REMEMBERING MISS MEITNER:
The historical background to the play [1]

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Lise Meitner (1878-1968) devoted her life to physics. Having come to Berlin early in the century, she quietly shattered barriers against women in science and emerged as Germany's leading nuclear physicist. That is, until Nazi persecution forced her to flee. Meitner lost virtually everything, but it was as a refugee in Sweden that insult and intrigue compounded injury; shattering her self-confidence and reputation.

Having been the initiator and scientific leader of the Berlin team investigating uranium that included her long-time collaborator, chemist Otto Hahn (1879-1968), and even secretly remaining in contact with Hahn after fleeing Germany, she was nevertheless denied credit in the discovery in the winter of 1938-1939 of nuclear fission, one of the century's most significant events as this process releases the enormous energy locked in the atomic nucleus. Hahn subsequently refused to remember her role during the crucial months after she had left Berlin. He also conveniently forgot his own confusion and misconceptions while 'discovering' fission.

In Stockholm as a sixty-year-old refugee, Meitner received a cool welcome. Manne Siegbahn (1886-1978), Sweden's most powerful physicist, provided a room in his laboratory, but little more. Moreover, the Nobel committees for both physics and chemistry ignored Meitner's contributions. The Royal Swedish Academy of Sciences awarded Hahn alone in 1945 a Nobel

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 This essay is taken from Chapter 13 of the author's *The Politics of Excellence: Behind the Nobel Prize in Science* (New York: Freeman-Times Books, Henry Holt & Co, 2001). References to archival and published materials can be found in the printed version.

chemistry prize; the physicists refused to acknowledge fission with a prize. Siegbahn and other committee members relied on the secrecy of Nobel proceedings to bury their biased evaluations. Meitner remained silent. Historical research based on private and institutional archival materials now provide insight into how and why these events happened.

The Meitner-Hahn Team and Fission

In July of 1938, Lise Meitner was forced to flee Berlin where she had lived and worked for thirty years. Only the second woman to receive a doctorate in physics from Vienna, Meitner came to Berlin in 1907, where she had to overcome restrictions on women's academic activities. This shy but intelligent and determined woman soon gained social and intellectual acceptance in the circle of prominent Berlin scientists.

Although women could not hold a university position, the privately-funded Kaiser Wilhelm Institutes [KWI; after 1945, Max Planck Institutes] offered an alternative. Meitner and Hahn teamed up in the new chemistry institute just prior to World War I. Hahn was also young and fresh to Berlin; he had worked abroad in the new field of radioactivity. To continue his work, he needed help from a physicist, while she wanted greater experimental collaboration. Their joint efforts brought results. They published a series of well-received papers and in 1918, they announced the discovery of a missing radioactive element, # 91 in the periodic table (protactinium). After the war, they were given their own respective departments and professorial posts at the KWI for Chemistry -- Meitner in physics and Hahn in chemistry. In the meantime, Meitner moved away from radioactivity, and started working in nuclear physics.

Following a series of remarkable discoveries in the early 1930s, nuclear physics began to blossom as a vigorous research specialty. The discovery of the neutron gave scientists a particle without charge that could be directed at an atomic nucleus without having to overcome repulsive electrical charges. Soon thereafter, the discovery of artificially-induced radioactivity showed that active isotopes can be produced even from non-radioactive elements. In Rome, Enrico Fermi followed up on this sensation by systematically investigating the periodic table for radioactive isotopes. When he and his team reached the heavier elements, they achieved surprising results.

Even before it became explosive, fission produced much embarrassment. In the mid- and late-1930s nobody could quite make sense of the confusing results arising from bombarding heavy elements, including uranium – the heaviest, with neutrons. Although the nuclei of light elements might be shattered by collisions with particles, nobody quite knew what to expect from the heavier elements. Fermi announced in 1934 that the heavy elements tended to absorb neutrons and then decay by emitting beta-rays (highly energized electrons). This would favor creating an isotope of an element one place higher on the periodic table. It seemed that as irradiated uranium (# 92) decayed, new man-made elements were created beyond what had been thought to be the end point of the periodic table; so-called transuranic elements. Fermi initially announced his sensational finding tentatively, but soon scientists and the public accepted these as fact.

These results riveted Meitner's attention. She began a new, intense research program, in which she enrolled Hahn as well as a gifted, young analytic chemist, Fritz Strassmann. They, and others working in this field, tried to make sense of Fermi's results. Through a series of papers, they assumed a commanding position internationally.

For all three, it was important to stay on top of this controversial and prestigious line of research. All of them were suspect in Nazi Germany. Hahn refused to join the party; Strassmann was staunchly and openly anti-Nazi. Although she had been baptized, Meitner was a Jew under German's new racial laws. But first when Austria entered the Greater Reich in 1938, was she subject to these laws that ensured she would be fired, and threatened worse. She was smuggled out of the country in July 1938 to Holland and then finally received refuge in Sweden.

Meitner's circumstances did not however stop her from sharing in the discovery of nuclear fission. A summary of the events in the winter of 1938-39, and then a review of Meitner's relations with Siegbahn, provide valuable background to the chemistry and physics committees' evaluations of the discovery of fission. More than a tale of injustice, this episode reveals an important role given the Nobel prizes in the then emerging world of Big Science: a means to confer or withhold authority.

From July 1938, Meitner, the intellectual leader of the team, was separated from Hahn and Strassmann. At this time, from Hahn's perspective as chemist, the existence of the trans-uranium elements was "no longer in doubt," and that "no further discussion" was necessary on their distinction from other elements. It was Meitner who was disturbed by the findings; it was she who repeatedly pushed the team to re-do the chemical analyses. After Meitner fled, Hahn continued to report on their work by letter, asking her advice on matters of interpretation and direction for further studies. They met secretly in Copenhagen in November, where, according to Hahn's diary, they continued discussing the uranium problem for many hours. Meitner knew that their results could not be correct and urged yet further refinement of the most critical experiments. Strassmann noted many years later, that it was fortunate her opinion and judgment carried so much weight that he and Hahn immediately set about with a new round of measurements.

It was this latest effort that led to Hahn and Strassmann identifying a substance resembling the element barium, roughly half the atomic number of uranium. Greatly agitated, Hahn wrote to Meitner on December 19th asking her if she "can come up with some fantastic explanation" for why the isotopes acted like barium. He conceded that they know that uranium cannot actually "burst into ba[rium]." He repeated his plea for guidance a few days later, afraid to publish this seemingly absurd physical result. Without her guidance on the physical interpretation, he was losing confidence in his finding.

Hahn began writing an article. He concluded that the chemical results prompted them to say that the products from irradiated uranium were forms barium; however:

> 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.

Meitner who was by now driven to frustration with all earlier attempts to make sense of the transformations of uranium, shot back a response to Hahn’s letter. She gave him new courage to interpret the results. She agreed that the barium was puzzling, and the thought of a “large-scale breakup” of the uranium nucleus was very difficult... “but in nuclear physics we have experienced so many surprises that one cannot unconditionally say, ‘It is impossible.’” Of course, how can a neutron result in a smashed uranium nucleus; how can the impact of a marble on an office building cause the structure to collapse into rubble? Meitner was ready to accept the possibility of some new physical process; she urged Hahn to reason his way to an explanation of the chemical findings. Four days after receiving her reply, on December 23rd, Hahn added a short paragraph to the page proofs of his article suggesting, in a rather muddled manner, that what had been previously thought of as trans-uranium elements might actually be forms of elements much lighter than uranium. He offered no explanation or possible process that could account for this result.

In the meantime, Meitner continued to ponder the curious results. She visited her friend Eva von Bahr-Bergius in Kungsälv, outside of Gothenburg. Here she was joined by her nephew Otto Robert Frisch, a young physicist at Bohr’s institute. The two went out into the snow-covered landscape to discuss the uranium puzzle. Using an earlier theory by George Gamow and Niels Bohr that conceived the atomic nucleus to behave like a drop of liquid, they arrived at a model for how the heavy uranium atom could split after absorbing a neutron. The added neutron induces an instability that provokes the drop to split into two parts and to release an enormous amount of energy.

Back in Copenhagen, Frisch confirmed their conclusions and suggested the term nuclear fission, by analogy with the process by which a living cell splits in two. Meitner concluded that the other product of fission must be an isotope of the element krypton, which would decay into a series of other elements. She communicated all these results to Hahn, who had not considered krypton, had not suggested any mechanism for the splitting, and had not considered the release of energy.

Frisch discussed their conclusions on January 3rd with Bohr, who allegedly exclaimed “Oh, what fools we have been! We ought to have seen that before.” Bohr urged them to publish this sensational insight as quickly as possible. This they did, sending a manuscript to the British journal *Nature*. But it was not quick enough. Bohr left immediately thereafter for America. Although he had promised not to say a word until the paper was published, he could not keep quiet. Word spread like a chain-reaction. Bohr’s leak jump-started wide-spread efforts to repeat and extend the experiments and measurements. The original insight, so difficult to arrive at, was taken to be almost intuitively obvious – once understood. By the time Meitner and Frisch’s article appeared, it was no longer sensational news. Soon came the self-imposed secrecy among Allied nuclear researchers in an effort to keep the Nazi war machine from creating a bomb.

Even more disturbing for Meitner, Hahn’s subsequent publications and personal communications showed the start of a process by which he cut her out from any role in the discovery. After she related to him the results of her and Frisch’s efforts, he incorporated these insights into his and Strassmann’s next article, but again did not provide any hint of their communications.

Just a few weeks earlier while Hahn was still unsure and confused, he had turned to Meitner for assistance. Hahn even asked Meitner if she had something she could publish at the same time so the three of them who had always been a team could still be seen as being behind this work. Naturally, they could not publish together; Meitner, who illegally fled Germany, was an enemy of the Reich. But once the phenomenon of fission was clear, and once Hahn understood that fission was critical for his professional and personal security under the Nazi regime, he began – first haltingly and then more decisively – to take all the credit for himself. He claimed that the discovery had nothing to do with physics and that Meitner, the physicist, would probably not have allowed him to make the discovery.

When he started to retell this version of the story, Meitner despaired. She feared that her new Swedish colleagues would interpret events in this manner. As it turned out this was the case, but what she could not have guessed was that some of her new colleagues simply did not want to find out what she had contributed.

Adding insult to injury

In the 1930s Sweden was by no means a friendly haven for refugees from Germany. More specifically, Manne Siegbahn reluctantly accepted Meitner as a guest in his new institute. It might have worked out favourably for both scientists, but unfortunately did not.

Siegbahn rose to a dominant position in Swedish physics, first in Lund then Uppsala. He helped perfect X-ray spectroscopy, providing increasingly more precise measurements of electron behaviour for the growth of quantum theory. Although his own command of the new atomic physics was superficial, he understood the need for attaining greater accuracy. He was a master of experimental design. His Uppsala colleagues on the Nobel committee for physics helped insure that he received the 1924 prize, even though he had received only minor support from nominators. Moreover, those members of the five-man Nobel committee who opposed Siegbhan noted that his candidacy conflicted with the statutes regulating the prizes: he had neither made a significant discovery nor created a new instrument. He perfected that which others had begun. Although he attained greater authority at home and on the committee, Siegbahn's initial efforts to raise money to establish his own research laboratory proved difficult. The Rockefeller Foundation noted that although Siegbhan's work was excellent, it was also extremely narrow. He showed little interest or insight into other developments in physics; his requests for funding failed. Finally in 1936, with the help of the Nobel Foundation and other Swedish sources of funding, he was given a new institute at the Royal Swedish Academy of Sciences in Stockholm, the Nobel Institute for Experimental Physics.

Siegbahn decided to join the growing international interest in nuclear physics. He invested his funds in building a cyclotron, the relatively new and costly instrument developed in Berkeley by E. O. Lawrence that accelerated charged-particles so that they became highly-energized bullets to smash into atomic nuclei. Although Siegbahn did not have any particular research plans or problems in mind, his experimental acumen told him that a cyclotron was a sort of "ante" if a laboratory wanted to join in the emerging competitive fields of nuclear and particle research. For a physicist such as himself, who was more at home with instruments and machines, as well as administration, rather than with complex theories, a cyclotron and related instrumentation could provide opportunities for him and his assistants.

Both Meitner and Siegbahn were used to being in charge. Both were strong, silent; reserved, but determined, proud, and stubborn. Unfortunately, bad personal chemistry was compounded by their strong differences in scientific style. In contrast to his narrow focus on instruments, Meitner engaged experiment for developing and confirming theory. Siegbahn expected all members of his scientific staff to be responsible for their own experiments: a researcher designed instruments and conducted experiments without an assistant. Meitner was used to conceiving and designing experiments, but she almost always needed someone to carry them out. In the hastily written correspondence, those who tried to aid Meitner neglected to mention to Siegbahn that she would need an assistant.

Siegbahn considered her a burden from the start. He did not know what to do with her: "she can do nothing with her hands." But that was only a minor part of the problem. Her requests for a budget, apparatus, and an assistant quickly resulted in a strained relationship. Having been a professor and head of a prominent research unit in Berlin, Meitner did not expect to be treated as a dependent student.

Siegbahn had no intention of giving her either authority or resources. She had to ask him for even the smallest of appropriations and for permission to use any instrument.

Already dejected by her abrupt move to a new country, Meitner became thoroughly depressed. She had no access to funds, instruments, or assistants; she was completely dependent upon Siegbahn. He was absorbed in plans for building a cyclotron and not especially concerned with securing resources for a talented refugee. She of course had her own definite ideas on how to organize and run a laboratory. She did not keep secret her disapproval of his leadership and his scientific abilities. A rocky path was in store for her. Within a short time, she wrote to friends that she felt like a wind-up doll, acting mechanically and without feeling. She understood that Siegbahn was not a nuclear physicist and that her own command of this field was vastly superior, he was, nevertheless, the boss.

During the war, the neutral Swedish government greatly increased its research budget, for defense and for national self-sufficiency. Siegbahn and a number of other leading researchers forged links between science, industry, and the military. They helped create and lead new military research advisory committees and defense research organizations. Soon after the shock of the American atomic bombs, the Swedish government and scientific leadership began discussing policy on how to mobilize to exploit nuclear energy. Swedish uranium deposits, it turned out, were among the largest in Europe; although of low-grade quality, the amount of Swedish ore was estimated to be enormous. Some saw a Swedish atomic bomb as the best means to guarantee future neutrality. Others discussed the potential of inexpensive nuclear energy to power a materially-secure and socially-just welfare state. Although the specifics of Sweden's nuclear future were unclear, scientists understood that a new golden age for research was dawning. The immediate postwar climate – heated by the realization that the energy in the atom could be released – was one of optimism especially for those who looked to establish large, expensive installations for the study of the atom. Siegbahn's day had arrived.

Only one fact clouded Siegbahn's prospects of securing a major share of the anticipated resources and playing a pivotal role in policy. He was not a nuclear physicist. He knew how to plan, build, and operate a cyclotron; he knew how to raise money for such machines, but – as critics asserted – he had little insight into what could be done with them. In Sweden, there were other researchers with greater competence in nuclear science; and they also wanted facilities. They also recognized that Lise Meitner was a valuable asset for the development of Swedish nuclear research. After the news of the American atomic bombs, journalists converged upon her. A few months later, she visited the United States where physicists gave her a V.I.P. treatment. She met with President Truman and leaders of American nuclear agencies; she was interviewed on radio by Eleanor Roosevelt, and she received a number of awards. Many believed that she should share the honors for the discovery of fission, including a Nobel Prize. But, to give a Prize to Meitner would inevitably elevate her from a powerless, despairing, and dependent refugee in Siegbahn's laboratory, into a recognized, leading authority in nuclear physics, even in Sweden. That scenario made Siegbahn and others uneasy, but actually they had already all but buried Meitner's chances for a prize.

Nobel arrogance

The Nobel Prize may well be international in scope, but since its beginnings in 1901 the Royal Swedish Academy of Sciences has determined the outcome. During the first fifty years of evaluating nominations, the Academy's respective five-member Nobel Committees for Physics and Chemistry, relied on their own judgement. Nominators, moreover, rarely provided clear mandates. Even if they had, committees have hardly ever selected the rare consensual or even majority candidate. Individuals enjoying clear mandates, such as Henri Poincaré and Albert Einstein (for relativity theory) were rejected; winners such as Arthur Harden and Harald Urey each received but one nomination. No juggling of statistics related to nominations – number, frequency, origin - can explain the awards. Similarly, success or failure have not depended upon timeless, fixed standards of excellence. Rather, the changing priorities and agendas of committee members, as well as their comprehension of scientific accomplishment have been critical for the outcome.

Deliberations at times became enmeshed in the process by which factions within the Swedish science community attempted to define scope, methods, and priorities for physics and chemistry. The Prize helped shape the growth of these scientific disciplines, and influenced developments abroad. Some committee members have tried to be dispassionate; others championed their own agendas, some openly and some cunningly. Naturally, as in any committee, the internal 'chemistry' of its members, can influence the outcome of a difficult decision. The Nobel committees evaluate and then make a recommendation as to how their prizes that year should be allocated. The Academy's respective 10 member sections for physics and chemistry then either approves or makes its own proposal. Then the full one hundred members of the Academy vote. Normally, the committees' proposals are accepted, but occasionally their recommendations are overturned. A powerful committee member who also enjoys respect among the members of the Academy can largely decide the fate of a candidate in his area of specialization.

Although in the theatre-drama, Siegbahn alone represents all the Nobel committees, in reality he was not alone in working against Meitner. Meitner was a candidate for both a physics and a chemistry prize; neither committee lived up to its own ideals and accepted procedures in evaluating her contributions.

Confusion over what had been discovered and by whom in nuclear research began in 1939. The clandestine communications between Hahn and Meitner was just the start; secrecy became the norm as significant results were withheld from publication during the war years. Even after 1945, the record of accomplishments was not immediately obvious. The Academy of Sciences and the Nobel committees had ample reason to pause until well after the war and to wait until greater certainty was at hand. A mere trickle of nominations during and immediately following the war, difficulties in following foreign research journals, and finally an understanding that an enormous amount of research had been kept secret, should have made it clear that even the most conscientious committee might have trouble making fair judgements.

During the war, the committees evaluated the few nominations that arrived in Stockholm. Although potential candidates for a prize were named, as long as the war continued, the prizes were reserved and rolled-over to the following year. Old timers in the Nobel Foundation were sceptical whether the committees could obtain impartial proposals and a sufficiently large sampling of nominations. The case of erroneous evaluations based on inadequate information, such as happened during the first world war, was clearly forgotten, or ignored.

Errors in the evaluation of Meitner and Hahn for the chemistry prize beginning in 1939 provided a basis for subsequent treatment of their candidacy; there was opportunity to redress misunderstandings, but the will to do so was lacking. For its part, the physics committee tried to avoid evaluating fission; it conveniently used the chemistry committee's negative assessment of Meitner's role to ignore the question of fission's importance in physics. That was the pattern prior to 1945. After the war more overt measures for hindering Meitner's recognition became necessary, once the stakes and the available information grew.

Svedberg's deep interest in nuclear science, and his plans to establish a major research program in nuclear chemistry, resulted in the chemistry committee taking up the discovery of fission soon after its announcement. In January 1939, immediately after the initial announcements related to fission, Svedberg nominated both Hahn and Meitner and then he evaluated their contributions. In his report he concluded that the work on the trans-uranium elements was now understood to have been erroneous. He noted that Meitner and Hahn were not the only ones who had been mistaken. But he cut Meitner out of any role having to do with fission, pointing out that Hahn's latest studies were made after the collaboration allegedly ended. He implied that Hahn could make the discovery of fission once Meitner left. A divided prize based on the discovery of fission would be out of the question. Furthermore, he claimed, also erroneously, that the role of elucidating the theory of fission belonged largely to Bohr. Svedberg concluded that because of the rapid developments in the field, the committee should wait before making any decision on awarding a prize for fission.

In 1940 nominators provided a corrective. This time the physics committee had to take a stand. Former prize-winner Arthur Compton proposed Hahn and Meitner for a divided physics prize. “As I understand the matter, Professor Hahn and Fräulein Meitner should be included in the award for their work respectively in identifying the fission process and in showing the tremendous energy liberated when the fission occurs.” He admitted that it was “difficult for me to judge” whether their respective collaborators (Strassmann and Frisch) should be included. Compton considered the “discovery” of fission consist of both Hahn’s chemical and Meitner’s physical results. Had he been unsure whether his proposal was justified, he would have added a note to this effect. That was his style. Compton did not nominate without careful reflection; he tended to confess reservations. Typically he added in this same letter that the only other recent discovery worthy of consideration would be the new sub-atomic particle, the mesotron, which was not fully understood and therefore “too early to make the award intelligently.”

In response, the physics committee simply referred to Svedberg’s earlier evaluation that concluded it would be best not to make a decision as yet. The physicists added that nothing new has happened to change that view; the committee would not evaluate them.

In 1941 Compton’s opinion received further confirmation. In a detailed letter of nomination, laureate James Franck presented the case for dividing a physics prize between Hahn and Meitner. Franck was one of the more morally upright physicists of his time. Although he had not been immediately threatened, he resigned his professorship in Göttingen as soon as the Nazis started dismissing less prominent Jewish and dissident physicists. After some years at Johns Hopkins, where he found anti-Semitism prevalent - as was the case in most élite American universities of the time - he again resigned, moving to the University of Chicago. Franck claimed in his letter of nomination: “I do not need to emphasize the importance of this discovery which is certainly the greatest in physics in the last ten years, but I would like to explain why I think that Hahn and Meitner should be honored together.” He reviewed their thirty years of team-work, and especially the tight collaboration up to the very last step prior to the discovery. But because Meitner had to leave Germany,

> she was not co-working in the paper which Hahn published with Strassmann, which actually contained the solution, but Hahn himself did not draw the consequence. Lise Meitner did it in collaboration with Frisch, and she was the first to see the whole importance of the result and drew the consequences that the fission products should fly from another [sic] with tremendous energy. She and Frisch were also the first to observe this fact experimentally.

The committee again asserted that the discovery was more suitable for chemistry and refused to take it up for evaluation.

Although the chemistry committee also received nominations in 194 calling for them to share a prize, Svedberg again concluded that Hahn alone deserved to be rewarded for fission. He now began specifying Hahn’s work as having great significance for nuclear *chemistry*, and claimed that Hahn’s group continued to produce excellent work. In contrast, as if to emphasize that Meitner could not possibly have played a role in fission’s discovery, he underscored that in contrast to Hahn, she had not produced any work of great significance during the past two years. He did not mention why she had not shown continued creativity. Although she and Frisch had, in March 1939, performed in Copenhagen the definitive analyses showing that the trans-uranium products were indeed nothing more than the products of fission, Svedberg erroneously gave credit for this confirmation to Americans. He also mistakenly claimed that the work she or she and Frisch published immediately after Hahn’s publication “have not exerted to any significant degree an influence on the development." Svedberg concluded that Hahn himself and in part a number of Americans had been leaders in fission research. Theoretically Bohr "has shown the way." Svedberg had spoken; his colleagues now knew what he thought.

One committee member, who had at first himself nominated Hahn alone, began to have doubts. In 1942, Wilhelm Palmaer openly expressed his uncertainty over the committee's one-sidedness; he wanted a second look and nominated Hahn and Meitner. "It seems to me clear that it would be in accordance with the demands of fairness to let, if possible, both researchers divide an eventual prize. He was to submit his own evaluation, but died before it could be written. Instead, Svedberg's long-term friend and ally Westgren assumed the responsibility, even though he had already made his opinion clear by having nominated Hahn alone.

Westgren's report revealed an unwillingness to consider Meitner's case. First, rather than accepting that Palmaer and others' nominations hinted at a case where Hammarsten's principle applied – ie., wait until certainty can be attained, rather than risk leaving out a deserving researcher from an award – he wrote his evaluation based upon the supposition that Hahn alone should be given a prize.

Admitting that had Meitner remained in Berlin she certainly would have been part of the discovery, Westgren wrote that unfortunately for her, she was not. He ignored the issue raised in some of the nominations that it was Meitner's work that actually identified the physical process of fission along with the enormous amounts of energy released. By and large he repeated Svedberg's evaluations, except for one point, which indeed Svedberg had also changed after his original 1939 evaluation. At first Svedberg rightly maintained that several leading nuclear scientists propagated the error of the trans-uranic elements. But now Meitner remained alone, a scapegoat. No mention was made of the others. Westgren claimed that Hahn and Meitner's trans-uranic "mistakes" should be kept separate from Hahn's experiments on splitting uranium. Even without knowing the private communications between the two of them, an unprejudiced review of the literature would suggest otherwise. In asserting that the earlier work on the trans-uraniums in no way led to, or made possible, the discovery of fission, Westgren ignored or missed the intimate linkage of the two in terms of the research problem and experimental methods. The report contributed to the illusion that Meitner was the sole reason fission had not earlier been discovered. The committee agreed once again that Hahn should receive an undivided prize -- but because of international tensions, recommended reserving the prize.

In 1943 the situation remained the same with respect to the chemistry prize. Westgren alone nominated Hahn and simply repeated his conclusion. For the physics prize, Siegbahn nominated Hahn. Franck repeated his proposal of Hahn and Meitner again and reminded the committee that Hahn and Strassmann's article only hinted at fission; Meitner and Frisch were the first to identify the process and confirm it. In an odd decision, the committee again refused to evaluate. It wanted the chemists to judge, even though Meitner was nominated only for the physics prize, and for the significance of fission for *physics*. Of course, the physicists knew full well that the chemists had not previously evaluated the physics of fission; in fact, the chemists had completely ignored all aspects of fission as part of physics. Seigbahn and the rest of the committee also knew that the chemists had no intention of recognizing Meitner's contribution. She had already been eliminated. The physicists made no effort to bring into focus what Franck had called the most important discovery during the past decade in physics.

In 1944 Westgren again nominated Hahn; and Meitner was not mentioned in the report. Hahn had visited Sweden in 1943; being a German patriot but anti-Nazi Hahn made a good impression. Westgren praised Hahn in the report for the continued excellence of his work and in the spring meeting the committee proposed him for the 1944 prize. The committee added that if, when the Academy voted, the political situation still prohibited Hahn from accepting the prize – that is, if the Nazi regime and its ban were still in place – then it recommended reserving the 1944 prize. Once again, the only proposal for Hahn was Westgren's. In contrast, several strong nominations continued to arrive for the leader of British organic chemistry, Robert Robinson. Why the rush? Were the Swedish chemists already considering Hahn as a possible leader for German science once a de-Naizified Germany began re-building? Was Svedberg getting ready to launch a major effort to claim nuclear research as part of chemistry?

The European war ended in May 1945. The atom bomb helped bring the Pacific conflict to an end in early September. Soon it became clear how significant fission had been, as well how much secret research on fission had been done. The chemistry committee made an about-face -- it would be best not to rush a decision. At its meeting on June 4th it decided to propose giving the reserved 1944 prize to Hahn and the 1945 prize to the Finnish chemist A. I. Virtanen. After the war ended, Westgren and Svedberg received a double “a-ha” experience. First, they obtained missing American scientific journals which surprisingly contained few contributions related to fission; then they learned these were but a tiny visible tip of a gigantic secret research endeavor mobilized to create the atom bomb. The committee agreed on Sept 10th to propose reserving the ’45 prize and giving Virtanen the reserved ’44 prize. The reason given was that some of the Allied work could possibly compete with the discovery of fission for worthiness of a prize. Guidance could come with the next year’s nominations, especially from former prize-winners who had direct contact with war-time developments, such as Bohr (who fled from Denmark to America), James Chadwick, G. P. Thomson, and Harold Urey. In the committee protocol the next sentence originally stated that this postponement would shed light on whether Meitner should divide the prize with Hahn. But this was certainly not to Svedberg’s liking; Meitner’s name was subsequently crossed over and replaced with “other researchers.” The Chemistry Section agreed with the committee to wait for more information.

But, when the Academy deliberated, a member of its medical class – Göran Liljestrand – objected. He argued that the Academy must now give the prize to Hahn. The reasoning was convoluted, but effective: by not to awarding Hahn it would appear as if the Academy was being influenced by the Americans who ostensibly would not look kindly on giving a prize to a German, especially one who was being held in isolation along with other captured nuclear researchers.

It is hard to know what really happened at the meeting and what guided a majority of the Academy's members to give Hahn the prize in 1945 against the recommendations for postponement. Liljestrand could not alone have mobilized the Academy to disregard an unambiguous and unanimous proposal from its authoritative members that fairness required waiting. Other powerful persons in the Academy certainly had reason for wanting to see Hahn alone rewarded for the discovery of fission.

Fallout: 1945-46

Lise Meitner and Manne Siegbahn disliked each other. She made no secret of her feelings. She had little patience with his obsession with bigger – and more difficult to operate - instruments at the expense of research. Money that could go to personnel went instead to technicians and technology. Whatever Siegbahn actually thought of her as a researcher and person, he did see Meitner as a potential threat to his plans in that she knew more nuclear physics and enjoyed greater prestige abroad. He treated her shabbily and without respect. She was kept in the dark about events in the laboratory, such as the procurement of instruments important to her work. Once the cyclotron finally started working in 1942, she had to apply for time, but virtually none was made available to her. Siegbahn preferred to prioritize the production of isotopes for medical use. She had insight and experience with nuclear physics, but he had power, authority, and unbridled ambition. The Nobel Institute was from its first visionary birth an institution where internationalism was supposed to flourish. Again and again, committee members had spoken of Nobel laboratories where foreign researchers could come as guests or staff. But as with so much of the idealism surrounding the Nobel legacy, the harsh realities of narrow professional interests at times undermined these noble aspirations.

Against this background, the prospect of massive funding that commenced following the dropping of the atomic bombs made Meitner a problem for Siegbahn. A new generation of physicists was coming into positions of authority, some had contacts with Meitner and held no allegiance to Siegbahn. Some were much closer to the emerging postwar social democratic political power-base. Central among these were Lund University physicist Torsten Gustafson and Tage Erlander, who had studied physics in Lund, served as the wartime cabinet minister responsible education and research, and became in 1946 Sweden's prime minister. Gustafson was Erlander's informal advisor on nuclear matters; during the war he put the politician in touch with Niels Bohr, Oskar Klein, and Meitner. After the war, Gustafson continued to have access to Erlander and tried to guide policy. That Klein and Bohr, with whom Meitner had close relations, had access via Gustafson to the highest levels of decision making could not have pleased Siegbahn. The stakes soon became clear.

Following the bombing of Hiroshima and Nagasaki, the Supreme Commander of the Swedish Armed Force gave research on the atomic bomb top priority. An immediate half million crowns were put at the government's disposition during the fall of 1945; the Defense Research Institute followed up with a petition for 1,2 million crowns in the 1946/47 budget for research related to atomic weapons. The government also established in November an advisory group, based largely. of scientists, the Atomic Committee [*Atomkommittéen*]. Officially, little was mentioned with respect to either atomic weapons or energy; recognizing the sensitivity of the matter, not the least diplomatically, the expression "different practical applications" was commonly the given goal for creating a program for research and development. From the start, the heavy representation of academic scientists on the committee resulted in exaggerated claims for the need to shore up basic research. Political scientist Stefan Lindström, who pioneered in historical study of early Swedish atomic energy policy, noted scientists' efforts at the origins to downplay the technological problems and to exploit the opportunity to support academic-based "pure" nuclear research.

Researchers began positioning to claim shares of the expected bonanza of state support to the nuclear effort. Things were about to happen, but just what, was unclear. Siegbahn had been chairman of the wartime committee to coordinate defense-related research; he now became a member of the new committee (as was

Gustafson and soon also Svedberg). In the fall of 1945 he was once again anxious over prospects for raising large-scale funding for a new, huge cyclotron. Certainly, Siegbahn's institute was already the best-equipped laboratory in Sweden for nuclear research. Critics feared that it could easily dominate to the extent that would not be healthy for the field. It was just this prospect that prompted Gustafson to express concern that critically important nuclear research will be "monopolized" by Siegbahn and his institute.

Competitors challenged Siegbahn's position. Several researchers had designs for major initiatives in nuclear research and for claiming a say in national policy. Svedberg had plans for Uppsala, although he was in principle willing to begin by trying to gain access to Siegbahn's laboratory. Gustafson wanted to create opportunities in Lund. And leaders of military research institutions were also making noises about needing adequate facilities. Oskar Klein hoped to create a nuclear physics research unit connected with his institute at Stockholm Högskola. He wanted to recruit Meitner as a professor and senior advisor; given her age, she could not be expected to take on major administrative tasks of leading the initiative. In discussing plans with Bohr and some Swedish colleagues, Klein suggested that maybe Frisch, who was a refugee in Britain, could be recruited as leader. Here, then, was a challenge to Siegbahn's dominance. Not only would this research unit possess real expertise in nuclear physics, it would have very close ties to Bohr and his institute as well as potential support from several members of the Atomic Committee, including Gustafson.

Siegbahn had good reason to be worried. Meitner's enormous international reputation finally became clear to Swedes after the end of the war. Media focus upon her at home and abroad, as well as the now well-known stories circulating within the scientific community of the poor treatment she received in Sweden, undoubtedly rattled Siegbahn. For him, she was a ticking bomb. Her closest relationships in Sweden were with persons – such as Klein and Hans Pettersson -- outside Siegbahn's network, and even hostile. Now she and those who appreciated her expertise were in contact with Erlander. Klein supported Meitner for a Prize and for spearheading a rival center for nuclear physics. Moreover, Klein was finally elected to the Academy's Physics Section early in 1945. And if that was not troubling enough, the Swedish Parliament indicated its reluctance to provide the extra funding Siegbahn requested for his massive cyclotron.

In 1945 the physics committee had to face Meitner and Frisch as candidates. Nominators Klein and Bohr hoped Hahn and Strassmann could divide the chemistry prize, while Meitner and Frisch took the physics prize. This would be the fairest distribution of recognition for this epoch-making discovery. Again, the physics committee refused to accept the responsibility of evaluating work on the physics of fission. It again claimed that the chemistry committee had this responsibility.

Moreover, Ivar Waller, Oseen's former student, who replaced him on the committee, was now in position to undo his former mentor's unfair treatment of a candidate. Waller overturned Oseen's intransigent, antagonistic refusal to award a prize to the brilliant but arrogant theoretician, Wolfgang Pauli. Pauli had made major contributions to quantum mechanics almost two decades earlier, but Waller masterfully argued for Pauli's continued central importance to atomic and nuclear physics. It was a wise strategic choice, as both Klein and Bohr had long wanted to see their colleague Pauli appropriately recognized by the Academy.

Klein was heartened at first when the chemists recommended waiting another year before re-considering Hahn and fission. Maybe next year, he mused to Bohr, both prizes could still go to those responsible for fission. But when the Academy suddenly disregarded the chemists' recommendation and gave the chemistry prize to Hahn, Klein understood that the task of achieving fairness would not be easy.

Meitner did not have just Siegbahn's opposition to worry about. Moreover, it was Siegbahn's style to let others do his dirty work. Committee member Erik Hulthén, Klein's colleague at the Högskola, was a problem. Hulthén was well aware of Klein's plans for Meitner and for his own institute, and opposed them. Hulthén made an agreement with Siegbahn in November 1945 to prevent introducing nuclear physics at the Högskola. But even if Hulthen and Siegbahn blocked this plan, other schemes were under discussion for rescuing Meitner and using her expertise to establish nuclear physics elsewhere. A number of physicists discussed possible scenarios - also with Erlander - to create a professorship for Meitner at the Royal College of Technology or at Lund, or maybe to create a position for her at the Defense Research Institute. In the meantime, Siegbahn did not know whether he would get the money for his huge cyclotron. Perhaps that was why he went to the press and announced, in November, that if he were given enough money, his institute could build a Swedish atomic bomb within weeks.

When the Nobel Committee for Physics met in February to begin assessing the nominations for the 1946 prize, Meitner and Frisch were again among the candidates. Klein and Bohr proposed them for their role in clarifying the physical process of fission and especially for being the first to discover the great release of energy during the fission of heavy atoms. Among others, Max von Laue, James Franck and the prominent Norwegian theoretical physicist Egil Hylleraas also proposed Meitner or alternatively a prize shared between Meitner and Frisch.

Although they knew the prejudices against Meitner, Bohr and Klein tried to set the record straight. They knew from direct contact with the participants the actual events leading to the articulation of fission during the winter of 1938-39; they tried to explain why the published record was inadequate for assessing credit. Although the committees had claimed Bohr was mainly responsible for the theory of fission, they revealed to the committee the informal communications that enabled Bohr and American colleagues to begin work on fission in January 1939 before Meitner and Frisch's pioneering article appeared. They also countered in detail the charges that others also had produced the same results almost simultaneously. Franck' proposal further provided a corrective to the illusion that Meitner had little to do with the discovery of fission. The committee could no longer claim ignorance and simply hide behind the misleading wartime record of publication or rely exclusively on Hahn's version.

In even greater detail, Hylleraas argued against cutting Meitner out from recognition for the discovery of fission. Aware that the committee would be tempted to claim that Hahn's chemistry prize already took care of fission, Hylleraas wrote that fission was of such great significance for physics that that "it would be striking [*påfallende*]" if it was not rewarded with a Nobel Prize in physics. In reviewing the literature on fission he pointed out that even the published record showed Meitner's integral role in the research program that culminated with fission. Reaching back to 1934, he claimed that clearly it was Meitner who had mobilized the Kaiser Wilhelm Institute to enter uranium research so quickly and effectively, and then followed developments to 1939. Hylleraas elaborated the circumstance by which Bohr's visit to America in 1939 set in motion America's rapid entry into the field of fission studies. "It has become all in all very clear what significant historic role Lise Meitner in this case has played." Both Hahn and Meitner merited prizes. Then, alluding to news and rumors from the Academy, he let his annoyance spill into the letter: While conceding that awarding Nobel Prizes in the natural sciences certainly differs from those in peace and literature, he was under no illusion about bias here as well. "I am nevertheless aware that natural science is completely lacking an unambiguous, objective standard [*målestokk*] for awarding prizes. It can therefore be that my own subjective view on the significance of Lise Meitner's contribution will not gain the requisite support [*fornödne tilslutning*]."

The committee allowed Hulthén to write a special report on Meitner and Frisch. Hulthén understood full well what was at stake; in the midst of positioning and bargaining over the future of Swedish nuclear research – new laboratories, new research programs, and new authorities – a prize to Meitner along with the inevitable media attention would propel her into a position that would be hard to assail. Could she then be kept off of commissions, denied significant funding and ability to lead research projects, and even be excluded from the Nobel committee? Previously Siegbahn, Svedberg, and von Euler's prizes had shown how local laureates garnered immediate prestige and authority. Klein and others were hoping to capitalize on Meitner, and even Frisch, to establish a significant research unit in nuclear physics, if not at Stockholm Högskola, then elsewhere. Moreover by winter 1946 it was no secret that Meitner held strong ethical convictions. She opposed nuclear research for military purposes; she sided with Bohr and a growing international movement to ban nuclear weapons. Hulthén and Siegbahn both had connections with the military's research efforts. And of course Siegbahn was not only nervously waiting the outcome of his petition for an extra appropriation, he was looking ahead for further funds for staffing his institute and embarking on actual research.

In addition, Hulthén had something in common with Meitner. He was also left holding the bag of trans-uranium elements. He had written the evaluation in 1938 for Fermi that recommended a prize in part for the trans-uranic elements. Fermi could have been given a prize for any number of extraordinary contributions in both theory and experiment. It was Hulthén who had reversed the committee's earlier cautionary conclusions to wait for further confirmation before using this discovery to justify a prize. Hulthén's evaluation resulted in an embarrassment for the committee and Academy. He might have used the opportunity in 1946 to argue that virtually all major researchers in nuclear studies had at that time accepted the reality of Fermi's trans-uraniums. He could have also set the record straight based on the leads provided in the nominations that the discovery of fission grew out of efforts to understand the physics of nuclear processes. Instead, Hulthén dug in. He conveniently emphasized that Meitner had been a hindrance to fission. He added that Meitner and Frisch's work had no special significance, but was merely one of many simultaneous contributions, in fact of less importance than many others. He scarcely addressed the points raised in the letters of nomination. He insisted that he was compelled to evaluate based on the published record; he disallowed Bohr's testimony, he refused to look any further. No grounds existed, he concluded, for giving a prize to Meitner or to Meitner and Frisch. Whether out of conviction or of expediency, Hulthén understood full well that in defense of his claims, he could rely on the committee's strongman, Siegbahn.

The committee agreed. Although the Academy had just declared it did not want to appear to be getting too cozy with America, in 1946 it did just that. After having rejected Harvard professor Bridgeman for over fifteen years, the committee decided that 1946 was a good time to heed the Cambridge, Massachusetts lobby. But when the Physics Section took up the measure, Klein was waiting. He attacked Hulthén's report, pointing out errors, distortions, and omissions. Klein stood alone. In the Academy, he lost his temper.

Perhaps compounding the shame Klein felt over how shabbily his countrymen were treating Meitner, and the committee's refusal to give her a fair hearing, his own frustration with the Academy's physics establishment fuelled his outburst. In spite of his strong international reputation, he was repeatedly denied a place on the committee, and, until recently, in the Academy's Physics Section. Indeed he would soon be passed over again, as Seigbahn and others preferred to recruit less quarrelsome and less intellectually-gifted colleagues. Klein no doubt felt the absurd imbalance between reputations in the Academy as opposed to internationally. For example, when the prestigious Solvay Congress soon met for the eighth time since its founding in 1911 - this time to discuss new results and problems in the study of sub-atomic particles - Sweden was for the first time since 1921 invited to send delegates: Klein and Meitner. But probably more to the point, he was seeing first hand how inconsequential factual knowledge and a sense of fairness were in the face of – to use Ibsen's phrase – a "compact majority" whose own interests were threatened.

Klein blasted the errors and misconceptions in Hulthén's report; he attacked the committee's prejudice. But even if Klein was right in his charges, he could not expect the Academy to turn against its committee in this case. First, he was all too emotional and antagonistic; second, to give a prize to Meitner – an outsider in its midst – the Academy would thereby insult its professor of physics and national leader of the discipline, Siegbahn.

Over the next few years, Klein and others continued to try getting Meitner and Frisch a prize. Bohr understood the impossibility of overcoming Siegbahn and others' opposition; he mistakenly thought the chemists might be more open to consider Meitner. Actually that was not the case; they washed their hands of the matter; Meitner or Meitner and Frisch's work belonged to physics. Moreover, the chemistry committee declared that through the Academy's decision to award Hahn alone "a clear definitive position" had been taken on the question of awarding the discovery of the process of nuclear splitting.

The case was closed. Both committees refused to take up the question again. For those who might have waited, the following year brought nominations for Meitner again from Compton, Maurice and Louis de Broglie, and Planck, among others. Perhaps what mattered more was encapsulated in a New York Times notice not long after the Academy's vote: "Sweden Aids Atom Study" reporting that the Swedish government allotted $1,748,000 mainly to enlarge the atomic facilities of Professor Manne Siegbahn.

Hahn came to Stockholm in 1946 to claim his prize. He basked in the attention that journalists and colleagues showered on him. Meitner remained in the background, a shadow. Hahn largely stuck to his version of the story. He did, however, nominate Meitner for a prize the following year. The chemists were so intent on awarding Hahn – not Hahn and Strassmann, not Hahn and Meitner – that Westgren re-nominated Hahn alone for the 1946 prize, in case he could not collect his 1945 prize in time. If the achievement was one of pure chemistry – which it wasn't – then Strassmann certainly deserved to share the prize. He was the chief analytic chemist and co-author of each article. For the committees the stakes were too high to accept that there was honor enough for all.

The Nobel Prizes were awarded not on the basis of recognizing merit, these had become to a great extent instruments in the politics of science. Hahn, whom they all knew personally from his earlier visit in 1943 to Sweden, was to be one of the 'good' Germans to lead the re-building of the once proud national scientific community. Without the taint of party membership, and with the prestige of a Nobel Prize, he could help represent and guide German science under the Allied occupation. Hahn's great patriotism was well known; he long fretted over the future of German science once the Nazi debacle ended. His nationalism was of the sort that devoted friends of German science happily supported: no wallowing in apology, only focussing on the injustices done to

Germany, and claiming the moral high for not having managed to make an atom bomb. “Good” was a relative term in the 1940s.

Meitner was promised a professorship and other inducements to remain in Sweden. Some younger physicists, along with Klein and Gustafson, pleaded her case. Somehow petitions to the government got misplaced, or delayed, or hindered by formal rules. Finally after further gaffs and postponements, she was given a small research unit in nuclear matters at the Engineering Science Academy’s laboratory. Although she subsequently received many prestigious prizes abroad, her fate as a researcher was first crushed by Nazi hatred, and then sealed by Swedish scientific leaderships’ insensitivity and self-interest. She eventually moved to Cambridge, where Frisch was working, and died there in 1968. The shibboleth that the search for truth transcends political realities and personal prejudices might be comforting for some, but rarely are such ‘truths’ pure and clean, especially once money and authority become critical resources in the quest to know.

For further reading:

Robert Marc Friedman, *The Politics of Excellence: Behind the Nobel Prize in Science* (New York: Freeman/Times Books, Henry Holt & Co, 2001)

Ruth Lewin Sime, *Lise Meitner: A Life in Physics* (Berkeley & London: University of California Press, 1996).