HOW IT CAME ABOUT:

Radioactivity, Nuclear Physics, Atomic Energy

by
Elizabeth Rona

Oak Ridge Associated Universities

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ORAU 137

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Acknowledgments

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Foreword

Doctor Elizabeth Rona joined the staff of the Special Training Division of Oak Ridge Associated Universities (ORAU) in 1950 as a Senior Scientist. This Division had been organized in 1948 as a unit of the Oak Ridge Institute of Nuclear Studies, as ORAU was then called, in order to meet the rapidly growing needs of professional personnel for training in the use of the radioactive isotopes that had become available in quantity in the postwar period. This training was accomplished through a series of courses in basic radioisotope techniques, each 4 weeks in duration, involving both lectures and extensive laboratory practice. Dr. Rona's long experience with radioactivity from the early period of its discovery made her an especially valuable member of the teaching staff for these courses. When in 1954 the courses were opened to foreign participants through President Eisenhower's Atoms for Peace program, her fluency in a number of languages was a valuable additional aid in her teaching. Her own research at ORAU on the geochronology of marine sediments based on determinations of uranium and thorium in seawater, carried out in collaboration with the Texas A&M University, was an added stimulus for her teaching. She generously made time available to spend with individual participants to clarify experimental procedures or technical aspects. She continued in this capacity at ORAU for 15 years. In 1965 she joined the staff of the Institute of Marine Sciences of the University of Miami.

Her long and fruitful career in radioactivity began near the end of the second decade of this century before the discovery of quantum mechanics and shortly after the discovery of radium. Her personal involvement in the history of nuclear science thus covers the full span from its earliest beginnings to its present maturity. Now she has written this account of her personal recollections of the exciting discoveries as well as the false leads and the people involved in them through the discovery and implications of nuclear fission. Of special interest is her personal acquaintance with most of the scientists involved in research on radioactivity throughout this long period. This places her in a unique position to tell the story of this chapter of 20th century science. Oak Ridge Associated Universities is pleased to make this memoir of an esteemed member of its scientific staff available in this form.

William G. Pollard
Executive Director, 1947-1974
Oak Ridge Associated Universities

Budapest and Karlsruhe

For a long time my friends have urged me to write about my scientific activities and about the distinguished scientists whom I have known and with whom I have been associated. Ernest Rutherford, in a letter to Lord John W. Rayleigh in 1936, wrote: "I am sure that we all ought to give some of our recollections of those past and gone before they are lost and gone for good." In this short paper I shall try to do just that.

My interest in science, I think, had its inception in my early childhood. At that time, in the early 1900's, science was nebulous and not well understood, but it was extremely exciting. I remember that one day very early in the morning I looked out of my bedroom window, which faced the large porch of our summer home in Budapest, and saw my father writing, deeply involved in something, oblivious of the outside world. "What can be so interesting that it makes it worthwhile to get up at the crack of dawn?" I asked my father. He cryptically answered, "research." I think that my interest in scientific research started unconsciously at that early age. When I entered Latin School (gymnasium as it is called in my native country, Hungary), my father Samuel Rona, M.D., who was a well-known physician, showed and explained to me the x-ray machine in the Holy Stephen's Hospital in Budapest. One day he came home very much excited and showed me red and inflamed spots on his leg from a small tube, which he carried in his pocket. "Radium," he explained, "and some day it will cure skin diseases, even as stubborn as lupus erythematosus."

At about the same time at the turn of the century, Pierre Curie had initiated experiments in Paris in which animals were exposed to the radiation from radium; this procedure was later called Curie therapy or radium therapy. Two French physicians named L. Wickham and H. Dominici were using radiotherapy at that time and were in close contact with my father. They came to an international congress of medicine, sponsored by the university and the city of Budapest, with moulages showing skin diseases, before and after treatment with radium. The participants were not familiar with the city of Budapest; they expected to come to an exotic place and found instead a highly cultured, sophisticated city, which was then called "the little Paris of eastern Europe."

In the early spring it was customary for me to go with my grandmother to her summer home. I was the only granddaughter who shared her love of nature. We observed the slow development of flowers, from spring to summer, the lilac, mock orange shrubs, the irises, which slowly opened their fragrant flags, and my grandmother's favorite pine trees, which she planted as seedlings and which had grown very tall. During the summer three French boys from Paris visited nearby relatives; they were lively companions, and we had lots of fun together; French became my second language. On every Sunday during the summer, the assistants and collaborators who came from other parts of the world to work with my father spent the afternoon at our summer home. They were an international group; some Japanese, some Germans. They gave my sister Marie and me much attention, and we became familiar, at an early age, with persons of different nationalities.

During my sophomore year at the University of Budapest, my father died of a disease of which he had cured many, erysipelas. He wrote a monograph, "Hundred Cases of Cured Erysipelas," but his case was especially virulent. After treating a patient he had touched a slight wound on his head before he disinfected his hand. I had always wanted to study medicine. I grew up in an atmosphere of medical research, but my father opposed my choosing it as a career. He thought it was too hard for a woman. After his untimely death, I respected his intentions and chose to pursue a career in chemistry and physics. I became very interested in physical chemistry.

I received my Ph.D. in chemistry, physics and geophysics

at the age of 21, but was very much aware of my limited knowledge and experience. In an effort to deepen and widen my knowledge, I joined the postgraduate students at the Technical University of Karlsruhe, Germany, intending to work under the direction of George Bredig, who was considered the leading physical chemist of his time. I sought information from my fellow students about the scientists in the department before making a final choice about which one I would work with. They told me, "If you want a paper published, work with Bredig, but, if you would like to enter a new and exciting field (radioactivity), work with Fajans."

My work with Kasimir Fajans exposed me to what was then the new field of radioactivity, which was to capture and hold my imagination and interest for years to come. Fajans, a Pole, had all the qualities of a real scientist and teacher. He inspired his students in such a way that they regarded their work more as an adventure instead of as a chore. The students liked him and felt at home with him; they felt differ-

George Bredig

ently about Bredig, who was very much the authoritative German professor, called the "Schreckliche" (the Terrible).

Under his surface sternness and aloofness, however, Bredig was a warm, sensitive person devoted to his students' welfare. He was, however, so afraid of any accidents that he posted signs reading "Danger" at any place in the laboratory where a voltage higher than 110 volts was used. From time to time we were invited to his home, where his lovely wife served us tasty German cakes. The only drawback was that, because I was the only woman at the time in the laboratory, I had to join the ladies. I felt much out of place in this group. The conversations dealt with children, cooking preserves; recipes were exchanged. To these discussions I could not contribute. How I longed to be with my colleagues, to hear and talk shop. Fajans gave many small parties in the laboratory with no discrimination against women. On his 26th birthday, we gave him a party and presented him with a cartoon which showed Fajans in the middle of broken glass and mashed-up

Cartoon presented to Kasimir Fajans by his staff on his 26th birthday.

ionization chambers. It carried the inscription: "Experimental Schwierigkeiten gibt es nicht" (There are no such things as experimental difficulties). He was not a skilled experimenter. We feared his handling our instruments, but he claimed over and over again that one could carry out any experiment, even complicated and difficult ones, if one only tried.

A few years earlier Fajans and his student Oswald Göhring

Oswald Helmuth Göhring (1889-), left; Kasimir Fajans (1887-1976), right, front; and Max Ernest Lembert (1891-1925), right, back; photographed at Technische Hochschule Karlsruhe in 1915.

had discovered protactinium. At the time when it was placed in the periodic system, Fajans observed that the element (which was called UX in the nomenclature of that time) was not a single element but a mixture of two consecutive radioelements, UX I and UX II. This conclusion followed from the displacement law, discovered by Fajans. According to that law, the uranium series should be formulated as:

$$\mathrm{UI} \rightarrow \mathrm{UX\ I} \rightarrow \mathrm{UX\ II} \rightarrow \mathrm{UII}$$

In today's nomenclature it would be written as:

$$^{238}_{92}\mathrm{U} \xrightarrow{\alpha} {}^{234}_{90}\mathrm{Th} \xrightarrow{\beta} {}^{234}_{91}\mathrm{X} \xrightarrow{\beta} {}^{234}_{92}\mathrm{U}$$

But no radioactive element was known in the group at the place between uranium and thorium. In an attempt to find this new element, Fajans and Göhring applied the generalization that, after a beta transformation, the daughter element is electrochemically more noble than the mother element. A solution of UX I (thorium-234) was placed in a lead dish in the expectation that "ekatantalum" (element X in the above equation) was more noble and thorium was less noble than lead. Radioactive material was indeed formed on the surface of the lead; it decayed with a half-life of 1.1 minutes. Because it occupied a previously vacant space, it was a new element and was named "brevium" (Bv). A few years later Otto Hahn and Lise Meitner separated from uranium residues the homolog of tantalum, which was found to have a long half-life (determined later to be 32,000 years). Hahn and Meitner traced the continuous formation of actinium, proving that the element with the long-lived activity was indeed the parent of actinium. They named it protactinium (Pa). This name was then adopted instead of brevium.

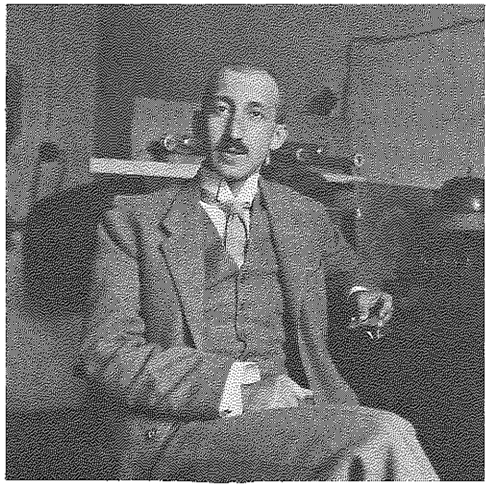
Fajans' work on lead isotopes does not exactly fit into this narrative, but it demonstrates a great scientist's ingenuity, foresight, and courage. Fajans sent one of his gifted students, Max Lembert, to Harvard to work with Theodore William Richards. Richards was the accepted authority in determining the atomic weights of elements. Lembert brought from Karlsruhe lead samples separated from minerals poor in uranium and those poor in thorium. Fajans believed they would prove to have atomic weights different from that of ordinary lead. Lembert, who believed in Fajans' views, expected to prove that the samples of lead that he brought would have identical chemical and spectroscopic properties but different atomic weights. Lembert's expectation was based on Fajans' theory of radioactive transformation. He had put forward a comprehensive scheme for placing the members of three different radioactive disintegration series in the periodic system. The relation of one member to the preceding one was determined by whether the transformation is accompanied by beta or alpha particles. The end product of each series, lead, was assumed to have different weights and to be different from ordinary lead. Soon after, Frederick Soddy explained with great clarity the relationships between the radioactive elements of the three series and called forms of a particular element differing in mass isotopes. The ideas of Lembert and, of course, Fajans were met with rejection, or at best skepticism. Even though his experiments with Lembert showed significant differences between radioactive lead and ordinary lead, Richards was convinced that the differences in atomic weight

were caused either by some impurity or some other external factor.

In a recent letter from Dr. Göhring, the first since thirty years, he describes how the discovery of a new element came about. With his permission I quote him: "Fajans did not play any musical instruments, but he was very fond of music and attended concerts and operas. One day we went to hear Wagner's Tristan and Isolde. After a long day of work, Fajans was very tired and soon he fell into a state of somnolence, his eyes closed. I thought that he was asleep, but suddenly he took a piece of paper from his pocket and wrote down an equation. I kept this paper as a relic because the development of this equation led to the discovery of hitherto unknown isotopes."

I stayed in Karlsruhe for 8 months. I remained in touch with Fajans even after leaving his laboratory. This friendship lasted until his death a few months ago, with occasional visits and frequent correspondence.

Returning to Hungary, I was lucky to have the opportunity to work with George von Hevesy. Hevesy had recently returned from the Radium Institute of Vienna, where he had



George von Hevesy

worked with Fritz Paneth, among others. At that time he was a lecturer at the University of Budapest. His great contribution was to follow physical, chemical, or analytical reactions by adding to the system a radioactive element with the same chemical property as the element of interest, thus using radioactive isotopes as tracers of their stable element. I was still very much interested in physical chemistry, and I had the opportunity to work on problems that had not been possible to solve by ordinary methods, e.g., the solubility of molecular layers. One interesting research project was the determination of the diffusion constant of radon and its atomic radius. I found the diffusion constant of radon in water to be 0.985 centimeter per day, and the atomic radius 1.75×10^{-8} centimeter. F. Wallstabe had determined the diffusion constant of radon to be 0.066 centimeter per day. This figure is too low because the atomic radius, calculated from this number, would be 40×10^{-8} centimeter, much too great; only complex molecules have this large radius. Eva Ramstead, a Swedish scientist, verified my results approximately at the same time. I met her many years later in Stockholm and renewed a friendship which had been initiated a long time before by correspondence.

My first paper was analytical. Hevesy drew my attention to G. N. Antonoff's work at Rutherford's Laboratory in Manchester, England. He had separated a new radioactive element from uranium salts: a beta emitter, UY. Later, Soddy and A. Fleck were unable to verify Antonoff's results. Hevesy wished to apply his radioactive tracer method to this problem and asked me to repeat Antonoff's experiments, using methods of precipitation and fractionation to eliminate the interference of uranium and all its daughters with the new element. I succeeded in verifying Antonoff's results. I separated UY from all of the interfering elements and found that it was a beta emitter with a half-life of 25 hours. Next it was necessary to decide where to place this new element in the periodic table. As far as we were able to determine, Antonoff did not know where to place it either. Only after the uranium isotope uranium-235 had been discovered and established as the first element of a new series (the actinium series) was UY found to be an isotope of thorium: thorium-231, daughter of uranium-235 and parent of protactinium-231. Soon after my paper was published by the Hungarian Academy of Science, Soddy, Hahn, and Meitner also verified Antonoff's results.

Working with Hevesy was an exciting and pleasant experience. There was no pressure, and, although I did not have much experience in radioactivity, Hevesy let me use my own imagination; there was a free flow of ideas. He had brought to Budapest from his stay in Manchester the habit so dear to the English of five o'clock tea. Across from the laboratory was the tea room Gerbaud, one of the best pastry shops in the city. We had a cup of tea and some delightful pastries there in the afternoon, discussing our experiments or theories, or engaging in just plain small talk, which pleasantly relieved the pressure of daily work. This pleasant life ended when Hevesy left Budapest to work at his father's copper plant in northern Hungary. My last contact with him was on his 80th birthday, when I sent him a telegram congratulating him on a lifetime of accomplishments. He was then in a hospital in Freiburg, Germany being treated for cancer. I received in return a lovely, handwritten letter. He died soon after.

Hevesy had a wide cultural background but lacked the sophistication and affectation that were so often found in affluent middle-class families of Europe. He was not autocratic, nor did he feel the need to keep his students in their place. On the contrary, he tried to help them in every way he could. These traits made it very pleasant to be associated with him. His later achievements, the discovery of samarium, hafnium, and other elements are well known from the literature.

After Hevesy left Budapest, a challenging new job was offered to me. Dr. Francis Tangl, a well-known biochemist and physiologist at the University of Budapest, needed a scientist who could set up courses to complement the chemical training of his graduate and postgraduate students. He did not believe that the chemistry a medical student got was sufficient to enable him to deal with the research problems that he was likely to encounter. He offered the job of setting up the necessary chemistry courses to me. That was an unusual thing to do, but, as I found later, Tangl did not shy away from unusual decisions. I was a woman and younger than most of the research scientists whom I was supposed to teach. I had a Ph.D. degree from the philosophical faculty of the university and I was to work with the medical faculty. My duties were to think of experiments, supervise the students, give some lectures, have discussions, make the students' studies easier to comprehend, and help them to carry out research in their chosen field. It turned out remarkably well. I did not have to give up my own research. I had time enough to do my new job and all the material help I needed to buy radioactive equipment and to arrange for the loan of radioactive material.

After Tangl's untimely death from pernicious anemia, the political situation in Hungary changed suddenly and dramatically. Almost overnight the Communists took over. Their leaders, indoctrinated in Russia, took over all the political positions and had enough armed forces to back them up in the takeover. The putsch occurred on my birthday in 1919, and I was supposed to join my family at a matinee of Strauss's opera "Fledermaus." I received a frantic telephone call at the laboratory; by no means was I to go to the opera house; instead, I should go home as soon as possible. I learned later that a group of Communists entered the opera house; everybody had to stand up while they sang the Internationale. One Catholic priest who did not stand was later executed.

A reign of terror followed. Ten Communists took over our apartment, leaving us only one room. The situation was impossible; my mother and I moved into my aunt's already overcrowded apartment. The rate of inflation was soaring; food for nonmembers of the Communist Party was extremely scarce, available only from peasants, who would not take money in payment but who demanded jewelry, fur coats, and other valuables. Communists plundered the homes; whatever money we had we hid under the wood paneling of one room. The reign of terror lasted only a few months and was followed by the equally bloody counterrevolution, the "White Terror." Some of my colleagues joined the Communist Party, for purely idealistic reasons, and the Institute of Physiology and Biochemistry of the University of Budapest was beleaguered by the "White Terrorists"; but the new director, Elemér Poor, stood behind the heavy door and put up a heroic resistance. It became clear that nobody who had had anything to do with the Communists was safe. Very soon the institute was almost depleted of staff, and I was called upon to aid in filling the vacancies, so that the teaching and laboratory exercises could go on, though at a slower pace. As soon as I could, I resigned. Otto Hahn had a grant available for research and offered it to me. I made up my mind that radioactivity would be my career.

Berlin-Dahlem and Vienna

At this time Otto Hahn was the director of the Radioactivity Department of the Kaiser Wilhelm Institute in Berlin-Dahlem. The department was very well equipped then, with the latest instruments. Dahlem was a scientific community, and a wide range of scientific pursuits were being conducted. Fritz Haber headed the Institute of Physical Chemistry at the Kaiser Wilhelm Institute; his weekly colloquia were not only instructive but exciting, especially when he and W. H. Nernst got into heated discussions, which happened quite often. We had an opportunity to hear Otto Warburg talking about the new developments in biochemistry. The most rewarding activity, however, was working with Otto Hahn and Lise Meitner. Meitner was a theoretical physicist at the Kaiser Wilhelm Institute. She came to Berlin from her native Austria (Vienna) to enlarge her knowledge by working with Max Planck (she had been Ludwig Boltzmann's student in Vienna). Before joining Hahn, she had been Planck's assistant. The collaboration of Hahn and Meitner could not have been more fortunate; Hahn's talents in chemistry and Meitner's ability as a theoretical physicist created the combination so necessary for productive research in radioactivity.

Meitner at that time was interested in beta decay. She believed firmly in the simplicity of nature; she was convinced that beta particles, in the same way as alpha particles, must form a group of well-defined energy. Even when the existence of a continuous energy spectrum became established, she felt that such a continuous spectrum must be a

Otto Hahn

Lise Meitner

secondary effect and that the primary electrons left the nucleus with a fixed energy, a varying part of which they then lost in the form of a continuous spectrum of gamma radiation on their way through the strong electric field around the nucleus. C. D. Ellis and W. A. Wooster showed with a microcalorimeter that the average energy lost by each electron in their apparatus was not, as Meitner's views demanded, equal to or above the upper limit of the continuous spectrum but was equal to the mean energy calculated from the spectrum. With meticulous experiments, Meitner confirmed the results of Ellis and Wooster. But she was correct in another respect: by accurately measuring the discrete electron energies of actinium, she showed that the electrons were ejected from the electron shell of the product nucleus rather than from the parent nucleus. This meant that the gamma ray was given off after the radioactive transformation and did not, as Ellis had suggested, trigger it off.

I was given the task of separating thorium-230 from uranium ores. I did not know at that time that much later I would use this experience for a different kind of research, as described in the final chapter. I was fortunate to be able to work in such a stimulating atmosphere. The temperaments and personalities of Meitner and Hahn complemented each other; Hahn was gay and self-confident with a pleasant sense

of humor. It was a pleasure to work with him. He never took it as a deadly sin if one made a mistake or error in calculation. Meitner was an introvert, shy and reserved. It was difficult to have close personal relationships with her, but one had to admire her devotion to her work and her critical search for the right solution to scientific problems. She told me later that, even after a long scientific career, she had continued to experience stage fright at the beginning of a lecture or a speech. Her voice quivered a bit when she started to speak, but soon an eloquency became apparent. Once she became caught up in her scientific theme, she could deliver an interesting and clear lecture. Later she played a major role with O. R. Frisch in explaining fission.

Life at this time in Germany was not easy, and it soon became worse. The housing situation for students and out-of-town research participants was bad. My first experience with the slogan that woman's place was "kitchen, church, children" occurred when I rented a room at the home of a couple, both Ph.D.'s in zoology. The wife was not supposed to pursue any scientific work; she was a kind of maid or slave. I became acquainted with the family of a high school teacher. I observed that the head of the family and the sons had meat at meals but not the women of the family. My next room was at the home of a Lutheran minister, whose manner typified the life philosophy of southern Germany, which was less rigid and less self-centered than that of Berlin-Dahlem's Prussian inhabitants. My walls were covered with holy slogans engraved in wood, such as "You should not honor earthly goods" and the like.

It was a fortunate coincidence that my close friends, the Szegos, stayed on the third floor of the same building. Gabor Szego was a distinguished mathematician. The Szegos immigrated to the United States, and he became chairman of the Mathematics Department of Stanford University, Palo Alto, California. Ann Szego had received a Ph.D. degree from the University in Budapest at about the same time as I. At their home I met John von Neumann, who became very famous later as professor of theoretical physics at the Institute of Advanced Studies in Princeton, New Jersey. He made a very important contribution to the atom bomb. He was at that time a young college student; his father, a wealthy banker, entrusted him to Gabor Szego. The senior von Neumann could not envision mathematics as a career, but his son was so insistent that he finally gave in under one condition: that Szego, whom he admired and trusted, would assure him that his son had real talent. If so, he might go on. Szego assured him that von Neumann was not only talented, but he was a genius.

Conditions in Germany became so bad that only institutions whose research was important to the nation's economy could receive grants. I was transferred to the Textile Institute of the Kaiser Wilhelm Institute. I did not mind getting experience in a completely different field; it was a new challenge. The rate of inflation was tremendous. We received our salaries daily, and we had to spend the money immediately. We frequently pooled our funds and bought some food that was not highly perishable, such as a bag of potatoes. We had to carry million-mark notes in large sacks. If one went by subway in the morning to attend a seminar in Berlin without a return ticket, one could not buy one in the evening because the value of the mark had diminished so much during the day.

When I returned to Hungary to try to obtain a job, I found that the economic situation there was almost as bad. Here, too, industry offered some hope. I gave a lecture at the annual meeting of the Chamber of Commerce in 1927, which was attended by the leading industrialists. My theme was new developments in textile technology. I talked about the instruments which were being developed in Dahlem to test the smoothness, elasticity, and durability of different textiles, and illustrated my talk with slides and graphs. I got a telephone call the next morning asking me to have an interview with the president of one of the biggest mills in Hungary. He explained to me that, because of the country's financial situation, he could not import the raw material that was used to make containers for flour. He owned a property around a shallow lake and swamps, with acres and acres of flax; he had bought a patent from two inventors, and the two were already working on a method to make a burlap-like material with which to sack flour. They were, however, making no headway on their method. What he wanted me to do was to go over the inventors' procedure and make recommendations to improve it. There was already a factory and some kind of work going on full blast. The staff consisted of the two inventors and a director of the factory; the three were close friends and allies. None of them, as I found out very soon, had any knowledge of or experience in chemistry. There was, however, a very able and intelligent lab technician. With his help I set up a small-scale model to determine which method would give the best results. We finally succeeded in making

burlap-like material. In the meantime, we tested pH, the amount of chemicals, the heat needed, and the durability of the product.

At the same time the inventors and the director went on with their useless experiments on a huge autoclave. One day the director gave the signal to open the autoclave before the dial showed zero pressure; a young worker was killed instantly by the escaping steam. At the inquest, all three swore that the death of the worker was brought about by his negligence; his family did not get a penny. That was just too much for me. It became clear that I was wasting my time, and I resigned.

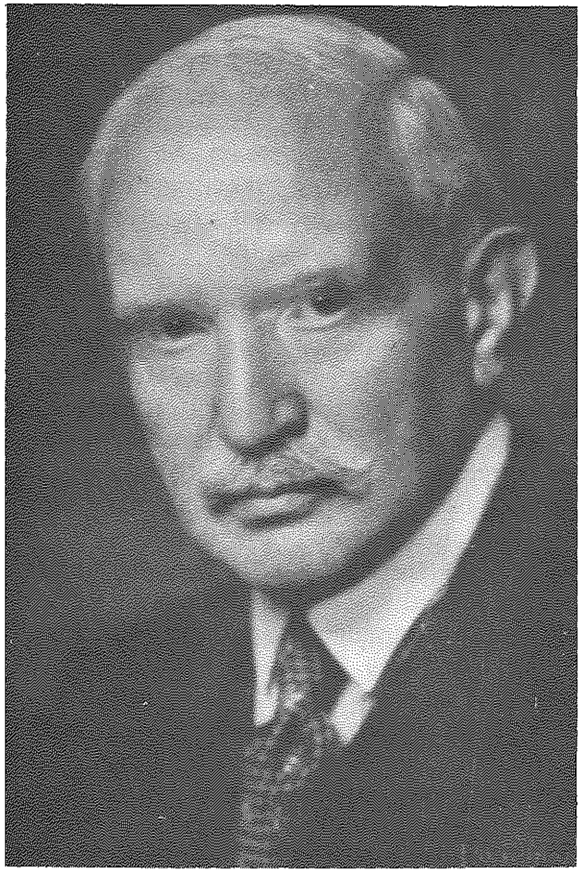
I then rejoined my family at Ischl, an Austrian summer resort. Two days after I had arrived there, someone knocked at the door of the pension where we were staying. I opened the door and saw a square-built, middle-aged gentleman with an engaging smile. He introduced himself as Stefan Meyer, the director of the Vienna Radium Institute. What visitor could have been more welcome! We saw each other often that summer; our common interest was the love of nature. We walked the trails in the woods, scented by wild strawberries and the sweet-smelling wild cyclamen. Finally, the unexpected happened. He asked me to join the staff of the Radium Institute. What a windfall for me to be able to work full time in my chosen field in well-equipped laboratories, with scientists expert in the field of radioactivity!

When I joined the staff, the only office available was that of the retired director, F. Exner. It was given to me under the condition that nothing would be changed or removed, because sometimes Exner paid surprise visits. It was a very comfortable room with a large desk, and on the desk was a collection of pipes of all sizes and types. I often wonder what my visitors, not knowing the history of the room, thought of me when they came into it.

The atmosphere at the institute was most pleasant. We were all members of one family. Each took an interest in the research of the others, offering help in the experiments and ready to exchange ideas. Friendships developed that have lasted to the present day. The personality of Meyer and that of the associate director, Karl Przibram, had much to do with creating that pleasant atmosphere.

Stefan Meyer, one of the pioneers in radioactivity, already had a distinguished career. At the time when I joined the staff of the Radium Institute he scarcely did any research himself, but he did closely follow the research activities of his staff and the students, sometimes with more benevolence than criticism.

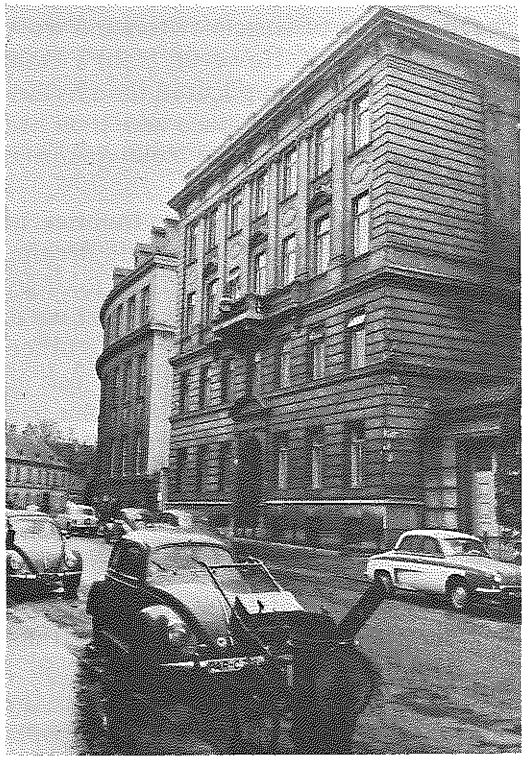
Stefan Meyer



When I joined the staff, I was surprised to find that there was a raging controversy between Gerhard Kirsch and Hans Pettersson of the Radium Institute and Ernest Rutherford and James Chadwick of the Cavendish Laboratory in Cambridge, England. I have to go back a few years in my narrative to explain the situation.

E. Marsden and Hans Geiger, two of Rutherford's collaborators, initiated experiments at the Manchester Laboratory to bombard thin gold leaves with alpha particles. A few of the alpha particles returned toward the source. "It is as if a 15-inch rifle bullet fired at a sheet of paper would bounce back from it," Rutherford wrote to Meyer. The scattering experiments led to Rutherford's theory of the atomic nucleus, one of the most fruitful theories in nuclear science. Geiger and

Marsden's experiments showed that reflections at large angles (well above 90°) occurred quite frequently. Rutherford postulated (in 1911) that the observed large scattering could be



The Radium Institute, Vienna, Austria

produced only by an intense electric field. Consequently, the positive charge and most of the mass of the atom must be concentrated in a very small region, later known as the nucleus. The electrons were thought to be distributed over a sphere of atomic dimensions.

Experimental verification of the scattering formula led to the general acceptance of Rutherford's picture of the atom as consisting of a small positively charged nucleus, containing nearly the entire mass of the atom, surrounded by a distribution of negatively charged electrons. The nuclear charge was also first determined from the scattering experiments. It led to the suggestion that the atomic number Z of an element, indicating its position in the periodic table, was identical with the nuclear charge (expressed in units of electrical charge). Later Henry Moseley confirmed this hypothesis by brilliant x-ray experiments. He identified the atomic number with the charge on the nucleus. This number, which is also the number of extranuclear electrons, was thus shown to be closely related to the chemical properties of an element. Rutherford's idea of the atom was further developed by Niels Bohr. He took the mechanics of Isaac Newton and wove into them the quanta of Max Planck. However, Bohr's atom theory did not apply to atoms more complicated than hydrogen. The description of atomic events in space and time had to be abandoned and be replaced by the new quantum mechanics.

Rutherford was not interested in the new theories of quantum mechanics and wave mechanics of Louis de Broglie, Werner Heisenberg, and Erwin Schrödinger. He once jokingly said: "The theorists play games with their symbols, but we in Cavendish turn out the real solid facts of nature." Rutherford was also interested in the question, "How close can an alpha particle approach the target nucleus?" The alpha particle which bounced back in the scattering experiments from the thin gold leaves came as close as 3×10^{-13} centimeter to the nucleus which scattered it; 10^{-13} centimeter came to be called a "Rutherford unit," but is now a unit of length called a "fermi."

Another puzzle in need of explanation was how a high-energy alpha particle could escape from a nucleus? Rutherford was unable to explain this. The answer came from a young Russian scientist, George Gamow, who visited the Cavendish Laboratory. He explained that the problem could be solved not by classical mechanics but by wave mechanics, which allows the alpha particle to leak through or tunnel through the potential barrier around the nucleus rather than climb over the top, as it would have to do in classical mechanics.

Marsden and Geiger also bombarded organic materials such as paraffin wax, a hydrocarbon, with the alpha particles of the daughters of radon. They found that the particles produced had a longer range than the bombarding alpha particles. Ten years later Rutherford, working with his able technician William Kay, showed that similar long-range particles were emitted when alpha particles were fired through nitrogen gas. He observed the particles on a zinc sulfide fluorescent screen with the help of a microscope. A few long-range particles were seen beyond the range of the alpha particles. These particles were identified as hydrogen nuclei or protons. Prior to this experiment, Rutherford had once written Meyer, "Unless it is possible to transmute matter by the action of alpha particles, we are not likely to see it in our times." Transmutation of elements was an old dream of Rutherford's. He was fond of quoting Michael Faraday: "To decompose metals, to reform them, and to realize the once absurd notion of transmutation–these problems are now given to chemists." This dream came true when nitrogen was bombarded with alpha particles. Occasionally an alpha particle penetrates the nitrogen nucleus, which then breaks up with the emission of a proton and the formation of an oxygen isotope. The reaction can be written:

$$^{14}N + {}^{4}He \rightarrow {}^{17}O + {}^{1}H.$$

Thus the first transmutaton of one element into another was achieved!

Rutherford continued his experiments by bombarding light elements with alpha particles. Meyer believed firmly that the transmutation of elements could be achieved one day, and he was delighted when he received the news that it had happened. He encouraged Gerhard Kirsch, a staff member of the Radium Institute, and Hans Pettersson, from Sweden, who was visiting professor at the Radium Institute, to do research along similar lines. Rutherford and Chadwick had bombarded the light elements boron, fluorine, and sodium with energetic alpha particles of polonium-214 and polonium-212; they tried to bombard heavier elements too. So did the scientists of the Radium Institute, and soon a controversy between the two groups developed. It was at this time that I joined the staff of the Radium Institute. The two groups agreed qualitatively with the results obtained from the alpha particle bombardment of light elements, except those of carbon and oxygen, but disagreed about the disintegration of elements heavier than magnesium. The controversy became more and more bitter.

Pettersson made many improvements in the counting technique. In the technique used by Rutherford the alpha particles from the radioactive source were aimed directly at the target material. A microscope was used to count the scintillation of the particles produced by the bombardment on a zinc sulfide screen. With this method the protons that were produced from the humidity or other contaminants were also counted. Pettersson used the "retrograde" method. The transmutation protons were counted at a large angle to the angle of the bombarding alpha particles (around 180°). The stray protons were not counted because they were emitted at an angle of less than 90°.

This method was later adopted by Rutherford and his group. Hans Pettersson also used a much more powerful microscope. But the differences between the two groups became wider in two respects. They continued to disagree about apparent disintegrations of elements heavier than magnesium, and there was also a severe discrepancy in the yields; those found by the Viennese group were considerably higher than those of the Cavendish scientists. Whereas the improvement in counting equipment reduced some of the error, the subjective errors remained. Students and staff members with good eyesight were chosen to count the scintillations; errors from hallucinations resulting from long counting sessions in a dark room and fatigue could not be eliminated. A possible visit by one of the groups to supervise the experiments in the laboratory of the other group was frequently discussed in the Rutherford-Meyer correspondence. Finally Chadwick came to Vienna. All of us sat in a dark room for half an hour to adapt to the darkness. There was no conversation; the only noise was the rattling of Chadwick's keys. There was nothing in the situation to quiet our nerves or make us comfortable. Short spells of scintillation counting followed for each member of the group, and then the radiation source was exchanged with a blank, unknown to the persons who were doing the counting. The impression made on us by Chadwick in this short visit was not favorable. He seemed to us to be cold, unfriendly, and completely lacking in a sense of humor. Probably he was just as uncomfortable in the role of judge as we were in that of the judged. I learned later that his ordeal in a concentration camp in Germany during World War I had much to do with his behavior, and those who knew him better reported that he was a warm and kind person.

As far as I know, the discrepancies between the two laboratories were never resolved. They were due to differences in equipment and methods. It was still a time of pioneering. Neutrons had yet to be discovered. It is possible that energetic alpha particles reacting with light elements created neutrons which produced secondary proton emissions. The higher yields of Pettersson and Kirsch could have been brought about by long-range alpha particles from their radiation source.

A big improvement in experimental accuracy occurred with the construction of the cloud chamber by the Scottish scientist C. T. R. Wilson. This apparatus made it possible to observe and photograph what is happening during a nuclear

transformation. The sudden expansion of humid air causes a mist to form on the ions along the paths of swift alpha and beta particles. The expansion is brought about in a cyclic way. An arrangement consisting of a light source, mirror, and camera allows one to photograph a fresh track at each expansion.

The long friendship between Rutherford and Meyer did not dim even though there were discrepancies in their respective scientific results. The same warmth and kindness in their correspondence ended only with the death of Rutherford. It is our great good fortune that Mrs. Rutherford carefully preserved the incoming and outgoing letters. The book "Rutherford" written by A. S. Eve, a close friend of Rutherford's, is based on that correspondence. Meyer carefully preserved the letters that he received and copies of those that he wrote. He let me read them. This collection is a treasure, as it provides a history of radioactivity and nuclear science, and a close look at the scientists who made the history, almost from the beginning. We learn from the correspondence how anxious Rutherford and Meyer were to further each other's research.

Rutherford had at his disposal only 17 milligrams of radium when he started his radioactive experiments in Manchester. He applied to the Academy of Science of Vienna for 0.5 gram of radium. The Academy agreed to lend him 350 milligrams of radium bromide which had to be shared with William Ramsay at the University College, London. Difficulties arose. Ramsay wanted to have sole custody for 1½ years and to send to Rutherford radon only. Ramsay already had 150 milligrams of radium. Rutherford was very bitter about this, and a not altogether pleasant correspondence followed. Meyer was anxious to see Rutherford's work proceed without hindrance. Through his intervention, the Academy of Science of Vienna gave Rutherford 170 milligrams of radium as 300 milligrams of radium bromide with no strings attached. Never was a sample so well used. It gave Meyer great satisfaction to be able to help Rutherford in his great achievements. Rutherford repaid this great favor. The economic situation in Vienna during the first few years after World War I was very bad. Meyer wrote about it to Rutherford, complaining that the scientific work at the Radium Institute had come almost to a standstill for lack of funds. Rutherford, with his usual energy, initiated efforts to do something about it. The Royal Society granted him a sum of several hundred pounds to purchase part of the radium that had been loaned to him. This news was received at the Radium Institute with joy and hope; spirits were high again. Payment from the Royal Society was spread over several years until finally all the radium, initially loaned to Rutherford, had been paid for.

The magnetic personality of Hans Pettersson attracted several able scientists to the Vienna Radium Institute, and some valuable work was done. His spirit of fair play, enthusiasm for scientific ideas and research above personal ambitions influenced his collaborators and created a pleasant atmosphere. Pettersson had a talent for obtaining grants for the scientific research of the Institute. One of the largest donations came from the Rockefeller Foundation, but he also received grants from the Nobel Foundation and some private donations.

A discovery of Pettersson's led to a great improvement in the transmutation experiments. He found that alpha particles with energies as low as 5 MeV (MeV = million electron volts; 7 and 8 MeV had been used before) are able to produce transmutations. This finding opened the door to the use of polonium-210. This polonium isotope emits alpha particles only, and decays with a comfortable halflife of 138 days to the inactive lead-206. The parent of polonium-210 is the lead-210 isotope, which has a half-life of 22 years. It can be extracted from radium or uranium residues. A source used was spent radon needles, received from hospitals. To prepare polonium sources, it is not enough to separate polonium from other elements; the polonium must also be radioactively pure and concentrated on a small surface. By using polonium-210 as an alpha source, one can avoid the short half-lives and the luminescent background of the zinc sulfide screen caused by beta particles.

Both institutes were anxious to use this radiation source. The Cavendish Laboratory had no radiochemist. Before he left Vienna, Chadwick extended Rutherford's invitation to me to join the staff at Cavendish Laboratory. How challenging this invitation was! However, I decided to stay at the Radium Institute to prepare radiation sources for the needs of my colleagues' research and my own. The logical place to learn how to do this was at the Curie Institute in Paris.

Paris

I came to Paris in 1928. On a quiet street in the XVI Circuit on the Left Bank of the Seine was Pierre Curie Street and the Curie Institute. Behind the building was a small garden. Both Mme. Marie Curie and her daughter Irene liked flowers. A balcony faced the garden, and that was the place where Frederick Holweg, a senior scientist at the Curie Institute, took a snapshot of Mme. Curie without her knowledge, which he later gave to me. I cherish that small photo, not only because it pictures her just as I knew her but also because Holweg, a close friend of mine, gave it to me. During the German occupation of Paris, Holweg was shot to death because he would not give away the secret of an automatic weapon that he had developed.

I was thrilled by the prospect of meeting Mme. Curie but I also had some misgivings. I believed that such a famous person, like many of the European professors, would live in an ivory tower and could be approached only by appointment. When I entered the laboratory, however, I saw a slight, graying lady in a lab coat working among the students. From then on, I saw her among us all the time. When her co-workers and students went to lunch, she often stayed in the laboratory. Gourmet food is very important to the French. They start by consulting the chef and choose the specialty of the day. The students could afford this arrangement because many good but inexpensive bistros were located near the Institute. But frequently Mme. Curie's lunch consisted only of a piece of bread, which she took from her lab coat pocket.

Mme. Curie's lectures were very scholarly but were presented in a low and monotonous voice. It was difficult for the students to concentrate, because her lectures were presented immediately after lunch.

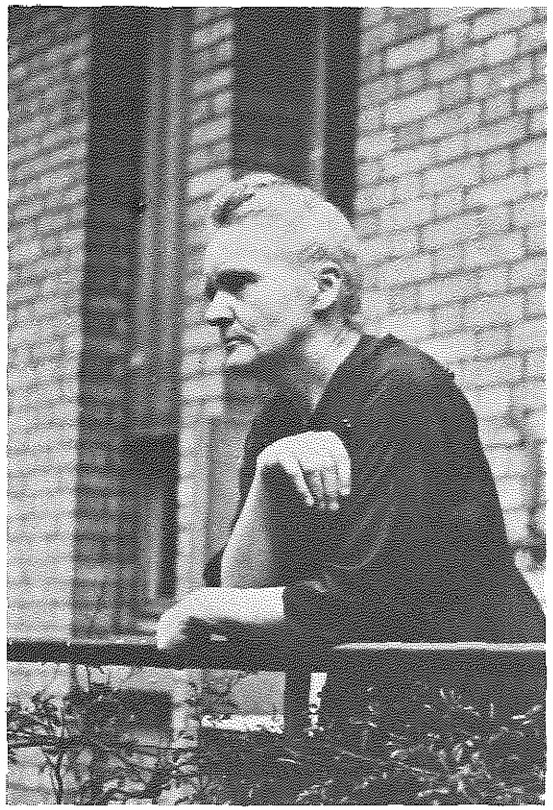
The Curie Institute, Paris

We were so impressed by Mme. Curie's ascetism that an episode, showing her human traits, surprised us. One of our colleagues handed some candies around. Irene Curie, who worked with her mother, took two and gave one to her mother; a little later she asked for more because Mme. Curie liked them so much.

Many of the undergraduate students, postgraduate students, and visiting scientists at the Curie Institute, were from Poland, Mme. Curie's native country, and quite a few were women. What impressed me most was that, each time there was a dangerous experiment, she carried it out herself. As I was sent by her old friend, Stefan Meyer, she tried to teach me as much as possible. One day she asked me to come to the laboratory the following Saturday afternoon but not to tell anybody. The project was to open a flask containing a solution of a strong radium salt, which had been closed for many years. It is well known that under the intense radiation the solvent water is decomposed and hydrogen peroxide accumulates, so that, if proper precautions are not taken, there is a violent explosion. That was just what happened on that

memorable afternoon. After Mme. Curie scratched the neck of the glass flask with a file and approached a narrow flame, a violent explosion scattered glass all over. It was a miracle that we were not hurt or highly contaminated. Mme. Curie was not a highly skilled experimenter at that time; that was probably due to her severely burned fingers, from her long work with radioactive materials. At that time she prepared secondary standards, weighing the radium on a microbalance, without any protection against the intense radiation. I still wonder that she lived to be 67 before finally succumbing to pernicious anemia.

Mme. Curie was extremely reserved. Even after I had been in Paris for several months, my only contact with her was professional. The day before my departure her extreme reserve broke down. She talked about her long friendship with Meyer. It is well known that tons of pitchblende from

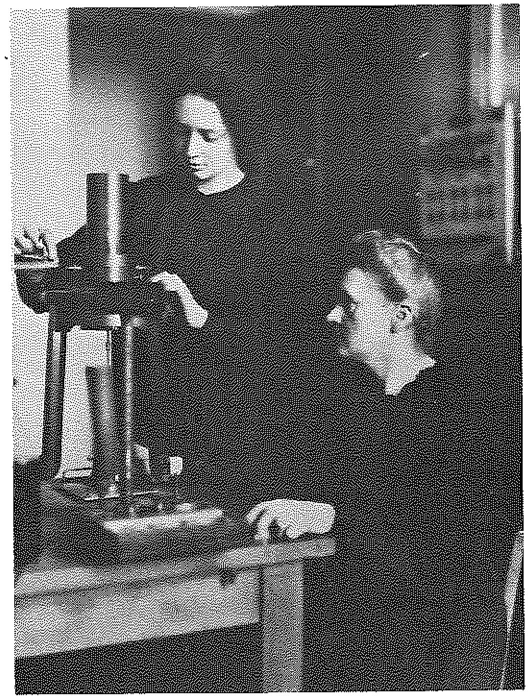


Madame Marie Curie

Joachimstal, Czechoslovakia (at that time part of Austria), came to her through his intervention. She extracted polonium first, then radium. However, it is generally not known that Meyer was a great help to Mme. Curie during and immediately after World War I in obtaining news from her family in Poland. Poland and Austria were on the opposite side from France. Through diplomatic channels and some maneuvering, Meyer got news from her family to Mme. Curie. He was also able to send them food packages, which saved them from starvation. She told me all this and gave me a beautiful photograph of herself with Irene Curie for the Radium Institute of Vienna.

I admired Mme. Curie's modesty, and I thought of Einstein's remark: "Mme. Curie is the only person I know who is completely unspoiled by fame." Eve Curie in the biography of her mother writes: "She never learned to be famous." Rutherford attended one of the Solvay Conferences and wrote that Mme. Curie was also present in her modest and noble self.

Irene Curie was the expert on polonium. She had recently married Frédéric Joliot. Her official position at the Institute



Madame and Irene Curie

was assistant to Mme. Curie, which many regarded as favoritism on the part of Mme. Curie because she chose Irene over scientists higher in rank and older. The position as assistant

was somewhat awkward for Irene also. But the two worked well together, shared all the excitement and disappointments of their work. Their correspondence is ample proof of that. Nothing shows the closeness of mother and daughter and their deep love and tenderness better than their correspondence. Included is correspondence beginning from when Irene was 8 years old and continuing until the death of Mme. Curie in 1934.

I was attracted to Irene Curie from the start and appreciated those character traits that her lady colleagues disapproved of: her disinclination toward gossip and small talk, her extreme seriousness, her reserve, and her disregard of her appearance, all of which they regarded as manifestations of arrogance. The longer I knew her, the more I saw her as a warm, candid, even romantic, person. She liked poetry; her favorites were Heine, whose poetry she read and recited in the original German, and the French romantic Beranger. Her love of nature and the outdoors and sports attracted me most.

I was assigned to work with Irene Curie, and I followed her method of preparing polonium sources. Nothing was basically new in her method. Before this time, George von Hevesy and F. Paneth had used electrolysis to separate polonium from lead-210 and bismuth-210. Irene Curie used basically the same method. She used platinum and gold electrodes and a weak solution of nitric acid. Polonium was then dissolved from the metal and deposited on a silver film, which was rotated in the nitric acid solution. I was supposed to use her method and attain a high yield (almost 100%) as she did, but I never was able to do that. It was impossible to prepare the high concentrations of polonium that were needed for nuclear experiments. The deposition of polonium on a foil was limited to a mono layer; once this layer had been deposited, no more polonium would deposit. A different technique had to be used to increase the concentration; one possibility was distillation. Whenever a platinum cathode became saturated with polonium, it was replaced with a new electrode until the solution was free of all polonium. When a modified method of Paneth and Hevesy was used, a palladium foil of the desired shape and dimension was inserted into the narrow part of a quartz tube. The foil was held by a rectangular copper piston, whose end was immersed into liquid air. Strips of the platinum electrode saturated with polonium were heated to about 900° C, and a stream of hydrogen gas carried polonium to the cold palladium foil in the quartz tube, which stayed in place until the platinum electrode was replaced with a new electrode. Consequently, the polonium concentration could be increased on a small piece of palladium foil.

Polonium on the head of a pin was a strong enough source to be used as an alpha particle source for biological experiments of single cells. An amusing episode occurred much later, during my stay at the Argonne National Laboratory in Chicago. A biologist from the Medical Division needed a polonium source for his single-cell experiments. Knowing he could get a polonium source from Dr. Rona, he asked his colleagues where in Europe she could be contacted. He was told, "She is in the next room."

The Curie Institute was highly contaminated. The staff was more concerned with the safety of the radium sources than with their own. Mlle. C. Chamie, who became my friend, was the custodian of the radium preparations. It was her duty to get the preparations out of a safety box in the morning and return them in the evening. A small cart with some (but not enough) lead bricks around the radium preparation was used, which she pushed to and from the safe. We left together at the end of the work day because we did not live far from each other. But each evening I had to wait outside, because she felt the need to go back to see whether she had really returned the radium preparation. The fear of losing some of the precious material certainly contributed to the high toll which overexposure took of the scientists at the Curie Institute. Health physics was nonexistent in that day, and the radiation dosage to which one was exposed was unknown.

Another friend of mine at the Curie Institute was Mme. Cotelle. She was pretty, charming, and a competent scientist. She worked on the chemistry of polonium, and one day in pipetting a polonium solution she inadvertently swallowed some. When I was still at the Curie Institute, she was already worried about it. Some time later symptoms of overexposure developed: loss of hair, stomach troubles, weakness. Irene Curie corresponded with her mother about the problem, and they suggested that rest would be advisable. They thought maybe the solution which Mme. Cotelle used lost polonium to the air or possibly she had swallowed some, which of course she had. This was at a time when the danger of radiation overexposure should have been well known.

During my stay in Paris, I tried to meet André Debierne. He had worked with the Curies almost from the start and had discovered actinium in 1899 and a little later, almost simultaneously with H. Geitel, actinon, which had an extremely

short half-life. After the death of Mme. Curie, he succeeded her as director of the Curie Institute. When I asked for an interview with Debierne, then a senior member of the scientific staff, I was met with incredulity. They would say, "One does not meet with Debierne, one does not ask questions of Debierne." I found out later that his personality changed after World War I, from an amiable person to practically a recluse. My quest for information on actinium was unsatisfied.

When I returned to the Vienna Radium Institute, the demand for polonium sources was high and this was not to my liking, because it took too much of my time. In that day, before accelerators were built, polonium was the only pure source of alpha particles. As I mentioned above, it had to be separated from uranium residues or old radium salts, a lengthy procedure and a health hazard. This was especially true of its preparation from 1 gram of radium at the Radium Institute, which was "milked" for radon from time to time, before the polonium from the solution was precipitated with hydrogen sulfide. The solution was not properly shielded. My colleagues and I were greatly concerned about health hazards from the high contamination in the Radium Institute.

When I first arrived and set up my counting equipment, I noticed that the background counts were extremely high. When I asked about this, I was told that Otto Honigschmidt used this gram of radium to carry out his atomic weight experiments. Later, when I attended one of his lectures, I understood why that room was so contaminated. To homogenize the radium solution, he shook it by hand, unprotected. It was not surprising that he died of lung cancer a few years later.

Ernest Rutherford had an intuitive sense of scientific discoveries that would be made. He foresaw the existance of the neutron many years before it was discovered. In his second Bakerian lecture in 1920, Rutherford said: "It seems very likely that an electron can bind two hydrogen atoms, which entails the possible existence of an atom of a charge of one and a mass of two [deuterium, which was discovered very much later by H. C. Urey] or one electron combined with one hydrogen ion. This involves the idea of the possible existence of an atom of mass one, which has zero nuclear charge. Such an atom would have novel properties. The external field would be practically zero, and in consequence it would be able to move freely through matter. It would enter readily into the structure of atoms and may either unite with the nucleus or be disintegrated by its intense field."

In 1930 two German scientists, W. Bothe and H. Becker, found that a penetrating radiation was produced when they bombarded boron with a source of alpha particles. This radiation was so strong that after penetrating 10 centimeters of lead its intensity was scarcely reduced. They wondered whether a new type of radiation was being emitted in this reaction as gamma radiation is emitted by radium. They asked, "Under what conditions was the radiation emitted? Was it a primary effect, or an effect due to an intermediate reaction of the atomic nuclei?"

The Joliot-Curies repeated the experiments of Bothe and Becker. They irradiated beryllium with the alpha particles of a strong polonium source. The experiments were carried out in an ionization chamber. In order to be quite sure that they were dealing only with the very penetrating radiation, they first passed it through 15 millimeters of lead before allowing it to enter the ionization chamber. They interposed sheets of paraffin wax absorbers in the beam entering the ionization chamber. The radiation in the chamber should have diminished; in fact it increased (a phenomenon later explained by Enrico Fermi). They also observed that high-energy electrons were emitted simultaneously. They decided to use a cloud chamber to observe and photograph this radiation and to identify it. They were able to establish that the Bothe-Becker radiation was capable of colliding with and scattering nuclei of hydrogen, helium, or nitrogen. They published their results in January 1932. A month later James Chadwick identified the penetrating rays as neutrons.

The Joliot-Curies missed the discovery of the neutron by a hair. Joliot stated that, if he and Irene could have read Rutherford's Bakerian lecture, they would not have missed identifying the neutron. Joliot added that he seldom read lecture notes because he seldom found anything in a lecture that was not published elsewhere. Chadwick's discovery was not by chance. He was equally as obsessed with the idea of the neutron as Rutherford. He had available a linear amplifier with which he was able to measure individually the impulses produced by the knocked on protons, and to separate these impulses from those caused by the electrons. He described the new particle, the neutron, as a chargeless particle with a mass about that of hydrogen, which goes through all matter, but which is slowed by hydrogen in the paraffin wax absorbers.

Soon after Chadwick's discovery of the neutron on a visit to Bohr's laboratory, Rutherford gave two lectures in Copenhagen. He stated: "If a neutron hits an oxygen atom, it is transmuted to carbon; if it hits nitrogen, it transmutes it to boron:

$$^{16}_{8}O + ^{1}_{0}n \rightarrow ^{13}_{6}C + ^{4}_{2}He; \quad ^{14}_{7}N + ^{1}_{0}n \rightarrow ^{11}_{5}B + ^{4}_{2}He$$

Again, if neutrons strike lithium, some few will be captured. In that case the lithium atom splits into two fragments which part with great velocity." (Later, two scientists at the Cavendish Laboratory, Sir John Cockroft and E. T. S. Walton, constructed the first nuclear accelerator, hurling 600,000-eV protons into lithium, splitting it into two alpha particles).

Two years later, the Joliot-Curies made a startling discovery. This time it was by chance. For a period of time they had carried out transmutation experiments with alpha particles from a strong polonium source on different target materials. They used Geiger counters and also often cloud chambers. The latter were Joliot's favorite instrument. He once said: "In this chamber an infinitely tiny particle reveals its own trajectory, in a succession of drops of condensation." He always had several of them in the laboratory, and enjoyed spending hours observing the trajectories in the instruments.

It was New Year's Eve. The Joliot-Curies were finishing up an experiment with the cloud chamber. The chamber's window was covered with a thin aluminum film; the alpha particles entered the chamber through the aluminum window. The Joliot-Curies were ready to leave after the day's work to spend the evening with friends. They were called back by their assistant, a German scientist named W. W. Gentner, who had started to dismantle the equipment. He observed that, after he removed the radiation source, he could observe beta tracks; these disappeared after a short time. The first reaction of the Joliot-Curies was that the observations were caused by contamination. They repeated the experiment, surrounded the chamber with a magnet, and removed the alpha source. The trajectories of the electrons were still visible, slowly decreasing in number with a half-life of 3.5 minutes, and the charge on the rays was positive. The following reaction had taken place:

$$^{27}_{13}Al + ^{4}_{2}He \rightarrow ^{30}_{15}P + ^{1}_{0}n; \quad ^{30}_{15}P \xrightarrow{\beta^+} ^{30}_{14}Si$$

The beta particles were identified as positrons. P. A. M. Dirac had predicted these particles, and later the American physicist C. D. Anderson discovered them in cosmic radiation. The Joliot-Curies replaced aluminum with boron, with the same result, except that the half-life of the product radioactive element was 14 minutes:

$$^{10}_{5}B + ^{4}_{2}He \rightarrow ^{13}_{7}N + ^{1}_{0}n;\ ^{13}_{7}N \xrightarrow{\beta^+} ^{13}_{6}C$$

It was about midnight when Mme. Curie and Paul Langevin arrived to witness the new phenomenon. The next step was to separate the radioactive element produced. Joliot with his usual skill separated the radioactive phosphorus-30 from the bombarded aluminum. Joliot reported that he remembered with pride and affection the expression of intense joy that came over Mme. Curie's face when she held the first artificial radioactive element to the Geiger counter, to hear the crackling of the rate meter. This may have been Marie Curie's last visit to the laboratory. She would be dead a few months later.

In October 1932, the Joliot-Curies attended the Solvay Conference. These conferences were arranged annually by the Solvay Foundation in Belgium, and were attended by famous scientists from around the world. Present were Mme. Curie, Paul Langevin, Niels Bohr, André Debierne, Enrico Fermi, Louis de Broglie, Wolfgang Pauli, the Joliot-Curies, Otto Hahn, and Lise Meitner. The Joliot-Curies presented the results of their experiments, that neutrons and positrons were emitted simultaneously. The account aroused animated discussions with especially heated arguments from Meitner. She said she had carried out similar experiments but never observed anything but the reemission of a proton. The Joliot-Curies were depressed.

Two years later the first report of artificial radioactivity by the Joliot-Curies was also met with skepticism. Bohr wrote to Rutherford: "In a letter from Mme. Joliot, she says she thinks that she has evidence of electron emission under the influence of alpha particles on beryllium, but I suspect that the beta particle tracks on her photographs are due to Compton effects in the atoms of the wall of the Cloud Chamber."

Stefan Meyer was less skeptical. During his whole scientific career, he had dreamed of artificial radioactivity. After receiving from the Joliot-Curies a telegram announcing their discovery, he invited them to give a lecture in Vienna, and so I had the opportunity to hear a first-hand report about this fundamental discovery, which was to have such far-reaching consequences for different branches of science.

The talk was given by Irene Joliot-Curie. A reception followed at the French Embassy. The visit of the Joliot-

Curies gave me an opportunity to know them better. I could see that both were depressed; something was very much on their minds. This was when I learned that Mme. Curie was very sick; the long exposure to intense radiation was taking its toll. She was sent to Switzerland in the hope that the high altitude would improve her pernicious anemia.

Berta Karlik, who spoke French fluently, and I were in charge of entertaining the Joliot-Curies. We knew that both were fond of the outdoors and were happiest walking in the woods. We hiked for hours in the Vienna forest. Irene Joliot-Curie slowly relaxed and opened up, and talked and talked about things that were on her mind. During my busy days in Paris I had had no time to talk with her of anything but science. On this occasion I came to know her as a compassionate but somewhat naive person. It was the time when most of the European countries were in a turmoil, plagued with inflation and food shortages. The possible spread of Fascism was very much on her mind. She was very bitter against America, where the farmers destroyed their potato crop rather than selling it for a lower price. She talked at great length. We had dinner on the top of a hill in Schonbrun, where the castle of the Austrian emperors is located, and enjoyed the slowly fading colors of the flowers as night fell.

Before the Joliot-Curies left Vienna, they invited me to come to Paris and work with them on problems of artificial radioactivity. I gladly accepted.

The atmosphere of the Curie Institute then was quite different from what it had been during my first visit. No work was going on. Frederick Holweg, my close friend, told me "la patrone" was dying. I would like to say a few words about Holweg: He was the senior scientist at the Curie Institute when he was bypassed for the leading position, which was offered to Irene Curie. It was a hard blow for Holweg, but his worship and love for Mme. Curie continued. He went on with his work on radioactivity and instrumentation, but he also worked with Lacassagne at the Pasteur Institute on biological problems, using radioisotopes.

The last wish of Mme. Curie was to be buried in the cemetery of Saeux near her husband. I did not go to the funeral. I understood that Irene Joliot-Curie wanted only the family to be present. This is why I did not present the wreath of red roses from the Vienna Radium Institute myself. Holweg asked me the next day why I was absent. I gave him my reason. "You are one of the family," he said.

I visited the Curie Institute later, in 1957 and 1968. In

1957, I attended the meeting on Radioactive Isotopes in Research. The participants were welcomed by Frédéric Joliot; (Irene Joliot-Curie had died from radiation exposure in 1956). I did not know at that time that this was to be the last time I would see him and talk to him. He died in 1958. He recognized that his wife's final illness was caused by radiation exposure but did not believe that his liver ailment was due to the same cause.

At the time of this visit to the Curie Institute, it had become clear that space was necessary for the many scientists who were working in the field of nuclear science. Since research is best done in a quiet atmosphere, amid forests and greeneries, the site chosen for the new laboratory was at Orsay, near Paris. Joliot worked hard on the project and saw it completed before his death.

In 1967, prominent nuclear scientists from all nations and the former students of Mme. Curie were invited to Paris to celebrate the anniversary of her 100th birthday. We were guests of the French government. Work in the new laboratory was already in full swing. Helen, the daughter of the Joliot-Curies, herself a nuclear scientist, was our competent guide. It was a pleasure to know her and talk with her and with her very congenial husband, Paul Langevin, son of the famous theoretical physicist and longtime friend of the Curies.

There is an old restaurant on the left bank of the Seine, La Closerie des Lilas. Mme. Curie used to have lunch there with her students. Only her old students were invited this time. It was a nostalgic reunion, because many of our old friends were missing; a list of their names was read; it was quite a long one. The name of one of my old friends, whom I had looked forward to meet, was that of S. Rosenblum. I was told that he had committed suicide by throwing himself below an oncoming train and had been decapitated. Nobody could guess the reason. He had always exhibited a good, happy disposition. During my visit to the Curie Institute in 1957, he proudly showed me in his Bellevue Laboratory the energy spectrum of the alpha particles of polonium-214 and polonium-212, which he had discovered with his powerful electromagnetic spectrometer.

Until 1929, it was thought that each alpha-emitting species had only one alpha particle energy associated with it. Because an alpha particle loses only a very small fraction of its energy in a single collision, the paths of alpha particles are nearly straight lines. The ranges of all alpha particles of the same initial energy are approximately the same. Irene Curie's first

experiments consisted of the determination of the range of alpha particles of radioelements, especially those of polonium-210. She constructed many curves of the range of alpha particles versus distance from the source, and Bragg curves showing the number of ion pairs per millimeter of air against the residual range in centimeters. But she accepted the discovery of the alpha particle "fine structure" by Rosenblum with enthusiasm. In March 1929 Irene Curie wrote to her mother to prepare the strong actinium source to produce actinon for Rosenblum's experiments of long-range alpha particles. A few months later Mme. Curie wrote back that she had seen excellent photographs of the alpha particle fine structure of bismuth-212 and that Holweg was enthusiastic too. The origin of these "long-range" alpha particles can be explained in the following way. Some of the excited states of polonium-212 and polonium-214 are so unstable with respect to alpha emission that alpha decay from these states occurs before the deexcitation by gamma emission can occur. Rutherford received the news of Rosenblum's discovery with pleasure, though the simplicity of the alpha particle emission no longer could be accepted.

During the Curie Centenary celebration in 1967 (my fourth visit to the Curie Institute), I renewed my acquaintance with my old friend Ellen Gleditch. She had worked for 5 years with Mme. Curie and was one of the Curie family. At an earlier meeting, she had been presented with the key to the city of Paris. Her main interest was geochronology, based on the use of uranium and lead. She started these experiments in B. Boltwood's laboratory at Yale University. From La Closerie we walked to the Pantheon and parted with sadness; we felt that we would not see each other again.

Cambridge and Fission

The time after the discovery of the neutron and artificial radioactivity was exciting and busy. Scarcely a day went by without a new radioactive isotope being announced in the literature. That was approximately the time when I visited the Cavendish Laboratory. One of the scientists at the Vienna Radium Institute received a grant to work at Rutherford's laboratory. She stayed at the famous Girton Women's College, where I also was invited to stay during my visit to Cambridge. The college was very conservative, holding strictly to conventions and traditions. My friend was very anxious to live up to these standards and was afraid that I might make some departures from the accepted etiquette. We assembled before dinner in evening dresses and waited for the headmistress to arrive, then we went in single file to the dining room. As a guest, I was seated at the high table. Mutton was served. Because it was very dry, I started to reach for the mustard. My friend nervously whispered into my ear: "Only a moron eats mustard with mutton. Mint sauce is all right, naturally."

I went to see Ernest Rutherford and found him in his favorite laboratory in the basement. There were other people in the laboratory, but it seemed that only Rutherford was there. His booming voice, his liveliness, his sparkling blue eyes filled the room. I remembered Niels Bohr saying that what startled him most was Rutherford's simplicity—simplicity in his life, simplicity in the way he viewed nature, which made it possible for him to make discoveries where others before him had not been able to. A talk with him was a refreshing experience. His appearance was unassuming. A *New York Times* reporter declared once that he had an extraordinary stranger as a table companion. He remembered him because of the unusual range of conversation. He described the man as large and heavily built, with a shaggy, reddish mustache, altogether a most unscholastic figure. "Who was that Australian farmer who sat with me?" he demanded. "That was Lord Rutherford," he was told.

When I got over my first feelings of awe, I looked around Cavendish Laboratory. I remembered E. Marsden's remark, "I was astonished that someone was not hurt by the radioactivity which was all around the lab." It seemed to me that there was more danger of electrocution—the high-voltage wires hung low. The instruments looked primitive, self-made. Rutherford is quoted as having said, "I can do research at the North Pole." At the time of my visit, he no longer had to rely on sources of natural radioactivity. The Cockroft-Walton accelerator was being used to transmute several elements with protons. Cockroft assisted Peter Kapitza, a Russian scientist who worked for quite a time at the Cavendish Laboratory, building bigger and bigger magnets. Kapitza also designed new forms of liquefiers for hydrogen and helium so that he could carry on his magnetic experiments at very low temperatures. In this very strong magnetic field he repeated S. Rosenblum's experiments and improved them by obtaining better resolution of the fine structure of alpha particles. I was very much interested to see these experiments, after having seen those of Rosenblum. Kapitza's magnetic and low-temperature laboratory had been especially built for him with a grant from the Royal Society. But not everything was going well. Kapitza had been absent from his native country, Russia, for 13 years, and he was summoned home and later was forcibly detained in Russia. Naturally we were alarmed by these developments. Kapitza wanted to establish residency in England, at least for his children, and tried to achieve that by the purchase of real estate. I missed further happenings because I left Cambridge, but I heard that he left England and that, after some negotiations, the Soviet government purchased Kapitza's apparatus from the Cavendish Laboratory.

With the discovery of the neutron and artificial radioactivity, feverish activities started up in all the laboratories concerned with radioactivity. A new group joined the field, that of Enrico Fermi and his co-workers at the University of

Rome. The attention of the scientists was directed toward neutron reactions with highly charged nuclei. As the neutrons have no charge, they can penetrate the potential barrier, even of highly charged nuclei. The Fermi group bombarded practically all the elements of the periodic system and soon made a chance discovery. They had been using a Geiger counter, resting on a wooden table; the neutron source and the target material were mounted on a wooden stand. For some reason or other, when the whole arrangement was changed to a metal one, the counts diminished. This did not go unobserved. They inserted between the neutron source and the target material hydrogen-containing organic materials; each time they did this the counts increased. Fermi explained this phenomenon in the following way: When a neutron hits moderately heavy or heavy elements, it loses relatively small amounts of energy. When a neutron hits elements of mass comparable to its own, it loses energy by elastic collisions and slows down quickly. In collisions with hydrogen (approximately the same mass as the neutron), the kinetic energy transferred in the elastic collision is the greatest. Slow neutrons are also called thermal neutrons, because they are slowed down to approximately the energies of gas molecules in thermal equilibrium at ordinary temperature.

Slow neutron reactions proceed through the formation of compound nuclei. Actually, Niels Bohr developed the compound nucleus model especially to explain these reactions. Since the excitation energy of a compound nucleus is only a little greater than the binding energy of the captured neutron, it takes a relatively long time for a fluctuation, which concentrates the escape energy back on a neutron, to occur, and there is a greater probability that the excitation energy will be emitted as a gamma ray. Slow neutrons can induce nuclear reactions at very low kinetic energies, since there is no Coulomb barrier to surmount. The probability that a nuclear reaction will occur is high; its cross section is large. One day Fermi, who had moved to the United States some time before, was strolling with a friend in the outskirts of Chicago. As usual, the friends were talking shop. They passed by a barn, and Fermi exclaimed, "The cross section of a slow neutron is as big as a barn." When a neutron is captured by a nucleus, a nucleus with an atomic mass one unit greater than before is formed. The barn is now accepted as a unit of cross section (10^{-24} square centimeter). The cross section for collisions with fast particles is never larger than 10^{-24} square centimeter (the radii of the heaviest nuclei are approximately 10^{-12} centimeter).

The next question was obvious. What happens to elements that are already radioactive if they are bombarded with neutrons? Experiments along this line were taken up by Fermi's group and by Otto Hahn and Lise Meitner, and later by F. Strassmann, the Joliot-Curies, and our group in Vienna. Before we began these experiments, we had been bombarding rare-earth elements. The Vienna Radium Institute had a collection of rare earths, which had very little radioactive contamination. This material had been given to the Radium Institute by Auer von Welsbach, the developer of the gas mantle. We discovered a thulium isotope, thulium-171, with a half-life of about 3 months, a europium isotope with a half-life of 60 minutes, and in addition to the rare earths a cesium isotope with a half-life of 2 years. These were the longest artificial half-lives known at that time. We were fortunate in this work because the Radium Institute had strong neutron sources. Professor Stefan Meyer was a collector; he possessed secondary standards, kept in a safe. We finally persuaded him to let us use them for neutron sources. At that time they consisted of a mixture of radium or polonium and beryllium. Because the alpha particles of the range of radium and polonium are short, an intimate mixture of radium or polonium and beryllium powder was necessary. Preparing the mixture was a dangerous operation, not only because of the threat of radiation but also because of the possibility of inhalation of beryllium powder, which can cause lung lesions similar to silicosis or black lung.

We soon joined the other laboratories in bombarding radioactive material. The Fermi group and Hahn, Meitner, and Strassmann used uranium as a target; the Joliot-Curies used uranium and also thorium. We chose thorium. We had a thorium source which had been purified from radium-228 several times each year, and so for extended periods was reasonably free from radioactive daughter products. We maintained a steady correspondence with the Joliot-Curies, comparing results. We discovered a product with a half-life of 1 minute, which was confirmed by Irene Curie, and one with a half-life of 42 minutes, which resembled a 3.5-hour product discovered by Irene Curie. Both resembled a rare earth, especially lanthanum. Conditions at that time did not permit us to draw any conclusion other than that they were actinium isotopes and that they could be arranged in a series, simulating those of natural radium families. We also bombarded thorium with slow neutrons and found a 23-minute thorium, thorium-233, which decays to protactinium with a very long half-life, also discovered by Hahn and Meitner.

The Fermi group was the first to bombard the heaviest element, uranium. They discovered two products of short half-life, 10 and 40 seconds. They made the obvious assumption that they were short-lived uranium isotopes. Because they are beta emitters, they decay to an element of the same mass but a higher atomic number, 93. Hahn, Meitner, and Strassmann began neutron bombardment of uranium also. They found a substance with a half-life of 23 minutes, long enough to permit them to identify it unquestionably as a uranium isotope, uranium-239, a beta emitter. Hahn, Meitner, and Strassmann had finally obtained two artificial radioactive products that appeared to form from the supposedly short-half-life uranium isotopes. They could be classified according to their chemical properties and beta emissions from the preceding element into different groups; decay schemes were drawn up to element 96. The chemistry was thought to be similar to that of the elements above them in the periodic table, and rhenium, osmium, and iridium were used to separate these eka rhenium, eka osmium, and eka iridium elements. Element 93 was separated later by two American scientists, E. McMillan and P. Abelson, and named neptunium after the planet Neptune, which is the first beyond the planet Uranus. Glenn Seaborg, much later, named element 94 plutonium (Pu), after the planet Pluto, for the next planet beyond Neptune. Hahn, Meitner, and Strassman produced, in addition to the transuranium elements, a still greater number of elements, among them some presumably produced by two successive alpha emissions, resulting in three artificial beta-active radium isotopes with different half-lives, which they thought at that time changed into an artificial beta-active actinium isotope. Irene Curie was not satisfied with the classification of the 3.5-hour product as actinium because all her experiments pointed to a lighter element, a rare earth, possibly lanthanum. With some hesitation the Joliot-Curies published their results, which were met with unbelief.

Sometime later, Joliot met Hahn at a meeting in Rome. Hahn writes about that encounter: "We had known of each other's work for a long time without ever having met in person. We quickly established a personal and friendly contact." Later, during the meeting, he mentioned to Joliot: "I have a great friendship and admiration for your wife; nevertheless, I have decided to repeat her experiments, and I think I shall be able to show that she made a mistake." Hahn and Strassmann repeated Irene Curie's experiments and found that the 3.5-hour isotope was not actinium but a lighter element, closely resembling lanthanum. This moved Hahn to have a closer look at his product, closely resembling radium, which was produced by slow-neutron bombardment of uranium. Hahn carried out fractional crystallization and coprecipitation with different barium salts; the isotope always followed barium. Finally, he added a tracer, radium-228, a beta emitter, and carried out separations. The radioactive tracer could be separated from the new barium isotope. He checked it in another way. If the alkaline earth was radium, then beta decay should produce actinium; if it was barium, lanthanum should be produced. All these experiments unquestionably proved that it was a barium isotope. Before Hahn and Strassmann had finished the whole experiment, they published their tentative results in *Naturwissenschaften* (6 January 1939). They described phenomena which were in opposition to all that had been observed thus far in nuclear physics. Hahn and Strassmann suggested, with some hesitation, a splitting of the uranium atom into two parts, one a barium atom of mass 138, the other a noble gas atom of mass 101, making up the

Ellen Gleditch

atomic mass of the compound uranium isotope uranium-239.

I was in Oslo at that time. My close friend Ellen Gleditch, chairman of the Chemistry Division of the University of Oslo

located in the suburb of Blindern, had invited me to replace a staff member who was on a leave of absence. I read the article in the *Naturwissenschaften* with excitement, and had the privilege soon after of hearing Hahn talk for the first time in Oslo on the splitting of the atom. He started his lecture with the words: "I am sorry to announce that the artificial radioactive elements are not transuranium elements, as I thought before, but products of the splitting of the uranium atom." He then went on to describe his experiments, which had led to the production of barium as a consequence of the splitting of the uranium atom. Hahn got permission from Fascist Germany to give a talk in Norway under two conditions: He would speak *only* German, and he would present his compliments to the German ambassador. Hahn was a fierce liberal, opposed to Fascism and Hitler's phony philosophy. He talked mostly English and went to see the ambassador at noon, knowing that he would be out for lunch; he left his card. One day Hahn and I were on the porch of the University of Oslo, which faced the road. A batallion of Norwegian soldiers passed by. Hahn exclaimed: "What a beautiful sight! Each soldier holds his gun in a different way, and they do not march in goose steps."

When Frédéric Joliot read the paper of Hahn and Strassmann in January 1939, confirming his wife's results, he started work immediately on problems concerning the new concept of the splitting of the atom. If the uranium atom splits in two, it should be possible to demonstrate the existence of fragments. He devised a very simple and ingenious experiment. The uranium, in form of a thin paint, was spread over the exterior of a cylinder. This cylinder was placed inside a Bakelite cylinder of slightly larger diameter (2.6 centimeters) to catch the fragments of the splitting uranium atoms. With his usual thoroughness, Joliot carried out three experiments. In the absence of the neutron source, no radioactive atoms were produced, and none were produced when uranium was removed and the neutron source was replaced. When both uranium and the neutron source were replaced, the inner surface of the Bakelite cylinder showed a mixture of radioactive atoms. Joliot's estimation of the energy liberated from the splitting of the uranium atom was 200 MeV. If this calculation were correct, the fragments should travel a distance of around 3 centimeters, which was confirmed by Joliot's experiment. He also formulated the basic idea of a chain reaction by the suggestion that more than one neutron was emitted at each splitting; he estimated the number of these neutrons in addition to the incident neutron to be 3.5; the accepted number today is 2.5. Joliot repeated his experiment using the cloud chamber and produced the first photograph of fission tracks.

Just a few days earlier, Otto R. Frisch, at the Niels Bohr Laboratory in Copenhagen demonstrated the splitting of the uranium atom with a slightly different arrangement. An American microbiologist, William A. Arnold (presently in the Biology Division of the Oak Ridge National Laboratory), was working at the Bohr Laboratory with G. von Hevesy at that time. This is how he recounts this exciting event: "Dr. Frisch asked me to join the other members of the laboratory to witness his experiment. He used an oscillograph, and we could see the tall spikes produced by the energetic fragments of the splitting. Frisch turned to me and asked, 'What do you call the splitting of bacteria?' I answered, 'Fission'." This term henceforth was used to describe the splitting of uranium into fragments.

The annual meeting of the American Physical Society was held in New York on 26 January 1939. During the meeting a telegram was handed to Niels Bohr, who read it aloud. It was from Frisch and Meitner, and it announced experimental proof of the fission of the uranium atom (quotation marks around "fission" had been removed). The reading of the telegram was received in complete silence. According to an eyewitness, the auditorium quickly became almost empty; the scientists rushed to their laboratories to carry out some simple experiments to prove fission.

Bohr was the first to suggest that it is the rare uranium isotope uranium-235, which when bombarded with neutrons, is responsible for fission; the relatively plentiful uranium-238 does not fission. During the fission more neutrons (two to three) are liberated; the fact that each can cause a new fission suggests the possibility of a chain reaction; this possibility was first perceived by Hahn and Strassmann, but Joliot, H. von Halban, and L. Kowarski experimentally proved the existence of surplus neutrons first.

When uranium-235 captures a neutron, uranium-236 is produced in a highly excited state, and divides into two unequal parts. The fragments fly apart, releasing the surplus neutrons and creating a chain reaction. If a great amount of uranium were present, these reactions might occur at an ever-increasing rate and involve so many atoms that an overwhelming liberation of energy would produce an uncontrolled chain reaction.

Joliot foresaw this possibility. Already in 1935 in his acceptance speech of the Nobel Prize, he said: "If we look back at the past and consider the progress of science at an ever-increasing pace, we may be entitled to believe that researchers, building up or breaking down elements at will, will be able to bring about nuclear reactions of an explosive nature—veritable nuclear chain reactions. If such reactions will be propagated in matter, one can imagine the release of useful energy that will take place. But, alas, if the elements on our planets are contaminated, we can only look forward with apprehension to the consequences of the unleashing of such a cataclysm. Astronomers sometimes observe how a star of mediocre brightness, which is ordinarily invisible to the naked eye, suddenly increases in size, becomes very bright and visible without the aid of instruments, the apparition of a supernova. It may be that the apparition of a supernova is caused by these explosive chain reactions—a process that researchers will no doubt try to bring about, while I hope taking the necessary precautions."

At that time, the implication of the discovery of fission did not escape the scientists. The situation in Europe was becoming more serious every day with the increasing power and spread of Fascism. The possibility of unleashed chain reactions, should the idea become known to Hitler, would mean the end of civilization. Hahn's position was the most difficult. He was well aware of the dangers. He worked in Germany; some of his collaborators were staunch followers of Hitler. I visited him in Dahlem, on my return from Norway. We sat outside the laboratory in the shade of a big elm tree and talked. Hahn complained bitterly about his situation. "I am a prisoner of my collaborators," he said. He accompanied me to my train, talking (not lowering his voice) about the evil of Fascism. I often thought that he deserved a second Nobel Prize, the Nobel Prize for Peace.

At that time, some European nuclear scientists were finding a haven from the Fascist regime by immigrating to the United States and joining the group of scientists headed by Fermi. They did not know how far along the Germans were in developing an atom bomb, but they felt that speed and secrecy were essential. Joliot's many discoveries in the field were well known. As early as February 1939, Leo Szilard wrote to Joliot asking him not to publish his results concerning fission or anything about uranium. Two months later Victor Weiskopf sent a telegram to Halban, making the same suggestion.

Withholding scientific information and working in secrecy were against the principles of the Joliot-Curies. Joliot went on publishing. The Joliot-Curies closely followed the principles that Marie and Pierre Curie had set down concerning their discoveries. They were most liberal; they believed that science is a universal language and that scientific information should not be withheld. The Joliot-Curies had often discussed these problems with Mme. Curie, but they soon realized that in this case scientists were faced with a unique situation.

The behavior of uranium on bombardment with neutrons of different velocities turned out to be very complex. If uranium-235 is bombarded with slow neutrons, it will split in two unequal parts with the production of artificial radioactive isotopes with atomic charges from 30 to 68. During the fission, more than one neutron is produced, making a chain reaction possible. The resonance capture of a neutron by uranium-238 produces uranium-239, which, in turn, is transformed into elements 93 and 94, the transuranium elements. It is obvious that the two uranium isotopes compete for the neutrons. As uranium-235 makes up only one part in 140 of natural uranium, it is necessary to prevent the capture of neutrons by uranium-238 if a chain reaction is to occur. This can be done by separating uranium-238 from uranium-235, so that uranium-238 does not interfere with neutron capture by uranium-235. None of the sophisticated equipment used today for isotope separation was available at that time. I mentioned earlier that elastic collisions with light elements slow down neutrons (materials that tend to slow down neutrons are called moderators). The choice of ways to slow down neutrons is very limited. Water is a poor moderator, for hydrogen captures neutrons too readily, and neutrons would be lost. Heavy water, however, is a suitable moderator, because the number of neutrons captured by deuterium is small. At that time, the only place where heavy water, D_2O, was being produced in quantity was in Kjeller, Norway. Just before the German occupation of Norway, the whole world reserve of heavy water had been shipped to the College de France, where the nuclear work was being done. Joliot had a great amount of uranium from the Union Mines of Katanga. As a result, Joliot had the means to continue work on fission chain reactions and their application. Unfortunately, there was little time to proceed with the experiments. On 16 May 1940, Joliot was informed that the Germans had broken through the French front at Sedan. Paris was in imminent danger; the heavy water and the uranium

supply had to be removed to a safe place at all costs.

I have only sketchy information about the fate of the laboratory of the College de France, based on conversations with my French colleagues during my two later visits to Paris. I knew, of course, that the heavy water and the uranium were sent to England, where Halban and Kowarski continued their work, first in England and then in Canada; finally, they joined the group of scientists in the United States. W. W. Gentner, a German scientist who was first to witness the discovery of artificial radioactivity and who was devoted to Joliot, was told by the Germans to supervise the laboratory of the College de France. That was fortunate; through him, Joliot, who stayed in France, was informed about the intentions of the Germans. Naturally, they were interested in the whereabouts of uranium, heavy water, and laboratory notes on the research that was going on. Their first impulse was to remove all the valuable instruments, cyclotron and others, to Germany. As that proved to be impractical, they sent German scientists to work at the laboratory, retaining Joliot; Joliot and his team were thus able to manufacture Molotov cocktails during the night in facilities adjacent to the laboratories where the German scientists worked during the day. Finally, Joliot left the laboratory and joined the resistance movement.

Great Scientists

Having known and worked with so many famous scientists, I often find myself reflecting on what makes a great scientist. Can one make a sweeping generalization? I would like to try.

The most outstanding trait that all the great scientists I have known had in common is imagination: all of them were able to see things which would not be obvious but which one had to perceive in a visionary way, as in a dream, and which can be developed by experiments and logical thinking. We know that Ernest Rutherford dreamed about the neutron and predicted it, as well as heavy hydrogen and tritium, 20 years before these species were discovered. Frédéric Joliot saw in a vision the coming of the atomic age, with all its fearful consequences. But imagination alone is not enough; courage to carry out experiments without fear about whether the results will come out the way one would expect is also necessary. Rutherford worked with simple ideas, and his most successful experiments were carried out with simple apparatus; but he had an extraordinary confidence in his methods, and most of the times he was right. He took naive pleasure in accepting honors, lavishly bestowed on him, but he was unable to follow the crooked ways of politics. Like all the great scientists whom I have had the privilege of knowing, he was oblivious of material advantages; the deep satisfaction that research brought to him was sufficient compensation. Another trait of a great scientist is an intense curiosity, a need to investigate in order to understand the elusive mystery.

Most of the characteristics that typify a great scientist were true of Madame Curie. Her life consisted of long, assiduous, unbroken labor without any hope of immediate distinction or reward; simply the pure love of the work itself, much toil solely in the interest of pure science.

The environment in which one grows up has a deep influence on one's life, molding character and directing actions. Rutherford was born and grew up in New Zealand where he led a carefree, happy, calm life; he seemed free from complexes. His simplicity, so much admired when he became famous, had its roots in his early childhood and youth. For him, a successful experiment gave him all the satisfaction that a more sophisticated person would derive from the arts and poetry.

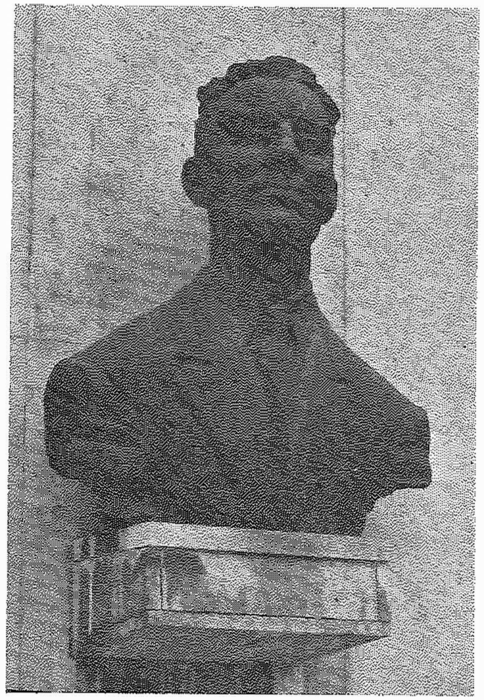
Joliot grew up in Paris, the center of European culture. It was not unexpected that, in addition to having an all-encompassing interest in science, he would love and practice music and read and enjoy poetry. During his fatal illness he turned to painting.

Rutherford died before the atom bomb had been perfected and used at Hiroshima and Nagasaki to bring an end to World War II. I often wonder what would have been his attitude toward the atom bomb. During World War I, we learned Rutherford's answer to a related question put to him at a meeting of engineers: "What would be the consequence of the liberation of radium for useful purposes?" He said, "If we could liberate the energy of a pound of this substance at a suitable rate, this would correspond to the use of 100 million pounds of coal." He added: "Fortunately, we have not at present discovered the method of doing this. I personally hope it is not discovered until men are living at peace with one another."

During Joliot's lifetime, the method not only had been discovered, it had been used for mass destruction; this development was very much on Joliot's mind. He foresaw future destruction, and he predicted the destruction that might occur in the future. He worked for peace. He planned the constitution of the World Federation of Scientific Workers and became the first president of that organization. Speeches and writing took much of his time.

Both the Joliot-Curies were extremely interested in people. They were concerned with differences in the standard of

living throughout the world and the anomaly of rich people wasting their food while the poor were starving. I remember Irene Curie's outburst during our hike in the Vienna woods



Bust of Frédéric Joliot in Budapest

over the destruction of potatoes in America. It was natural for Joliot to become interested in Marx and Engel's doctrines. He idealized Communism, and even during Stalin's time he saw only the brighter side. He joined the Communist Party soon after the end of World War II, in 1946. As a result, he experienced much harassment and the loss of high position and prestige. He believed that Communism would heal all the wrongs of society. Had he lived longer, I believe Joliot would have altered his views about the merits of the Communist form of government.

The United States

In 1941 I made a big decision. Hungary was threatened from two directions; on one side, the right bank of the Danube, were the Russians; on the left, the Germans. There was no future for me in Hungary. It is a hard decision to leave a country to which you are bound with many ties. Because I still had some hopes for a free Hungary, I applied for and received a visitor's visa to the United States. It happened that Bela Bartok emigrated to America at the same time. The evening before I left, he gave a piano concert; it was his last in Hungary. The concert hall was filled. He started to play. After he finished his program, it became clear that the audience did not want to let him go, and he himself was transfixed. He played one piece after another, quite oblivious of his surroundings. He never came back to Hungary.

My first stop on my trip to the United States was Vienna. The train ride, which normally takes 4 hours, took more than 8, and it was evening when I arrived. The city was blacked out. My friend Berta Karlik met me and escorted me to the home of my old boss, Stefan Meyer. All my old friends were there to say goodbye to me. During the evening, one by one, each person conveyed the same message: tell Roosevelt, or ask Einstein to ask Roosevelt, to get America to enter the war against the Germans.

When I arrived in New York, one of my first visits was to

Columbia University, where some of my former colleagues were doing research. I went to the Physics Department, where I saw Enrico Fermi, Leo Szilard, and quite a few people whom I had met previously. I had the peculiar impression that they were acting as if they did not know me. Nobody talked to me. A scientist, formerly from Vienna, to whom I brought special greetings from his former professor, turned his back and left rapidly. Many years later Professor John Dunning, the only scientist who talked with me and was kind to me on this visit, explained the situation. Men from the FBI were present, questioning the scientists about what I, a person with a visitor's visa, was doing at the laboratory where such secret work was being done. Surely I would go back to Germany and report some secret results. Szilard, however, could not pretend that he did not know me; we had been friends for a long time, and our paths had crossed often. He took me out one evening to dinner in a Chinese restaurant so dark that nobody could tell with whom he was dining.

I was very much interested in Harold Urey's work. I decided to look him up at his laboratory in New York. I was lucky; instead of an icy encounter, my questions were greeted with genuine interest. Urey suggested that we go for a walk. He very skillfully started to question me about whether the Germans were making any progress in the separation of isotopes. To lead me into a discussion of more important research work, he asked me about K. Clusius's separation of chlorine isotopes. At that time the information that was most intensely sought was how far along were the Germans in their efforts to build the atom bomb. One of the most important problems to be solved was how to separate the fissionable uranium-235 from the non-fissionable uranium-238. Urey told me that he was so nervous that he could not read serious literature, only the comics. All this apprehension sounded strange to me, but it was impossible for me to guess what it was all about. I did not understand until 3 years later, when atom bombs were exploded at Hiroshima and Nagasaki. I was having lunch with some nuclear scientists at the University of Rochester when the news of the first atomic bomb was announced. The announcement was heard with utter unbelief and amazement. Each of these scientists had worked on some part of the project, not knowing what their results contributed to. They were not the only ones in this situation; hundreds of scientists around the country were in a similar position. This was perhaps one of the best kept

secrets in history. To keep the atom bomb a secret was almost more of an accomplishment than to make the bomb.

My visit with Urey raised my spirits. I was not discouraged as a result of my fruitless job-hunting in the United States. Deep inside I knew that sometime, somehow I would succeed. There were so many things to see in New York; everything was new. I loved this country almost at first sight. After 3 months of joblessness, I was advised to attend the annual meeting of the American Physical Society, considered to be the "slave market" where employers looked over possible employees. By chance, I met Professor Karl Herzfeld, well-known theoretical physicist at Catholic University of America. He asked me whether I would be interested in a job. I said of course I would. Trinity College, a Catholic college for women in Washington, D.C., was looking for a chemistry teacher with a Ph.D. degree. I got the job. There was only one catch. A few days before the meeting, I went to see C. S. Piggot and W. Urry at the Geophysical Laboratory of the Carnegie Institute in Washington, D.C. I was interested in their work; they were doing research on the radioactivity of ocean sediments; they had a whole collection in an air-conditioned, low-temperature room. They needed data on the uranium content of seawater from the same location from which their cores had been taken; they had read my papers on this subject and offered me a grant to collect seawater at the Woods Hole Oceanographic Institute at Cape Cod, Massachusetts, and then to carry on the uranium determinations at the Geophysical Laboratory. In time, an agreement was reached with Trinity College. Starting in the fall, I would teach from 8 a.m. until noon at Trinity, and from 1:30 to 6 p.m. I would work at the Geophysical Laboratory. I received the most helpful collaboration from the scientists of the Woods Hole Oceanographic Institute. A special water sampler that would hold 200 liters of seawater was designed by Maurice Ewing, and the *Atlantis* brought seawater from the stations from which Piggot and Urry took their bottom sediment samples. Uranium was precipitated from seawater with iron, and this concentrate was further analyzed in the Geophysical Laboratory and Trinity College.

I had never before taught in a women's college. It was a good experience for me to meet American girls from different backgrounds. I soon learned that I had to use low-pitch teaching and to go slowly. This was necessary not only because of the course content but also because of my foreign accent, which was pronounced at that time. Soon, however, some of my students adopted some of my Hungarian idioms, to the mirth of girls who were not my students. Another difficulty was that the students had been assigned a textbook to use during the course. They were unhappy that my lectures did not follow this text. Some of the students who had received A+ grades before were lost and frustrated. Fortunately, they got used to this new style, and discussion and laboratory periods helped. One very reluctant and rebellious student became interested, and after graduation she told me that I was an inspiration to her. This is the greatest reward a teacher can have.

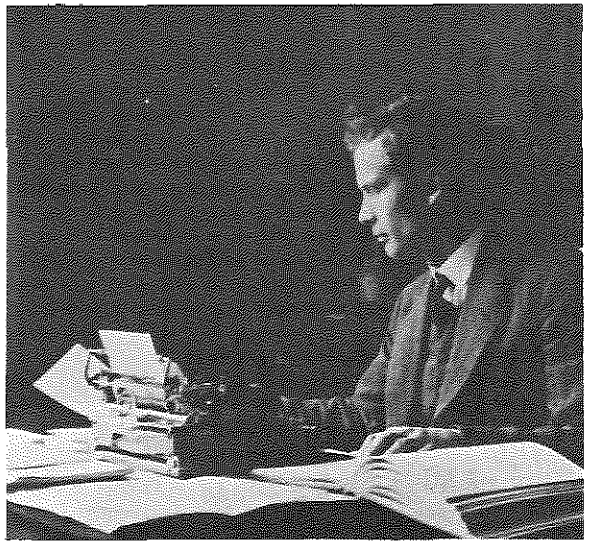
While at Trinity, I received an urgent call from an important chemical company, requesting that I come for an interview at my earliest convenience, which I did. They did not tell me what they wanted me to do; from the questions I understood that I was supposed to do some radioactive work, especially with polonium. I found out later that they did intensive work for the war effort, especially on polonium. Nothing came from this interview, however; my visitor's visa, with my family still in Hungary, was too much of an obstacle for such employment.

When I returned to Trinity College after spending the summer in Los Altos, California, I was handed a telegram; it was from Brian O'Bryen, professor in the Institute of Optics at the University of Rochester. It read as follows: "In connection with a certain war work, immediate need has arisen for large quantities of polonium, and probably also for lead-210. A stockpile of radon seeds will be available. At first, it will be necessary to produce these elements in amounts corresponding to about 50 milligrams of radium, and it is desired to obtain this quantity in the shortest period of time. It is probable that considerably larger amounts will be needed thereafter. Solutions of polonium-210 and lead-210 should be without contamination with inactive material. It is also desirable that these solutions should be strong and either not at all or slightly acid. I believe that your unusual experience in radiochemistry will ensure quick and reliable results. You would be making a substantial contribution to the war effort." This message bore the designation "restricted." I replied immediately that I would be interested in that assignment. At 11 p.m. the next day there was a knock on my door. A tall, thin gentleman introduced himself as Professor Brian O'Bryen. He explained the details of my work and suggested that one of the students should help me in some

of the manual work, but that she should be neither a physics nor a chemistry student, in order that she would be quite oblivious to the nature of her work. My choice was a very serious, conscientious girl who was majoring in French and who could well use the $500 that was offered her. O'Bryen must have been satisfied with my work, because at the end of the school year he called on me to come to Rochester to try to solve a difficulty that arose in work for the Office of Scientific Research and Development (OSRD). I was not yet a U. S. citizen, and the work was highly confidential. Soon I found out that O'Bryen did not allow difficulties to interfere with the completion of an assignment. After a short vacation I got word that my security clearance had come through. I soon solved the difficulty and explained the procedure to a representative of the Canadian Radium and Uranium Company, which had contracted to do a mass production job for OSRD. I was asked if I would give away my method without selling it. I gave all the details without compensation. The method then went into mass production. Part of the gadget, the metascope, was a zinc sulfide screen, with the best characteristics of a scintillation screen. I was asked to spend the rest of the summer working on developing the metascope. It took several months, but finally I succeeded. The work at Rochester was a great challenge and satisfaction, but it was also a unique opportunity to know Brian O'Bryen. He was full of ideas and was ebullient. Because he felt the need to talk with his wife about his work when he came home late at night, he accepted his assignments from the Government only if his wife received a clearance also.

Radioactivity and Oceanography

I had always been fascinated by the ocean: its vastness, covering 75% of the earth's surface, and its mysteries, scarcely revealed, challenged me. My opportunity to look



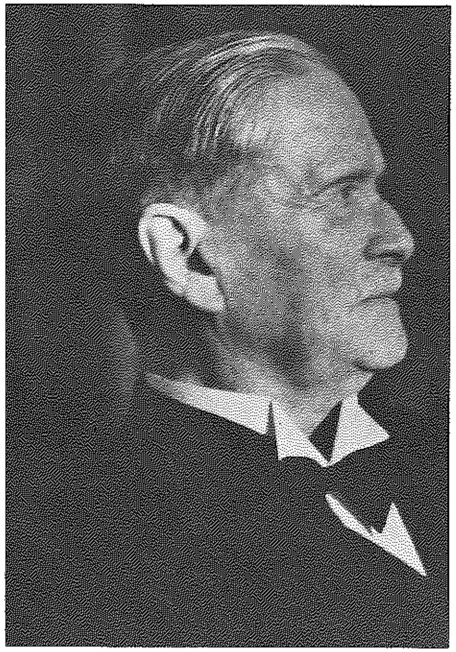
Hans Pettersson

into these mysteries came when Hans Pettersson, a famous oceanographer, came to the Vienna Radium Institute in 1928 with a few red clay samples. He became interested in the radioactivity of sea bottom sediments when he read James

Joly's *Radioactivity and Geology*. He became especially intrigued by the high radium content of red clay, brought from the bottom of the sea by the *Challenger* expedition (1872 through 1876). He asked Stefan Meyer to assign one of his staff members to analyze these samples for their radium content. I was given the job. I soon found that the contamination of the Radium Institute was too high to permit small amounts of radium to be determined. The needed equipment was moved to the oceanographic station of Bornö, on Gullmarfjord, in south Sweden. Here I spent many summer months, staying sometimes well into the fall. Before I go into the details of the work that was carried on in Bornö, I would like to describe the man with whom I worked, and who, until his death, was a close friend.

Hans Pettersson grew up in a cultural and scientific atmos-

Otto Pettersson



phere. His father, Otto Pettersson, was professor of chemistry at the University of Stockholm. Otto Pettersson was a colleague of Svante Arrhenius, and one of the few people to accept with enthusiasm the theory of electrolytic dissociation. He took an early retirement and gave all his energies to the intricate problems of the ocean. His home and several small islands that he owned were on the Gullmarfjord. He became the founder of the International Council for the Exploration of the Sea. In 1902 he became its first president.

On the largest of his islands, Bornö, Otto Pettersson founded the first Oceanographic Institute in Sweden. He was not a wealthy man, but all expenses were paid out of his own funds. Later the station was enlarged and improved with federal money. He maintained boundless energy and enthusiasm until his old age. He told me once, when he was past 90 and still had the spirit of a young man: "What will sustain me in my last moments is an infinite curiosity as to what is to follow." Hans Pettersson wrote about his father: "He became much annoyed with me when I sent him one of my books on popular science, with a chapter at the end on the universe, in which I quoted Swinburne's immortal lines from the *Garden of Proserpine*:

> From too much love of living,
> From hope and fear set free,
> We thank with brief thanksgiving,
> Whatever gods may be,
> That no life lives forever,
> That dead men rise up never,
> That even the weariest river,
> Winds somewhere safe to sea.

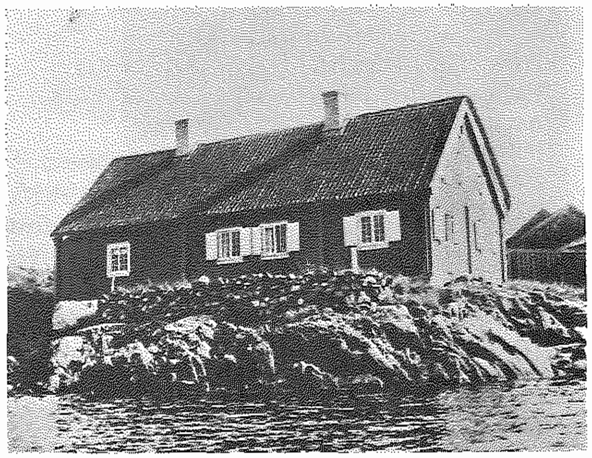
To my father the idea that 'no life lives forever' was unacceptable, and 'dead men rise up never,' he considered a personal insult."

Hans Pettersson inherited his father's vitality and love of life; he enjoyed the beauty of nature and the arts. His description of the last rays of the sun, breaking on the turbulent sea, is a beautiful part of his charming little book *Westward Ho with the Albatross*. He was a romantic. His favorite composer was Chopin. Before he left Vienna, he attended Mozart operas regularly and visited the Art Museum, where he was particularly fond of the paintings of Brueghel; in London he was especially interested in the dream landscapes of Turner at the Tate Gallery.

Hans Pettersson believed that science should be shared with the general public and that scientists had an obligation to give the people an understanding of their environment. His popular books were expertly written and seasoned with wit. Over a period of 30 years, he presented the most recent advances in scientific knowledge to the readers of the "third page" in the *Goteborg Handels Och Sjofartstiding*. He also wrote biting articles attacking crooked politicians and policies. He could not stand foul play or injustice, and did not shrink from making enemies by speaking freely. However, he collaborated with scientists when he felt they could contribute to any essential part of oceanography. He used to say, "If necessary, I will work with the devil." If a friend needed a helping hand, his assistance was immediately extended.

Hans Pettersson once told me that for generations his ancestors had lived near the sea and had become familiar with its multiple problems. Their ancestral home Kalhuvedet (meaning Cabbage Head) was built on a rock emerging from the deep sea. It is a very old house, containing old furniture salvaged from shipwrecks of bygone times. How I loved to visit that beautiful place and have a dip in the deep water surrounding the rocks. With such an ancestry and environment, the sea became an obsession to both Otto and Hans Pettersson.

Kalhuvedet



Hans Pettersson graduated in physics; his doctoral thesis at Sir William Ramsay's laboratory at the University College in London dealt with the construction of a microbalance whose sensitivity still has not been surpassed. Here he became interested and familiar with the theories and methods of radioactivity that led to his interest in the transmutation of elements at the Vienna Radium Institute. Some of the bottom samples that he wanted to have analyzed had been collected by Sir John Murray of the famed H. M. S. *Challenger* expedition that circumnavigated the world between 1872 and 1876. Murray was one of the principal scientists on the voyage; he is credited with having been the first scientist to collect bottom sediments. Ten years later Pettersson joined Murray and John Hjort on a short cruise

around the western Mediterranean. He was later asked by Prince Albert I of Monaco to analyze deep-sea sediments collected by the crew of the *Princess Alice II*. These were some of the samples Pettersson brought to the Radium Institute for analysis.

Bornö Station was an ideal place in which to work, to think, to meditate, and to enjoy the outdoors. The laboratories were well equipped, and the living quarters were comfortable; the library, with its many books and periodicals and a magnificent FM radio, was a pleasant place to spend the evenings. Except for the station and a small harbor, the island was an untouched wilderness of rocks, trees, and wildlife; one had to be a brave soul (which I believe I was) to explore the island. One could become hopelessly lost, which I did once. After a day of unsuccessful wandering, I found my way to the station, in almost complete darkness. The next day I took a pot of white paint and a pot of red paint and marked the trees on the south with one color and those on the north with the other color. Pettersson came to Bornö a few days later. When he saw what I had done, his anger had no limit; I had desecrated his island.

When I analyzed the ocean sediments, I found that in several samples of red clay and radiolarian ooze the radium content was indeed high. A high radium content, however, was not found in all the samples taken from great depths; some were even low in radium; nor was there any apparent relationship between radium content and water depth, as had been inferred from Joly's results. We thought this discrepancy could only be resolved if we knew exactly the concentration of the radioactive elements in seawater. Such measurements had been carried out before by several groups of investigators, but there were sizeable variations in the results. Joly found values as high as 4×10^{-11} gram in 1000 milliliters of seawater, whereas other scientists had found no radioactivity. We started to determine the radium content of seawater, taken from Gullmarfjord and from the more open sea of Skagerak. An average value of 7×10^{-16} gram per 1000 milliliters of water was found for water samples taken from shallow depths to depths of 670 meters.

The difference in the radium content of seawater and sediments found by different scientists made it imperative to know the uranium content of seawater, because uranium was the parent of radium. To understand the geochemistry of radium one has to know whether it is in equilibrium with its parent uranium or whether it deviates from it. The equilibrium ratio of radium to uranium is 1 gram of radium to 3 x 10^6 grams of uranium. The method used to determine radium is relatively simple, but no method had then been developed to determine the amount of uranium in seawater. Because of the long half-life of uranium-238 of 4.5 billion years, the activity is so low that a direct measurement, in volumes possible to handle, is out of the question. Frederick Hernegger used a different approach at the Vienna Radium Institute. His method takes advantage of the brilliant fluorescence in ultraviolet light of uranium fluoride, a method so sensitive that minute quantities of uranium can be determined. The method was further developed and applied to seawater by Hernegger and Berta Karlik. The method to determine radium and uranium was carefully checked with respective standards of known uranium and radium content. By dividing the seawater samples, it was feasible to determine both elements in the same water sample. Later work demonstrated the shortcomings of the uranium method, but for a time it was the only method available. A rough estimate of the ratio of radium to uranium in seawater by this method was too low.

During my stays in Bornö, from 1928 to 1940 during summer vacations, Ernest Føyn, who collaborated on the bombardment of thorium with neutrons, came from the University of Oslo in Blindern to participate in seawater research. He was an excellent experimenter and a very agreeable collaborator. Berta Karlik also spent a few weeks in Bornö, and Ellen Gleditch, from Norway, and Hans Pettersson, from Goteborg, came for weekends for lively discussions of our results. It seemed likely that the deviation of the equilibrium ratio was due to the precipitation of radium from seawater (uranium is very stable in seawater). This would also explain the high radium content of certain sediments. Pettersson was the first to suggest that the deficiency of radium in seawater is due to the precipitation of the parent of radium, thorium-230, probably with iron. This hypothesis made it possible to determine the geochronology of the Pleistocene epoch.

It is customary to classify sedimentary strata in periods, which were gradually built up by pioneer workers of the last century.

Quaternary:

Recent or Holocene	Modern man
Pleistocene	Stone age man

Tertiary:

Pliocene	Great variety of mammals
Miocene	Plants

The most interesting period is the Pleistocene, during which man evolved from the higher primates and the temperature of the earth alternated between that of an ice age and that of an interglacial period; this variation was harmonically following a sine curve.

The determination of the age of the earth has always been a subject of considerable interest. Before the discovery of radioactivity, geologists had to be satisfied with putting scattered information into some kind of order, and with establishing a purely relative chronology, a chronology without dates. James Hutton (1726-1797), the founder of modern geology, was the first to clearly grasp the immensity of geological time. Lord William Kelvin (1824-1907) proposed that the earth was not of infinite age. Holes dug in the earth revealed that the temperature increases with depth. Thus there is a flow of heat from the interior to the surface. The earth is losing heat; it must have been progressively hotter in the past. Kelvin postulated that the earth was thrown out of the sun as a molten planet. He calculated the time that had elapsed since the earth consolidated from this molten state as 20 to 40 million years. The discovery of radioactivity necessitated a change in this estimate. The radioactive decay constitutes a "clock" by which method, much later, the earth was found to be 4.5 x 10^9 years old. Using the method of radioactive decay, scientists were able to date minerals and rocks. To date geological periods was not easy. The surface of the earth has undergone enormous changes through its history. The crust has contracted and cracked; the temperature has changed; the surface of the sea has risen and fallen, inundating the lowlands or laying them bare. In the sedimentary rocks, imprints of plants, shells, and animals are preserved. Geologists may decipher past events from these imprints; however, many important data are missing, and so the whole chronology is incomplete.

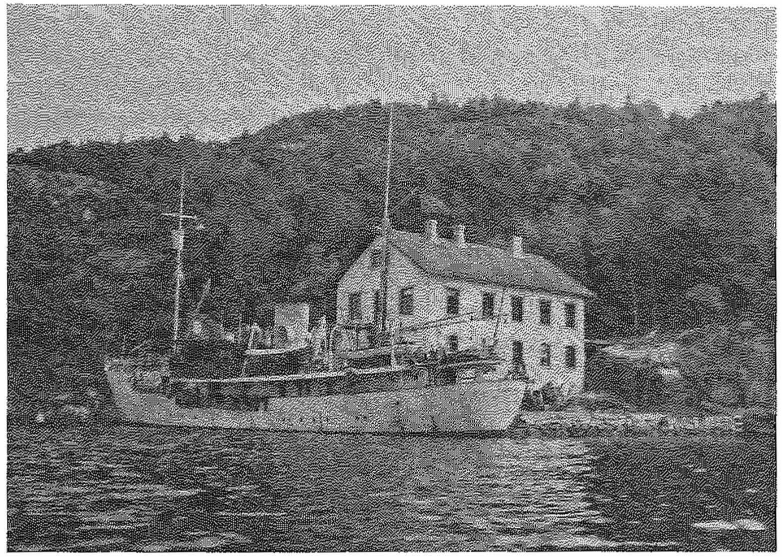
Over periods of hundreds of million years the bottom sediments did not change because they were never or rarely disturbed. Minute particles of the remnants of animals and plants settle down with infinite slowness on the ocean floor. The silica and the calcareous shells spread over the ocean floor testify to the conditions that prevailed on the ocean surfaces a long time ago. The ocean bottom, lying thousands of meters below the protecting mass of seawater, is a unique archive of the past. To decipher this record, appropriate equipment was necessary. During the *Challenger* expedition, bottom samples were grab or snap samples, some taken by sounding tubes. Fifty years later, the German *Meteor* expedition still could not penetrate deeper than 1 meter. A big improvement was C. S. Piggot's coring tube, an explosive device which shot into the bottom sediment and discharged at contact with the bottom—a kind of submarine gun. This device could bring up sediment cores about 3 meters in length. To decipher the history of a greater span of time, it became necessary to develop a device capable of penetrating deeper into the bottom. Bjorje Kullemberg, a close collaborator of Hans Pettersson, constructed the so-called vacuum or piston core, in which high water pressure was used, forcing the column of sediment to rise inside a long coring tube made of steel. With improvements in design, he could obtain cores of 6 to 7 meters in length.

To test the vacuum corer and other new instruments, Pettersson and his group used the state-owned research boat *Skagerak* and navigated around the western Mediterranean. To carry out meaningful experiments a much larger boat was necessary, large and sturdy enough to carry heavy equipment and suitable for ocean navigation. As a result of Pettersson's activities to popularize science, principally through his electrifying personality, he attracted the interest of leading men of Swedish finance; soon private donors gave to the Royal Society of Goteborg means that were sufficient to equip and run the training ship *Albatross* of Bostrom (a large steel company) for 15 months.

For a deep-sea expedition planned to last for several months, various instruments were necessary. The team worked for several years on planning and designing them. Like the *Challenger*, the *Albatross* had on board several specialists (physicists, botanists, zoologists), some for short times and some for the duration.

To circumnavigate the world's oceans is a fascinating experience. Many parts of the ocean floor have deep trenches, enormous submarine ridges, or mountain chains higher than the Alps. Pettersson's most important contribution was the study of internal waves and research on the effect of light penetration on productivity. He later built a plankton tower, an intermediary between the laboratory and the sea. He also introduced continuous echo soundings and the use of a

reflection method for subbottom investigations. He was the first to recognize that the heat flow through the ocean floor

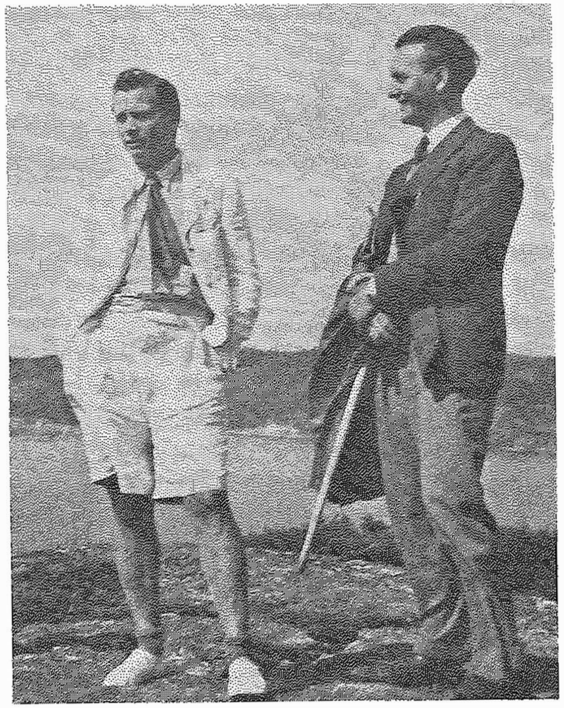


The *Skagerak* at Bornö Station, Sweden

could be a tool for the study of geophysical processes under the ocean floor.

F. F. Koczy joined Pettersson's staff in 1939; he had assisted in establishing the Oceanographic Institute in Goteborg, Sweden. He was especially active in the conversion of the *Albatross* into a research vessel and participated in the entire 15-month expedition. The experience gained during this time was of great assistance to him in his future career. He wrote a diary during the cruise, and I have this diary. It is written in German and is dedicated to his parents and new wife. From the diary we learn of the lighter side of the expedition, activities during the short amounts of leisure time, and the hilarious experience of the "baptism" of the whole crew during the crossing of the equator. During the cruise the study of water layers from the surface to the bottom was entrusted to Koczy. His special interest was the study of the radioactive elements uranium and radium. He organized and analyzed the experimental material collected during the expedition.

Extensive determinations of radium in cores from the Pacific and Atlantic Oceans were carried out by J. E. Kroll, a visiting scientist on the *Albatross*. In one of the cores he integrated the total radium content down to the level where practically all the thorium-230 that had precipitated from the supernatant water had time to disintegrate. This information made it possible to compare the radium content in the core and the supply of radium derived from the uranium in the superimposed water column of seawater. Kroll found that the radium content was three times higher than the uranium equivalent should have been; he based his calculations on the uranium content of the seawater, 1.3×10^{-6} gram in 1000 milliliters, accepted at that time. Koczy later put forward the hypothesis that several thousand years ago the uranium concentration of the sea was three times that of the present. I could think of a simpler explanation for the discrepancy, namely, the shortcomings of the fluorometric method used at that time. I set out to use a method that was not dependent on the determination of the absolute amount of uranium; this is possible, with the use of isotope dilution. If this technique is used, a standard, commonly called a "spike," is added to the sample to be



Fritz Koczy, right, with Hans Pettersson

analyzed; the standard is enriched with uranium-235. With the "spike" added, the seawater sample will show a new

isotopic ratio, determined by mass spectrometry. Once the ratios of uranium-235 to uranium-238 in the standard and that of the recovered sample from the seawater were known, one could calculate the uranium concentration. The result of this investigation was a uranium concentration three times that determined before, and it explained the discrepancy Kroll had found. An important result was that the uranium content of the world oceans is remarkably constant, and is also the same from the surface to the depths of the oceans.

As it became more and more evident that radium is not a reliable marker for thorium-230, scientists started to determine thorium-230 directly. Thorium-230 is "born" in the sea; its immediate parent, uranium-234, remains in seawater as a stable uranium carbonate complex ion; thorium-230 precipitates to the bottom, where it decays with a half-life of 76,000 years. The greater the height of the superimposed water column from which the precipitation takes place, the greater the concentration of the thorium isotope. As sediments containing thorium-230 are added to the bottom over periods of thousands of years, the strata in a long core increase with age. Another isotope, protactinium-231, a member of the uranium-235 family and a parent of actinium, has a similar chemistry; its residence time in seawater is short, about 50 years. When it reaches the bottom, it decays with a half-life of 32,200 years. The ratio of thorium-230 to protactinium-231 is a function only of time, and it is the best method to determine ages up to 200,000 years. This dating method was established by J. N. Rosholt, Jr. and his co-workers. The direct determination of thorium-230 is not easy; protactinium chemistry, at that time, was considered even more difficult. Scientists used different analytical methods, which made it arduous and confusing to compare the results. I thought it would clarify the situation if the interested scientists would get together for a discussion. I was at that time a member of the staff of the Oak Ridge Institute of Nuclear Studies (now known as Oak Ridge Associated Universities). I had come to Oak Ridge in 1950. My duties were to teach scientists with Ph.D. and M.D. degrees special nuclear theories and methods. I found time to carry out research in geochronology and geophysics, and was assisted by visiting scientists who collaborated with me.

A suitable occasion for a meeting was the approaching 75th birthday of Hans Pettersson, pioneer of the geochronology of the Pleistocene. The meeting was dedicated to him. He and his wife Dagmar, who was his helpful companion through all of their married life, were invited and attended. The meeting, "Geochronology of Marine and Fluvial Sediments," 16 to 19 October 1963, in Oak Ridge, was a great success. Fifty invited scientists attended; some brought students and assistants. There was much discussion. It was hoped that during these discussions it would be possible to establish items of agreement, determine explanations, identify points of disagreement, and also examine the validity of the basic assumptions on which various dating methods rested. Some of the discussions were heated and quite agressive. The central figure, as at other meetings on this topic, was Fritz Koczy. He attacked scientists (with acrimony) for using the wrong isotope and the wrong technique, but during the social hours, dinners, and picnics, all argument was forgotten. Then there was much back-slapping, laughter, and plain good spirits.

I had seen Koczy at meetings before but only for short times. This time we had the opportunity to renew our friendship, which dated back to years before in Vienna, when he was a student interested in certain problems of radioactivity. Even then he was a well-known marine scientist. In Goteborg, shortly before World War II, I met Koczy again. Hans Pettersson invited me to the grand opening of the Oceanic Institute in Goteborg, and he asked Koczy and me to demonstrate instruments used in oceanography and radioactivity, exhibited at the new institute, which Koczy had helped to design and build.

In 1957 Koczy accepted an invitation from F. G. Walton Smith, director of the Institute of Marine Sciences at the University of Miami, to develop a department that would include all the divisions of oceanography: physical, chemical, biological, and geophysical. Koczy was the person best able to do this. He had the wide experience, the imagination, and the personality to attract a group of world-famous scientists, and he made the Institute a leading oceanographic research center. He continued to do research in many fields of oceanography, but he was especially interested in geochronology and pioneered with Rosholt on the use of protactinium. He also showed that radium diffuses from the upper layer of the sediments into overlying seawater, and that this diffusion can be used to identify different water masses. He also investigated the biological uptake of radioactive material, including that from the fallout of nuclear weapon

tests. This interest led to his active participation in the first Atoms for Peace meeting held in Geneva in 1955.

In 1965 Koczy invited me to join the staff of the Institute of Marine Sciences at the University of Miami to date some sediment cores that were of special interest for geochronology. It was an invaluable opportunity, because I would have the right kind of cores and knowledgeable colleagues with whom to consult. Koczy was interested not only in geochronology but in many problems of geophysics and marine sciences. It was refreshing to discuss problems with him, usually around 5:00 in the afternoon when he was not disturbed by telephone calls or visitors. After talking shop we often discussed some cultural topic in music or literature; after all, we were old friends.

My most important duty at the Institute of Marine Sciences was to date sediment cores, which had been analyzed in considerable detail both isotopically and micropaleontologically. I have mentioned above that during the last million years, the Pleistocene period, cold and warm temperatures alternated. Cesare Emiliani, at the Institute of Marine Sciences, developed a paleotemperature dating method. A short description is in order. The idea of using as a thermometer the variations with temperature of the fractionation factors in isotopic exchange equilibria of the oxygen isotopes, oxygen-18/oxygen-16, was first formulated by H. C. Urey in 1947. It was based on the discovery that the proportion of oxygen-18 to oxygen-16 in calcium carbonate secreted from seawater by shell-building organisms (foraminifera) depends on the temperature of water. Using a sensitive mass spectrometer, one can determine the temperature closely from the ratio of the two oxygen isotopes. Emiliani developed and applied this method to bottom sediments in 1955 while at the University of Chicago. Studies of submarine cores are based mainly on carbonate content and variation in the relative abundance of cold and warm pelagic foraminifera. The glacial layers in a core are represented by foraminifera belonging to a species that now live only in the polar regions, whereas the interglacial cores contain species which lived in warm regions. Ideally, the change of temperature can be described by a sinusoidal curve. Emiliani constructed a generalized paleotemperature curve based on isotopic analysis of planktonic foraminifera from deep-sea cores.

The times of the onset of the ice ages and of the onset of the interglacial periods and their durations can only be known from absolute dating; that is where the dating with the thorium-230/protactinium-231 dating method comes in. And that was what I was supposed to do. Koczy viewed this project with high expectation and optimism. He came often to my laboratory and asked me how many sediments I would be able to date in a week. It was difficult to answer such a question because the dating method based on the thorium-230/protactinium-231 ratio rests on a few conditions which must be met. The rate of sedimentation must be undisturbed, continuous, and synchronous with the deposition of thorium-230 and protactinium-231. The change in concentration of these two radionuclides depends on the radioactive decay only, if they are deposited simultaneously. These conditions are not often met, and thus it is not surprising that sediment cores which can be dated are the exception and not the rule.

The most reliable dating method is that based on carbon-14. This method has a serious time limitation of 40,000 years. Radiocarbon is continuously produced in the atmosphere by the reaction of cosmic radiation. The impact of the primary and very high-energy secondary cosmic rays, mostly near the top of the atmosphere, produces violent nuclear reactions in which many neutrons, protons, and alpha particles are emitted. The neutrons react with the ever-present nitrogen to produce carbon-14, which is a radioactive beta emitter. The rate of decay is 5,730 years. The half-life of carbon-14 is long enough for the radionuclide to become thoroughly mixed with all the carbon in so-called reservoirs. The discovery that all the carbon in the world's living cycle is kept uniformly radioactive through the production of carbon-14 by cosmic radiation led W. F. Libby (1955) to propose and develop the carbon-14 dating method.

In 1968 Koczy took a year's leave of absence to teach, write, and think at the University of Hawaii in Honolulu. He needed a change, a respite from his sometimes boring administrative duties. He needed to be undisturbed to write a book, which it was expected would occupy him for a long time. The beautiful, quiet environment of Hawaii was just the place in which to write a book. His approach was to correlate previously unrelated aspects of the chemistry (physical and biological) of the ocean, hitherto largely treated as completely separate fields. The idea underlying his approach was that all reactions in the oceans occur at interfaces: ocean-ocean, ocean-atmosphere, and ocean-

sediment. It was intended to be not a comprehensive up-to-date book on oceanography but a thought-inspiring resume of reactions occurring in the sea. He finished three chapters before his sudden death.

After Koczy's death, no scientist could be found who had the broad knowledge needed to be the head of a department which included all divisions of oceanography. Thus, the department at the University of Miami was divided into three divisions: chemical, physical, and geophysical oceanography. As time went on, each division acquired its own individuality.

One other important contribution of Koczy deserves mention. After Project Mohole, the plan to drill a hole through the sea bed to the Mohorovicic discontinuity was abandoned. Koczy, with a group of scientists, was instrumental in bringing about one of oceanography's most imaginative investigations, the Joint Oceanographic Institute Deep Earth Sampling (JOIDES). This highly successful deep-sea drilling project was carried out by the oceanographic research vessel *Glomar Challenger* (the ship bears the name *Glomar* because it was designed by the Global Marine Corporation of Los Angeles). Its purpose is to drill deep into the ocean floor to obtain long cores, in an effort to expand the understanding of the origin and history of the ocean basin.

For 7 years the *Glomar Challenger* has been drilling and collecting samples of deep-sea sediments on a grand scale. This project is as international as the oceans; scientists from France, West Germany, tiny Switzerland, and Russia participate in the enterprise. Geophysicists, paleontologists, and sedimentologists all will profit from the very large number of measurements already made.

In this short paper, I have expressed an interest in the climatic conditions of the last million years. But we can learn even more from the drilling of samples which span a hundred million years. We learn that the world was much warmer and more uniform that long ago, since the distribution of the continents and the configuration of the oceans has changed dramatically through the process of continental drift. These tectonic changes played a major role in cooling the earth's climate, and relatively recently in the permanent formation of the ice caps. The course of these events led to the fluctuating glacial and interglacial climate of the last million years. Whether the cooling will continue and lead to a permanent ice age is of intense interest to scientists. There is, however, speculation that a warm-up will be brought about by the carbon dioxide from burning fossil fuels.

In order to evaluate whether carbon dioxide will upset the cooling trends in the near future, it seemed necessary to analyze sediments of a more recent period, the Holocene. Recent results come from a detailed study of sea-floor sediments and other climate-related evidence from the National Science Foundation CLIMAP Project (Climate: Long-Range Investigation, Mapping, and Prediction), recently undertaken by scientists of 18 institutions. Samples are taken by a large-diameter piston corer which does not disturb the upper layer of the sediment.

Our knowledge of ocean history has come a long way since the pioneering work of Hans Pettersson, C. S. Piggot and W. Urry. To quote William Hayes of the University of Miami: "We will know more from the cores recovered by the *Glomar Challenger* about the composition of the sediments in the deep sea bottom than we do about the average composition of what is on land. Gone are the times when geologists and geochemists asked: 'What can one learn from the mud hole (meaning the ocean bottom), the 'sink' for the detrital material brought by the erosion of the land?'"

A Final Word

As with any story that is based on personal experience, the story I have just presented is incomplete because I only viewed a small part of the total epic. I hope, however, that the story does give the reader a more complete insight into the characters of the dedicated men and women who pioneered the early scientific work that has led to the modern sciences of atomic and nuclear physics and oceanography.

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