LETTERS

More on How Nazi Germany Failed to Develop the Atomic Bomb

I read with great interest "Bomb Apologetics: Farm Hall, August 1945" by Jeremy Bernstein and David Cassidy (August 1995, page 32). I believe I can contribute some direct knowledge about these interesting historical events.

I got my PhD under Werner Heisenberg in 1955 and thereafter my first job under Kurt Diebner. In Heisenberg's institute, little if any conversation about the German uranium project took place. However, Heisenberg proudly displayed in front of his office a photograph of the Haigerloch deuterium oxide (heavy water) natural uranium reactor, which did not go critical only for the lack of available D_2O. Samuel A. Goudsmit's book *Alsos Mission* was not kept in the institute library but was circulated clandestinely. I believe Heisenberg must have felt that Goudsmit's account of the German uranium project was not objective, in particular in his downplaying Heisenberg's knowledge of bomb physics.

The question has been raised repeatedly as to whether it was true, as Carl Friedrich, Freiherr von Weizsäcker, said at Farm Hall, that the Germans could have built the bomb but did not want to build it, or whether this interpretation (*Lesart*) was later invented by von Weizsäcker to absolve the Germans of any guilt. Extensive conversations I had with Diebner around 1955 fully confirm the truthfulness of von Weizsäcker's statement at Farm Hall. In this context it should have been made clear by Bernstein and Cassidy that publicly voicing an opinion such as von Weizsäcker's ("We could have built it but we did not want to build it") would have been suicidal in a dictatorship.

In 1939, shortly after the discovery of nuclear fission, Paul Harteck experimented with a heterogeneous assembly of uranium rods placed in carbon dioxide ice. Subsequently, a heterogeneous arrangement with plates of uranium in D_2O was recognized to be advantageous. It was with such an arrangement that Heisenberg and R. Döpel reached for the first time an infinite-assembly neutron multiplication factor $k_\infty > 1$. When Diebner suggested that a three-dimensional cubic lattice of uranium and D_2O might be even better, this was at first rejected by Heisenberg, who at that time did not recognize the importance of resonance absorption in uranium-238 (favoring cubes over plates or rods). Diebner, remaining unconvinced, succeeded in reaching a much larger k_∞-value with a lattice of uranium cubes in D_2O ice. From that moment on, it was clear that a D_2O natural uranium reactor could be built, and that with it "element 94" (the name the Germans gave plutonium) could be produced in appreciable quantities as a bomb explosive. Following his success, Diebner approached a leading scientist involved in the project (I believe he told me it was Walther Gerlach), and mentioned that he was going to write to the War Ministry to inform the authorities about their ability to produce an atomic bomb explosive. It was Diebner's responsibility to write such a letter because he was the head of the uranium project under the German Army Ordnance. But whoever it was that Diebner approached at that fateful moment (Gerlach?) advised Diebner that if he were to write such a letter, "Everyone will be against you" (I quote Diebner directly).

Diebner therefore did not write the letter, thereby supporting von Weizsäcker's "silent conspiracy" theory not to build the bomb. This may also explain why Heisenberg never went through the trouble of accurately determining the critical mass of ^{235}U, concentrating his efforts instead on the "uranium machine." Diebner's story was confirmed for me by Dr. Walter Trinks, a high-explosive expert who had worked under Diebner and who, when I talked with him many years after the war, blamed Diebner for the fact that Germany did not get an atomic bomb.

It was Diebner who also told me that Wernher von Braun had visited

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Heisenberg around 1942 and had asked Heisenberg about the potential of uranium fission for space propulsion. As I was later told by von Braun, Heisenberg explained to him that he could only think of a nuclear propelled submarine, not a rocket. The conversation is noteworthy because it is one of the earliest known discussions about nuclear rocket propulsion between two leading scientists. In my conversation with von Braun, he expressed his awe and admiration for Heisenberg, admitting that he had incorrectly believed that Heisenberg had been an ivory-tower scientist, removed from any practical problem.

Another point worth mentioning is that it was Fritz Houtermans, not Leo Szilard, who had first suggested the possibility of a nuclear chain reaction with neutrons. This view is also shared by Soviet scientists who had known Houtermans well, because Houtermans had emigrated before the war to the Soviet Union and had been arrested there, but had returned to Germany around 1940 in a Soviet–German prisoners exchange and then had worked for the German uranium project. And it has been forgotten that simultaneously with Lise Meitner and Otto Frisch, Gottfried von Droste and Siegfried Flügge of the Kaiser Wilhelm Institute for Physics in Berlin had reached the same conclusion regarding the energy released in uranium fission, with their results being published in the *Zeitschrift für Physikalische Chemie.*

Following the end of World War II, Diebner became the cofounder of the Society for Nuclear Ship Propulsion in Hamburg. Then, with the rearmament of West Germany in the course of the cold war, he got back his civil service post at the Ministry of Defense. But because, shortly after 1945, he had signed a declaration against the use of nuclear weapons, with the declaration coming out of East Germany where his parents still lived, he had to resign his position, as I heard, under pressure from the US government. Diebner, I believe, had recognized that a nuclear war on the territory of Germany would mean the end of the nation. Even though Diebner found (by pure guesswork) a greatly improved heterogeneous reactor design, Heisenberg never seems to have given Diebner much recognition. This was consistent with my own observation that Heisenberg did not always recognize the ingenuity of others, in particular if their ingenuity did not fit his own scientific way of thinking.

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From what the German nuclear scientists detained at Farm Hall discussed immediately after the atomic bombing of Hiroshima—and also from other sources—it is quite obvious that they were aware of the possible significance of element 94 (plutonium) as fissile material for atomic bombs. They were also aware of the production process, involving neutron capture in uranium-238 and subsequent beta decay into element 94 by way of two already known intermediates, uranium-239 and neptunium-239.

Plutonium itself, however, remained undiscovered in Germany during the war. There was not even a search for this element, although it would have been a straightforward continuation of research initiated in 1934 by Otto Hahn, Lise Meitner and Fritz Strassmann at the Kaiser Wilhelm Institute for Chemistry in Berlin. They had found ^{239}U (halflife 23 minutes), in neutron irradiations of uranium and had assigned it, by radiochemical methods, to atomic number 92 and, by its formation in a resonance neutron capture, to mass number 239. Furthermore they showed that ^{239}U emits negative beta particles. Thus, it must decay into the element 93. But all attempts to find this decay product in Berlin at that time failed (in contrast to what is pointed out in Bernstein and Cassidy's article). In the spring of 1940, Edwin McMillan and Philip Abelson[1] at the Lawrence Radiation Laboratory were successful in identifying the new element 93, neptunium, in the form of a nuclide with a halflife of 2.36 days, again being a beta-ray emitter that must now decay into element 94. Because of the much lower neutron fluxes available at the Berlin institute, completely different procedures for chemical enrichment and detection of element 93 had to be worked out. Then, early in 1942, Strassmann and Hahn and, independently, Kurt Starke reported its confirmation.[2]

Not even an attempt to find the decay product, element 94, is mentioned in the laboratory notebooks of Hahn and Strassmann.[3] Their wartime research, conducted together with a handful of students and postdocs, was focused on the radiochemical identification of fission products, and the results were published in the open literature. This work was motivated as being important for the time when a nuclear reactor would be in operation, and the purification of the nuclear fuel from fission products would be required from time to time. Why did the Berlin group leave aside a search for element 94, by far the most important product formed in such a machine?

One may argue that Hahn deliberately did not undertake any work in this direction, in line with his passive attitude in the *Uranverein* ("Uranium Club"). However, it seems unlikely that Hahn would not have revealed such a decision during the emotional days at Farm Hall, or in one of his many later statements on the history of nuclear fission. More likely, he and Strassmann considered a search for the decay product of ^{239}Np as a hopeless enterprise since McMillan and Abelson[1] had already tried, without success, to find alpha particles emitted by an element 94 growing in a strong ^{239}Np sample. The source used in that experiment had a strength of 11 millicurie, or 400 megabecquerel. Production of such a source or even a stronger one, as apparently required, was out of reach at the Berlin institute. As McMillan later admitted, their experiment was a failure, probably because the sample was too thick for alpha particles to escape.[4] Thus, the estimated halflife of plutonium-239 was much too long, a million years or more[1] (the present figure is 2.41×10^4 years). The activity of ^{239}Pu accumulated in McMillan and Abelson's ^{239}Np sample can now be estimated as about 100 becquerel. The strongest sample of ^{239}Np in the hands of Hahn and Strassmann[5] after an irradiation with neutrons at a small linear accelerator corresponded to 5 megabecquerel for a prolonged irradiation, and at least one order of magnitude stronger neutron fluxes were available to them at the Paris cyclotron. Thus, ^{239}Pu was in fact already accessible, in very small amounts of a few bequerel but sufficient enough for detection.

The fact that Hahn's group was so easily discouraged by the McMillan–Abelson report from independently searching for element 94 lends credence to the view that atomic bomb research was not seriously pursued in Germany during World War II.

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I would like to amplify several points made in the articles on the Nazi and US A-bomb projects.

There is no doubt that Werner Heisenberg already understood the basics of how to obtain fissile materials for an atomic bomb early during World War II. In December 1939, about three months after war was declared, he published a paper entitled "Die Möglichkeit der technischen Energiegewinnung aus der Kernspaltung" ("The Possibility of Technically Obtaining Energy from Nuclear Fission").[1]

As for Heisenberg's personality, it is worth noting the following characterization, which was published in a German weekly magazine in the late 1980s: "The lover of Beethoven and Mozart, a pure product of the German national educated middle classes, veering between criticism and the pursuit of a career. In a treatise which he laid under the Christmas tree of friends in 1942, he formulated that the 'demons' had seized political power. . . . In spite of it, he took up the directorship of the Kaiser Wilhelm Institute for Physics and he gave thanks on 15 December 1943 for the War Cross of Merit First Class being bestowed on him."[2]

Regarding the views of the German scientists on the development of the atomic bomb, it should be noted that Paul Rosbaud, a Berlin publisher who was a wartime spy for the British, commented that if the Germans had known how to build a bomb, "they would have presented it to their Führer on a silver platter."[3]

Finally, it is debatable that, as Bernstein and Cassidy write, "all the really hard problems were left untackled and unsolved" by the Germans. For instance, the Germans experimented with several methods of isotope separation, and they also had a uranium purification plant in Oranienburg, north of Berlin, that survived Allied bombing raids until almost the end of the war (it was finally bombed on 15 March 1945, on orders of Leslie Groves, to prevent the pure uranium stored there from falling into Soviet hands.[4]) Of course, the German effort to solve problems lost momentum after various technical setbacks in 1942 and 1943 and as the Allied air offensive set about destroying German industry. Then, as Alexander von Cube and his colleagues observed in retrospect, "It became clear to every participant in the uranium project that one could not count on finishing the atomic bomb during the war." [1]

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The question of the ethics and the motives of the German scientists, particularly Werner Heisenberg and Carl Friedrich, Freiherr von Weizsäcker, has been the subject of much discussion over the last 50 years. The simplistic comments by Jeremy Bernstein and David Cassidy (August 1995, page 32) do this serious subject a disservice.

To begin to approach this subject, one must consider the situation of Heisenberg during World War II. He lived under a terrorist dictator with the fear that some misstep would send him to prison or to death. His country was being destroyed by Allied fire bombings in which tens of thousands of civilians were being killed. It is presumptuous for those who have not known these conditions to make simple statements as to Heisenberg's or Weizsäcker's motives.

The suggestion by the authors that Heisenberg was "trusted" by the Nazi authorities because he was allowed to travel to Switzerland ignores the complex layers of mistrust in a totalitarian society. Heisenberg had no doubt that he would be followed by German agents in Switzerland although he did not know that a US agent would be there prepared to assassinate him.

The question of whether Heisenberg and von Weizsäcker would have worked on an atomic bomb for Hitler if circumstances had allowed can never be answered, because luckily they and the government correctly concluded there was not time to finish such a project. Heisenberg did help save the lives of a number of endangered scientists. As Victor Weisskopf says in his introduction to Heisenberg's widow's memoirs, "[S]uch acts weigh more than any statements."[1]

Reconstructing one's motives is

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difficult even under the far easier circumstances in the US. How many of the Los Alamos physicists can explain why they continued to work on the bomb after the original motive of keeping ahead of the Germans had disappeared? How many are willing, like Bob Wilson,[2] to admit they should have stopped?

In looking back 50 years, let us recognize that the Oppenheimers and the Heisenbergs were honorable men caught in a terrible moment of human history. Let us not talk about "apologetics" but let us try to learn the lessons that may help us face our responsibility in the future.

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Your unsentimental journey through the Nazi and Allied A-bomb projects (PHYSICS TODAY, August 1995) was fascinating and timely in this 50th anniversary year. Your articles—taken together with *Time Bomb* by Malcolm C. MacPherson (E. P. Dutton, 1986)—illustrate that "history as wishful thinking" is just as dangerous in atomic apologetics as it is in other areas.

MacPherson's book demonstrates how lucky the Allies were to have gotten the A-bomb. For one thing, the Allies used relatively abundant and inexpensive graphite as a moderator whereas the Germans used scarce heavy water (they also looked at graphite but got discouraging results from their tests). One can imagine Werner Heisenberg slapping his forehead on learning that the Allies had used graphite.

For another thing, although the Germans had access to uranium from Czechoslovakia, uranium was so scarce in the US that the whole Manhattan Project was endangered. But by a fantastic stroke of luck, back in the fall of 1940, a Belgian named Edgar Sengier, the chairman of the Union Minière de Haut-Katanga, had shipped 1140 metric tons of rich uranium ore (originally from the Belgian Congo) to a warehouse on Staten Island. When General Leslie Groves sent Captain Kenneth Nichols on a top secret mission to find uranium, Sengier was already expecting him. Absent Sengier's prescience, the Allies might have been as hamstrung by a lack of uranium as the Germans were by a lack of heavy water.

The problems that came into being with the dawn of the nuclear age are still with us, and among the most serious is the proliferation of nuclear weapons. This will continue be a problem as long as plutonium is seen as a feasible energy source. The plutonium used to generate power is the same as the plutonium used in a bomb. The consequences for consistent foreign policy are readily apparent, as demonstrated by recent events involving Iran and North Korea.

America's traditional antipathy toward government involvement in science, which waned in the postwar era, is now undergoing an ideological resurgence. Many individuals are pressing for less government involvement in science, as in business and the arts. However, if there is one area in which the interests of science and government should coincide, it is in the development of energy alternatives, such as fusion power, which could leapfrog plutonium as an energy source.

In conclusion, I offer a conjecture about Otto Hahn's statement that "the fast [neutrons] in 235 do the same as the 238, but 130 times more" (see Bernstein and Cassidy's interpretation of Hahn, page 35): Could it be that Hahn was simply referring to the fact that uranium-238 is approximately 130 times more abundant than uranium-235?

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Manhattan Project: Book Faulted, Heisenberg Paged

The article "Groves and the Scientists, Compartmentalization and the Building of the Bomb" (PHYSICS TODAY, August 1995) lists in its references *Manhattan Project: The Untold Story of the Making of the Atomic Bomb*, a 1967 book by Stephane Groueff. Readers without an intimate knowledge of the project will most likely accept without question the accuracy of the Groueff text. However, as a research scientist and subsequently division director in the wartime Substitute Alloy Materials Laboratory (Columbia University), I would like to point out an erroneous report in his account of the SAM research.

The diffusion separation cascade for concentrating uranium-235 had to be sealed as completely as possible from the external atmosphere. That required reducing the air leakage through the shaft seals of the gas compression machinery to an unprecedentedly minute amount. Groueff alleges that the design of this seal was undertaken by me at SAM and independently by a staff engineer at the Kellex Corp (engineers for the construction of the cascade), and that I designed a seal that completely failed a cascade pilot plant test. Groueff's claim and much of the accompanying detail are erroneous. No seal was designed by me or members of my group; the failed seal mentioned by Groueff involved only Kellex personnel.

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The wartime weapons laboratories at Los Alamos were all connected by a public address system. If one was unable to reach a person at his or her regular phone extension, one could call and ask the telephone operator to page the person. Many times each day, paging calls for specific named individuals were heard throughout all the laboratories. I remember that, on a few occasions, one heard the PA system call out, "Werner Heisenberg, please call extension ____" or "Werner Heisenberg, please report to the director's office."

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Spin Model Skyrmish

The article "Where Does the Proton Really Get Its Spin?" by Robert L. Jaffe (September 1995, page 24) explains very clearly why this question has been exercising the minds of many theoretical and experimental physicists ever since the surprising measurements from the European Muon Collaboration. However, despite our generally favorable opinion of the article, we feel that we must react to the author's statement that "because the Skyrme model has many problems with more traditional hadronic phenomenology, no one takes it very seriously as a way out of the spin crisis" particularly because this assertion is made as a comment on a joint paper we wrote.

It is true that the phenomenology of the Skyrme model is neither completely understood nor perfect, but the model does have some striking successes to its credit—for example in fitting pion–nucleon scattering phase