Richard Rhodes

THE MAKING OF THE ATOMIC BOMB

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he would say, 'Oh, well, I know this on *c.i.f.*' He spoke Italian. '*C.i.f.*' meant '*con intuito formidable*,' 'with formidable intuition.' So how he did it, I don't know. On the other hand, Fermi made a lot of calculations which he kept to himself."

Leona Woods' version sheds light on Teller's:

> Why was Dr. Noddack's suggestion ignored? The reason is that she was ahead of her time. Bohr's liquid-drop model of the nucleus had not yet been formulated, and so there was at hand no accepted way to calculate whether breaking up into several large fragments was energetically allowed.

If Noddack's physics was *avant garde*, her chemistry was sound. By 1938 her article was gathering dust on back shelves, but Bohr had promulgated the liquid-drop model of the nucleus and the confused chemistry of uranium increasingly preoccupied Lise Meitner and Otto Hahn.

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An Extensive Burst

"I believe all young people think about how they would like their lives to develop," Lise Meitner wrote in old age, looking back; "when I did so I always arrived at the conclusion that life need not be easy provided only that it was not empty. And this wish I have been granted." Sixty years old in 1938, the Austrian physicist had earned wide respect by hard and careful work. When Wolfgang Pauli had wished to propose an elusive, almost massless neutral particle to explain the energy that seemed to disappear in beta decay—it came to be called the neutrino—he had made his proposal in a letter to Lise Meitner and Hans Geiger. James Chadwick was "quite convinced that she would have discovered the neutron if it had been firmly in her mind, if she had had the advantage of, say, living in the Cavendish for years, as I had done." "Slight in figure and shy by nature," as her nephew Otto Frisch describes her, she was nevertheless formidable.

During the Great War she had volunteered as an X-ray technician with the Austrian Army; "there," says Frisch, "she had to cope with streams of injured Polish soldiers, not understanding their language, and with her medical bosses who interfered with her work, not understanding X-rays." She arranged her leaves from duty to coincide with Otto Hahn's and hurried to the Kaiser Wilhelm Institute for Chemistry in Dahlem to work with him; that was when they identified the element next down from uranium that they named protactinium. After the war she did physics separately until 1934, when, challenged by Fermi's work, she "persuaded Otto Hahn to renew our direct collaboration" to explore the consequences of bombarding uranium with neutrons. Meitner headed the physics department at the institute then, of which Hahn had become the director. She had attained by middle age, Hahn remarks fondly, "not only the dignity of a German professor, but also one of his proverbial attributes, absent-mindedness." At a scientific gathering "a male colleague greeted her by saying, 'We met on an earlier occasion.' Not remembering that earlier occasion, she replied in all seriousness, 'You probably mistake me for Professor Hahn.' " Hahn supposed she was thinking of the many papers they had published together.

If she hid her shyness behind formidable reserve, among friends, Frisch says, "she could be lively and cheerful, and an excellent storyteller." Her nephew thought her "totally lacking in vanity." She wore her thick dark hair, now graying, pulled back and coiled in a bun and her youthful beauty had muted to bright but darkly circled eyes, a thin mouth, a prominent nose. She ate lightly but drank quantities of strong coffee. Music moved her; she followed it as other people follow trends and fashions in art (a family cultivation—her sister, Frisch's mother, was a concert pianist). She made a duet at the piano on visits with her musical nephew, "though hardly anybody else knew that she could play." She lived in an apartment at the KWI and when there was time she took long walks, ten miles or more a day: "It keeps me young and alert." Her most holy commitment, Frisch thought, "the vision she never lost" that filled her life, was "of physics as a battle for final truth."

The truth she battled for through the later 1930s was hidden somewhere in the complexities of uranium. She and Hahn, and beginning in 1935 a young German chemist named Fritz Strassmann, worked to sort out all the substances into which the heaviest of natural elements transmuted under neutron bombardment. By early 1938 they had identified no fewer than ten different half-life activities, many more than Fermi had demonstrated in his first pioneering survey. They assumed the substances must be either isotopes of uranium or transuranics. "For Hahn," says Frisch, "it was like the old days when new elements fell like apples when you shook the tree; [but] Lise Meitner found [the energetic reactions necessary to produce such new elements] unexpected and increasingly hard to explain."

Meanwhile Irène Curie had begun looking into uranium with a visiting Yugoslav, Pavel Savitch. They described a 3.5-hour activity the Germans had not reported and suggested it might be thorium, element 90, with which Curie had years of experience. If true, the Curie-Savitch suggestion

would mean that a slow neutron somehow acquired the energy to knock an energetic alpha particle out of the uranium nucleus. The KWI trio scoffed, looked for the 3.5-hour activity, failed to find it and wrote the Radium Institute suggesting a public retraction. The French team identified the activity again and discovered they could separate it from their uranium by carrier chemistry using lanthanum (element 57, a rare earth). They proposed therefore that it must be either actinium, element 89, chemically similar to lanthanum but even harder than thorium to explain, or else a new and mysterious element.

Either way, their findings called the KWI work into doubt. Hahn met Joliot in May at a chemistry congress in Rome and told the Frenchman cordially but frankly that he was skeptical of Curie's discovery and intended to repeat her experiment and expose her error. By then, as Joliot undoubtedly knew, his wife had already raised the stakes, had tried to separate the "actinium" from its lanthanum carrier and had found it would not separate. No one imagined the substance could actually be lanthanum: how could a slow neutron transmute uranium into a much lighter rare earth thirty-four places down the periodic table? "It seems," Curie and Savitch reported that May in the *Comptes Rendus*, "that this substance cannot be anything except a transuranic element, possessing very different properties from those of other known transuranics, a hypothesis which raises great difficulties for its interpretation."

In the course of this exotic debate Meitner's status changed. Adolf Hitler bullied the young chancellor of Austria to a meeting at the German dictator's Berchtesgaden retreat in Bavaria in mid-February. "Who knows," Hitler threatened him, "perhaps I shall be suddenly overnight in Vienna: like a spring storm." On March 14 he was, triumphantly parading; the day before, with the raw new German Wehrmacht occupying its capital, Austria had proclaimed itself a province of the Third Reich and its most notorious native son had wept for joy. The *Anschluss*—the annexation—made Meitner a German citizen to whom all the ugly anti-Semitic laws applied that the Nazi state had been accumulating since 1933. "The years of the Hitler regime . . . were naturally very depressing," she wrote near the end of her life. "But work was a good friend, and I have often thought and said how wonderful it is that by work one may be granted a long respite of forgetfulness from oppressive political conditions." After the spring storm of the *Anschluss* her grant was abruptly withdrawn.

Max von Laue sought her out then. He had heard that Heinrich Himmler, head of the Nazi SS and chief of German police, had issued an order forbidding the emigration of any more academics. Meitner feared she might be expelled from the KWI and left unemployed and exposed. She made contact with Dutch colleagues including Dirk Coster, the physicist who had worked in Copenhagen with George de Hevesy in 1922 to discover hafnium. The Dutchmen persuaded their government to admit Meitner to Holland without a visa on a passport that was nothing more now than a sad souvenir.

Coster traveled to Berlin on Friday, July 16, arriving in the evening, and went straight to Dahlem to the KWI. The editor of *Naturwissenschaften*, Paul Rosbaud, an old friend, showed up as well, and together with Hahn the men spent the night helping Meitner pack. "I gave her a beautiful diamond ring," Hahn remembers, "that I had inherited from my mother and which I had never worn myself but always treasured; I wanted her to be provided for in an emergency."

Meitner left with Coster by train on Saturday morning. Nine years later she remembered the grim passage as if she had traveled alone:

> I took a train for Holland on the pretext that I wanted to spend a week's vacation. At the Dutch border, I got the scare of my life when a Nazi military patrol of five men going through the coaches picked up my Austrian passport, which had expired long ago. I got so frightened, my heart almost stopped beating. I knew that the Nazis had just declared open season on Jews, that the hunt was on. For ten minutes I sat there and waited, ten minutes that seemed like so many hours. Then one of the Nazi officials returned and handed me back the passport without a word. Two minutes later I descended on Dutch territory, where I was met by some of my Holland colleagues.

She was safe then. She moved on to Copenhagen for the emotional renewal of rest at the Carlsberg House of Honor with the Bohrs. Bohr had found a place for her in Sweden at the Physical Institute of the Academy of Sciences on the outskirts of Stockholm, a thriving laboratory directed by Karl Manne Georg Siegbahn, the 1924 Physics Nobel laureate for work in X-ray spectroscopy. The Nobel Foundation provided a grant. She traveled to that far northern exile, to a country where she had neither the language nor many friends, as if to prison.

Leo Szilard was looking for a patron. Frederick Lindemann had arranged an ICI fellowship for him at Oxford beginning in 1935, and for a while Szilard worked there, but the possibility of war in Europe made him restless. From Oxford in late March 1936 he had written Gertrud Weiss in Vienna that she should consider emigrating to America; he appears to have applied his reasoning to his own case as well. Szilard had met Weiss in his Berlin years and subsequently advised and quietly courted her. Now she had graduated from medical school. At his invitation she came to Oxford to see him. They walked in the country; she photographed him standing at roadside before a weathered log barrier, rounding at thirty-eight but not yet rotund, with a budding young tree filigreed behind him. "He told me he would be surprised if one could work in Vienna in two years. He said Hitler would be there. And he was"—the *Anschluss*—"almost to the day."

Szilard had written in his letter that England was "a *very* likeable country, but it would certainly be a lot smarter if you went to America. . . . In America you would be a free human being and very soon would not even be a 'stranger.' " (Weiss went, and stayed to become a distinguished expert in public health and, late in their wandering years, Szilard's wife.) During the same period Szilard wrote Michael Polanyi he would "stay in England until one year before the war, at which time I would shift my residence to New York City." The letter provoked comment, Szilard enjoyed recalling; it was "very funny, because how can anyone say what he will do one year *before* the war?" As it turned out, his prognostication was off by only four months: he arrived in the United States on January 2, 1938.

Before then Szilard had located a possible patron there, a Jewish financier of Virginia background named Lewis Lichtenstein Strauss, his first and middle names honoring his East Prussian maternal grandfather, his last name softened in Southern fashion to *straws*. Forty-two years old in 1938, Lewis Strauss was a full partner at the New York investment-banking house of Kuhn, Loeb, a self-made millionaire, an adaptable, clever but thin-skinned and pompous man.

Strauss had dreamed as a boy of becoming a physicist. The recession of 1913–14 had staggered his family's Richmond business—wholesale shoes—and his father had called on him at seventeen to drum a four-state territory. He did well; by 1917 he had saved twenty thousand dollars and was once again preparing to pursue a physics career. This time the Great War intervened. A childhood accident had left Strauss with marginal vision in one eye. His mother doted on him. She allowed his younger brother to volunteer for military service but looked for some less dangerous contribution for her favorite son. It turned up when Woodrow Wilson appointed the celebrated mining engineer and Belgian relief administrator Herbert Hoover as Food Administrator to manage U.S. supplies during the war. The wealthy Hoover was serving in Washington without pay and assembling a prosperous, unpaid young staff, Rhodes scholars preferred. Rosa Lichtenstein Strauss sent her boy.

He was twenty-one, knew how to ingratiate himself, knew also how to work. Improbable as it appears against a field of Rhodes scholars, within a month Hoover appointed the high-school-graduate wholesale shoe drummer as his private secretary. After the Armistice young Strauss shifted with Hoover to Paris, hastily picked up French at tutoring sessions over lunch and helped organize the allocation of 27 million tons of food and supplies to twenty-three countries. On the side he assisted the Jewish Joint Distribution Committee in its work of relieving the suffering of the hundreds of thousands of Jewish refugees streaming from Eastern Europe in the wake of war.

Strauss believed God had planned his life, which contributed greatly to his self-confidence. God let him take up employment when he was twenty-three, in 1919, at Kuhn, Loeb, a distinguished house with a number of major railroads among its clients. Four years later he married Alice Hanauer, daughter of one of the partners. His salary and participation reached $75,000 a year in 1926; the following year it escalated to $120,000. In 1929 he became a partner himself and settled into prosperous gentility.

The 1930s brought him pain and grief. After resisting Chaim Weizmann's attempts to convert him to Zionism at a Jewish conference in London in 1933—"My boy, you are difficult," Weizmann told him; "we will have to grind you down"—he returned to the United States to discover his mother terminally ill with cancer. She died early in 1935; the disease took his father as well in the hot summer of 1937. Strauss looked for a suitable memorial. "I became aware," he reports in his memoirs, "of the inadequate supply of radium for the treatment of cancer in American hospitals." He established the Lewis and Rosa Strauss Memorial Fund and turned up a young refugee physicist from Berlin, Arno Brasch. Brasch had designed a capacitor-driven discharge tube for producing bursts of high-energy X rays, a "surge generator." When Leo Szilard was working at St. Bart's with Chalmers in the summer of 1934 he had arranged for Brasch and his colleagues in Berlin to break up beryllium with hard X rays; the experiment had been a success and Brasch and four other contributors had signed the report to *Nature* along with Chalmers and Szilard. If X rays could break up beryllium they might at least induce radioactivity in other elements. "An isotope of cobalt thus produced," writes Strauss, "would be radioactive and would emit gamma rays similar to the radiation produced by radium. . . . Radioactive cobalt could be made . . . at a cost of a few dollars a gram. Radium was then priced at about fifty thousand dollars a gram. . . . I foresaw the possibility of producing the isotope in quantity and of giving it to hospitals as a memorial to my parents."

Enter Leo Szilard, still in England:

> August 30, 1937
>
> Dear Mr. Strauss:
>
> I understand that you are interested in the development of a surge generator with the view of using it for producing artificially radioactive elements. . . .

> At present . . . I am not in the position of [offering manufacturing rights under this patent]. It is possible, however, that at a later date . . . I shall obtain full liberty of action concerning this patent. If this happens I shall let you have a non-exclusive license, royalty free, *but* limited to the production of radioactive elements by means of high voltage generated by a surge generator.
>
> Yours very truly,
> Leo Szilard

Brasch and Szilard owned the patent in question jointly. Szilard's letter offers to give his interest away free of charge nonexclusively to Strauss, a politic salutation to a rich man. But not even Leo Szilard could live on air, and as Strauss makes clear in his memoirs, the two young physicists eventually "asked me to finance them in the construction of a 'surge generator.' " On the other hand, Szilard as usual seems to have sought no personal financial gain from the project beyond, perhaps, basic support. In the time he could spare from observing the developing disaster in Europe he was apparently trying to promote the building of equipment with which he might explore further the possibility of a chain reaction.

He crossed the Atlantic in late September to reconnoiter. A friend remembers discussing the feasibility of an atomic bomb with Szilard during this period. "In the same conversation he spoke of his ideas for preserving peaches in tins in such a way that they would retain the texture and taste of the fresh fruit." When the surge-generator negotiations bogged down in debates among the lawyers, the resourceful Szilard distracted Strauss with the idea of using radiation to preserve and protect the natural products of farm and field. The tobacco worm might be exterminated, for example. But would irradiation harm the tobacco? Among Szilard's surviving papers is lodged a fading letter from Dr. M. Lenz of the Montefiore Hospital for Chronic Diseases that reports the decisive experiment:

> On April 14, 1938, at 2:30 p.m., your six cigars were irradiated with 100 kv., a filter focus distance of 20 cm. with ten minutes in front and ten minutes over the back of each cigar. This gave them 1000 r. in front and 1500 r. in back of each cigar.
>
> I hope that your friend finds the taste unchanged.

Szilard also bought pork from a meat market on Amsterdam Avenue, saving the receipt, and arranged its irradiation to see if X rays might kill the parasitic worm of trichinosis. He even dispatched his brother Béla to Chicago to discuss the matter with Swift & Company, which reported it had in fact made similar experiments of its own.

The surge-generator project developed through the year, incidentally giving Strauss the opportunity to meet Ernest Lawrence, who dropped by to pitch the new sixty-inch cyclotron he was building—the pole pieces were sixty inches across, but the magnet would weigh nearly two hundred tons. Lawrence and his brother John, a physician, had arrested their mother's cancer with accelerator radiation and intended to use the big cyclotron to further that research. Strauss remained loyal to the surge generator.

Segrè encountered Strauss's Hungarian wizard in New York that summer. The elegant Italian was professor of physics at Palermo by then, married to a German woman who had fled Breslau to escape the Nazis, with a young son:

> I left Palermo with a return ticket, and I arrived in New York. I met Szilard. "Oh, what are you doing here?" He was a good friend of mine. I knew him quite well. "What are you doing here? What's going on?"
>
> I said, "I'm going to Berkeley to look at the short-lived isotopes of element 43," which was my plan. "I'll work there the summer, and then I'll go back to Palermo."
>
> He said, "You are not going back to Palermo. By this fall, God knows what will happen! You can't go back."
>
> I said, "Well, I have a return ticket. Let's hope for the best."
>
> But I had gotten a passport for my wife and my son before leaving, because I smelled that the situation was dangerous. So I took the train in New York, Grand Central, and I bought the newspaper in Chicago. I still remember it. I will remember it as long as I live. I opened the newspaper, and I found out that Mussolini had started the antisemitic campaign and had fired everybody. So there I was. So I had the ticket and went to Berkeley. I started to work on my short-lived isotopes of technetium, but at the same time I tried to get some job. Then I got my wife here.

The pall of racism had dropped over Italy.

The physicists at the institute on Via Panisperna had been alert to the darkening Italian prospect since at least the mid-1930s. Segrè remembers asking Fermi in the spring of 1935 why the group's mood seemed less happy. Fermi suggested he look for an answer on the big table in the center of the institute reading room. Segrè did and found a world atlas there. He picked it up; it fell open automatically to a map of Ethiopia, which Italy in a show of Fascist bravado was about to invade. By the time the invasion began all but Amaldi were examining their options.

Fermi went off to the University of Michigan's summer school in Ann Arbor, renewing an affiliation he had begun with Laura in the summer of 1930. He liked America. "He was attracted," Segrè notes with an ear for Fermi's priorities, "by the well-equipped laboratories, the eagerness he sensed in the new generation of American physicists, and the cordial reception he enjoyed in academic circles. Mechanical proficiency and practical gadgets in America counterbalanced to an extent the beauty of Italy. American political life and political ideals were immeasurably superior to fascism." Fermi swam in Michigan's cool lakes and learned to enjoy American cooking. But the pressure of events in Italy was not yet sufficiently extreme, and Laura, Roman to her fine bones, was more than reluctant to leave the city of plane trees and classical ruins where she was born. Nor was anti-Semitism yet an issue in Italy—Mussolini had even declared he did not propose to make it one.

There was less to hold the other men. Rasetti summered at Columbia University that year, 1935, and decided to stay on. Segrè had shifted to Palermo but began looking toward Berkeley. Pontecorvo moved to Paris. D'Agostino went to work for the Italian National Research Council. Amaldi and Fermi pushed on alone, Amaldi remembers, Fermi even jettisoning his daily routine for the distraction of experiment:

> We worked with incredible stubbornness. We would begin at eight in the morning and take measurements [they were examining the unaccountably differing absorption of neutrons by different elements], almost without a break, until six or seven in the evening, and often later. The measurements . . . were repeated every three or four minutes, according to need, and for hours and hours for as many successive days as were necessary to reach a conclusion on a particular point. Having solved one problem, we immediately attacked another. . . . "Physics as soma" was our description of the work we performed while the general situation in Italy grew more and more bleak, first as a result of the Ethiopian campaign and then as Italy took part in the Spanish Civil War.

Fermi taught a summer course in thermodynamics at Columbia University in 1936 as the civil war began in Spain that would last three years, claim a million lives and set Mussolini decisively at Hitler's side. The following January Corbino died unexpectedly of pneumonia at sixty-one and the hostile occupant at the north end of the institute's second floor, Antonino Lo Sordo, a good Fascist, was appointed to succeed him. "That was a sign that Fermi's fortunes were declining in Italy," Segrè notes. "America," he concludes of those depressing years, "looked like the land of the future, separated by an ocean from the misfortunes, follies, and crimes of Europe."

If the *Anschluss* was a test of Hitler's strength, it was also a test of

Mussolini's willingness to acquiesce to complicity in crime. He had posed as Austria's protector; on the night of the March 1938 invasion Hitler waited near hysteria at the Chancellery in Berlin for a response from Rome to a letter he had sent justifying his action. The call came at 10:25 P.M. and the Führer snatched up the phone. "I have just come back from the Palazzo Venezia," his representative reported. "The Duce accepted the whole thing in a very friendly manner. He sends you his regards. . . . Mussolini said that Austria would be immaterial to him." Hitler replied: "Then please tell Mussolini I will never forget him for this! Never, never, never, no matter what happens! . . . As soon as the Austrian affair has been settled I shall be ready to go with him through thick and thin—through anything!" The Führer visited Rome in triumph in May, parading into districts the Duce had ordered hastily face-lifted to conceal their decay. Fermi's circle repeated the verse passed around the city by word of mouth by which an indignant Roman poet greeted the Nazi dictator:

Rome of travertine splendor
Patched with cardboard and plaster
Welcomes the little housepainter
As her next lord and master.

Italy would only be saved, Fermi told Segrè bitterly, if Mussolini went crazy and crawled on all fours.

The summer of 1938, July 14, brought the anti-Semitic *Manifesto della Razza* of which Segrè read in the Chicago newspaper on his way from New York to Berkeley. Italians are Aryans, the manifesto claimed. But "Jews do not belong to the Italian race." In Germany the vicious distinction had been commonplace; in Italy it was shocking. Italian Jews, only one in a thousand, were largely assimilated. The Fermis' two children—Giulio, a son, had been born in 1936—might be exempted since they were Catholic, born of a nominally Catholic father. But Laura was a Jew. She was spending the summer with the children in the Dolomites, the South Tyrol district named for the magnesian limestone that rings broad basin meadows with the flat, sharp formations Italians call "shovels." Enrico came up preoccupied in August to the meadow of San Martino di Castrozza to break the news. When Mussolini pushed through the first anti-Semitic laws early in September the Fermis decided to emigrate as soon as they could arrange their affairs. Fermi wrote four American universities and to avoid suspicion mailed each letter from a different Tyrolese town. Five schools shot back invitations. In confidence he accepted a professorship at Columbia and went off to Copenhagen to Bohr's annual gathering of the brethren.

The previous month the International Congress of Anthropological and Ethnological Sciences had invited Bohr to address it at a special session in Helsingør, Shakespeare's Elsinore, on the coast of Zealand north of Copenhagen. In the Renaissance castle there Denmark's most prominent citizen used the occasion to challenge Nazi racism publicly before the world. It was a brave statement by a brave man. Bohr understood that the major Western democracies were not likely to rally to the defense of his small, unprotected nation when Hitler eventually turned to look its way. George Placzek, a Bohemian theoretician working in Copenhagen whose tongue was almost as sharp as Pauli's, had already encapsulated that cruel truth. "Why should Hitler occupy Denmark?" Placzek quipped to Frisch one day. "He can just telephone, can't he?"

Against the brutal romanticism of German Blood and Earth, Bohr set the subtle corrective of complementarity. He spoke of "the dangers, well known to humanists, of judging from our own standpoint cultures developed within other societies." Complementarity, he proposed, offered a way to cope with the confusion. Subject and object interact to obscure each other in cultural comparisons as in physics and psychology; "we may truly say that different human cultures are complementary to each other. Indeed, each such culture represents a harmonious balance of traditional conventions by means of which latent possibilities of human life can unfold themselves in a way which reveals to us new aspects of its unlimited richness and variety."

The German delegates walked out. Bohr went on to say that the common aim of all science was "the gradual removal of prejudices," a complementary restorative to the usual pious characterization of science as a quest for incontrovertible truth. To a greater extent than any other scientist of the twentieth century Bohr perceived the institution of science to which he dedicated his life to be a profoundly political force in the world. The purpose of science, he believed, was to set men free. Totalitarianism, in Hannah Arendt's powerful image, drove toward "destroying all space between men and pressing men against each other." It was entirely in character that Bohr, at a time of increasing danger, publicly opposed that drive with the individualistic and enriching discretions of complementarity.

It was also entirely in character, when Fermi came to Copenhagen, that Bohr should lead him aside, take hold of his waistcoat button and whisper the message that his name had been mentioned for the Nobel Prize, a secret traditionally never foretold. Did Fermi wish his name withdrawn temporarily, given the political situation in Italy and the monetary restrictions, or would he like the selection process to go forward? Which was the same as telling Fermi he could have the Prize that year, 1938, if he wanted it and was welcome to use it to escape a homeland that threatened now despite the distinction he brought it to tear his wife from citizenship.

Leo Szilard's Cambridge collaborator Maurice Goldhaber emigrated to the United States in the late summer of 1938 and took up residence as an assistant professor of physics at the University of Illinois. Szilard appeared at Goldhaber's new apartment in Champaign in September to finish work they had begun together in England and stayed to follow the Munich crisis, for which purpose his host went out and bought a radio. Szilard understood, as Winston Churchill also understood and told his consituents at the end of August, that "the whole state of Europe and of the world is moving steadily towards a climax which cannot long be delayed." Before deciding between residency in England or the United States, Szilard said later, "I just thought I would wait and see."

The Sudetes, the border region of mountainous uplift that continues across Czechoslovakia from the Carpathians to the Erzgebirge, sustained at that time a German-speaking urban and industrialized population of some 2.3 million, about one-third of the population of western Czechoslovakia, formerly Bohemia. Nazi agitation began early in the Sudetenland; by 1935 a surrogate Nazi organization had become the largest political party in the Czechoslovakian republic. Hitler wanted Czechoslovakia next after Austria to facilitate his dream of German expansion, *Lebensraum*, and to deny airfields and support to the Soviet Union in the war he was well along in planning. The Sudetenland was his key. Czechoslovakia had built fortifications against German invasion across the Sudetes; after 1933 it imposed restrictions on the Sudeten Germans in an effort to protect that flank from subversion. Hitler opened his Czechoslovakian campaign even before the *Anschluss*, asserting the Reich's duty to protect the Sudeten Germans. Through the summer of 1938 German pressure on Czechoslovakia increased while the Western democracies maneuvered to avoid confrontation.

By the time Szilard began listening to Maurice Goldhaber's new radio the Czech government had established full martial law in the Sudetenland but also offered autonomy to the region in excess of what the Sudeten German Party had demanded. These developments prompted the British Prime Minister, Neville Chamberlain, to propose a meeting with Hitler. Hitler was delighted. He invited the Prime Minister to Berchtesgaden. The last outcome he wanted was a Czechoslovakian settlement. He signaled the Sudeten Nazis to increase their demands. Chamberlain heard the extremist proclamation on the radio on September 16 as he rode out by train from Munich: a call for immediate annexation to the German Reich. Back in

London on September 17 he recommended the annexation. Hitler, he said, "was in a fighting mood."

"The British and French cabinets at this time," writes Churchill, "presented a front of two overripe melons crushed together; whereas what was needed was a gleam of steel. On one thing they were all agreed: there should be no consultation with the Czechs. These should be confronted with the decision of their guardians. The Babes in the Wood had no worse treatment." The two governments, citing "conditions essential to security," decided that Czechoslovakia should cede to Germany all areas of the country where the population was more than 50 percent German. France had treaty obligations to Czechoslovakia but chose not to honor them. Facing such isolation, the small republic capitulated on September 21.

The Anglo-French proposals invoked self-determination for the German-speaking areas they defined. Hitler had agreed to such self-determination when he saw Chamberlain on September 16. Now the Prime Minister met with the Chancellor again, this time at Bad Godesberg on the Rhine outside Bonn, near Remagen. Hitler escalated his demands. "He told me," Chamberlain reported immediately afterward to the House of Commons, "that he never for one moment supposed that I should be able to come back and say that the principle [of self-determination] was accepted." Hitler wanted Czech acquiescence without self-determination by September 28 or he would invade. Chamberlain did not believe, however, he informed the Commons, that Hitler was deliberately deceiving him. The Nazi leader also told the Prime Minister "that this was the last of his territorial ambitions in Europe and that he had no wish to include in the Reich people of other races than Germans."

The Czechs mobilized a million and a half men. The French partly mobilized their army. The British fleet went active. At the same time a secret struggle may have been taking place between Hitler and the German general staff, which resisted any further plunge toward war. The result should have been stalemate, but Chamberlain moved again to concession. "Appeasement" was at that time a popular and not a pejorative word.

"How horrible, fantastic, incredible it is," the Prime Minister admonished the British people by radio on September 27, the night before Hitler's deadline, "that we should be digging trenches and trying on gas-masks here because of a quarrel in a faraway country between people of whom we know nothing!" He volunteered "to pay even a third visit to Germany." He was, he said, "a man of peace to the depths of my soul." He made the offer of a visit to Hitler at the same time directly by letter, and the Führer took him up on it the following afternoon. Chamberlain, French Premier Edouard Daladier, Mussolini and Hitler met at Munich on the evening of

September 29. By 2 A.M. the following morning the four leaders had agreed to Czech evacuation of the Sudetenland without self-determination within ten days beginning October 1. At Chamberlain's suggestion he and Hitler then met privately and agreed further to "regard the Agreement signed last night . . . as symbolic of the desire of our two peoples never to go to war with one another again." Before he left Munich, closeted with Mussolini, the Führer discussed Italian participation in the eventual invasion of the British Isles.

Chamberlain flew home. He read the joint declaration to the crowd gathered at the airport in welcome. Back in London he waved the declaration from an upper window of the Prime Minister's residence. "This is the second time there has come back from Germany to Downing Street peace with honour," he told the multitude below. "I believe it is peace in our time."

A group of refugee scientists was gathered outside the Clarendon Laboratory at Oxford the next morning discussing the Munich agreement when Frederick Lindemann drove up. Churchill had described the Czechoslovakian partition as amounting to "the complete surrender of the Western Democracies to the Nazi threat of force." Lindemann, Churchill's intimate adviser, was equally disgusted. One of the refugees asked him if he thought Chamberlain had something up his sleeve. "No," the Prof snapped, "something down his pants."

A cable came along to Lindemann then:

> HAVE ON ACCOUNT OF INTERNATIONAL SITUATION WITH GREAT REGRET POSTPONED MY SAILING FOR AN INDEFINITE PERIOD STOP WOULD BE VERY GRATEFUL IF YOU COULD CONSIDER ABSENCE AS LEAVE WITHOUT PAY STOP WRITING STOP PLEASE COMMUNICATE MY SINCERELY FELT GOOD WISHES TO ALL IN THESE DAYS OF GRAVE DECISIONS
>
> SZILARD

Szilard and Goldhaber found time during the crisis to write up a series of experiments with indium that they had started in England in 1937 and that Goldhaber and an Australian student, R. D. Hill, had completed before leaving for the United States. Szilard had thought indium might be a candidate for chain reaction but the results indicated that the radioactivity in indium of which Szilard had been suspicious was caused by a new type of reaction process, inelastic neutron scattering without neutron capture or loss. Szilard was discouraged. "As my knowledge of nuclear physics increased," he said later, "my faith in the possibility of a chain reaction gradually decreased." If other kinds of radiation also induced radioactivity in indium without producing neutrons, then he would have no more candidates for neutron multiplication and he would have to give up his belief in the process he still nicknamed "moonshine." That final experiment would be worked by friends at the University of Rochester in upstate New York, where he would travel in early December.

Otto Hahn opened the September 1938 issue of the *Comptes Rendus* to a shock. Part two of the Curie-Savitch study of the elusive 3.5-hour activity of uranium appeared there; amid much conjecture its most challenging conclusion was: "Taken altogether, the properties of $R_{3.5h}$ are those of lanthanum, from which it is not possible to separate it except by fractionation."*

Curie and Savitch believed that their $R_{3.5h}$ activity could be at least partly separated from lanthanum. It apparently did not occur to them that what was crystallizing out of solution might be another activity with a similar half-life, leaving a 3.5-hour lanthanum activity behind. They still could not believe—nor could anyone else—that uranium bombardment might produce an element thirty-five steps away down the periodic table. A Canadian radiochemist then visiting Dahlem records their German critic's response: "You can readily imagine Hahn's astonishment. . . . His reaction was that it just could not be, and that Curie and Savitch were very muddled up."

Despite his threat to Joliot in May, Hahn had not yet repeated the Curie-Savitch work. Now he passed the *Comptes Rendus* along to Fritz Strassmann. Strassmann studied the French paper and speculated that the muddle might have a physical cause—two similar radioactivities mixed together in the same solution. He told Hahn. Hahn laughed; the conclusion seemed improbable. On second thought, it was worth examining. As the Czechoslovakian crisis broke across Europe the two men bombarded uranium in peaceful Dahlem. They used a lanthanum carrier to precipitate rare-earth elements such as actinium (if any), a barium carrier to precipitate alkaline-earth elements such as radium (if any). (Carrier chemicals made it possible to separate from the parent solution the few thousand atoms of daughter substances produced by neutron bombardment. A chemically similar daughter substance, traceable by its unique half-life, would

*Fractionation—fractional crystallization—was a technique of chemical analysis pioneered by Marie Curie in the course of purifying polonium and radium. Most substances are more soluble at a high temperature than a low. Make a strong boiling solution of a substance—for rock candy, for example, sugar in water—cool the solution, and at some point the substance will emerge out of solution to form pure crystals. Fractional crystallization further involves separating out of the same solution several different, chemically similar substances by taking advantage of their tendency to crystallize at different temperatures according to differences in their atomic weights, lighter elements crystallizing first.

lodge in the spaces of the carrier's crystals as those regular solids formed from solution by chemical precipitation and would thus be carried away. Which carrier accomplished the carrying gave a clue to the part of the periodic table to which the unknown daughter substance belonged. Then it became a matter of further separating the daughter substance from the carrier by fractional crystallization, following it as before by tracing its characteristic radioactivity.)

After a hard week's work Hahn and Strassmann succeeded in identifying no fewer than sixteen different activities. Their barium separations gave them their most startling results: three previously unknown isotopes which they believed to be radium. They reported their findings in November in *Naturwissenschaften.* The creation of radium, element 88, from uranium, they pointed out, "must be due to the emission of two successive alpha particles."

If the physicists had found it hard to swallow that slow-neutron bombardment might produce thorium (90) or actinium (89), they found it even harder to swallow that it might produce radium. Lise Meitner wrote in warning from Stockholm suggesting pointedly that the two chemists check and recheck their results. Bohr invited Hahn to Copenhagen to lecture on the strange findings and tried to concoct a sufficiently crazy explanation:

> Bohr was skeptical and asked me if it was not highly improbable. . . . I had to reply that there was no other explanation, for our artificial radium could be separated only with weighable quantities of barium as carrier-substance. So apart from the radium only barium was present, and it was out of the question that it was anything but radium. Bohr suggested that these new radium isotopes of ours might perhaps in the end turn out to be strange transuranic elements.

Of the sixteen activities they had identified in neutron-bombarded uranium Hahn and Strassmann therefore now turned their full attention to the three controversial activities carried out of solution by barium.

Laura Fermi woke to the telephone early on the morning of November 10. A call would be placed from Stockholm, the operator advised her. Professor Fermi could expect it that evening at six.

Instantly awake to his wife's message, Fermi estimated the probability at 90 percent that the call would announce his Nobel Prize. As always he had planned conservatively, not counting on the award. The Fermis had prepared to leave for the United States from Italy shortly after the first of the year. Ostensibly Fermi was to lecture at Columbia for seven months and then return. For stays of longer than six months the United States required immigrant rather than tourist visas, and because Fermi was an academic he and his family could be granted such visas outside the Italian quota list. The ruse of a lecture series was devised to evade a drastic penalty: citizens leaving Italy permanently could take only the equivalent of fifty dollars with them out of the country. But the plan required circumspection. The Fermis could not sell their household goods or entirely empty their savings account without risking discovery. So the money from the Nobel Prize would be a godsend.

In the meantime they invested surreptitiously in what Fermi called "the refugee's trousseau." Laura's new coat was beaver and they distracted themselves on the day of the Stockholm call shopping for expensive watches. Diamonds, which had to be registered, they chose not to risk.

Near six o'clock the phone rang. It was Ginestra Amaldi wondering if they had heard. Everyone had gathered at the Amaldis to wait for the call, she reported. The Fermis turned on the six o'clock news. Laura long remembered the news:

> Hard, emphatic, pitiless, the commentator's voice read the second set of racial laws. The laws issued that day limited the activities and the civil status of the Jews. Their children were excluded from public schools. Jewish teachers were dismissed. Jewish lawyers, physicians, and other professionals could practice for Jewish clients only. Many Jewish firms were dissolved. "Aryan" servants were not allowed to work for Jews or to live in their homes. Jews were to be deprived of full citizenship rights, and their passports would be withdrawn.

The passports of Jews had already been marked. Fermi had contrived to keep his wife's passport clear.

They probably heard the news from Germany as well: of a vast pogrom the previous night—*Kristallnacht,* the night of glass. A seventeen-year-old Polish Jewish student had attempted to assassinate Ernst vom Rath, third secretary in the Germany Embassy in Paris, on November 7, in reprisal for Polish mistreatment of the student's parents. Vom Rath died on November 9 and the assassination served as an excuse for general anti-Semitic riot. Mobs torched synagogues, destroyed businesses and stores, dragged Jewish families from their homes and beat them in the streets. At least one hundred people died. A volume of plate glass was shattered that night across the Third Reich equal to half the annual production of its original Belgian sources. The SS arrested some thirty thousand Jewish men—"especially rich ones," its order had specified—and packed them into the concentration camps at Buchenwald, Dachau and Sachsenhausen, from which they could be ransomed only at the price of immediate pauperized emigration.

Fermi took the Stockholm call. The Nobel Prize, undivided, would be awarded for "your discovery of new radioactive substances belonging to the entire race of elements and for the discovery you made in the course of this work of the selective power of slow neutrons." In security the Fermis could leave the madness behind.

Lise Meitner had written Otto Hahn of her worries a few days before the Fermis arrived. "Most of the time I feel like a wind-up doll running on automatic," she told her old friend, "smiling along happily and empty of real life. From that you can judge for yourself how productive my efforts are at work. And still in the end I'm thankful for it because it forces me to keep my thoughts together, which isn't always easy." She was sorry Hahn's rheumatism had returned and was afraid he wasn't taking care of himself; she asked after Planck and von Laue by their private Hahn-Meitner nicknames, Max Sr. and Max Jr.; she greeted Hahn's wife, Edith, and wondered what Christmas plans he had for his son. His uranium work was "really very interesting." She hoped he would write again soon.

She was living in a small hotel room—there was hardly space to unpack—and having trouble sleeping. People told her she was too thin. Worse, conditions at the Physical Institute were not what she had expected them to be. A Swedish friend, Eva von Bahr-Bergius, a physicist she knew from Berlin who had been a lecturer at the University of Uppsala, had helped with arrangements and was gradually breaking the bad news. Manne Siegbahn had not wanted to take Meitner on. He had no money for her, he had complained; he could give her a place to work but no more. Von Bahr-Bergius had pursued the Nobel Foundation grant. But it provided nothing for equipment or assistance. Meitner blamed herself: "Of course it's my fault; I should have prepared much better and much earlier for my leaving, should at least have had drawings made of the most important apparatus [she would need]."

She was a strong woman, but she was miserable and alone. Hahn responded with sympathy. At midmonth she thanked him for that "dear letter," then changed moods and charged him with indifference: "Concerning myself I sometimes suspect you don't understand my way of thinking. . . . Right now I really don't know if anyone cares about my affairs at all or if they will ever be taken care of."

Hahn was pursuing Meitner's affairs as well as his own. With her moody letter at hand he stormed down to the revenue office, which was responsible for inventorying her furniture and other property before allowing its release, and laid on what he called "a little seizure of my 'ecstasy,' " after which "the matter went somewhat better." That news he wrote to Meitner

on Monday evening, December 19, from the KWI. Only then did he report why he had not yet left the laboratory:

> As much as I can through all of this I am working, and Strassmann is working untiringly, on the uranium activities. . . . It's almost 11 at night; Strassmann will return at 11:30 so that I can see about going home. The fact is, there's something so strange about the "radium isotopes" that for the time being we are mentioning it only to you. The half-lives of the three isotopes are quite precisely determined; they can be separated from *all* elements except barium; all the processes are in tune. Just one is not—unless there are extremely unusual coincidences: the fractionation doesn't work. Our radium isotopes act like *barium*.

Hahn and Strassmann worked in three rooms on the ground floor of the Kaiser Wilhelm Institute for Chemistry, the building with the *Pickelhaube* dome: Hahn's large personal chemistry laboratory north off the main lobby, a measurement room across the hall at the near end of the wing that extended northwest along Faradayweg and an irradiation room at the far end of the wing. They separated the three functions of irradiation, measurement and chemistry to avoid contaminating one with radiation from another. All the rooms were fitted with worktables of unfinished raw pine roughed out by a careful carpenter who took the trouble to add a graceful taper to the legs. On the table in the irradiation room rested cylinders of beeswax-colored paraffin like angelfood cakes drilled for the neutron sources, which were gram-strength radium salts mixed with beryllium powder. Handmade Geiger counters, fixed in hinged, hollowed-out bricks of lead shielding on the table in the measurement room, connected through thin coiling wires back to breadboard amplifiers worked by silvered vacuum tubes like inverted bud vases. The amplifiers actuated gleaming brass clockwork counters with numbers showing black through angled miniature windows on their spines. Kraftboard-covered 90-volt Pertrix dry batteries that powered the system packed a shelf below the table. Hahn's laboratory table held the brackets, beakers, flasks, funnels and filters of radiochemistry. The two men moved in their work from room to room on a regular schedule determined by the duration of the half-lives they were studying. There would have been a pungency of nitrates in the air, mingled with the aroma of Hahn's inevitable cigar.

In his fifty-ninth year Hahn stooped slightly but looked younger than his age. His hairline had receded and his eyebrows had grown bushy; he had trimmed back to the edge of his upper lip the waxed Prussian mustache of his youth; his brown eyes still sparkled with warmth. By now he was unquestionably the ablest radiochemist in the world. He needed all his forty years' experience to decode uranium.

He and Strassmann had begun their renewed examination of the three "radium" isotopes early in December by attempting a purer separation from uranium. Strassmann suggested using barium chloride as a carrier rather than the customary barium sulfate because the chloride, Hahn explains, "forms beautiful little crystals" of exceptional purity. They wanted to be sure their separations would be free of contamination from other bombardment products with similar half-lives, the difficulty that had muddled Curie and Savitch. The procedure for the 86-minute activity they were studying, which they called "Ra-III," required them to irradiate about fifteen grams of purified uranium for twelve hours, wait several hours for their more intense 14-minute "Ra-II" to retreat from the foreground by decaying, then add barium chloride as a carrier and accomplish the separation. The Ra-III came out of the uranium solution with the barium, but it refused then to remain behind during fractionation when the barium crystallized away. Instead it crystallized with the barium.

"The attempts to separate our artificial 'radium isotopes' from barium in this way were unsuccessful," Hahn would explain in his Nobel Prize lecture; "no enrichment of the 'radium' was obtained. It was natural to ascribe this lack of success to the exceptionally low intensity of our preparations. It was always a question of merely a few thousands of atoms, which could only be detected as individual particles by the Geiger-Müller counter. Such a small number of atoms could be carried away by the great excess of inactive barium without any increase or decrease being perceptible." To check that possibility they retrieved from storage a known radium isotope they often worked with, the isotope they called "mesothorium." They diluted it to match the pale radioactivity of their few thousand atoms of Ra-III, then ran it through barium precipitation and fractionation. It separated away cleanly from the barium. Their technique was not at fault.

On Saturday, December 17, the day after Hahn stormed the revenue office on behalf of Meitner's furniture, he and Strassmann carried out a further heroic check. They mixed Ra-III with dilute mesothorium and precipitated and fractionated the two substances *together*. Then the chemical evidence was certain, whatever it might mean in physical terms: the mesothorium remained in solution when the barium carrier crystallized out but Ra-III went off with the barium, distributing itself uniformly and indivisibly throughout the small pure crystals. Hahn wrote an enthusiastic note in his pocket appointment book to mark the day: "Exciting fractionation of radium/barium/mesothorium."

It seemed their "radium" isotopes must be barium, element 56, slightly more than half as heavy as uranium and with just over half its charge. Hahn and Strassmann could hardly believe it. They conceived an even more convincing experiment. If their "radium" was really radium, then by beta decay it ought to transform itself one step up the periodic table to actinium (89). If, on the other hand, it was barium (56), then by beta decay it ought to transform itself one step up to lanthanum (57). And lanthanum could be separated from actinium by fractionation. They were carrying out this definitive project late Monday night, December 19, when Hahn sent Meitner the news.

"Perhaps you can suggest some fantastic explanation," he wrote. "We understand that it really *can't* break up into barium. . . . So try to think of some other possibility. Barium isotopes with much higher atomic weights than 137? If you can think of anything that might be publishable, then the three of us would be together in this work after all. We don't believe this is foolishness or that contaminations are playing tricks on us."

He closed by wishing his friend a "somewhat bearable" Christmas. Fritz Strassmann added "very warm greetings and best wishes." Hahn posted the letter to Stockholm late at night on his way home.

The two men took time from their readings to attend the annual KWI Christmas party the next day, though Hahn had little joy of it with Meitner gone. They continued the actinium-lanthanum experiment even as they worked up the radium-barium findings. After the party the institute would close for Christmas; they kept a typist busy until the end but were unable to finish their report. Hahn had called Paul Rosbaud at *Naturwissenschaften*, told him the news and asked him to make space in the next issue. Rosbaud was willing to pull a less urgent paper from the journal but cautioned that the manuscript must be delivered no later than Friday, December 23. Hahn arranged for a laboratory assistant to serve as typist on Thursday. In the meantime he and Strassmann would carry on alone.

Meitner received Hahn's Monday-night letter in Stockholm on Wednesday, December 21. It was startling; if the results held she saw it meant the uranium nucleus must fracture and she immediately wrote him back:

> Your radium results are very amazing. A process that works with slow neutrons and leads to barium! . . . To me for the time being the hypothesis of such an extensive burst seems very difficult to accept, but we have experienced so many surprises in nuclear physics that one cannot say without hesitation about anything: "It's impossible."

She was traveling on Friday to the village of Kungälv in the west of Sweden for a week's vacation, she told Hahn; "if you write me in the meantime please address your letter there." She sent him and his family "warmest greetings . . . and much love and the very best for the New Year."

That day Hahn and Strassmann had finished the actinium-lanthanum experiment—and confirmed lanthanum from barium decay. In the late evening, after they turned off their counters, Hahn wrote his exiled colleague again. The paper was not yet finished; a phrase from the letter would be reworked to more cautious language for the final draft: "Our radium proofs convince us that as chemists we must come to the conclusion that the three carefully-studied isotopes are not radium, but, from the standpoint of the chemist, barium."

Hahn had hoped Meitner might quickly find some physical explanation for his unprecedented chemistry. That would strengthen his conclusion and also put Meitner's name on the paper, the best possible Christmas gift. With the lanthanum confirmation at hand he could no longer delay. As it was he had withheld the news from physicists on his own staff and at the new physics institute nearby. Someone else—Curie and Savitch, for example—might very well have made the same discovery. And whatever the explanation, the discovery was clearly of major importance, a reaction unlike any other yet found. "We cannot hush up the results," Hahn wrote Meitner, "even though they may be absurd in physical terms. You can see that you will be performing a good deed if you find an alternative [explanation]. When we finish tomorrow or the day after I will send you a copy of the manuscript. . . . The whole thing is not very well suited for *Naturwissenschaften*. But they will publish it quickly."

Hahn mailed the letter to Stockholm. He did not yet know about Meitner's Kungälv vacation.

Leo Szilard's work at the University of Rochester confirmed that no neutrons came out when indium was irradiated. On December 21, as Hahn and Meitner exchanged their excited letters, Szilard advised the British Admiralty by letter:

> Further experiments . . . have definitely cleared up the anomalies which I have observed in 1936. . . . In view of this new work it does not now seem necessary to maintain [my] patent . . . nor would the waiving of the secrecy of this patent serve any useful purpose. I beg therefore to suggest that the patent be withdrawn altogether.

Szilard's faith in the possibility of a chain reaction, as he said later, had "just about reached the vanishing point."

Hahn and Strassmann had originally titled their paper "On the radium isotopes produced by the neutron bombardment of uranium and their behavior." With their new data they realized "radium" would no longer do. They considered changing "radium" to "barium" throughout the paper. But most of it had been written before the lanthanum experiment firmed their convictions. They would have had to rewrite from beginning to end, "especially," says Hahn in retrospect, "since in view of this result its major portion was not especially interesting any more." Christmas and the journal deadline were upon them and they had no time. They decided to jury-rig what was on hand. The results would be no less effective for being inelegant. They substituted the noncommittal phrase "alkaline-earth metals" for "radium isotopes" in the title—both barium and radium are alkaline-earth metals, as are beryllium, magnesium, calcium and strontium. They went through the draft putting equivocal quotation marks around their many references to radium and actinium. Then they attached seven cautious paragraphs at the end.

"Now we still have to discuss some newer experiments," this final section began, "which we publish rather hesitantly due to their peculiar results." They then summarized their series of experiments:

> We wanted to identify beyond any doubt the chemical properties of the parent members of the radioactive series which were separated with the barium and which have been designated as "radium isotopes." We have carried out fractional crystallizations and fractional precipitations, a method which is well-known for concentrating (or diluting) radium in barium salt solutions. . . .
>
> When we made appropriate tests with radioactive barium samples which were free of any later decay products, *the results were always negative. The activity was distributed evenly among all the barium fractions*. . . . We come to the conclusion that our "radium isotopes" have the properties of barium. As chemists we should actually state that the new products are not radium, but rather barium itself. Other elements besides radium or barium are out of the question.

They discussed actinium then, distinguished their work from that of Curie and Savitch and pointed out that all so-called transuranics would have to be reexamined. Not quite prepared to usurp the prerogative of the physicists, they closed on a tentative note:

> As chemists we really ought to revise the decay scheme given above and insert the symbols Ba, La, Ce [cerium], in place of Ra, Ac, Th [thorium]. However as "nuclear chemists," working very close to the field of physics, we cannot bring ourselves yet to take such a drastic step which goes against all previous laws of nuclear physics. There could perhaps be a series of unusual coincidences which has given us false indications.

Promising further experiments, they prepared to release their news to the world. Hahn mailed the paper and then felt the whole thing to be so improbable "that I wished I could get the document back out of the mail box"; or Paul Rosbaud came around to the KWI the same evening to pick it up. Both stories survive Hahn's later recollection. Since Rosbaud knew the paper's importance and dated its receipt December 22, 1938, he probably picked it up. But Hahn also visited the mailbox that night, to send a carbon copy of the seminal paper to Lise Meitner in Stockholm. His misgivings at publishing without her—or some dawning glimmer of the fateful consequences that might follow his discovery—may have accounted for his remembered apprehension.

The Swedish village of Kungälv—the name means King's River—is located some ten miles above the dominant western harbor city of Göteborg and six miles inland from the Kattegat coast. The river, now called North River, descends from Lake Vänern, the largest freshwater lake in Western Europe; at Kungälv it has cut a sheer granite southward-facing bluff, the precipice of Fontin, 335 feet high. The modern village is built along a single cobblestone lane on the narrow talus between the bluff and the river, its back to the wall.

As Norwegian Kongahalla the village was founded at a less constricted place downstream around A.D. 800. But an island hill rises from the river at Kungälv and is thus guarded by a natural moat, a defensive geography which the precipice of Fontin reinforces. In 1308, to mark the border there between Norway and Sweden, the Norwegians began to build on that island hill a monumental granite fortress, Bohus' Fäste (i.e., King Bohus' Fort), sod-ridged block walls mazing inward and upward to a cylindrical tower of thick stone with a conical roof that dominates the entire coastal valley. An accident of placement of three of the deep windows that penetrate the tower—two open above, one centered below—transforms it into a face staring with hollow eyes toward the Fontin bluff. To soften the grimness of that face the people of the valley named the tower *Fars Hatt*, Father's Hat, as if it evoked a workman in a cap. Through four hundred years of occupation Bohus' Fäste was besieged fourteen times while the settlements in the valley were put to the torch and the graveyard filled on the island below its hard walls.

The village was ordered moved upriver onto the island in 1612. The Danes ruled Norway from the fifteenth century to the early nineteenth century; they ceded the Kungälv region, Bohuslän, to Sweden by the Treaty of Roskilde in 1658. Fire in 1676 burned the island village and its burghers shifted for safety to the narrow shore. They laid out their lane and

strip of houses extending west and east from a cobblestone marketplace where the talus widened to make room. Despite its fortress Kungälv is peaceful, especially in winter with the river frozen and a depth of clean snow on the ground. Its snug wooden houses, painted pastel, enclose rooms cozy with ships' chests and china cabinets and lace curtains, warmed by corner fireplaces faced with decorative tile, aromatic with coffee and baking. Eva von Bahr-Bergius and her husband Niklas built a house there in 1927, larger than most Kungälv houses but constructed in the same style. In 1938 Lise Meitner was alone in Stockholm. Otto Frisch was alone in Copenhagen, his mother, Meitner's sister, beyond reach in Vienna, his father incarcerated at Dachau, a victim of *Kristallnacht*. The Bergiuses therefore considerately invited aunt and nephew to Kungälv for Christmas dinner.

Meitner left Stockholm Friday morning, two days before Christmas. Frisch took the train ferry across from Denmark. His aunt arrived before him and registered at a quiet inn on Västra gatan, West Street, where they both would stay, a pale green building much like its modest neighbors but with a café on the ground floor. It faced a shadowed strip of garden north across the lane; above the stunted garden trees the dark bluff loomed. The other way, behind the inn, the flat, snow-covered flood plain of the river extended into open woods. The Bergiuses' house was a short walk eastward past the marketplace and the white church. Tired from travel, Frisch and Meitner met only briefly in the evening when Frisch came in.

In Copenhagen that winter he had been studying the magnetic behavior of neutrons. To further his work he needed a strong, uniform magnetic field, and on his way to Kungälv he had sketched out a large magnet he meant to design and build. He came downstairs on the morning before Christmas prepared to interest his aunt in his plans. She was already at breakfast and had no intention of discussing magnets: she had brought Hahn's December 19 letter downstairs with her and insisted Frisch read it. He did. "Barium," he told her, "I don't believe it. There's some mistake." He tried to change the subject to his magnet; she changed it back to barium. "Finally," says Meitner, ". . . we both became absorbed in my problem." They decided to go for a walk to see what they could puzzle out.

Frisch had brought cross-country skis and wanted to use them. He was concerned that his aunt would be unable to keep up. She could walk as fast as he could ski on level ground, she told him. She could and did. He fetched his skis and they went out, probably eastward to the Kungälv marketplace, which gave onto the flood plain of the river, then across the frozen river and into the open woods beyond.

"But it's impossible," Frisch remembers them saying in their collective effort to understand. "You couldn't chip a hundred particles off a nucleus in one blow. You couldn't even cut it across. If you tried to estimate the nuclear forces, all the bonds you'd have to cut all at once—it's fantastic. It's quite impossible that a nucleus could do that." Thirty years afterward Frisch summarized their thinking in more formal terms:

> But how could barium be formed from uranium? No larger fragments than protons or helium nuclei (alpha particles) had ever been chipped away from nuclei, and the thought that a large number of them should be chipped off at once could be dismissed; not enough energy was available to do that. Nor was it possible that the uranium nucleus could have been cleaved right across. Indeed a nucleus was not like a brittle solid that could be cleaved or broken; Bohr had stressed that a nucleus was much more like a liquid drop.

The liquid-drop model made a division of the nucleus seem possible. They sat down on a log. Meitner found a scrap of paper and a pencil in her purse. She drew circles. "Couldn't it be this sort of thing?"

Frisch: "Now, she always rather suffered from an inability to visualize things in three dimensions, whereas I had that ability quite well. I had, in fact, apparently come around to the same idea, and I drew a shape like a circle squashed in at two opposite points."

"Well, yes," Meitner said, "that is what I mean." She had meant to draw what Frisch had drawn, a liquid drop elongated like a dumbbell, but had drawn it end-on, indicating with a smaller dashed circle inside a larger solid circle the dumbbell's waist.

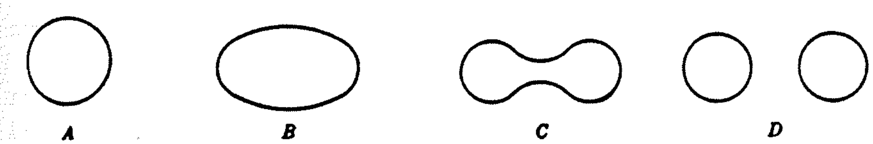
Frisch: "I remember that I immediately at that instant thought of the fact that electric charge diminishes surface tension." The liquid drop is held together by surface tension, the nucleus by the analogous strong force. But the electrical repulsion of the protons in the nucleus works against the strong force, and the heavier the element, the more intense the repulsion. Frisch continues:

> And so I promptly started to work out by how much the surface tension of a nucleus would be reduced. I don't know where we got all our numbers from, but I think I must have had a certain feeling for the binding energies and could make an estimate of the surface tension. Of course we knew the charge and the size reasonably well. And so, as an order of magnitude, the result was that at a charge [i.e., an atomic number] of approximately 100 the surface tension of the nucleus disappears; and therefore uranium at 92 must be pretty close to that instability.

They had discovered the reason no elements beyond uranium exist naturally in the world: the two forces working against each other in the nucleus eventually cancel each other out.

They pictured the uranium nucleus as a liquid drop gone wobbly with the looseness of its confinement and imagined it hit by even a barely energetic slow neutron. The neutron would add its energy to the whole. The nucleus would oscillate. In one of its many random modes of oscillation it might elongate. Since the strong force operates only over extremely short distances, the electric force repelling the two bulbs of an elongated drop would gain advantage. The two bulbs would push farther apart. A waist would form between them. The strong force would begin to regain the advantage within each of the two bulbs. It would work like surface tension to pull them into spheres. The electric repulsion would work at the same time to push the two separating spheres even farther apart.

Eventually the waist would give way. Two smaller nuclei would appear where one large nucleus had been before—barium and krypton, for example:



"Then," Frisch recalls, "Lise Meitner was saying that if you really do form two such fragments they would be pushed apart with great energy." They would be pushed apart by the mutual repulsion of their gathered protons at one-thirtieth the speed of light. Meitner or Frisch calculated that energy to be about 200 MeV: 200 million electron volts. An electron volt is the energy necessary to accelerate an electron through a potential difference of one volt. Two hundred million electron volts is not a large amount of energy, but it is an extremely large amount of energy from one atom. The most energetic chemical reactions release about 5 eV per atom. Ernest Lawrence was that year building a cyclotron with a nearly 200-ton magnet with which he hoped to accelerate particles by as much as 25 MeV. Frisch would calculate later that the energy from each bursting uranium nucleus would be sufficient to make a visible grain of sand visibly jump. In each mere gram of uranium there are about 2.5×10^{21} atoms, an absurdly large number, 25 followed by twenty zeros: 2,500,000,000,000,000,000,000.

They asked themselves what the source of all that energy could be. That was the crux of the problem and the reason no one had credited the possibility before. Neutron captures that had been observed before had involved much smaller energy releases.

When she was thirty-one, in 1909, Meitner had met Albert Einstein for the first time at a scientific conference in Salzburg. He "gave a lecture on the development of our views regarding the nature of radiation. At that time I certainly did not yet realize the full implications of his theory of relativity." She listened eagerly. In the course of the lecture Einstein used the theory of relativity to derive his equation $E = mc^2$, with which Meitner was then unfamiliar. Einstein showed thereby how to calculate the conversion of mass into energy. "These two facts," she reminisced in 1964, "were so overwhelmingly new and surprising that, to this day, I remember the lecture very well."

She remembered it in 1938, on the day before Christmas. She also "had the packing fractions in her head," says Frisch—she had memorized Francis Aston's numbers for the mass defects of nuclei. If the large uranium nucleus split into two smaller nuclei, the smaller nuclei would weigh less in total than their common parent. How much less? That was a calculation she could easily work: about one-fifth the mass of a proton less. Process one-fifth of the mass of a proton through $E = mc^2$. "One fifth of a proton mass," Frisch exclaims, "was just equivalent to 200 MeV. So here was the source for that energy; it all fitted!"

They converted not quite so suddenly as that. They may have been excited, but Meitner at least was profoundly wary. This new work called her previous four years' work with Hahn and Strassmann into doubt; if she was right about the one she was wrong about the other, just when she had escaped from Germany into the indifferent world of exile and needed most to confirm her reputation. "Lise Meitner sort of kept saying, 'We couldn't have seen it. This was so totally unexpected. Hahn is a good chemist and I trusted his chemistry to correspond to the elements he said they corresponded to. Who could have thought that it would be something so much lighter?' "

Christmas dinner at the Bergiuses' came and went. Frisch skied and Meitner walked. Nineteen thirty-eight was ticking to its end. With a week to pass in a small village they would certainly have visited the fortress and looked down from its ramparts onto the snow-covered valley, onto centuries of violent graves. Though they understood its energetics now, the discovery was still only physics to them; they did not yet imagine a chain reaction.

Hahn's letter of December 21, confirming lanthanum, was still not forwarded from Stockholm, nor was the carbon copy of the *Naturwissenschaften* paper. Hahn was eager to win Meitner's support and wrote Kungälv directly on the Wednesday after Christmas to woo her. Careful not to seem to usurp her place, he called the discovery his "barium fantasy" and questioned everything except the presence of barium and the absence of actinium, taking the humble chemist's part. "Naturally, I would be very

interested to hear your frank opinion. Perhaps you could compute and publish something." He had continued to hold off telling other physicists, though he itched for physical confirmation of his chemistry. It was as though a maker of hand axes had discovered fire by striking flints while the sorcerers pondered how to harness lightning. He might hardly believe his luck and urgently seek their authentication even though he knew what burned his hand was real.

The letter reached Kungälv on Thursday; by return mail that day Meitner responded that the radium-barium finding was "very exciting. Otto R[obert] and I have already puzzled over it." But she let slip no answer to the puzzle and she asked about the lanthanum result.

Friday she sent Hahn a postcard: "Today the manuscript arrived." An important page was missing but it was all "very amazing." Nothing more; Hahn must have bitten his lip.

In Dahlem Rosbaud passed along the galley proofs. Hahn was more certain now of his findings. The manuscript had set the barium results "against all previous laws of nuclear physics." He moderated the phrase in proof to "against all previous experience."

But even with the carbon copy, the missing page and the December 21 letter finally at hand in Kungälv, Meitner hesitated to leap. On January 1, after conveying New Year's greetings to Hahn, she wrote: "We have read your work very thoroughly and consider it *perhaps* possible energetically after all that such a heavy nucleus bursts." She veered off to worry about their misbegotten transuranics, "not a good reference for my new start." Frisch added a New Year's wish of his own and a more genial reservation: "If your new findings are really true, it would certainly be of the greatest interest and I am very curious about further results."

Meitner returned to Stockholm later that day and Frisch to Copenhagen. He was "keen to submit our speculations—it wasn't really more at that time—to Bohr." The note of hesitancy in their letter to Hahn suggests they sought the authority of Bohr's blessing. Frisch saw him on January 3: "I had hardly begun to tell him, when he struck his forehead with his hand and exclaimed, 'Oh what idiots we have all been! Oh but this is wonderful! This is just as it must be!' " Their conversation lasted only a few minutes, Frisch wrote his aunt that day, "since Bohr immediately and in every respect was in agreement with us. . . . [He] still wants to consider this quantitatively this evening and to talk with me again about it tomorrow."

In Stockholm that day Meitner had received Hahn's revised proofs. Independently they quieted her doubt. She wrote Hahn emphatically: "I am fairly *certain* now that you really have a splitting towards barium and I consider it a wonderful result for which I congratulate you and Strassmann very warmly. . . . You now have a wide, beautiful field of work ahead of you. And believe me, even though I stand here very empty-handed at the moment, I am still happy about the marvelousness of these findings."

Now those findings needed interpretation. Aunt and nephew outlined a theoretical paper by long-distance telephone. Frisch drafted it Friday, January 6, and that evening took the trolley to the House of Honor to discuss it with Bohr, who was leaving for the United States the next morning for a term of work at the Institute for Advanced Study. There was time the next morning to type only part of the draft; Frisch delivered two pages to Bohr at the train station from which he and his nineteen-year-old son Erik were departing for Göteborg harbor. On the assumption that Frisch would immediately send the paper along to *Nature* Bohr promised not to mention it to their American colleagues until he heard from Frisch that it had been received and was in press. Among the notes he brought to that final discussion Frisch mentioned an experiment to confirm by physical means the Dahlem chemistry.

Hahn's and Strassmann's article had been published in Berlin on January 6. When it arrived in Copenhagen the next day Frisch thought to go over the whole business with George Placzek. Placzek was characteristically skeptical and characteristically witty about it. Uranium already suffered from alpha decay, Frisch remembers him scoffing; to think that it could be made to burst as well "was like dissecting a man killed by a falling brick and finding that he would have died of cancer." Placzek suggested that Frisch use a cloud chamber to look for energetic fragments that would prove the nucleus had split. The institute's radium-based neutron sources would fog a cloud-chamber photograph with gamma radiation, Frisch realized. But a simple ionization chamber would do. "One would expect fast-moving nuclei, of atomic number about 40-50 and atomic weight 100-150, and up to 100 MeV energy to emerge from a layer of uranium bombarded with neutrons," he explained his experiment in a subsequent report. "In spite of their high energy, these nuclei should have a range, in air, of a few millimetres only, on account of their high effective charge . . . which implies very dense ionization." In the course of their short passage his highly charged nuclear fragments would strip about 3 million electrons from the nuclei of air gases. They should be easy to find.

His chamber consisted of "two metal plates separated by a glass ring about 1 cm. high." The charged plates, which would collect the air ions, connected to a simple amplifier, which connected to an oscilloscope. To the bottom plate he attached a piece of uranium-coated foil. He set up the experiment in the basement of the institute and retrieved three of the neutron sources from the covered well. He placed the sources close to the foil and

looked for the expected nuclei to emerge. Since they were highly energetic and strongly ionizing they would create quick, sharp, vertical pulses of the sweeping green beam of the oscilloscope.

Frisch started measurements on the afternoon of Friday, January 13, and "pulses at about the predicted amplitude and frequency (one or two per minute) were seen within a few hours." He ran checks with either the neutron sources or the uranium lining removed. He wrapped the sources with paraffin to slow the neutrons and "enhanced the effect by a factor of two." He continued measurements "until six in the morning to verify that the apparatus was working consistently." As had Werner Heisenberg before him, he lived upstairs at the institute; exhausted, he climbed the stairs to bed. He remembers thinking that 13 had proved once again to be his lucky number.

Even luckier than that: "At seven in the morning I was knocked out of bed by the postman who brought a telegram to say that my father had been released from concentration camp." His parents would move to Stockholm and share an apartment with his aunt, whose possessions, thanks to Hahn, were eventually shipped.

In "a state of slight confusion" Frisch spent the next day repeating the experiment for anyone who cared to see. One who came down in the morning to the basement laboratory was a black-haired, blue-eyed American biologist of Irish heritage named William A. Arnold who was studying on a Rockefeller Fellowship with George de Hevesy. Arnold was thirty-four, Frisch's age, on leave from the Hopkins Marine Station at Pacific Grove, California. He had made his way to Europe from San Francisco the previous September by freighter with his wife and young daughter. He could have gone to Berkeley to pick up radioisotope technique, but would have missed living in Copenhagen, learning from de Hevesy—would have missed contributing a coinage to the gamble that is history. Frisch showed the American the experiment and pointed out the pulses on the oscilloscope. "From the size of the spikes," Arnold recalls, "it was clear that they must represent 100-200 MeV, very much larger than the spikes from [uranium's natural background of] alpha particles."

> Later that day Frisch looked me up and said, "You work in a microbiology lab. What do you call the process in which one bacterium divides into two?" And I answered, "binary fission." He wanted to know if you could call it "fission" alone, and I said you could.

Frisch the sketch artist, good at visualizing as his aunt was not, had metamorphosed his liquid drop into a dividing living cell. Thereby the name for a multiplication of life became the name for a violent process of destruction. "I wrote home to my mother," says Frisch, "that I felt like someone who has caught an elephant by the tail."

Aunt and nephew conferred by telephone further over the weekend to prepare not one but two papers for *Nature*: a joint explanation of the reaction and Frisch's report of the confirming evidence of his experiment. Both reports—"Disintegration of uranium by neutrons: a new type of nuclear reaction" and "Physical evidence for the division of heavy nuclei under neutron bombardment"—used the new term "fission." Frisch finished the two papers on Monday evening, January 16, and posted them airmail to London the next morning. Since he and Bohr had already discussed the theoretical paper and since the experiment only confirmed the Hahn-Strassmann discovery, he did not hurry to let Bohr know.

Bohr sailed on the Swedish-American liner *Drottningholm* with his son Erik and the Belgian theoretician Léon Rosenfeld. "As we were boarding the ship," Rosenfeld recalls, "Bohr told me he had just been handed a note by Frisch, containing his and Lise Meitner's conclusions; we should 'try to understand it.' " That meant a working voyage; a blackboard was duly installed in Bohr's stateroom. The North Atlantic was stormy in that season; it made him "rather miserable, all the time on the verge of seasickness" but hardly stopped the work. The first question he wanted to answer was why, if the nucleus oscillated more or less randomly when it was bombarded, it seemed to prefer splitting into two parts rather than some other number. He was satisfied when he saw that the heaviest nuclei, because of their instability, require no more energy to split than they do to emit a single particle. It was a question of probabilities and two fragments were greatly more probable than a crowd.

The Fermis had arrived in New York on January 2, Laura feeling distinctly alien, Enrico announcing with his usual mock solemnity, "We have founded the American branch of the Fermi family." They put up temporarily at the King's Crown Hotel, opposite Columbia University, where Szilard was also living. George Pegram, the tall, soft-spoken Virginian who was chairman of the physics department and dean of graduate studies at Columbia, had met the Fermis as they debarked the *Franconia*; now in turn they waited at dockside to meet Bohr. The American theoretician John Archibald Wheeler, then twenty-nine years old, who had worked with Bohr in Copenhagen in the mid-1930s and would be working with him again at Princeton, joined them on the crowded West 57th Street pier. He had taught his regular Monday morning class, then caught a midday train.

As the *Drottningholm* berthed, at 1 P.M. on January 16, Laura Fermi saw Bohr on an upper deck leaning on the railing searching the crowd. She thought him worn when they met: "During the short time that had elapsed since our visit to his home, Professor Bohr seemed to have aged. For the last few months he had been extremely preoccupied about the political situation in Europe, and his worries showed on him. He stooped like a man carrying a heavy burden. His gaze, troubled and insecure, shifted from the one to the other of us, but stopped on none." No doubt Bohr was worried about Europe. He had also been seasick.

He had business in New York; he and Erik went off with the Fermis. Wheeler took Léon Rosenfeld along to Princeton. Keeping his promise to Frisch, Bohr had not mentioned the Hahn-Strassmann discovery and the Frisch-Meitner interpretation to either Fermi or Wheeler, but he had neglected to tell Rosenfeld of his pledge. Rosenfeld thought Frisch and Meitner had already sent off the paper that would give their work of interpretation priority. He passed on to Wheeler what Bohr had passed on to him. "In those days," Wheeler remembers, "I was in charge of the Monday evening journal club"—a weekly gathering of Princeton physicists to discuss the latest studies they found in physics journals, a way of keeping up. "It was the custom to get three things reported then, and here was something hot, as I had learned from Rosenfeld on the train." America first heard the news of the splitting of uranium—the term "fission" had not yet crossed the Atlantic—at the Princeton physics department journal club on the chill Monday evening of January 16, 1939. "The effect of my talk on the American physicists," says Rosenfeld ruefully, "was more spectacular than the fission phenomenon itself. They rushed about spreading the news in all directions."

Bohr arrived in Princeton the next day to take up residence and Rosenfeld casually mentioned the journal club talk. "I was immediately frightened," Bohr wrote his wife that night, "as I had promised Frisch I would wait until Hahn's note appeared and his own was sent off." It was more than a point of honor, though that would have been sufficient in itself to trigger the Bohr conscience. It was also that Meitner and Frisch were refugees who could use so spectacular a coup to establish themselves securely in exile. Bohr had at hand the work he and Rosenfeld had accomplished aboard the *Drottningholm*; for the next three days he labored to convert it into a letter to *Nature* that would give credit pointedly at the outset to Meitner and Frisch. Three days to produce a seven-hundred-word paper was for Niels Bohr great haste.

"Can you guess where I found out about [Bohr's news]?" asks Eugene Wigner. "In . . . the [Princeton] infirmary. Because I contracted jaundice and was in the infirmary for six weeks." Wigner and Princeton had not immediately got along; in 1936 "they said I should look for another job."

Princeton then, he thought, was "an ivory tower; people did not have any normal thinking about the facts of life and so forth and they looked down upon me." He sought another job and found one at the University of Wisconsin at Madison. "From the second day on I felt at home there. Somebody suggested we go to the track and we ran around the track and we were friends. We talked not only about the most difficult problems but about the daily events. We got down to earth almost." He met a young American woman in Wisconsin; they were quickly married. She became ill:

> I tried to conceal it from her that she had cancer and that there was no hope for her surviving. She was in a hospital in Madison and then she went to see her parents and I went with her but I didn't want to stay with her parents, of course, because I was, after all, a stranger to her parents. I went for a little while away to Michigan, Ann Arbor, and then I came back and saw her in her bed at her parents'. And then she told me essentially that she knows that she is close to death. She said, "Should I tell you where our suitcases are?" So she knew when she talked to me. I tried to conceal it from her because I felt that it would be better if a reasonably young person does not realize that she is doomed. Of course, we are all doomed.

He returned to Princeton in 1938, the university by then having more sensibly assessed his worth (a sophisticated and highly respected theoretician, Wigner shared the Nobel Prize in Physics in 1963 for his work on the structure of the nucleus).

After Bohr's arrival Szilard traveled down from New York to visit his sick friend and won a long-overdue surprise:

> Wigner told me of Hahn's discovery. Hahn found that uranium breaks into two parts when it absorbs a neutron. . . . When I heard this I immediately saw that these fragments, being heavier than corresponds to their charge, must emit neutrons, and if enough neutrons are emitted . . . then it should be, of course, possible to sustain a chain reaction. All the things which H. G. Wells predicted appeared suddenly real to me.

At Wigner's bedside in the Princeton infirmary the two Hungarians debated what to do.

In the meantime Bohr had sent his letter for *Nature* to Frisch in Copenhagen, asking him to forward it on "if, as I hope, Hahn's article has already been published and your and your aunt's note has already been submitted." He asked for the "latest news" on that front and wondered "how the experiments are proceeding." In a postscript he added that he had just seen the Hahn-Strassmann paper in *Naturwissenschaften.*

Ideas infect like viruses. The point of origin of the fission infection was Dahlem. From there it spread to Stockholm, to Kungälv, to Copenhagen. It crossed the Atlantic with Bohr and Rosenfeld. I. I. Rabi and the young California-born theoretician Willis Eugene Lamb, Jr., two Columbia men working at Princeton that week, both heard the news, Lamb perhaps from Wheeler, Rabi from Bohr himself. They returned to New York—"probably Friday night," Lamb thinks. Rabi says he told Fermi. In 1954 Fermi credited Lamb: "I remember one afternoon Willis Lamb came back very excited and said that Bohr had leaked out great news." Lamb recalls "spreading it around" but does not recall specifically telling Fermi. Possibly both men talked to the Italian laureate within a space of hours; it was information he of all physicists would most need to hear, since the Nobel lecture he had delivered only a month earlier, not yet printed, was now partly obsolete and an embarrassment. (Fermi confined revision to a footnote: "The discovery by Hahn and Strassmann . . . makes it necessary to reexamine all the problems of the transuranic elements, as many of them might be found to be products of a splitting of uranium." The many other radioactivities he and his group identified and his slow-neutron discovery still secured his Nobel Prize.)

Szilard also hoped to talk to Fermi: "I thought that if neutrons are in fact emitted in fission, this fact should be kept secret from the Germans. So I was very eager to contact Joliot and to contact Fermi, the two men who were most likely to think of this possibility." He had borrowed Wigner's apartment and had not yet left Princeton. "I got up one morning and wanted to go out. It was raining cats and dogs. I said, 'My God, I am going to catch cold!' Because at that time, the first years I was in America, each time I got wet I invariably caught a bad cold." He had to go out anyway. "I got wet and came home with a high fever, so I was not able to contact Fermi."

Fever or not, by January 25—Wednesday—Szilard had returned to New York, had seen the Hahn-Strassmann paper and was writing Lewis Strauss, whose patronage might now be more important than ever:

> I feel I ought to let you know of a very sensational new development in nuclear physics. In a paper . . . Hahn reports that he finds when bombarding uranium with neutrons the uranium breaking up. . . . This is entirely unexpected and exciting news for the average physicist. The Department of Physics at Princeton, where I spent the last few days, was like a stirred-up ant heap.
>
> Apart from the purely scientific interest there may be another aspect of this discovery, which so far does not seem to have caught the attention of those to whom I spoke. First of all it is obvious that the energy released in this new reaction must be very much higher than all previously known cases. . . .

> This in itself might make it possible to produce power by means of nuclear energy, but I do not think that this possibility is very exciting, for . . . the cost of investment would probably be too high to make the process worthwhile. . . .
>
> I see . . . possibilities in another direction. These might lead to large-scale production of energy and radioactive elements, unfortunately also perhaps to atomic bombs. This new discovery revives all the hopes and fears in this respect which I had in 1934 and 1935, and which I have as good as abandoned in the course of the last two years. At present I am running a high temperature and am therefore confined to my four walls, but perhaps I can tell you more about these new developments some other time.

The same day Fermi stepped into the office of John R. Dunning, a Columbia experimentalist whose specialty was neutrons, to propose an experiment. Dunning, his graduate student Herbert Anderson and others at Columbia had built a small cyclotron in the basement of Pupin Hall, the modern thirteen-story physics tower that faces downtown Manhattan from behind the library on the upper campus. A cyclotron was a potent source of neutrons; the two men talked about using it to perform an experiment similar to Frisch's experiment of January 13-14, of which they were as yet unaware. They discussed arrangements over lunch at the Columbia faculty club and afterward back at Pupin.

While Fermi was away from his desk Bohr arrived to tell him what he already knew. Finding an empty office, Bohr took the elevator to the basement, to the cyclotron area, where he turned up Herbert Anderson:

> He came right over and grabbed me by the shoulder. Bohr doesn't lecture you, he whispers in your ear. "Young man," he said, "let me explain to you about something new and exciting in physics." Then he told me about the splitting of the uranium nucleus and how naturally this fits in with the idea of the liquid drop. I was quite enchanted. Here was the great man himself, impressive in his bulk, sharing his excitement with me as if it were of the utmost importance for me to know what he had to say.

Bohr was en route to a conference in Washington on theoretical physics that would begin the next afternoon; he left to catch his train without seeing Fermi. As soon as Bohr was gone Anderson hunted up the Italian, who had returned to his office by now. "Before I had a chance to say anything," Anderson remembers, "he smiled in a friendly fashion and said, 'I think I know what you want to tell me. Let *me* explain it to you. . . .' I have to say that Fermi's explanation was even more dramatic than Bohr's."

Fermi helped Anderson and Dunning begin organizing the experiment he had discussed with Dunning earlier in the day. Anderson happened not long before to have built an ionization chamber and linear amplifier. "All we had to do was prepare a layer of uranium on one electrode and insert it into the chamber. That same afternoon we set up everything at the cyclotron. But the cyclotron was not working very well that day. Then I remembered some radon and beryllium which had been used as a source of neutrons in earlier experiments. It was a lucky thought." It came too late in the day; Fermi was also attending the Washington conference and had to leave. Anderson and Dunning closed up shop.

The Washington Conferences on Theoretical Physics, of which the 1939 meeting would be the fifth, were a George Gamow invention. He had stipulated their creation as a condition of his employment at George Washington University in 1934. He took Bohr's annual gathering in Copenhagen for a model; since there was no comparable assembly in the United States at the time, the Washington Conferences met with immediate success. At the instigation of Merle Tuve, Ernest Lawrence's boyhood friend and the driving force at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, the Carnegie Institution co-sponsored the conferences with GWU, though expenses were modest, for travel only, no more in total than five or six hundred dollars a year. People attended because they were interested. Edward Teller recalls the meetings as "in general small and exciting, thoroughly absorbing, and also a little tiring. Somehow, most of the running of the conferences Gamow left to me." The two men simply chose a topic and made up a list of invitees. Graduate students crowded in to listen. This year's topic was low-temperature physics.

Bohr sought out Gamow as soon as he arrived in Washington that evening. Gamow in turn called Teller: "Bohr has just come in. He has gone crazy. He says a neutron can split uranium." Teller thought of Fermi's experiments in Rome and the mess of radioactivities they produced and "suddenly understood the obvious." In Washington Fermi learned to his further disappointment from Bohr that Frisch was supposed to have done an experiment similar to the one left unfinished at Columbia. "Fermi . . . had no idea before that Frisch had made the experiment," Bohr wrote Margrethe a few days later. "I had no right to prevent others from experimentation, but I emphasized that Frisch had also spoken of an experiment in his notes. I said that it was all my fault that they all heard about Frisch and Meitner's explanation, and I earnestly asked them to wait [to make a public announcement] until I received a copy of Frisch's note to *Nature*, which I hoped would be waiting for me at Princeton [i.e., after the conference]." Fermi, understandably, seems to have argued against further delay.

Herbert Anderson returned to the basement of Pupin Hall that evening. He retrieved his neutron source. He calculated how many alpha particles the uranium oxide coated on a metal plate inside his ionization chamber would eject spontaneously in its normal process of radioactive decay: three thousand per minute. He calculated the probability of ten of those alphas appearing simultaneously to produce a spurious high-energy kick of the scanning beam of his oscilloscope: "practically never," he concluded in his laboratory notebook.

He set the neutron source beside the ionization chamber a little after 9 P.M. and began observing the effect on the oscilloscope. "Most kicks are due to .4 cm range α part[icles] [of approximately] .65 M[e]V," he noted. Then he saw what he was looking for: "Now large kicks which occur infrequently about 1 every 2 minutes." He counted them against the clock. In 60 minutes he had counted 33 large kicks. He removed the neutron source. "In 20 min" without a neutron source, he wrote, "0 counts." It was the first intentional observation of fission west of Copenhagen.

Dunning showed up later that evening, Anderson remembers, and "was very excited by the result I'd gotten." Anderson thought Dunning would telegraph Fermi immediately, but he seems not to have done so. Frisch, as he told Bohr later, had cabled no news of his confirming Copenhagen experiment because it seemed to him "just additional evidence of a discovery already made" and "cabling to you would have appeared unmodest to me." Dunning, despite his excitement at seeing the new phenomenon for himself, may have felt the same way.

Bohr woke to his dilemma. The conference would begin at two. As recently as three days previously he had written Frisch again, chiding him for not sending a copy of his and Meitner's *Nature* note. But he was less concerned now with that delay than he was with protecting the priority of Frisch's experiment, if any. Reluctantly he acceded to public announcement, stressing, he wrote Frisch afterward, "that no public account . . . could legitimately appear without mentioning your and your aunt's original interpretation of the Hahn results."

Fifty-one participants sat for a photograph in the course of the Fifth Washington Conference, and even a partial list of their names confirms the event's prestige. Otto Stern attended; Fermi; Bohr; Harold Urey of Columbia, who won the 1934 Nobel Prize in Chemistry for isolating a heavy form of hydrogen, deuterium, that carried a neutron in its nucleus; Gregory Breit, a waspish but inspired theoretician; Rabi; George Uhlenbeck, then at Columbia, who had been Paul Ehrenfest's assistant; Gamow; Teller; Hans Bethe down from Cornell; Léon Rosenfeld; Merle Tuve. Conspicuously absent was the Western crowd, probably because the two sponsoring institutions chose not to budget such long-distance travel.

Gamow opened the meeting by introducing Bohr. His news galvanized the room. A young physicist watching from the back saw an immediate application. Richard B. Roberts, Princeton-trained, worked with Tuve at the Department of Terrestrial Magnetism, the experimental section of

the Carnegie Institution, located in a parklike setting in the Chevy Chase area of the capital. Roberts—thin, vigorous, with a strong jaw and wavy dark hair—still remembered the occasion vividly in 1979 in a draft autobiography:

> The Theo. Phys. Conference for 1939 was on the topic of low temperatures and I was not eager to attend. However, I went down to sit in the back row of the meeting. . . . Bohr and Fermi arrived and Bohr proceeded to reveal his news concerning the Hahn and Strassmann experiments. . . . He also told of Meitner's interpretation that the uranium had split. As usual he mumbled and rambled so there was little in his talk beyond the bare facts. Fermi then took over and gave his usual elegant presentation including all the implications.

Roberts noted in a letter to his father the Monday after the conference ended that "Fermi also . . . described an obvious experiment to test the theory"—Frisch's experiment, Fermi's, Dunning's and Anderson's experiment. "The remarkable thing is that this reaction results in 200 million volts of energy liberated and brings back the possibility of atomic power."

Bohr was calling the fission fragments "splitters." For the time being everyone borrowed that comical usage. Lawrence R. Hafstad, a longtime associate of Tuve, was sitting beside Roberts. When Fermi finished, the two men looked at each other, got up, left the meeting and lit out for the DTM. If "splitters" issued forth from uranium they intended to be among the first to see them.

In New York that day Szilard dragged himself to the nearest Western Union office and cabled the British Admiralty:

> KINDLY DISREGARD MY RECENT LETTER STOP WRITING

The secret patent had revived.

Naturwissenschaften reached Paris about January 16. One of Frédéric Joliot's associates recalls that "in a rather moving meeting [Joliot] made a report on this result to Madame Joliot and myself after having locked himself in for a few days and not talked to anybody." The Joliot-Curies were once again appalled to find they had barely missed a major discovery. In the next few days Joliot independently deduced the large energy release and considered the possibility of a chain reaction, as Szilard had thought he might. He tried to track down the neutrons from fission first, found that approach difficult, then set up an experiment somewhat like Frisch's. He detected fission fragments on January 26.

* * *

The newest building on the DTM grounds was the Atomic Physics Observatory, the working contents of which had just been brought on line two weeks before: a new 5 MV pressure Van de Graaff generator that Tuve, Roberts and their colleagues had built for $51,000 to extend their studies in the structure of the nucleus. The Van de Graaff was named for the Alabama-born physicist who invented it, but Tuve was the first—in 1932—to put it to practical use in experiment. It was essentially a monumental static-electricity generator, an insulated motor-driven pulley belt that picked up ions from discharge needles in its metal base, carried them up through an insulated support cylinder into a smooth metal storage sphere and deposited them on the sphere. As ions accumulated the sphere's voltage increased. The voltage could then be discharged as a spark—Van de Graaffs discharging lightning-bolt sparks have been staples of mad-scientist movies—or drawn off to power an accelerator tube. The new machine was built inside a pear-shaped pressure tank, as large as the tank of a water tower, that helped reduce accidental sparking.

When Tuve had first proposed the Van de Graaff to the zoning board of the prosperous Chevy Chase neighborhood the board had turned him down. Smashing atoms smacked of industrial process and the neighborhood had its property values to consider. Tuve noted the popularity of the Naval Observatory, across Connecticut Avenue a few miles west, and rechristened his project the Atomic Physics Observatory, which it was. As the APO it won approval.

Roberts and Hafstad chose to work with the APO. They had intended to use the old 1 MV Van de Graaff in the building next door to make neutrons for their splitter experiment, but that machine's ion-source filament was burned out. Although the APO's vacuum accelerator tube leaked, finding the leak looked to be less tedious than replacing the filament. In fact it needed two days. Hafstad went off Friday night on a ski weekend and another young Tuve protégé, R. C. Meyer, took his place.

Roberts' laboratory notebook entries summarize Saturday's work:

> *Sat 4:30 PM*
> *Set up ionization chamber to try to detect*
>
> $U^{238}_{92} + n \rightarrow U^{239}_{92} \rightarrow Ba^{?}_{56} + Kr^{?}_{36}$
>
> *Neutrons from Li + D* [accelerated deuterium nuclei bombarding lithium]
>
> . . .
>
> *With uranium lined I.C. observed*
> α's [approximately] *1-2 mm and occasional 35 mm kicks* (*Ba + Kr?*)

The APO's target room was a small circular basement accessible down a steel ladder, a chilly kiva that smelled pleasantly of oil. As soon as Roberts saw the "tremendous pulses corresponding to very large energy release" he and Meyer ran every test they could think of. "We promptly tried the effect of paraffin (for slow neutrons) and then cadmium to remove the slow neutrons. We also tried all the other heavy elements available [to determine if they would split] and saw the same [i.e., fission] with thorium." Having made that original discovery (Frisch had made it independently in Copenhagen before them) they stopped to eat. "I told Tuve after supper and he immediately called Bohr and Fermi and they came out Saturday night."

Not only Bohr and Fermi came, in heavy, dark, pin-striped three-piece suits, Fermi swarthy with a day's growth of beard, but also Tuve; Rosenfeld; Teller; Erik Bohr, handsome in a heavy overcoat over a decorative Danish sweater; Gregory Breit, owlish in spectacles; and John A. Fleming, the conservative director of the DTM, who had the presence of mind to bring along a photographer. All except Teller posed in the target room with Meyer and Roberts for a historic photograph. The box of the ionization chamber in the foreground is stacked with disks of paraffin; Bohr holds the stub of an after-dinner cigar; Fermi's grin reveals the gap between his front teeth left by a baby tooth he shed late; Roberts looks into the camera weary but satisfied. Fermi had been amazed at the ionization pulses on the oscilloscope and had insisted they check for equipment malfunctions: he had never seen such pulses in Rome (they were captured by the aluminum foil Amaldi had wrapped around his uranium to block its alpha background). Bohr was still fretting. "I had to stand and look at the first [*sic*] experiment," he wrote Margrethe, "without knowing certainly if Frisch had done the same experiment and sent a note to *Nature*." Back at Princeton on Sunday he learned from other family letters that Frisch had. "There followed," Roberts concludes, "several days of excitement, press releases and phone calls."

Science reporter Thomas Henry had attended the conference; his story appeared in the *Washington Evening Star* on Saturday afternoon. The Associated Press picked it up. Shortened, it earned a place on an inside page of the Sunday *New York Times*. Dunning may have seen it there; he finally wired Fermi news that morning of the Columbia experiment. As Herbert Anderson remembers it, "Fermi . . . rushed back to Columbia and straightaway called me into his office. My notebook lists the experiments he felt we should do right away. The date was January 29, 1939." They had already agreed, says Anderson, that "I would teach him Americana, and he would teach me physics." Both lessons began in earnest.

The *San Francisco Chronicle* picked up the wire-service story. Luis W. Alvarez, Ernest Lawrence's tall, ice-blond protégé, a future Nobelist whose father was a prominent Mayo Clinic physician, read it at Berkeley sitting in a barber chair in Stevens Union having his hair cut. "So [I told] the barber to stop cutting my hair and I got right out of that barber chair and ran as fast as I could to the Radiation Lab . . . where my student Phil Abelson . . . had been [trying to identify] what transuranium elements were produced when neutrons hit uranium; he was so close to discovering fission that it was almost pitiful." Abelson still remembers the painful moment: "About 9:30 a.m. I heard the sound of running footsteps outside, and immediately afterward Alvarez burst into the laboratory. . . . When [he] told me the news, I almost went numb as I realized that I had come close but had missed a great discovery. . . . For nearly 24 hours I remained numb, not functioning very well. The next morning I was back to normal with a plan to proceed." By the end of the day Abelson found iodine as a decay product of tellurium from uranium irradiation, another way the nucleus could split (i.e., tellurium 52 + zirconium 40 = U 92).

Alvarez wired Gamow for details, learned of the Frisch experiment, then tracked down Oppenheimer:

> I remember telling Robert Oppenheimer that we were going to look for [ionization pulses from fission] and he said, "That's impossible" and gave a lot of theoretical reasons why fission couldn't really happen. When I invited him over to look at the oscilloscope later, when we saw the big pulses, I would say that in less than fifteen minutes Robert had decided that this was indeed a real effect and . . . he had decided that some neutrons would probably boil off in the reaction, and that you could make bombs and generate power, all inside of a few minutes. . . . It was amazing to see how rapidly his mind worked, and he came to the right conclusions.

The following Saturday Oppenheimer discussed the discovery in a letter to a friend at Caltech, outlining all the experiments Alvarez and others had accomplished during the week and speculating on applications:

> The U business is unbelievable. We first saw it in the papers, wired for more dope, and have had a lot of reports since. . . In how many ways does the U come apart? At random, as one might guess, or only in certain ways? And most of all, are there many neutrons that come off during the splitting, or from the excited pieces? If there are, then a 10 cm cube of U deuteride (one would need the D [deuterium, heavy hydrogen] to slow them without capture) should be quite something. What do you think? It is I think exciting, not in the rare way of positrons and mesotrons, but in a good honest practical way.

The next day, in a letter to George Uhlenbeck at Columbia, "quite something" became "might very well blow itself to hell." One of Oppenheimer's students, the American theoretical physicist Philip Morrison, recalls that "when fission was discovered, within perhaps a week there was on the blackboard in Robert Oppenheimer's office a drawing—a very bad, an execrable drawing—of a bomb."

Enrico Fermi made similar estimates. George Uhlenbeck, who shared an office with him in Pupin Hall, was there one day to overhear him. Fermi was standing at his panoramic office window high in the physics tower looking down the gray winter length of Manhattan Island, its streets alive as always with vendors and taxis and crowds. He cupped his hands as if he were holding a ball. "A little bomb like that," he said simply, for once not lightly mocking, "and it would all disappear."