm^{243} . He showed the group decay schemes.

A. L. Thompson then described for the group some of the recent work that has been done at McGill University, including work on neutron deficient isotopes of mercury and mass number assignments in the rare earth regions. Thompson mentioned their rabbit that whips a cyclotron sample in front of a scintillation counter (three feet away) within 1/2 second.

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Up on the hill I dictated a couple of letters. I wrote to F. Paneth (University of Durham) to say that it is satisfactory to me if the Joint Commission on Radioactivity meets in Stockholm rather than in Copenhagen from July 29 to August 4, 1953. I reported that I have not heard how the difficulty with respect to the reduction in membership that he raised in his letter of November 21, 1951, was resolved, pointing out that I offered my resignation in my letter to him dated November 26. I added that I am not certain whether I shall be able to attend the meeting next summer, but I should like to hear from him about the reduction in membership problem before considering next summer's meeting.

I sent a short note to Bernard Harvey, asking for his judgment about my writing to W. B. Lewis to tell him about our impending job offer (to Harvey).

I also wrote a note to Joe Kennedy about the symposium of the Division of Physical and Inorganic Chemistry for next summer. I wrote that I think we should strive to have it as late as possible to best coordinate with the Gordon Research Conference on Nuclear Chemistry the following week.

I also answered a July 25 letter from Donald F. Mastick (Technical Assistant, Division of Military Application, AEC), who is interested in a date for the new edition of the "Table of Isotopes." I reported that I discussed it with Perlman and Hollander, and we feel that November 15 is a good bet.

Friday, August 1, 1952

Yesterday one of the graduate students, Bob Carr, came into my office and asked if I would write a supporting letter for his application for residence at International House. This I agreed to do, and this morning I wrote to Miss Jean Sullivan (Resident Manager), "...we have found him to be a congenial, dependable person, well-liked by his fellow workers."

Ray Sheline spoke to me at the Gordon Conference about receiving a copy of Hari Sharma's thesis, "Investigations of Some Unusual Nuclear Reactions and Study of Double Beta Decay," and a copy of my Ohio State University lecture "Nuclear Thermodynamics of the Heaviest Elements." Doral checked into this, and I wrote to Sheline at Florida State University that Sharma's thesis was issued as report UCRL-1265, that my lecture is being reproduced by our Information Division, and we shall send him a copy as soon as copies are available.

My day was filled with phone calls, discussions, and conversations with our research people. As usual I also visited the labs to look over the work. I am particularly interested in the study that our visitors, Ellis Steinberg and Larry Glendenin of Argonne, are making on the distribution of the fission products in the spontaneous fission of Cm^{242} . They are working with about 1.7 mg of curium and are separating the fission products chemically.

The work load of the office staff has increased considerably in recent weeks and since Millie Davis, Iz's secretary, will be typing the revised "Table of Isotopes," Doral is in the process of interviewing for an additional clerk. Millie did some investigating, and we purchased an electric typewriter (IBM) for the tremendous typing job of the "Table of Isotopes."

Saturday, August 2, 1952

I worked in the yard, did some reading, and played with the kids. Young Steve took his first steps a couple of days ago, and everyone was very excited--from Steve himself to Pete and his parents.

Helen and I have learned from my mother that Jeanette and Eino have separated; this really didn't surprise us, and I told Helen to write and tell Jeanette that I think she did the right thing. Jeanette plans to rent her home in Whittier and live with my parents.

Sunday, August 3, 1952

Again I worked in the yard for a while. I also thought about a couple of talks I am scheduled to give soon and drafted a summary for a speech, "Nuclear Energy for Industry," for the American Institute of Chemists talk on September 25, which one of the staff will type tomorrow.

Sundays often bring a visitor or two out to see us and to enjoy our country setting. While we were in Ishpeming, Gus Dahlstrom asked us to drop in to see Alma (Nolan) Meagher, her husband Raphael, and daughter who live on Grizzly Peak Blvd. in Berkeley; instead we invited them to come out for dinner tonight. They are very nice. Nancy, the daughter,

is engaged to be married although she doesn't expect to do so for another year and a half. [Alma is the granddaughter of Sophia Seaborg Dahlstrom (my grandfather John Eric Seaborg's sister), and the niece of Gus Dahlstrom. John and Sophia Dahlstrom had four children--John, Gus, Alma, and Julia. Alma married Timothy Nolan and had a daughter Alma. Julia married a man named Johnson while John and Gus never married.]

Monday, August 4, 1952

This morning I read a carbon of a letter that arrived on Saturday, from Robert M. Underhill to Donald E. Lane. Underhill said that members of the Regents committee dealing with the Atomic Energy Commission contracts considered his (Lane's) letter of July 8 at a meeting of that committee held on July 25. He said the committee reviewed the statement made by The Regents to the Commission and its Patent Compensation Board under date October 8, 1951, at the time it considered his letter. Underhill wrote, "The committee came to the conclusion that its position, which was adopted as the position of the Board of Regents, had been clearly set forth in the letter of October 8, 1951, and that nothing has been called to its attention to induce it to alter the position. The committee then reported to the Board of Regents that it was making no change in its previous unanimous recommendation, and this report was approved and adopted by The Regents." I found this response not unexpected and asked Doral to have usual copies made for Kennedy, Wahl, and Segrè.

Over the weekend I looked over the prospectus I received Friday from J. C. Dillon (Department of Engineering, UCLA) for the course "Modern Physics for the Engineer." Although my time is very limited, the course looks fine with lecturers such as Louis N. Ridenour, Leonard I. Schiff, Fred Seitz, Charles Kittel, Joseph Kaplan, et al. However, this morning I wrote to Dillon and said I feel I cannot speak on the subject of nuclear structure on October 27, 1952 (actually some secretary made a mistake for the letter read 1953 although Dillon had told me 1952) because I am already scheduled for two trips to Los Angeles at the end of September or early October. I also pointed out that I do not feel my schedule will permit me to write up something for publication that will suit me. I wrote, "However, as a UCLA alumnus I am reluctant to answer you completely in the negative, especially since I notice that you mention you will consider alternative suggestions. Could you extend your program one week so as to make possible an appearance on March 16, 1953? I shall be in Los Angeles for the week of March 15 to attend the annual spring meeting of the American Chemical Society..."

After a morning of phone calls, etc., the senior staff had its luncheon meeting in my office. One of the subjects covered at these meetings of late has been the development of a replacement for the carbon ion beam of the 60-inch cyclotron, whose high energy beam is weak and accompanied by a useless strong low energy carbon beam. Ernest Lawrence has suggested using an external ion source and linear accelerator for the acceleration of a range of heavy ions; discussions have been held with Luis Alvarez, Craig Nunan, and others about the feasibility of this device.

Darrell and Marjorie Osborne and their four daughters--Kathleen, Carol, Lorraine, and Frances--arrived for a two-day visit with us. The children seem to be having a good time--Frances is the same age as Lynne.

Tuesday, August 5, 1952

I sent to T. F. Bewley (Chairman, Los Angeles Chapter of the American Institute of Chemists) a very rough draft of my proposed September 25 talk to the group as a basis for a publicity release. I suggested that Bewley contact Maynard T. Morris (Manager of the office of Public Information, University of California in Berkeley) or the UCLA equivalent, Andrew J. Hamilton, for help with the preparation of the release.

A telegram arrived from Clarence N. Stover, Jr. (a former graduate student now at the University of Utah), "OUR ELECTRONICS SPECIALIST GARTH WESTENSKOW DESIRES VISIT AUGUST 12 THROUGH 14 WITH HERMAN ROBINSON AND DAVE O'KELLEY TO DISCUSS SCINTILLATION COUNTERS AND CIRCUITRY. PLEASE ADVISE IF THESE DATES SATISFACTORY, WE ARE REQUESTING IDAHO FALLS SEND NECESSARY CLEARANCE INFORMATION." Doral had one of the staff check with Herman and Dave, and I wired their approval.

I made the usual rounds of the labs to talk with various people about their work. One project of considerable interest is going on here in the decontamination shed behind Building 5, where americium is being extracted from some nickel-plated plutonium slugs. This is a joint venture between Argonne and Berkeley. Burris Cunningham helped devise the chemistry that is being carried out in unshielded, but well-monitored, Berkeley boxes with the assistance of Bill Ruehle of Nels Garden's group. The men are working around the clock--Jim Wallmann (now an Argonne employee) and Steve Carniglia (one of Cunningham's graduate students) have the morning shift, Sherman Fried (also from Argonne) works from noon until 8 p.m., and Ken (Kenneth L.) Mattern (who recently received his Ph.D. under Bob Connick) has the graveyard shift.

Wednesday, August 6, 1952

I just received a July 29 letter from Edward A. Shils of Bulletin of the Atomic Scientists, explaining that they plan to devote their September issue to the barriers to the free interchange of scientific ideas that have been erected by the McCarran Act and the recent reluctance of the State Department to grant passports to some American scientists. Shils described what they plan to have in the issue and asked if I would write a brief statement of my opinion as to the implications of the present American policy on this matter. I replied that, although I am sympathetic with what they are trying to do, I do not feel that I can prepare a statement suitable from my own point of view in the time available (Shils wants the statement by August 7). I wrote, "Although I have a general idea concerning the state of affairs, I am not familiar with the details of the McCarran Act and haven't much information on the actual experiences which have been encountered by scientists as a result of it."

I took care of some administrative matters, checked on the research, and read.

Thursday, August 7, 1952

Present at this morning's "first of the month" group meeting were Asaro, Carniglia, Carr, Clark, D. Conway, Dauben, Feay, Glass, Glendenin, Hoff, Hollander, Hyde, Jaffe, Kalkstein, Kofstad, Levy, Mathur, Michel, Robert A. Naumann (former student now in graduate school at Princeton), Nervik, Passell, Rasmussen, Martin G. Redlich (visitor from Princeton), Seaborg, Shudde, Templeton, and Thompson.



Robert Naumann



Martin Redlich

Kofstad presented cross sections from his work on the spallation of silver with 340 Mev protons. His silver target had 0.05% copper and 0.003% iron in it, and he said that these impurities make large contributions to the yield for copper and elements below (he made corrections in some cases for the copper impurity). I pointed out the Ni^{65} can be produced by a $d,2p$ reaction on Cu^{65} . I also told the group that silver spallation is very interesting in that silver lies in the middle of the elements and that iodine is another such element that will be done later. I also suggested to Kofstad that he try to obtain purer silver.

Naumann, upon my suggestion, described the work that he is doing this summer here in Berkeley. He said that he is attempting to make a mass spectrographic identification of the 6.7-hour activity assigned to Mo^{93m} and produced by $Nb^{93}(p,n)Mo^{93m}$. Naumann explained that, if the activity is proved to be Mo^{93m} , it is of interest because the presently accepted level scheme appears incompatible with shell model consideration of an even Z nuclide with one neutron past the closed shell at 50 neutrons. I said that Mo^{93m} is one of the few exceptions to Goldhaber's correlation of isomers. Naumann also mentioned that they have studied Nd^{141} at Princeton and presented its decay scheme.

Hoff reported that, in his study of electron capture isotopes, he recently prepared some At^{211} and examined it in the scintillation counter for gamma rays. He saw no gamma rays and said it appears that At^{211} is an example of ground to ground electron capture, pointing out that the Mayer shell model predicts a parity change in this decay.

Jaffe presented the values he obtained with the bent crystal x-ray spectrometer for the uranium x-rays produced from Pu^{238} alpha decay and talked about his results.

Glendenin reviewed at length some fission yield work done at Argonne recently in which they found fine structure in the yield curves from slow neutron-induced fission of Pu^{239} , U^{235} , and U^{233} . He and Ellis Steinberg

did the radiochemical work and Mark Inghram's group did the mass spectrographic work; they plan to continue the work.

* * * * *

Before going to the hill, I checked in at the Department of Chemistry office.

A letter arrived from William Shockley, who accepted my invitation to be the after-dinner speaker at the meeting of the ACS Division of Physical and Inorganic Chemistry in Atlantic City on September 17. ~~Shockley said his topic will be "Transistor Physics and Chemistry."~~ I asked Doral to have a copy of the letter sent to Kennedy and Frank Long.

After looking over my schedule and checking with a number of people, I wired C. B. Marquand (Executive Director, ACS Committee Advisory to the Chemical Corps), "IT WOULD SUIT MY SCHEDULE BETTER TO SPEAK IN THE AFTERNOON TO BE ABLE TO CATCH WASHINGTON-CHICAGO PLANE AROUND 9 P.M." My talk is scheduled for September 18.

Friday, August 8, 1952

I took care of a number of administrative matters before doing some writing.

Saturday, August 9, 1952

I worked in the yard for much of the day, assisted by the kids.

Helen and Candy have the kids well settled in their new rooms. This makes for a smoother running Seaborg household.

Sunday, August 10, 1952

I did some yard work and then spent some time organizing some of the talks I am scheduled to give in the near future.

Monday, August 11, 1952

I had the usual number of phone calls this morning before visiting the labs to look over the research. I also telephoned Frank L. Lambert at Occidental College in Los Angeles to change the date of my talk before the Pacific Southwest Association of Chemistry Teachers to Saturday, September 27 because of a conflict in my schedule. I also gave Lambert the title of my talk--"Some Notes on the Transuranium Elements." (My old UCLA friend, L. Reed Brantley, is active in this organization, and I did not want to cancel the talk completely. I am also very interested in inspiring quality teaching among science teachers.)

The usual senior staff luncheon meeting was held in my office at noon.

Tuesday, August 12, 1952

Again the day was taken up with administrative matters. I also spent some time going over some of the questionable points in the revision of

the "Table of Isotopes" with Iz Perlman and Jack Hollander. We plan to have Gert Friedlander go over the table carefully and completely when he visits next month, and later Hollander will probably visit various labs and universities to obtain the latest, unpublished data.

After lunch Al Ghiorso and Luis Alvarez joined me at Mira Vista for conversation and 18 holes of golf (AG-86, LWA-98, GTS-93).

Wednesday, August 13, 1952

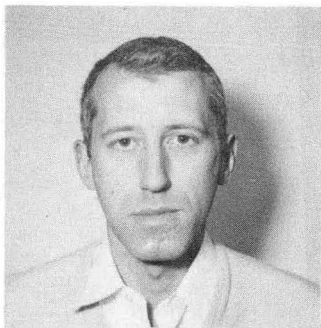
This morning I wrote to Warren H. Crowell (President, UCLA Alumni Association) to give him the latest information on my schedule in connection with the coming UCLA Alumni Association banquet at which I am scheduled to receive the Dickson Award. I said I shall be tied up the entire week of Monday, September 15, the night of Thursday, September 25, and from October 20 to 23, inclusive. I added that I doubt these dates will be in conflict but thought I should keep him informed.

After taking care of some routine administrative duties, I made my usual rounds of the labs.

A letter arrived from Wayne Meinke, who said they have become interested in isotopes of niobium. He has found some confusion in the literature about whether most of the niobium isotopes are well known or not, specifically asking about Nb⁹⁸ and Nb⁹⁶. He asked that I have Jack Hollander write him a note about these isotopes. Wayne also asked, if at all possible, to have his "no-pay" consultantship, which ran out on June 30, renewed. He said it helped him resign his commission in the Naval Reserve this past year and has helped several times in the past when they have been stuck for information or special bombardments. I routed the letter to Hollander and immediately dictated a memo, addressed to W. B. Reynolds, requesting the renewal of Wayne's consultantship.

Thursday, August 14, 1952

The research group meeting this morning was attended by Carr, Clark, Conway, Dauben, Dunlavey, Feay, Glass, Glendenin, Hollander, Hulet, Jaffe, Levy, Ralph D. McLaughlin (a graduate student from Sacramento State, who is doing his research with Burris Cunningham), Nervik, Passell, Perlman, Rasmussen, Redlich, Seaborg, Shudde, Snyder, Templeton, and Thompson.



Ralph McLaughlin

The entire meeting was taken up by a report by Larry Glendenin on the interesting work he and Ellis Steinberg have been doing this summer on the distribution of fission products in the spontaneous fission of Cm²⁴². Glendenin explained to the group that the half-life of Cm²⁴²

is 163 days and there is approximately one fission per 1.6×10^7 alpha particles. They started with about 1.7 mg of curium, which corresponds to 6×10^{12} alpha counts per minute, and therefore there were about 4×10^5 fissions per minute. He described the chemistry, whose chief difficulty is the separation of the fission products from the curium. Glendenin mentioned that one of the peculiar things was the formation of hydrogen by the curium alpha activity; since hydrogen is a powerful reducing agent, it reduced palladium to the metal while they were separating that element. He went on to give their experimental results (fission yield in percent for each isotope counted): Sr⁹¹ 0.6, Sr⁹² 1.1, Mo⁹⁹ 5.5, Ru¹⁰⁵ 7.2, Pd¹⁰⁹ 4, Pd¹¹² 1, Cd¹¹⁵ 0.03, Sb¹²⁷ 0.3, Sb¹²⁹ 1.3, Te^{131m} 1.4, Te¹³² 4.5, Ba¹³⁹ 6, Ba¹⁴⁰ 4.5, Cs¹³⁶ 0.6. Glendenin explained that, with respect to Te^{131m}, although Sb¹³¹ decays to the isomer and the ground state of Te¹³¹, they counted only the 30-hour isomer and therefore obtained only one-fourth of the fission yield of the mass 131 chain. In discussing the plot of their data, he said that Cd¹¹⁵ is in lowest yield at 0.03%, which is their limit on determining the bottom of the trough--I added that the bottom of the valley may never be found. The work evoked much discussion, including comments about equipment, samples, number of neutrons produced per spontaneous fission (between 2 and 3), a discussion of the theory of fission, etc.

* * * * *

Up on the hill I read a response to my recent letters to Bernard Harvey. Harvey wrote, "Many thanks for your two letters with the good news that our troubles are (I hope) nearly over. I do not think that there is any need to write to Lewis. He heard accidentally about the whole affair a long time ago from Dave Langmuir, and I discussed it with him at that time...." I routed the letter to Iz, Stan, Earl, and Al.

Yesterday I received a letter, along with a check for \$100, from Hugh W. Handsfield (Editor, College Department, McGraw-Hill Book Company), apologizing for lack of contact about their Advisory Committee on Nuclear Science. Handsfield explained that they intend to reconsider the advisability of continuing such a sizeable committee of advisers but have by no means abandoned the idea in principle. He also mentioned that he was excited to learn from Dr. Ridenour that I may contribute to the lecture course that Ridenour is organizing for the Engineering Extension at UCLA for they are going to publish the symposium-text. In today's reply I wrote that, since I haven't made any appreciable contribution to his program, I haven't missed hearing from him. However, I shall pass on any suggestions or recommendations that might occur to me in the future. I also wrote that I am not sure whether I shall be contributing to Dr. Ridenour's symposium text since I was unable to meet his original requirement.

I wrote to Wayne Meinke and reported that we have requested an extension of his consultantship-without-pay contract to June 30, 1953. I also said that I understand Jack [Hollander] has written him about the other questions he raised in his letter.

A response went to Miss Alma Luise Olson, who wrote me in June about her family in Ishpeming. I told her that I showed the letter to my

parents. I wrote, "Since your parents and relatives apparently lived in Ishpeming before my father's time, he does not remember them. He thinks it quite likely that his father, my grandfather, may have known them but we have no way of checking up on this. My mother came from Grängesberg and her ancestors did not have any contact with Kopparberg at the time you mention. Even though we didn't establish proof of our families' contact, I was pleased to receive your interesting letter."

I also wrote a reply to Frank R. Ward's letter of July 24 and said that I shall be glad to continue to serve as a member of the Editorial Advisory Board of the journal Reactor Science & Technology and that I shall try to stop in to see him during the course of my next visit to Washington. I wrote that my snap judgment, in response to his inquiry about the desirability of publishing review or survey type articles, is in favor of doing so. I also wrote that I believe I could attempt a guest editorial sometime during the coming year.

I received and studied my copy of an August 11 memo addressed to Winston Manning from W. C. Bentley, A. Ghiorso, and G. L. Pyle about the information they obtained about the thermal neutron fission cross section of U^{239} from experiments conducted at the Materials Testing Reactor in Idaho last week. They reported the data indicate that the nuclide U^{239} has a thermal neutron fission cross section equal to or less than 10 barns and incomplete data give an upper limit of the same order of magnitude for the thermal neutron fission cross section of Th^{233} . The report described the experiment and said the final results in both cases depend upon further observation of the growth of the fissionable daughters, Pu^{239} and U^{233} , and on more detailed analysis of the data.

I was recently contacted by William E. Elliott (Chief, California Patent Group, Berkeley), saying that he had been contacted by Foster York (Chicago Patent Group) and asked to determine whether I made any inventions during my visit at Argonne. I told Elliott that I did not recall discussing or suggesting anything of a patentable nature. Today I received a copy of Elliott's reply to York.

A note arrived from E. B. P. Morava (Assistant Trust Officer, Bank of America re Trust No. TW-50960-2, Estate of Jake Gimbel, Deceased): "You will recall that last May you wrote a letter to the Graduate Division of the University of California recommending Mr. Dean C. Dunlavey as a candidate for one of the Jake Gimbel Scholarship Loans in the amount of \$1,000.00. You will be pleased to learn that Mr. Dunlavey has been selected as one of the recipients."

Friday, August 15, 1952

At my recent visit to Ohio State I met Vincent J. Wottle, S.M. (University of Dayton), and I vaguely agreed to speak some time at the Dayton Section of the ACS. Yesterday I received a formal invitation from him to visit and speak before the upcoming Atlantic City ACS meeting in September. I wrote to Wottle this morning and explained that my schedule is such that I shall be unable to visit Dayton on this trip. I said that I shall see whether it will be possible to work out a visit later in the year and will let him know if this can be done.

I recently read a summary report (AERE C/R 908) from J. K. Dawson at Harwell. Dawson's results led him to the conclusion that my actinide theory is invalid, and this morning I wrote a rather lengthy letter to him, saying in part:

I don't feel that your results, or even your interpretation of them, differ from those expected on the basis of my own views as much as you feel they do. Thus your electron configurations in Table VI, deduced from magnetochemical data alone, do not essentially differ from mine given in your Table I, if you include my alternatives, except in the case of uranium.

Even in the case of uranium I feel that it was a matter of choice to place four electrons in the 6d rather than in the 5f-6d column on the basis of your own evidence alone. Your results on U(III) compounds point to either 6d or 5f shells for three unpaired electrons but you prefer to put the four electrons in neutral uranium in the 6d shell. I think this illustrates the difficulty of assigning the electrons in neutral atoms on the basis of work with compounds and indeed raises the question of the significance of this procedure.

However, perhaps we do differ on the question of the assignment to these elements of positions in the periodic system and here I am led to my view by a consideration of evidence in many different areas and especially chemical evidence. For example, the chemical properties of U(IV) have no counterpart in tungsten and I feel confident that the aqueous solution chemistry of Pa(IV) will be found to be like that of Th(IV), U(IV), etc. I feel that the gradation in properties characteristic of 5f electrons is completely uniform going up the series and that there is no discontinuity (for example like that proposed between U and Np by some people or between Pu and Am or Pa and U by others) and therefore that the whole series should be placed uninterruptedly as homologues of the rare earths if it is desired to place each element only one place in the periodic table. This, of course, is not the whole argument. An interesting historical precedent can be found in Ce which was placed in the periodic table in group IV as a homologue of Zr before the understanding of the proper relationship to the rare earths took place....

I concluded by saying that I hope to have another opportunity to visit Harwell some day and that we may have a chance to discuss some of these matters in person.

Doral has found a nice girl (age 19) to hire as a clerk for our group. Shirley J. Daney will begin working when she receives a clearance. She has had two years of college (Ottawa University in Ottawa, Kansas), and her husband is attending Berkeley Baptist Divinity School. Doral is also interviewing girls to replace Virginia Hempel, one of our secretaries, who is terminating.

Since some of our summer guests have left and the lab is relatively quiet, I have decided to take next week off as vacation and work around the house.

Saturday, August 16, 1952

My main project during the coming week of vacation will be to build a redwood retaining wall on the property line between the Seaborg and the Perlman lots. This will consist of setting 4" x 4" redwood posts into the ground and nailing redwood boards onto the posts up to a height of about four feet. The dirt behind the wall will be dug out to make room for drain tile set in rocks, which will be connected to our backyard drain tile system. Since this involves a rather large amount of digging and moving of earth, I contacted the student employment office at nearby St. Mary's College in Moraga for a husky young man to work with me.

Sunday, August 17, 1952

I worked in the yard for a while. The contractors left a fair amount of debris when they built our addition.

Marvin Kalkstein, our postdoctoral student, is scheduled to marry Edda Davis of Oakland today.

Monday, August 18, 1952

On vacation. The student from St. Mary's College arrived promptly this morning, and we began working on the retaining wall project.

Iz said that he would look over my mail and inform me each evening of the significant events in the office and laboratories. This evening he told me that the Belgian chemist, Maurice D'hont, has arrived in Berkeley. Iz and Earl will handle D'hont's program although Helen and I will arrange to have D'hont visit our home to have dinner with the family.

Iz also mentioned that the local AEC office informed our office that Sir John Cockcroft will be visiting San Francisco October 1 through 4, that Sir John is interested in unclassified information at UCRL, and that he particularly mentioned that he desires to see me, if possible. Iz informed the AEC office that I am scheduled to be in town then.

Tuesday, August 19, 1952

On vacation. I worked much of the day on the retaining wall.

Wednesday, August 20, 1952

On vacation. I continued the work on the retaining wall. I also spend some time each day on the mail and papers that Iz Perlman brings home each evening from the office and on the speeches that are scheduled in the near future.

Thursday, August 21, 1952

On vacation. My student helper proved to be so unenthusiastic that he was really useless, so I continued my work alone on the retaining wall.

The research group met as usual this morning with the following

people attending: Carniglia, Carr, Clark, Conway, Feay, Glass, Gunn, Higgins, Hoff, Hollander, Hulet, Jaffe, Russell A. Kurtz (du Pont visitor), Levy, McLaughlin, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Shudde, Snyder, and Templeton.

Jaffe again reported on his study of Browne's 33 keV gamma ray of Am^{241} . He said they used a new mounting technique (developed by Browne) to mount another sample that Gary Higgins purified. This time there was some indication of splitting of the 33.4 keV group into 33.1 and 33.5 keV groups. He reported that their x-ray compilation gives the K x-ray groups of lanthanum as $K\alpha_1$ 33.5 keV, $K\alpha_2$ 33.1 keV and the three $K\beta$ groups as 37.8, 37.9, and 38.8 keV. He pointed out these last three could give the appearance of a single group around 38 keV. Jaffe went on to say there is no fission product activity of sufficiently long half-life to produce these lines without observation of an intensity change during a year but the results could be due to the photoelectric effect with inactive lanthanum, which was present to the extent of 15 to 25% in the americium sample. Jaffe also reported that he and Bob Carr purified another sample of americium by means of a concentrated hydrochloric acid column. When the sample, which now contained only 1% lanthanum, was run on the x-ray spectrometer, the 33 keV group was not seen. He said that, since the corresponding transition is known from alpha ray spectrography, it is a matter of setting an upper limit on the abundance. Perlman commented that Asaro's alpha ray spectrographic data in conjunction with Jaffe's new data indicate that the 33 keV gamma ray must be largely converted, perhaps over 90%.

Hoff talked about the measurement of the alpha spectra of At^{211} and Po^{211} on the alpha ray spectrograph. He pointed out that Perlman and Neumann, using the alpha pulse analyzer, found the following alpha groups of Po^{211} : 7.43 MeV (98.8% abundance), 6.90 (0.71%), 6.90 (0.48%), 6.34 (0.075%). Hoff has found the first three but not the last (because of low intensity) on the alpha ray spectrograph. He also said that he did not see the 7.28 MeV alpha particle of the isomeric state of Po^{211} , and it is concluded that less than 1% of At^{211} decays to the isomeric state of Po^{211} .

Feay described the methods they are trying in order to obtain x-ray diffraction pictures with five micrograms or less of sample. It is necessary to use such a small amount of material when they work with curium in order to avoid blackening of the plate by radiation. One definite line has been seen in one picture. Rasmussen asked about the breakup of the crystal structure by alpha radioactivity, and Feay said they plan to photograph the samples as soon as possible after preparation and then, if a good picture is obtained, photograph a few days later.

Higgins presented calculated yields of various nuclides produced in a sample of americium that received a total of 10^{21} neutrons per square centimeter in the Chalk River pile. The amounts of Cm^{242} and Pu^{238} found checked their calculation. Higgins presented similar calculated yields for another Chalk River bombardment of 118 mg of Pu^{239} . In this case they recovered about 14 mg of plutonium, including 1 mg of Pu^{242} . The total weight of americium recovered was about 100 micrograms. He said this particular bombardment produced the highest specific activity of

Pu²⁴¹ ever produced.

* * * * *

Friday, August 22, 1952

On vacation. The retaining wall was completed today.

Saturday, August 23, 1952

It has been a productive week. In addition to building the retaining wall and cleaning up the yard, I completed work on some of the talks I have scheduled.

Sunday, August 24, 1952

I pattered around the yard and did some reading.

Monday, August 25, 1952

The first thing I did this morning was to look over the mail that arrived last week.

Donald Lane sent me a draft of a report that he proposes to file with the AEC Patent Compensation Board about his "negotiations" with the Regents and requesting a further prehearing conference. Lane wrote, "It is my present thought that we should contend that the applicants' title is clear, since the Government has accepted assignments of entire right, title and interest to hold in escrow, and the Government has acknowledged applicants' commercial rights in contract eng-30. Therefore, if the AEC desires to question applicants' title by the Regents' unsupported claim of an undefined interest, the burden of ascertaining the Regents' interest should rest on AEC counsel."

Lane went on to say that he would like to get the Patent Compensation Board reaction to this argument before we make any decision to push a separate claim solely for an award based on reports as distinguished from the claim for compensation based on inventions. I read copies of letters from Kennedy, Segrè, and Wahl addressed to Lane in which they approved of his proposal.

I also read an August 14 letter from Sherman Fried that Iz had described to me. Sherman said that he has been put in an uncomfortable position at Argonne and will probably have to look for another position. He explained that the medical people insist upon giving him routine x-rays at this time and since he took a sizable dose of radiation during his recent stay at the Radiation Laboratory, he is unwilling to take more just for the sake of keeping the medical files up to date. He went on to say that the administration does not agree with his point of view and the choice may be between submitting to the medical people or leaving the Laboratory. He believes he will choose the latter and asked if I know of any position that he could suitably fill.

I read an August 13 reply from F. A. Paneth to my letter of July 31. Paneth described the question of membership on the Joint Commission on

Radioactivity, which is still a confused situation. He wrote that it seems to him the whole question will have to be discussed at the next meeting (the XIIIth International Congress of Pure and Applied Chemistry) in Stockholm. He added that he would much regret it if I resigned.

I had two letters from Louis N. Ridenour, who accepted my suggestion that instead of the lecture on nuclear structure on October 27 (William A. Fowler of Cal Tech will give this lecture), I speak on March 16, 1955 on "The Actinide Elements and Atomic Power." In his second letter he asked for a summary and a glossy print of a recent photograph.

In an August 18 letter Warren H. Crowell thanked me for keeping him posted about my fall schedule and said he hopes to hear shortly from President Sproul about the date for the UCLA Alumni Banquet.

I looked over an August 21 letter from Frank Ward (Editor, Reactor Science & Technology), who thanked me for my letter of August 14 and my acceptance of their invitation to continue to serve as a member of the Editorial Advisory Board. Ward said they would be pleased to have me write a guest editorial on any subject, classified or unclassified, for the Seventh Issue, scheduled for April 1953 production. He said they would like to have the editorial for review by January 1, 1953.

In an August 14 letter Pfc. Eric W. Kjellmark, Jr. (Enlisted Specialists Chemical Engineering Club, Army Chemical Center, Maryland) expressed appreciation that I have agreed to speak to them on September 18. He asked me to let him know if I need any equipment for the talk. I immediately wrote this morning and asked for a blackboard and a standard projector.

I also replied to an August 19 letter from Roger Adams (University of Illinois) and said that I shall be able to attend the meeting of the Chemical Corps Research Committee at the ACS meeting in Atlantic City on Sunday, September 14 at 1 p.m. and shall meet him in his room at the Traymore Hotel.

The senior staff had its regular brown-bag staff meeting in my office at noon.

Part of the afternoon was spent visiting a few labs and talking with various people about the research.

Tuesday, August 26, 1952

I stopped on campus to speak with some of my colleagues before going up to the hill today.

After the usual phone calls, etc., I replied to Donald Lane's letter of August 14, saying in part:

I approve in general but should like to raise several points. Do you think that it would be worth getting into the appended correspondence the thought that The Regents has essentially refused in a broad sense to define its claimed interest using as a basis Paragraph 5 of Underhill's letter to me of May 6, 1952? You state in the last

paragraph of the summary of negotiations in your proposed report that "The Regents refuses to define in any way its claim of an 'interest'" but you document this only with specific refusal to accept Mr. Harter's suggestion.

In the fourth paragraph of your letter of August 14, 1952, you mention "an award based on reports, as distinguished from the claim for compensation based on inventions." Is this the way the law reads? I believe we had in mind, perhaps erroneously, that a claim for an award and separate claim for compensation should both be based on the same things.

I went on to inform Lane that I shall attend a meeting in Washington on October 21, 22, and part of October 23 and will try, as usual, to get in touch with him.

In today's mail was a note from Lars Melander (Nobel Institute, Stockholm). Melander included in his letter a circular about the XIIIth International Congress of Pure and Applied Chemistry, which will be held in Stockholm next summer. He added, "We sincerely hope that it might be possible for you to arrange your participation. It seems highly desirable that your important contributions especially to the knowledge of the chemical behaviour and electronic structure of the heaviest atoms are represented in the Physical Chemistry Section." Lars sent his best regards to me and Helen.

Julia Stanford, secretary to Warren H. Crowell, wrote to inform me that President Sproul suggested the night of October 18 for the UCLA Alumni Banquet.

During my rounds today I learned that Gunn and Hulet have been taken off the americium purification project; Surls and Mark Snyder (du Pont man) are scheduled to begin to work on it this afternoon.

Wednesday, August 27, 1952

I took care of some miscellaneous administrative duties and then talked for a while with Gerhart Friedlander. Friedlander, who is on the staff at Brookhaven, is going to spend a couple of months with us and, among other things, will look over the revised "Table of Isotopes." [I have known Gert since he was an undergraduate and graduate student working with me here in Berkeley before the war.]

I went out to Mira Vista for a game of golf with Alvarez, Ghiorso, and Perlman. We played the last nine holes first (LWA-47, AG-43, GTS-49, IP-47); Alvarez and Ghiorso won the low-ball, low total match, 9 and 8. We then began the first nine. Unfortunately, I had to leave after 14 holes.

Thursday, August 28, 1952

The research group meeting this morning was attended by Carr, Clark, D. Conway, J. Conway, Dunlavey, Glass, Friedlander, Higgins, Hoff, Hollander, Huffman, Hulet, Hyde, Jaffe, Levy, McLaughlin, Naumann, Nervik, Passell, Perlman, Ruben, Seaborg, Shudde, Slater, Snyder, Street,

and Templeton.

Naumann talked about the progress in his attempts to mass spectrographically identify the 6.7-hour high lying state of Mo^{93} . He stated the upper transition has been designated E4 by Goldhaber and that one of the objections to the decay scheme lies in the absence of crossover transitions expected from a consideration of states available to a 51st neutron. He bombarded niobium foil on the 60-inch cyclotron with 10 Mev protons for 6 hours (200 microamperes). He described the subsequent chemistry. In response to a question from Perlman, he said that he and Jaffe saw molybdenum x-rays on the bent crystal spectrometer in the sample. Naumann said the activity apparently should decay to the long-lived ground state, to which a lower limit of one year has been set for its half-life. I suggested that it might be better if the activity were assigned to Mo^{94} , and Friedlander agreed, saying that would be an even-even nucleus decaying to a stable ground state and the large level spacings would be much more plausible.

Dunlavey reported that he bombarded some neodymium metal in the internal helium ion beam of the 60-inch cyclotron to look for Sm^{146} , which is predicted to be a long-lived alpha particle emitter. He described the column chemistry and said that spectrographic analysis showed the neodymium mass to be spread out and that the samarium activity came off at just about the same time as the first neodymium mass but in a sharper peak. Dunlavey went on to say that the sample has too much neodymium mass to pulse analyze but can be impregnated into emulsions for alpha energy measurements after the majority of the beta activity dies out. There was some discussion about the bombardment and the chemistry.

Shudde described his alpha ray spectrometer that utilizes the magnetic field of the cyclotron. He said they standardized the setup with Ra^{226} and compared his results with those of Asaro. In the discussion about the work, I suggested that he should use short bombardments in order to reduce the beta activity induced in the apparatus by stray neutrons.

John Conway mentioned that he is going to Argonne soon to look for an isotope shift in plutonium in the optical spectra. He said they possibly will see the fine structure. I added the spin will most likely be $7/2$ or $5/2$.

Friedlander told the group about work going on at Brookhaven by John A. Miskel and Morris Perlman. They have been investigating the primary charge that atoms carry following radioactive processes. He went into some detail about their setup and experiments--the first case they looked at was A^{97} , in which they found the average charge per disintegration to be 3.85 ± 0.2 . I mentioned that work has been done at Argonne on the charge of the daughters in beta decay, and I believe the results are higher than first expected.

* * * * *

Up on the hill, as a result of our recent discussions and consultations, I sent the following memorandum to W. B. Reynolds.

August 23, 1952

MEMORANDUM:

TO: W. B. Reynolds
FROM: Glenn T. Seaborg

SUBJECT: Source and 250 KV injector for heavy ions-project request.

An intense beam of multiply charged ions such as carbon, nitrogen, oxygen, fluorine or neon at 10 MEV per nucleon would provide the possibility of producing elements of higher Z than Californium ($Z=98$) and would provide new methods for studying nuclear structure. Research at present with carbon ions in the 60 inch cyclotron is severely limited because the high energy beam is weak and is accompanied by a useless strong low energy carbon beam. It is difficult to increase the high energy carbon intensity because the ion source in the center of the cyclotron is limited in volume.

Prof. E. O. Lawrence has suggested using an external ion source and linear accelerator. If a good ion source can be developed, a new linear accelerator can be designed or the present building #10 linear accelerator can be converted to produce useful beams of the above ions. The purpose of this memorandum is to request authorization for a project to develop a good ion source and 250 KV injector for the above ions. Prof. L. W. Alvarez requests that he be considered a consultant, not an administrator, on this project and suggests that the coordinator, Craig Nunan, be assigned responsibility for approving purchases and construction with the understanding that Prof. Alvarez will be consulted on all major items prior to taking action.

Present plans are to spend about 2 months making measurements with the 4 MEV Van de Graaff and with the large ion pump in building 51. After this period an ion source and injector will be designed and constructed. It is expected that the injector will be about half the size of the 500 KV Cockroft-Walton for the bevatron. About 3 months from the start of the project, space will be required for assembly of the ion source and injector. This space should be about 30 x 30 feet with overhead crane. If space can be made available in the new synchrotron building, the 250 KV D. C. power supply in the corn crib back of the synchrotron can be used to power the injector, thereby eliminating the cost of the Cockroft-Walton oil-filled capacitor stack, r. f. power supplies and oil cooling system at a saving of several thousand dollars. The whole project should last about 8 months and will require the following personnel:

Mechanical Design (Brobeck) - one engineer, half time.

Electrical Design (Norton) - one engineer, quarter time.

Mechanical Assembly (Twitchell) - two accel. techs., half time.

Mechanical Shops (Harvie) - all mechanical parts for injector.

Electric Shops - control chasses for injector.

Electrical Installation - Move control eqpt. for ion pumps.
Install control eqpt. and power supply for
Injector.

Research Group - Craig Nunan, Bob Watt and one physicist.

Costs are estimated as follows:

Material costs and shop salaries for construction of:

(a). Ion source	5,000
(b). Injector	25,000
Miscellaneous test equipment	10,000
Salaries of Design, Assembly and Research Groups	<u>20,000</u>
	\$ 60,000

The above estimate assumes that the ion pump can be borrowed at no cost for the duration of the project and can be dismantled for parts for the final ion source. It also assumes the use of the corn crib 250 KV supply.

Glenn T. Seaborg

cc: L. W. Alvarez
C. S. Nunan

A note went to William Shockley, who has agreed to be the after-dinner speaker at the meeting of the Division of Physical and Inorganic Chemistry at the Atlantic City ACS meeting. I informed Shockley that the dinner meeting will be held in the main dining room of the Brighton Hotel starting at 6:30 p.m. on September 17 and that we are looking forward to having him with us.

I also wrote to Frank L. Lambert (Pacific Southwest Association of Chemistry Teachers) and asked if he could arrange a pickup for me at my parents' home in South Gate (9237 San Antonio Street) around 9:45 a.m. before the lecture on September 27. I also asked to be driven to the Los Angeles Airport to catch a 2:00 p.m. (PDT) plane.

I looked over a letter from Richard L. Doan (Manager, Atomic Energy Division, Phillips Petroleum Company, Idaho Falls, Idaho), who enclosed twelve copies of a proposed letter agreement between the University of California and Phillips Petroleum Company relative to experimental work that will be sponsored by the University of California at the Materials Testing Reactor. Iz and I looked it over--this is something that has to be executed by W. B. Reynolds.

A memorandum arrived for me from Hubert N. Alyea, who is in charge of a symposium on "Recent Advances in Nuclear Chemistry" in the Division of Chemical Education at the ACS meeting in Atlantic City. I am scheduled to be the second speaker (from 9:30 until 10 a.m. on Tuesday, September 16). Alyea said the hotel and room number will be announced in the final program.

The Division of Physical and Inorganic Chemistry generates a lot of mail, most of which I merely have to note in my position as Chairman. Today I read a copy of a letter from my old fellow graduate student, David C. Grahame, to Joe Kennedy (Secretary and Treasurer of the Division) with a question about two of his speakers who originally said they did not want to submit papers but now have changed their minds.

Friday, August 29, 1952

After the usual phone calls, etc., I dictated answers to some of the correspondence that has accumulated.

To Sherman Fried's letter of August 14, I wrote, "It seems to me that you are contemplating a serious step, and I feel that I should like to have a talk with you before helping you in the way that you suggest. I am planning to visit Argonne on Friday, September 19th, on my way home from the Atlantic City ACS meeting, and perhaps we could have a talk at that time. I have given the matter a good deal of thought, but I feel that I would rather defer discussion of it until we can talk it over in person rather than to go into it by the cumbersome method of correspondence."

A teletype arrived yesterday from John C. Tervo (Personnel Representative, Los Alamos): "CONSIDERING EMPLOYING CHARLES I. BROWNE, JR. PLEASE ADVISE BY RETURN COLLECT WIRE YOUR COMMENTS REGARDING ABILITY, EXPERIENCE, PERSONALITY, INITIATIVE, AND ANY OTHER STATEMENTS YOU FEEL MAY BE OF INTEREST TO US. COMMENTS STRICTLY CONFIDENTIAL." My

reply today read, "CHARLES I. BROWNE, JR. RANKS WITH OUR BEST PH.D. GRADUATES IN NUCLEAR CHEMISTRY OF RECENT YEARS. UNQUALIFIED RECOMMENDATION. THOROUGH MASTERY FUNDAMENTALS CHEMISTRY AND NUCLEAR SCIENCE. EXCELLENT COURSE RECORD. DISPLAYED GREAT INITIATIVE, RESOURCEFULNESS, AND PRODUCTIVENESS IN RESEARCH PROBLEM IN MEASUREMENT OF NUCLEAR RADIATIONS. EXPERIENCE IN STRAIGHT RADIOCHEMICAL OPERATIONS SOMEWHAT LIMITED. PERSONALITY EXCELLENT. CAPABLE INDEPENDENT WORK. COOPERATIVE TEAM WORKER."

Lyle H. Jensen (former member of my group at the Met Lab and now in the Department of Anatomy, School of Medicine, University of Washington) wrote me a note, saying that he and Mrs. Jensen will be in the Bay Area on September 5 on their way to southern California and in Berkeley on September 15 or 18 on their way back to Seattle. He said he would enjoy seeing me if I happen to be in Berkeley then. I replied that I shall be in the laboratory on September 5 and shall be delighted to see him. Then, I mentioned that I shall be attending the Atlantic City meeting during the week of September 15 but many other of his friends will be here then.

I wrote to Charles Coryell to tell him that shortly we shall mail him one of a very limited number of pre-issue rough draft copies of our 1952 revision of the "Table of Isotopes." I said we would be appreciative if he and anyone else whom he thinks would be interested would check it over. I told Charles that Dr. Hollander is planning to be in Boston on October 20th and 21st and, if convenient, would like to meet with him to discuss suggestions and additions. I pointed out that parts of the table are quite out of date, but we hope to remedy this before his trip. I also wrote that we would like to receive comments and criticisms as soon as possible and especially would like to receive any unpublished data that should be included.

Cliff Garner (UCLA) sent me on August 25 some information about postdoctoral positions at UCLA. I wrote him a note today, saying that I shall call his letter to the attention of the members of our group. I wrote, "Unfortunately, some new projects are being started here and these are tending to absorb an appreciable number of our Ph.D. output."

On August 14 Howard A. Meyerhoff (Chairman, Editorial Board, Science) wrote requesting a couple of different kinds of summaries (600 words or 1000 to 4000 words) of the Gordon Conference on Nuclear Chemistry. Meyerhoff described the summaries in some detail and said he realized that no publicity will be given of the papers presented without the consent of the authors, but George Parks was so enthusiastic about our conference that he hopes I will prepare one of the summaries he mentioned. In my reply I explained that the participants had a detailed discussion about the question of publishing a summary with the result that it was decided that this would not be done this year. I wrote that I believe this recommendation should be changed and will so recommend next year. I wrote, "Since the recommendation this year was definite, I don't feel that I or anyone else would care to participate in the preparation of a summary for publication."

I wrote a joint letter to John C. Dillon and Louis N. Ridenour, enclosing the information and the photograph that was requested in Dr.

Ridenour's letter to me of August 20, 1952. This is in connection with the course "Modern Physics for the Engineer," in which I have promised to lecture on March 16, 1953.

Saturday, August 30, 1952

My day was spent on various chores, playing with the kids, reading, etc.

Sunday, August 31, 1952

This was an ordinary Sunday.

Since we returned from Chicago, Helen has been working with an interior decorator, Bill Simpson, on the refurnishing of our living room. (We have been living with the old furniture moved from our Ellsworth Street home.) With Simpson's advice she has purchased custom-built furniture and drapes and is now in the process of shopping for end tables, lamps, etc.

Monday, September 1, 1952 (Labor Day)

This holiday was spent with my family. Pete and Lynne are getting excited about going to school tomorrow.

Tuesday, September 2, 1952

I had a number of phone calls and conversations this morning, in addition to an appointment in First Aid for a blood count. Then I caught up on some report and journal reading.

At home I learned that Pete and Lynne had a successful first day of school. Pete is now in the first grade; his teacher is Phyllis Ryerson. Lynne has the same kindergarten teacher Pete had last year--Ruth Anderson. They are attending, via bus, the Vallecitos School.

Wednesday, September 3, 1952

I signed a couple of replies that Doral wrote to letters I received yesterday. To Pfc. Eric W. Kjellmark, Jr. of the Enlisted Specialists Chemical Engineering Club of the Army Chemical Center, Maryland, I wrote that I shall need a projector that will take the 3 1/4" x 4" slides (this is for my talk there on September 18.)

Also in connection with my trip to the Army Chemical Center I replied to an August 22 letter from Miss Juanita M. Keister (Secretary to Dr. C. B. Marquand). I wrote that I shall need no reimbursement and therefore no TR's. I also gave Miss Keister my travel schedule to Edgewood from Atlantic City on September 18 and asked her help in arranging transportation from Edgewood to Washington, where I have airline reservations the same day on American Flight 71 at 8 p.m.

Iz showed me a letter he had received from Rod Spence (Los Alamos), who included the chapter he wrote on high energy fission for the Annual Review of Nuclear Science. Spence also sent a summary of their fast fission results on U^{235} and a report by George P. Ford on independent fission yields from the 14 Mev neutron fission of U^{235} (not declassified). Rod asked Iz to tell Al and me that he took a ten on a par four hole the other day after lacing his drive 250 yards down the middle of the fairway.

I made the rounds of some of the labs to check on the work. Stan gave me a copy of the letter he wrote to Fred Albaugh with the analysis of the CR-6 plutonium (the latest plutonium sample irradiated at Chalk River): 239 (30.4%), 240 (39.4%), 242 (6.8%).

Thursday, September 4, 1952

The "first of the month" research group meeting was attended by Asaro, Gerald A. Behman (technical editor), Friedlander, Glass, D. Conway, Higgins, Hoff, Hollander, Hulet, Kalkstein, Kofstad, Larsh, Levy, Mathur, Michel, Naumann, Nervik, Passell, Perlman, Rasmussen, Ruben, Seaborg, Shudde, Slater, Street, Templeton, and Thompson.

Slater spoke about his study of the excitation functions for the

$\text{Th}^{232}(\text{d},\text{p})\text{Th}^{233}$ reaction in which he found that the maximum cross section is considerably lower than for the $\text{U}^{238}(\text{d},\text{p})\text{U}^{239}$ reaction. He repeated the work on Th^{232} and found the results about the same in shape of the curve and position of peak. The entire Th^{232} excitation function curve is shifted upward. He said both peaks are at about 14.8 Mev with the maximum for U^{238} at about 1/3 barn and that for Th^{232} about 150 mb; however, the plot of the U^{238} excitation function is considerably wider than for the Th^{232} excitation function. Slater also commented that he believes the discrepancy in the cross sections between the two thorium runs is due to a short in the Faraday cap that measures the beam intensity on the first run. He plans another run. There was considerable discussion about the reasons for the difference between the uranium and thorium peaks.

Higgins talked about his work with Bob Carr on the neutron-deficient curium isotopes produced in the bombardment of Pu^{239} (2 gt) with helium ions in the 184-inch cyclotron. He said the energy of the incident helium ions was varied and the differences in the activities produced in the curium were noted. The activities (previously reported) were: Cm^{238} 2.3 hours, 6.5 Mev alpha particle, EC, 23-day partial alpha half-life; Cm^{239} about 12 hours, EC; Cm^{240} 26 days, 6.26 Mev alpha particle, 26-day partial alpha half-life. Higgins went on to say that it is apparent that Cm^{238} could not have about 250 kev more alpha particle energy than Cm^{240} and still have a partial alpha half-life that is anywhere near as long as that measured. They concluded that there was some other EC activity of approximately the same half-life (2.3 hours) that was being included with those electron capture events attributed to Cm^{238} and hence increasing the ratio of EC to alpha disintegrations and lengthening the partial alpha half-life. By varying the helium ion bombardment energies (47, 50, 55 Mev), they concluded that there is another about 2.3-hour activity in the curium. They calculated (assuming that less than half of the 2.3-hour activity seen in the highest energy bombardments is due to Cm^{238}), that the partial alpha half-life of Cm^{238} is less than 13 days. He said the assumption that less than half the 2.3-hour activity is due to Cm^{238} is strengthened by the fact that the ratio of $\text{Cm}^{240}/\text{Cm}^{239}$ was 10^3 - 10^4 .

Higgins also said they have also concluded, since separations from americium were performed, that 12-hour Cm^{239} does exist since the 12-hour activity was about six times too large to have been produced from the decay to Am^{239} of the 2.3-hour activity. He went on to suggest that the rest of the 2.3-hour activity may be an isomer of Cm^{239} , Cm^{240} , or Cm^{241} although no isomer was observed in either of the latter two. Further work will be done. Higgins said they saw a 700-800 kev gamma ray decaying with the 12-hour half-life when they examined one of the americium fractions in a crystal scintillation counter; this may be produced in the decay of Am^{239} . He also noted that the 12-hour half-life of Am^{239} is accurate but the 12-hour curium half-life is subject to large uncertainties. Perlman commented that it would be interesting if there were a metastable state of Cm^{239} since it has the same neutron number as U^{235} , which is suspected of having a metastable state.

Larsh described a new type pulse height analyzer that has been designed around a tube that has two independent control grids and is often used for coincidence-work. A ten-channel model has been

constructed.

* * * * *

Up on the hill I wrote a memorandum to Don Cooksey requesting approval for my trips to Los Angeles this month (September 24-27) and next month (October 18). I explained the purpose of the trips and said that I shall go directly from Los Angeles to Washington, D.C. to attend a meeting of the AFOAT IO-7 Panel and to confer on various matters at the AEC offices, spending October 20-23 inclusive in Washington. I said that it would be simpler to charge my travel to the Laboratory, but I could request TR's from the Research and Development Board for most of this trip East if he would prefer.

In today's mail was a reply from Donald Lane to my letter of August 26. He said he adopted my suggestion of adding a copy of Mr. Underhill's letter of May 6, 1952 to me to the report of negotiations, saying that perhaps it is well to have the Patent Compensation Board learn that Mr. Underhill charges that we are attempting to reduce the Regents to a licensee status. Lane included some retyped pages for his draft reply papers that he sent me on August 14. In addition, he went into some detail to explain my question about compensation and awards under the Atomic Energy Act of 1946 and concluded by saying, "You will therefore understand that it is my thought that your claim is in a more valuable area if we can make it depend on patentable inventions which the Act keeps you from patenting, licensing, and using. In the event that your legal title to patentable inventions cannot be established and compensation therefore not be obtained, we can then push the pending claim for a suitable award on the theory that valuable discoveries have been reported as set forth in this claim. I am not quite ready to bypass the compensation angle because I don't see how the Compensation Board can uphold or place a value on the Regents' undefined claim of interest in inventions."

I read a note from Bill Glenn, now with General Electric in Schenectady, who thanked me for some reprints, asked if Herb York ever wrote his report on thermonuclear power, and said that he and Dick Shuey are now working on color television. I routed the letter to Iz, Stan, Al, Earl, and Herman.

I spent some time going over some information provided by Craig Nunan in order to prepare "Proposal for Construction and Research Use of a Heavy Ion Accelerator." I wrote an introduction and am incorporating some material that John Rasmussen has prepared.

Friday, September 5, 1952

I took care of a number of miscellaneous administrative duties and talked with some of the men and students about their research.

Lyle Jenson, who worked in my group at the Met Lab, stopped in for a visit, and I spoke with him for a while.

This morning Doral had one of the staff type the draft proposal "Proposal for Construction and Research Use of a Heavy Ion Accelerator" and distribute it to various interested people.

PROPOSAL FOR CONSTRUCTION AND RESEARCH USE
OF A HEAVY ION ACCELERATOR

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 of 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.

I. PROPOSED RESEARCH PROGRAM

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 such processes; this adds much to the 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.

2. Nuclear Reaction Mechanisms and Structure

Much can be learned from observing the behavior of heavy ions upon contacting light, medium and heavy nuclei. There are interesting

questions concerning the extent to which the intermediate compound nucleus gives a description of the reaction mechanism and concerning the general problems of nuclear coalescence and surface tension effects in this process which can be regarded as a sort of reversal of the fission process. The competition between the various reactions and the excitation functions for the various end reactions should give information on the mechanisms for these reactions, which should be of interest to the general development of nuclear theory. A possible practical application may be in the area of experimentation in connection with the investigation of thermonuclear reactions.

Heavy ion bombardments may offer a good test of the compound nucleus theory at higher excitation energies. High energy proton bombardment spallation product analysis is made more complicated by the nuclear transparency to high energy nucleons. One must consider "knock-on" particle emissions that precede establishment of "thermal equilibrium" in the compound nucleus. With heavy ion projectiles 100-200 Mev excitation can be transferred to the target nucleus by the aggregate of low (10-20 Mev) energy nucleons without the complication of transparency and knock-on effects.

3. Production and Study of Highly Neutron Deficient Nuclides of the Heaviest Elements

Heavy ion bombardments can lead to the preparation of many isotopes of the presently known transuranium elements which cannot be prepared in similar isotopic purity or in as high yield by simple (p, xn) , (d, xn) or (α, xn) reactions. These are of interest from the standpoint of the basic

understanding of nuclear structure and theory for essentially the same reasons as already cited in connection with the search for transcalifornium elements.

The absence of suitable target materials of elements 84 through 89 makes impossible the production of nuclides of Z from 86 through 90 by simple (p,xn), (d,xn) or (α ,xn) reactions. Thus the availability of heavy ion beams opens up a new field for isotope study in this region. The alpha decay energies are relatively low for isotopes with 124, 125 or 126 neutrons; hence, their half-lives are relatively longer than neighboring isotopes. Study of these nuclides of elements 86 through 90, free of heavier isotopes, is made possible by heavy ion bombardments of lead and bismuth targets.

4. General Study of Highly Neutron Deficient Nuclides

In the medium heavy region (Ag through Pb) the half-lives of nuclides as many as ten or twelve neutrons on the light side of beta stability are often long enough for study (i.e., 4 hr Tb¹⁴⁹ - stable Tb¹⁵⁹). However, production by high energy proton and alpha bombardments inevitably produces mixtures containing many heavier isotopes, as well as the desired ones. Careful study of the radiations accompanying decay of these highly neutron deficient nuclides is rendered difficult or impossible by the presence of so many other activities in the samples. Production of highly neutron-deficient nuclides, relatively free of heavier isotopes, is possible. For instance, it will be possible to look for alpha activity in the 50 neutron region by heavy ion bombardments, where proton bombardments have failed because of large amounts of undesired beta-gamma active heavier isotopes.

5. Study of Interactions of Heavy Ions with Matter

Monoenergetic beams of heavy ions would allow systematic studies of range-energy relations in solids, liquids, and gases. Studies of range, grain density, and multiple scattering of heavy ions in photographic emulsions could be carried out.

The problem of the ionization vs energy relation in ion chamber pulse height studies could be greatly facilitated. The nature of the "ionization defect" noted in fission fragment counting might be better understood and numerical formulas for the defect derived.

The effects of heavy ion radiation in altering physical properties of materials may be significantly different from radiation damage effects caused by the less heavily ionizing radiation now available for such studies. The same considerations apply to radiation chemistry studies.

The radiation damage to living organisms caused by heavy ions may, as Tobias has suggested, be significantly different from that caused by the less heavily ionizing radiation now available for bio-radiation studies. That is, the single heavily ionizing carbon or oxygen ion might be able to destroy a nerve ending, whereas many of singly or doubly charged particles would be needed to accomplish the same damage. A study of such effects has a possible practical application in connection with the problem of the protection of personnel from cosmic rays in ultra high altitude flying.

Saturday, September 6, 1952

I worked in the yard for a while today. Later Lynne's fifth birthday was celebrated with a party.

Sunday, September 7, 1952

I again worked in the yard, played with the kids, and read.

Monday, September 8, 1952

One time consuming activity is the reading of reports and journals; this is something I try to do daily.

In today's mail was a letter from Jim (James W.) Cobble, informing me that he will be able to report to Berkeley during the last week in October as their new daughter has arrived; they anticipate leaving Oak Ridge on October 11. In response to my request, Jim said he talked with the various people working on decay schemes. He enclosed some information by W. S. Lyon on gamma branching in K^{42} and Cr^{51} and said that most other people feel that the Quarterly Reports summarize their work fairly completely. He mentioned that G. E. (George) Boyd is finishing up his tin and technetium work and may have a preprint ready before our deadline (for the "Table of Isotopes") and that A. R. (Albert R.) Brosi will send a preprint on his Ga^{67} decay scheme. Cobble suggested that Dr. Hollander can get the information when he arrives. Cobble also said that he had contacted Mr. Everson about the shipment of their household effects and has given Mr. Everson an address and phone number through which he can be reached while they are in transit. I acknowledged Cobble's letter and said that we are looking forward to seeing him during the last week of October.

One topic at our noontime senior staff luncheon meeting was the draft of "Proposal for Construction and Research Use of a Heavy Ion Accelerator," which some of the men have looked over.

Doral is in the process of assembling slides, etc. for my forthcoming talks.

Tuesday, September 9, 1952

This morning I signed a memorandum addressed to George Everson about the fall changes in the Chemistry Division payroll. I listed the graduate students whose hours will change to half-time on September 16: Frank Asaro, William F. Biller, Stephen C. Carniglia, Robert J. Carr, Edward S. Clark, Darrell C. Feay, Richard A. Glass, Stuart R. Gunn, Richard W. Hoff, E. Kenneth Hulet, Charles A. Hunt, Harold Jaffe, Harris B. Levy, Robert E. Lundin, Ralph McLaughlin, Floyd F. Momyer, Walter E. Nervik, Thomas O. Passell, John E. Powers, Homer Rea, Rex E. Shudde, Louis M. Slater, Harry E. Spencer. I then described a few special cases, such as Dean C. Dunlavey (who has completed doctorate requirements and will enroll in the University of California Law School but will continue to do research with us on a half-time basis at \$145/month beginning September 16), Maynard C. Michel (who will continue to do research with us but should go off the payroll because he has been awarded an Ethyl

Corporation scholarship), Charles W. Koch (who will continue on the Chemistry Department payroll), Edward J. Lynch (who should complete his doctorate thesis within the month and should be continued at the full-time rate of \$290 monthly, and Marshall W. Cook (who will continue on the same hours as last fall--24 hours per week at \$174 a month).

After some phone calls and other administrative matters, I went through the labs and checked on the research.

I replied to a telegram from J. D. Cameron and T. L. Cole (General Electric, Schenectady), who asked for information about William E. Glenn: "WILLIAM E. GLENN EMPLOYED RADIATION LABORATORY 10/47 TO 3/52. SERVICES EXCELLENT. WOULD BE DELIGHTED TO REHIRE. KNOW OF NO REASON TO QUESTION LOYALTY TO U.S." (These days nearly all of our requests for evaluation ask about the candidate's loyalty to the United States.)

This was a school holiday for Pete and Lynne--Admission Day.

Wednesday, September 10, 1952

After the usual phone calls, etc., I replied to a letter I received yesterday from C. G. A. Rosen (Caterpillar Tractor Co.), who heard me speak on the potential uses of nuclear energy at the semi-annual meeting of ASME in San Francisco late June of 1949. Rosen, who is preparing a paper for presentation before the American Society for Metals in Detroit on October, asked for a copy of my address for review. In my response I explained that the speech was never published since I spoke extemporaneously, but I sent him a sort of summary of the talk that I was asked to prepare. I wrote, "Although in the summary I generalize in the first sentence of the second paragraph, I do recall that in the actual talk I emphasized the importance and indeed the absolute necessity of solving numerous problems in the domains of mechanical engineering and metallurgy before we might have nuclear energy for industrial power."

At the end of August a number of rough drafts of UCRL-1928 ("Table of Isotopes" by Hollander, Perlman, and Seaborg) were sent to active participants in the field. We asked for criticism and mentioned that Hollander will be visiting the various labs and universities in October. We are beginning to receive some responses. Today a note arrived from the secretary to Martin Deutsch (MIT), explaining that he is in Europe and will return the end of the month.

Coryell wrote that he will have the radiochemists look over the areas they are currently up on before Hollander arrives. He suggested we send copies to Ronald A. Brightsen of Westinghouse in Pittsburgh and Alexis C. Pappas at the University of Oslo. On other matters Coryell mentioned that he cannot attend the dinner meeting of the Advisory Board of the Gordon Research Conferences in Atlantic City on September 15 and asked for a substitute. He also asked for one or two reprints of my lecture on beta systematics (at Ohio State University last spring) and for comments on the draft of his Annual Reviews of Nuclear Science manuscript.

During my rounds, Herman Robinson introduced me to Mrs. Elinor G. Potter, who will work with him as a counting girl.

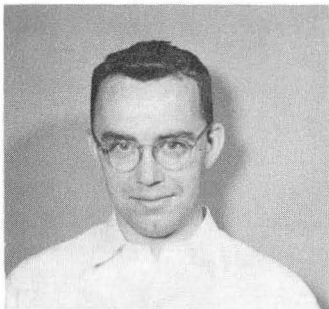
Elinor Potter



Thursday, September 11, 1952

The meeting of the research group this morning was attended by Asaro, Carr, D. Conway, Cunningham, Dauben, Dunlavey, Feay, Glass, Gunn, Friedlander, Higgins, Hollander, Huffman, Hyde, Jaffe, Kalkstein, Levy, McLaughlin, Michel, Nervik, Passell, Perlman, Rasmussen, Ruben, Seaborg, Shudde, Slater, John H. Shenk (a visitor and head of Research Department, Naval Ordnance Test Station in China Lake, an x-ray diffractionist), Frank S. Stephens, Jr. (a new graduate student, who graduated from Oberlin College in June),

Frank Stephens



Templeton, and Thompson.

Michel reported that they have used the now-functioning time of flight mass spectrometer to make some mass assignments of thallium isotopes produced by the bombardment of gold with helium ions (60-inch cyclotron). They collected mass number 198 (half-lives 2 hours and 7 hours) and mass number 199 (half-life of 7.1 hours). Activity collected at mass number 200 and 205 is believed to represent background of vaporized material. Michel said the total collection time was four hours and that it seems possible that some mass number 199 collected on the 198 mass number plate; however, it may be that mass number 198 does have a metastable isomer and hence both a two-hour and a seven-hour half-life.

Asaro said they had run a 50 microgram sample of Pu^{241} (with contaminating $\text{Pu}^{239-242}$ in the alpha particle spectrograph. (The sample was obtained from Thompson and Higgins and had been mass analyzed; it is the heaviest sample that has been used in the spectrograph.) Asaro went on to state that the alpha particles from the two known alpha groups of Pu^{240} predominated (about 90% of the alpha activity), and they found some new alpha groups lower in energy than those of Pu^{240} . They observed 4.90 ± 0.01 Mev (70% new relative abundance), $\sim 0.5\%$ abundance relative to Pu^{240} ; 4.85_6 Mev (23% new relative abundance), $\sim 0.14\%$ abundance relative to Pu^{240} . Asaro said pulse analysis gives 4.91 Mev for the alpha-particle energy of Pu^{241} and 4.88 Mev for the alpha-particle energy of Pu^{242} . Asaro went on to say that the expected abundance of Pu^{242}

alpha activity was calculated at about 0.1% of the Pu^{240} activity from the half-life and mass analysis. These two alpha groups may be due to Pu^{240} , Pu^{241} , or Pu^{242} . They plan to use a 0.25 mg sample in order to increase the intensities of these low abundance groups. In the discussion that followed, I said that, if the alpha-particle energy of Pu^{241} is 4.90 Mev and Pu^{242} is 4.88 Mev, this will give a stepwise effect in the alpha systematics plot of alpha energy vs A or (A-Z). I also said a second point is that, in calculating energy cycles, a current discrepancy in the Pu^{241} cycle would be resolved only with a greater alpha energy. This discrepancy, I stated, hinges upon a recent beta decay energy measurement for U^{237} of 0.52 Mev instead of an earlier value of 0.68 Mev.

Cunningham announced that Feay's previously reported observation of a single diffraction line in Cm^{242} is still persisting without the expected diminishing intensity that would result from Cm^{242} decay. They now doubt that even this line is from Cm^{242} .

Hyde described recent work by Momyer, who bombarded lead with ${}^6\text{C}^{12}$ ions to produce Ra^{213} , which represents the first radium isotope with fewer than 126 neutrons. Hyde said that Momyer was able to carry out the bombardment on campus, drive to the 184-inch cyclotron area, perform a chemical separation to take out the radium fraction, and still have 1000 alpha c/m for pulse analysis. The data fit the predicted properties satisfactorily with an alpha-particle energy of 6.56 Mev and a half-life of 2.7 minutes. Momyer also observed the daughter Em^{209} , whose quantitative growth and decay will be followed more closely in subsequent preparations. There was considerable discussion about work in this region.

I spoke extensively about spectral states in shell structure in the heavy region, saying that several sensitive tests exist for determining subshell effects. These include alpha activity considerations and spin measurements. I went on, in some detail, about work that will help obtain information on this subject, such as the spacing of the first excited levels of even-even nuclides as revealed by complex alpha decay that is being investigated by Asaro and by Dunlavy. I mentioned the subject of spin predictions as compared with spin measurements, the beta decay work by Browne, and the comparison between the rare earths and the actinides. The subject evoked some interesting discussion, including remarks by Friedlander.

* * * * *

Before going up to the hill, I stopped in the Department office.

Friday, September 12, 1952

Since I am going out of town tomorrow, I tried to clear my desk top of pending correspondence. Yesterday, at home, I received a letter from Gordon S. Watkins (Provost, Riverside Campus, UC), asking me to speak on "Science and the Atomic Age" at the Wednesday, March 25, 1953 meeting of the Present Day Club. Watkins described the club, which celebrated its Fiftieth Anniversary last year. He also reported confidentially that he has suggested to the University's Committee on Public Ceremonies that I

give the Charter Day Address at the alumni and faculty banquet in the Mission Inn on either Tuesday, March 24, or Thursday evening, March 26. This morning I answered that I have decided that I should accept his invitation or invitations, and I suggested that we discuss the general subject matter at the time of the fall UCLA Alumni Association banquet. I also pointed out that I shall be attending a national meeting of the American Chemical Society in Los Angeles during the week of March 15 and, should I be asked to give the Charter Day address, the date of March 24 would fit sufficiently close so that I probably would not need to return to Berkeley for the intervening time. I said, however, that I realize there may be no choice of dates.

Also yesterday I received a note from Warren H. Crowell (President, UCLA Alumni Association), saying that President Sproul came to the conclusion that October 18 would not be a suitable date for the banquet. He said that Sproul is now considering November 7 and November 14. In today's reply to Crowell, I said that I hope Drs. Sproul and UCLA Chancellor Raymond Allen will be able to make the November 7th date since, according to my present schedule, I can not come on November 14th. I also reminded Crowell that I shall be in the East all of next week and from October 20th to 23rd.

A note went to Professor W. George Parks (Director, Gordon Research Conferences) to thank him for his letter of August 29 about the 1952 Gordon Research Conference on Nuclear Chemistry. I wrote, "I personally feel that it was a very worthwhile conference and have found that many of my colleagues have the same impression."

I acknowledged Charles Coryell's letter of September 8 and said that we have sent copies of the draft of the "Table of Isotopes" to Brightsen and Pappas. With regard to the dinner meeting of the Advisory Board of the Gordon Research Conference on September 15, I explained that neither Kennedy nor I can attend because of a dinner meeting of the Executive Committee of the ACS Division of Physical and Inorganic Chemistry at the same time and that neither Perlman nor Friedlander are going to attend this ACS meeting. I told Coryell that if I see Dodson or someone else suitable, I may suggest that they attend the dinner meeting. I also explained that I haven't published anything on the analysis of the beta energetics of the heavy elements and the portion of my Ohio State addresses that was reproduced is somewhat along different lines. I enclosed a copy. I said that I received his Annual Reviews of Nuclear Science manuscript, found it interesting on one quick perusal but hope to read it more carefully.

In today's mail was a note from William H Sullivan, thanking me for the rough draft of the "Table of Isotopes." Sullivan said that he will be away at the time Dr. Hollander is planning to visit Oak Ridge but shall return on the afternoon of October 8. He added that if Hollander is there on the 9th, he will be glad to talk with him.

I visited some of the labs to check on the research before going home for the day.

Saturday, September 13, 1952

The lab driver picked me up at home at 7:45 a.m., drove me to San Francisco in time to catch the 9:30 a.m. United Flight 708 to Chicago. The flight arrived in Chicago at 6:15 p.m., and I transferred to United Flight 612, which left Chicago at 6:50 p.m., bound for Philadelphia, where I arrived at 10:30 p.m. A limousine took me to the Pennsylvania Station for Train 1041 (it left at 12:05 midnight).

Sunday, September 14, 1952

I arrived in Atlantic City at 1:35 a.m. and went by taxi to the Hotel Chelsea (Morris Ave. and Boardwalk), checked in, and retired.

The Chemical Corps Research Committee met at 1 p.m. in Roger Adams' room in the Hotel Traymore for a luncheon meeting, which I attended.

Later I registered for this 122nd National Meeting of the American Chemical Society in Convention Hall, where I met and talked with many old acquaintances. To some of the people I mentioned the revised "Table of Isotopes" and said that Hollander will be visiting labs and universities in October for the purpose of getting the latest, but unpublished, information.

Monday, September 15, 1952

In Atlantic City. At 9:30 a.m., in the Mandarin Room of the Hotel Traymore, I met with other members of the ACS Committee Advisory to the Chemical Corps. H. F. Johnstone, Chairman of this committee, called for reports of the various subcommittees: Research Subcommittee of which I am a member, Roger Adams (Chairman); Publicity Subcommittee, Sidney Kirkpatrick (Chairman); Industrial Relations Subcommittee, Dr. Ernest H. Volwiler (Chairman); Educational Subcommittee, Dr. Alphon H. Corwin (Chairman).

At noon I joined the chairmen of the various sections of the Division of Physical and Inorganic Chemistry for lunch.

I went to the meeting of the ACS Publications Committee in Room 17 of Convention Hall at 3:30 p.m., and then at 6 p.m. I chaired the dinner meeting of the Executive Committee of the Division of Physical and Inorganic Chemistry in Convention Hall. Present, in addition to me, were John C. Bailar, Milton Burton, Paul C. Cross, George Glockler, Joseph O. Hirschfelder, Frank A. Long, and Joseph W. Kennedy. Frank T. Gucker, Jr., the incoming Secretary, was present by invitation. We discussed the symposia for the Los Angeles (Spring, 1953) meeting--John Turkevich has agreed to chair the symposium on "Nucleation" and W. M. Latimer will chair the symposium on "Hydration of Ions in Solution". The Secretary will invite Anton Burg to be the chairman for "Recent Advances in the Chemistry and Technology of the Rarer Elements." Joe Katz' name was also proposed for this symposium. We also decided to have no more than eight sessions for contributed papers at the UCLA meeting, and names were suggested as potential session chairmen.

The 1953 Summer Symposium can be held at the Knolls Atomic Power

Laboratory in Schenectady the week of June 15, the Secretary reported. The topic is "Impurity Phenomena," but suggestions are wanted for a better name. We also agreed on four symposia for the Chicago (Fall, 1953) meeting: "Heterogeneous Catalysis," "Experimental Molecular Structure," "Inorganic Polymers," (joint symposium with Division of High Polymers), and "Influence of Neighboring Groups," (joint symposium with Division of Organic Chemistry). Again we discussed possible chairmen. Other matters discussed at this meeting were various changes in by-laws, etc. The Committee also decided to have a Division mixer instead of a dinner at future spring meetings.

Edgar C. Britton, President of the American Chemical Society, presided at the general meeting, which was held at 8:30 p.m. in the Ballroom of Convention Hall. Samuel C. Lind, winner of the Priestley Medal, spoke on "Chemistry at Mid-Century," and Britton's Presidential Address was entitled "Our Stake in Research." Afterward I went to the mixer in the main arena of Convention Hall and talked with many people. Robert A. Staniforth told me about some work on the neutron absorption cross section of Ac^{227} , which they found to be about 490 barns. Ratigan of the Journal of Chemical Education chatted with me for a while. Edwin M. Kinderman, who was at the Rad Lab during the war and is now with General Electric in Richland, Washington, talked with me about fission cross sections of some plutonium isotopes. A young fellow, Alfred Chetham-Strode, approached me about graduate work at Berkeley--he graduated from Rice Institute in Texas and is now working at Hanford.

[In Berkeley Bill Crane wrote today to L. R. Hafstad (Director of Reactor Development, AEC), requesting that the MTR Policy Board assign the Whitney Project a permanent hole in the Arco Pile and approve the use of the thermal column at Arco for fission counting measurements. Crane also wrote to W. B. (Brad) Lewis in Idaho Falls, noting his request to Hafstad. Crane said the position they requested permanently will have to have a flux of 10^{14} n/sec/cm² and be in such a position that samples can be changed easily when the pile is operating. He explained that secrecy limits the details that he can provide as to the experiments.]

Tuesday, September 16, 1952

In Atlantic City. This morning I went to Trimble Hall of the Hotel Claridge for the Symposium on Recent Advances in Chemistry: Nuclear Chemistry. This was sponsored by the Division of Chemical Education and chaired by Hubert N. Alyea. Samuel Glasstone spoke at 9 a.m. on "The Role of Chemistry in Nuclear Energy," and I talked at 9:30 a.m. on my familiar topic, "The Transuranium Elements," which I illustrated with 13 slides. Other speakers at this symposium were Melvin Calvin on "Isotope Studies on Photosynthesis," J. Laurence Kulp on "Dating with Carbon-14," and Paul C. Aebersold and Edwin A. Wiggin on "The Scope and Future of Isotope Utilization."

At 11:35 a.m. I was interviewed for 15 minutes by Bob Norton (ACS News Service) and G. B. Lal (Hearst Science Editor) on WMID (Atlantic City) for later radio broadcast. The topic was nuclear science, and we covered such points as isotopes, reactors, transuranium elements, etc.

In the afternoon I sat in on some of the papers given in the Division

of Physical and Inorganic Chemistry sessions. Then at 6 p.m. I went to the dinner meeting of the combined editorial boards of the ACS in the Club Room of the Hotel Traymore. During the evening I talked with a number of people, including W. A. Noyes, who gave me some disquieting statistics on the severe decline of bachelor's degree chemists and chemical engineers from 1950 to 1954. Up to now the number of chemists have been doubling every 15 years and the physicists, every 8 years. This is the sort of information that I believe is worth presenting in my talk before the Pacific Southwest Association of Chemistry Teachers in Los Angeles on September 27.

I also ran into Dieter Gruen who told me about his paper with Katz (which I missed hearing this morning) on neptunium(V) oxalate complexes in aqueous solution. He told me that he is publishing a paper describing some of his work on electronic structures of actinides in the Journal of Chemical Physics.

Wednesday, September 17, 1952

In Atlantic City. At 7:30 this morning I went to a breakfast of the ACS News Service in the Hotel Traymore, and then at 10:30 a.m. I went to the meeting of the Council of the ACS in the Rose Room on the Lobby Floor of Hotel Traymore.

After lunch I heard some of the papers in the general session of the Division of Physical and Inorganic Chemistry--Joe Kennedy presided at this session in Room 13 of Convention Hall. The first paper, "Radiochemical Studies on the Photofission of Thorium" by Dale M. Hiller and Don S. Martin, Jr. of Iowa State University, was very interesting; they chemically separated 13 fission products from thorium plus 69 Mev x-rays, found a factor of ten dip in the yield curve at $A = 115$. I particularly wanted to hear "Occurrence of the $4n + 1$ Series in Nature" by Donald F. Peppard, G. W. Mason, Peter R. Gray, and J. P. Mech (from Argonne Laboratory). At 3 p.m. I stopped in Room 21 to hear B. Bleaney speak on "Electronic Paramagnetic Resonance"; with uranium and plutonium salts he found no resonance in measurements down to 20°K.

The dinner meeting of the Division of Physical and Inorganic Chemistry was held in the main dining room of the Hotel Brighton at 6:30 p.m., and I presided. William Shockley presented a fascinating after-dinner talk on "Transistor Physics and Chemistry."

Later I had a nice chat with Bill Jenkins, who talked with me enthusiastically for some time about a girl he has recently met, Bitti Miller.

Thursday, September 18, 1952

I took a taxi to the station and left Atlantic City on Pennsylvania Railroad Train 1008 at 8:30 a.m.; this arrived in Philadelphia in time for me to catch Train 109 at 10:17 a.m., bound for Edgewood. The train arrived in Edgewood about noon, and I was met and had lunch with members of the Executive Committee of the Enlisted Specialists Chemical Engineering Club in Drago's Restaurant. After lunch I talked with Wilson Green and his top assistant about outlying research contracts before

giving a 3 p.m. talk on "The Transuranium Elements," again illustrated with slides, before the Enlisted Specialist Chemical Engineering Club in the Post Theater No. 1 of the Army Chemical Center. The lecture was attended by more than 500 enlisted men and was well received. Arrangements had been made for me to be driven to the train station to catch a train, via Baltimore, to Washington. In Washington I went by cab to the airport, caught American Flight 71 (which left at 9 p.m.) to Chicago. I arrived in Chicago at 10:40 p.m., was picked up by an Argonne driver, and taken to the Argonne Guest House, where I spent the night.

[In Berkeley, the research group met as usual with the following people attending: Clark, D. Conway, J. Conway, Cunningham, Dauben, Feay, Friedlander, Hollander, Hyde, Levy, McLaughlin, Nervik, Passell, Rasmussen, Ruben, Slater, Templeton, and Thompson.

Dauben gave a report on a study of various silicide systems, saying that one of Professor Brewer's students prepared silicides of tantalum, niobium, titanium, zirconium, tungsten, molybdenum, and cerium. She said that one of the purposes of the study was to find the relative stability of these compounds when heated with carbon and nitrogen. They found several interesting things: the niobium-silicon system has three phases isostructural with the tantalum-silicon phases; they found that niobium and tantalum have another phase isomorphous with hexagonal Ti_5Si_3 , which they identify as Nb_5Si_3 and Ta_5Si_3 . The lattice parameters of Nb_5Si_3 and Ta_5Si_3 have not yet been determined. They found that when molybdenum silicides were heated with carbon, the patterns gave evidence for $MoSi_2$ and a phase isostructural to Ti_5Si_3 , probably Mo_5Si_3 . Dauben mentioned that the samples are prepared by induction heating in a molybdenum crucible from 1500° to 1850°C.

John Conway talked about the work he did while he was in Chicago recently, in which they used the large spectrograph and observed an isotope shift for 27 lines in the spectrum of plutonium. He said the first plate was obtained by using a copper and a Pu^{239} sample with an iron arc for wave length reference. The second plate came from the use of the same Pu^{239} as well as 200 micrograms of a $Pu^{239,240}$ mixture (49% Pu^{240} from Stan Thompson). The third plate was obtained by using a 100 microgram $Pu^{238,242}$ mixture. There was considerable discussion about the work.

Dwight Conway said they used Pm^{147} and $Sr^{90}-Y^{90}$, mounted on a thin silver coated tygon film in a 4π counter, to determine the absolute disintegration rate. Conway described the experiment and presented the results, noting that the Pm^{147} run was not too good. Rasmussen said the purpose of the experiment was to calibrate the windowless 2π proportional methane counter ("Nucleometer") for absolute beta counting.

Passell said that he and Hyde have been studying the 21-minute half-life Fr^{223} in the region of the lower half of its beta spectrum. The work is still incomplete, but there may be two components in the Fermi plot of 1.10 ± 0.05 Mev and 0.76 ± 0.05 Mev. There will be more detailed information forthcoming on the gamma ray.

* * * * *

Friday, September 19, 1952

In Chicago. One of the first things I did on this consulting visit (\$75/day) at Argonne was to check on the situation with Sherman Fried. I learned that Hasterlik is the doctor insisting that Fried have the x-ray examination. (Fried feels that he took enough exposure to radiation while working in Berkeley last month that he doesn't want the additional amount from chest x-rays.) Later, when I talked with Fried, I suggested that Manning and I talk with Norman Hilberry and Wally Zinn to try to solve the problems involved. I also said I thought he should stay until he is fired (or nearly so); I said that, if he is fired, I shall try to find something for him, noting that it would be difficult to get a regular Rad Lab position but possibly something will be available with California Research or with the Whitney program.

I had conversations with a number of the Chicago men, including Glendenin and Steinberg, who asked that I send them the Frenkel reference. When I talked with Paul Fields, I learned that their sample of Pu²⁴² has twice the concentration that our sample has. I looked up George Barton, who said he will report to Berkeley 20 to 25 days after they sell their house here in Chicago.

An Argonne driver took me to the airport in time to catch a 3:45 p.m. flight to San Francisco. I arrived in San Francisco at about 9:30 p.m., was picked up by the lab driver, and got home about 11 p.m. Helen seemed happy to see me.

Saturday, September 20, 1952

Most of the morning was spent with the kids. In the afternoon Helen and I went in to Berkeley to watch Cal beat College of the Pacific by a score of 34 to 13. John Olszewski and Bill Mais are captains of the team this year. This again appears to be a good team, but whether it is good enough to go to the Rose Bowl again remains to be seen.

Sunday, September 21, 1952

This was a typical Sunday in Lafayette.

Monday, September 22, 1952

This morning I made some phone calls, handled some administrative matters, and then looked over some of the accumulated mail.

One relatively unimportant piece of mail was from H. H. Brown, Lieutenant of Police, Radiation Laboratory. Brown reported that regulations require that notice be sent to an employees' group leader if and when an employee has been issued two citations within any six months' period in order that the group leader has the opportunity to warn the employee that, if a third citation is issued to him within such six months' period, his parking privileges will be revoked. Brown wrote, "On 9/9/52 Marvin Kalkstein was issued a citation for a violation of the parking regulations. On 9/16/52 this employee was issued a second citation. The above named employee now stands in imminent danger of having his parking privilege suspended as provided for in the

regulations." I routed the notification to Earl for handling.

I also filled out a form to recommend Asher J. Margolis (a former member of my Metallurgical Laboratory group) for active membership in the Institute of Chemical Engineers.

Gordon S. Watkins (Provost, Riverside campus of the University of California) wrote on September 18 to say the Board of Directors of The Present Day Club and he are delighted I accepted their invitation to address the Club on the evening of March 24. He said he also has every reason to believe the local Committee on Charter Ceremonies will make the recommendation for me to be their Charter Day banquet speaker.

I also read a September 19 note from C. B. Marquand, saying that he has heard good reports about my visit to the Army Chemical Center and pointing out that he is sorry that he was unable to be present. Marquand asked that I write to Fraser Johnstone and indicate my feeling about the attendance of the members of the ACS Committee at the Los Angeles meeting of the American Chemical Society.

We had our usual senior staff meeting in my office at noon, and I reported on some of the things I learned during my trip.

Later I visited some of the labs to look over the work. I also consulted with Hollander and Perlman about the responses we have received from the rough draft copies of the "Table of Isotopes" that have been sent out.

Tuesday, September 23, 1952

After a few phone calls, I dictated several letters.

To Dr. Frank L. Lambert (Occidental College) I expressed my appreciation for his arranging to have me picked up at my parents' home on Saturday morning. I explained that I have had to change my plans and will leave on a 1:00 p.m. plane instead of a 2:00 p.m. plane; therefore, I shall be unable to accept his 12:30 luncheon invitation. I also wrote that, if it is convenient, I should like to show slides (regular size, 3 1/4" x 4"), but I can easily get by without if it is difficult to arrange.

I learned that Lincoln T. Work, National President of the American Institute of Chemists, will attend the Los Angeles meeting on September 25 and that there will be a reception for him before the dinner. This morning I wrote to T. F. Bewley to say that I shall be happy to attend the reception. I also asked if anyone would be coming to the meeting from the southeast area who could conveniently pick me up that afternoon at my parents' home in South Gate, explaining that, if this cannot be done conveniently, I shall arrange other transportation.

Yesterday I read a September 18th request from M. Haissinsky for a draft copy of the "Table of Isotopes," which he learned about from Mme. Joliot. He asked for several other reprints, and I had all of these sent to him today, along with the publications lists of members of our group. In response to another of his requests, I wrote that Dr. Perlman and I are very pleased to give him permission to use the figures in our alpha

systematics paper [Phys. Rev. 77, 26 (1950)] and the paper by Perlman and Ypsilantis [Phys. Rev. 79, 30 (1950)]. I routed the letter to Perlman and Hollander.

Iz gave the 11 a.m. introductory lecture to the Chemistry 123 class. The following students have signed up for the course: David L. Allred, John F. Below, Jr., Albert T. Bottini, Heber C. Brill, Roman I. Bystroff, Edward S. Clark, Nancy G. Colby, William P. Cox, John W. Crump, Clarence M. Davenport, Margaret J. Edson, Walton P. Ellis, Belmont S. Evans, Jr., Stanley D. Fair, Don J. Farquhar, Watson B. Fearing, Carl E. Fieber, Joseph J. Franaszek, Peter R. Gray, Edward A. Grens, James R. Grover, Joseph F. Gustaferrero, Rex Gyax, Loren G. Hepler, Terrance V. Hogan, Alfred Holston, Peter O. Jackson, Paul R. Kromann, John W. Kury, Richard M. Lessler, John R. McCarthy, Don S. McClelland, Ralph D. McLaughlin, Gilbert J. Mains, Hirdaya B. Mathur, Stanley A. Meyerhoff, Mary S. Nakata, Hawkins Ng, George Pávlov, Robert L. Pierce, Bruce A. Raby, Everett E. Roberts, Robert M. Rodden, George G. Sapp, Julian J. Schamus, Rex H. Shudde, Hugh C. Silcox, Stanley G. Smith, Warren G. Smith, Lawrence C. Snyder, Frank S. Stephens, Jr., Donald G. Stoffey, Joseph P. Surls, Jr., Mikio Suzuki, Krisna Tantranon, Robert D. Thompson, Ulrich Toggweiler, Vernon H. Troutner, William E. Wallace, Charles L. Weaver, Byron G. Weissberg, Robert A. Wessman, Lawrence W. Wolf, David D. Work, and Harold J. Zabsky. A number of people also signed up to audit the course: Elton M. Baker, Joan E. Baker, Edwin D. Becker, Jr., Robert D. Chaffe, Hildred L. Jensen, Emily B. King, Dwight H. Kouns, Walter J. Laird, Almon E. Larsh, Robert J. McCarter, Patricia W. Maguire, James L. Olsen, Raymond Robinson, Helena W. Ruben, and Mark D. Snyder.

After lunch I continued working with my correspondence. Verne A. Stadtman (California Alumni Association) asked on September 17 for a 5" x 7" dull-finish photograph of me, representative of my field of endeavor, for a display in the new "Alumni House" of identically framed photographs of the nine distinguished Californians who have been chosen as "Alumnus of the Year." I sent Stadtman a photograph we found in our files and asked what he thought of it, noting that we have the negative. I pointed out that I am not sure just what we would do in a new photograph to better represent my field.

I declined an invitation, dated September 8, from the German Bundestag (Herr Hallstein, Der Staatssekretär des Auswärtigen AMTS, Bonn, Germany) to join a group of leading Americans on a work-study tour of the German Federal Republic and West Berlin. The trip for the group would have been from November 10 to December 10, 1952. I wrote, "I certainly wish that I could take advantage of such a fine opportunity to visit your country and to get acquainted first hand with more of your countrymen and conditions in new Germany. I am sorry to have to say that I cannot accept your invitation since I have previous commitments for the time you mention and unfortunately already have so many engagements involving travel that I find it difficult to carry on my duties at the University of California. I believe wholeheartedly in such exchange programs and wish you every success in your endeavors."

In my role as Chairman of the Division of Physical and Inorganic Chemistry of the ACS, I wrote to Paul H. Emmett (Mellon Institute, Pittsburgh) to tell him that I discussed with Long and Kennedy his idea

that the tryout of a Division mixer should be postponed until the fall meeting of the ACS in Chicago. Their reaction, I wrote, was that they wouldn't mind trying out this idea at a smaller meeting (such as the 1953 spring meeting in Los Angeles) and that the benefit to the local young people on the Coast would be sufficient so far as this aspect is concerned. I added that, if any changes in plans were to be made, it would fall in the power of next year's officers (Long, Kennedy, and Gucker) to make such a change, but I believe they will want to give the idea a try in Los Angeles.

A letter also went to W. George Parks (Director, Gordon Research Conferences) to explain that I learned from Dr. J. W. Kennedy that there is possibly some misunderstanding as to the date for next summer's Gordon Research Conference in Nuclear Chemistry. I said the choice was for the same week as this year, namely the week of June 22 rather than the week of June 29 as suggested at the meeting of the Advisory Board. I said that this should offer not difficulty since Dr. Milton Burton has graciously offered to exchange his week on radiation chemistry scheduled at Atlantic City for June 22 with our week on nuclear chemistry. I also told Parks that Coryell had asked that either I or Dr. Kennedy attend the Advisory Board meeting, but we both had to attend a meeting of the Executive Committee of the Division of Physical and Inorganic Chemistry, which took place at the same time.

Wednesday, September 24, 1952

After taking care of some routine administrative chores, such as requesting a consultantship without fee for Herbert R. Johnston from W. B. Reynolds, I spent the morning looking over photostats of the galley proof of PPR 14A, Chapters 1, 2, and 3. Later I returned them to Joe Katz, noting that my corrections and suggestions are in red. I pointed out that there were no figures with Hagemann's Chapter 2 (actinium), but since his chapter is so good, I presume there is no need to check these at my end. Another comment I made was that, since Katzin's absorption cross section for ionium (in Chapter 3) was so garbled in the copy that went through clearance, I wonder whether it can be included without its being cleared.

I read a September 22 letter from Michael Kasha, who worked on the Manhattan Project in Berkeley during the war and got his Ph.D. here. Kasha, who is now a professor at Florida State University where Ray Sheline also has a position, asked if I could possibly arrange a detour through Tallahassee on some trip east during the year in order to speak to their Sigma Xi group.

Later I talked with some of the fellows and checked with some of the students about their research.

After dinner, at 7:15 p.m., a lab driver picked me up at home and drove me to the San Francisco airport, where I caught United Flight 678 at 9 p.m. I arrived in Los Angeles at 10:40 p.m. and went directly to my parents' home in South Gate.

Thursday, September 25, 1952

In South Gate. My parents were delighted to have me spend the day with them, to hear about the kids' progress, and to learn about my activities and recent honors.

At 4 p.m. I attended a reception at the University Club (614 South Hope Street) in honor of Dr. Lincoln T. Work, National President of the American Institute of Chemists. T. F. Bewley (Chairman of the Los Angeles Chapter) opened the meeting at 8 p.m. (the dinner began at 6:30 p.m.). There were several brief talks, in addition to 10 minutes of remarks by Work on "Streamlined Set of Objectives and Aims of the AIC." Professor William Crowell, for whom I served as a reader and teaching associate during my student days as UCLA, spoke for a few minutes on my life then, and Leo F. Pierce gave the usual biographical information; then, after a standing ovation, Dr. Work presented me with an honorary life membership award. I accepted the award and then spoke for about 40 minutes on "Nuclear Energy for Industry." I began by stating that it is fortunately a characteristic of the nuclear chain reaction operating on fissionable material that tremendous amounts of energy can be released in a slow controlled fashion as well as instantaneously as is the case for the nuclear weapons and then went on to cover topics such as construction of reactors, fuel elements, coolants, temperature, radiation, control, shielding, chemical processing, and waste disposal. I spoke about the AEC programs, various reactors, the role of industry, and the cost. I concluded by saying:

It should perhaps be possible within the next decade to develop and build a power plant which produces useful energy on a scale of some hundreds of thousands of kilowatts. Although continued development to the stature of large industry will be technically feasible, its actual development will be subject to political and economic considerations. Should it prove feasible to go ahead, as seems possible from the standpoint of the technical problems alone, it does not seem possible to build up a nuclear energy industry of such proportions that any appreciable fraction of the world's energy production is produced in this manner before several decades.

Although some estimates as to the cost of producing nuclear power have been made, it does not seem possible to make any very sensible statements on this point of view at the present time. Important to this matter is the question of ore supply and whether or not breeding will be successful. It is not out of the question that the nuclear source will eventually compete economically with coal as a source of energy, but it is also quite possible that this will never be the case except for localities where the price of coal is very high due to transportation difficulties.

Even should the nuclear source never provide cheaper energy than the present common sources, it may still have an important future because of its advantages as a compact and almost inexhaustible source of energy, characteristics which give it advantages which no other fuel can come close to equaling, advantages which cannot be evaluated in terms of dollar value because there is no alternate way of accomplishing the same things at any price.

[In Berkeley the research group met as usual with the following people attending: Carr, Conway, Cunningham, Dauben, Dunlavey, Feay, Friedlander, Gunn, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Richard Marshall Lessler (an Air Force cadet, who has an A.B. in mathematics from UCLA and who will do graduate work with us),

Richard Lessler



Levy, Michel, Momyer, Nervik, Passell, Perlman, Bruce Alan Raby (an Army man who has been assigned to graduate work at Berkeley), Rasmussen, Reynolds, Ruben, Shenk, Slater, and Templeton.

Friedlander reported on an experiment that he ran with Ghiorso and Perlman to look for the 0.8-second isomer of Pb^{207} in the decay of Po^{211} . They used At^{211} as a source. Friedlander went over the background of the problem--previous studies of the alpha spectrum of Po^{211} indicated a difference in energy of 1.09 Mev between the highest and lowest energy groups observed, and they wished to determine whether the 1.09 Mev state reached in the Po^{211} decay could be identified with the 0.8-second Pb^{207m} . He described the setup and the experiment and said that, from the data, an upper limit of 10^{-9} could be set for the fraction of alpha-particle decays going to an isomeric state with half-life of the order of 1 second. The abundance of the alpha group in Po^{211} decaying to the 1.09 Mev state is about 10^{-3} . The result of the experiment indicates that the isomeric state is not at approximately 1.1 Mev above ground. Friedlander also pointed out that the 0.8-second activity has never been definitely assigned to Pb^{207} .

Gunn talked about the experiments they have planned to determine the americium(III)-americium(VI) potential. They intend to measure the heat of solution of crystalline sodium americium dioxytriacetate in 1.00 M $HClO_4$ with and without Fe^{++} present.

Hyde went over their investigation of the alpha branching in samples of Fr^{223} (as large as 10^8 disintegrations per minute) obtained from the 1% alpha branching in the decay of Ac^{227} . Hyde explained that the estimated alpha branching of Fr^{223} is between 10^{-4} and 10^{-6} and that half-life predictions for the daughter At^{219} are difficult to make as it is an odd Z nuclide. Hyde said that, from the estimated alpha and beta decay energies, both the alpha and beta half-lives are expected to be of the order of minutes. The At^{219} alpha particle energy is expected to be 6.4 ± 0.1 Mev. Hyde went on to say that, due to the uncertainty in these predictions, it appeared at the outset that At^{219} could be predominantly either a beta or an alpha emitter and could conceivably have a half-life too short to permit chemical separation and counting. He went over the pulse analysis data they obtained on a sample of

astatine separated from Fr^{223} ; this indicated that Po^{215} was in equilibrium with a beta-emitting Bi^{215} of 8-minute half-life. No At^{219} was present when the sample was pulse analyzed, but the presence of the Bi^{215} - Po^{215} activities indicated its existence in the extract at the time of separation. An additional experiment with a shorter chemical separation showed an alpha peak that they assigned to At^{219} , which decays with about a 0.5-minute half-life and an energy of 6.3 ± 0.1 Mev, and an alpha peak due to Em^{219} . Since no Ra^{223} was present and the Em^{219} peak decayed with a short half-life, it appears that the Em^{219} results from the beta decay of At^{219} . The results represent the discovery of the existence of astatine in nature as part of the U^{235} chain.

Jaffe reported negative results in his attempt to find low energy gamma radiation that could be assigned to Pu^{241} . He used a sample that contained 17% Pu^{241} by weight and 10^{10} beta c/m, which he obtained from Stan Thompson, in a run with the bent crystal x-ray spectrometer.

* * * * *

Friday, September 26, 1952

In South Gate. Although I spent most of the day with my parents, I got together in the late afternoon with my high school friend, Clayton Sheldon, and went out to Rio Hondo Golf Club for nine holes of golf (CES-54, GTS-46). On each of an additional two holes we both scored 6.

[In Berkeley Earl prepared for my signature a reply to William A. Popp (obviously a high school student). Popp was interested in finding a supplier for a number of chemicals.]

Saturday, September 27, 1952

In South Gate. Mrs. Sheila Bauer picked me up at my parents' home at 9:45 a.m. and drove me to Occidental College, where I spoke in the Alumni Chapel of Johnson Hall at 10:30 a.m. on "Some Notes on the Transuranium Elements" to the Southern California Section of the Pacific Southwest Association of Chemistry Teachers. I began with a rather historical talk about the transuranium elements, but then I talked about the current low college enrollment in chemistry. (Some of this information was based on my talk with Noyes at the Atlantic City meeting.) The audience was quite small but enthusiastic, and I was later able to chat with my old UCLA friend, L. Reed Brantley.

I was driven to the Los Angeles airport in time to catch United Flight 651 at 1 p.m. My flight arrived in San Francisco at 2:40 p.m., I was picked up by a lab driver and driven to Lafayette.

Unfortunately, I missed the California-Missouri game in Memorial Stadium, but Cal won by a score of 28 to 14.

Sunday, September 28, 1952

This was an ordinary Sunday for the Seaborgs.

Monday, September 29, 1952

I had the usual phone calls and conversations this morning. I also met the secretary whom Doran hired as a replacement for Virginia Hempel, Mrs. Louise Kalm. She is attractive and seems bright--her husband Max is a graduate student on campus. Louise will work in Bldg. 5 when her Q clearance comes through.

Louise Kalm



Eric Mendelsohn, architect of our new chemistry building, invited Helen and me to his home (2423 Leavenworth St., San Francisco) for dinner on October 17 (the invitation came to our home). This morning I dictated an acceptance.

In today's mail was a thank-you note from Paul A. F. Mourier-Peterson (Secretary-Treasurer, Enlisted Specialists Chemical Engineering Club) for my talk there on September 18.

George Boyd wrote on September 16 to say that they will be happy to have Hollander visit on October 7 and 8, although he will be abroad. He stated that Ralph Brosi and Bruce Ketelle will be on hand. Boyd also said that, if he is able, he will write to me from The Netherlands about the "Table of Isotopes." As with all the mail about the "Table of Isotopes," I routed the letter to Perlman and Hollander.

R. L. Doan (Atomic Energy Division, Phillips Petroleum Company, Idaho Falls) wrote on September 26 to request another executed copy of agreement PPCo-AED-7, covering the experimental work sponsored by the University of California at the Materials Testing Reactor (MTR) at the National Reactor Testing Station (NRTS).

I also read a September 24 letter from Bob Folger asking about reprints from his paper with Rudstam and Stevenson ["Nuclear Reactions of Iron with 340-Mev Protons" by G. Rudstam, P. C. Stevenson, and R. L. Folger, Phys. Rev. 87, 358 (1952)]. He asked about the "aluminum monitor" paper and said that Perlman can tell me about his work in Oak Ridge.

Later in the afternoon and after our lunchtime senior staff meeting, Luis Alvarez, Al Ghiorso, and Ellis Steinberg (our Argonne visitor) joined me at Mira Vista for nine holes of golf (LWA-48, AG-45, GTS-46, ES-50). We went on to play an additional three holes.

Tuesday, September 30, 1952

I spent most of the morning on campus--talking to colleagues in the Department, keeping office hours, and giving the Chemistry 123 lecture (today I began lecturing on fundamental nuclear concepts).

One matter that is taking up a great deal of my time is Project Whitney, its problems and the staffing of the chemistry group, including finding a top notch person to head the group.

Wednesday, October 1, 1952

This morning some of the senior staff and I, including Iz Perlman and Ken Street, had a rather lengthy conference in my office about some of the problems with which the Project Whitney chemistry group is involved. Of immediate concern is the preparation of a polyethylene-uranium hydride sphere and the preparation of a polystyrene foam-uranium hydride hemisphere. During the discussion Amos Newton remarked that this sounds like an industrial chemistry problem and further suggested that I contact someone at Dow Chemical Co. I telephoned Dow in Pittsburg, California, and I talked with Wilhelm Hirschkind, the Director of Research there. When I explained that this was an urgent matter of national defense and that I need to talk with someone with a Q clearance, Hirschkind said that I should talk with their main laboratory in Midland, Michigan. He mentioned a couple of names--Dr. R. H. Bowndy (Director of Research at Midland) or as an alternate, a Dr. Veazey. Hirschkind said someone will call me at about 10:30 a.m. tomorrow with the proper name and telephone number.

Another matter discussed in the meeting about Whitney was the need for a top-flight young nuclear chemist to help supervise the project. The consensus was for Bob (Robert B.) Duffield at the University of Illinois. [Bob obtained his Ph.D. with Melvin Calvin and me at the beginning of World War II.] In my phone conversation with Duffield, he pointed out that he is teaching radiochemistry this semester (the semester ends about February 1). Duffield also mentioned that Roger Adams (Head of the Department of Chemistry) was opposed to his taking leave last year to work on Project Crave. So I then telephoned Roger Adams, whom I know well. Adams was not enthusiastic about Duffield taking another leave, explaining that it would create a hardship in the Department because Duffield is teaching a course in radiochemistry; however, he said he will discuss it with Duffield next week. Later I wrote to Duffield, again telling him about the problem and suggesting that he visit here at our expense to discuss the nature of the project. I also pointed out that we have in mind a leave of absence for a couple of semesters and are willing to wait until the next semester (February) if this would make it possible for him to come.

Later I suggested to Amos Newton that he work with the Whitney group for a couple of months, assuming that he can get permission from Eastman Kodak, his employer.

In today's mail was a letter from Wayne Meinke, who thanked us for allowing a couple of his students to look around the hill, particularly at counting equipment. Wayne also thanked me for help in continuing the no-pay consultancy, which came through a few days after my letter. He said he will send comments on the "Table of Isotopes" along in a week or so. Finally, he brought me up to date on life in Ann Arbor, including football, his courses, and his students.

Thursday, October 2, 1952

The "open" meeting of the research group this morning was attended by Asaro, Carniglia, Clark, Cunningham, Dauben, Dunlavey, Feay, Friedlander, Hoff, Hollander, Hulet, Hyde, Jaffe, Kofstad, Lessler, Levy, McLaughlin,

Michel, Nervik, Passell, Perlman, Raby (we have decided to have Raby work with Earl Hyde), Rasmussen, Ruben, Seaborg, Stephens, Templeton, and Thompson.

Dunlavey said that he had impregnated a photographic plate with At^{211} from Hoff in order to study electron-alpha particle coincidences. He observed no electron coincidences after counting 2000 alpha tracks and then stopped because Asaro had done a more thorough job with similar results. Dunlavey also looked at a sample of Pu^{234} from Hoff (approximately 1-5% Pu^{234} by activity with contaminants of U^{233} and Pu^{239}). Dunlavey pointed out that the percentage of Pu^{234} decreased during the time needed to complete an exposure because of its short half-life, but he was able to distinguish about 200 alpha tracks due to Pu^{234} . He found that about 14% had electrons in coincidence with them; the electrons indicated a gamma energy of 45 to 50 keV. He also found lower energy electrons from Cf^{246} . During the discussion I mentioned that I would have expected the energy of the gamma ray from Cf^{246} to be at least as high as 50 keV. Dunlavey also pointed out that all these results are temporary and will be improved with the use of better samples.

Asaro reported on an attempt to find a method of determining spin differences in the heavy nuclei by the investigation of the empirical ratio of the M and L conversion coefficients for radiations of various multipole orders. He pointed out that the energy difference between the M and L shells is about 14 keV, so there should be no difficulty in resolving the M from the L lines. Asaro went into some detail about the method.

Asaro then went on to report on his run on a sample of $\text{Pu}^{239-242}$ that he obtained from Stan Thompson and Gary Higgins. He showed a plot of the alpha ray spectra and said that the peak at the lowest energy is due either to Pu^{240} , Pu^{241} , or Pu^{242} . The very tall peaks at the high energy end are due to Pu^{240} while the peak that is approximately 120 keV above the main Pu^{241} peak can be due to either Pu^{240} or Pu^{241} but not Pu^{239} . It has an abundance of about 0.1% relative to Pu^{240} . Asaro went on to say that the other smaller peak with about 40 keV more energy may be a real peak or just a statistical variation. He added that the people concerned with closed cycles would not like to add 120 keV to the Pu^{241} ground state transition. I said this isn't strictly true and that we have been forced to add about 90 keV to the ground state transition already because of the Argonne U^{237} beta disintegration energy. There was considerable discussion, and Asaro said he would like to show eventually that even-even nuclei in this Z region do have a third alpha group separated by about 150 keV. Asaro also discussed the peak of about 6.00 MeV, first assigned tentatively to Cm^{243} but later decided possibly to belong to Cm^{242} . He presented a reinterpreted decay scheme, based on work of M. S. Freedman, A. H. Jaffey, and F. Wagner, Jr. of Argonne and of John W. Mihelich. I then went over in some detail the reason for raising the alpha disintegration energy of Pu^{241} by 90 keV when we calculated the masses of the translead nuclides.

Hyde again talked about his work with Ghiorso on the alpha branching of AcK (Fr^{223}), which they determined to be 5×10^{-5} . The value may be low because of losses during the chemical separation.

Passell said his previously reported beta particle energies from Ack are in error. The high energy particle has the energy 1.38 to 1.79 Mev and the lower energy component about 1 Mev. Hyde said they have not determined that the high energy beta particles follow the 21-minute half-life.

* * * * *

Iz gave the Chemistry 123 lecture, and I went up to the lab where I received a call from Dow in Pittsburg, giving me the phone number for R. H. Bowndy in Midland. I then called Bowndy and described the uranium hydride mixing problem. Bowndy said he will probably be able to send us a Q-cleared man with whom we can discuss the problem. He will call me tomorrow.

At noon I joined a group having lunch in the cafeteria with Sir John Cockcroft. About 1:30 p.m. I drove Cockcroft from the hill cafeteria to Bldg. 5, where we had a short discussion in my office. He began by telling me that the chemists at Harwell are getting well underway with transuranium chemistry, that they have successfully reduced 500-gram lots of plutonium to metal, and they plan to go into the study of americium chemistry in the near future. I said that we have followed the work of D. West and others at Harwell on gamma-ray determinations in Pu^{239} and Am^{241} through scintillation counting and that their work compliments our own alpha spectroscopy work. Cockcroft asked whether we are making progress in the search for isotopes of elements 99 and 100, and I said that probably some of the work could be termed progress although it is difficult. For example, I said, we feel that these elements will be chemically similar to the rare earths holmium and erbium and so we have a lead on the chemical procedures needed.

I added that Ghiorso and members of Hamilton's 60-inch cyclotron crew are making a modest effort to investigate the conditions for accelerating heavy ions such as carbon, but the isotopes most easily prepared by bombardment with heavy ions are so neutron deficient and thus short-lived, that chemical identification is difficult; however, we are making efforts to build up heavier isotopes as starting material. We briefly discussed the systematics of spontaneous fission, which I and W. J. Whitehouse and W. Galbraith of Harwell have been studying simultaneously, and I told him that I am trying to deduce an expression for activation energy from the spontaneous fission data that could be applied to the slow neutron fission process. We talked about the use of the Chalk River pile for neutron bombardments, and I mentioned that we have just received back a sample of americium that has had about a 2 1/2 years' neutron bombardment in the Chalk River pile although we have not calculated the neutron cross sections in detail as yet. I expressed the opinion that some of our neutron absorption cross sections that differ from those obtained at Chalk River might be accounted for by the difference in energy distribution of the epithermal neutrons in the bombarding spectrum.

After I gave Cockcroft a copy of my Ohio State lecture, "Nuclear Thermodynamics of the Heaviest Elements" (March 1952), and a preprint of our "Table of Isotopes" (UCRL-1928), I took him to Room 111, where Al Ghiorso showed him the new pulse analyzer based on the 6BN6 vacuum tube.

Ghiorso described the principle of the instrument, pointed out its advantages, and gave him a circuit diagram. In Room 102 Stan Thompson talked about our investigation of the heavy isotopes of the transuranium elements and said that the isotopes formed in the 2 1/2 years' Chalk River bombardment of americium are proving very useful as target isotopes for bombardment in the 60-inch cyclotron and should lead to new isotopes of berkelium and californium. I said that we are beginning a study of the chemical properties of these elements.

Nelson Garden showed Cockcroft the cave room (Room 106) and the new setup using uranium metal in the ball joints for remote control operation. We discussed the advantages of the Berkeley method of working in boxes as opposed to the method of working with large hoods and air flow through the building. Finally, in Room 105 Burris Cunningham showed Cockcroft his microbalance and attendant apparatus for measuring the magnetic susceptibility of microgram amounts of the actinide elements, such as curium. We discussed the disadvantages of working with an isotope as highly radioactive as Cm²⁴².

In Building 4 Fred Reynolds and Frank Asaro presented the alpha-ray spectrometer in Room 107, along with some sample data on the Am²⁴¹ alpha spectrum obtained with it. In Room 108 Reynolds also showed Cockcroft the mass spectrograph, the Siegbahn-type beta-ray spectrometer, and the lens-type beta-ray spectrometer under construction. In Room 109 Cockcroft was shown the Cauchois x-ray spectrometer; Herman Robinson and Maynard Michel showed him the time-of-flight mass spectrometer in Room 103; and we looked into Room 103A at the scintillation counting apparatus.

At about 2:45 p.m., after Earl Hyde (Room 110) described his recent experiments in which he found At²¹⁹ as the alpha-branching daughter of Fr²²³, and therefore natural astatine, and his results on the daughter of At²¹⁹ (Bi²¹⁵). Cockcroft then continued his tour of other areas in the company of Dr. Cooksey.

Friday, October 3, 1952

Among my phone calls this morning was one from R. H. Bowndy (Dow Chemical, Midland, Michigan), who said that he has arranged for Alden W. Hanson to visit next Wednesday and Thursday to discuss the Project Whitney problems. I passed this information on to Street and Newton.

I also learned that the meeting in Washington scheduled for the October 21 through October 23 has been cancelled, so I wrote a note to this effect to Donald Lane and said that I shall let him know when my next trip is scheduled so that we can get together.

Much of the rest of the day was spent looking over reports and journals and talking some of the fellows about the research.

Saturday, October 4, 1952

I worked around the yard for a while and played with the kids. The California-Minnesota football game was played in Minneapolis today; California won by a score of 49 to 13.

Sunday, October 5, 1952

The Seborgs gave their annual cocktail party for members of our group in our newly decorated home. As usual Stan acted as bartender.



Iz Perlman and Stan Thompson as bartenders (in kitchen) October 5, 1952



Carol Dauben, ?, Jack Hollander; Marv Kalkstein and Rex Shudde in background (Seborg patio), October 5, 1952



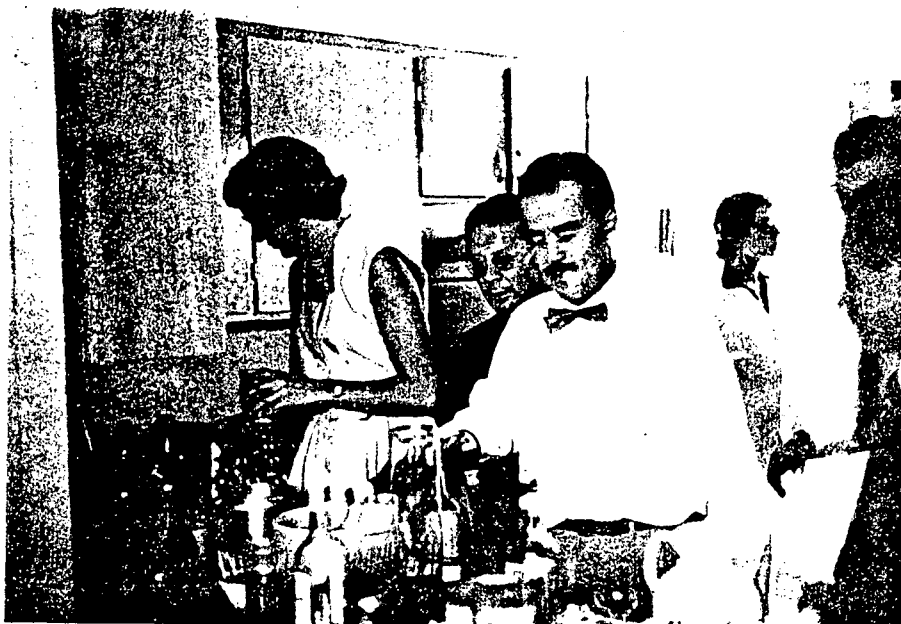
Mildred Davis, Helena Ruben, and Lilo Templeton, October 5, 1952



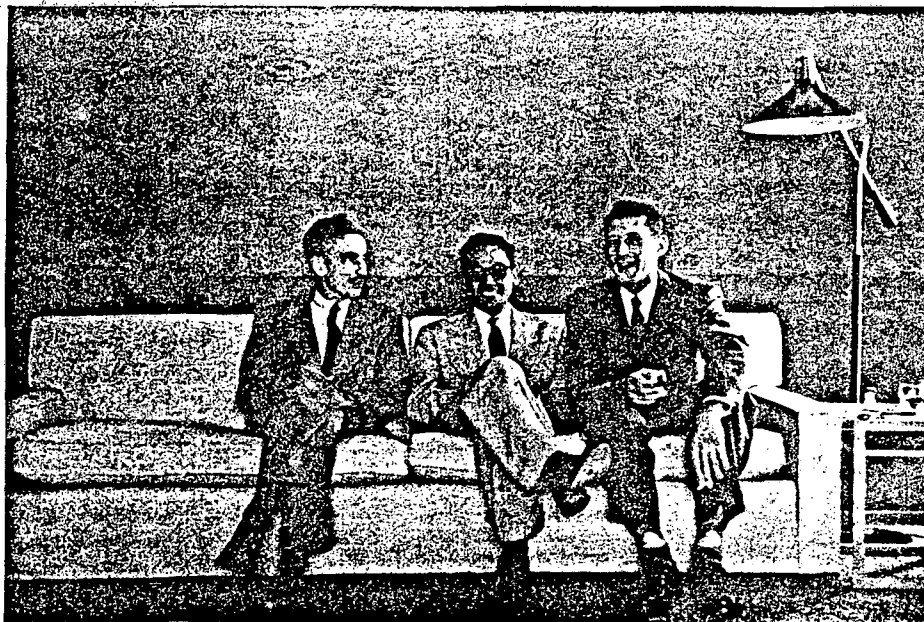
Dorothy Abrams and daughter, October 5, 1952



Margaret Rees, Walter Nervik, Dave O'Kelley (back), October 5, 1952



Hildred Jensen, Jim McKennon (Docia's husband), Stan Thompson, Iz Perlman (far right), October 5, 1952



Bill Rice (architect), H. Mathur, Ralph McLaughlin (living room), October 5, 1952



Maynard Michel, Helen Seaborg holding Stephen with Lynne, October 5, 1952

Monday, October 6, 1952

I took care of some administrative matters and phone calls before looking over the lab work.

The senior staff had its usual brown-bag luncheon meeting in my office. Since this is football season, we discussed the weekend games, in addition to scientific matters.

A note arrived in today's mail from Verne A. Stadtman (Managing Editor, California Monthly), who said he had not seen a better picture of me than the one I sent him (this is for a display of Alumni of the Year in the new Alumni House). Stadtman asked that I lend him the negative, and so I instructed Doral to procure it from the Information Division.

In the first part of September Helen and I had dinner at the home of Amos and Elizabeth Newton (833 Ramona Ave., Albany), along with Professor and Mrs. Kasimir Fajans. During the course of the evening Fajans and I talked about our mutual interest in the history of radioactivity. Today I received from him a list of several historical books in which I am interested that are available from Walter J. Johnson, Inc., 125 East 23rd Street, New York 10, New York. Johnson is a used book dealer and, according to Fajans, was formerly connected with the famous "Gustav Fock Antiquariat" in Leipzig. Fajans offered to have the firm send me a catalogue.

Tuesday, October 7, 1952

I sent the negative of the photograph that Verne Stadtman requested down to him today, saying that I would appreciate his returning it to me for transmittal back to our Information Division.

In the mail today was a nice letter for Helen and me from Maurice D'hont [183, Lierse Steemweg, Herentals, Belgium], who visited Berkeley in August and has now returned to Belgium. D'hont thanked us for arranging his stay in Berkeley and included some snapshots (rather poor) taken while he was here (including one of his young daughters, Myriam and Lieveke). He then discussed briefly their program and his hope that we visit Belgium. D'hont then asked that we extend his greetings to Dr. Perlman and the Hydes.

After giving the Chemistry 123 lecture, again on fundamental nuclear concepts, I stopped in the Department office and talked with a number of my colleagues.

Later Al Ghiorso and Luis Alvarez went with me out to Mira Vista for 18 holes of golf (AG-99, LWA-103, GTS-98).

Wednesday, October 8, 1952

Much of my day was spent in conferences. Alden W. Hanson, the consultant from Dow Chemical in Midland, Michigan, arrived. I met with him and some of the members of the Whitney chemistry program, including Ken Street and Amos Newton, for a discussion about the preparation of both the polyethylene-uranium hydride sphere and the polystyrene

foam-uranium hydride hemisphere for their project. Hanson seemed capable; eventually, when the discussion about the project was well underway, I returned to my own work.

Newton told me this morning that he had informed John A. Leermakers of Eastman Kodak (Newton's employer) that I have asked him to help with Whitney for a couple of months. Today I wrote to Cyril J. Staud (Director of Research, Eastman Kodak), saying that Newton's cooperation is needed on this high priority project because of his experience in certain fields that will expedite the work and because it is impossible to get new men and have them cleared in time to meet the deadline for completion of the project. I wrote, "You can be assured that we have not lightly asked Dr. Newton to relegate his own work in radiation chemistry to a subordinate role during this time, but we have done so only because of the urgency of the project we have undertaken. We will certainly appreciate all the cooperation he can give us."

We are also continuing to look for personnel for Project Whitney--Doral has been interviewing for a capable, unencumbered (with family) secretary, who will be willing to work in Livermore eventually.

In the mail today was an October 6 letter from Donald Lane. Lane informed me that the Patent Compensation Board has scheduled a further prehearing conference on Docket No. 7 for November 5, and he (Lane) hopes that we can determine at this conference the status of the Regents, if any, in this proceeding. Lane said he hopes to have the Board press General Counsel for a position on validity of Contract eng-30 and for analysis of inventions involved. Lane reported no formal response to the proposed stipulation we filed with the AEC on January 21, 1952, and doubts that the General Counsel has completed the patent claim analysis, which he has indicated orally from time to time that his office is preparing. Lane suggested that possibly the Board can place some time limit on General Counsel when informed that Applicants are not receiving cooperation. Finally, Lane asked me to advise him of the present status of each of the applicants with respect to AEC committee work (a possible source of difficulty with the Patent Compensation Board) and of any other points I suggest for discussion.

Thursday, October 9, 1952

The following attended the research group meeting this morning: Asaro, Carniglia, Carr, Clark, Cunningham, Dauben, Feay, Glass, Hoff, Huffman, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Rasmussen, Ruben, Seaborg, and Templeton.

Carniglia spoke for some time about the separation of neptunium from a Hanford solution, containing about 30 grams of plutonium, 2 grams of neptunium, and impurities of lanthanum, potassium, chromium, iron, and lesser cations in a total volume of about 25 liters. Crane's California Research and Development group processed about 2/3 of this and obtained 1.4 grams of neptunium. Cunningham's group is following Crane's recommendations about processing the remaining 1/3 of the solution. Carniglia described the changes they have made in Crane's flow sheet and the plans they have for further purification procedures for the neptunium (Crane reported 99.5% pure neptunium after his final purification).

Cunningham said Crane's purity is not sufficient for their needs; he also said this neptunium will be split about 1/3 for Crane's group, 1/3 for Thompson's group, and 1/3 for their own use and represents several years' supply for their presently envisioned chemical research program. Carniglia also described the experimental setup and equipment, designed and engineered by Health Chemistry, for a clean, safe operation.

Perlman announced that we expect to receive soon a sizeable amount of U^{232} , produced from pile neutron bombardment of ionium (Th^{230}), and that it will be interesting to check the energy levels of the daughter Th^{228} with those seen from the beta decay of M_sTh_2 .

Hoff talked about his work with Hyde on At^{210} and At^{211} and their daughters. At^{211} is produced by 28-29 Mev helium ions on bismuth and a mixture of $At^{210,211}$ is produced by 36 Mev helium ions. Hoff went on to say they have seen gamma rays from the decay of Bi^{207} , with the scintillation counter, of 540 and 880 kev and a 550 kev gamma ray following alpha decay of Po^{211} to an excited level of Pb^{207} . Perlman noted that there are numerous gamma rays in the Bi^{207} decay, and Hoff said the 540 and 880 kev gamma rays were the most obvious. Perlman added that the 880 kev level of Pb^{207} has not been detected through conversion electrons of Bi^{207} ; however, the level was first seen in the alpha decay of Po^{211} . In the decay of At^{210} by electron capture to Po^{210} , Hoff said they saw gamma rays of 250 kev, 1.15 Mev, and 1.40 Mev. He presented a level scheme for Po^{210} , but they have not yet done coincidence and angular studies. Hoff said they put 10^8 alpha c/m of mixed $At^{210,211}$ in the alpha-ray spectrograph and found two groups of low abundance at 5.516 and 4.434 Mev although further sweeping may turn up additional ones. They feel these are due to At^{210} , but the question will be resolved when they look at a plate of At^{211} .

Levy said that their alpha/beta branching of 270:1 of Bi^{210m} has been confirmed by recent experiments; the half-life for the beta process is $\sim 2.7 \times 10^8$ years since the alpha half-life is $\sim 10^6$ years. He said the spin is still highly tentative: the spin of RaE is likely either 2 or 0.

Templeton remarked that a similar problem exists at Rb^{82} , that Castner found 25-day Sr^{82} and assigned it by excitation function data. Karraker had assigned 6-hour Rb^{82} with the mass spectrograph, but Castner was unable to find it as a daughter of his Sr^{82} . Templeton said that Sr^{82} emits 3 Mev positrons, which were interpreted as due to a short-lived Rb^{82} daughter activity and milking experiments set an upper limit of a few minutes for its half-life. He also stated that Lawrence M. Litz produced more Sr^{82} and found only a 1.25-minute daughter.

* * * * *

I gave the final lecture on fundamental nuclear concepts to the Chemistry 123 class, stopped in the Department office, and then went up to the hill for conferences, etc.

Friday, October 10, 1952

Again I spent much of the day in conferences, primarily about Project

Whitney. Newton agreed to writing up for me the conclusions of the visit of Alden W. Hanson.

Donald F. Martin, a young physicist who has been working with our electronics group, terminated today.

Saturday, October 11, 1952

I spent the day with my family. On my recent trip to Los Angeles (August), I convinced my parents that they should visit us in Lafayette and see for themselves how the children are developing. This is working out well--the kids and their grandparents are happy; my mother has agreed to make Halloween costumes (I hope they will stay until after Halloween); and my father is helping with my yard work.

California played Oregon in Eugene and won by a score of 41 to 7.

Sunday, October 12, 1952

I again stayed home with my parents, Helen, and the kids. I did some reading and then went over my paper called "Activation Energy for Fission."

Monday, October 13, 1952

I worked for a while on promotional letters for Burris Cunningham (to full professor) and David Templeton (to associate professor).

Again, at the lunchtime senior staff meeting, we went over in detail the weekend football games.

Tuesday, October 14, 1952

After a few phone calls, etc. this morning, I signed and mailed two promotional letters to Dean K. S. Pitzer. One was for the promotion of Burris B. Cunningham to the rank of Professor of Chemistry. I covered his historical work at the Metallurgical Laboratory on the transuranium elements and his postwar research here at Berkeley, including important and significant work in the field of the transuranium elements and investigations of the homologous elements in the rare earth group. I said that he and his students have 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. I described other research, told about his extraordinary facility to devise, build, and use special apparatus, mentioned his personality and discussed his teaching ability. I concluded by saying that, "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 without reservation. I believe that the University of California is very fortunate to have a man of his accomplishments and ability and I feel every effort should be made to reward him adequately."

The second letter was to support the proposed promotion of David H. Templeton to the rank of Associate Professor, and I said that I

understand that such a promotion at this time would correspond to a rate of advancement faster than normal but feel that this is thoroughly justified and called for in this case. I described Templeton's research in the field of nuclear chemistry and said, in addition to this work, he is carrying on a program of investigation in an entirely different field; namely, x-ray diffraction measurements as applied to crystal structure determinations. Finally, I wrote, "I can recommend Dr. Templeton without qualification and feel that he more than deserves to be advanced to the associate professorship at this time."

I wrote a note in response to Professor K. Fajans' letter of October 4, saying that I am going to try to order some of the books that he called to my attention.

I also answered a recent letter from Bob Folger. I said that we are sending him six reprints of "Nuclear Reactions of Iron with 340-Mev Protons" by Rudstam, Stevenson, and Folger for his file. I suggested that he send additional requests for reprints to Margot Carlson here for filling. In addition, I told Bob that Pete Stevenson will write him about the "aluminum monitor" paper. Finally, I wrote that I am glad to hear that he is finding the work at Savannah River interesting and enjoyable and that I have heard a number of good reports about his performance.

On September 30 Bill (W. H.) Johnston wrote me about a mix-up with the mail and the talks he wanted me to make at Purdue University--to the Chemistry Colloquim and the local Sigma Xi chapter (I apparently did not receive a letter he wrote on July 28). Today I wrote and said that I don't think any harm was done since it now appears that I couldn't have made the visit this fall anyway. I went on, "It is now clear that you people will want two talks and I will see whether I can fit them into my travelling schedule this spring. I am sorry that I haven't been able to do better about this but my schedule seems to have taken a turn for the worse rather than lessening a bit as I expected."

A note went to L. W. Douglas (The Academy of Political Science, Columbia University), in which I said that I should like to thank him for his kind invitation which he extended to me in his letter of September 16 to become a member of the Academy of Political Science. I wrote, "I feel, however, that I must decline your kind invitation. I hold memberships in a number of organizations connected with my work and related subjects, and I do not feel that I should add to the list."

Iz gave the lecture to the Chemistry 123 class on spontaneous nuclear reactions.

I took my usual tour through the labs to look over the research and talk with the students.

The Svedberg wrote from the University of Uppsala to thank me for the draft copies of the "Table of Isotopes." He said they have nothing to suggest so far and asked if I wanted the copies returned or if they could keep them, adding that they are very useful to have.

Later in the afternoon Dan Wilkes (Office of Public Information on

campus) and Al joined me at Mira Vista for a round of golf (DW-81, AG-87, GTS-96).

Wednesday, October 15, 1952

After numerous phone calls, I wrote both to Winston Manning (Argonne) and John Willard (University of Wisconsin) to ask that they write supporting letters for Burris Cunningham's promotion to the full professorship. I explained that, as they know, outside letters are of great help and that I believe the letters are due about November 1. In both cases I sent a copy of the letter that I wrote but explained that they needn't write such a long letter.

I received an October 13 letter from Kasimir Fajans about the draft of the "Table of Isotopes." Fajans had several suggestions, both for this and for future editions; I asked Doral to have the letter copied for Hollander. He mentioned that he is going to Oak Ridge for a lecture trip and, after his return, he will send me some old reprints of his with comments. He also enclosed some correspondence with A. V. Grosse and with the Editor of Rubber Handbook about the name for element 91.

A response arrived in the afternoon mail from Robert B. Duffield (University of Illinois) to my offer to him to help head the chemistry group of Project Whitney. Duffield said that he talked with Roger Adams (Head of the Department of Chemistry), who takes a dim view of this particularly since he (Duffield) was away just a year ago. Duffield wrote that he thinks strong and definite arguments would have to be presented to him (Adams) before he would approve the idea. He commented that his own reaction is somewhat less than enthusiastic because he is interested in the work that he has going on there and should not like to leave it. He feels that he has put in some time on defense work and that such jobs should rotate among the available people. Duffield added that the Physics Department would have to be consulted and, although they would agree, they would not be happy about it. Finally, he said that, if I still feel that I would like to talk it over in some detail, he would be happy to come to Berkeley for a few days after November 1.

In light of Duffield's letter and in view of the urgency of the matter, I made a number of calls and set up a conference for tomorrow morning to discuss the problem of heading the chemistry group of Project Whitney. Since several of our men will be involved, I told Doral to cancel the regular Thursday morning meeting tomorrow.

Thursday, October 16, 1952

In this morning's conference on Project Whitney, one of the names suggested to head the chemistry group, at least on an interim basis, was Ken Street, now with California Research and Development. With regard to staffing the group, Iz and I feel that our former Ph.D. graduate, Charles I. Browne, would be an asset to the group. Browne, a bright fellow and a member of the Air Force, is now rather firmly entrenched at Los Alamos; however, it might be possible, in view of the urgency of this project, to have Browne transferred. Later Iz and I got on the phone and called him and learned he would love to return to this area. Bill Crane also talked with him about the setup.

Iz gave the Chemistry 123 lecture, again talking about spontaneous nuclear reactions.

Later I had a short conference with T. Evans Wycoff (West Coast Office, McGraw-Hill) and Dr. Handsfield of the New York office. Wycoff suggested I call him if I ever want a McGraw-Hill text.

Friday, October 17, 1952

I again spent time in conferences, particularly about staffing the chemistry group of Project Whitney.

Iz and I have had several conversations about his secretary, Mildred Davis, who has proved to be highly efficient, accurate, and productive. Today a request for a promotion (Secretary 11.5 to Administrative Assistant 11.29) and a merit raise (\$317/month to \$342/month) was sent to the Salary and Wage Committee.

In today's mail was an October 15 letter from Alden H. Emery (Executive Secretary, ACS). He wrote, "We have learned with regret that you no longer are Chairman of the Division of Physical and Inorganic Chemistry. It is only fair that burdens and responsibilities be shared, but it is a real loss to the Society to have you relinquish the post which you have filled so well. Please accept my personal thanks for the services you have rendered to the American Chemical Society...."

I also received a letter from R. Salat, who expressed regret that I am unable to accept the invitation of the German Federal Government to take part in a study tour this November. Salat said he hopes that I will be able to participate in next year's repeated program.

Helen and I had dinner this evening at the home of Eric Mendelsohn (2423 Leavenworth St., San Francisco). Mendelsohn is the head of the architectural firm designing our new chemistry building.

Saturday, October 18, 1952

Helen and I went in to Berkeley to Memorial Stadium for the football game. The Golden Bears defeated Santa Clara by a score of 27 to 7.

Sunday, October 19, 1952

I played with the kids, worked in the yard, and read, etc.

Monday, October 20, 1952

Hollander has returned from his trip to collect new information, comments, and criticism on the draft of the "Table of Isotopes," and Iz and I spent some time going over the information.

The senior staff held its usual brown-bag luncheon meeting in my office at noon. Again, we quarterbacked the weekend football games; the upcoming presidential election also received some attention.

Tuesday, October 21, 1952

I had a multitude of phone calls and conversations while Iz gave the Chemistry 123 lecture, continuing on the subject of spontaneous nuclear reactions.

In the afternoon Luis Alvarez, Stan, Al, and I went to Mira Vista (LWA-95, SGT-92, AG-93, GTS-91). Al and I won the low ball, low total match, 2 up.

Wednesday, October 22, 1952

After innumerable phone calls, conversations, and conferences, I sent a memorandum today to George Everson, our personnel manager, stating that Dr. Kenneth Street, Jr. and Dr. W. W. T. Crane are coming from California Research and Development Company to join the Radiation Laboratory in connection with the Project Whitney chemical research. Dr. Street, I said, will head the chemistry group that is contemplated to number some forty people by next summer and perhaps sixty people by within the following year. Street and Crane will start as of November 1. I told Everson that Dr. Street's present salary is \$737 per month with a 10% raise coming up and suggested that a starting salary of \$830 would be commensurate with his new responsibilities. I suggested that Crane start at \$650 in his new capacity where he will have additional responsibilities.

About noon I telephoned Alden Hanson in Midland, Michigan, to tell him that he should put the polystyrene foaming project on a slower time schedule than the other project (polyethylene). Hanson gave me the names of other Dow Chemical employees with a Q clearance--W. E. Donaldson, W. C. Bauman, and M. E. Putnam. He also said he is willing to come to Berkeley at the time of the mixing, if he is wanted. Hanson also questioned me if any other company, such as Union Carbide, is doing anything on this project. I told him that I thought not.

Iz and I received a letter from Charlie Browne. Browne said that, as far as his personal wishes are concerned, this would be an excellent prospect. Charlie wrote, "At any rate, my impression is that it would take quite a bit of trouble to obtain my release since it must be done in such a way that General Mills is not caught in the middle. In the unlikely event that you want to go to this much wire-pulling, I am of the opinion that the following is necessary; first, clearance of my release from Los Alamos with some high administrative office of this laboratory--probably with Bradbury's office, and second, a call or letter, or both, to General Mills from as high a level as possible in the UCRL structure. The military is generally unaware of the existence of chemists and chemistry, so that it would be most effective if the letter could be signed by Teller himself, if this is possible, or at Lawrence's level. If any organizational designations exist in the group under discussion, it might be worthwhile mentioning the level at which I would be used. Crane mentioned that I would be at his (Crane's) level, directly under Ken Street, which might interest the military." It is obvious that Browne would like to return to this area but such a change would involve a great deal of effort.

In the late afternoon I received a phone call from James Wikle Cobble, our new postdoctoral man from Oak Ridge. Cobble, who had just arrived in town with his wife (Margaret) and infant daughter (Catherine), had checked into the Durant Hotel. I suggested that he attend our Thursday morning meeting tomorrow and meet some of the fellows, check in at Personnel, and then tackle the housing problem before reporting for work.

James Cobble



Thursday, October 23, 1952

The research group meeting this morning was attended by Asaro, Biller, Cobble, Cunningham, Dauben, Feay, Glass, Gray, Higgins, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Michel, Nervik, Perlman, Rasmussen, Ruben, Seaborg, Slater, Surls, Templeton, and Thompson.

Gray described to the group the work of Peppard and co-workers at Argonne in which they found that members of both the actinides and the lanthanides can be extracted into tributyl phosphate from either hydrochloric acid or nitric acid. He presented a plot of the logarithm of the ratio of the concentrations in the solvent and acid phase against the atomic number of the element; this gives a series of straight lines. Gray mentioned that the values they obtained for berkelium and californium were much higher than were expected from the behavior of the other actinides, and I commented that it is possible that these elements might not be anomalous but rather the value for actinium might represent the anomaly. Hyde added that, since a fair number of other elements may be extracted with tributyl phosphate, he supposes the main attraction of this solvent behavior is the separation of the rare earths from each other rather than in separating the rare earths from other elements.

Thompson talked about his work with Higgins to determine carefully the positions of americium, curium, berkelium, and californium on a Dowex Al column (it is known that berkelium and californium are held more strongly than are americium and curium). He presented the results from elution with 13 M HCl. With 10 M HCl all the elements washed out immediately. Thompson said that the ratios of the activities of the two HCl solutions is not much different from the ratio of their concentrations. His high dependence on chloride ion concentration suggests ions of the form CfCl_4^- or CfCl_5^- . At any rate, he said, they feel that there is something special about this change. Thompson also talked about their attempts to make the (V) oxidation state of californium employing americium as a carrier for californium and converting Am(III) to $\text{KAmO}_3(\text{s})$, using a strong carbonate solution with

hypochlorite. He went over the details of the experiment; their conclusion was that californium is not converted to a +5 state under these conditions. There was a discussion about other methods, and I added that under these conditions one can at least conclude that californium is not so easily oxidized as americium.

Hulet presented information they have obtained from the 30 mg sample of americium that was neutron-irradiated in the Chalk River pile for about two years. About 80% of the original americium was converted into fission products. The curium-americium fraction was separated from the plutonium isotopes and fission products. There was then an unsuccessful, due to an inadequate beam intensity, attempt to bombard the Cm-Am fraction in the 60-inch cyclotron. Glendenin and Steinberg used the samples for fission studies.

Now the curium and americium have been repurified and bombarded with 35 Mev helium ions. Berkelium and californium were separated by ion exchange methods, and in some cases the separation factor between berkelium and californium and the target material was greater than 10^{14} . They made measurements on a number of the berkelium and californium products. Hulet said that my predicted spontaneous fission half-life (log of the half-life vs Z^2/A) for Cf^{246} was 2,000 years; their results yielded $2,100 \pm 200$ years. They were able to prove, by decay measurements, that the fissions counted were from the 35-hour Cf^{246} . They also may have observed the alpha particles of Cm^{245} , the electron capture daughter of Bk^{245} , for the first time. They allowed a pure sample of Bk^{245} to decay for several days, separated a curium fraction, and observed no Cm^{242} . An alpha pulse analysis indicated 0.7 c/m of a 5.52 Mev alpha particle, which could be due to Cm^{245} or Am^{241} . Thompson said they intend to pursue the investigations by using the technique of alpha-electron coincidences in photographic emulsions. Hulet also noted that a pulse analysis of the californium fraction, after Cf^{246} decayed, indicated the presence of a 6.3 Mev alpha particle, which may be due to Cm^{248} or Cf^{247} (the latter is, however, expected to decay predominantly by electron capture).

Feay described their rough measurement of ΔH for the reaction $CeF_4 = CeF_3 + 1/2 F_2$; the measured value was found to be 15 kcal, in fair agreement with the estimated value.

* * * * *

Iz continued talking about spontaneous nuclear reactions, etc. to our Chemistry 123 class, and I stopped in the Department office and then went up to the hill. There I dictated a few letters.

I wrote to Bob Duffield and said I feel, with reluctance, that his letter of October 13 offers such obstacles that we are not justified in pursuing further the matter of his coming here. I wrote, "I do, however, want to reiterate that our project is one of great urgency and importance and that we did not lightly decide to contact you, but did so only after a very careful consideration of the people in the country who we thought might be suitable and possibly available. In fact, we haven't yet decided on a second choice and are going to try to carry on for a while by increasing each of our own work loads in order to cover the position

temporarily in this manner...."

I acknowledged the September 29 note from Hubert N. Alyea (Chairman, Division of Chemical Education, ACS) in which he thanked me for my participation in their symposium on September 16 and told him that my schedule is such that I can't undertake the publication of my talk in the Journal of Chemical Education, which Norris Rakestraw (Editor) desires.

A note also went to Donald Lane. I thanked him for letting me know about the prehearing conference and said that I shall look forward to the results with interest. I also said that, so far as I know, I no longer have any commitments with the AEC that might possibly interfere and that I have no other suggestions to make beyond those I have mentioned in my previous letters.

Friday, October 24, 1952

I had the usual phone calls and conversations and then I wrote a few letters.

Donald A. McPherson (formerly with John Wiley) wrote on October 8 to congratulate me belatedly on the Nobel Prize and to tell me about his new position with Row, Peterson Publishing. I thanked him for his congratulations and said that I learned at the last national meeting of the American Chemical Society from one of his John Wiley associates that he had gone on to different fields.

To The Svedberg I wrote that he may keep his draft copies of the "Table of Isotopes," but I pointed out that the draft will be very thoroughly corrected and revised on the basis of the suggestions that we have received. I added that we will sent him reprints of the final publication.

A longer letter went to Kasimir Fajans, and I said we shall add explanatory phrases to our introductory material for the "Table of Isotopes" in order to clear up the points he made. I said that I was interested to learn more of the background in connection with the naming of element 91 since the method applied to essentially all of the other radioactive elements did not occur in this case.

I also wrote to F. A. Paneth, saying that I delayed in answering his letter of August 13 to try to foresee my schedule so that I would know whether I could take the trip to Europe next summer. With regard to my remaining on the Joint Commission, I said I shall be glad to serve although I may not be able to attend the meeting in Stockholm next year. I pointed out that I have not written to Dr. Sizoo due to the uncertainty of the composition of the Joint Commission and the additional uncertainty of my own plans.

Also in connection with the trip to Stockholm, I wrote to Lars Melander that I am not certain but it seems unlikely that I shall be able to attend the Congress next summer in spite of the fact that I love visits to Sweden. I said, in addition, that I am a little concerned that the program doesn't actually touch very closely on my own interests so that I find some difficulty in justifying such a long trip on the basis

of the program.

Jim Cobble and I discussed his future research for a while, and then I made the rounds of the labs to check on the work.

Saturday, October 25, 1952

I worked around the yard and then listened on the radio to the California-USC football game, which was played down South today. Unfortunately, Southern California won by a score of 10 to 0.

Sunday, October 26, 1952

After a morning of yard work, fatherly duties, etc., I went with Marv Kalkstein and Jack Hollander to Kezar Stadium, where the 49ers ran up a 48 to 21 victory against the Dallas Texans. The game featured a 77-yard touchdown run by Joe Perry, an 82-yard run by Hugh McElhenney, and a touchdown pass by Frankie Albert and by Y. A. Tittle. We stopped for dinner at Grison's Steak House on Van Ness Avenue after the game.

Monday, October 27, 1952

Iz and I prepared a midterm for the Chemistry 123 class, which will be given on Thursday.

Then the mail brought me a photostated galley of PPR, Volume 14A, Chapter 4 ("The Chemistry of Thorium" by L. I. Katzin), and I spent some time looking it over. I also received the galley for my Chapter 13, "Nuclear Properties of the Transplutonium Nuclides."

I also received a nice note from Gert Friedlander, who is now getting resettled at Brookhaven. Gert said he enjoyed his stay in Berkeley and is grateful for the opportunity to learn firsthand about the researches being carried on by our group and to exchange ideas with us. I routed the letter to Iz, Al, Stan, et al.

Amos Newton, accompanied by Don Bruce of our business office, has gone to Dow Chemical in Michigan to look over their plans for ethylene production for Project Whitney. Bruce will handle the business aspects, but Newton will look over the operational details.

We had our usual lunchtime senior staff meeting in my office and quarterbacked the weekend football games.

Later I wandered through the labs to check on the research. Stan Thompson and Dick Hoff are working in a couple of Berkeley boxes (Box 206 and Box 170) in order to purify some of our americium. Box 170 now contains about 121 mg of americium.

Tuesday, October 28, 1952

In yesterday's mail was a letter from G. Curtis Pritchard (Secretary, Board of Directors of City Trusts, City of Philadelphia), informing me that on October 17 the Board of Directors of City Trusts selected me as recipient of the John Scott Award (with a premium of \$1,000). This

morning I wrote that I consider this as a great honor. I sent Pritchard the biographical material he requested, a photograph, told him that the two degrees to be engraved on the Medal should be A.B. (University of California at Los Angeles, 1934) and Ph.D. (University of California, Berkeley, 1937). I also wrote that I shall look forward to hearing about the arrangements for the presentation of the Award, noting that although the date is presumably fixed by other considerations, I shall be glad to let him know the dates of my planned trips to the East if he wishes.

I returned the galleys to Chapter 4 of Volume 14A to Joe Katz, noting that I have not taken the time to go over it very carefully, but I presume he will. I pointed out a couple of points that Katz should especially check, including some recent European references since I think some of the Europeans are sensitive to omissions in authoritative American literature. I told Katz I shall send back Chapter 13 as soon as I have a chance to look at it.

Stan's americium purification project is continuing in Box 170; he has taken three fractions off the column.

In the afternoon Al and I went out to Mira Vista with Luis Alvarez for 18 holes of golf (LWA-100, AG-90, GTS-85).

Wednesday, October 29, 1952

A couple of days ago I received a letter from Robert F. Gould (Managing Editor of Chemical and Engineering News) asking me to prepare a review of the book, Modern Radiochemical Practice by G. B. Cook and J. F. Duncan. I talked with Earl about it and today wrote to Gould, saying that I feel I am too busy at present to undertake such a review. I suggested that Earl Hyde of this laboratory, who is an authority in radiochemistry, would be a good man for the purpose.

A letter also went to Joe Katz, in which I asked if he had seen the new report LA-1439 by Eugene Staritzky and Donald I. Walker on the "Optical Properties of Some Compounds of Uranium, Plutonium, and Related Elements," and questioned if we should and can make any changes in Chapter 19 of PPR, Volume 14A ("Optical Properties of Some Compounds of Uranium, Plutonium, and Related Elements" by E. Staritzky and A. L. Truitt). I added that there is some information in this report that I believe will be of interest to Dieter Gruen.

I responded to Friedlander's recent note, saying that we want to repeat that we are very grateful for his visit and feel it was extremely profitable for us. I wrote, "We feel that you helped us in numerous ways and that no other consultant in the country could have done as much."

I also wrote to Lloyd Van Doren (Secretary, The American Institute of Chemists) and said that I am glad to accept membership on the Advisory Group on Policies to the Membership Committee of the American Institute of Chemists as he suggested in his letter of October 21, 1952. I added the reservation that my present schedule is so heavy that I shall be unable to spend much time on this activity.

Iz and I have had some serious conversations about Charlie Browne's

response to our offer; today Iz wrote him and said that we want to talk with Rod Spence first. Iz wrote that, unless his contemplated house change involves a long term commitment, we would suggest that he go ahead with it since it might be to our advantage to have him stay where he is through next spring. Iz added that Charlie should not interpret this letter as any faltering in interest on our part, but we do feel that he should contact Spence before going further.

I looked in on the americium purification project. The group is now working in three boxes in Room 107: Stan in Box 206; Stan, Higgins, and Hulet in Box 193; and Dick Hoff in Box 170.

Later I worked on the galley proof to my Chapter 13 "Nuclear Properties of the Transplutonium Nuclides" of Volume 14A of the Plutonium Project Record.

Thursday, October 30, 1952

The following people attended the research group meeting this morning: Asaro, Carniglia, Carr, Cunningham, Dauben, Dunlavey, Feay, Glass, Gunn, James Robb Grover (a new graduate student working with me from the University of Washington),

James Grover



Hoff, Hollander, Hyde, Kalkstein, Levy, Michel, Passell, Perlman, Raby, Rasmussen, Ruben, Seaborg, Slater, Templeton, and Thompson.

Hoff spoke extensively on calculations and experimentation on the electron capture isotopes among the heavy elements, saying that he has applied the Fermi theory of beta decay to the electron capture activities in the heavy elements. He is now setting up a proportional counter to observe L x-rays, and he said he has some data on Pu^{234} that might be expected to have an allowed transition. U^{233} was bombarded with helium ions, the plutonium was separated from the target material, neptunium and fission products by an oxidation-reduction cycle, and the sample was cleaned up on an anion column. He pointed out that Pu^{234} has a 9-hour half-life and the scintillation spectrometer showed only K and L x-rays; this indicates decay to the ground state of Np^{234} . The Np^{234} was observed to grow in, and in weak samples only the 442 keV gamma ray was seen. Using the scintillation spectrometer, he redetermined the alpha/electron capture ratio of Pu^{234} (first determined as 1/25 by Higgins using the counting efficiency of the Nucleometer) to be 1/50. He said, if this is correct, then the counting efficiency used for the Nucleometer was too high. Hoff added that Dr. Friedlander suggested a way to

determine the counting efficiency of the Nucleometer. He pointed out they did not have quite enough daughter Np^{234} activity to do accurate work on the scintillation spectrometer. He said the ft value of Np^{234} indicates it is 2nd forbidden. The work evoked much discussion.

Levy then spoke for some time on the shell effects in alpha systematics. He suggested an explanation of why the minimum in the alpha energy vs neutron (N) plot comes at $N = 125$ rather than at $N = 126$.

* * * *

The Chemistry 123 class was given its first midterm today:

Chemistry 123

Midterm Examination
October 30, 1952

1. (25)

- (20) (a) From masses given in Friedlander and Kennedy calculate the binding energies of the last neutron in O^{16} and in O^{17} .
- (5) (b) Explain the difference briefly in terms of rules affecting nuclear stability in the light element region.

2. (20)

The packing fraction of a nuclide of mass number 240 is about $+6.0 \times 10^{-4}$ while for mass number 120 the packing fraction is about -5.0×10^{-4} . Calculate the energy released in the fission of the heavy nuclide. Give your answer in Mev.

3. (15)

Predict the mode or modes of decay for the following whether observed or not and explain briefly your reasoning:

- (5) (a) ${}_{47}\text{Ag}^{108}$
(5) (b) ${}_{62}\text{Sm}^{146}$
(5) (c) ${}_{72}\text{Hf}^{183}$

4. (25)

Explain the following:

- (5) (a) The origin of K x-rays of tellurium in the decay of I^{125} .
- (5) (b) Energetic photons from C^{11} decay.
- (10) (c) An electron line from an isotope of gold is found with a beta ray spectrograph to have an energy of 200 kev. If a thick piece of beryllium followed by a thin layer of lead is placed on the sample, an electron line of 193 kev is seen.
- (5) (d) Both nuclide A and B have β^- transitions of 1 Mev. Nuclide A has a half-life of 1 hour and nuclide B has a half-life of 1 year.

5. (15)

We have spoken of isobaric pairs of the type: X protons, X-1 neutrons -- X-1 protons, X neutrons as mirror nuclides and reasons were given why the one of higher atomic number is a positron emitter. In the case of H^3 - H^3 it is found that H^3 is slightly heavier decaying by β^- emission to He^3 . Explain why this apparent reversal has taken place.

After stopping in the Department office and speaking with some of my colleagues, I went up to the hill.

In today's mail was a nice letter from Miss Lucille L. Bennett (Chairman, Graduation Committee, David Starr Jordan High School, Los Angeles): "We are making plans for the graduation program for the Winter 1953 class of David Starr Jordan High School. The faculty and class would like to have you deliver the address. The program will be Thursday, January 29, 1953. We hope you find it convenient to be with us." Since this is my high school, I find it an exceedingly flattering invitation.

I made my usual rounds of the labs and found the purification of the americium is still going on in Room 107. In addition, Peter Gray is beginning to work on actinium in a box in that room.

In the last few days I have faced a rather serious decision. Clark Kerr, the new Chancellor of the Berkeley campus, has asked me to accept the post of Faculty Athletic Representative to the Pacific Coast Conference to replace Stanley B. Freeborn, who has been named Provost on the Davis campus. (Kerr had learned that I have a keen interest in athletics.) My logic in dealing with the decision is that I already attend many of the intercollegiate events and this new position will give an official status to my attendance without increasing too much the time I already devote to the activity. So I called Kerr today and told him that I have decided to accept the appointment.

Friday, October 31, 1952

After hearing about my appointment as Faculty Athletic Representative from Chancellor Kerr, Brutus Hamilton, Director of Athletics, telephoned me this morning. He seemed pleased and suggested we get together for a press conference to discuss the appointment.

One of the projects that I have been working on for some time is my paper, "Activation Energy for Fission". I have had discussions with a number of people about it, including John Rasmussen, Jack Hollander, David R. Inglis (at Argonne), and Gert Friedlander. Today I finally sent the manuscript to S. A. Goudsmit (Editor, The Physical Review), saying that I am submitting it for publication as a "Letter to the Editor." I told Goudsmit that I prefer to have it published as a "Letter," but I am including an abstract in case he feels it is too long for publication in this manner. In this paper I use the experimentally determined experimental dependence of spontaneous fission rate on Z^2/A to derive an expression for the dependence of the fission activation energy on Z^2/A . I calculate the activation energy for slow neutron induced fission and correlate these calculated values with the slow neutron fissionability for a large number of nuclides.

I looked over a document prepared by Amos Newton about his visit to Dow Chemical Company. Amos pointed out that Dow requested the visit in order to go over their method for deuterated ethylene production for duPont and the areas that may give difficulty. The document was most complete, and I routed it to Iz and Earl for comments.

Recently Doral had our room and phone number list revised; this includes Project Whitney people, guests, and so on:

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg.</u>
Abrams, Dorothy	396	102	4
	378	339	50
Asaro, Frank	397	107	4
Baker, Joan E.	466	107	4
Balkwell, William R.	393	203B	4
Barton, George W.	397	107	4
Batzel, Roger E.	319	350A	50
Biller, William F.	380	346	50
Buchholz, Doral	246	114	5
Carlson, Margot M.	246	114	5
	380	346A	50
Carniglia, Stephen C.	349	105	5
Carr, Robert J.	250	201B	4
Case, Marjory	397	107	4
Clark, Edward S.	7-8159	318	Lewis
Cobble, James W.	250	201B	4
Coleman, George H.	319	350A	50
Conway, John G.	374	316	50
Cook, Marshall W.	467	203E	4
Crane, William W. T.	467	203E	4
Cunningham, Burris B.	349	105	5
Dauben, Carol H.	7-8159	318	Lewis
Davis, Mildred J.	246	114	5
DiGrazia, Herbert X.	470	355	50
Dunlavey, Dean C.	378	341	50
Feay, Darrell C.	349	105	5
Fick, J. Leonard	466	203E	4
Gallagher, M. Fran	393	203B	4
Garrett, Roberta	376	332	50
Gede, Victor	470	355	50
Ghiorso, Albert	358	111	5
Gilbert, Richard S.	393	203F	4
Glass, Richard A.	250	201B	4
Goda, Lilly	395	103	4
Goeckermann, Robert H.	319	352	50
Gray, Peter R.	260	104	5
Grover, James R.	378	343	50
Gunn, Stuart R.	250	202	4
Hanson, Donald N.	7-473	120	Gilman
	467	203E	4
Hartzell, Alfred J.	395	103C	4
Heppler, Winifred	349	105	5
	250	202	4
Hicks, Harry G.	393	203F	4
Higgins, Gary H.	349	104	5
	466	203C	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

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg.</u>
Hulet, E. Kenneth.	260	104	5
Hunt, Charles d'A.	7-472	221	Gilman
	466	203E	4
Hutchin, William H.	393	203F	4
Hyde, Earl K.	345	110	4
Iddings, Glen M.	466	203C	4
Jaffe, Harold	397	109	4
Jensen, Hildred	397	109	4
Jost, John	467	203E	4
Kalkstein, Marvin I.	358	108	5
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
Larsh, Almon E.	358	111	5
Latell, Frances	466	102	4
Lessler, Richard M.	250	201B	4
Levy, Harris B.	7-8109	21	Lewis
Lindner, Manfred	393	203F	4
Litz, Lawrence M.	393	203B	4
Lundin, Robert E.	7-8159	306	Lewis
Lynch, Edward J.	7-472	221	Gilman
Maguire, Patricia W.	250	201A	4
Marlowe, James	395	103C	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
Mohler, Bobby Ann	381	353-4	50
Momyer, Floyd F.	345	110	4
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	355	50
Passell, Thomas O.	397	109	4
Perlman, Isadore	246	115	5
Petzold, Dora A.	395	103	4
Potter, Elinor G.	395	103	4
Powers, John E.	7-472	222	Gilman
Raby, Bruce Alan	345	110	4
Rasmussen, John O.	375	330	50
Rea, Homer E.	7-473	101	Gilman
Reynolds, Fred L.	397	108	4
Ring, Stanley A.	393	203B	4
Robinson, Herman P.	395	103	4
Robinson, Raymond W.	467	203E	4
Ruben, Helena W.	7-8159	318	Lewis
Seaborg, Glenn T.	246	114A	5
Shalimoff, George	470	355	50

<u>Name</u>	<u>Local</u>	<u>Room</u>	<u>Bldg.</u>
Shudde, Rex H.	378	341	50
Slater, Louis M.	377	342	50
	379	347	50
Smith, Warren G.	377	342	50
Snyder, Mark D.	349	103	5
Spencer, Harry E.	7-8159	306	Lewis
Stephens, Frank S.	397	107	4
Stevenson, Peter C.	393	203F	4
Street, Kenneth	467	203E	4
Surls, Joseph P.	349	103	5
Tarrant, James R.	250	201A	4
Templeton, David H.	7-649	106	Gilman
	7-8159	318	Lewis
Tewes, Howard	319	351	50
Thompson, Stanley G.	260	102	5
Tolman, Laurin	381	353	50
	372	311	50
Tuttle, William N.	374	316	50
Vermeulen, Theodore	7-473	211	Gilman
Waite, Jane A.	396	102A	4
Williams, Lawrence A.	470	355	50
Zalkin, Allan (messages only)	246	114	5

Later, at home, I took the older kids out for a bit of traditional "trick or treating."

Saturday, November 1, 1952

After a morning with the kids, Helen and I went to the UCLA game in Memorial Stadium, where UCLA beat Cal by a score of 28 to 7. I had an opportunity to talk with some of the people in the Athletic Department, including Brutus Hamilton, who suggested I accompany the team to Seattle for next Saturday's game with the University of Washington. That sounds like an ideal opportunity to get to know members of the staff, their ideas and ideals, and to get to hobknob with the football players.

Sunday, November 2, 1952

This morning I worked on the photostatic copy of the galley proof to my Chapter 13 of Volume 14A of the PPR ("Nuclear Properties of the Transplutonium Nuclides").

In the afternoon Kalkstein, Hollander, and I went to San Francisco, where we saw the undefeated 49ers beaten by the Chicago Bears, 20 to 17. The 49ers got off to a 10 to 0 lead by the second quarter, but the Bears came back to tie it 10 to 10. By the 4th period the 49ers were up 17 to 10, but a fake punt by Frankie Albert on a 4th down deep in 49er territory backfired, setting up a Chicago touchdown. George Blanda kicked a 48-yard field goal with about five minutes left in the 4th quarter to win the game. We had our usual steak sandwich at Grison's Steak House on Van Ness Avenue before going home.

Monday, November 3, 1952

Today's newspapers, including the Daily Californian and later the Oakland Tribune, carried the announcement of my appointment as Faculty Athletic Representative, resulting in numerous phone calls.

Seaborg is U.C. rep to P.C.C.

Nobel prize winner Glenn T. Seaborg will replace Stanley Freeborn, as the University's faculty representative to the Pacific Coast Intercollegiate conference, according to an announcement made today by Chancellor Clark Kerr.

Seaborg was described by Kerr as one of the top chemists in the world today and also an avid sports enthusiast. He graduated from UCLA in 1934 and has taught at Berkeley for 13 years.

In making the appointment, Chancellor Kerr stated, "It is a remarkable tribute to the quality of the athletic program at the University of California that one of our most distinguished scientists, Dr. Glenn T. Seaborg, replaces one of our most distinguished scholars and administrators, Dr. Stanley Freeborn."

An appointment has been expected since the recent transfer of Freeborn to the post of Provost of the University at Davis. Freeborn, who has represented U.C. in athletic matters at Pacific Coast conference since 1940, has been serving at Davis since the beginning of the semester.

"Here at the University we have maintained a careful balance between the demands of top-flight collegiate athletic competition and the academic requirements of a fine University.

"It is due in no small part to the skill and integrity of Brutus Hamilton, and our excellent coaching staff, that this proper proportion has prevailed. Brutus Hamilton and his staff have been greatly aided in this work by the cooperation of such talented and interested people as Dr. Stanley Freeborn. I am sure that Dr. Seaborg will continue the excellent work done by Dr. Freeborn."

Seaborg received his Ph.D. on the Berkeley campus and has been a full professor of chemistry since 1945.

Daily Californian

Seaborg to Represent U.C. On Conference Group

By ED SCHÖENFELD

It's a long way from nuclear chemistry to such items as touch-downs, home runs and 100-yard dashes, but one of the world's great atomic experts made the move today in Berkeley.

Dr. Glenn T. Seaborg, the University of California's Nobel Prize-winning chemistry pro-

fessor, turned his attention from the 60-inch Berkeley cyclotron to intercollegiate athletics.

The noted chemist accepted an appointment as the University of California's faculty representative to the Pacific Coast Intercollegiate Conference.

AVID SPORTS FAN

Chancellor Clark Kerr announced Seaborg has succeeded Dr. Stanley Freeborn, who served in the capacity for 12 years. Freeborn was recently appointed provost of the university's Davis extension.

The 40-year-old Seaborg—who admits to being an avid sports fan, a former sandlot softball and football player, but never a collegiate athlete—let it be known right from the start . . . he won't be a crusader in sports.

"I would like it to be known that I am not taking this job with any pre-conceived plans in mind," he declared. "I feel that athletics play a worthwhile part in the university life."

Athletic officials and sports fans who fear educators might

de-emphasize intercollegiate athletics have nothing to worry about in the university's new faculty representative.

"I think that if our intercollegiate athletic program is carried on properly, which is at the present level, there is no further need for any de-emphasis," stated Seaborg.

The chemist said he had "mixed feelings" about post-season football games, such as the Rose Bowl game at Pasadena on New Year's Day.

"Personally, I enjoy attending post-season games," declared Seaborg, "but I want to be sure they fit into the academic spirit and tradition and there is no abuse in it."

Calling himself a "rookie," Seaborg said he hopes to lean heavily on Freeborn for assistance during the first Pacific Coast Intercollegiate Conference meeting, December 7 to 11 at Pasadena.

EYE ON SPORTS

"I hope Dr. Freeborn will carry the ball and allow me to work into the job," he said.

Despite his important scientific work, Seaborg always has allowed himself to keep an eye on sports.

"Yes, you can say I'm quite a sports fan," stated Seaborg. "Football is my favorite, but I also like to take in basketball games and track meets here at

the university.

"By the way, how do you like the way the 49ers are doing? I guess I hit it right this time because I have a season ticket to all the 49ers' home games."

Seaborg was talking about the San Francisco 49ers, the hottest team in the National pro league this season.

There are two reasons Seaborg wasn't an athlete at U.C.L.A. He admits one of them—"I wasn't heavy enough to play football." The other is found in his news clippings—he was too busy serving in a series of widely-varied jobs, which helped pay his way through school.

"Although I've always had a great interest in sports, my only experience was playing softball and football on corner lots as a youngster in Los Angeles," reported Seaborg.

U.C.L.A. GRADUATE

The chemist is well fitted for his job with the Pacific Coast Intercollegiate Conference because he had all his schooling in two of its member institutions. Seaborg graduated from U.C.L.A. in 1934 and three years later received his PhD at the University of California.

Except for three years during the war when he was doing scientific work for the Government,

Seaborg has been on the Berkeley campus. He has been a full professor in chemistry since 1945.

He was a co-winner of the Nobel Prize last year for his discovery of plutonium.

Chancellor Kerr, in announcing Seaborg's appointment, declared:

"It is a remarkable tribute to the quality of the athletic program at the University of California that one of our most distinguished scientists replaces one of our distinguished scholars and administrators, Dr. Freeborn. Here at the university we have maintained a careful balance between the demands of top-flight collegiate athletic competition and the academic requirements of a fine university.

SKILL PRAISED

"It is due in no small part to the skill and integrity of Brutus Hamilton, and our excellent coaching staff, and that this proper proportion has prevailed. Hamilton and his staff have been greatly aided in this work by the co-operation of such talented and interested people as Dr. Freeborn. I am sure Dr. Seaborg will continue the excellent work done by Dr. Freeborn."

Hamilton, director of the university's intercollegiate athletic program, hailed Seaborg's appointment as "great for sports."

"I am very pleased that a man of Dr. Seaborg's stature has evidenced enough interest in our intercollegiate athletic program and enough sympathy with sports to take this job," declared Hamilton. "He will be a worthy successor to a worthy man, Dr. Freeborn."



Tribune photo

Dr. Glenn T. Seaborg (right), University of California professor of chemistry, confers with Brutus Hamilltop, A.S.U.C. director of athletics, on his new job of faculty representative to the Pacific Coast Intercollegiate conference.

Among other phone calls this morning was one to Joe Katz to tell him that I am sending back my copy of the galley proof to Chapter 13 today. Naturally, one of the main reasons for my call was to tell Joe about my new position as Faculty Athletic Representative to the Pacific Coast Conference. Joe responded to this announcement appropriately. Later, when I was about to mail him the proof, I remembered that I received no figures with my copy. I wrote Joe a note and said that it might be worthwhile for me to see these if it isn't too difficult to arrange.

On October 23 Harry Levin (Chairman, Conference on Analysis, Gordon Research Conference 1953) wrote and asked for recommendations for a speaker for their conference, the general title of which will be "Unique Applications of Analytical Chemistry to the Solution of Problems." In my reply today I said that I am not sure that I have any worthwhile suggestions to make, but I will pass on one thought. I wrote that a good speaker might be Clement J. Rodden of the New Brunswick Laboratory of the

U. S. Atomic Energy Commission (P. O. Box 150, New Brunswick, New Jersey)
on the applications of analytical chemistry to atomic energy.

In today's mail was an interesting letter from Professor Manne Siegbahn in Stockholm:

Our large cyclotron is now functioning rather nicely with deuterons and we are arranging for alphas and later C-particles. We should now like to take up some researches in the field of the transuranium elements especially on the β -radiation where we have the "know how" and good instruments. We should prefer to do this as an humble complement to the research work in your laboratory on the transuranium elements. To start and coordinate this work here, it would be of a very great value if we could invite some of your people as visiting professor at the Nobel Institute for a period, say of 6 or 7 months beginning in January or February 1953.

I would be very grateful if you could help us with this matter eventually give us the name of one of your men, who may be interested of this task. We would of course pay the travelling expenses as well as an ordinary salary.

Not only did we discuss tomorrow's presidential election at our noontime senior staff meeting, but we also talked about some science, the weekend football games, and my new appointment. Naturally, the fellows had lots of semi-serious advice.

Iz and I looked over the midterms for Chemistry 123, which our reader had graded. Our graduate student, Joe Surls, scored 100 on this examination.

When I got home, I read Clark Kerr's formal letter, dated October 31, about my new appointment:

It is a very great pleasure to inform you that you have been selected as the Faculty Representative to the Pacific Coast Intercollegiate Conference representing the University of California at Berkeley.

I am delighted that you have been able to accept this position. It is my hope that it will prove a rewarding experience to you. Certainly the Berkeley Campus and the Pacific Coast Conference will benefit greatly by your participation.

It is my hope that you can assume the duties of the Faculty Representative immediately.

Due to a number of reasons, Provost Freeborn will probably attend the December meeting of the Conference and in a memorandum has expressed the desire that you accompany him. This will allow you to undertake the new position under the helpful eye of the incumbent.

Tuesday, November 4, 1952

I had more than the usual number of phone calls this morning, but eventually I went down to campus. There I returned and explained the midterms to the Chemistry 123 class and then lectured about alpha decay.

In the afternoon Al Ghiorso and I went out to Mira Vista for 18 holes of golf (AG-85, GTS-94) and much conversation. One thing we talked about was the request I received from Manne Siegbahn yesterday for someone to spend some time in their laboratory. The obvious choice for this "sabbatical" is Stan Thompson, but Stan has many research plans and may not want to leave our lab at this time.

Iz Perlman rode home to Lafayette with me this evening [we often share rides to the Rad Lab since we live next door to each other], and we listened on the car radio to the beginning presidential election returns. Practically immediately it sounded apparent that General Dwight D. Eisenhower was defeating Adlai E. Stevenson. In the late evening Stevenson made a gracious and eloquent talk conceding the election to Eisenhower.

Wednesday, November 5, 1952

Again, I had numerous phone calls and conferences. I also wrote a formal acceptance letter to Chancellor Kerr:

I am happy to accept the appointment as Faculty Representative to the Pacific Coast Intercollegiate Conference representing the University of California at Berkeley. It will be a pleasure to serve in this capacity, and I sincerely hope that I shall be able to carry on the duties nearly as well as has Dr. Freeborn.

I shall get in touch with Dr. Freeborn during his next visit to the Berkeley Campus in order to work out with him a satisfactory method for transfer of responsibilities. As a step in this direction, I have accepted Mr. Hamilton's invitation to travel to Seattle this weekend with the football team.

It seems very desirable to attend the December meeting of the Conference, and I shall plan to attend either a part or all of it. Unfortunately the time, December 7th to 11th, is in conflict with the December 8th luncheon meeting in San Jose of the judges' panel for the Distinguished Service Award of the California State Junior Chamber of Commerce. I shall discuss this matter with both Mr. Hamilton and Mr. Parton, State Vice-Chairman for the Distinguished Service Awards, before trying to decide which course seems to be indicated.

Thursday, November 6, 1952

Present at this morning's open group meeting were Asaro, Carr, Clark, Cunningham, Dauben, Feay, Gunn, Stephens, Hoff, Hollander, Huffman, Hulet, Hyde, Jaffe, Kofstad, Lessler, Levy, Mathur, Michel, Nervik, Passell, Perlman, Raby, Rasmussen, Ruben, Seaborg, Templeton, and Thompson.

Levy corrected a statement he made at last week's meeting. He said that last week when offering a possible explanation for the occurrence of the bottom of the valley in curves of alpha energy vs A (mass number) at $N = 125$ instead of $N = 126$, he had stated that it was definitely known that the neutron levels fill in the order $p_{3/2}$, $i_{13/2}$, $p_{1/2}$ if we

consider just paired neutrons as we approach $N = 126$. This, he said, is not definitely known. Levy made a few more remarks about this subject.

Kofstad presented a tentative contour map of the yield of products obtained in his study of the spallation of silver with 340 Mev protons and explained that the contour spacing gets closer as they go to lower Z and lower N . I questioned whether this indicated that fission is unimportant, and Templeton explained that this is different than the corresponding curve for copper, which flattens out at low Z --there one has fission going on at the same time as spallation. In the discussion I suggested that Kofstad might make a closer study comparing isotopes of similar stability.

Mathur talked about his bombardments of CaI_2 and KI with 100 Mev helium ions. After the chemical separation, he found the cesium fraction to contain a 46-minute activity (?), a 5 1/2-hour activity (Cs^{127}), and a 32-hour activity (Cs^{129}). He did not see a 30-minute activity corresponding to the reported Cs^{130} . Three runs on the time of flight mass spectrograph showed a 45-minute half-life on the Cs^{125} collector plate. Xe^{125} was shown to grow into the gross cesium activity, but they did not establish the genetic relationship between the 20-hour Xe^{125} and the 45-minute activity. They assigned a 2.1-2.2 Mev positron to Cs^{125} .

Feay talked about work he has done on some samples of rare earth tetrafluorides, in which he has attempted to determine their purity by heating and comparing their observed loss of weight with the theoretical loss of weight expected. He worked with samples of CeF_4 and TbF_4 . Cunningham noted that they hope eventually to use the method on AmF_4 in order to check other data.

Raby, who is working with Earl Hyde, described three methods they have tried to isolate a bismuth fraction in good yield within 10 to 20 minutes that will give them a high decontamination factor from radium and lead. This is needed for work on the alpha branch products of AcK .

* * * * *

I continued lecturing on alpha decay to the Chemistry 123 class and then stopped in the Department office before going up to the hill.

At my office I telephoned L. L. Bennett (David Starr Jordan High School), who had asked me to be the graduation speaker for their January class. I explained that my schedule would not allow me to deliver the graduation speech, but I am very flattered that I have been asked. I offered to talk at the school at some later time when I am visiting Los Angeles.

A brief note went to Felix Bloch at Stanford, "May I extend my heartfelt congratulations for the well-deserved award to you of the Nobel Prize in Physics which I heard announced today." [Bloch is sharing the physics prize this year with Edward Mills Purcell of Harvard "for their development of new methods for nuclear magnetic precision measurements and discoveries in connection therewith." The Prize in Chemistry is being shared by Archer John Porter Martin (Great Britain) and Richard Laurence Millington Sygne (Great Britain) "for their invention of

partition chromatography."]

In today's mail a November 3 letter arrived from G. Curtis Pritchard (Secretary, City of Philadelphia, Board of Directors of City Trusts), who thanked me for my response to his letter of October 21 about the Scott Award. Pritchard said that he will endeavor to give me plenty of notice about the date for the presentation of the award and will try to make it coincide with one of my trips East.

A nice congratulatory letter arrived from Miss Ethel Bishop, who acted as secretary to Dr. Stanley Freeborn while he was on this campus. She said that, if there is anything in connection with the athletic phase of the functions of Dr. Freeborn's Berkeley office in which she might be of some assistance, she hopes I will feel free to call upon her. Miss Bishop explained the athletic files are still maintained in the Berkeley office at 101 Giannini Hall although the majority of the recent correspondence has been forwarded to Dr. Freeborn in Davis for his handling. She added, "Best wishes to you in this most pleasurable field of endeavor."

Much of my afternoon was spent with phone calls, conferences, and checking on the research.

Then, in the early evening, I left with the Cal Bears, coaches, members of our Athletic Department, press, etc. on their trip to Seattle aboard the Southern Pacific's Cascade.

Friday, November 7, 1952

This was a most exciting day for me, travelling with our football team and coaches. Will Connolly, a sports writer for the San Francisco Chronicle, interviewed me during the the trip. I refrained from offering Pappy Waldorf any advice on tomorrow's game (my skill is in postgame quarterbacking). I believe my presence may have limited the number of raunchy stories.

After we arrived in Seattle, I had dinner with members of the California Alumni Association at the College Club (University of Washington, Seattle), where I made a few general remarks and met a number of alumni.

Saturday, November 8, 1952

This morning I spoke to members of the Chemistry and Physics Departments here at the University of Washington on "Recent Advances in the Transuranium Elements." The talk was well received.

In spite of my sitting on the bench and high up in the press box where some of the coaches were operating, Cal lost to the University of Washington by a score of 22 to 7. We later took the train for Berkeley.

Sunday, November 9, 1952

It was a rather sad trip back to Berkeley today. I read the interview of me that Will Connolly wrote for the Chronicle. Sports

writers certainly have a more dramatic style than science writers.

Will Connolly

Cal's Faculty Rep Admits He's a Football Filbert

SEATTLE, Nov. 8.—University of California's new faculty representative to the Pacific Coast Conference is merely a Nobel Prize winner for his work in nuclear chemistry, specifically with transuranium and plutonium. That would be atomic bomb stuff.

The young man's name is Glenn T. Seaborg, familiar in scientific journals throughout the world but not exactly a household word in sports pages. Dr. Seaborg made his first trip with an athletic team when he came up here with the Cal footballers for the Washington game. He sat in the diner and lounge cars of the S. P.'s Cascade and enjoyed the company of the varsity.

Why should a great brain up in the clouds bother his head about campus sports? Wasn't his acceptance of the job as faculty monitor of an athletic league a stepdown in class? He's a laboratory genius and the Coast Conference is muscle.

"I see nothing inconsistent or incongruous in my accepting the "In fact, I was flattered. Athletics post," said the Nobel Prize man. In fact, I was flattered. Athletics contribute to the dignity of man, as an individual, by developing his physical body and his moral stamina under stress."

Dr. Seaborg isn't preachy. It's like pulling teeth to get him to talk about himself. The only information he volunteered is that he is a football nut. He even bought a season ticket to 49er pro games. In the past 15 years, more or less, he has seen every California home game except for a period when he was stationed in Chicago. University of Chicago big domes had a part in the first mushroom firecracker that went off at Los Alamos, so you can guess why Dr. Seaborg commuted between Berkeley and Chicago.



GLENN T. SEABORG

He Intimidated the Coaches, All of Them

One of the beauties of having Dr. Seaborg on the train was that the coaches, from Lynn O. Waldorf down, were intimidated. They refrained from drawing play diagrams on the menu card. Pappy, Wes Fry and Herm Meister were itching to mystify the press and other laymen with X's and O's, but they were afraid the transuranium man would top them with chemistry symbols which only a handful of Einstein-type bright boys could interpret.

Dr. Seaborg is Lincolnnesque in stature and mood. He stands six feet three and is "skinny" at 172 pounds. He dresses in dark gray, wears the wrong necktie, with the wrong suit and has a humorously button nose which makes him look younger than he is. He could pass for a basketball center.

From other members of the California entourage, we learn Dr. Seaborg won a trunkful of national and international awards, but you'd never get it from him. He was voted the Young Man of the Year, a few seasons back, by the U. S. Junior Chamber of Commerce. He owns a medal for achievement in pure chemistry bestowed by the American Chemistry Society. About the time he won the technical Nichols award, California people saluted him as alumnus of the year.

Diogenes Is Looking For This Type Golfer

The man doesn't pretend to be an ex-athlete. The only participation sport he pursues is golf. Otherwise, he is content to be a spectator. He was born in Michigan, reared in Southern California and never did get around to trying out for a team. Too busy working for a Bachelor of Arts degree at UCLA and a Ph.D. at Berkeley.

What does he shoot at golf? In the low 90's on the Mira Vista course, which used to be the Berkeley Country Club.

"I once broke 80 but that was a fluke and wasn't scientifically arrived at," Dr. Seaborg apologizes. "The mean average is the scientific approach. Put me down for a 93 golfer."

The Seaborg family moved into a new home in LaFayette recently. At the moment, the professor's hobby is building a patio and making other improvements around the house. Of course, a technician of his caliber has no trouble with hammer and saw. A guy posted in the esoteric secrets of atomic energy shouldn't have any trouble bricklaying a barbecue pit.

"That's what you think," Dr. Seaborg protested. "I'm only an amateur at hanging a picture. I've bruised my thumb."

Helen Seaborg, his wife, can play a spot of golf with him, but, as he says, Helen has been rather busy rearing a family. First things first with the Seaborgs. They have a handsome brood of three sons and a daughter, Lynne. Peter, 6, was the first born and after Lynne came David and Stephen. Helen's golf has declined, but she's young enough to catch up later. The 2 a. m. formula doesn't last forever.

Dr. Seaborg is fascinated by football, though he has an appetite for other sports.

The tactics, the evolution of strategy, appeal to his high mind.

"I must confess I follow the ball carrier," he confesses. "Some of the greatest thrills I experienced were provided by Sam Chapman, Vic Bottari, Jackie Jensen and Johnny Olszewski. Occasionally I sit in the end zone, the better to appreciate the line play."

The Prof. Played Hookey in 1945

The professor reads baseball box scores, Coast and major, and, when he was isolated in Chicago in 1945, he snuck away from nuclear classes to see the last two games of the Chicago Cubs vs. Detroit Tigers World Series.

"The Cubs didn't have the pitchers," he recalls. "Chicago was using Hank Borowy every other day."

Dr. Seaborg succeeds Stanley Freeborn, another civilized gent, as Berkeley's delegate to the councils of the Pacific Coast Conference. Professor Freeborn has been promoted to provost of the Call Aggies at Davis. He is the world's greatest authority on the Imhoff tank.

From what we gather, Dr. Seaborg accepted the Nobel Prize rather casually. He flew to Stockholm for the investiture and caught the next flight home. The Nobel Prize consists of a cash award, a certificate or scroll and a medal.

Of course you think the scroll and medal are enshrined in the LaFayette household, right next to the Seaborgs' wedding picture? No?

The transuranium and plutonium man wouldn't know where to put his fingers on the Nobel certificate. It's somewhere around the house, most likely in a bureau drawer under his socks.

Monday, November 10, 1952

Back to the world of science! However, since I had received several congratulatory letters about my new appointment, I wrote thank-you notes to some of the writers--to Dr. William Goodricke Donald (Cowell Hospital), to Harold R. Deal (Tide Water Associated Oil Company), and to James K. Dobe (Oakland).

On November 4 Leo Yaffe wrote from McGill University that The Physical Chemistry Section of the Chemical Institute of Canada is planning a symposium entitled "Recent Techniques in Physical Chemistry." Leo said that he had suggested that a talk on the manipulation of dangerous and/or radioactive materials be given and that Nelson Garden would be an ideal speaker. He wrote Garden about a month ago but has received no reply, and Leo asked that I check on this. I talked with Garden, and this morning I wrote to Leo that Garden contemplates giving him an early answer. I wrote, "I can imagine that you might be very busy getting things underway at McGill. We are looking forward to seeing good things continue to come out of there."

J. Bart Sutton (Manager, San Francisco Sales of E. I. du Pont and a

friend since the Met Lab days) wrote on October 28 to ask if I would speak at a Friday luncheon meeting next April of the Golden Gate Paint, Varnish, and Lacquer Association. Sutton said that they will hold all four Fridays open until they hear from me. In today's acceptance I explained that I am not sure about my travelling schedule next April, but I gave him a tentative day of April 10.

Other mail that I looked at included a November 5 update of the course "Modern Physics for the Engineer" from Louis Ridenour, who said there are about 500 paid registrants in the class. Ridenour said that he would appreciate receiving the manuscript of the notes to accompany my lecture (which will be given at UCLA on March 17, 1953) at least three weeks before I am scheduled to speak.

I noted a verifying letter from Warren H. Crowell (President, UCLA Alumni Association), who phoned me last week about the uncertainty in the date of their Alumnus of the Year program because of the change in plans made by the Regents.

Donald Lane wrote on November 6 about an informal conference he had on October 22 with Roland Anderson at the AEC to plan for the Pretrial Conference, which was held on November 5. Lane said that he will send me the transcript of the present pretrial conference, but he can report that progress is being made. He wrote that Chairman Ooms indicated that the Board considers that the Regents are no longer a factor in this proceeding. The Board agreed to rule on the validity of Contract eng-30 as requested but instructed counsel to file written briefs in support of their contentions with respect to eng-30 validity. Lane said that he is to file his brief on this point by January 9, Anderson will file his brief by February 9, and Lane will file a reply brief by February 20, if he considers it necessary. Lane said he expects that the Board will decide this point in March and he gathers that they plan to meet in April to arrange for final hearing of this proceeding. Later in today's mail the transcript of the conference arrived from Lane, who asked that Segrè and I mail it to St. Louis after we have read it. He would like it returned by early December. He wrote, "You will note that Chairman Ooms raises an interesting point in his remarks starting on page 68, namely, may an inventor who has no title to his invention nevertheless apply for an award? You will note also on page 78 that Mr. Anderson indicates that applicants may be entitled to an award on the basis of eng-30 being valid, or on the basis of other facts if eng-30 is invalid." In order to have a complete record and so that I may study the transcript, I asked Doral to have it copied, with copies for Segrè, Wahl, Kennedy, and Lane.

I also received a report from Amos Newton about the visit of Dr. Alden W. Hanson to Berkeley and Livermore (Project Whitney) on October 8 and 9.

Naturally, at our noontime senior staff luncheon meeting I was able to pass on some interesting information about the Cal Bears and Saturday's game. Other matters discussed at this meeting was the continuing need for additional Whitney personnel and Manne Siegbahn's request for one of our men to work with them at the Nobel Institute of Physics in Stockholm.

Tuesday, November 11, 1952

Most of my morning was spent in conferences on campus. Then, at 11:10 a.m., I gave the final lecture on alpha decay to the Chemistry 123 class.

Then, after lunch, Al Ghiorso and I joined Luis Alvarez and Harvey E. White (physics professor here in Berkeley) at Mira Vista for 18 holes of golf (HEW-105, AG-91, GTS-90, LWA-99). Luis and I won a low ball-low total match, 4 up.

Wednesday, November 12, 1952

Again there were numerous phone calls this morning. One was from Doyle L. Northrup (Office of Atomic Energy, Headquarters Air Force, Washington), asking about a convenient date for a meeting of the Boner Panel [the Panel on the detection of nuclear weapons tests, so named because Professor Charles P. Boner is the chairman]. I told him that I will be in Washington on January 8, 9, and 10; an ideal time would be just after this, beginning on Sunday, January 11. Later I wrote Doyle a note confirming this information.

Also, during the morning I stopped in First Aid for routine lab tests (urinalysis and blood).

I returned the reporter's transcript of the pretrial conference to Lane, along with an extra copy for his use. I wrote:

I was very interested to read this transcript and your letters of November 6 and 8, 1952. I am pleased to learn that the Board seems to consider that the Regents are no longer a factor. However, it does seem that Mr. Anderson may want to try to invalidate eng-30 on the basis of the unsigned Regents' disclaimer so that to this extent the Regents may still be in the picture. As I have mentioned before, I feel that the Government urged us to sign eng-30, almost desperately, at a stage when there was no indication that a Regents' disclaimer was forthcoming and after the inventors had made it fairly clear that they couldn't accept responsibility for obtaining such a disclaimer. It was finally assumed that Lavender would go after this signature, but it appears he gave up entirely. You will remember that the disclaimer was attached to eng-30 on an unnumbered page. You may notice in Anderson's letter of May 15, 1947 that he seems to assume the validity of eng-30 at this late date.

There seemed to be considerable discussion at the hearing as to whether the inventors had assigned any or all of their inventions to the University. You will recall that, to the best of the inventors' memories, no such assignments were made even though the University did make assignation forms available for such purposes.

I replied to a November 6 letter from J. C. Dillon (Department of Engineering, UCLA), who invited me to repeat my lecture of March 16, 1953, "The Actinide Elements and Atomic Power," on Tuesday, March 17, 1953, at China Lake (Naval Ordnance Test Station, Inyokern). I explained that I will not be able to do this since I will be attending a national

meeting of the American Chemical Society in Los Angeles that week. However, I wrote that if the talk could be scheduled for the end of the week, on Friday, March 20, I would be willing to undertake it. Dillon also asked for an estimate of my expenses for the March 16 lecture (for the series, "Modern Physics for the Engineer"), and I said this is complicated by the fact I am combining other business on my trip, but I would guess that about \$25 would cover the expenses.

A note also went to G. Curtis Pritchard (Board of Directors of City Trusts, Philadelphia) to inform him the only trip to the East for which I have a date scheduled will be to Washington on January 8th, 9th, and 10th. Therefore, the evening of January 7th would be ideal (for the Scott Award) although I could probably get away from my Washington meeting for an evening or stay over in the East until early the following week.

In today's mail was a note from Mike J. Koll, thanking me in behalf of the California Alumni Association for attending their Seattle meeting and taking part in the program. Koll wrote:

I am not going to make a criticism of the game Saturday as I realize that you were not at your post long enough to fully install the "atomic Seaborg system." However, this Saturday we will all be expecting developments along the above-mentioned line and better results on the score board. I'm sure I need not remind you of results after your predecessor's two losses.

I also made a few notes about Iz Perlman to be used in my remarks on his accomplishments when he receives a gold medal from the California Section of the American Chemical Society Friday.

Later I visited a number of the labs to check on the research. I have had several talks with both Stan Thompson and John Rasmussen about Manne Siegbahn's proposal. Stan doesn't feel he should leave Berkeley before next summer, and I am not sure that the Swedes will be able to pay his equivalent salary; however, John is willing to go in January even with his year-old twins.

Thursday, November 13, 1952

Present at the research group meeting this morning were Asaro, Carr, Cobble, Conway, Cunningham, Dauben, Feay, Glass, Hoff, Hollander, Hulet, Hyde, Jaffe, Kalkstein, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Rasmussen, Ruben, Seaborg, and Templeton.

Rasmussen gave a lengthy, but interesting talk on the theoretical formulation of high energy d,p cross sections; this evoked considerable discussion.

Feay described a number of experiments he has done to determine the composition of terbium tetrafluoride. When I asked if he couldn't prepare pure TbF_4 by fluorinating until he got a minimum in the lattice constant, he said that it could be hard to do because the fluoride samples generally are not in very good crystalline form. I mentioned that Katz has a very good method of fluorinating. Cunningham added that

they want to measure the magnetic susceptibility as a further check on the composition.

Asaro spoke about their measurements of the alpha spectra of a sample, obtained from Higgins, of Am^{241} containing 1% Am^{243} . He said the main alpha group of Am^{243} has the alpha-particle energy of 5.267 ± 0.005 Mev (alpha-particle spectrograph) and 5.27 Mev (pulse analyzer). Asaro said they saw another alpha group of about 10% abundance with an energy of 5.226 Mev. He said that since the alpha-particle spectrum of Am^{243} is quite similar to that of Am^{241} , it would not be unexpected if the main alpha group of Am^{243} did not leave the daughter Np^{239} nucleus in its ground state, corresponding to the situation in Am^{241} in which there is a prevalent 60 kev gamma ray. Asaro reported that Don Martin ran an Am^{243} sample on his scintillation pulse analyzer and found a 75 kev gamma ray (0.9 gamma ray per alpha particle). They checked these results by making alpha, gamma coincidence measurements on a sample of Am^{243} and found a 75 kev gamma ray in coincidence with 90% of the alpha particles. They determined the conversion coefficient of the 44 kev gamma ray in Cm^{242} , using Am^{241} as a calibration standard. He said that in Am^{241} there is a 60 kev gamma ray associated with 40% of the alpha transitions and that the conversion coefficient came out to be $\alpha_L = 5 \times 10^2$, using Dunlavey's figures of 4% and 19% respectively for the M and L conversion electrons in coincidence with the alpha particles.

* * * * *

After giving the Chemistry 123 lecture, which today was about accelerators and neutron sources, I stopped in the Chemistry Department office to talk with Miss Kittredge and some colleagues before going up to the hill.

After lunch I dictated (to Millie Davis) a response to Manne Siegbahn's recent letter:

I would very much like to see your laboratory take up research in the field of transuranium elements and feel sure that you will make many worthwhile contributions through the use of the excellent facilities and people which you have. I have discussed with some of our people the opportunity which you presented in your letter and I believe that I have some suggestions that may interest you.

I am not sure from your letter whether you are interested in having more than one such visitor, and therefore I will tell you about two possibilities, Drs. Stanley G. Thompson and John O. Rasmussen, both of outstanding ability and accomplishments. I should add at the beginning that both of these men have families (wife and two children each) and do not feel that they could come without them; this, of course, may impose a greater expense than you feel that you should undertake.

Dr. Thompson has had about ten years' experience in the transuranium field, is about 40 years old, and, as you know, is co-discoverer of berkelium and californium; he is probably the best man in the world on the chemical separation methods for the transuranium elements. His salary here is \$845 per month. He would find it difficult to

rearrange his extensive research program so as to make it possible to come before the summer of 1953 but possibly could come sooner if this is necessary.

Dr. Rasmussen is a younger man, 27 years old, for whom we predict a brilliant future. He has done work on beta and alpha radioactivity and is familiar with our chemical separation methods for the transuranium elements. His salary is \$490 per month. Dr. Rasmussen feels that he could come as early as January or February, 1953.

We shall await with interest your reaction to these possibilities.

In the mail that arrived today was another note from Manne Siegbahn, requesting that we send him a copy of Gary Higgins' thesis ["An Investigation of the Isotopes of Americium and Curium"] via airmail. He also asked for other related papers. I added a P.S. to my letter to tell Siegbahn that I am enclosing a copy of the thesis and some lists from which he may choose other reprints that he wants.

The rest of the day was spent on administrative duties, conferences, and conversations.

Friday, November 14, 1952

Again there were numerous phone calls the first thing this morning about a variety of subjects.

I have realized that my position as Faculty Athletic Representative is going to require the services of an assistant for various routine tasks. I asked one of our sports enthusiasts, Marv Kalkstein, if he would like to take on the task; he eagerly accepted. Margie Hollander agreed to handle the needed secretarial duties. Her unsolicited remark was that I really should begin to wear tweed suits, which would be more fitting for my new position than the type I normally wear (durable hard worsteds). [Actually, Helen has suggested that my wardrobe needs renewal and has offered to come into Berkeley soon to help me pick out fabrics.] I told both Marv and Margie that I have heard that they will be given passes for athletic events for their services. [Marv is well known for procuring tickets for "sold-out" events and cut-rate tickets for others, so this should be an incentive for his help.]

Yesterday I received an acceptance note from S. A. Goudsmit (Editor, The Physical Review) for my manuscript "Activation Energy for Fission." Goudsmit agreed to publish it as a "Letter," but he identified a little ambiguity. Today I wrote to explain how the ambiguity can be fixed.

In my rounds of the labs I noticed that Health Chemistry is replacing the box in Room 103 with Box 220; this will be used by Glendenin and Steinberg of Argonne, who are here now working on the distribution of the spontaneous fission products of Cm^{244} .

Helen and I attended the second annual award dinner and dance at the Claremont Hotel this evening, sponsored by the California Section of the American Chemical Society. I introduced Iz Perlman, who was presented with a gold medal (the 1952 California Section Award) for his

"outstanding contributions in the field of nuclear chemistry." Dancing, with music by Johnny Blake, followed the program.



A. H. Batchelder presenting 1952 California Section Award to Isadore Perlman, November 14, 1952

Saturday, November 15, 1952

I did a bit of yard work before Helen and I went into Berkeley for the football game. Today Cal defeated Washington State by a score of 28 to 13. Unfortunately, Billy Mais, our quarterback, dislocated his thumb on his throwing hand. Naturally I went to the locker room after the game to congratulate Pappy Waldorf and "my boys," and I was invited to attend the training table on Monday (Big Game week).

Sunday, November 16, 1952

This was a relatively relaxing day--as relaxing as a day can be with four children demanding some fatherly attention.

Monday, November 17, 1952

Again phone calls and conferences--I certainly have less free time than I used to have.

I interviewed Herbert R. Johnston, an electrical engineer who is working for his doctorate, as a possible employee in our group or in the

Whitney program. Johnston, who will take his prelims next spring, said that he had been an associate professor at Northwestern University and that Argonne had offered him \$650/month to stay there without a Ph.D. I brought Iz and Al in on the conversation about research projects in which we are interested. Johnston will return in a month or so for another conference.

Joan Louise Feeney began working in Bldg. 4. She is the secretary Doral interviewed and hired for Project Whitney, but she will work with us until she has a complete clearance.

Joan Feeney



Another letter, dated November 10, arrived from Paneth, saying that he is glad I am reconsidering the question of my resignation from the Joint Commission on Radioactivity of the International Council of Scientific Unions. He wrote that they will be happy to have me stay on even if I cannot attend the Stockholm meeting. Paneth also discussed the provision in the statutes of the Commission that allows "advisory councillors" to claim travelling expenses if they take the place of a "member." Paneth asked that I let him know, if I cannot attend the Stockholm meeting, for it might be possible for someone who is interested in our work to attend.

We had the usual senior staff luncheon meeting and talked about some scientific matters, in addition to sports events.

At 6:30 p.m. I had dinner at the training table of the Bears in the Turquoise Room of the one of the student cafeterias. I understand the varsity table usually has about 53 players, but tonight there were about 75, including some of the Ramblers who played in Saturday's game. I sat with Don Johnson, Ray Willsey, and Johnny Olszewski. I enjoyed the conversation, but I found I could not eat what was on my tray: chicken gumbo soup, a 12-ounce grilled steak garnished with two small baked potatoes and string beans, a gelatin salad with pineapple, peaches, and pears, a pint of milk, a pint of lemonade, a pot of tea, and a chocolate sundae. I was told that the players are never given fried foods nor gas-forming foods. After the meal, there were movies of Saturday's game with Washington State, along with Pappy's critique. It really was a much more exciting dinner than we have at home in Lafayette.

Tuesday, November 18, 1952

Before going down to campus, I handled a number of administrative matters.

In today's mail was the following postcard from Lars Gunnar Sillén:



He wrote: "Hello Glenn, This is Kiruna, north of the Arctic Circle, where some 20,000 people live around one of the nicest lumps of Fe_3O_4 in the world. It is so fine that both Allies and Germans wanted to liberate it in WW II. It is -10°C , lots of snow and good skiing. I am here for a few days doing some inorganic chemistry, and shall then return to our new house north of Stockholm and to Birgit, Gunnar, Bo, and Birgitta (a girl as ordered, born 2.I.52). Regards to all your family."

I gave the final lecture on accelerators, neutron sources, and neutrons to the Chemistry 123 class at 11 this morning.

In the afternoon Harvey White, D. Schaeffer, Luis Alvarez, and I went out to Mira Vista (HEW-109, DS-95, LWA-94, GTS-95). Luis and I won our low ball, low total match, 9 and 8.

Wednesday, November 19, 1952

I took care of a bit of accumulated correspondence this morning and then looked over some reports and journals.

M. A. Stewart (Dean of the Graduate Division) requested that I fill out an evaluation of one of last year's visitors, Ugo Croatto, for the Committee on International Exchange of Persons (Washington). I did this and returned it to Dean Stewart for signature and transmittal.

On November 10 William H Sullivan sent a number of suggestions for the "Table of Isotopes," along with a request for information about when we need the data. I thanked H for his suggestions, explained that we have not yet sent the manuscript to the journal, and said that we shall look forward to seeing him and discussing the Atomic Energy Handbook and

his Trilinear Chart when he visits Berkeley.

I also responded to an October 27 letter from Katharine Way (National Bureau of Standards) and said that we should be happy to participate in her new nuclear data collection and exchange program. I suggested that she list Dr. J. M. Hollander as our official representative and deal directly with him in carrying out the program.

W. W. Gay (Sutro & Co.), Secretary of the Class of 1913, invited me to be a guest of the Class of 1913 at their annual Steak and Stein party on the night before the Big Game, Friday 21st in the Comstock Room of the Palace Hotel. Gay explained this is the class of President Sproul and Governor Warren of the Class of 1912 will also join with men of their college generation. Their guests will be coaches of major sports who will be presented by Brutus Hamilton. Stan Freeborn will introduce me as his successor. I promptly accepted the invitation.

G. Curtis Pritchard (City of Philadelphia, Board of Directors of City Trusts) wrote again (November 17) and asked for another copy of the UC Office of Public Information data--outline of professional career, personal biography, and memorandum covering scientific contributions. Doral mailed these to him.

I noted a memo from F. A. Jenkins (Associate Dean of the Graduate Division) that I, along with L. W. Alvarez and B. J. Moyer, are on the thesis committee of Boris Ragent, who was just advanced to candidacy for the degree of Doctor of Philosophy in the field of physics.

I looked over some of the research going on in the labs and chatted with some of the fellows.

Thursday, November 20, 1952

The research group met as usual this morning and the following people attended: Asaro, Carr, Clark, Cobble, Cunningham, Feay, Glass, Grover, Hollander, Hulet, Jaffe, Lessler, Nervik, Passell, Raby, Rasmussen, Ruben, Seaborg, Slater, and Templeton.

Templeton gave a report about the work of Jan Siemons, who has made some progress on the crystal structures of $2\text{NH}_3 \cdot \text{H}_2\text{O}$ and $\text{NH}_3 \cdot \text{H}_2\text{O}$. Templeton said the crystals are colorless and are made and kept at low temperatures. Templeton also talked about the work, including determination of the heat of formation, of Professor Giaque's group on these compounds.

Asaro presented a plot of the gamma activity as a function of energy made from data obtained by counting Pu^{238} in a scintillation pulse analyzer and then he showed a possible energy level diagram. In the discussion he said that the measured energies seem to correspond with the differences observed between the alpha-particle energy groups. The intensities were L x-rays (too much lost in absorber for accurate calculations), 75 keV (6×10^{-5} gamma/alpha), 100 keV (3×10^{-5} gamma/alpha), 150 keV (10^{-6} gamma/alpha). There was considerable discussion about the significance of the work.

Jaffe reported that the uranium L_{β_1} x-ray has been found to be in error by 60 volts from measurements of the x-rays following the decay of Pa^{232} , Pa^{233} , Pu^{238} , and Pu^{240} . Jaffe said they found 17.28 ± 0.02 kev although the accepted value is 17.218 kev. Jaffe talked about the installation of a new arc in the machine, after which they found that all measured energies were below the accepted values. On examination of the new arc, they found that it had been sprung in machining. They are now having the arc remachined.

Hulet said that the alpha particles of Cf^{248} , produced in a helium ion bombardment of heavy curium isotopes, and possibly seen on the alpha pulse analyzer now seem definite. The best value for the energy is 6.32 ± 0.03 Mev. The mass assignment is based upon the energy fit with alpha systematics although there is a possibility that the isotope is Cf^{247} . I asked if he could measure the half-life, and he said that they possibly can, if it is not too long; he said they have only 1 c/m. The estimated half-life is between six months and one year. The amount of target material, Cm^{245} , is experimentally unknown, so that the approximate three-month half-life based on yield depends on a number of assumptions. Hulet also reported on more work on the half-life of Cm^{245} produced from the electron capture decay of Bk^{245} : the lower limit is now set at 1500 years and the upper limit is set at 2800 years. He suggested the best value might be 2200 years. He also reported the alpha-particle energy to be 5.52 Mev although they expect higher energy groups in lower abundance.

* * * * *

Iz gave the Chemistry 123 lecture today; the subject was induced nuclear reactions.

After I checked in the Department office, spoke with some of my colleagues, I went up to the hill.

I acknowledged a November 18 letter from Stanley Parton (State Vice-Chairman, Distinguished Service Awards, California State Junior Chamber of Commerce), who stated the date for their luncheon meeting has been changed to December 15 in order that they might receive all of the nomination blanks and will be held in the Hotel St. Claire, Market and San Carlos Sts., San Jose. I wrote that I shall attend the luncheon meeting.

It was a normal afternoon: phone calls, conversations, reading, and writing.

Friday, November 21, 1952

There were the usual administrative matters to handle today, in addition to phone calls and conversations with some of the men. In my walk through the labs I learned that Steinberg and Glendenin had a curium spill in Room 103 of Bldg. 5. The floor, sink, and the hall were hot, plus one spot by the balance in Room 105. The Health Chemist was busy checking, cleaning, and taping the areas.

I received another memo from Dean M. A. Stewart, this time requesting

that I fill out an evaluation form for Mario A. Rollier for the Conference Board of Associated Research Councils in Washington. I filled out the form and returned it to Dean Stewart for signature and transmitting.

I left the lab rather early in order to prepare for my evening at the Steak and Stein party in the Comstock Room of the Palace Hotel at 6:30 p.m. I was the guest of the Class of 1913. This turned out to be a congenial gathering of alumni of that generation; the guests were coaches of major sports, who were introduced by Brutus Hamilton. Stan Freeborn introduced me as his successor as Faculty Athletic Representative, and I made a few appropriate remarks.

Saturday, November 22, 1952

Today's event was the "Big Game," held at Memorial Stadium, with Cal winning by a score of 26 to 0.

Sunday, November 23, 1952

I read, wrote, and played with the kids.

Monday, November 24, 1952

I responded with the following to a November 18 telegram from Abraham Feinberg (President, American Committee for the Weizmann Institute of Science, New York) for an appropriate message for inclusion in a booklet they are issuing in connection with a "Salute" to Weizmann's memory on December 17 at the Waldorf-Astoria:

The life of Chaim Weizmann was, as he put it, a "tug of war" between Science and Zionism. Yet in another sense the forces which struggled for fulfillment at his hands were not opposed, rather they were motivated by the same burning faith in the dignity of the human mind and soul. And there have been few whose lives could offer as much testimony to the victory of such faith in so many of the issues and enterprises of humanity. In the passing of Dr. Weizmann, science has lost a brilliant investigator, Israel a faithful son, and the world truly great man.

In the office Lorraine M. Hanna began working with us as an additional secretary.

Lorraine Hanna



Also, other new employees are continuing to arrive for the Project Whitney group.

Much of our senior staff luncheon meeting today was devoted to the field of sports.

Later I did some reading and conversing with members of our research group.

Tuesday, November 25, 1952

The morning was spent in conferences and conversation. Iz gave the Chemistry 123 lecture and again discussed induced nuclear reactions.

After lunch I again took advantage of the good weather and played golf at Mira Vista with Harvey White and Ellis Steinberg (HEW-106, ES-110, GTS-98).

Wednesday, November 26, 1952

Much of my day was spent trying to catch up on my report reading.

In the mail that arrived today was a November 19 letter from David Z. Beckler, explaining that he joined the Atomic Energy Commission's new Office of Industrial Development on November 3. Beckler gave his new address and said that Lt. Col. Francis T. Bradley (AF) of the staff of the RDB Committee on Atomic Energy will be taking over his IO-7 Panel duties. In addition, he wrote that Robert Blau of AFOAT-1 will serve the Panel in connection with duties involving the Joint Intelligence Committee, JCS; adding that a mid-January meeting of the Panel is contemplated. Beckler said that I will be notified shortly as to the possible dates.

I also received a thank-you letter from Stan McCaffrey (California Alumni Association) for my photograph and biographical information that was sent him at his request as one of the leading alumni of the University of California.

Thursday, November 27, 1952 - Thanksgiving Day

Al and Wilma Ghiorso, along with their children Bill and Kris, came out to Lafayette to join the Seaborgs for a traditional Thanksgiving dinner of turkey and all the trimmings. Ellis Steinberg and Larry Glendenin also joined us.

Friday, November 28, 1952

This was a relatively quiet day at the lab--some of the men are taking an extra holiday. However, I again did some reading.

Saturday, November 29, 1952

I read and played with the kids.

Sunday, November 30, 1952

I played with the kids for a while before going to San Francisco to see the 49ers game with Marv Kalkstein, Allan Zalkin (I had an extra ticket that I was able to give Zalkin), and Jack Hollander. We saw the Los Angeles Rams dominate most of today's game; they gained 358 yards to the 49ers 208 yards and beat the 49ers by a score of 34 to 21. The 49ers were down in the fourth quarter by a score of 7 to 27, but they rallied to 21 to 27 on Y. A. Tittle's two touchdown passes. Unfortunately, Los Angeles scored again to make the final score 34 to 21; a 51-yard run set up the score. As usual, the four of us had a steak sandwich at Grison's after the game. I then easily talked them into stopping in at a burlesque show in North Beach for a while before going home.

Monday, December 1, 1952

I had the usual number of phone calls, administrative matters, and conversations this morning. There was a power failure, but the monitors reported that the Berkeley boxes were ok.

I also signed a response to a November 26 letter from Helen M. (Mrs. Watson) Davis (Science Service), who asked that I check the masses of the most abundant and/or most stable isotopes on the Periodic Table of the Elements that they issued in 1947. I made two remarks on the table; first, that the symbol for francium has been officially changed to Fr instead of Fa. The other comment was that, although Re^{187} is unstable with a very long half-life, it might be desirable to use its mass number in the Table of Chemical Elements since its abundance is so much greater than that of Re^{185} . I added that, in the case of the heavy radioactive elements, especially those beyond americium, longer-lived isotopes will surely be found but those listed are the longest lived of those presently known.

The senior staff met at noon in my office for our regular Monday meeting. Ken Street gave Iz and me a memorandum he prepared on the manpower and space requirements for the Project Whitney chemistry group.

Stan Thompson spoke with me about an idea he has, and then, I wrote to Eric R. Jette (Los Alamos) about the possibility of recovering some americium from some crude material they have.

Later I learned that my graduate student, Walter Richard Nervik, passed his prelims today with the comment "good, solid, but not very imaginative." His committee consisted of William D. Gwinn, Wendell M. Latimer, David H. Templeton, Charles W. Tobias, and William A. Nierenberg.

Tuesday, December 2, 1952

After the phone calls, I visited some of the labs to look over the research.

Iz gave the Chemistry 123 lecture; today's topic was nuclear fission and the chain reaction.

In today's mail was a lengthy letter from Ronald A. Brightsen (Westinghouse Electric Corporation), whom I met at the Gordon Conference this past summer and who was a graduate student with Charles Coryell. Brightsen inquired about the possibility of part-time employment while he completes graduate work for a Ph.D. Brightsen has an impressive background; in fact, I was unaware that he did not yet have his Ph.D. However, his base pay at Westinghouse is \$6840 per year with regular overtime of \$1800 per year--far more than we pay graduate students.

A reminder arrived from Brutus Hamilton about the meeting of the Athletic Advisory Board on Wednesday, December 3.

I spent some time looking over a draft of a report I am preparing (with Earl Hyde) on our group's program of nuclear chemical research and recent results for Ken Pitzer to take to a meeting in Oak Ridge.

Wednesday, December 3, 1952

After I took care of a number of administrative matters, I worked on the report for Ken Pitzer. Under our program are covered: I. Basic 1. Continued work on radioactive properties on elements above thallium, all of which are tied together by alpha radioactivity; 2. Physical and inorganic chemical investigations of rare earth and actinide elements; 3. Investigation of mechanism of high energy nuclear reactions; 4. Beta and gamma ray and mass spectroscopy. II. Basic and Applied. Our program of heavy isotope research (production, fission measurements, neutron capture cross sections, etc.) at the Materials Testing Reactor at Arco, Idaho. Complete chemical and nuclear instrumental laboratories are being constructed for us on a subcontract and mentioned is the application to Whitney and Long Range Detection. III. Applied 1. Whitney chemistry program (K. Street) and 2. Long-range detection of foreign effort (A. Ghiorso). Some recent results of our research include 1. Interesting New Heavy Isotopes (Cm^{245} and Cf^{248} - still classified work by E. K. Hulet, A. Ghiorso, and S. G. Thompson); 2. Spontaneous Fission of Cf^{246} (predicted spontaneous fission half-life of 2,000 years for Cf^{246} was verified experimentally within the error of the experiment in still classified work by E. K. Hulet, S. G. Thompson, and A. Ghiorso); 3. Activation Energy for Fission (my unclassified report UCRL-1935); 4. Alpha-Particle Spectroscopy (extensive work by F. Asaro and I. Perlman covering the whole heavy element region); 5. Occurrence of astatine in the Actinium Decay Series (work by E. K. Hyde and F. F. Momyer on the rare alpha branching of 23-minute Fr^{223} has been proved by the isolation of a new astatine isotope of 0.9-minute half-life, which proved to be At^{219} , a member of the actinium series; also found was a new 8-minute beta-emitting isotope of bismuth, Bi^{219}); 6. Magnetic Susceptibility Measurements (work by B. B. Cunningham on curium (III) compounds show large values matched only by its chemically homologous gadolinium (III) compounds).

In the afternoon Al Ghiorso, Luis Alvarez, and I went to Mira Vista for a round of golf (LWA-94, AG-90, GTS-93).

I returned to the lab for a while after the game and learned that my graduate student, Robert J. Carr, passed his prelims today with the comment that he should continue with his course work and do some teaching. His committee consisted of Isadore Perlman, Robert E. Connick, George C. Pimentel, Theodore Vermeulen, Jack M. Peterson, and Raymond H. Sciobereti.

At 6:30 p.m. I attended my first meeting of UC's Athletic Advisory Board in the French Room of the Hotel Durant. We discussed the Pacific Coast Intercollegiate Athletic Conference meeting, which I will attend as incoming Faculty Athletic Representative, to be held next week in Pasadena, and I received an invitation to attend a dinner given by the Pasadena Tournament of Roses next Tuesday evening. I was also handed a letter from Jim Ross (President, Big "C" Society) inviting me to attend the Society's initiation ceremony on December 11, 1952 at 7:15 p.m. at the campus cafeteria. Unfortunately, I had to say that I wouldn't be able to make it.

Thursday, December 4, 1952

This "first of the month" research group meeting was attended by Asaro, Carniglia, Carr, Clark, Cobble, Glass, Gunn, Grover, Hoff, Hollandér, Jaffe, Kalkstein, Kofstad, Lessler, Levy, Michel, Nervik, Passell, Perlman, Raby, Rasmussen, Ruben, Seaborg, and Stephens.

Rasmussen described the instrument they are using at the present time for fast coincidence counting; this incorporates one of Don Martin's 10-channel pulse analyzers. They have used it for alpha-gamma and beta-gamma coincidence measurements. Then Kalkstein talked about their work on Am^{241} using this instrument. He referred to work by J. K. Beling, J. O. Newton, and B. Rose, who made use of Asaro and Perlman's energy level scheme. For the 26 and 60 keV gamma rays, they got delay half-lives of 6×10^{-8} seconds for both, indicating they are both coming from the same level. In response to a question from Perlman, Kalkstein said they ran a mixture of 30% Am^{241} and 70% Am^{243} and found that the tail had a half-life of about 6×10^{-8} seconds but the majority of the gamma rays were not delayed. There was quite a bit of discussion about the work.

Asaro gave a lengthy report on their work on the decay schemes of Np^{239} and Cm^{243} , which give the level scheme for their common daughter, Pu^{239} . He went into considerable detail and said they find that the Am^{243} atom is 4 keV lighter than the Cm^{243} atom. He reported that the delayed gamma rays of Np^{239} are at the 277 keV level (in Pu^{239}) and the proportions of the Cm^{243} alpha groups that decay to the 325 keV level are (13%), 376 keV (81%), and 67 keV (6%). Asaro also announced that they are trying to do alpha-gamma coincidence measurements on the spectrograph and then described the instrumental setup. I asked if he thought it is possible to add any energy to the decay scheme of Np^{237} , and Asaro said it was quite possible. I also asked about an extra gamma ray in the Pu^{241} decay, and Asaro referred to the work of Freedman, Wagner, and Engelkemeir (Argonne) who found a 145 keV gamma ray in Pu^{241} , which is probably part of the alpha decay scheme. He said they plan to run a sample in which the plutonium 242, 241, 240, and 239 ratios are varied in order to establish the ownership of their peaks more definitely.

* * * * *

Iz gave the Chemistry 123 lecture today, again on nuclear fission and the chain reaction. He also announced a midterm for next Tuesday.

I checked in the office of the Department of Chemistry, where I gave the report on the program and research results of our group to Ken Pitzer, and then went up to the hill.

Last evening I was given an invitation to attend a dinner given by the Pasadena Tournament of Roses to honor the Pacific Coast Conference at the Annandale Country Club in Pasadena on Tuesday evening, December 9. The first letter I dictated today was an acceptance note.

A couple of weeks ago (November 20) I received a letter from an old friend, Paul R. Saunders, whom I first met in Pahrump, Nevada during the summer of 1935 and who is at the School of Medicine of the University of

Southern California. Saunders asked if I knew whether C¹⁴-labelled compounds, such as glucose, pyruvic acid, etc., are available here. Doral sent copies of Saunders' letter down to Melvin Calvin and Richard M. Lemmon, and I asked Lemmon to respond. I looked over a copy of Lemmon's response and then wrote Paul that I believe, if Lemmon's letter does not meet his need, he should again write to Professor Calvin.

Donald A. McPherson (Row, Peterson & Co.) wrote on November 28 to say that he is planning a trip to California for December 10 to 12 and would like to have lunch with me. He asked me to return the enclosed carbon of his letter with a note as to my availability; this I returned today with "Unfortunately, I shall be in Los Angeles on the days you plan to visit. Perhaps there will be another time in the not too distant future."

In reply to Ron Brightsen's letter of November 25, I wrote that I hadn't realized that he hadn't received his Ph.D. yet for I had the impression that he had finished his graduate work with Charles Coryell. I said that we would be delighted to have him come to do his graduate work toward the Ph.D. degree with us, but he may find it impossible from the financial point of view since we have a rule that all graduate students are paid on the same basis no matter what their previous experience is. In his case we might be able to start him at the third year rate, which is \$302 per month full-time during the summer months and \$151 on the half-time basis during the school year. I mentioned that there are various predoctoral fellowships available on the national scale, such as the AEC fellowships, but I don't know whether these amount to more or not. If he feels he can come under these conditions, I wrote, the first step is to get admitted to the Graduate School by sending a transcript of his record to Dean K. S. Pitzer of the College of Chemistry here.

Iz and I received copies of a couple of very interesting, secret telegrams from James G. Beckerley (Office of Classification, AEC, Washington) addressed to such people as R. K. Wakerling, Hoylande D. Young (Argonne), and J. M. B. Kellogg (Los Alamos):

WE ARE TRYING TO WITHHOLD PUBLICATION OF FURTHER INFORMATION ON U-240 UNTIL WE HAVE HAD AN OPPORTUNITY TO REVIEW ENTIRE FIELD OF ISOTOPES IMPORTANT TO LRD WORK. IT IS OUR UNDERSTANDING THAT PERLMAN IS PREPARING FOR PUBLICATION REVISED UCRL-1928 WITH SOME ADDITIONAL DATA ON THE ISOTOPE. REQUEST THAT ANY NEW DATA BE WITHHELD FROM PUBLICATION UNTIL FURTHER NOTICE EVEN THOUGH GUIDE PERMITS DECLASSIFICATION. WE EXPECT TO REVIEW HEAVY ISOTOPES SITUATION AT JANUARY SENIOR REVIEWER MEETING AND ISSUE BULLETIN CLARIFYING POSITION.

RADIOCHEMICAL DATA ON RECENT ENIWETOK TEST INDICATES PRESENCE OF SOME UNIQUE HEAVY ELEMENT ISOTOPES SUCH AS PU-244. WE DO NOT WANT TO RELEASE ANY INFORMATION ON THE PROPERTIES OF THESE ISOTOPES, NOT EVEN THEIR EXISTENCE, AT THIS TIME EVEN THOUGH INFORMATION IS DECLASSIFIABLE UNDER GUIDE. ACCORDINGLY YOU ARE REQUESTED TO WITHHOLD PUBLICATION OF ANY INFORMATION ON THE EXISTENCE AND PROPERTIES OF ISOTOPES PRESENT IN DEBRIS SAMPLES AND CONSIDER SUCH INFORMATION AS SECRET, RESTRICTED DATA. THIS PROHIBITION APPLIES EVEN WHEN INFORMATION IS DISSOCIATED FROM TEST. PLEASE INFORM THOSE

IN LABORATORY WHO MIGHT HAVE ACCESS TO THESE DATA.

This is exciting news indeed! In discussions with Al Ghiorso and Stan Thompson, we deduced that Mark Inghram (Argonne) had been able to observe Pu^{244} in the plutonium fraction of Mike (a thermonuclear detonation) debris, which had been worked up by Sherman Fried's group there at Argonne. [See frontispiece.]

Friday, December 5, 1952

There were the usual number of phone calls and some serious conversations with Stan and Al about yesterday's telegrams before I tackled my correspondence.

On December 2 Charles Coryell wrote that there will be an opening at the instructor level or assistant professor level in inorganic chemistry at MIT within the next semester or so. Coryell wrote that they would not especially be interested in a man who is primarily a nuclear chemist and secondarily an inorganic chemist although the inorganic chemistry of the heavy elements is one area that would be promising. He said that he has also written to Connick about the position. On other matters Coryell wrote that he understands that Glendenin and Steinberg are finishing up some repeat work on the yield-mass curve in the spontaneous fission of Cm^{242} and that he would like a copy of their data if available as a UCRL report. Coryell also said that Arthur Fairhall has promised to send the data he promised Hollander within a week. In my response today I said that I will talk with Connick and Latimer about men for academic rank at MIT, explaining that we now have undertaken a project where some of our own output is being hired and that we are especially short of inorganic men. I also mentioned the letter I received from Ron Brightsen and said we would be glad to have him but I am not sure whether we can pay him as much as he may need in view of the salary level to which he has become accustomed. I explained that we have pretty stringent rules that place graduate students pretty much on the same salary level.

I also answered Don Stewart's (Argonne) letter of November 28, in which he inquired about possible men from our Ph.D. group. I explained that our new project is hiring much of our output so that the prospects are not so good as usual. I did mention Harris B. Levy, who will get his degree with Professor Perlman next summer and whom we think is a good man.

On November 28 Mademoiselle Yvette Cauchois mailed me a report presented at Societe de Physique, which provoked heated objections from M. Haissinsky. She asked for my opinion in order to help orient their research. I wrote that I think her result concerning the proximity of the 5f and 6d levels in the region of thorium and uranium seems extremely reasonable. I explained that I doubt I am well enough acquainted with this field of research to make sensible suggestions, but I did offer a few ideas. I then wrote:

I cannot understand why M. Haissinsky should want to take exception to the expected result that the 5f and 6d levels have closely similar energies in elements like thorium and uranium. I should think that this must be conceded no matter how one differs in the matter of how best to describe the chemical properties. The actual assignment of

the electrons is probably different depending upon whether one has in mind the gaseous atom, the metallic state, or the various chemical compounds. For the gaseous atoms, spectrographic work suggests the configuration $5f^36ds^2$ for uranium but the ground configurations for protactinium and thorium are indefinite due to the proximity of the 5f and 6d levels. My own views are summarized in Table 5 of the enclosed reprint where you will notice I do not suggest occupation of the 5f level in the ground state of gaseous thorium as my first choice.

The position of these elements in the periodic system is an interesting related problem and it is here where Haissinsky seems to differ most definitely with my views. I suggest that the gradation toward rare earthlike properties is entirely uniform in proceeding from actinium to curium and that there is no point of discontinuity so that one can sensibly segregate some of the elements from the others. Of course the elements thorium, protactinium, and uranium, where the 6d level is in a position to be important, have chemical properties similar to hafnium, tantalum, and tungsten but the former also have chemical properties similar to the rare earths. Thus Th(IV), Pa(IV), and U(IV) are similar in their chemical properties to Ce(IV) while U(III) is very similar to the typical rare earth of oxidation state (III). When everything is taken into account it seems to me that the best position for these elements in the periodic table is as members of the 5f transition group (actinide elements) in a position analogous to the 4f transition group (lanthanide elements).

I enclosed a copy of my Ohio State lecture, "Nuclear Thermodynamics of the Heaviest Elements," with my letter.

In the mail that arrived this morning was a copy of a letter from Spof English to Doyle L. Northrup about the isotopes from in Mike debris. English included a copy of the telegram from Ralph Carlisle Smith (Los Alamos) to James G. Beckerley (AEC, Washington) and dated December 2:

AT THE REQUEST OF LOS ALAMOS, A SAMPLE OF MIKE SHOT HAS BEEN ANALYZED AND FOUND TO CONTAIN PLUTONIUM TWO FOUR FOUR. OTHER HEAVY ELEMENT UNIQUE ISOTOPES ARE ALSO PRESENT IN THE SAMPLES. THIS INFORMATION INDICATES AN ESPECIALLY HIGH NEUTRON FLUX, WHICH REVEALS SOMETHING OF NATURE OF DEVICE DETONATED. SAMPLES OF THE BOMB DEBRIS HAVE BEEN MADE AVAILABLE TO ARGONNE, LIVERMORE, AND AFOAT-1 ORGANIZATIONS. THEREFORE BRADBURY, KELLOGG, GRAVES, AND I RECOMMEND THAT YOU ADVISE ALL ORGANIZATIONS INCLUDING JOINT TASK FORCES THAT ALL ANALYSES OF THE DEBRIS BE CONSIDERED PART OF THE IVY REPORT SERIES AND GRADED SECRET RESTRICTED DATA. FURTHERMORE ADVISE ALL CONCERNED THAT THE EXISTENCE OF PLUTONIUM TWO FOUR FOUR IS CONSIDERED SECRET RESTRICTED DATA AT THIS TIME IN ORDER TO AVOID AN INJUDICIOUS ANNOUNCEMENT BY ORGANIZATIONS WHICH MIGHT ACQUIRE SAMPLES. IF THE SOURCE OF THE SAMPLE WERE NOT UNIQUE, IT IS UNDERSTOOD THAT THIS TYPE OF INFORMATION IS DECLASSIFIABLE. PAST EXPERIENCE SHOWS THAT SOME ORGANIZATIONS FAIL TO DISTINGUISH BETWEEN DECLASSIFICABLE AND OPEN FIELDS, HENCE OUR REQUEST FOR THE ADVISORY COMMUNICATION.

I immediately showed this to Al and Stan. This reaffirmed our

decision this morning to look for transcalifornium elements in Mike debris. We plan to try to get some of the filter paper from the Whitney group to use as source material. If Pu^{244} is present in large yield, this means an unexpectedly large number of neutrons are captured in successive neutron absorption reactions; this process might proceed to such high mass numbers that detectable quantities of a transcalifornium element or elements are present in debris.

Later I signed a letter addressed to the Secretary, Committee on Fellowship Awards, American Association of University Women, in Vera Kistiakowsky Fischer's behalf. This was similar to letters I have written in the past to support Vera's applications for fellowships.

Earl prepared a confidential rating form for Louis M. Slater, addressed to Employment Supervisor, Shell Development Company, Emeryville, California, which I signed and mailed today.

During the last couple of days Iz and I prepared questions for a Chemistry 123 midterm (to be given next Thursday), which Millie Davis typed. I looked it over this afternoon.

Saturday, December 6, 1952

Stan Thompson and Gary Higgins are preparing to do chemistry on a filter paper from a B29 that flew through the debris cloud from the Mike explosion on October 31 and collected products produced by the multiple capture of neutrons in the uranium present. They hope to get this through Ken Street's Whitney group at Livermore who in turn will try to obtain it from Rod Spence in Los Alamos.

Since I will be out of town next week, I spent much of the afternoon with the kids.

Later I took the night train to Los Angeles with members of UC's Athletic Department and Stanley Freeborn (outgoing Faculty Athletic Representative) for a meeting of the Pacific Coast Intercollegiate Athletic Conference at the Huntington Hotel in Pasadena.

Sunday, December 7, 1952

In Pasadena. The Pacific Coast Intercollegiate Athletic Conference was founded on December 2, 1915 in Portland by representatives of the University of California, the University of Oregon, Oregon State College, and the University of Washington. A year later Stanford University and Washington State College were accepted into the Conference. The University of Idaho and the University of Southern California were admitted in 1922; in 1924 Montana State University was added, and in 1927 UCLA became a member. The office of Commissioner was established in 1940 with Edwin N. Atherton appointed to the position. Victor O. Schmidt, the present Commissioner, succeeded Atherton on the latter's death in 1944. The duties of the Commissioner include enforcement and interpretation of the Conference Athletic Code, the conducting of Conference business between the semi-annual meetings of the Conference representatives, and the appointment of football and basketball officials for Conference games. There are now nine member schools--Montana State University

resigned in 1950 to join the Mountain States Conference.

The first session was held at 2:00 p.m., called to order by the President, Dean Orlando John Hollis. Present were Freeborn, Seaborg, Bjork, Hollis, Holcomb, Everest, Moore, and Commissioner Victor O. Schmidt. At this session Hugh C. Willett was elected as Secretary for the current meeting (John B. Hurlbut of Stanford recently resigned). This session then adjourned.

The second session began at 8:10 p.m. and was a joint session with Faculty Representatives, Directors, and Football Coaches: University of California--Dean Stanley B. Freeborn (Faculty Athletic Representative), Brutus Hamilton (Athletic Director), Greg Engelhard (Assistant Athletic Director), Lynn Waldorf (Football Coach); University of California at Los Angeles--Professor David K. Bjork (F), Wilbur C. Johns (D), Henry Sanders (C); University of Idaho--Dean T. S. Kerr (F), Gale L. Mix (D), Raymond Curfman (C); University of Oregon--Dean Orlando John Hollis (F), Leo A. Harris (D), Leonard Casanova (C); Oregon State College--Professor Glenn W. Holcomb (F), R. S. Keene (D), LaVerne Taylor (C); University of Southern California--Professor Hugh C. Willett (F), Willis O. Hunter (D), Jesse Hill (C); Stanford University--Professor Rixford Snyder (F), Alfred R. Masters (D), Charles Taylor (C); University of Washington--Professor H. P. Everest (F), Harvey Cassill (D), Howard Odell (C); State College of Washington--Professor Emmett Moore (F), Dean Golden Romney (D), Alton Kircher (C).

At this session the new Faculty Athletic Representatives (Holcomb and Snyder) were introduced, as well as the prospective ones (Glenn T. Seaborg, University of California, succeeding Freeborn on January 1, 1953 and Claude Jones, University of California at Los Angeles, succeeding Bjork on February 1, 1953). In addition, Lathrop K. Leishman (Chairman, Tournament of Roses Football Committee), a Committee member John Biggar, and President of the Tournament of Roses Association William Nicholas were also introduced. We heard reports from a number of committees, the minutes of the last meeting, etc.

Later I heard that the Pittsburgh Steelers shut down the San Francisco 49ers, 24 to 7 today. (I was scheduled to go to this game with Kalkstein and Hollander.) The 49ers failed to score after the first quarter. Frankie Albert threw five interceptions, and the net rushing gain for the 49ers in the second half was -15 yards. Gordon Soltau was injured late in the second half and was removed from the game with a sprained ankle.

Monday, December 8, 1952

In Pasadena. The third session of the Faculty Athletic Representatives began at 9:30 a.m. at which time consideration was given to student petitions and institutional violations. A telegram from President Sproul about the admission of students to the University of California and the University of California at Los Angeles by "principal's recommendation" was read. We adjourned at 11:35 a.m.

The fourth session began at 11:45 a.m., attended by the Faculty Representatives and the Athletic Directors. Final action was taken on a

number of special petitions, including granting the petition of UC student Walter Briant who asked for an exception to the "academic progress rule," granting the petition of UC student Steve Dimeff that his competition for 2 1/2 minutes in football in 1952 be disregarded, and granting the petition of UC student Gerald C. Hays that he not be declared ineligible because of his acceptance of two fees of \$1.00 each for officiating basketball games while a high school student. The Representatives reaffirmed its approval of the ruling of the Commissioner that the Stanford-Shrine Game scrimmage on December 22, 1951, was in violation of Conference rules; that a record be made in this minute of President Sterling's decision not to take disciplinary action for the reasons given in his letter of December 1, 1952; and that the case be closed. It was also moved, seconded, and carried unanimously that, on the basis of information from President Sproul that the admission of students to UC and UCLA on "principal's recommendation" was to be discontinued, the report of President Sproul be accepted and the case closed. This session adjourned at 12:25 p.m.

The fifth session began at 2:05 p.m., and institutional violations were again discussed. We adjourned at 4:00 p.m. and reconvened at 4:10 p.m. with the Directors in attendance. Final action was taken on a number of matters, including the motion that the Faculty Representatives approve the recommendation of the Directors that the mimeographed copy of the Directors Code as circulated and as now amended be adopted. This sixth session adjourned at 4:55 p.m.

At 5:00 p.m., there was a seventh session with the Faculty Representatives, Directors, and Football Coaches at which recruiting problems and related matters were discussed. This continued until 6 p.m.

There was a social hour for Faculty Athletic Representatives during the early evening--the Vat 69 Club--and I was given the formula for the beverage:

2 bottles Vat 69
1 bottle bourbon
2 qts soda
2 qts ginger ale
plenty of ice

This is the makings for eight old fashioned for 10 people. Snacks, approximately eight servings per person, should also be provided.

[In Berkeley much discussion was taking place about the significance of Friday's telegram, and Thompson was able to confirm by inference, in a telephone conversation with Mark Inghram about another matter, that our speculation last week was correct. The men also realized that, if the high yields for capture of successive neutrons continued to hold up to much higher masses, it would be possible to find isotopes of transcalifornium elements. Ken Street has agreed to give us a piece of filter paper (collected by a B29 and obtained from Rod Spence in Los Alamos).]

Tuesday, December 9, 1952

In Pasadena. The eighth session of this meeting began at 9:30 a.m. and was attended by the Faculty Representatives, during which Commissioner Schmidt continued reporting on cases relative to alumni activity (begun yesterday afternoon). This session adjourned at 11:30 a.m. to reconvene at 11:35 a.m. with the Directors. Final action was taken on some more cases. It was also moved, seconded, and unanimously carried that the Commissioner shall be the judge as to what matters, including Code violations, are of sufficient importance for reference to the Conference, and what matters of less importance can be satisfactorily adjusted by direct action between the Commissioner and the member institution involved; and further, that the Commissioner shall consider it his duty, on the occasion of his visit to the campus of member institutions, to bring to the attention of the director and faculty representative any matter which in his judgment indicates an institution's non-conformance with the provisions of the Athletic Code. We adjourned at 12:20 p.m.

At 2 p.m. the Faculty Representatives reconvened to discuss a number of matters, including termination dates for freshman football practice and basketball practice. This session was over at 3:45, but we reconvened at 4:05 p.m. with the Directors, when final action was taken on several items. A motion allowing Oregon State College to use freshmen on the varsity football team in 1953 lost. The Code was amended to read "No freshmen shall participate in football practice before the next to the last Saturday in September or after the second Saturday before the last Thursday in November" and "The opening date of the basketball practice season shall be the third Monday in October, and the closing date shall be the date of the last scheduled basketball game of the season." It was decided that the Pacific Coast Conference Track Meet be held at Stanford on May 29-30, 1953. The final activity of the afternoon was a report by Mr. Hal Deal (Tide Water Associated Oil Company) on the radio program of the 1952 football season and plans for the coming basketball season.

During the evening I attended the dinner to honor the Pacific Coast Conference given by the Pasadena Tournament of Roses Association at the Annandale Country Club.

Wednesday, December 10, 1952

In Pasadena. The Faculty Representatives met from 9:35 a.m. until 11:30 a.m. to consider a number of items. Then from 11:40 a.m. until 11:30 a.m. they met, along with the Directors. It was moved, seconded, and carried (7 for, 0 against, 2 abstaining) that at a joint session the coaches be requested to give their individual reports on the discussion in the Coaches Committee meeting of violations of playing rules during the last football season. Also, final action was taken on a number of items.

At the 2 p.m. session the coaches (except for the University of Washington) expressed their views on the subject of violations of playing rules during the 1952 season. This session was over at 2:40 p.m. At 2:40 p.m. the Faculty Representatives heard the Commissioner read a

letter from Mr. Walter Byers, Executive Director of the NCAA in which the Council of the NCAA invited the PCC to send three representatives to a meeting of similar representatives from the ten allied conferences in the NCAA to discuss common problems. (The Directors will nominate one of their own number as a representative.)

The sixteenth session went on from 4:15 until 6:15 p.m., during which time the Representatives accepted the invitation of the NCAA to send representatives to the meeting of the allied conferences on February 7-8, 1953. Among other things the Directors reported the election of officers for the coming year: President of the Directors Association--Leo Harris, Secretary of the Directors Association--Harvey Cassill, Member of Commissioner's Advisory Committee--Dean Romney. Wilbur Johns was nominated as Conference Treasurer. The Coaches elected Alton Kircher as Chairman of the Football Coaches Committee and LaVerne Taylor as Secretary. There were other reports and discussions before adjournment.

Thursday, December 11, 1952

In Pasadena. During the seventeenth session (9:25 a.m. until 11:40 a.m.) the following Conference officers nominated by the Nominations Committee were unanimously accepted: President--Dean Kerr, Secretary--Professor Moore, Treasurer--Mr. Johns, Comptroller--Professor Claude Jones (effective February 1, 1953), Member of Commissioner's Advisory Committee--Professor Snyder. At the eighteenth session (11:45 a.m. until 12:20 a.m.), a joint meeting with the Directors, it was moved, seconded, and carried that upon the adjournment of the NCAA Convention next month (assuming no NCAA restrictive legislation) the Commissioner propose to the Pasadena Tournament of Roses and the Big Ten Commissioner a renewal of existing agreements with each of them. A motion also carried that, starting in 1956, the restriction to nine football games per year be rescinded and that a return to the ten-game schedule be authorized. At the 2:30 p.m. nineteenth session, which was a joint meeting of the Faculty Representatives and the Directors, a motion carried that the Directors be authorized to formulate a proposed PCC plan for the television of the 1953 football games and to present the plan to the NCAA TV Committee or other appropriate NCAA organization; and that the Directors be permitted to release the proposed plan to the press. A motion also carried that, except for live television of football games, the Conference TV policy for 1953-54 be the same as that for 1952-53. The Winter 1952 Meeting of the Conference adjourned "sine die" at 3:00 p.m.

I then took a taxi to my parents' home in South Gate to visit with them for a while. Although they were interested in my new position, they seemed more anxious to hear all about their grandchildren and Helen. I convinced them to come up and spend the holidays with us. Later I caught the night train back to Berkeley.

[In Berkeley Stan and Al are working up a piece of filter paper that Bill Crane of Ken Street's Whitney group at Livermore had obtained from Rod Spence at Los Alamos. This is about half of a filter collected by a B29 from the Mike test. They speculate that this may contain isotopes of transcalifornium elements.

The research group met as usual this morning with the following people in attendance: Carr, Cobble, Feay, Gunn, Grover, Hollander, Hulet, Jaffe, Levy, Nervik, Passell, Perlman, Rasmussen, Slater, Stephens, and Templeton.

Cobble spoke extensively on his interest in the entropy of ions in aqueous solution, particularly noting the recent work of W. M. Latimer and Richard E. Powell on the entropy of simple ions. He spoke of his study of complex ions, stating that he chose oxygenated anions for his study because most data is available for these ions. His conclusion was that the equation he evolved agreed within 10% of that of Powell and Robert E. Connick.

Hulet reported that they learned the pulse analyzer was not linear above about 6.0 Mev and that they therefore have a new value for the alpha particle energy of Cf^{248} of 6.24 ± 0.02 Mev. He said that, as a consequence, the mass assignment is no longer certain. In response to a question by Perlman, Hulet said they are going to try to measure the spontaneous fission rate, which Ghiorso predicts to be one count every 2-1/2 days, assuming that the activity is due to Cf^{248} . Perlman added that that will be greater by a factor of 10 or 100 than if it is Cf^{247} .

* * * * *

Perlman gave the Chemistry 123 class the following midterm.

Chemistry 123
Second Midterm Examination
December 11, 1952

1. (20)

Sketch the shape of the E_α vs. A curve for polonium and radium over the range of known mass numbers (that is, for $\text{Po}^{206-218}$, $\text{Ra}^{220-226}$) and discuss the differences between the two.

2. (20)

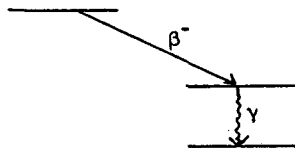
An alpha particle of velocity (v) has a range of 10 mg/cm^2 in aluminum. What would be the range of an H^3 atom of the same velocity in (a) aluminum, and (b) gold?

3. (15)

Why should D_2O be better than H_2O as an agent for slowing fission neutrons to thermal energies in a nuclear reactor?

4. (25)

The decay scheme for a particular isotope of a low Z element is as follows:



The maximum beta energy is 1.2 Mev, the gamma ray transition is 200 kev and it is 50% converted. Sketch the aluminum absorption curve which you would expect if a sample of the radioactivity is counted with a Geiger-Müller counter (assume that all electrons are counted with 100% efficiency and gamma rays with 1% efficiency). Show all components of the absorption curve, label the axes and indicate all pertinent data on the curve. (Use absorption data in Friedlander and Kennedy.)

5. (20)

Calculate the amount of (a) Cu^{64} and (b) Cu^{66} activity (in millicuries) produced in a 10-minute bombardment of natural copper (100 mg/cm^2 thickness, $10 \text{ cm} \times 10 \text{ cm}$ area) with 100 microamperes of 10 Mev deuterons. The reactions are $\text{Cu}^{63}(\text{d}, \text{p})\text{Cu}^{64}$ and $\text{Cu}^{65}(\text{d}, \text{p})\text{Cu}^{66}$ for which the cross section is 0.2 barn in each case.]

Friday, December 12, 1952

One of the first things I did this morning was to check with Stan Thompson, who told me about progress on the filter paper sample that he and Gary Higgins are processing to isolate elements above plutonium. He is assuming that the chemical properties of the elements immediately beyond californium can be accurately predicted by the actinide concept.

I learned that the clearance for Bernard Harvey has finally come through, so I telephoned him in Chalk River with the news. We discussed salary, and I suggested a salary of \$650 to \$700 per month plus moving expenses, which was agreeable with him. [Later, when I called Everson's office, I suggested that they offer Harvey \$750 per month.] I told Harvey that he should give first consideration to good relations between the two laboratories in determining his moving time, noting that it will take at least three months to get his visa anyway. Harvey seemed somewhat distracted apparently by some laboratory activity.

In looking over the mail that had arrived during my absence, I noted a letter from E. Wiberg (Munich), thanking me for sending him reprints of our work.

In a December 3 letter Erling Kofstad wrote from Oslo and asked for a report on his son, Per, such as the progress of his studies, his behaviour, and other things of interest. I asked Earl to compose an answer.

Much of the rest of my day was spent with the men, particularly Stan and Gary Higgins, who are working on the Mike debris sample.

Saturday, December 13, 1952

I checked with Al Ghiorso about their experiment and learned that, in a 2% aliquot of the actinide element fractions from their second Dowex 50--13 M HCl column containing elements 95 and those with higher atomic numbers, they saw, along with the alpha particles of Am^{241} , Cm^{242} , and Am^{243} , previously unobserved alpha particles of energy about 6.6 Mev.

Sunday, December 14, 1952

I again checked with Stan and Al about their continued experiments, which indicate on the basis of a separation on a Dowex-50 column with elution by citrate (Mike Citrate I), that they have a transcalifornium activity with 6.6 Mev alpha particles and spontaneous fission activity.

Kalkstein and Hollander went with me to see the 49ers beat the Green Bay Packers in the final game of the season by a score of 24 to 14. Gordon Soltau, despite his ankle injury, kicked one field goal and three conversions and became the league's leading scorer. Joe Perry rushed for 109 yards and one touchdown.

Monday, December 15, 1952

I talked with Al and Stan. One of the most important items under

discussion today was their work on what we believe is a new transcalifornium element. Al, in particular, believes this is due to an isotope of element 100, based on their observation of spontaneous fission activity. It was concluded that it is advisable to repeat the experiments, and Ken Street is arranging for a sample of heavy element fraction from Lloyd Zumwalt of Tracerlab in Berkeley. The men are also preparing a sample of 1.5-day Cf^{246} in the 60-inch cyclotron to use as tracer.

I then took care of some administrative matters, including signing an evaluation, which Earl prepared, of Barun Chandra Haldar for The Royal Institute of Chemistry (L. W. Winder, Assistant Registrar) in London in connection with his application for election to their Fellowship. This was similar to other letters I have written for Haldar, who did research with us from October 1950 to June 1951.

G. Curtis Pritchard telephoned me about the John Scott Award. He said that it has been suggested that I be given the award at a meeting of the local ACS chapter in Philadelphia and then address the group. I agreed, providing we can arrange a suitable date; he suggested February 19, 1953, and I said that I will find out the date of my next trip to Washington (IO-7 Panel) and call him at noon on Wednesday. Pritchard gave me the name of a contact with the Philadelphia ACS Section--John W. Iliff of du Pont in Philadelphia--and explained that the Board of Directors of City Trusts of the City of Philadelphia has difficulty in meeting any expenses beyond the medal, scroll, and \$1,000 premium; he understands that the local ACS people have a small sum available for travel expenses incurred in coming from New York or Washington.

At 10 a.m. I met with Eugene Burdick of the Chancellor's staff to report on the PCC meeting in Pasadena. On other athletic matters connected with the PCC meeting, I telephoned George Schroth (swimming coach) about some scheduling problems.

After my meeting with Burdick, I drove to San Jose for a luncheon meeting at the Hotel St. Claire (Market and San Carlos Sts.) of the judges panel for the California State Junior Chamber of Commerce Distinguished Service Awards. Bob Mathias, the Decathlon title holder and a member of Stanford's football team, was chosen for the athletic award.

I returned to the lab before going home and saw that Stan is working up the heavy element fraction received from Lloyd Zumwalt; this is in the form of a lanthanum fluoride precipitate derived from a B29 filter that had been flown through the debris cloud of the Mike explosion.

Tuesday, December 16, 1952

This morning I tried unsuccessfully to reach Doyle Northrup to find out the date of the next IO-7 Panel meeting.

I looked in on Stan, who is beginning the elution of the sample obtained from Tracerlab.

Iz. gave the Chemistry 123 lecture today; he returned and explained

the midterm and then began lecturing on the preparation and identification of isotopes.

In today's mail was a December 11 letter from Dwight Conway (now at the University of Chicago), who asked that I complete a National Science Foundation Report for him. I routed this to Earl to handle.

In the afternoon I played golf at Mira Vista with Dan Wilkes (DW-83, GTS-90). I stopped in the lab to talk with Al and Stan before going home. They plan to pulse analyze their new sample tomorrow. I suggested that that these experiments be written up in great detail.

Wednesday, December 17, 1952

This morning I reached Doyle Northrup by phone and learned that February 19 (the date Pritchard suggested for the awarding of the Scott Medal) will possibly be compatible with the IO-7 meeting, scheduled for February 17 to 19.

After checking with Stan and Al, I took part in a 10 a.m. filming for "Science in Action."

Later, after talking with Stan and Gary who are now eluting with citrate from Dowex-50 resin (Mike TL Citrate), I took care of some of my correspondence.

I accepted President Sproul's December 15 invitation to deliver the Charter Anniversary Address at the Riverside campus on Tuesday evening, March 24, 1953.

I also answered Frank R. Ward's letter of December 11, in which he reminded me of my offer to write a guest editorial for Reactor Science and Technology. I said that I recall my commitment to write a guest editorial and would like to suggest the March issue, with the understanding that my editorial will reach him by February 1.

In today's mail was a letter from Truman Kohman, who said that he hasn't had a chance yet to send corrections and suggestions for the new "Table of Isotopes." He did ask that we change the unpublished reference for his work on the specific activity of radium to one in Volume 14B of the PPR. Truman, as usual, concluded with "I wonder whether you have considered the advisability of changing the name to "Table of Nuclides."

Al reported that the pulse analysis of Mike TL Citrate still shows about a count per minute of a new activity of 6.6 Mev, which elutes ahead of the tracer californium. This proves that a new element has been found. Al believes this is element 100 because of the spontaneous fission activity although Gary Higgins thinks the 6.6 Mev alpha activity could be due to element 99 on the basis of the spacing of its elution position ahead of californium (element 98).

Thursday, December 18, 1952

The group meeting this morning was attended by Asaro, Carniglia, Clark, Cobble, Cunningham, Dauben, Glass, Grover, Hoff, Hollander, Jaffe,

Kalkstein, Levy, Nervik, Passell, Perlman, Raby, Rasmussen, Seaborg, Slater, and Templeton.

Asaro reported that they had investigated the alpha spectrum of Fr^{212} to determine if it, like two other 125-neutron nuclides, contains more than one alpha group. They found three groups: 6.409 Mev (37%), 6.387 Mev (39%), and 6.330 Mev (24%). (The alpha pulse analyzer energy is 6.36 Mev.) Asaro went on to say the other nuclides possessing 125 neutrons whose alpha spectra have been measured are Em^{211} (two groups of comparable abundance separated by 69 kev) and At^{210} (three groups of comparable abundance spaced at 80 kev intervals). Po^{209} has no observable complex structure. He found no fine structure in Em^{208} , Em^{210} , Em^{212} , nor At^{211} .

Asaro also presented data from the gamma-ray spectrum of samples of curium (the ratio of Cm^{243} to Cm^{242} differed by a factor of 10 in the two samples). The results of the studies confirmed the previously proposed decay scheme for Cm^{243} . He said that the calculated alpha-particle energy for the ground state transition of Cm^{243} is 6.051 Mev and, since there is a Cm^{242} group at 6.066 Mev, a low abundance peak of Cm^{243} could not be resolved.

Carniglia spoke about their separation of neptunium from a shipment received from Hanford, which Hanford claimed contained 6.02 g (CR&D purified part of the shipment). Carniglia reported that their current figures indicate a yield of about 0.5 g of neptunium (CR&D recovered 1.4 g), making a total of 1.9 g neptunium. The total plutonium recovered appears to be about 30 g (Hanford reported the shipment contained 40 g). Carniglia also described the apparatus they are constructing to perform vapor pressure measurements on heavy element compounds.

* * * * *

On the hill I answered the December 11 letter from Edwin O. Wiig (The University of Rochester), who described a postdoctoral fellowship they have open. Wiig also mentioned some work a first-year graduate student has done on the spallation of tantalum and asked how far our work had progressed so that he can determine whether it is worthwhile for them to continue their efforts. I told Wiig that I am doubtful we will have any men available for a fellowship since we have undertaken a new big project for the AEC. I then went into some detail about our work and results on tantalum.

I also finally reached Pritchard and again went over the dates for the Scott Award. I told him that February 19 is possible, although probably not the best time for me. We went over the other possibilities (the Philadelphia ACS section meetings are held on the third Thursday of the month). He told me that January 15 is not available, April 16 is good, and May 21 is barely possible. I said that I will wire him tomorrow (after Doral checks travel possibilities from Washington).

I spent some time with Al and Stan going over the memorandum, addressed to Ken Street and me, on their recent experiments. It will be typed and sent out tomorrow.

Friday, December 19, 1952

Doral looked into the travel possibilities, and I concluded that I could be in Philadelphia for the John Scott Award on February 19 if there is only a half-day session of the IO-7 Panel on the 19th.

In today's mail was a 2-1/2 page report by G. Pyle, S. Fried, D. C. Hess, M. Inghram, and P. R. Fields, entitled "Beta Decays of a New Plutonium Activity and Its Probable Daughter," who describe the beta activity in a one microgram sample containing about 0.1% Pu²⁴⁴ that consisted of a 0.1-0.2 Mev component (which follows plutonium chemistry) and a 1.2 Mev component (follows americium chemistry). They described their reasoning that leads them to believe the pair to be Pu²⁴⁶ and Am²⁴⁶ although Pu²⁴⁵ and Am²⁴⁵ cannot be ruled out. They suggest reinvestigation of the original assignment to mass number 244 of the 25 minute, 1.2 Mev americium activity.

Stan and Al completed the following memorandum, in which they describe the chemical identification of the 6.6 Mev alpha activity as proof that it is due to a transcalifornium element, suggest tentative assignment to 100²⁵⁴ on the basis of the observed spontaneous fission activity, but state that the experiment in itself was not adequate to distinguish between element 99 and element 100.

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BERKELEY: RADIATION LABORATORY

December 19, 1952

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TO: Glenn T. Seaborg and K. Street, Jr.
 FROM: A. Ghiorso and S. G. Thompson
 RE: Mike Experiments

MB-IP No. 719
 This document consists of 5
 pages and 10 figures.
 No. 1 of 11 copies, Series A

The following report summarizes the results obtained to date by the Radiation Laboratory Chemistry Group on a sample of the debris collected just after the Ivy-Mike test in the Pacific on October 31, 1952. This report covers experiments done since December 11, 1952 which was the date on which our group first obtained samples. The work done at the Radiation Laboratory to date in this connection has been primarily concerned with the detection and estimation of the amounts of heavy isotopes produced particularly those of the elements above neptunium.

The first sample was obtained on December 11 from W. W. T. Crane and K. Street of the Radiation Laboratory Whitney Project. Dr. Crane had obtained the filter paper from Dr. R. W. Spence of Los Alamos. This sample was part of one of the early filters collected by means of jet aircraft. The paper was subjected to standard "wet ashing" procedures and the well known chemical separations designed to isolate the elements above plutonium. The chemical separations have been well described in many previous publications and will not be discussed here except in the following brief outline form.

- Wet ashing with HNO_3 and HClO_4 .
- KOH precipitation of $\text{La}(\text{OH})_3$.
- Fuming with HNO_3 and HF.
- Repeated precipitation of $\text{La}(\text{OH})_3$ with NH_4OH .
- LaF_3 precipitation.
- Dissolution of LaF_3 by fuming with HNO_3 .
- Passage through Dowex A-1 resin in 10 M HCl to remove plutonium and anionic impurities.
- Passage (twice) through Dowex 50 resin with 13 M HCl to remove rare earths.
- Separation of elements above plutonium using a Dowex 50 resin column with ammonium citrate at 87°C as the eluting agent.

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Classification changed to
 authority of D. J. Callahan 7/2/61
5/5/61
 Date
 Person making change D. J. Callahan

The eluants from the column runs were collected separately and the individual fractions on the platinum plates were subjected to alpha particle pulse analysis and in some cases to beta counting with a methane gas proportional counter. The results obtained on a 2% aliquot of the actinide element (95 plus) fractions from the second Dowex 50 - 13 M HCl column are shown in Figure 1. The alpha particles of a number of well known isotopes including

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Am²⁴¹, Cm²⁴², and Am²⁴³ were observed, together with previously unobserved particles of energy about 6.6 Mev.

An elution curve from the Dowex 50 - citrate column was obtained as shown in Figure 2. Unfortunately the column or resin was apparently accidentally contaminated with a small amount of Am²⁴¹; however, this did not unduly interfere with the most important aspects of the experiment. It can be clearly seen that there are four elution peaks. Three of these are due to activities from known elements--americium (95), curium (96), and californium (98). The fourth is due to a new element with atomic number 100.

Figure 3 is an alpha pulse analysis of the americium fraction. The alpha energies correspond to Am²⁴³ and Am²⁴¹. Am²⁴¹ presumably has come from the beta decay of 14 year Pu²⁴¹ during the 45 days since the production of the atomic debris. The 8,600 year Am²⁴³ would grow within hours through short-lived beta decaying ancestors from U²⁴³. The ratio of Am²⁴³ to Am²⁴¹ at this time after separation from plutonium, was 0.85 by activity corresponding to an atom ratio of 14. From these data one can calculate then that the ratio of U²⁴³ to U²⁴¹ produced was 0.1.

Figure 4 presents the alpha spectrum of the curium fraction. The alpha activity labeled "Cm²⁴²" has only been identified by its energy since sufficient time has not elapsed to measure its 162 day half-life. It can only be produced by neutron capture in small amounts of Am²⁴¹ present in the device because of Pu²⁴¹ beta decay in "aged" plutonium. Peak C corresponds in alpha energy (5.4 Mev) to our estimates for Cm²⁴⁶ and would probably come directly through intermediates from U²⁴⁶. We would predict its half-life to be ~3,000 years; thus the atom ratio of Cm²⁴² to Cm²⁴⁶ would be 6×10^{-4} if there is no holdup in the mass 246 chain. We also calculate the atom ratio of Cm²⁴⁶ to Am²⁴³ and thus U²⁴⁶ to U²⁴³ to be 0.03. It is notable that no 20 year Cm²⁴⁴ is detected; from the limit of detection one can set a lower limit on the beta half-life of Pu²⁴⁴ since the atom ratio in the sample of Pu²⁴⁴ to Pu²⁴¹ is known. This calculation indicates a value of greater than 1,000 years. Cm²⁴⁵ may also be present in the sample, but as its alpha energy is the same as that of Am²⁴¹ further chemical purification must be performed before identification can be assured.

Figure 5 shows the alpha spectrum of the californium fraction of the sample. Peak D corresponds in energy to Cm²⁴², but reference to the elution curve indicates that this is californium rather than curium. If this alpha peak is not due to Cm²⁴² contamination and is indeed Cf²⁵⁰ or Cf²⁵², then for this alpha energy its half-life would be estimated as ~10 years from alpha systematics. Its yield can then be calculated if we assume this half-life. As will be shown later, the use of yield arguments indicate that for a 10 year half-life a mass number of 252 rather than 250 would correlate more closely with the other data. On the other hand, reference to alpha systematics shows that an alpha energy of 6.1 Mev corresponds to Cf²⁵⁰ rather than Cf²⁵². Therefore, it is most reasonable to propose that the 6.1 Mev alpha particles arise from Cf²⁵⁰ whose yield is low because of a long-lived parent such as Cm²⁵⁰. The discrepancy in yield is explained if one postulates a beta half-life of about 5 years for Cm²⁵⁰. An alternative explanation, of course, is that Bk²⁵⁰ is long lived, but the former

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explanation seems more likely since it corresponds better with the predictions of heavy isotope beta stability systematics.

Figure 6 is a presentation of the alpha spectrum of the element 100 fraction. Peak A has an energy of 6.58 Mev and a half-life of very crudely 10-1 years. This half-life and energy correspond with predictions for even-even isotopes $Z = 100$ but if $Z = 98$, the half-life should be about three days. The most powerful evidence that the element assignment is correct, however, is that this sample exhibits a spontaneous fission rate of about one fission every 20 minutes! The same sample undergoes alpha decay at a rate only 40 times this value. Such a behaviour is exactly that expected for 100254 . If the yield of this activity is calculated assuming it has a 30 days half-life, the yield value corresponds closely to the empirical curve at mass 254 (Figure 8).

Figure 7 is a crude pulse analysis of the spontaneous fission distribution. The chamber used was the same grid chamber used for alpha spectra analysis and was not adjusted for the densely ionizing fission fragments, hence the relatively poor resolution.

In Figure 8 we have plotted the yields of the various nuclides relative to Pu^{239} as a function of mass number. The data for masses 240, 241, 242, and 244 are those obtained by Dr. M. Inghram at the Argonne National Laboratory. Our data for masses 243, 246, and 254 were calculated on the assumption that all of the Am^{241} observed came from beta decay of Pu^{241} during the 45 days that had elapsed since the explosion. It was also assumed that the amount of Pu^{241} , and thus Am^{241} , originally present before the event was small compared to that produced from beta decay of U^{241} . The yield data seems best fitted by two lines--one for even masses and one for the odd.

The plutonium fraction from this material was examined for Pu^{238} (5.48 Mev) alpha activity. It was found that 0.05% or less of the total plutonium alpha activity could be due to Pu^{238} . Correcting for the change in specific activity of the plutonium as determined from Inghram's mass analysis (~40% of the alpha activity should be due to Pu^{240}), we calculate the atom ratio of Pu^{238} to Pu^{239} to be equal to or less than 4×10^{-6} . When one corrects for the amounts of Pu^{239} produced from U^{238} neutron capture as compared to the Pu^{239} originally in the device (a factor of ~250), it is seen that the Pu^{238} content suggests that the original Pu^{239} atoms "saw" fewer high energy neutrons than the U^{238} .

After this preliminary work had been accomplished, it was deemed advisable to repeat the chemical experiments proving the chemical identity of the new element. We obtained a heavy element fraction in the form of a lanthanum fluoride precipitate which had been derived from a B29 filter by the Berkeley Tracerlab group under Dr. L. Zumwalt. Using the chemical procedures outlined for the first experiment, the actinide fractions were separated finally on a Dowex 50 - citrate column. Before the separations were undertaken, however, 1.5 day Cf^{246} tracer, obtained from carbon ion bombardment of U^{238} , was added

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to the material for the purpose of again ascertaining the existence of the new element. The results are shown in Figure 10 and illustrate the separation between californium and the new element. This experiment was not adequate in itself to differentiate between element 99 and element 100. We are, therefore, relying wholly for the moment on our recently acquired knowledge concerning spontaneous fission systematics to assign this highly spontaneously fissionable nuclide to an even-even isotope of element 100.

The americium fraction from a Dowex 50-citrate column separation was examined in a windowless methane proportional counter for possible beta radioactivity arising from beta emitting isotopes of americium. The absence of beta particles within the limits allowable for statistical fluctuations, together with the yield of nuclides of mass 245 estimated from the relationship shown in Figure 8, allows the calculation that the beta half-life of Am^{245} must be outside the limits two days to 3,000 years.

In view of the evidence that Pu^{244} is either beta stable or not more than slightly unstable towards beta decay, it appears likely that Pu^{246} could have a moderately long beta half-life. In this case, it could still be existent in the plutonium fraction and should maintain a short lived americium daughter in equilibrium with it. Experiments to confirm this possibility are in progress.

Experiments are continuing in an attempt to detect the alpha radioactivity of element 99. Although there is a good chance that these experiments will be successful, the detection is rendered difficult by the relatively long alpha half-life expected for 99253.

It should be mentioned that the properties expected for elements 101 and 102 are such that it should have been possible to detect their presence, if we had received samples of the debris a short time after its production.

In view of the tremendous store of information that investigations such as this might uncover we have deemed it worthwhile to include in this preliminary report an estimate of the heavier elements as regards to beta stability. Figure 10 is largely self-explanatory.

Among the many possible conclusions one can arrive at from these and other experiments are the following:

(1) Since there seems to be no sharp deviations in yield ratios as the mass number is increased, it seems unlikely that the neutron binding energies are decreasing rapidly enough to lower the capture cross sections.

(2) The consistency of yield ratios also suggests the absence of closed shell effects of any importance up to a neutron number of 162.

(3) A sensitive measurement of nvt for high flux devices is available through the measurement of yield ratios of some of the heavier isotopes (e.g. $\text{Cm}^{246}/\text{Pu}^{239}$).

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It is only as a result of our chemical studies of the heaviest elements during the past few years that enough knowledge concerning the expected chemical properties of the elements beyond californium has been acquired to permit the separation and identification of sub-microscopic amounts (approx. one count per minute) of element 100 in the experiments reported here.

Cordially yours,

A. Ghiorso

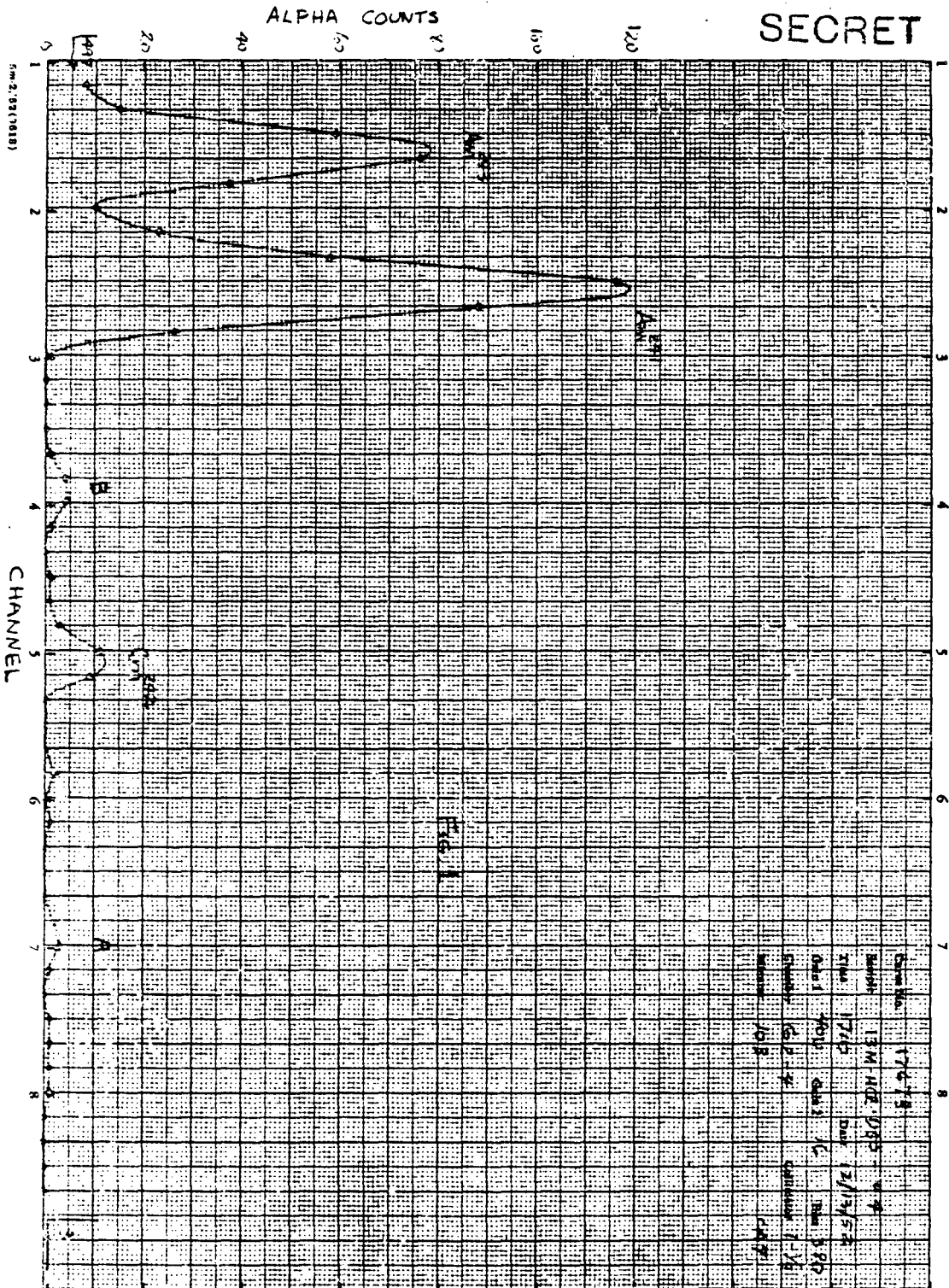
A. Ghiorso

S. G. Thompson

*S. G. Thompson***DECLASSIFIED**

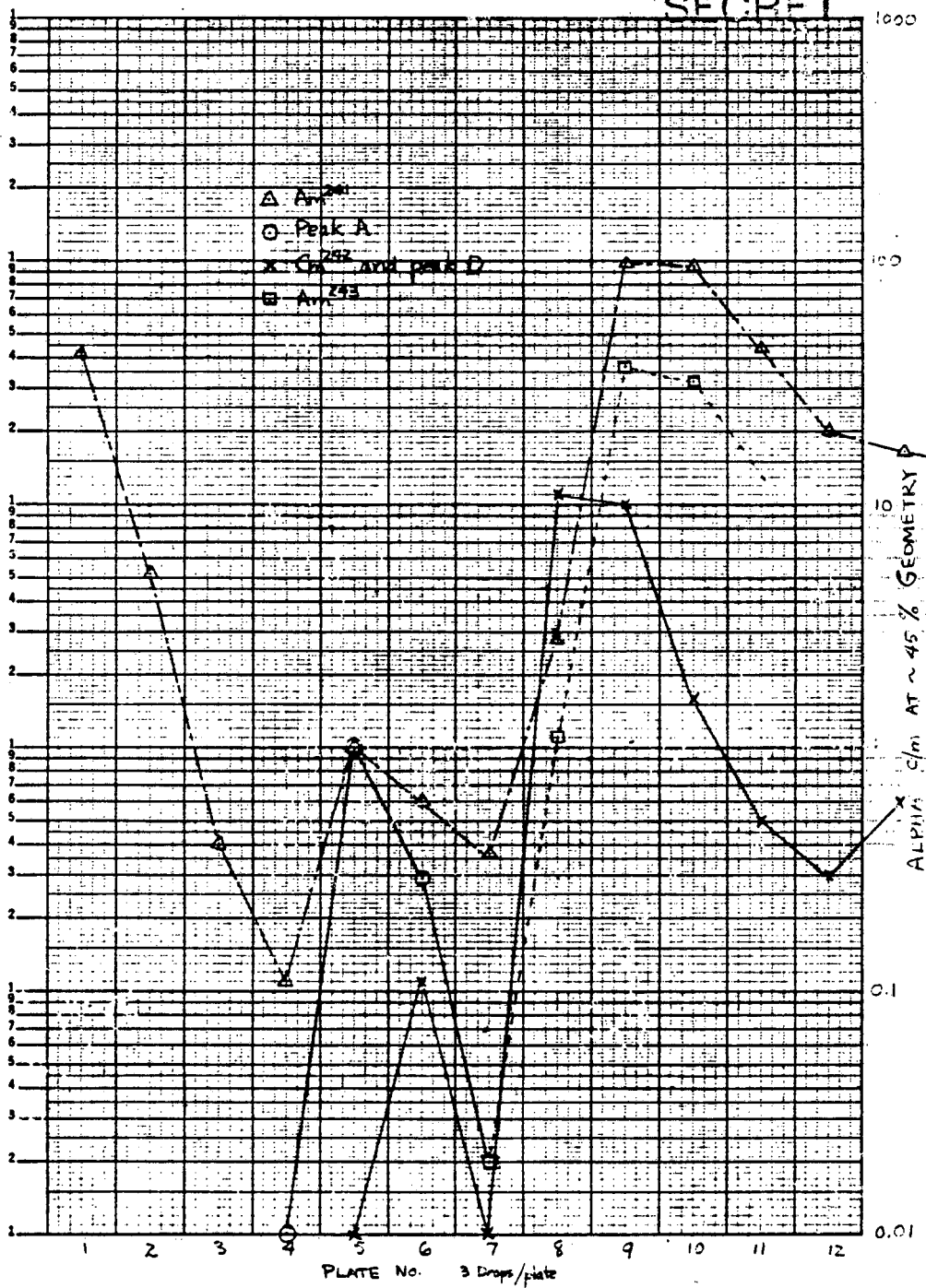
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MADE IN U. S. A.



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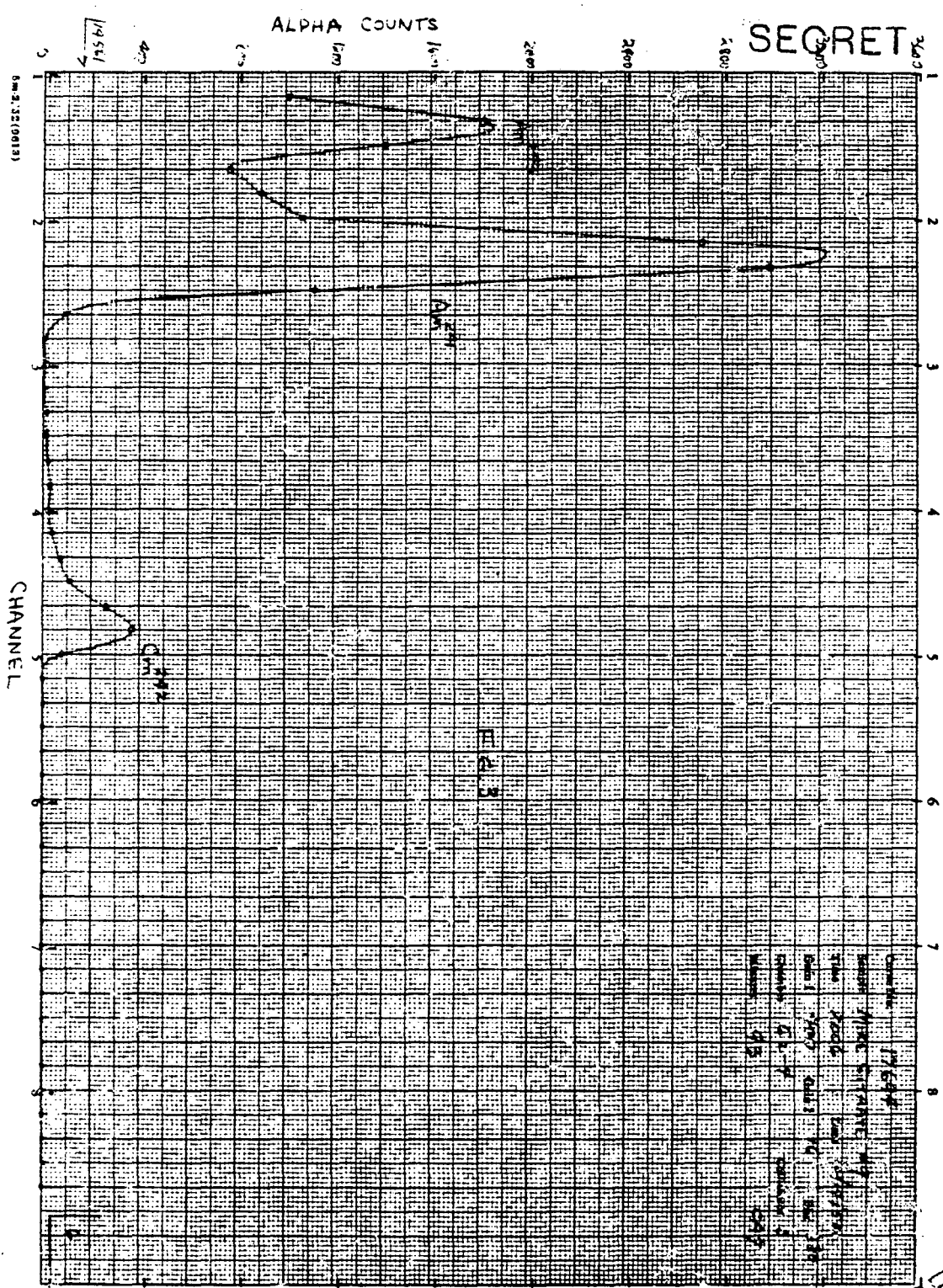


REUFFEL & BRASS CO., N. Y. NO. 88-88
Semi-Logarithmic, 5 Cycle X 18 to the Inch.
MADE IN U.S.A.

FIG. 2

SECRET

398-14 SECURITY & SAFETY CO.
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 MADE IN U.S.A.

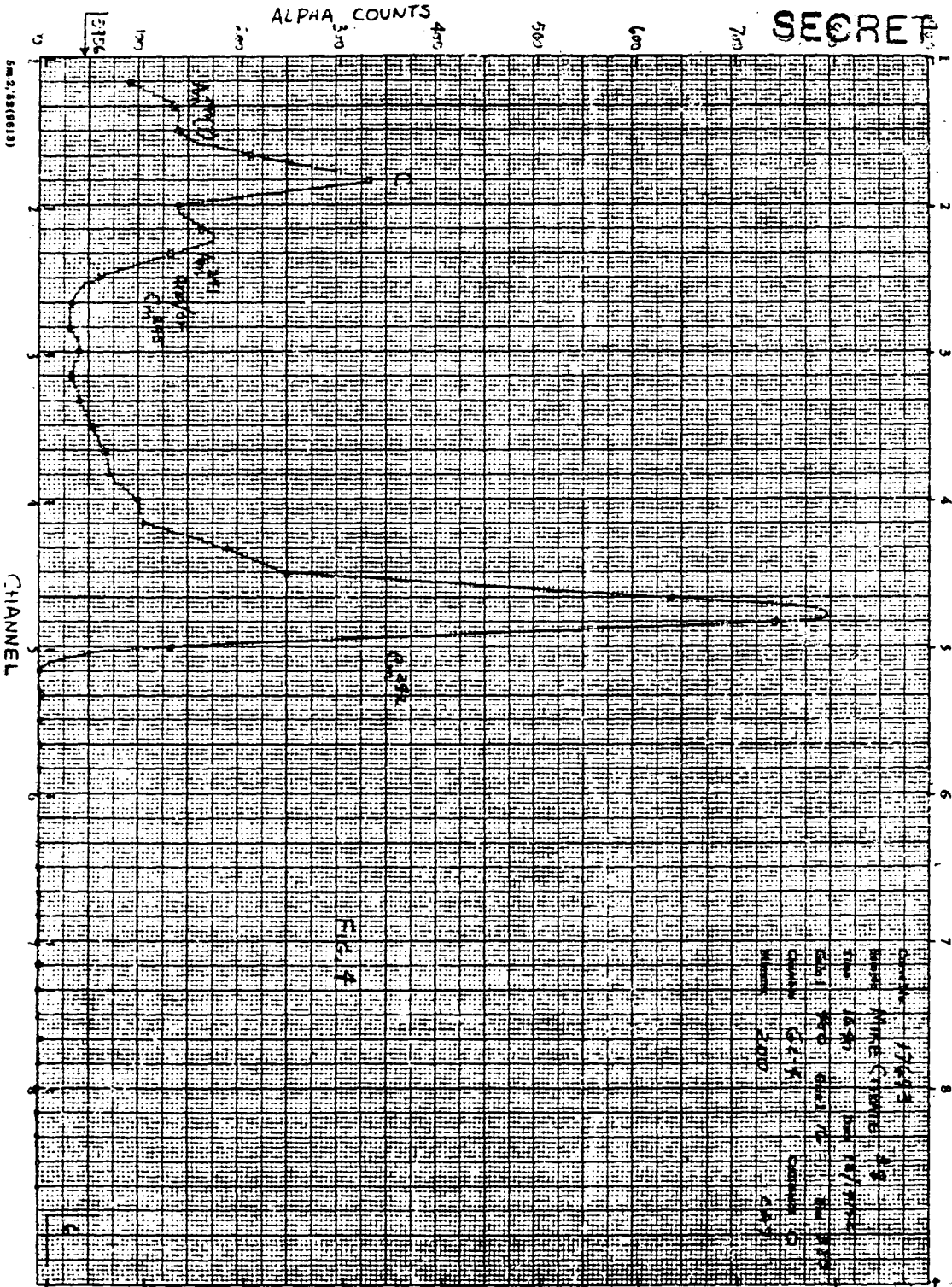


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88-14 KRYPTON & SEES CO.
Millimeter 5 mm. linear scale, on front panel.
and 1000



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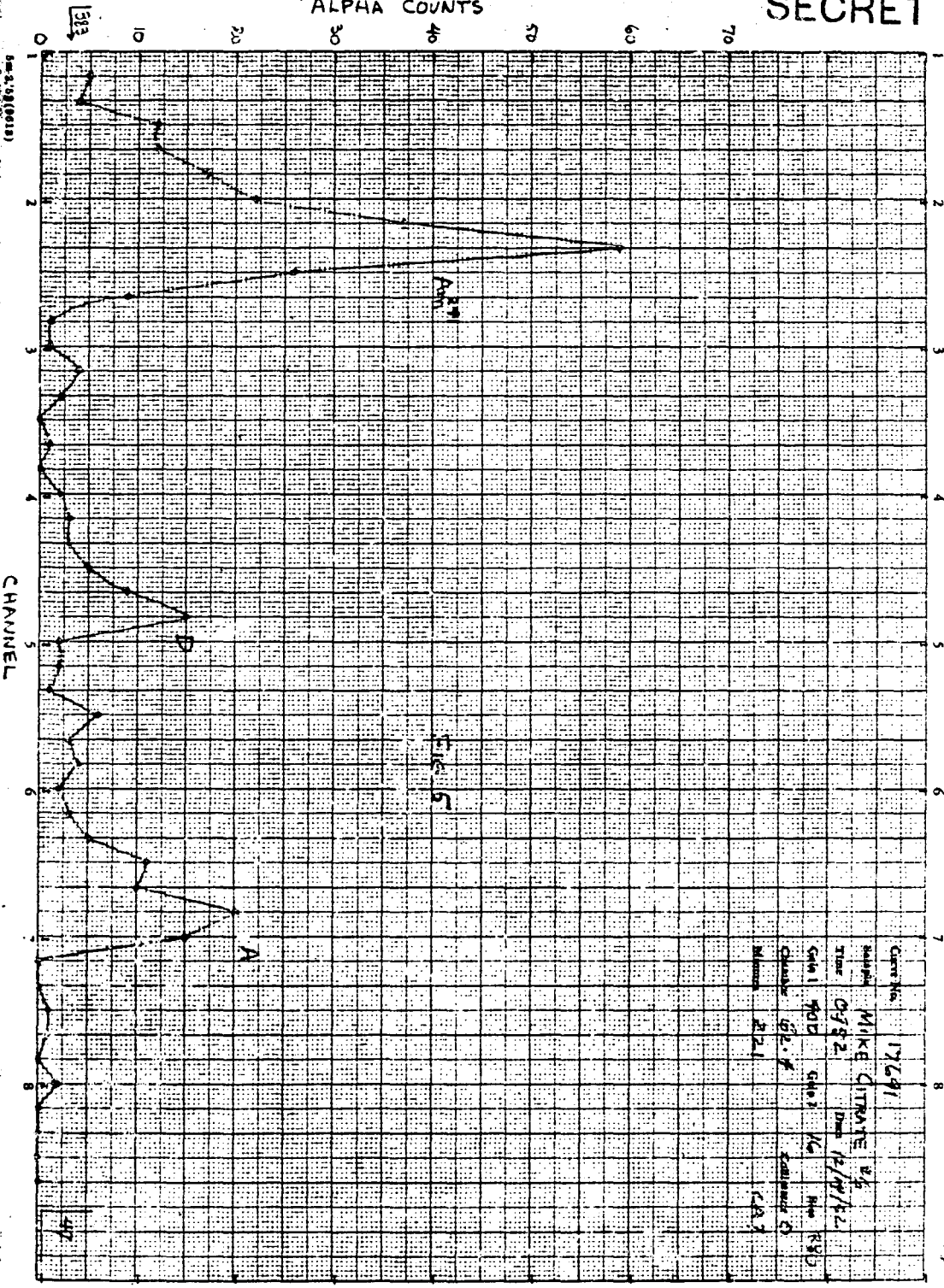
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CHANNEL

ALPHA COUNTS

SECRET

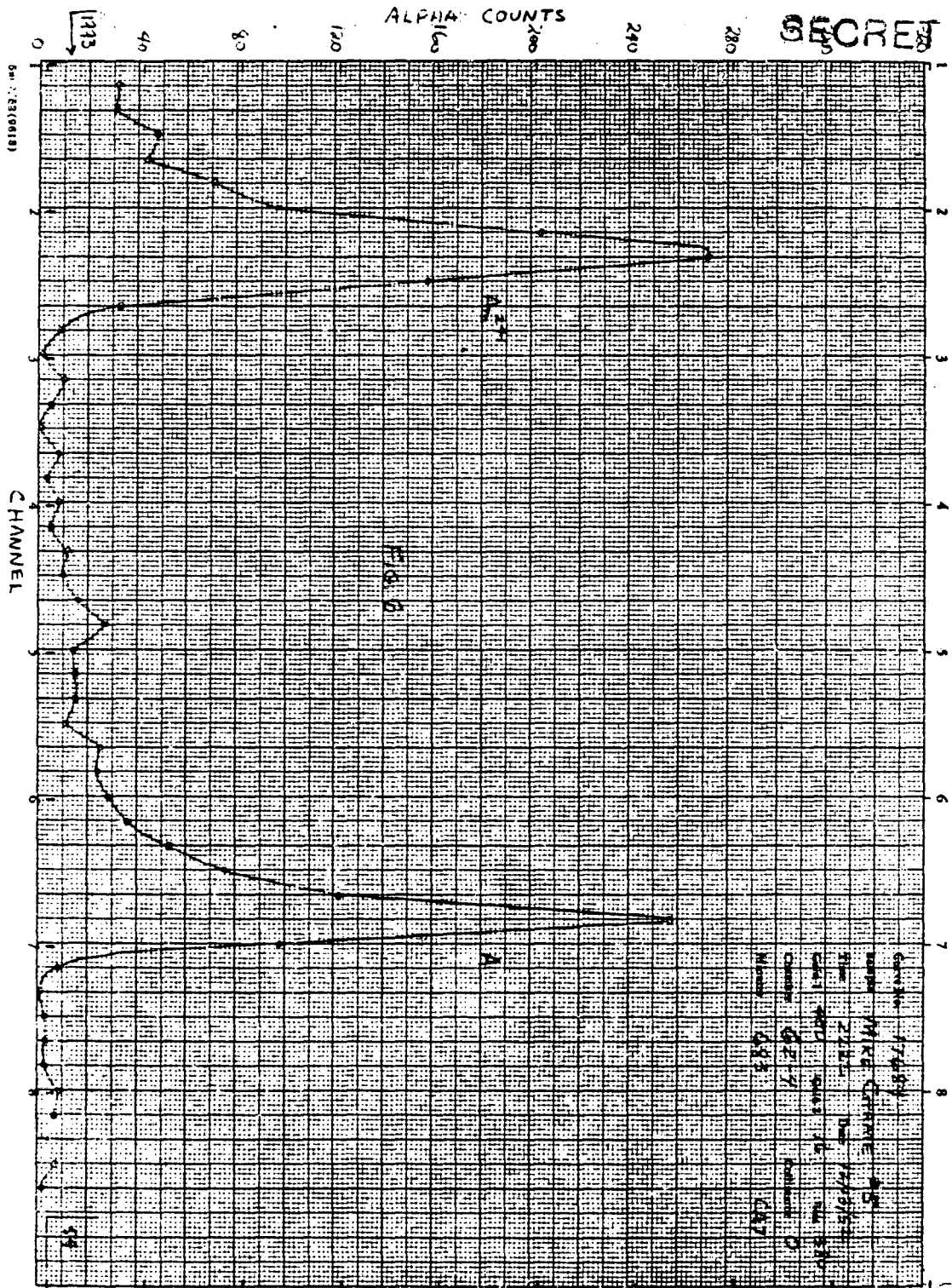
ALPHA COUNTS



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 Serial 7012 GMS KC Rm R30
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350-14 NEUFEL & CRAM CO.
 Millimeter, 5 mm. lines accurate, on film base.
 Model 100-1

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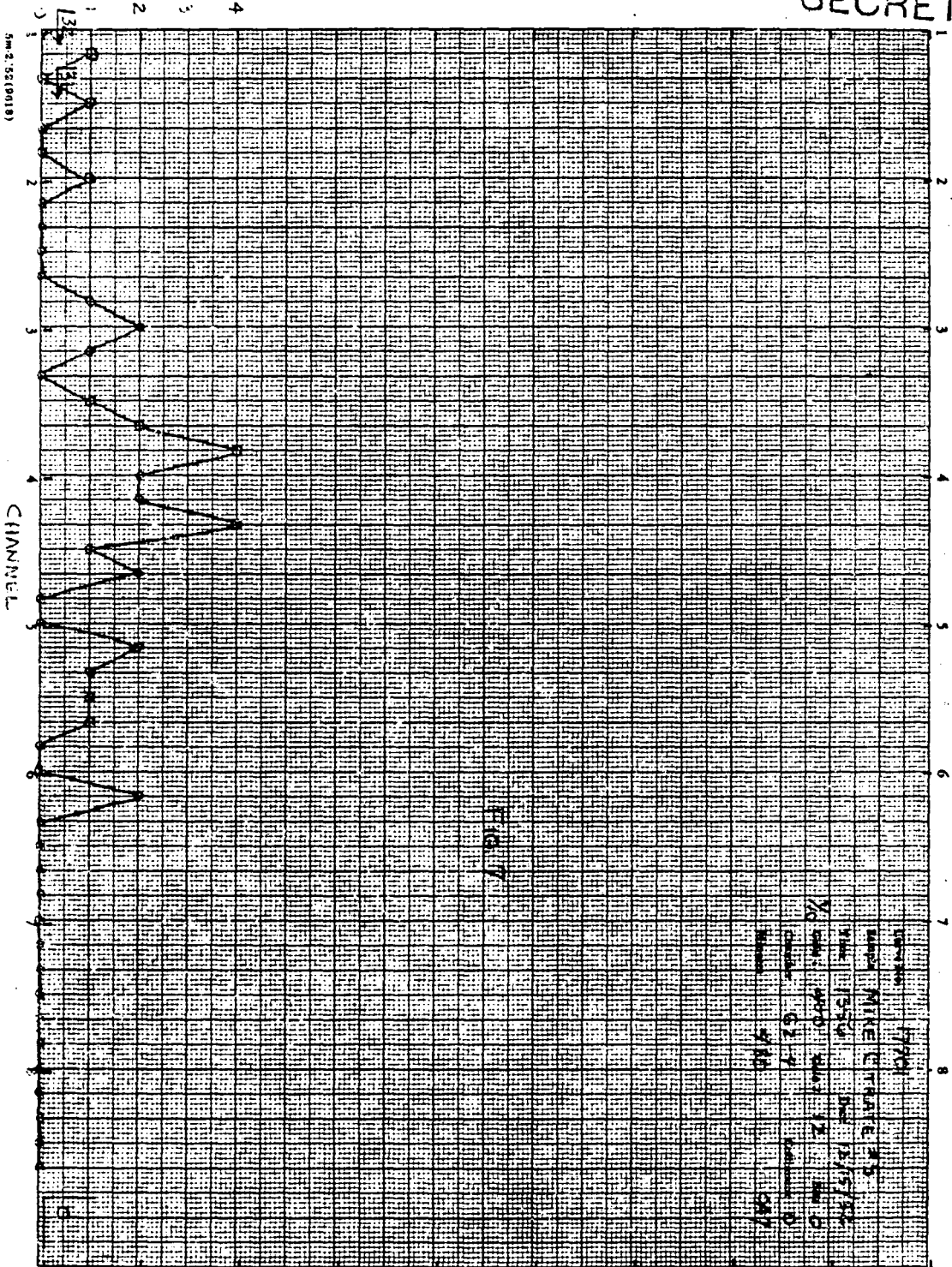
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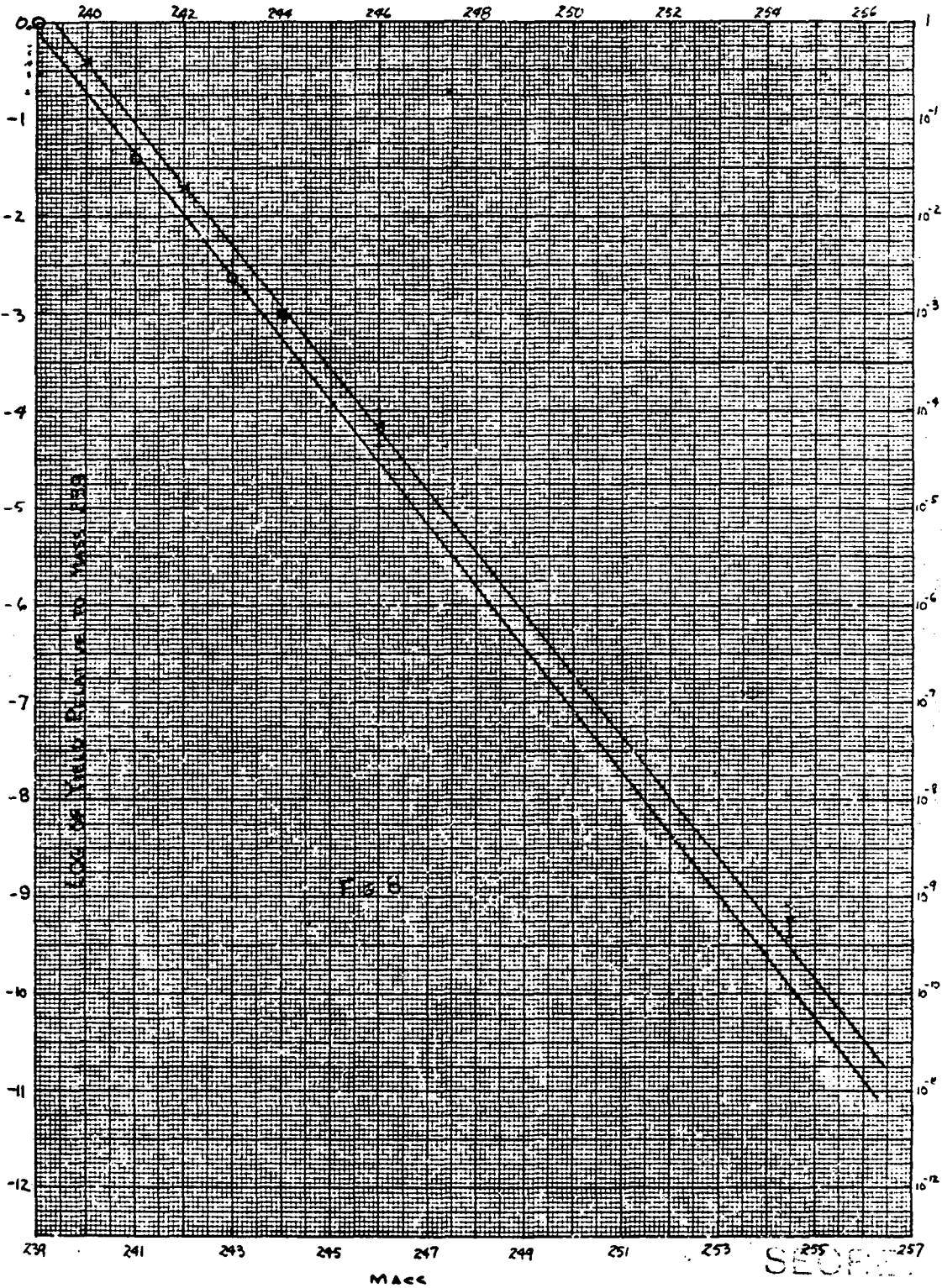
SPONTANEOUS FISSION COUNTS

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89-14 KEUPPEL & EASEN CO.
Millimeters, 5mm. line spacing, cm. line heavy.
made in U.S.A.

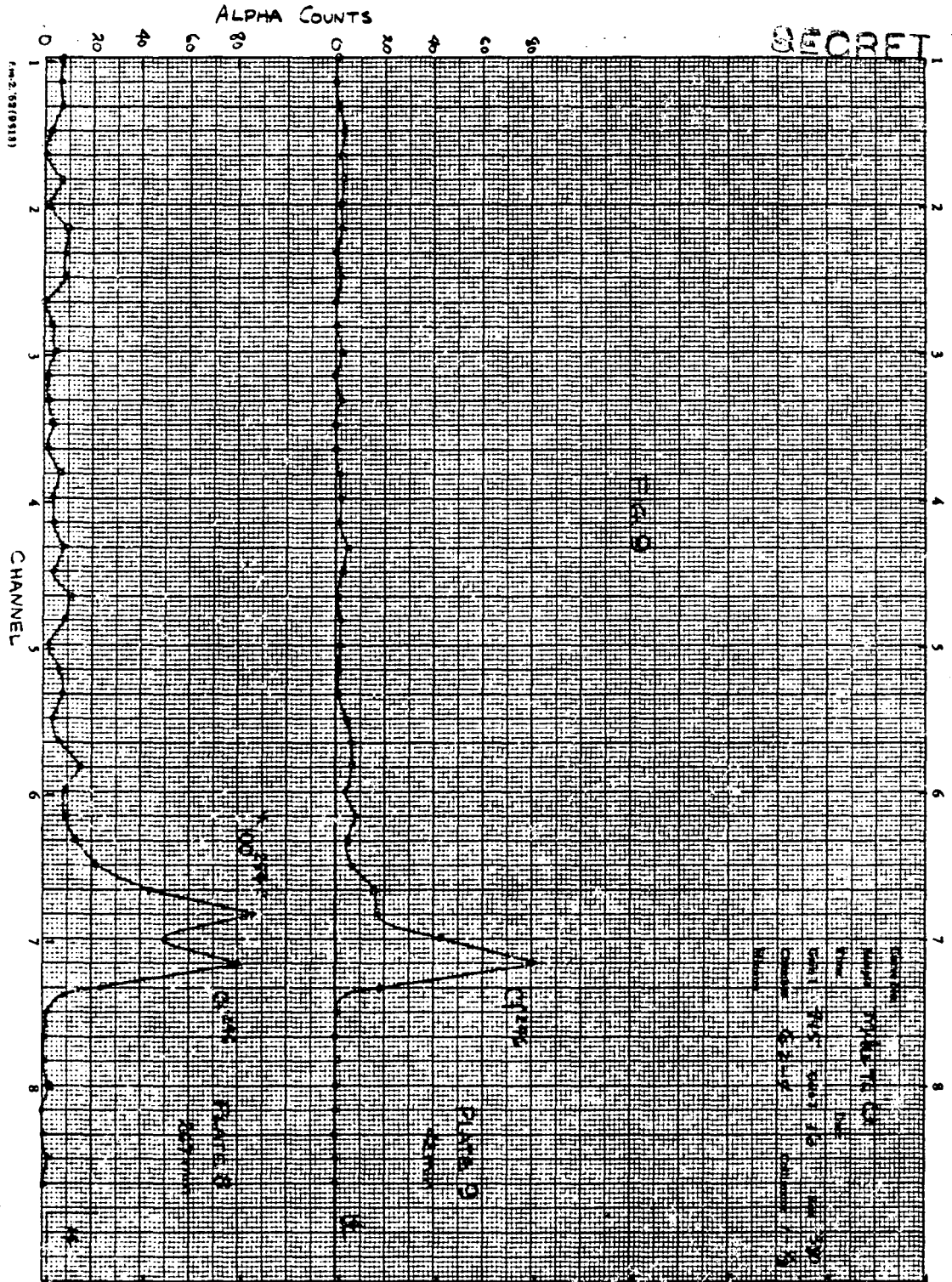
SECRET



889-16 KEUPPEL & ESSER CO.
Millimeters, 8 mm. lines available, cm. lines heavy.
MASS. U.S.A.

SECRET

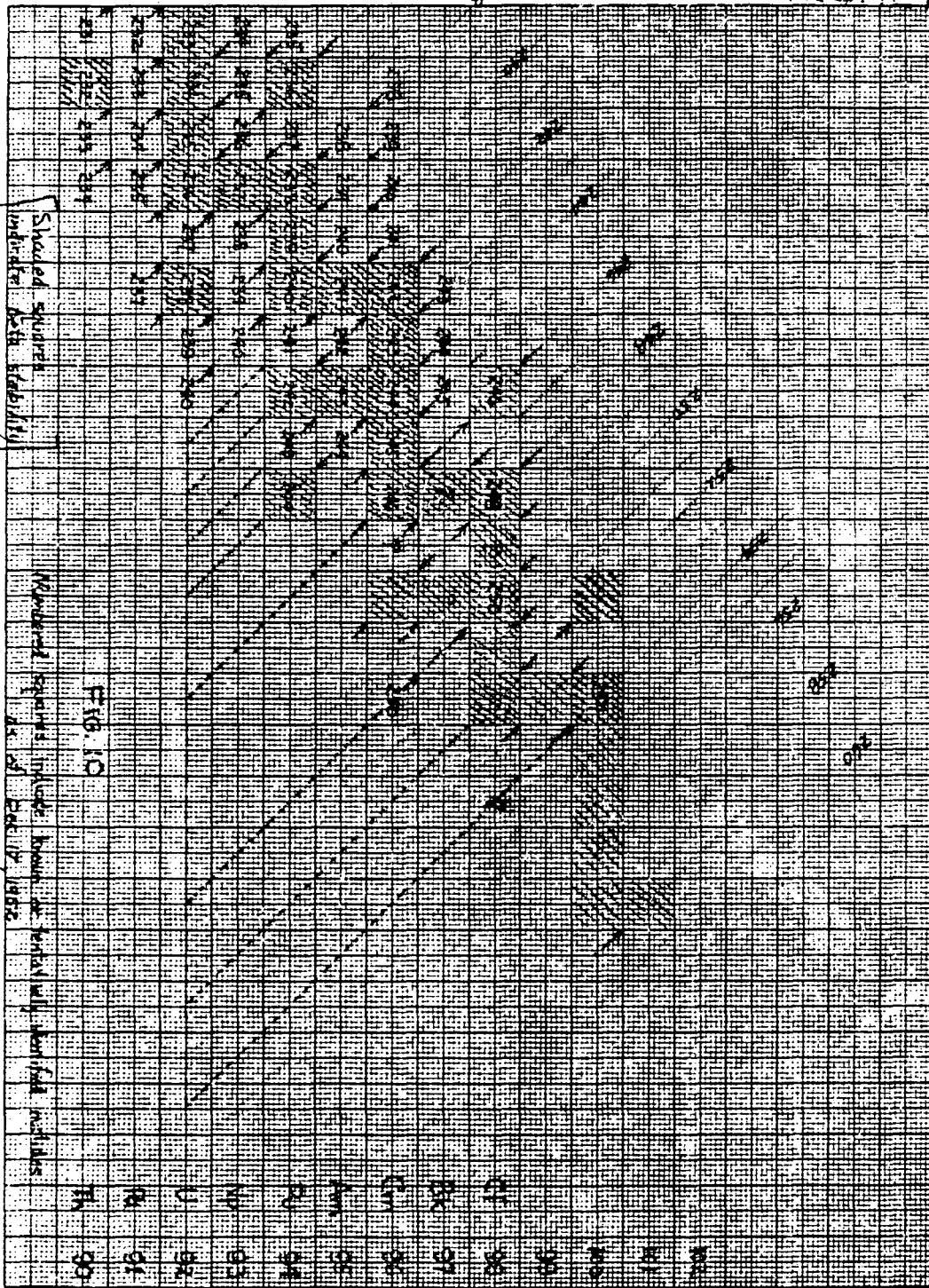
SECRET



950-14 KEUFEL & ESSER CO.
 Millimeters, 5 mm. lines accepted, cm. lines heavy.
 MADE IN U. S. A.

SECRET

SECRET



890-14 KUMHALL & SEGER CO.
 Millimeter, 5 mm. lines standard, cm. lines heavy,
 size 10 x 11.

Shaded Squares
 Indicate Both Visibility

Numbered Squares Indicate Boundaries of Visibility from Field Includes

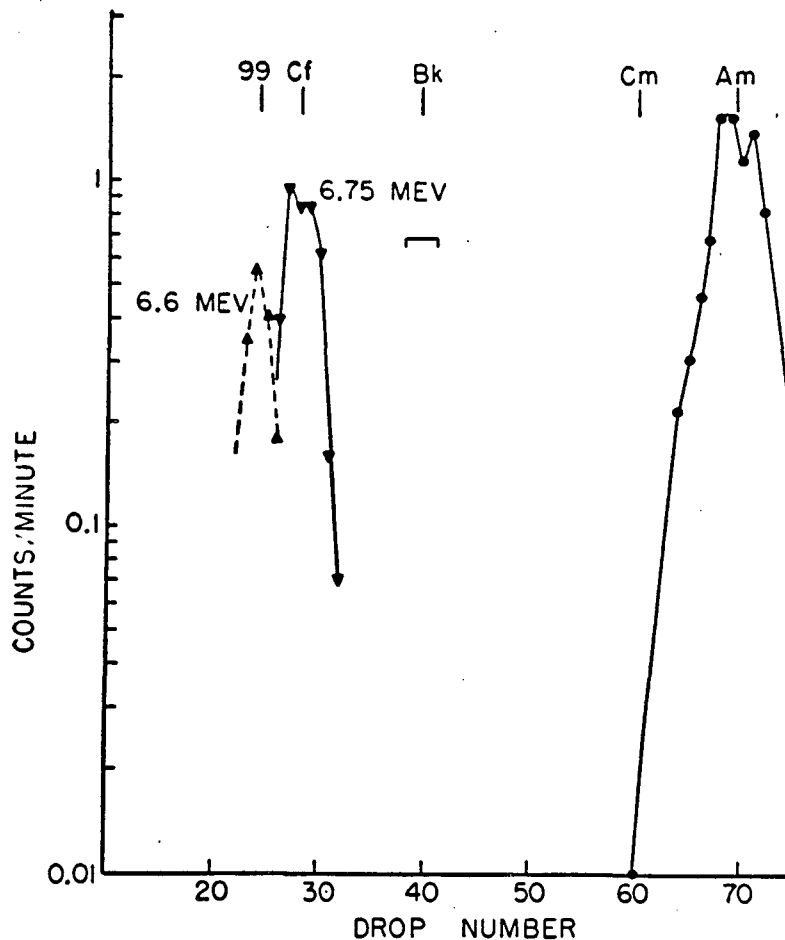
FIG. 10

At noon I telephoned Rod Spence and told him of the activity and of the memo we are sending him; he congratulated us, but when I asked if he could send us more material, he said that essentially all of it had been sent to Argonne. I called Winston Manning at about 2 p.m. and told him about our 6.6 Mev alpha activity that we are tentatively assigning to element 100 and about the memo describing the work that we are sending him; he also congratulated us and said, in response to my query for a fraction of their material, that he would take it under consideration. I telephoned the news to Doyle Northrup who also told me that the IO-7 Panel would have only a half-day session on the 19th, giving me time to reach Philadelphia for the Scott Award and talk; Doral sent a wire to Pritchard, saying that I can be in Philadelphia on February 19).

Copies of the Thompson-Ghiorso memo were sent to Spence, Manning, Northrup, and Herb York (Livermore).

Saturday, December 20, 1952

I went into the lab in Berkeley to talk with the fellows about the research. Since it seems unlikely that we shall receive any more starting material soon, yesterday Stan and Gary Higgins redissolved their second sample, made another citrate elution from Dowex-50 resin (named Mike TL Cit II), and began pulse analysis (run No. 3). This time the californium eluted for the first time in the proper position (that is, the exact position at which it eluted in the californium discovery experiment in 1950).



This showed chemically and conclusively that the 6.6 Mev alpha activity was due to element 99. Everyone present--Thompson, Higgins, Ghiorso, Street, and I--agreed with this interpretation. We decided not to bother writing a correction to yesterday's memorandum (because of the admonition for the maintenance of secrecy) but to tell the various interested parties when we see them or have phone conversations with them [and this proved later to be an unfortunate decision].

My parents arrived from South Gate to spend the holidays with their grandchildren, Helen, and me.

Sunday, December 21, 1952

I began to work on some of the speeches I am scheduled to give and the writing projects, such as the editorial for Reactor Science & Technology, that I have promised. Later I went out to the Contra Costa Golf Club, where I played with Dan and Evelyn Wilkes and a fellow named Art. For the first nine holes the scores were DW-41, EW-64, Art-46, GTS-44. Dan, Art, and I played an additional nine holes: DW-40, Art-41, GTS-46.

Monday, December 22, 1952

This morning I wrote a note to Mr. Warren H. Crowell (President, UCLA Alumni Association) to inform him that I am scheduled to give the Charter Anniversary Address at Riverside on Tuesday evening, March 24, 1953. Crowell has been trying for some time to arrange for me to speak at the Alumni Association banquet and to receive the Dickson Award.

Much of our lunchtime senior staff meeting was taken up with a discussion about the recent experiments and our conclusion Saturday that the activity was actually element 99 rather than element 100. With regard to the spontaneous fission activity, Al and Stan reported that careful spontaneous fission counting of the elution fractions showed that the main spontaneous fission activity was due to a californium isotope, indicating that the spontaneous fission previously observed in the transcalifornium elution region was probably due to the advance portion of the californium peak although there may also be some spontaneous fission due to element 100. Unfortunately, this can not be decided until there are further measurements on new material from another shot.

In today's mail was a Season's Greetings letter from my Japanese correspondent, Shizuo Fujiwara. Fujiwara described his present research and said that he sending me some reprints.

Tuesday, December 23, 1952

This morning I wrote a short note to Don Stewart at Argonne to ask him to make arrangements for transportation and Guest House reservations for my trip East next month. I told Stewart that I am scheduled to arrive in Chicago on TWA Flight 36 at 5:30 p.m. on Tuesday, January 6 and shall leave for Washington the following day at 6:00 p.m. on Capital Flight 902.

D. R. Stein (New York) wrote on December 18 to ask for assistance in

obtaining a number of reports, which he said are of great interest for editorial coverage in forthcoming volumes of the Gmelin Handbook. In my reply today I explained that I can send only one of the documents he requested (Appendix of Paper No. 21.1 of Volume 14B of the PPR). I said that item 3 (CN-3001) and item 5 (CP-3398) are, according to our records, still classified. I also suggested that he may be able to obtain items 1 and 2 (AERE-TR-437 and AERE INF/Bib-63) by writing to the Office of Technical Services, Department of Commerce, Washington, D. C.

I also wrote a note to Professor Dr. G. J. Sizoo (Amsterdam) in response to his letter of December 15, saying that I am in favor of inviting Professor Berta Karlik to our Stockholm meeting (International Union of Chemistry) and also of inviting Drs. G. Manov and W. Binks to serve as Advisory Councillors.

I made a tour of some of the labs, including those in Bldg. 50. I particularly wanted to see Dick Glass and Per Kofstad (whose father recently wrote for information about his son's progress, etc.). Apparently both fellows have taken off early for the Christmas holiday.

In the afternoon Al, Luis Alvarez, Harvey White, and I went out to Mira Vista and played the last nine holes (AG-48, LWA-52, HW-53, GTS-50). Al and I won a low ball-low total match, 4 up. Al and I then played the last nine again (AG-44, GTS-48).

Wednesday, December 24, 1952

When I got to the lab, I telephoned Joe Katz and told him that I shall be spending January 7 at Argonne in case there are some things to discuss about Volume 14A of the Plutonium Project Record. I also told Joe about our recent experiments.

I signed and mailed the evaluation sheet for Dwight C. Conway, addressed to the National Research Council, that Earl Hyde prepared.

I also signed a letter to Per Kofstad's father, saying that Per continues to maintain an excellent record in course work and has done a considerable amount of laboratory work in the past year. I wrote that he is held in high regard by his fellow students to whom he is a modest, well mannered, pleasant companion and that he remains in good health.

Earl had also prepared a letter of recommendation for K. Lynn Hall, a former graduate student here, and addressed to Bruce J. Miller, Personnel Administrator (Union Carbide and Carbon Corporation, Tonawanda, New York), which I signed.

A December 19 letter arrived in the morning mail from Manne Siegbahn, saying:

There has been some delay in my answering your kind letter of November 13, due to some necessary discussions regarding the program. We would be very glad to have Dr. Rasmussen here as visiting professor from, say February 1, to June 30. We will of course pay the travelling expenses for him and his family. As to the salary we are bound by the standardized salary for a Swedish

state-professor which corresponds rather well with his present salary (2.560:- Kr. per month). The tax is 34% so what he gets is 1.690:- Kr. per month. The lower living costs here compared to U.S.A. will probably well compensate the higher income tax. As soon as I have got his definite reply, we will try to find a furnished apartment for him. (This is a little difficult under the present circumstances, but I hope it will be possible to arrange it by February 1.)

We have in the meantime had negotiations with Dr. Randers, Director of the Norwegian-Dutch pile in Oslo, so we can get some plutonium from them in 3 or 4 months. Dr. Randers is very interested in having Dr. Rasmussen as adviser for some weeks after he has finished his research work here.

We would like to know if Dr. Rasmussen has some suggestions as to special equipment, which we ought to acquire before he gets here.

I checked, but John apparently has taken off for the holiday. I will speak to him after Christmas.

We had our usual Christmas Eve celebration, Swedish style--lutfisk, saffron bread, sill, risgryn, etc. with my parents. Afterwards there was the traditional opening of Christmas presents.

Thursday, December 25, 1952

The Seaborgs celebrated Christmas in their usual manner, with gifts from Santa Claus under the Christmas tree in the morning and a turkey dinner later.

Friday, December 26, 1952

It was a rather quiet day at the lab for many of the fellows are taking a long holiday. I tried unsuccessfully to make a few phone calls, so I spent some of the time catching up on my reading.

Later I went out to Mira Vista with Dan Wilkes and Art Renne for a round of golf (AR-87, DW-79, GTS-92).

Saturday, December 27, 1952

I worked on some of my writing projects, played with the kids and their new toys, and talked with my parents.

Sunday, December 28, 1952

Again I did some reading and writing.

Monday, December 29, 1952

I again tried to make some phone calls, such as to Ken Pitzer and Greg Engelhard (Assistant Director of Athletics), but almost everybody seems to be out of town.

Earl prepared a draft of a laudatory recommendation for David H.

Templeton, who is applying for a Guggenheim Fellowship, which I read and edited this morning. I wrote, in part, "I feel that his research program and effectiveness will benefit greatly through the award of a Guggenheim Fellowship to be used, as he suggests, at the leading crystallographic laboratories of western Europe, especially the outstanding laboratory of Professor Hägg at Uppsala." I then gave it to Doral to have typed for my signature.

I recently received another request (third) for a letter of recommendation from Vera Kistiakowsky Fischer, who is applying for a National Science Foundation fellowship. This I filled out and mailed.

A memorandum went to George Everson (Personnel) to inform him that we want to have Gary H. Higgins, now on the California Research and Development payroll, join the Radiation Laboratory in connection with the Project Whitney chemical research under Dr. Kenneth Street starting Wednesday, December 31, 1952. I suggested a starting salary of \$550 per month and explained the unusual starting date is in order that he not lose any time in his transfer from the CRD payroll. [We are continuing to hire scientists of various levels for the Project Whitney work; for example, Melvin S. Coops, a Chemist P-2 3.2, just began to work with us today at a salary of \$350 per month.]

I received a letter, dated December 24, from G. Curtis Pritchard about the arrangements for the Scott Award, verifying our recent phone conversation and my wire.

A confirmatory letter about the IO-7 Panel meeting arrived today from Doyle L. Northrup, who also said that they are looking forward to a great deal of interest to a report of our recent work here at Berkeley, in addition to an interesting and worthwhile session of the Panel.

Tuesday, December 30, 1952

I again worked on some of my writing projects and attempted to phone (sometimes unsuccessfully) a number of people. I wanted to tell John Rasmussen about Siegbahn's letter and learned that he is spending the holiday with Louise's mother and brother in Red Bluff and that we did not have his phone number. Eventually, Margie remembered that Louise's mother owned a drug store, and an alert Red Bluff telephone operator located John; I was able to pass on the news. I told John that I will take care of the administrative details of the visit.

I then looked over the research of some of the fellows. In my tour through the labs I learned that the Health Chemists are pressing americium slugs in the slug box in the Cave Room. I was told that they plan to clean the outside of the slugs in the hoods in Room 103 and 105 (Bldg. 5).

Dr. O. D. Shreve, who is Chairman-Elect of the Philadelphia Section, ACS, (Fabrics and Finishes Department, E. I. du Pont, Philadelphia) wrote on December 22 to ask for a title and abstract for the talk I am to give before the Philadelphia Section of the ACS on February 19 (at the time I am to receive the Scott Award). I mailed the title ("The Transuranium Elements") and abstract to him today.

I wrote a note to Donald Lane to let him know that I am scheduled to be in Washington, staying at the Statler, on January 8, 9, and 10 and that I shall try to phone him at his office sometime during my stay. [This trip is for a meeting of the NCAA.]

In the mail today was a notification from Foster York that Case No. S-3176, Serial No. 318,072 ("Solvent Extraction Process" by Glenn T. Seaborg, Walter J. Blaedel, and Matthew T. Walling, Jr.) was filed with the United States Patent Office on October 31, 1952. These wartime patent cases seem to go on forever.

I also received reprints from The Chemist [XXIX, 585 (1952)], which contained my talk, "Nuclear Energy for Industry," that I gave to the American Institute of Chemists on September 25 when I received an honorary membership from the organization.

Wednesday, December 31, 1952

I wrote to Professor Manne Siegbahn to tell him that I have told Dr. Rasmussen about his letter of December 19, 1952 and that he is happy to accept the invitation to spend about six months in Siegbahn's laboratory. I said that Rasmussen will let him know soon his (Rasmussen's) arrival time and other matters of interest. I then wrote a memorandum to Donald Cooksey:

Attached are two copies of correspondence between Professor Manne Siegbahn and my self concerning the visiting professor which the Nobelinstitutet för Fysik would like to have us provide for them. We feel that Dr. Rasmussen would fill the bill and he is willing to do so. There will be no expense to the Laboratory so far as I can foresee, but it would be very advantageous to Dr. Rasmussen if an advance loan for air transportation for himself and his family could somehow be arranged. I don't suppose there are any special steps that should be taken in connection with his obtaining a renewal of his passport and a new passport for his wife?

I presume that you may want to use the extra copy of this correspondence to show the proper people in the Area Office.

I also responded to a December 9 request for nominations for the "Alumnus of the Year" Award for 1952 from John P. Symes (President, California Alumni Association). I wrote that my first suggestion is Mrs. Lillian E. Gilbreth, whose accomplishments have largely been prior to 1952, but that I seem to recall that Stan McCaffrey once mentioned to me that consideration was being given to making the award for accomplishments over a period of time. My second suggestion, I wrote, is Dr. Selman Waksman of Rutgers University, who obtained his Ph.D. from the University of California in 1918 and received the full 1952 Nobel Prize in Medicine or Physiology for his work in the discovery of streptomycin.

W. B. Reynolds sent up a letter from H. A. Fidler for me to see, describing an accident to the Chalk River pile that has necessitated shutting down the pile for some time. I read the letter and, since this involves some of our irradiations and experiments, I showed it to a few of the fellows before returning it to Reynolds. [This accident occurred

the day, December 12, that I telephoned Harvey about his clearance and undoubtedly accounts for his seeming to be a bit distracted when we spoke.]

Helen and I spent a quiet New Year's Eve at home with the kids and my parents.

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