Demonstration of fission at the Department of Terrestrial Magnetism of the Carnegie Institution, shortly after the announcement of the fission experiments at the Fifth Conference on Theoretical Physics in Washington. From left to right are Enrico Fermi, Niels Bohr and Léon Rosenfeld. (Photo courtesy of Carnegie Institution of Washington.)

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Bringing the news of fission to America

The news of this discovery reached America in January 1939; notwithstanding communication problems, Niels Bohr succeeded in protecting the priority of Lise Meitner and Otto Frisch's interpretation of the experiment.

Roger H. Stuewer

In January 1939 the news of the discovery of nuclear fission burst in America, sending physicists into their laboratories to try to confirm the startling new discovery. Some aspects of the story of how this news reached America are well known. Others, however, are not; they have remained hidden in private correspondence and other unpublished documents. By examining these materials in conjunction with the published literature, one can reconstruct the circumstances that converged to produce this historic event.

Appointment in the US

The story begins quietly in early 1938, when Niels Bohr began to finalize his plans to spend the second semester of the 1938–39 academic year as a visiting professor at the Institute for Advanced Study in Princeton. Bohr was last in the United States about a year earlier, in February of 1937, at the beginning of a six-month around-the-world trip, during which he lectured widely on his recently published[1] theory of the compound nucleus. Now, in early 1938, he was looking forward to an extended stay in Princeton, exploring a host of fundamental problems in quantum theory and nuclear physics with Albert Einstein, John von Neumann, Eugene Wigner and other eminent colleagues. His appointment was arranged by Oswald Veblen, professor in the Institute's School of Mathematics. By the end of April 1938, word of Bohr's prospective visit had reached John Wheeler, who was then on the faculty of the University of North Carolina in Chapel Hill. On 30 April, Wheeler included[2] the following remarks in a letter to Bohr:

> Professor Veblen informs me that you will probably be at Princeton for half of the coming year. I shall be there permanently in the fall, and look forward very much to the opportunity of learning more from you of the fundamental principles from which one can hope to attack the problem of nuclear structure.

That Wheeler, too, would be in Princeton came as welcome news to Bohr, because the two had already established a warm working relationship when Wheeler, as a National Research Council Fellow, had spent the 1934–35 academic year in Copenhagen. Bohr, however, had already been considering another way of satisfying his need for a close collaborator: He intended to bring one with him to Princeton. The candidate he had in mind was Léon Rosenfeld.

Bohr first met Rosenfeld in April 1929, when Rosenfeld, as a 24-year-old postdoctoral student, attended the first small conference that Bohr arranged in Copenhagen.[3] Extended periods of intense collaboration followed and, now, in 1938, Rosenfeld was a full professor at his *alma mater*, the University of Liège. He was an expert on the problem of measurement in quantum electrodynamics, and it was on that problem that Bohr intended to work with him in Princeton.[4] Bohr probably raised the question of support for him through John von Neumann, because on 2 July 1938, von Neumann wrote[2] to Bohr from Budapest, telling Bohr that he had written to Veblen about Rosenfeld. Five weeks later, on 9 August, Bohr had his answer: von Neumann informed[2] Bohr by postcard that he had just learned from Veblen that Rosenfeld's trip could be financed by a stipend from the Committee for the Relief of Belgium. Two or three months later, Bohr, his son Erik and Rosenfeld booked passage on the Swedish–American liner *Drottningholm*, scheduled to sail from Gothenburg, Sweden, on 7 January 1939.

In the fall of 1938 ominous clouds spanned the European political sky, and Bohr made his travel arrangements with great trepidation. Adolf Hitler had annexed Austria in March, and six months later, on 29 September, he and Neville Chamberlain had dismembered Czechoslovakia in Munich. Under these threatening conditions, international scientific meetings could no longer be held, and the eighth Solvay Conference, scheduled for the end of October in Brussels, was cancelled. Bohr had planned to hold another of his small conferences in Copenhagen just prior to the Solvay Conference; now he changed his plans, rescheduling for the last week in October. He hoped to attract at least a few people who were still willing and able to travel.

One who came was Enrico Fermi. The Italian racial laws had been promulgated in early September, affecting Fermi's wife Laura, and had forced Fermi to think about leaving Italy with his family. In view of this situation, the rules were broken and Fermi was informed confidentially by Bohr in Copenhagen that he was likely to

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Lise Meitner and Otto Hahn photographed in 1913 in the Kaiser Wilhelm Institute for Chemistry in Berlin-Dahlem. Their scientific collaboration lasted 30 years. (This and subsequent photos courtesy of AIP Niels Bohr Library.)

receive the Nobel Prize in Physics for 1938.[5] Confirmation came by the traditional telephone call from Stockholm on 10 November 1938, two weeks after Fermi had returned to Rome. On 6 December, he and his family left for Stockholm. Then, after the festivities, the Fermis traveled to Copenhagen to talk with Bohr. Leaving Copenhagen for Southampton, they boarded the Cunard White Star *Franconia* on 24 December and arrived in New York on 2 January 1939, where Fermi took up a position at Columbia University. In Copenhagen, it seems, Enrico and Laura Fermi made arrangements to meet Bohr and his son Erik when they, too, would arrive in New York with Rosenfeld, a mere two weeks later. By then, Wheeler also had made plans to be in New York.

Understanding fission

Meanwhile, another chain of events had been set into motion. Lise Meitner, imperiled in Berlin after the *Anschluss* of Austria in March 1938, was spirited into the Netherlands by Dirk Coster on 13 or 14 July, and from there went to Stockholm. Five months later, Otto Hahn and Fritz Strassmann, pursuing the researches they had begun with Meitner in Berlin, bombarded uranium with neutrons and found highly mystifying results—so mystifying that Hahn, still in his laboratory at 11:00 pm on Monday evening, 19 December 1938, decided to first reveal them by letter only to Meitner. He and Strassmann, Hahn wrote,[6] were coming "again and again to the frightful conclusion" that one of the products "behaves not like Ra, but rather like Ba." Replying[6] by return mail on 21 December, Meitner asked Hahn if he was "absolutely certain" about these results. Hahn was certain; that same day, even before receiving Meitner's reply, he had written[6] again, telling Meitner that "as chemists" he and Strassmann were forced to conclude that they were finding barium as a product of the reaction. Moreover, Hahn said, they could not "hush up" their results, "even if they are perhaps physically absurd." They submitted[7] the results the next day for publication in *Die Naturwissenschaften*. As "nuclear chemists related in a certain way to physics" they still could not "make this jump," they added. "Perhaps a series of rare coincidences might still have simulated our results."

Meitner, after receiving Hahn's second letter, left Stockholm on 23 December to travel to Kungälv, a small town just north of Gothenburg, to spend the holidays with friends and, as had been her custom, with her physicist nephew, Otto Robert Frisch. Frisch had been forced to leave Hamburg in 1933, and after a year in London had gone to Bohr's Institute in Copenhagen.[8] Traveling to Kungälv, he arrived just after Meitner had received yet another letter[6] from Hahn, dated 28 December. In that letter, Hahn had proposed a "new fantasy." He asked, "Would it be possible that uranium 239 bursts into a Ba and a Ma?"—Ma standing for the element masurium, today called technetium (Tc). "A Ba 138 and a Ma 101 would yield 239." The atomic numbers "of course do not work out"—Ba ($Z = 56$) plus Ma ($Z = 43$) did not yield U ($Z = 92$)—so "some neutrons would have to be transformed into protons.... Is that energetically possible?" asked Hahn.

Meitner replied[6] the following day, 29 December, telling Hahn that she and Frisch had "racked [their] brains" over his and Strassmann's "very exciting" results. Much later, Frisch recalled[9] that he and Meitner had discussed Hahn's letter first at breakfast in Meitner's hotel and then on a hike outdoors in the snow—Frisch on skis, Meitner walking. It occurred to Frisch that Hahn and Strassmann's results might be explained on the basis of the liquid-drop model of the nucleus. They stopped to calculate on scraps of paper, and, as Frisch recalled[9] further:

> ... I worked out the way the electric charge of the nucleus would diminish the surface tension and found that it would be down to zero around $Z = 100$ and probably quite small for uranium. Lise Meitner worked out the energies that would be available from the mass defect in such a breakup.... It turned out that the electric repulsion of the fragments would give them about 200 MeV of energy and that the mass defect would indeed deliver that energy....

On 30 December 1938, Meitner informed[6] Hahn that she had just received a copy of his and Strassmann's manuscript for *Die Naturwissenschaften*, which Hahn had promised to send her. On 1 January 1939, she told[6] Hahn: "We have read and considered your paper very carefully, [and] *perhaps* it is indeed energetically possible that such a heavy nucleus bursts."

Meitner then returned to Stockholm and Frisch to Copenhagen. Arriving on New Year's Day 1939, Frisch sought Bohr out, catching him two days later. As Frisch explained[6] to Meitner in a letter on 3 January 1939:

> Only today was I able to speak with Bohr about the bursting uranium. The conversation lasted only five minutes, since Bohr immediately and in every respect was in agreement with us. He was only astonished that he had not thought earlier of this possibility, which

follows so directly from the present conceptions of nuclear structure. He was also completely in agreement with our view that this disintegration of a heavy nucleus into two large pieces is an almost classical process, which does not occur at all below a certain energy, but already occurs very easily a little above it. (One indeed has to require this in order to understand the great stability of natural uranium as compared to the very great instability of the (not so very much more energetic) compound nucleus.) Bohr still wants to consider this quantitatively this evening and to talk with me again about it tomorrow.

Much later, Frisch recalled[9] his meeting with Bohr in more dramatic terms (but erroneously placing it on the day Bohr was leaving for America). Frisch recalled that he had just begun to speak when Bohr burst out: "Oh, what fools we have been! We ought to have seen that before." That Bohr had not, however, is understandable in light of his past work. Although George Gamow had conceived[10] the liquid-drop model of the nucleus at the end of 1928, Bohr did not treat Gamow's model as seriously as he might have in succeeding years. He mentioned[11] it briefly and only once, in 1933. Furthermore, when Bohr proposed[1] his theory of the compound nucleus three years later, in 1936, he speculated that more and more energetic particles (for example, neutrons) when striking a heavy nucleus would simply dislodge more and more nucleons, eventually producing "an explosion of the whole nucleus." In the fall of 1937, Bohr still was much less inclined to view[12] a heavy nucleus as a liquid drop than as an elastic solid. Thus, to the extent that he still held these views on 3 January 1939, he would not have seen the possibility that Frisch and Meitner recognized—and he would have been all the more astonished that he had not.

Fritz Strassmann. The photo was taken in 1930.

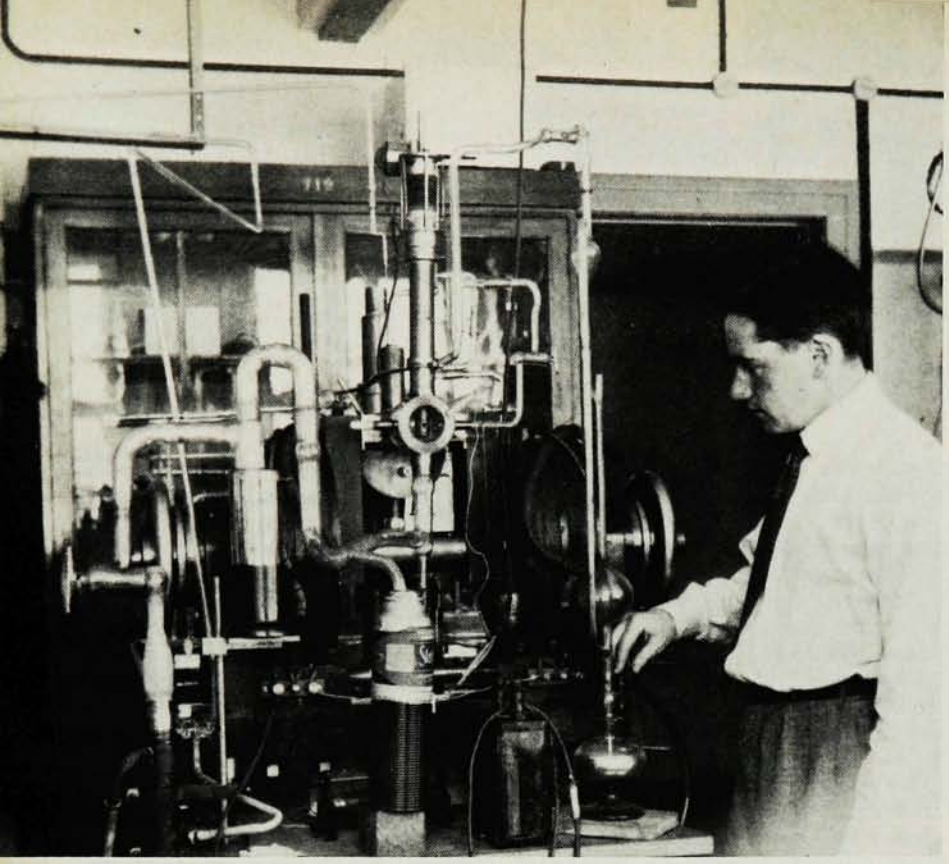
That Bohr wanted to talk everything over once again with Frisch on the following day suited Frisch, because Frisch already had made plans to call Meitner on 5 January to begin composing a note to *Nature* over the telephone. By Sunday, 8 January, Frisch had completed a draft of this note. He sent this draft to Meitner that same day, along with a cover letter[6] in which he brought Meitner up to date on his further discussions with Bohr:

> I wrote up a first draft on Friday [6 January] and on Bohr's request rode out to Carlsberg [Bohr's residence] still in the evening, where Bohr once again thoroughly discussed the matter with me. He let me recalculate my estimate of the surface tension, and he was in complete agreement with it; he had already hurriedly considered the electrical term, but had not realized it would be so large. Concerning the [formation of U^{239} by] resonance, he did not want to express himself directly, but seemed to see no difficulty with it. Later, I again considered this point a bit as it arises in the conclusion of the note; in any case Bohr did not take a position on this. Bohr only made several recommendations during the evening for a clearer formulation of several points; otherwise he was in agreement with everything. On the following morning [Saturday, 7 January], I then started to type up the draft and was able to take only two pages to Bohr at the train station (10:29 A.M.), where he put them in his pocket; he no longer had any time to read them.

Setting out to the US

By the time Bohr and his son Erik boarded their train to Gothenburg, to embark on the *Drottningholm* that same day—Saturday, 7 January 1939—Bohr had been thinking for no less than four days about Frisch and Meitner's interpretation of Hahn and Strassmann's experiments, and he was completely familiar with it.

Rosenfeld, who apparently traveled to Gothenburg independently to join the Bohrs there, has described[13] the



Otto Robert Frisch, photographed in Hamburg in 1931.

circumstances attending their transatlantic voyage:

> As we were boarding the ship, Bohr told me he had just been handed a note by Frisch, containing his and Lise Meitner's conclusions; we should "try to understand it." We had bad weather through the whole crossing, and Bohr was rather miserable, all the time on the verge of seasickness. Nevertheless, we worked very steadfastly and before the American coast was in sight Bohr had got a full grasp of the new process and its main implications.

The "most puzzling point" to Bohr concerned the high probability of the new process as compared to other competing processes. Rosenfeld continued[13]:

> The answer turned out to be very simple, on the basis of Bohr's treatment of the excited compound nucleus as a system in thermodynamic equilibrium: it is just a consequence of the equipartition of energy between all the modes of motion of the system. The relative oscillation of two large fragments of the nucleus competes on equal terms with any other mode, such as the relative motion of a single neutron, leading to its emission.

The *Drottningholm*, after a nine-day voyage, docked at the Swedish-American Line's West 57th Street pier in New York at 1:00 pm on Monday, 16 January 1939. Enrico and Laura Fermi were there early. As Laura Fermi recalled,[5] even before the ship "came alongside the wharf, we recognized in a crowd the man we had come to meet, Professor Niels Bohr. He was standing by the rails of an upper deck, leaning forward, scanning the people on the dock." John Wheeler was also there. He recalled[14]: "I had my regular morning class on Monday the 16th and then went in on the train to meet the Drottningholm coming in that afternoon; I of course shook hands with those waiting for Bohr and with him and Rosenfeld when they came off. Bohr was staying in New York for a little while, but Rosenfeld went down with me on the train to Princeton."

In Copenhagen on 7 January, Frisch had left Bohr under the impression that after finishing the typing of his and Meitner's note, he would submit it immediately to *Nature* for publication. Bohr then had promised Frisch that he would protect Frisch and Meitner's priority by not saying anything to anyone in America about the new discovery until he had received word from Frisch that the note was actually in press. On board ship, therefore, Bohr conveyed the impression to Rosenfeld that Frisch and Meitner's note no doubt had already been submitted for publication. Unfortunately, Bohr failed to inform Rosenfeld of his promise of confidentiality to Frisch. Moreover, quite by chance, on the very day that Bohr and Rosenfeld arrived in New York, on 16 January, one of the regular Monday-evening meetings of Princeton's Physics Journal Club was to take place, with Wheeler in charge. The inevitable occurred: As Rosenfeld recalled,[13] Wheeler "politely asked" him if he had anything to report, and Rosenfeld, "in spite of the fatigue of the voyage, . . . told them all about the problem we had struggled with during the journey."

Communication breakdown

The cat, therefore, jumped out of the bag—as Bohr learned, to his distress, when he himself arrived in Princeton shortly thereafter and found no news from Frisch awaiting him. He expected, however, that a letter from Frisch was imminent. As a result, immediately after his arrival in Princeton, he himself drafted a note outlining his deeper understanding of the new process that he had achieved on board ship with Rosenfeld. By 20 January, Bohr had still not received a letter from Frisch. He therefore decided to send his own note to Frisch, along with a cover letter[2] dated 20 January, asking Frisch to have his secretary, Betty Schultz, forward it to *Nature* "if, as I hope, Hahn's article has already been published, and your and your aunt's note has already been submitted to *Nature*." He was looking forward to hearing, he told Frisch, "about the latest news in this connection and how the experiments are proceeding at the institute, which I, despite the distance, follow in my thoughts." He added a "p.s." just prior to mailing his letter: "I have just seen Hahn and Strassmann's article in Naturwiss., which naturally

Léon Rosenfeld (right) talking to Walter Heitler. The photograph was taken during the 1934 conference at the Bohr Institute.

has caused much discussion here at the institute. . . . "

The publication of Hahn and Strassmann's paper, of course, increased Bohr's distress still further. Still, however, he received no word from Frisch. On 24 January, no longer able to contain himself, he wrote[2] a second letter to Frisch, saying:

> I still have not received any letter at all from the institute and sorely long to see the final version of your and your aunt's note to *Nature*, a copy of which you promised to send me. I therefore do not know whether you in your note reach the same conclusions about the splitting mechanism as those which are mentioned in my note and how far the latter contributes anything sufficiently new to be published.

A few lines later Bohr added:

> As I mentioned in my last letter, the physicists here at the institute are very caught up in the whole question, and I already have seen preparations for experiments to detect radioactive matter of very short half-life, the appearance of which should be an immediate result of the new type of splitting of the nucleus. . . . Working with Wheeler, I also have started a more thorough study of the different theoretical problems which the new splitting of the nucleus presents to us. Naturally, I am very interested in hearing more about what you yourself have been thinking in one direction or another, just as I am excited to hear about all of the investigations at the institute.

What, in fact, *was* Frisch doing all of this time in Copenhagen? In his letter to Meitner of 8 January, in which he reported[6] the results of his discussions with Bohr, Frisch also had told Meitner:

> Since Hahn and Strassmann's article appeared here yesterday, I discussed the entire matter somewhat, above all with [George] Placzek, who at the moment is very skeptical, but he of course always is. Early today he again flew back to Paris, and then will travel soon to America, to Bethe in Ithaca, where he has a position.

That discussion with Placzek, as Frisch later acknowledged,[9] prodded Frisch immediately into thinking about experimental confirmation of his and Meitner's interpretation. Assembling his apparatus, Frisch first observed the expected ionization pulses of the uranium fragments on Friday, 13 January, and he then confirmed his observations over the next three days. At that point he wrote up a report on his experiments, which he submitted to *Nature* along with his and Meitner's note. As he explained[6] in a letter to Meitner on 17 January:

> Yesterday evening I finished both notes, and at about 5 A.M. took them to the airmail deposit box, so that they should be in London today in the afternoon. With that, however, my energy was exhausted, so that I no longer wrote to you; rather, I do that now.

Unfortunately, Frisch at the same time did *not* write to Bohr in Princeton. Rather, he delayed five more days, until 22 January,[2] before writing the letter. Later, on 15 March, Frisch apologized to Bohr for this delay, explaining[2]:

> This was partly due to a lack of imagination on my side, as I did not imagine that the appearance of Hahn and Strassmann's paper would raise such a run as it did. And then I was pretty tired after the experiment (I had been working long after midnight for several nights in track) and instead of sending you the manuscripts at once (the obvious thing to do) I kept them until I managed to write you a letter, which meant about six days delay. When I think it over now I can hardly find an excuse for my letting you without information as I did, but, you see, I did not think my experiment so terribly important (it seemed to me just additional evidence of a discovery already made) and the idea of cabling to you would have appeared unmodest to me.

When he finally did bring Bohr up to date, on 22 January, Frisch noted that he was currently planning various new experiments on "these 'fission' processes." He added[2] the following parenthetical remark on that historic term: "I wonder how you like this word; it was suggested by the [American] biochemist Dr. [William A.] Ar-

nold, who told me it was the usual term for the division of bacteria."

Conference on theoretical physics

As Frisch's letter of 22 January was being slowly transported to Princeton, crossing with Bohr's letters of 20 and 24 January to Copenhagen, Bohr's tension was increasing to the breaking point. He knew that in only two days the possibility of containing the news of the new discovery would evaporate completely. For, two months earlier, another completely independent chain of events had been set into motion whose culmination was imminent. On 30 November 1938, Merle Tuve, George Gamow and Edward Teller had drawn up[15] a proposal that the Carnegie Institution of Washington and George Washington University sponsor the Fifth Conference on Theoretical Physics in Washington, D.C., sometime during the period of 21–30 January 1939. Quite innocently and with due deliberation, they had chosen as the subject of this conference "Magnetic, electric, and mechanical properties of matter at very low temperatures." They had drawn up a preliminary list of ten participants whose expenses would be paid, and of twenty-four who would be invited without paid expenses. Bohr was on neither list. A few weeks later, however, the organizers got wind of Bohr's visit to Princeton—and of Fermi's transfer to Columbia University. Both, therefore, were included on the final list of 15 invited participants whose expenses would be paid. John R. Lapham, dean of George Washington's School of Engineering, sent[15] out formal letters of invitation to these people on 22 December 1938—on precisely the same day that Hahn and Strassmann in Berlin submitted their paper for publication in *Die Naturwissenschaften*. On 6 January 1939, Lapham sent[15] out a second letter of invitation to 21 additional participants whose expenses would not be paid. The total number of participants would be increased by the attendance of about 20 local physicists. All, of course, had contributed to some area of low-temperature physics. The dates of the conference had now been fixed for 26–28 January 1939. On 23 January, J. A. Fleming, director of the Carnegie Institution's Department of Terrestrial Magnetism, sent its new president, Vannevar Bush, a list of all those who had been invited to attend, along with a notation indicating that William F. Giauque of Berkeley, Frederick G. Keyes and John C. Slater of MIT and Eugene P. Wigner of Princeton would be unable to come. Fleming also informed[15] Bush that "the first meeting will be held at the George Washington University in Room 105, Building C, 2029 G Street, N.W. at 2 p.m., January 26." (Today a plaque outside the lecture room commemorates this historic meeting.)

It was there—the building today is George Washington's Hall of Government—that the bombshell burst. Bohr knew that it was pointless to try to keep the new discovery secret any longer. If Hahn and Strassmann's article had arrived in Princeton on 20 January, it had certainly arrived elsewhere as well, its charge ready to explode. Moreover, Bohr knew that the news had already leaked a short distance northward. Isidor I. Rabi, as Wheeler recalled, actually had been present at Rosenfeld's discussion on 16 January and had carried the news of Hahn and Strassmann's discovery back to Columbia University.[4] Fermi himself, however, learned about it from Willis Lamb, who had been on a brief visit to Princeton.[16] Knowing, therefore, that Hahn and Strassmann's discovery was no longer a secret at Columbia University, Bohr himself stopped off there on his way to Washington, evidently on 25 January, seeking to discuss it with Fermi. Instead he found Herbert L. Anderson.[17] After a brief interchange with Anderson, Bohr then left for Washington, where Fermi joined him. They conferred, and on 26 January they took the floor—before a single talk had been delivered on low-temperature physics. As stated in the report of the conference[15,18] that was submitted on 1 February by C. F. Squire, F. G. Brickwedde, Teller and Tuve:

> Certainly the most exciting and important discussion was that concerning the disintegration of uranium of mass 239 into two particles each of whose mass is approximately half of the mother atom, with the release of 200,000,000 electron-volts of energy per disintegration. The production of barium by the neutron bombardment of uranium was discovered by Hahn and Strassmann at the Kaiser-Wilhelm Institute in Berlin about two months ago. The interpretation of these chemical experiments . . . was suggested by Frisch of Copenhagen together with Miss Meitner, Professor Hahn's longtime partner who is now in Stockholm. . . . Professors Bohr and Rosenfeld had arrived from Copenhagen the week previous with this news. . . . Professors Bohr and Fermi discussed the excitation energy and probability of transition from a normal state of the uranium nucleus to the split state.

Given their advance information, the experimental team at Columbia University—Anderson, E. T. Booth, John Dunning, Fermi, Gynther Glasoe and

Participants in the Fifth Washington Conference on Theoretical Physics. First row, left to right: O. Stern, E. Fermi, J. A. Fleming, N. Bohr, F. London, H. C. Urey. Second row: F. G. Brickwedde, G. Breit, J. B. Silsbee, I. I. Rabi, G. E. Uhlenbeck, G. Gamow, E. Teller, M. Goeppert-Mayer, F. Bitter, H. A. Bethe, H. Grayson-Smith, J. H. Van Vleck, R. Jacobs, C. Starr, M. H. Hebb, C. F. Squire. Third row: H. Kuper, A. J. Mahan, R. D. Myers, R. B. Roberts, C. L. Critchfield, L. Baroff, E. Bohr, R. C. Meyer, K. F. Herzfeld, R. C. Lord Jr , D. R. Inglis, O. R. Wulf, P. Wang, E. A. Johnson, F. Mohler, R. B. Scott, E. H. Vestine, L. Rosenfeld, F. Seitz, G. H. Diecke, J. E. Mayer, J. H. Hibben, M. A. Tuve, H. M. O'Bryan, L. R. Hafstad, K. Cohen, H. J. Hoge, A. L. Sklar, F. D. Rossini. Missing from photo: N. Bjerrum, V. Bush, N. P. Heydenburg, R. D. Potter, A. E. Ruark.

F. G. Slack—was first off the mark.[19] Fermi came into his office after Bohr had left for Washington, and Anderson told him all about Hahn and Strassmann's discovery—only to learn that Fermi already knew about it from Lamb.[17] Rushing into the laboratory, still on 25 January, Anderson and his colleagues managed to detect the fission fragments. By then, however, Fermi had already left for Washington, and although Dunning telegraphed the news to him there, it appears that he did not receive Dunning's telegram and hence did not learn about the success of the Columbia experiments until the close of the Washington meeting.[17] (Anderson claims[17] that Fermi did receive Dunning's telegram, but Fermi himself does not mention this, and Bohr told[2] Frisch in a letter of 3 February that the first reports of the detection of the fission fragments came in on the last day of the conference.)

Meanwhile, R. D. Fowler and R. W. Dodson of Johns Hopkins University, evidently tipped off by one of their six colleagues attending the Washington meeting, swung into action. They confirmed the discovery on Saturday morning, 28 January.[20] They managed, in fact, to just beat out R. B. Roberts, R. C. Meyer and L. R. Hafstad of the Carnegie Institution's Department of Terrestrial Magnetism, all of whom, immediately upon hearing the news, rushed across town and into their laboratory, confirming the discovery that same day, on 28 January.[21] Owing to their proximity to the conference site, however, this group was the most fortunate one of all in one respect: As reported[22] in the *Science News Letter* of 11 February 1939, Roberts and his colleagues, in "a historic midnight experimental conference" on 28 January, demonstrated the existence of the fission fragments to Bohr and Fermi.

Luis Alvarez in Berkeley did not lose out by much. He caught an announcement of the discovery of fission in the *San Francisco Chronicle* while having his hair cut in the student union. Rushing out of the barber chair and back to the Radiation Laboratory, he first broke the news—dramatically—to graduate student Philip Abelson and then telegraphed Gamow in Washington for further details. He received them on the morning of 31 January, and that same afternoon he and G. K. Green observed[23] the predicted ionization pulses.

Publication

In the midst of all of this furious activity on the east and west coasts of the United States, Bohr returned to Princeton on Sunday, 29 January. He was still totally unaware of Frisch's experiments in Copenhagen: Even bombarding Frisch with telegrams had raised no sign of life. Finally, at long last, on 2 February, Frisch's letter[2] of 22 January, along with its two enclosed notes, arrived in Princeton. Bohr fairly jumped for joy, as is evident from his reply[2] to Frisch on the following day:

> I need not say how extremely delighted I am by your most important discovery, on which I congratulate you most heartily.... The experiments of Hahn, together with your aunt's and your explanation have indeed raised quite a sensation not only among physicists, but in the daily press in America. Indeed, as you may have gathered from my telegrams and perhaps even, as I feared, from the Scandinavian press, there has been a rush in a number of American laboratories to compete in exploring the new field. On the last day of the conference in Washington (January 26–28), where Rosenfeld and I were present, the first results of detection of high energy splitters were already reported from various sides. Unaware as I was, to my great regret, of your own discovery, and not in possession even of the final text of your and your aunt's note to *Nature*, I could only stress (which I did most energetically) to all concerned that no public account of any such results could legitimately appear without mentioning your and your aunt's original interpretation of Hahn's results. When Hahn's paper appeared, information about this could of course, for your own sake, not be withheld and was, in fact, the direct source of inspiration for all the different investigators in this country. When I came back to Princeton I learned from an incidental remark in a letter from [my son] Hans the first news of the success of your experiments. I at once telephoned this information to Washington and New York, and succeeded in obtaining a fair statement in a *Science Service* circular of January 30, of which I have sent a copy to my wife, but I could not prevent various misstatements in newspapers. This is of course regrettable but without any importance for the judgment of the scientific world, which here even more than in Denmark is accustomed to such happenings.

With Frisch and Meitner's, and Frisch's, notes at last in his possession, Bohr could make "a few corrections" in his own note to *Nature* and enclose "a new copy" for Frisch to pass on for publication. "Quite apart from the question how much or little new the

George Placzek, photographed in 1946. In 1934 Placzek used Bohr's Nobel prize medal to measure the absorption of slow neutrons by gold.

note contains," Bohr said, "I think that its appearance at the earliest possible opportunity will contribute essentially to clear up the confusion as regards the history of the discovery and its theoretical significance." Actually, it took a relatively long time for the notes to appear in print: Meitner and Frisch's, and Frisch's (both submitted on 16 January), appeared only on 11 and 18 February, respectively,[24] while Bohr's (dated 20 January, revised 3 February) appeared on 25 February.[25] Frisch, when he wrote once again to Bohr on 15 March, remarked[2] that these long delays occurred "probably on account of an accidental increase in the number of letters [received by *Nature*] and, perhaps, because we had not sufficiently stressed the importance of quick publication, when writing to the editor."

On 7 February 1939, Bohr took yet another opportunity to sketch[26] the history of the discovery and interpretation of nuclear fission in a letter to the editor of the *Physical Review*. The main purpose of Bohr's letter, however, was entirely different. George Placzek had visited Princeton a few days earlier, and stimulated by Placzek's penetrating questioning and inevitable skepticism, Bohr had conceived an ingenious argument that had led him to conclude that it was not the heavy uranium isotope U^{238} that is primarily responsible for fission, but rather the rare, light isotope U^{235}. At the end of his letter, Bohr remarked that he and Wheeler were currently engaged in a "closer discussion" of the mechanism of nuclear fission. That discussion occupied the entire balance of time—until May 1939—remaining to Bohr in Princeton. It remains one of the finest collaborative efforts in physics.[27] No one did more to promote an accurate understanding of the history of nuclear fission, and of nuclear fission itself, than the man who brought the news of its discovery to America.

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