A Nobel Tale of Postwar Injustice

Recently released Swedish documents reveal why Lise Meitner, codiscoverer of nuclear fission, did not receive the 1946 physics prize for her theoretical interpretation of the process.

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In November 1945, three months after the end of World War II, a narrow majority of the members of the Royal Swedish Academy of Sciences decided to award the 1944 Nobel Prize in Chemistry to Otto Hahn for the discovery of nuclear fission. The award was and still remains controversial, primarily because Hahn's Berlin colleagues, the chemist Fritz Strassmann and the physicist Lise Meitner, were not included. Probably, Strassmann was ignored because he was not a senior scientist. Meitner's exclusion, however, points to other flaws in the decision process, and to four factors in particular: the difficulty of evaluating an interdisciplinary discovery, a lack of expertise in theoretical physics, Sweden's scientific and political isolation during the war, and a general failure of the evaluation committees to appreciate the extent to which German persecution of Jews skewed the published scientific record.[1]

The Royal Swedish Academy of Sciences' official records of Nobel Prize deliberations are kept closed for 50 years and only then released to scholars. The recently opened documents for 1945 and 1946 reveal that these above four factors shaped the outcome of Hahn's Nobel Prize in Chemistry. They were still in effect when the work of Meitner and her nephew, the physicist Otto Robert Frisch, was evaluated for the 1946 physics prize. This article briefly examines the two halves of the story behind the Nobel Prize for nuclear fission: why only Hahn received the chemistry prize, and why the subsequent effort by prominent physicists to reward Meitner and Frisch with the Nobel Prize in Physics failed.

It is important to critically reexamine this decision, not just because Meitner and Strassmann, or Meitner and Frisch, may well have deserved this honor, but because the role of physics was largely eclipsed when Meitner's closest collaborator was awarded a Nobel Prize and she was not. In other words, the perception and history of the discovery has been skewed by the one-sided award to Hahn.

Meitner and the discovery of fission

It should not have been difficult in the mid-1940s to accurately assess Meitner's contributions to the discovery of fission, or Meitner and Frisch's subsequent theoretical interpretation. The published scientific record dated back to 1934, when Meitner first repeated the neutron experiments of Enrico Fermi and his group in Rome and then, together with Hahn and Strassmann in Berlin, turned to the neutron irradiation of uranium and the products they assumed were transuranic elements. Over the next four years, in well over a dozen publications, the Berlin team reported a number of apparent "transuranics" and identified with certainty a new beta-emitting uranium isotope, ^{239}U, formed by resonance capture of slow neutrons by ^{238}U. Thoroughly interdisciplinary, the team relied on analytical chemistry for separations, radiochemistry for decay sequences and nuclear physics—both experiment and theory—for measuring and interpreting reaction conditions and mechanisms.

In the summer of 1938, as conditions worsened for Jews in Germany, Meitner, who was Austrian-born and of Jewish origin, fled Berlin, and took a position in Stockholm. (See box 1 on page 29 on her flight from Nazi Germany.) Her collaboration with Hahn continued, however, through their frequent correspondence and in their crucial meeting in Copenhagen in November 1938, when she objected to the most recent findings and urged him to verify them. These were the experiments that led Hahn and Strassmann, a few weeks later, to identify an isotope of barium among the uranium products—evidence that the nucleus had split. Although this was the end result of the team's long investigation, joint publication was not an option in Nazi Germany. The discovery was published in *Naturwissenschaften* on 6 January 1939 under the names of Hahn and Strassmann only.[2]

Meitner, together with Frisch, gave the first theoretical interpretation of the fission process, which they submitted to *Nature* on 16 January 1939, and which was published on 11 February.[3] Referring to Niels Bohr's liquid–drop model of the nucleus, they pictured a uranium nucleus with surface tension so small it was ready to split in two, estimated the enormous energy released in the process, suggested that the "transuranes" were fission fragments, correctly predicted the fissionability of thorium, pointed to ^{239}U as the precursor to the first true element 93 (also correct) and named the process "nuclear fission," a term that was immediately adopted. Prompted by the theoretical interpretation, Frisch quickly sought and detected the huge ionization pulses of the nuclear fragments—the first physical verification of fission—and submitted the results to *Nature* on 16 January; they were published on 18 February.[4]

The separate reports by Hahn and Strassmann on

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Lise Meitner, Theodor "The" Svedberg and Niels Bohr, as sketched by Carl Benedicks, a professor of physics at Stockholm Högskola. Meitner is shown at the regular weekly session of the Royal Swedish Academy of Sciences held on 14 November 1945, the day before the Royal Swedish Academy of Sciences decided to award the 1944 Nobel Prize in Chemistry to Otto Hahn at its Nobel session. As a foreign member of the academy, Meitner did not attend the Nobel session. Svedberg, chair of the Nobel chemistry committee, is shown at a 1932 meeting of the academy. Bohr—like Meitner, a foreign member of the academy—is shown at a session held on 27 November 1946. (Courtesy of the Royal Swedish Academy of Sciences and the Benedicks family.)

the one hand and Meitner and Frisch on the other divided the discovery of nuclear fission between chemistry and physics, experiment and theory, Germans and émigrés. These divisions did not reflect the science, which remained interdisciplinary to the end, but were instead the result of Meitner's forced emigration and German anti-Jewish policies. Strassmann regarded Meitner as a full codiscoverer despite her physical absence from Berlin, because, through her correspondence with Hahn, she had in fact continued their collaboration until the discovery of barium and beyond.

The Nobel committees assess fission

Nobel prizes in the physical sciences are awarded through a three-stage process involving the committees for physics and chemistry, the relevant sections of the academy, and finally, a meeting of the entire academy. The five-member committees evaluate only scientists who have been nominated. A select group of nominees is examined in special reports each prepared by a committee member. Radioactivity and radioactive elements had long been treated as subjects that belonged in the domain of the chemistry committee. Until 1945, nuclear fission had always been evaluated by this committee—even though the topic had also been discussed by the physics committee. Consequently, although the discovery of nuclear fission had been an interdisciplinary achievement, it came to be seen as a discovery in chemistry.

After the discovery of nuclear fission, Theodor "The" Svedberg, chairman of the chemistry committee, proposed a prize for Hahn alone or possibly Hahn and Meitner. Between 1940 and 1943, several international nominations proposed that Hahn and Meitner be recognized by a prize in physics, but the physics committee noted that their work appeared to belong to chemistry. During the war, there were no international nominations to the chemistry committee for fission, but Hahn's candidacy was kept alive through a nomination made each year by the committee secretary Arne Westgren. There were two more special reports on nuclear fission, in 1941 by Svedberg and in 1942 by Westgren. Both chemists made essentially the same argument: Hahn's work was important, while Meitner's and Frisch's experimental work was not extraordinary, and if there was a significant theoretical contribution, then Bohr should be given the credit. The two chemists thereby distorted and diminished Meitner's and Frisch's contributions. They also notably failed to take into account the external factors involved, including Meitner's forced separation from her team and the effect of German anti-Jewish policies on the published record.

It is true that Frisch's experiment was carried out independently and nearly simultaneously by others, but he had priority. And despite what Svedberg and Westgren claimed in their evaluations, the theoretical interpretation by Meitner and Frisch was considered seminal by the nuclear physics community. Referring to their *Nature* paper before publication, Bohr had cited their "ingenious" explanation as the starting point for further nuclear theory, and based on Meitner's 1937 measurements of reaction cross sections for neutrons of varying energy, he predicted that ^{235}U and not ^{238}U was the fissionable isotope of uranium.[5] Fission opened a fertile new field for experimental and theoretical physicists. In January 1940, Louis Turner reviewed nearly 100 fission papers and discussed at length the work of Meitner, Hahn and Strassmann, as well as the theoretical contributions of Meitner and Frisch.[6]

In 1944, the chemistry committee led by Svedberg and Westgren recommended that Hahn be given that year's prize for chemistry, but the academy rejected this recommendation and instead reserved the 1944 prize until the following year. (The committee's recommendation may have leaked out, for Hahn and others were now quite

Fritz Strassmann. With Otto Hahn, he first identified barium as a fission product. (Niels Bohr Institute for Astronomy, Physics and Geophysics; photo courtesy of AIP Niels Bohr Library.)

Max von Laue (left), Lise Meitner and Dirk Coster in the mid-1930s at the Kaiser Wilhelm Institute for Chemistry outside Berlin. With fellow Dutch physicist Adriaan Fokker, Coster helped Meitner get out of Berlin in 1938. (Courtesy of Ada Klokke-Coster.)

certain that the prize had been set aside for him.) In 1945, the committee members changed their minds and unanimously recommended that the decision—and therefore the award—be deferred. At the full meeting of the academy in November 1945, Westgren and Svedberg pleaded that the 1944 prize be held back once again, pending evaluation of the information about the discovery now available in the US and France.[7] Westgren and Svedberg's abrupt reversal apparently alienated many academy members, for when the vote was taken, slightly more than half the votes were cast in favor of the motion to award Hahn the prize in chemistry for 1944. Remarkably, both the chemistry committee and the full academy had reversed themselves between 1944 and 1945.

Meanwhile, in that same year, Oskar Klein, a professor of theoretical physics at Stockholm University, had nominated Meitner and Frisch for the physics prize. Although committee member Erik Hulthén's special report on the nomination in June 1945 was negative, the committee's decision was deferred. In 1946, Meitner and Frisch were nominated again by Klein, as well as by Bohr and by Egil Hylleraas, a professor of physics at the University of Oslo. Each nominator recommended that Meitner and Frisch share the prize for their theoretical explanation of Hahn and Strassmann's experimental results. They all stressed that Meitner and Frisch had shown that fission would generate enormous energy.

Meitner was also nominated by Max von Laue for her studies of radioactivity in general and especially for her work in the 1920s on beta–gamma spectra. Finally, there was one nomination that could hardly have helped Meitner's and Frisch's candidacy. Nominating for the first time in his capacity of Nobel prizewinner, Otto Hahn, while still interned in England, proposed Walther Bothe of the University of Heidelberg for his invention of the coincidence method. This was Hahn's chance to push for a Nobel Prize for Meitner, but he chose not to do so.

Hulthén evaluates Meitner and Frisch

The 1946 evaluation of Meitner and Frisch was again conducted by Hulthén. Why did the committee choose Hulthén, holder of the chair of experimental physics at Stockholm University, rather than Ivar Waller, a professor of theoretical physics at Uppsala University? This may be explained by the makeup and attitude of the physics committee. Three of the five members of the physics committee—Manne Siegbahn, his former student Hulthén and his eventual successor at Uppsala, Axel Lindh—belonged to the Siegbahn school of x-ray spectroscopy, which had dominated Swedish physics since the 1920s, influencing the research orientations, appointments and resources of Swedish university physics.[8]

The role of special reports in the Nobel Prize decision process is threefold: to guide committee members in their recommendation, to justify the committee's recommendation to the academy and to provide a historical record of the decision-making process. However, these reports do not fully reveal how or why a committee reaches its decisions. As Robert Friedman has argued, contextual factors can be equally important.[9] Here, one such factor may well have been the difficult readjustment of the Swedish scientific establishment from its traditional German orientation toward a recognition of Allied dominance in nuclear physics and science in general. More important may have been Meitner's poor relationship with Siegbahn, in whose institute, the Nobel Institute for Experimental Physics, she had worked since her arrival in Stockholm. She had never felt welcome in his institute and, in 1946, was preparing to leave.

Moreover, the committee members were part of the small Swedish physics community, which had its own rivalries and competition for funds. In 1945–46, the Swedish government was trying to rapidly acquire expertise in nuclear energy for military and civilian purposes. Siegbahn and Hulthén were actively involved, both as members of the government's "atomic committee" in charge of planning and allocation of funds, and as heads of institutes that received funds. Klein, by far the most forceful advocate of a Nobel Prize for Meitner and Frisch, hoped to recruit the two physicists to a new institute he was building for basic nuclear physics research at the University of Stockholm.[10] His institute, and especially its research emphasis, presented a real challenge to Siegbahn's school, both in terms of competition for resources and for prestige.

Hulthén's report on Meitner and Frisch was neither accurate nor exhaustive. In 1945, Hulthén had based his evaluation on the 1939 articles by Hahn and Strassmann in *Naturwissenschaften*, and by Meitner and Frisch in *Nature*. He claimed that that was the only information available to him, apparently overlooking the large number of fission articles in German, French, British and American journals that had been available to Svedberg when he had written his 1941 special report for the chemistry committee. Those articles included Turner's 1940 review, which highlighted Meitner and Frisch's theoretical contribution.

Although Hulthén's 1946 "Supplemental Report," dated 17 June 1946, was longer than his 1945 report (11 pages instead of 3) and cited more of the original 1939 articles, notably those by Bohr[5] and by Bohr and John Wheeler[11] in *Physical Review*, it did not refer to Henry Smyth's official report on the Manhattan Project, *Atomic Energy for Military Purposes*, which was avidly read by physicists worldwide when it appeared in 1945, nor to the British government report on the atomic bomb project issued shortly after the atomic bombing of Hiroshima. Both reports recognized the importance of the theoretical work of Meitner and Frisch. Every one of these publications was available in Sweden in 1946, when the Nobel physics committee began evaluating the contributions of Meitner and Frisch. Curiously, the only post-1939 work quoted by Hulthén was a 1942 nuclear physics textbook written by Ernest Pollard and William Davidsson for nonphysics students—and the passage quoted by Hulthén is a largely inaccurate account of Bohr's announcement of fission in the US.

On this scanty documentary basis, Hulthén presented a two-pronged argument against an award for Meitner and Frisch. Both arguments reflected Hulthén's experimentalist predilections. First, he argued that Frisch's physical demonstration of nuclear fission was shared by a large number of experimental physicists. In support of this argument, Hulthén presented a confusing and at times erroneous chronology of the experiments of Frisch, Meitner and Frisch, Frédéric Joliot, Willibald Jentschke and Friedrich Prankl and also Robert Fowler and Richard Dodson. Hulthén inconsistently used dates of submission and dates of publication and thereby gave priority to Joliot, whose experiment was performed after Frisch's but published earlier. Nowhere did Hulthén explicitly acknowledge that Frisch had indeed been the first, although Bohr and Turner had indicated as much in their publications and Joliot had not contested it.

Second, Hulthén argued that Meitner and Frisch's theoretical explanation influenced neither the original discovery nor the many experimental investigations that had been undertaken shortly thereafter. If Meitner and Frisch's hypothesis of fission had really inspired the experiments, Hulthén argued, then their contribution would appear in a different light, for when it came to theoretical work playing the role of guiding experiments, "a firm tradition" had been maintained when awarding Nobel prizes for physics. Here, Hulthén was making a fairly dubious argument, for this "firm tradition" had been observed only in the early days of the Nobel Prize, when the experimentalist majority on the physics committee would admit theoretical work only if it had contributed to experimental discoveries. He also disregarded the awards to a string of theoretical physicists in the interwar period—among others, Planck, Einstein, Schrödinger, Dirac—and most recently, to Wolfgang Pauli in 1945. More important, his viewpoint completely ignored the complex interplay between theory and experiment that had characterized the work on nuclear fission both before and after the discovery.

Meitner and Frisch's nominators had stressed in varying degrees two important consequences of the nominees' fission interpretation: that they had calculated the energy released in fission, and that they had given the first

Box 1: Meitner's Flight from Nazi Germany

The following passage is from Ruth Lewin Sime, Lise Meitner: A Life in Physics, *University of California Press, Berkeley, (1996), pages 184–205.*

On 12 March 1938, German troops poured over the border into Austria. . . . Spurred by the ecstatic welcome, Hitler proclaimed the *Anschluss*—annexation—of his native Austria, reducing it to a province of greater Germany. . . . Overnight, Lise Meitner lost the thin protection of her Austrian citizenship. . . . Whispers reached Hahn: "The Jewess endangers the institute." But she clung to the hope that she could keep her position and stay in Berlin. By May, she knew it was over. She decided to accept Bohr's invitation to work at his institute for a time. But at the Danish consulate she was refused a travel visa: Her Austrian passport was no longer valid. Very worried, she met with Carl Bosch [president of the Kaiser Wilhelm Society] on 20 May. He decided to openly request permission for Meitner to leave Germany. . . . On 14 June, she learned of new restrictions on emigration. . . . Hurriedly, she noted in her diary, "Go for information. Hear that technical and academic [people] will not be permitted to leave." On 16 June . . . Bosch received a response from the Ministry of the Interior. . . . Plainly before her, distilled onto a single page, was everything Meitner had feared. Her "resignation" was a foregone conclusion. She was forbidden to leave. And she had lost her anonymity: Her case had come to the attention of the Reichsführer of the SS, Heinrich Himmler. . . . Now Meitner knew she had to get out fast. Alerted by Bohr, Dutch physicists Dirk Coster and Adriaan Fokker worked feverishly to obtain an entry permit for Meitner. . . . In Berlin, only [Peter] Debye, Hahn, Max von Laue, and Paul Rosbaud [a scientific publisher] knew of Meitner's plans. The secrecy heightened the tension. . . . Coster arrived in Berlin late Monday evening, staying the night with Debye and his family. . . . On Tuesday, 12 July, Meitner arrived "early in the institute. Hahn tells me what Coster–Debye propose. Meet Coster in the morning with Hahn. . . . So as not to arouse suspicion, I spent the last day of my life in Germany in the institute until 8 at night. . . . Then I had exactly 1 1/2 hours to pack a few necessary things into two small suitcases. . . . I left . . . with 10 marks in my purse." Meitner spent the night at Hahn's house. . . . Rosbaud drove her to the train station. At the last minute, overwhelmed by fear, she begged him to turn back. But Coster was waiting in the train; they greeted each other as if by chance. The trip was uneventful. As they neared the Dutch border, Lise became very nervous, but they crossed without incident. . . . At 6 that evening they were in Groningen. For the first time in months, Lise was free to think beyond the moment of escape. Relief turned to shock. Completely uprooted, she had been torn from work, friends, income, language. . . . Stateless, without a passport, she did not know where she would live or how she could travel.

Box 2: Nobel Controversies

The failure of the Nobel Committee for Physics to recognize the experimental and theoretical contributions of Lise Meitner and Otto Frisch naturally raises the question: Was this an isolated event or are there other instances in which contextual factors or the personal predilections of committee members have influenced committee recommendations? The scholarship that has accumulated since 1974, when the materials in the Nobel archives for physics and chemistry first became available to researchers under a 50-year rule, makes it possible to give some preliminary answers to this question.

It is hardly possible to separate scientific from nonscientific influences in most Nobel recommendations. Instead, let us ask whether nonscientific influences have at times taken the upper hand, with the result that prizes have been awarded for undeserving discoveries or, conversely, that scientists have been unjustly barred from a prize they deserved.

According to Alfred Nobel's will, the prizes reward the authors of "discoveries" that preferably bring some "benefit to mankind" rather than individual scientists for general scientific achievements. The work of the Nobel committees is therefore very much directed at identifying prizeworthy discoveries and evaluating scientists who have participated in them.

There are two broad categories of nonscientific influences on awards that have been either ill-timed or perhaps should not have been made at all. The first category concerns the prizes awarded under the exceptional conditions that reigned in the immediate aftermaths of the two world wars. In both periods, decisions were influenced by the political notion that the prizes, awarded by Swedish scientists who had remained neutral in the conflicts, could be used to reestablish prewar internationalism in science. One way to do that was to rehabilitate the losers. Thus, in 1919, three German scientists were awarded prizes, among them Fritz Haber, the inventor of gas warfare, receiving the chemistry prize for his discovery of the Haber-Bosch process of ammonia synthesis. As this discovery was very important for the munitions industry during the war, it could hardly be considered to have been an unalloyed "benefit to mankind." Although this decision may have been an attempt to redress the balance of power in international science, it also reflected the pro-German leanings of Swedish scientists.[15] In 1945, when Hahn alone was awarded the chemistry prize of 1944, the Swedish scientific establishment was painfully shifting its support from the Axis Powers to the Allies, but the old ties with German scientists, many of whom (like Hahn), had visited Sweden during the war, still won out. However, the most questionable prize from this time—if not of all time—was the award to Finland's Artturi Virtanen of the 1945 Nobel Prize in Chemistry for his silo method of conserving cattle fodder. As intended by Virtanen's chief promoter on the chemistry committee, the pro-German Hans von Euler, the prize came to symbolize the survival of Finnish science and culture after the country had been defeated in two successive wars against the Soviet Union, the second one fought with German support. As a Finnish nationalist and newly crowned laureate, Virtanen flaunted his anti-Soviet views and caused problems for the Finnish political leadership.[16]

The second category of prizes in which nonscientific influences may have led to questionable awards consists of those awarded to Swedish scientists. With five prizes in physics and chemistry (four of which were awarded to members of the Nobel committees) during the period 1901–45, Sweden is overrepresented when compared to its Nordic neighbors: Denmark: one prize (Niels Bohr); Finland: one (Virtanen); Norway: none. The most questionable prize was the 1912 physics prize awarded to Nils Gustaf Dalén for his invention of automatic regulators in lighthouses. It was one of the few times when the technologists in the academy of sciences prevailed over the committee's partiality for basic science.[17] Since World War II, however, Swedish scientists have received only two prizes in physics and one in chemistry. It is difficult to say whether this shift is due to the committees' heightened sensitivity to allegations of favoring their own or whether work done in the physical sciences at Swedish institutions is no longer of Nobel caliber.

It is impossible to separate the scientific from the nonscientific influences that have prevented a discovery from being selected for a prize, for in the disciplinary politics that have often governed these selections, the two categories are intermingled. One kind of disciplinary politics concerns committee members' propensity to favor their own fields or research orientations—the most prominent examples being Svante Arrhenius' campaigns for physical chemistry which resulted in prizes for himself and his gang of "Ionists"; The Svedberg's campaigns for colloid chemistry and Manne Siegbahn's for x-ray spectroscopy, which yielded Nobel prizes for each of them; and Carl Vilhelm Oseen's campaign for theoretical atomic physics. As Robert Friedman has shown, the prizes came to represent a symbolic and material resource for Swedish physics and chemistry. An award in a given field not only brought prestige and visibility, it also meant that Swedish scientists in this field could tap new supplies of private and public, domestic and foreign funds.[18] If one also considers the general tendency to reward experimental work and, particularly in the interwar period, a strong emphasis on atomic physics and chemistry, one understands why many discoveries that deserved Nobel prizes in fields such as geophysics and astrophysics went unrewarded.

When the Nobel committees select a discovery for an award, they must review the contributions of all who participated in the discovery. From the beginning, the committees seem to have applied an unwritten rule that only the most senior scientists in a collaboration could be considered codiscoverers. This practice has led to the exclusion of countless younger collaborators, Fritz Strassmann among them. In particle physics, the rule still holds, even though "discoverers" and prizewinners such as Carlo Rubbia (physics, 1984) or Georges Charpak (physics, 1992) may each have been backed up by a "cast of thousands." Although such exclusion is regarded as problematic, few voices have been raised against it in the scientific community. (See John Heilbron's article, "Creativity and Big Science," PHYSICS TODAY, November 1992, page 42.)

Nevertheless, the serious evaluation of the claims of senior scientists has been the rule since 1903, when Pierre Curie prevailed on the physics committee to consider his wife Marie for the prize, pointing to her important role in the discovery of radium and polonium. Of course, not all such claims have led to an award. Nevertheless, it is highly unusual for the committees to review the contributions of a senior scientist as extensively as the chemistry committee did in the case of Lise Meitner between 1939 and 1945 without reaching an informed opinion as to whether or not she should be included in the award. It is also highly unusual for a review, such as the one conducted by Hulthén in 1946, not to be based on the established historical record of a discovery. The decision not to recommend Meitner for the physics prize of 1946 was a rare instance in which personal negative opinions apparently led to the exclusion of a deserving scientist.

explanation for the fission mechanism. Hulthén dismissed the importance of their theoretical work as the basis for the energy calculation, arguing that it could have been done from the available data on mass defects without recourse to a nuclear model.

Hulthén did not deny that Meitner and Frisch had provided insight into the fission process. They had understood that the false "transuranes" were fission products, while the ^{239}U was the precursor for the true transuranic elements 93 and 94. He also recognized that the physical measurements of reaction cross sections and neutron energies, as reported by Meitner, Hahn and Strassmann in 1937 and 1938, were essential for a quantitative theory of the fission process. But he argued that Meitner and Frisch were "far from a full understanding of what distinguished capture from fission processes" and that "the distinction between fission and capture processes, which was of decisive importance for the further development of atomic energy, was not answered in a satisfactory manner until a month later," through the work initiated by Bohr alone[5] and together with Wheeler.[11] Hulthén then summarized both Bohr's proposal that ^{235}U is the fissionable isotope of uranium and the 1940 experimental confirmation by Alfred Nier, Eugene Booth, John Dunning and Aristide von Grosse.[12] He finally concluded that although Bohr got the idea for his theory from Meitner and Frisch "intuitively," it was Bohr who should be recommended for the Nobel Prize in Physics.

MANNE SIEGBAHN, head of the institute in Sweden where Meitner worked, with little support, after leaving Germany in 1938. (Niels Bohr Institute for Astronomy, Physics and Geophysics; photo courtesy of AIP Niels Bohr Library.)

Hulthén succeeded in cutting Meitner's and Frisch's work into small bits and explaining each of the bits away. Indeed, he was so intent on his reductionist argument that he missed the larger picture: Meitner and Frisch, separately and together, were involved in every aspect of the fission discovery. Meitner made crucial contributions during the long investigation that led to the discovery. Frisch provided physical confirmation. Finally, they published a remarkably lucid and comprehensive theoretical interpretation that in one stroke brought together the liquid-drop nucleus, surface tension, an explanation for the mistaken transuranes, energy calculations and the recognition of ^{239}U as precursor to element 93. No other scientist put it all together, and in any case, Meitner and Frisch were the first.

OTTO ROBERT FRISCH, Meitner's nephew and coauthor of the first theoretical interpretation of fission and the first to verify fission by physical means. (Courtesy of Ulla Frisch.)

Hulthén concluded his evaluation by falling back on the statutes of the Nobel Foundation: The prize should be awarded to the person whose discovery or accomplishment was the most important, irrespective of whether or not this person had already been awarded the prize. In the present case, Hulthén concluded, this person was Bohr, but since he had not been nominated for the prize, no other person could be recommended.

In its September 1946 meeting, the Nobel physics committee endorsed Hulthén's view. Since the committee nevertheless expressed its esteem for Meitner and Frisch's contribution to the development of nuclear physics, its official view may not have been shared by all its members. The committee then recommended that Percy Bridgman of Harvard University be awarded the 1946 physics prize for "the invention of an apparatus to produce extremely high pressures, and for the discoveries he made therewith in the field of high pressure physics," a choice no doubt more congenial to the committee's experimentalist members.

Epilogue

Although Bohr nominated Meitner and Frisch for the prize in chemistry in 1947, Meitner already knew by then that the fate of her prize had been sealed. On the day of the announcement in 1946 of Bridgman's award, she wrote to Hahn: "The chance that I might become your Nobel colleague is finally settled. If you are interested, I could tell you something about it."[13] We do not know whether Hahn was interested, and we shall probably never know just what it was that Meitner learned about the award of the 1946 physics prize.

Lise Meitner and Otto Hahn, in about 1935, when both worked at the Kaiser Wilhelm Institute for Chemistry. (Courtesy of the historical archive of the Max Planck Society, Berlin.)

Oskar Klein and Niels Bohr. Both men nominated Lise Meitner and Otto Frisch for Nobel prizes. (Niels Bohr Institute for Astronomy, Physics and Geophysics; photo courtesy of AIP Niels Bohr Library.)

Fifty years later, however, we do have a documentary overview of these Nobel decisions. It appears that Lise Meitner did not share the 1944 chemistry prize because the structure of the Nobel committees was ill-suited to assess interdisciplinary work; because the members of the chemistry committee were unable or unwilling to judge her contribution fairly; and because during the war the Swedish scientists relied on their own limited expertise. Meitner's exclusion from the chemistry award may well be summarized as a mixture of disciplinary bias, political obtuseness, ignorance and haste. The chemistry award to Hahn alone was in many respects a careless decision. In effect, it excised physics from the fission discovery. That in turn seems to have unduly narrowed the 1946 evaluation of Meitner and Frisch's work, which was again judged unfairly, perhaps in part because of rivalries within the Swedish physics community.

More than any other award, Nobel Prizes attract immense attention from the scientific community and the general public. Coming as it did just after the first use of atomic bombs, the award for fission was sensational. Hahn became a public figure of iconic proportions in postwar Germany: the Nobel laureate, the decent German who was not a Nazi, the pure scientist who had discovered nuclear fission but never worked on a bomb. Hahn applied his uncommon prestige to the rehabilitation of German science. (See box 2 on page 30 on the Nobel controversies.) With respect to the discovery of fission, Strassmann was permanently in Hahn's shadow and Meitner nearly disappeared from view. In time, influential German scientists would publicly claim that Meitner would have prevented Hahn from making the discovery if she had remained in Berlin.[14]

Whether or not Meitner deserved a Nobel Prize, she hardly deserved what she got: Her reputation and standing as a scientist were denigrated in the process of being evaluated for a Nobel Prize that she had never asked to be given. But none of this embittered Meitner. She complained very little, and forgave a great deal.

We thank the Nobel Archives of the Royal Swedish Academy of Sciences in Stockholm and the Niels Bohr Archive in Copenhagen.

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