Lise Meitner and the Discovery of Fission

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Lise Meitner is associated with the discovery of fission, but there are always gaps and discrepancies: the team in Berlin was Meitner, Hahn, and Strassmann, yet the Nobel Prize went to Hahn alone; Meitner was not in Berlin for the discovery of barium, yet she was the first to offer a theoretical explanation; she and Otto Hahn were best friends, yet in his autobiographies, where most of us first meet her, she is a two-dimensional figure, pale and lifeless. At times she fades in and out of view, as in the Hahn/Strassmann Golden Anniversary Symposium where two historians of science spoke of her—yet the title did not bear her name. Often she is rendered completely invisible, as in the Deutsches Museum in Munich, for example, where the apparatus she designed for the neutron irradiation of uranium—the lead vessels, sources, paraffin blocks, counters, amplifiers that she assembled on a plain wood table in her laboratory in her physics section of the Kaiser Wilhelm Institute for Chemistry—is now displayed under a sign which reads: "Worktable of Otto Hahn". A plaque nearby mentions Fritz Strassmann; Lise Meitner does not appear at all (*1*).

To understand the life and work of Meitner, and to gain new insight into the discovery of fission itself, these gaps must be filled and these discrepancies explained. For the record shows that Lise Meitner was not only a member of the Berlin team but its leader, that even after fleeing Germany she continued her collaboration, that she provided crucial direction that led quickly to the discovery of barium. The record also shows that the same racial policies that forced Meitner out of Germany made it dangerous for Hahn to acknowledge their continued ties and soon caused him to suppress and deny not only Meitner's hidden collaboration, but the value of nearly everything she had done before. And because Hahn went on to great acclaim and dominated much of the history of this discovery, we find a distortion of the record that persists to this day. In bringing Meitner's role to light we gain not only appreciation for this remarkable scientist, but new understanding of the paths to the discovery, and a sobering reminder of its political context.

On 13 July 1938 Meitner fled Germany, illegally and in fear, with 10 marks in her purse and a few summer clothes (*2*). Things would get worse for Jews in Germany, but to her at that moment it seemed as bad as it could get, a forced separation from everything she loved: work, position, friends. She had known Otto Hahn for 30 years; she was godmother to his son, he was her "colleague–brother". She left behind her physics section, her creation, whose success in the 1920's had brought worldwide recognition to the Institute as a whole (*3*). And she was forced to leave the unfinished uranium investigation. The record shows that this project was hers from the start—that she was fascinated by Fermi's neutron irradiation experiments in 1934 (*4*), that she decided to bring them to Berlin, that it took weeks to persuade Hahn to join her (*5*), and that she led the uranium investigation for four years, until the night before she left. The publications of that period show that in context, direction, and purpose this was a problem of nuclear physics. The chemists, Hahn and Strassmann, steadily disentangled one presumed transuranium element after another, reporting their results with confidence and finality. The parallel physics papers, in contrast, resemble a battleground, as Meitner used every weapon in her arsenal to uncover and interpret the nuclear processes involved—cloud chamber photographs, fast and slow neutrons, measurements of resonance energies, yields, cross sections, the theoretical possibilities of nuclear isomerism—ending, finally, in uneasy stalemate, with Meitner unable to accept the constellation of difficulties that unaccountably arose from the relatively modest event of neutron capture by U-238: the unexplainable *triple* isomers[1] of U-239, the two parallel transuranium series with their baffling *inherited* isomerism, and the improbably long sequence of beta decays. Later it would be obvious that the investigation was led astray by two false assumptions: one from physics—that nuclear reactions would involve small changes only—and one from chemistry—that elements beyond uranium would be transition elements homologous to Re, Os, etc. For four years these two errors unserendipitously lent credence to each other. Chemistry, however, did not sense the error—it was physics that wrestled with unsolved difficulties and sustained the investigation. Thus in 1937, as Hahn was affirming in *Chemische Berichte* that the transuranium elements were "no longer in doubt" and their chemical distinction from known elements needed "no further discussion" (*6*), Meitner, in *Zeitschrift für Physik*, was judging the nuclear processes that produced those transuranium elements "very difficult to reconcile with current ideas of nuclear structure" (*7*) and preparing yet another experimental assault.

Politics was never absent in Nazi Germany, and political compatibility helped form and stabilize the Berlin team: Meitner, "non-Aryan"; Hahn, anti-Nazi; Strassmann, the unusually principled and courageous younger man who forfeited his career by refusing to join the Party or the Nazi-controlled Association of German Chemists. (Strassmann was truly a hero: at least once during the war he and his wife risked their lives by hiding a Jew in their apartment for months, this at a time when their child was very small (*8*). In 1986 Strassmann was posthumously honored by the Israeli Holocaust Memorial in Jerusalem (*9*), a distinction shared by very few.) To some extent political considerations influenced the selection of the problem itself—transuranium elements were a guaranteed international sensation—protection, it was hoped, for these political undesirables; their need for recognition certainly sharpened their competition with Irene Curie and her co-workers in Paris (*10*).

In September 1938 Meitner arrived in Stockholm for a position in the Nobel Institute for Physics under Manne Siegbahn. The institute was new, the organization and equipment incomplete, and her welcome there distinctly cool. On the other hand, it was not difficult to maintain contact with Berlin—mail service was very fast—and Meitner and Hahn wrote to each other every few days. Toward

[1] Nuclear isomers, nuclei with the same atomic number and mass but different radioactive properties.

the end of September they were greatly distracted by the fear of war over Czechoslovakia, but after the Munich Conference their thoughts returned to the uranium project, including an unexplained 3.5-hour activity whose existence Irene Curie and Paul Savitch had maintained for nearly a year (*11*). On October 25 Hahn wrote to Meitner that a detailed new article (*12*) by the French group had convinced him and Strassmann to look for radium in the 3.5-hour activity (*13*). As Meitner peppered Hahn with questions (*14*), Hahn and Strassmann found three radium isomers and their respective actinium decay products, all formed under the same reaction conditions as the known transuranium elements (*15*). Meitner was still a member of their team. "We would be grateful", Hahn wrote (*16*), "if you would think about how an alpha decay [U to Th to Ra] probably also with slow neutrons, could come about and directly yield several isomers." Meitner was "burning with desire to think about how Ra or Ac isotopes can result" and asked for more details, assuring Hahn she would tell no one (*17*). The uranium problem was murkier than ever.

In November 1938 Meitner and Hahn met in Copenhagen. This meeting was a well-kept secret at the time—in Germany things were getting increasingly rough—and it is still not widely known: they never mentioned it in their letters nor spoke of it to others, and later, when Hahn wrote his memoirs, he seems to have forgotten it. But the guest-book in Bohr's Institute (*18*) shows that they were both there on November 13 and 14, and we know from Hahn's own pocket calendar (*19*) that Meitner met his train at 6:48 on the morning of November 13 (three days after *Kristallnacht*), that they had breakfast together and talked for hours, that Niels and Margrethe Bohr invited them both to their home the next morning. Meitner had been an assertive presence in Berlin—often quoted (*20*) is her "Hahn, dear, of physics you understand nothing! (*Hähnchen, von Physik verstehst Du Nichts!*)"—and we can be sure she told Hahn in no uncertain terms that the new radium isomers and their actinium daughters represented everything that was wrong with the transuranium elements and more. For here again were not only triple isomers but inherited triple isomers; worse, the formation of radium from uranium required a wildly improbable double alpha emission, and still worse, neutron-induced alpha emission was known to require *fast* neutrons, but these radium isomers actually intensified when *slow* neutrons were used. They were a physicist's nightmare, and Meitner urged Hahn to reexamine these radium findings more rigorously than before. This was the message Hahn brought back to Berlin. He did not tell Strassmann he had actually spoken to Meitner, but Strassmann understood that this directive could only have come from her. He assumed she had written Hahn a letter; he still thought of her as the intellectual leader of their team. "Fortunately," Strassmann wrote later (*21*), "L. Meitner's opinion and judgement carried so much weight with us in Berlin that we immediately undertook the necessary control experiments."

Thus began the fractional crystallization and indicator experiments that culminated in the discovery of barium. Meitner meanwhile returned unhappily to Stockholm. It was a bad time for her: she was despondent about her work situation and terribly worried about a sister trapped in Vienna and a brother-in-law in Dachau. "I often see myself as a wind-up doll, who does some things automatically with a friendly smile, but has no real life in her. From that you can tell how useful my work is. And yet in the end I am grateful for it because it forces me to collect my thoughts" (*22*). She pressed Hahn for news of the radium experiments.

On December 19 Hahn wrote to Meitner for the first time about results "so peculiar that for now we shall tell only you . . . Our Ra isotopes act like *Ba*." It was a "frightful conclusion" and Hahn pleaded for interpretation. "Perhaps you can come up with some fantastic explanation. We ourselves know that it *can't* actually burst into Ba . . . So please think whether there is any possibility—perhaps a Ba isotope with an atomic weight much higher than 137?" Again two days later: "How beautiful and exciting it would have been if we could have worked together as before . . . . We cannot suppress our results, even if they may be physically absurd. You see, you will do a good deed if you can find a way out of this . . ." (*23*). In these letters we see the dual roles of chemistry and physics: Hahn was sure of the chemistry, but, with Meitner gone, uncertain overall. And so, in the report for *Naturwissenschaften* he[2] hedged (*24*): "As chemists we should actually say that the new substances are not radium but barium itself . . . . As 'nuclear chemists' fairly close to physics we cannot yet bring ourselves to take such a drastic step that contradicts all previous experience in nuclear physics. There could still perhaps be a series of unusual coincidences that has given us deceptive results."

To Meitner he wrote (*23*) "If there is anything you could propose that you could publish, then it would still in a way be work by the three of us!" This was not a suggestion that they publish together—both knew that would have been politically impossible—but instead an expression of their ongoing collaboration, confirmation that she was still very much a member of the team. As Strassmann later said (*25*), "What does it matter that Lise Meitner did not take *direct* part in the 'discovery'?? . . . [She] has been the intellectual leader of our team and therefore she was one of us, even if she was not actually present for the 'discovery of fission.' "

When Meitner received Hahn's first letter on December 21, she responded instantly. "Your radium results are very puzzling. A reaction with slow neutrons that supposedly leads to barium! . . . At the moment the idea of such a large-scale breakup [*weitgehend Zerplatzens*] seems very difficult to me, but in nuclear physics we have experienced so many surprises that one cannot unconditionally say, 'It is impossible' " (*26*).

Imagine what those words meant to Hahn! They came from the same Lise Meitner who just one month before had vehemently opposed the radium isomers; now she was puzzled but not incredulous: "One cannot say, 'It is impossible' "! Her letter arrived in Berlin on December 23 (*27*), surely the best Christmas present Hahn received. Now he was free to think about what she had called the large-scale breakup of the uranium nucleus. Four days later, on December 27, he added a short paragraph to the page proofs of the *Naturwissenschaften* article, suggesting that the "transuranium elements", which had always been thought to be higher homologues of Re, Os, Ir . . . might instead be the lower homologues Ma [Tc], Ru, Rh . . . . "The sum of the mass numbers of Ba + Ma [Tc], e.g., 138 + 101, equals 239!" (*28*). By suggesting that the nucleus actually split, this paragraph added substance to the paper, even though the primacy of atomic number seems to have deserted Hahn momentarily. It is highly unlikely that the very cautious Hahn would have put this "fantasy", as he called it, into print, were it not for Meitner's encouragement.

Meanwhile Meitner and her nephew, physicist Otto Robert Frisch, were celebrating Christmas with friends in the small town of Küngalv, near Göteborg. Walking in the snow together they developed the first theory for nuclear fission: the nucleus as an unstable liquid drop, bound by a vanishingly small surface tension, ready to stretch and split at the slightest provocation into two fragments with the release of enormous energy. From his narratives (*29*) we know of Frisch's delight. Meitner, however, was stunned. For she realized that barium formed under conditions identical to the "transuranium elements", so that they too must be fission fragments, not elements beyond uranium at all. It was a terrible blow. To be excluded from this wonderful discovery

[2] The Hahn–Strassmann articles were written exclusively by Hahn.

was bad enough, but then, to have four years of work proved wrong! "If the transuranium elements should disappear," she wrote to Hahn on New Year's Day (*30*), "you are in a much better position than I, since you and Strassmann have discovered it yourselves, while I only have [four] years of work to refute, not a very good recommendation for my new beginning."

Two days later, after returning to Stockholm and studying the literature, she was sure. "I am now quite *certain* that the two of you really do have a splitting to Ba, and I find that to be a truly beautiful result, for which I most heartily congratulate you and Strassmann .... Both of you now have a wide, beautiful field of work ahead of you. And believe me, although I stand here with very empty hands, I am nevertheless happy for the wonder of these findings" (*31*).

Meitner's hands were indeed empty—emptier, in fact, than when she left Berlin six months before. For then her reputation had been intact, and now she feared for it, worrying (*32*) that "people will say that the three did nonsense and, now that one is gone, the other two made it right." She hoped that Hahn and Strassmann would, in their next publication, acknowledge the importance of their earlier work together. She could hardly imagine that within a month Hahn would divorce the discovery from physics and claim it for chemistry alone.

The first weeks of January 1939 were actually good for Meitner. Her theoretical work with Frisch was exciting, and she stopped mourning the lost "transuranium elements" once she realized that as fission fragments they substantiated the fission hypothesis, making multiple isomerism unnecessary and accounting for the long sequence of beta decays. To her relief, she was able to salvage something from her earlier work: one uranium isotope, the 23-minute U-239, was still valid and was the prospective parent of the first true element 93. On January 16 Meitner and Frisch sent their manuscript to *Nature*, and a few days later a copy went to Hahn and Strassmann. It contained a number of experimentally verifiable predictions, including the suggestion, obvious to Meitner and Frisch from the start, that the fission fragment accompanying barium ($Z = 56$) must be krypton ($Z = 36$), which would decay to rubidium, strontium, yttrium, etc. (*33*).

For Hahn January was quite unpleasant. After the barium finding appeared in print on January 6, physicists in the Institute complained loudly because he had not immediately shared the result with them. Even after they knew of barium, however, they were not much help. Without clear direction from physics, Hahn became anxious about the barium, and he and Strassmann verified it over and over. Not until the Meitner–Frisch manuscript arrived did they look for and find krypton and its decay products (*34*).

Hahn's political vulnerability was real: grumbling Party members filled his Institute, and there was no shortage of opportunistic Nazi scientists lying in wait outside. He could not afford to make mistakes. And so, when Hahn reported the newest results to *Naturwissenschaften* a few days later (*35*), he played it safe, giving no indication that Meitner and Frisch's krypton suggestion was at least concurrent and may have initiated their search for krypton and its decay products. He mentioned the Meitner–Frisch manuscript only at the very end, making their theoretical discussion seem a rather meaningless description of experiments already done.

Meitner reacted with complete despair. It seemed to her that nothing she had done before or after the discovery would be recognized, her reputation was irreparably damaged, and she could never improve her status in Siegbahn's Institute. She had no rights there, she wrote to Hahn, no equipment; she had ideas for follow-up experiments but could not do them; she feared people would think she had contributed nothing to the fission discovery. Above all she did not get along with Siegbahn. The self-confidence she had slowly gained during 30 years in Berlin was eroding away. "Now Siegbahn will gradually believe—especially after your beautiful results—that I never did anything and you also did all the physics in Dahlem. I am gradually losing all my courage. Forgive this unhappy letter. I never wrote before how bad it really is. Sometimes I do not know what to do with my life. Most probably there are many people who have emigrated who feel as I do, but still it is very hard" (*36*).

On a personal level Hahn was sympathetic; scientifically he had drawn the line. The krypton suggestion, he told her, had come from all sides, "after the thing with atomic weights [Hahn's Ba + Ma (Tc) idea] didn't work." He had refused to talk to some physicists: "I fear that this is held against me .... I do not want to confess to these gentlemen that you were the only one who learned of everything immediately." Anyway, he did not understand why she was so upset, for the discovery owed nothing to physics. "[Strassmann and I] absolutely never did physics, but instead we did chemical separations over and over again. We know our limits and ... in this case it was useful to do only chemistry .... For me the uranium work [fission discovery] is a gift from heaven. Namely I was fearful sometimes that ... [I would lose] part of the Institute" (*37*).

This is a most revealing letter. Six weeks earlier Hahn had regarded fission as "work by the three of us"; now he was pulling the chemistry out of its physics context and *defining* the discovery of fission to be those three weeks of chemical separations that he and Strassmann had done in December. Bluntly and without apology he was divorcing fission from physics and himself from Meitner in order to shore up his prestige and cut his politically dangerous ties to a "non-Aryan" in exile. From Hahn's own words we see that this had nothing to do with science and everything to do with fear and his struggle for professional survival. His metaphor for fission as a "gift from heaven" says it all: it came from heaven, an answer to his prayers, a miracle to save him and his Institute—and it was a gift, no strings attached, no ties to physics, no ties to Meitner.

Hahn never retreated from this point of view. Tenaciously he clung to his "heaven-sent" discovery, and in the end he prevailed, his position safe, his Institute secure.

The damage to Meitner's reputation was, as she had first feared, very great. It was not only Hahn's disavowal, of course, but the collection of misfortunes that made his disavowal believable: her forced emigration, her absence from Berlin, her inability in Stockholm to follow up the discovery quickly with experiments of her own, her poor relationship with Manne Siegbahn, one of Sweden's most influential physicists, who without question made sure she would not receive a Nobel Prize.

When the war was over and the Nazis were gone, Hahn did not try to make restitution to Meitner. On the contrary, the fission that had brought him world renown would serve in a new struggle—not for himself this time, but for his defeated country. Recognized for his scientific achievements, he would lead the rebuilding of German science; respected worldwide as a decent man, he would become a symbol of the new, decent Germany. He did not look back to the injustices of the Third Reich. And he never looked back to his work with Meitner nor conceded that physics had been essential to the discovery of fission.

Eventually, in fact, Hahn would imply that physics had *impeded* the discovery. He would emphasize the physicists' false assumption of small nuclear changes, say little or nothing of the equally false assumptions of chemistry, and neglect Meitner's leadership entirely (*38*). According to Werner Heisenberg (*39*), Hahn was even known to say, "I don't know, I'm afraid Lischen would have forbidden me to discover the splitting of uranium." This quote is second-hand, from a not entirely reliable source, but consistent with Hahn's own writings and actions.

In time Hahn wrote several memoirs and autobiographies. In none of them did he recount how Meitner brought the

uranium project to Berlin, how she persuaded him to join her, or led the Berlin team. Never did he write of their November meeting in Copenhagen, of her crucial criticism there, or her encouragement later.

Meitner remained a far more private person. During her lifetime she never consented to a biography and was repelled by the idea of an autobiography. She did, however, preserve a rich collection of her letters and documents. I believe she was making certain that her part of the story would not remain buried forever.

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