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Lise Meitner and the discovery of fission

Michael Pearson, in his article "On the belated discovery of fission" (PHYSICS TODAY, June 2015, page 40), focuses on physics as being solely responsible for the "belated" discovery of nuclear fission, but that does not tell the whole story. Although physicists at the time did assume that nuclear changes would have to be small, chemists contributed their own false assumption, namely that elements beyond uranium would behave like transition elements. (We now know they are actinides.) For four years, as long as leading radiochemists like Otto Hahn were certain that the activities they found were from transuranic elements, though they were in fact fission fragments,[1] physicists saw no pressing reason to set aside their own nuclear concepts and predict nuclear fission.

The article does not make clear, moreover, just how crucial Lise Meitner was to the fission discovery. In the fall of 1938, Meitner and other physicists were highly skeptical of Hahn and Fritz Strassmann's finding that the slow neutron irradiation of uranium produced radium. Pearson omits Meitner's further contributions: It was she who urgently requested that Hahn and Strassmann test their radium more thoroughly, which led directly to the barium finding. She also was the one who immediately assured Hahn that a disintegration of the uranium nucleus was possible, after which he added to the proofs of the barium publication the suggestion that uranium might have split in two.[2]

Had Meitner been in Berlin at the time, the discovery of fission would, without question, have been understood as the superb achievement of an interdisciplinary team. Instead, Meitner was in exile, and she and physics were largely written out of the history of the discovery. The barium finding was published under the names of Hahn and Strassmann only—not, as Pearson's article implies, because Meitner failed to provide an explanation but because it would have been politically impossible for Hahn and Strassmann to include her, a Jew in exile, as a coauthor. The records also show that Hahn quickly sought political cover and distanced himself from Meitner, claiming that the discovery was due to chemistry alone and that physics had delayed and impeded it, a view that was eventually codified by the Nobel Prize decisions[3] and is, unfortunately, apparent in Pearson's article.

What kept Meitner from being completely obscured was that her theoretical interpretation with Otto Frisch was recognized as a brilliant extension of existing nuclear theory to the fission process.[4] But the separate publications created an artificial divide—between chemistry and physics, experiment and theory, discovery and interpretation. It is important to recognize that this divide and Meitner's exclusion from the fission discovery do not reflect how the science was done but are instead artifacts of her forced emigration and the political conditions in Nazi Germany at the time.

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■ **Pearson replies:** I am grateful to Ruth Sime for raising the issue of the incorrect positioning of element 93 in the periodic table. I intended to do so in the original article but space limitations prevented it. (A longer version of the article can be found at http://dirac.lps.umontreal.ca/~pearson/belated.pdf.) But one might ask whether the outcome would have been any different even if the transuranics had been correctly positioned in the periodic table: Would Enrico Fermi have then taken Ida Noddack more seriously? Conceivably not, since he failed to address another problem—namely, that the observed multiplicity of half-lives was serving as a warning that something more complex than a simple radiative capture of neutrons was taking place. Actually, in his *Nature* paper,[1] Fermi was very cautious in claiming that he had formed transuranics: It was his successors who accepted that interpretation uncritically, even as the anomalies accumulated.

Concerning Lise Meitner, the object of my article was not to attribute credit for the eventual discovery of fission but rather to understand why it took so long. In that respect I must remind the reader of Meitner's 1936 rebuff of Fritz Strassmann when he reported finding barium in neutron-irradiated uranium: "Leave that to us physicists, and throw your results in the garbage can." Meitner's earlier opposition to the very suggestion of fission makes all the more remarkable the assurance she gave to Otto Hahn in late 1938, as Sime mentions. That assurance is discussed in more detail in Sime's biography of Meitner (*Lise Meitner: A Life in Physics*, page 235), but one wonders whether Meitner was recalling Noddack's proposal from long before, probably without even identifying the source of her memory.

As for Sime's last point, I did not intend to suggest that Hahn had explicitly promised to include Meitner's name on the paper with Strassmann, had she come up with a physical explanation. I certainly believed, though, that such a promise was implicit in his request to the exiled Meitner seeking her advice on his puzzling results. However, a closer reading of Hahn's letter of 19 December 1938 to Meitner (see, for example, pages 233–34 of Sime's biography) shows that I was wrong: Hahn expresses the hope that Meitner will have something to publish on her own, so that "it would still in a way be work by the three of us!" Presumably Hahn wrote that for precisely the reasons Sime states in her letter; he acted in the only way that was open to him at the time. Any suggestion of deceit on his part at this stage would be inappropriate.

Reference

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A mathematical framework for falsifiability

Paul Steinhardt's criticism[1] that multiverse inflationary cosmology, which was flexible enough to explain both negative and positive results of the BICEP2 experiment (see the Commentary by Mario Livio and Marc Kamionkowski, PHYSICS TODAY, December 2014, page 8), is unfalsifiable has resulted this past year in a renewed interest in the old debate: What defines the scientific method?[2] What makes a good physical theory? While the underlying inflationary theory is mathematically sophisticated and modern, the current debate itself has been surprisingly qualitative, similar to what it could have been five decades ago, when Karl Popper brought falsifiability into the spotlight. Such data-less arguments that are often binary to the extreme—for example, whether falsifiability should be retired altogether[3]—seem out of place in the data-driven, nuanced scientific world.

In fact, we scientists already have a mathematical framework to deal with falsifiability quantitatively. It is based on statistical principles that have long been a part of science. In particular, falsifiability is not an independent concept: Its graded, real-valued generalization emerges automatically from the empirical nature of science, much like the way Occam's razor transformed itself from a qualitative philosophical principle into a statistical result.[4,5]

The emergence of falsifiability from statistical inference is easiest seen in the language of Bayesian statistics. Suppose we want to decide which of two theories, T_1 and T_2, explains the world better. Our *a priori* knowledge of that is summarized in Bayesian priors, P_1 and P_2. After experimental data $\mathbf{x}$ are collected, the ratio of posterior probabilities of the theories is given by Bayes's theorem, $P(T_1 \mid \mathbf{x})/P(T_2 \mid \mathbf{x}) = P(\mathbf{x} \mid T_1)P_1/P(\mathbf{x} \mid T_2)P_2$, where $P(\mathbf{x} \mid T_1)$ and $P(\mathbf{x} \mid T_2)$ are the likelihood terms—the probabilities to get the observed data within the theory. The likelihood increases when the theory "fits" the data. However, because probabilities must be normalized,

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