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The Peculiarities of the German Uranium Project (1939–1945)

Manfred Popp [1,*] and Piet de Klerk [2]

1 Karlsruhe Institute of Technology, Weberstr. 5, D-76133 Karlsruhe, Germany
2 University of Leiden, Rapenburg 70, 2311 EZ Leiden, The Netherlands
* Correspondence: popp@kit.edu

Abstract: An analysis of the peculiarities of the German Uranium Project (1939–1945) reveals that it was, in many ways, different from what one would expect. There was no work at all on a possible bomb, nor on plutonium. The reactor experiments were limited to subcritical systems and did not attempt to achieve the proclaimed goal of a self-sustaining chain reaction. The so-far identified deficits (lack of interest in Nazi circles, mismanagement, scientific mistakes, and deteriorating work conditions during the war) are relevant but not sufficient for explaining the peculiarities. We deduce that the scientists involved, and even the Heereswaffenamt (army ordnance), shied away from making progress, not only towards a bomb but even towards a reactor. They did not fail; they rather renounced a possible success in order not to provoke political interest in the development of a bomb.

Keywords: World War II; nuclear research; reactor; atomic bomb; Uranverein

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Introduction

This article further develops a new interpretation of the German Uranium Project 1939–1945 started by one of the authors in 2016 [1] and continued in this journal in 2021 [2]. The 2021 article focused on an interpretation of the most important source about the project, the official report of the Army Weapons Office of February 1942 [3]. Previous authors have only studied the 'introductory overview' of this report and have fallen for the raising of exaggerated expectations by its writer. The actual technical report speaks a completely different language. Previous authors also did not sufficiently consider that the German scientists had to work under an authoritarian regime. This necessarily led to compromises and to euphemistic statements. Therefore, in this article, we want to focus on an interpretation of what the German scientists actually *did* and what possible questions they *did not* investigate. In doing so, we discover many features that distinguish the Uranium Project from other research programmes. These features shed new light on events that have interested so many people for so long.

Part 1: Observation of the peculiarities of the German Uranium Project

Compared to normal scientific programmes, the German Uranium Project was characterised by many distinct peculiarities. In describing them, we must ask ourselves a number of key questions.

- *When Was the Uranium Project Created, and by Whom Was It Directed?*

In the 1930s, the interests of physicists turned from the atom to the nucleus, made of positively charged protons and uncharged neutrons. A first surprise was that, for nuclear reactions, slow neutrons were much more effective than powerful, fast particles. In December 1938, after bombarding uranium with such slow neutrons, Otto Hahn and Fritz Straßmann discovered, to their surprise, that barium had emerged, half as heavy as uranium. In the weeks thereafter, Lise Meitner, who was consulted in her Swedish exile by her former colleague Hahn, and her nephew, Otto Frisch, interpreted that as 'nuclear fission' and found out that every fission sets free the vast energy of 200 MeV. In March

1939, Frédéric Joliot-Curie reported that, during fission, several neutrons were emitted that could trigger new fissions. At that point, it was clear that a chain reaction could set free enormous amounts of energy.

The head of the research department of the German Army Ordnance (Heereswaffenamt, HWA), Erich Schumann, instructed his staff immediately to collect all publications on nuclear fission, to keep in close contact with the nuclear physics institutes, and to prepare the foundation of a new section for nuclear physics at the HWA, as well as the construction of an experimental facility at the Gottow experimental station, south of Berlin [4]. In April, the Hamburg physical chemist Paul Harteck wrote to the HWA about nuclear fission having the potential to produce a powerful explosive. After the war, he described his motive by saying that "there was no support for scientific research in those days" ([5], p. 97). Indeed, the First World War and the following economic crises had led to a scarcity of funds for science, which had shifted the interest to theoretical physics during the 1920s. Now, the experimental physicists saw an opportunity to restore their international position. At the same time, Georg Joos and his assistant Wilhelm Hanle of Göttingen University informed the German ministry for research about the potential of the new source of energy. Their letter was forwarded to Abraham Esau, responsible for physics at the Reich's Research Council (Reichsforschungsrat, RFR). He called an expert meeting on 24 April 1939, where it was decided to establish the 'Arbeitsgemeinschaft für Kernphysik', the so-called first *Uranverein* or the First Uranium Club. In the following months, Esau began to organise laboratory equipment and procured the supply of some uranium, including an export ban on uranium compounds ([6], p. 31).

On 9 June 1939, the Journal 'Die Naturwissenschaften' published an article by Siegfried Flügge from the Kaiser Wilhelm Institute for Chemistry (KWI-C) [7], which many earlier authors rated as very influential. It was, but it had massive deficits, which were only observed by Lise Meitner [8]. The often-quoted prediction that a chain reaction in uranium would lead to "an explosion [. . .] approximately equivalent to a volcanic eruption" refers to an explosion of natural uranium that is impossible. Flügge's lengthy comments about the possibility of natural uranium ore deposits exploding should have raised concerns among experts. The publication was discussed in a meeting called by the 'Chef des HWA' Karl Becker with his advisors Esau and Max Planck [4].

The new HWA section on nuclear matters was founded on 15 June within the physics division under Walter Basche, which was part of the research department. The physicist Kurt Diebner became its head. Two months later, the Deutsche Allgemeine Zeitung published a simplified version of Flügge's article in a supplement ([9], pp. 197–206). Surprisingly, the announcement of an artificial volcano did not arouse fantasies in the minds of National Socialist leaders. It would take until June 1942 for one of them, Albert Speer, to show interest in the atomic bomb.

Ten days before the outbreak of the Second World War, the HWA took over the management of nuclear activities with brute force: the Göttingen scientists who were the core of the First Uranium Club were drafted into military service, and their materials and equipment were brought to the HWA. Diebner called a number of leading scientists for a meeting on 16 September. It was followed by a second meeting on the 26th with broader participation, and so, the Second Uranium Club was founded, which continued until the end of the war without major changes [4]. At the second meeting, Werner Heisenberg, considered by many the best theoretical physicist of his time [10], was asked to prepare a basic description of the foundations for the use of nuclear energy. In December, Heisenberg presented his conclusions. Unlike Flügge's premature announcements, his findings contained the principles for the use of nuclear power that are still valid today.

On 17 January 1940, half of the KWI for Physics (KWI-P) was seized by the HWA after five months of negotiations with the Kaiser Wilhelm Society (KWS) ([9], pp. 235–238). The director of the Institute, the Dutch physicist Peter Debye, was forced to leave when he refused to become a German citizen. Diebner became the institute's acting director. The second part of the institute that remained under the umbrella of the KWS was headed by

Ludwig Bewilogua. Although the seizure of the institute under the directorship of Diebner meant that the employees worked for the military, the HWA was not able to avoid that, in 1941, ten of the younger employees were drafted as soldiers—among them, almost all doctoral students who worked directly for the Uranium Project [11]. Apparently, the project was not held in high esteem by the Wehrmacht.

In early December 1941, against the background of the deterioration of the situation at the fronts and Hitler's instruction to speed up efficient armament production, Schumann called a meeting [12] to discuss the demand that the HWA had to concentrate on armament projects that would yield results in the near future (that is, within nine months). This set a process in motion that the HWA withdrew from the project and that the RFR and the KWS took over.

In June 1942, the new armament minister Speer held a meeting with generals and scientists from the Uranium Club, because meanwhile, some in the Wehrmacht had become interested in an atomic bomb. The presentations confirmed that, after almost three years under the HWA (a period when life in Germany was not yet affected by the severe consequences of the war), there had been no major progress. Speer concluded that an atomic bomb would not become available during the war [13]. After the war, General Milch, who had been instrumental in organising the meeting, remembered the unconvincing presentation of the scientists ([6], p. 123). In late 1942, Esau was reinstalled as coordinator by the president of the RFR, Göring. However, the coordination continued to be handled by Esau's deputy, Diebner, who also continued with his own activities in Gottow. From early 1944 on, the programme was directed by the experimental physicist Walter Gerlach.

Peculiarity 1: At no point did the Uranium Project have political (Nazi) support. At the beginning of the war, the HWA showed some proactive interest, but by late 1941, when priorities had to be set and not much progress was made, it gladly handed the coordination of the project back to the RFR, although Diebner remained active in a coordinating role and continued the reactor experiments of the HWA. Therefore, as will be argued below, the HWA took over the project in a half-hearted way in mid-1939, and from the above, it is clear that, in 1941, it gave the project back in a half-hearted way.

- *What Was the Knowledge Base for the Project?*

The scientific discussion of the consequences of Hahn's discovery of fission began immediately in January 1939. Open international publications were stopped in Germany in the autumn of 1939 by governmental action and in the USA in mid-1940 when the scientists agreed among themselves to ask the journals to delay publication until the end of the war. In the conceptual phase of the programme, the Germans were still part of the open international scientific exchange.

In his paper of December 1939 [14] and a follow-up in February 1940 [15], Heisenberg presented an impressive theory of the reactor: "The safest method of making a [uranium] machine is by enriching the isotope U 235. The further the enrichment is pushed, the smaller the machine can be. The enrichment of U 235 is the only method by which the volume of the machine can be made smaller than one cubic meter. It is also the only method of producing explosives that exceed the explosive power of the most powerful explosives to date by several powers of ten". "However, this explosive transformation of the uranium atoms can only occur in almost pure U 235".

Heisenberg's important conclusion was correct, although it was based on an error. Heisenberg called for pure U 235 for the bomb, "since even with small admixtures of U 238, the neutrons are captured away at the resonance point of U 238". This resonance point is far below the energies of the fast neutrons that are needed for the bomb, a fact that Heisenberg had not yet recognised. The resonance capture during the cooling of the neutrons to 'thermal' temperatures is the competitive process for fission in a reactor. It plays no role in a bomb. In reality, U 238 has no place in the bomb, because its fission cross-section is too small.

Heisenberg called this conclusion 'great luck', because the production of pure U 235 "if it were possible at all, would require a quite enormous technical effort" [16]. He had the condition confirmed by the young theoretician Paul Otto Müller [17], who had been working at the KWI-P since May 1939 after completing an excellent doctorate under Erwin Schrödinger in Graz [18]. He was the only Nazi among the nuclear physicists, and Heisenberg might have found it important to have him on board for this important conclusion.

The enrichment of U 235 required isotope separation, according to Heisenberg's vague assessments, on the scale of many kilograms, if not more. As isotopes are chemically identical, only technical processes depending on the tiny mass difference between them could be employed. The only existing technology, mass spectroscopy, was limited to micrograms. As for other possible technologies, there were only a few preliminary ideas. The development of an industrial process would take many years. That meant that there was no prospect of building compact reactors for military vessels, nor of building an atomic bomb within a couple of years. On this basis, the 'Chef des HWA' Emil Leeb, Becker's successor, recorded after the war that "In the opinion of the Office, the use of an atomic bomb was not to be expected in the course of the war with the available research resources" [19].

A comparison with the US Manhattan Project confirms Heisenberg's and the HWA's judgement. The production of the single uranium bomb that destroyed Hiroshima consumed two-thirds of the total cost of two billion dollars. Such an effort would have been impossible for Germany.

Heisenberg focussed his report on the reactor: "Normal uranium can also be used without enriching U 235 if uranium is combined with another substance slowing down the neutrons [. . .] without absorbing them. Water is not suitable for this purpose. On the other hand, according to the data available so far, heavy water and very pure coal fulfil this purpose." The machine would best be structured in layers of at least one square meter [14].

With this theory, Heisenberg laid the foundation for the HWA's programme and took on the role of its unofficial scientific director. We do not know the work plan at the beginning, but the scope of the programme can be deduced from the 1942 HWA report. The HWA began to support the study of a variety of potential methods for enriching U 235 and the collection of nuclear physics data. The greatest priority was given to the provision of materials (uranium and moderator). "Due to the difficulty of obtaining material, much effort was put into increasing the multiplication factor, as it [...] meant a reduction in the critical size and thus material savings" [20]. The HWA stated that, as soon as enough material was available, the first self-supporting machine—still purely an experimental device—should be built. Only after that should the work on three tasks begin: the development of the machine into a technically viable device; the technical, especially military use, of the machine; and the development of a uranium explosive ([3], pp. 15–16), a much larger and technically oriented project.

The demonstration of the self-sustained chain reaction would have been a politically important event but was not necessary for the first phase of reactor development of procuring and minimising the amount of material needed. Even the decision to launch the huge Manhattan Project did not wait for this demonstration. In a later phase, the German scientists would have needed a critical reactor in order to learn how to control it. In the current project, they were protected from political influence by delaying the completion of the proclaimed task.

Peculiarity 2: The HWA started the project at an early phase without clear perspectives. After the development of a uranium bomb appeared out of reach, the main focus of the programme was reactor development with the goal of demonstrating a chain reaction, proclaiming a condition for the start of a larger reactor programme and the development of a bomb that was seen as necessary in order to convince politicians of the feasibility of a reactor, although it was scientifically not necessary for the first phase of the programme.

- *Was the Uranium Project a Military Programme?*

At the beginning of the war in September 1939, Diebner's section was the first military office for nuclear power in the world. However, the members of the Uranium Club continued to work as civilians in their institutes, receiving their normal salaries, and were given 'UK Status' (*UnabKömmlich* = indispensable).

The post-war scientific reports written under supervision of the Allied forces in Germany reveal that 75% percent of the work of the Uranium Club was on basic science, developing nuclear measuring techniques and applying them to collect nuclear data. The historical reception of the project has so far been limited to the 25% of the effort that was devoted to nuclear fission and nuclear technologies [21]. Reactor experiments were conducted at the KWI-P in Berlin and the University of Leipzig under Heisenberg's direction, with the Heidelberg experimentalist Walther Bothe as an advisor. The HWA performed similar experiments on its own site at Gottow. Six methods for isotope separation were studied in parallel. Some research areas were not addressed: (1) research into materials with low neutron absorption and the ability to withstand radiation and (2) the improvement of nuclear measurement technologies for which cyclotrons would be the appropriate instruments.

The scientists had to send their reports to the HWA in ten copies, produced by typewriter with carbon paper. Graphs and formulae had to be inserted by hand into all ten copies. Most of these reports were stamped 'GEHEIM' (secret) by the HWA. Occasionally, several secret reports were compiled into booklets, which were distributed among the groups for internal information. All these reports had the style and the quality of scientific publications, and several were allowed to appear in journals, although with some delay. The reports did not contain any demands for better financing or the procurement of personnel, materials, and equipment.

Within the general lines of the programme, the individual groups were relatively free to choose their research strategy. Willibald Jentschke, who later became a pioneer in high-energy accelerator physics, reported that Prof. Stetter's group in Vienna, to which he belonged, was free in its choice of research projects as long as they measured nuclear data [22].

The staffing of the reactor experiments was ridiculously small. For example, the group conducting experiments designed by Werner Heisenberg at Leipzig University consisted only of Robert Döpel, with his wife—a lawyer—as an assistant, plus a single technician.

The programme as a whole had rather moderate dimensions. The workforce was less than one hundred. It consisted almost exclusively of the existing staff of universities and KWI institutes. The HWA financing of the programme was restricted to providing materials and equipment. The exploratory character of the project was further underlined by the surprising fact that not a single German 'Diplom-Ingenieur' was participating, and no industrial company was involved, with the exception of the Auer Company for uranium treatment. The total costs for the period from 1939 to 1945 amounted to about 10 million Reichsmarks (RM) ([9], pp. 321–322), equal to two million dollars. In other words, the German effort was smaller than the Manhattan Project by a factor of 1000.

The Uranium Project was an exceptional case. The technological achievements of the Third Reich were remarkable and are still often transfigured into legends today [23]. The HWA was the centre of developments in military technologies, cooperating closely with top scientists in the fields of explosives physics and chemistry, shaped charges, ballistics, light guns, missiles, special propellants, ultra-red and visual ranges, biological weapons, and communications engineering. In all these fields, there was well-managed research on concrete, short-term achievable goals. The HWA research department was made up of approximately 100–150 officers, including Diebner's group of up to 10 persons [24]. This was a very small part of the 5000 civil servants and officers working at the HWA [25]. The focus of historians on the Uranium Project should not obscure the fact that it was a very small side activity of the HWA.

Peculiarity 3: No matter that the HWA managed the project until early 1942, it had an academic character, was grossly understaffed, and not equipped to reach technically viable results.

- *How Strong Was Coordination and Cooperation?*

Becker, who had a great interest in research, committed suicide in April 1940. His successor at the top of the HWA from 1940 to 1945, Erich Leeb, had no special interest in research in general and specifically not in the Uranium Project. In his so-far-unnoticed memoirs, written in 1947 [19], Leeb suggested that Becker was made the scapegoat for the ammunition crisis after the Polish campaign, which was actually triggered by the Wehrmacht High Command's contradictory orders. In any case, the pompous state funeral in Hitler's presence was reminiscent of the case of the popular General Rommel, who was faced with the alternative of being sentenced to death for high treason or being given an honourable burial after committing suicide, thereby securing his pension for his family [26].

Leeb devoted only 13 lines of his report to the research department. It said at the end that "It formed an atomic committee within itself, comprising the most renowned researchers in Germany. It functioned until about 1942/43, when it was transferred to the Reich's Research Council". In the main part of Leeb's report that lists the armament achievements of the HWA during the war, the Uranium Project was not mentioned at all. From this, it is clear that Diebner had no strong backing from his superiors. Schumann used to refer to the project as 'atomic nonsense' ([6], p. 147).

The workforce was dispersed over 22 institutes in 12 cities. There was no interaction between the groups, and occasions for discussions were rare. Groups working in the same field were never convened to discuss their progress and to focus their work on more promising approaches. In the beginning of 1940, the HWA issued an order to keep the results secret, even within the Uranium Club, and reserved the right to occasionally make secret reports known among the groups ([24], p. 171).

Another problem was the mistrust among supporters and opponents of the National Socialist regime. Some members of the Uranium Club—for example, Schumann, Diebner, and Erich Bagge—were members or followers of the National Socialist Party; others—among them, Hahn, Harteck, Heisenberg, Gerlach, and Karl Wirtz—were not. Everyone who chose not to openly support the regime had to find a way to deal with its factual power. Heisenberg decided against open opposition, because that would deprive him of any possibility to act and mitigate the consequences of the totalitarian regime ([16], pp. 208–210). After a year-long investigation ordered by SS leader Himmler with several interrogations at the SS Headquarters in Berlin in 1937, he had no illusions about the criminal character of the regime [27]. And he had to be careful not to cast doubt again on his alleged loyalty to the party line.

As a result of the different individual attitudes, there was, as Wirtz said, no atmosphere of confidence among the participating groups and no trust between the scientists and the political institutions ([28], p. 57). As a result of the fragmentation of the workforce, there was no discussion of the strategy to proceed among the scientists, because there was a lack of occasions and a reluctance by those living far from Berlin to speak up without sufficient knowledge of the official line. That made it easy for Heisenberg and Diebner to steer the project. There was no collusion of the scientists; their majority rather preferred to abstain from expressing critique or making proposals. They were content with their privileged situation and did not want to jeopardise it. They and their closest collaborators were exempt from war service, had better working conditions than in the previous 20 years, and enjoyed great freedom, although their work was not important for the war. It was a fragile idyll in which they lived, and therefore, they did not want to put it at risk by complaining because of minor problems or dissatisfactions. Their insecurity may have contributed to the 'intellectual thinness' exhibited by the discussions in Farm Hall ([29], p. 185) which certainly was also a result of the dismissal of excellent Jewish colleagues in 1933. However, this observation should not be overinterpreted. The Uranium Project did not make any demands that would have overwhelmed the scientific capacity of the remaining scientists. One should not forget that the many Jewish-German immigrants in the USA who worked on bomb development in Los Alamos, knowing very well both the problems to be solved and the capabilities of their former colleagues, were convinced that they were in a tight race for the first bombs.

A special problem arose, when Diebner became acting director of the seized KWI-P. "We have Nazis in the institute" was Wirtz' horrified reaction. In addition, members of the institute did not accept Diebner as director because of his mediocrity as a scientist.

The problem was solved by installing Heisenberg as his advisor ([28], pp. 32–33). With this move, Diebner's role was merely that of an administrative director. He was likely to have been primarily concerned with the construction of the externally erected, very simple building for the reactor experiments, which was baptised 'Virus House' to deter the curious. Otherwise, the takeover of the institute meant only a small increase in staff but no strong control of the content by the HWA, since Heisenberg now set the tone. In June 1942, when the institute was given back to the KWS, Heisenberg took over as director.

Peculiarity 4: The project suffered from mutual mistrust due to different attitudes towards National Socialism, from weak leadership, from caution against 'rocking the boat', and the fear of jeopardising the privileged position of the members of the Uranium Club.

- *What Did the German Scientists Know about Plutonium?*

In summer 1940, Carl-Friedrich von Weizsäcker, a theoretical physicist working closely with Heisenberg, explored what happens after the absorption of a neutron by U 238 to form U 239. He deduced from the Bohr-Wheeler theory of nuclei that the new element 93 derived after fast beta decay from the compound nucleus U 239 proposed by Hahn would be fissionable in a similar way as U 235 and would be a possible fuel for a reactor and an explosive. The advantage was that it could be derived from used reactor fuel by chemical rather than isotope separation, a much easier path to the bomb [30].

In the USA, McMillan and Abelson deduced from experiments with a cyclotron at Berkeley that U 239 decays by two subsequent beta decays to the isotope 94/239, later baptised plutonium 239. Their publication [31] was the last to be published about nuclear power during the war. The journal arrived in Berlin with some delay, but by July or August 1940, the German scientists learned that element 94 was the potentially extractable explosive, not element 93.

The normal reaction of scientists to such news would have been to try to produce as fast as possible samples of the new element, the first man-made element and the heaviest. However, this was impossible in Germany. Hahn was only able to detect the fission products by their radiation because their quantities were too small for other detection methods. Element 94, however, was long-lived; its half-life—24,000 years, as we now know—was, at that time, estimated by McMillan and Abelson to be almost one million years. Such rare decays were not detectable. One could produce and study element 94 only with a cyclotron.

- *Were Cyclotrons Available in Germany 1939–1945?*

In the course of the 1930s, cyclotrons became the workhorses for nuclear physics all over the developed world. By 1939, 14 cyclotrons were in operation: 9 in the US, 2 both in England and Japan, and 1 in Denmark. A further 27 were under construction worldwide. In Germany, except for a very small accelerator at Bonn University, only one cyclotron was being built in Heidelberg by the physicist Walther Bothe. It took six years until he could start its operation in the autumn of 1944 ([32], p. 2). A second cyclotron project in Leipzig filled a thick file of documents between April 1937 and June 1944 without ever leaving the stage of paperwork [33].

Stetter's Viennese group also suffered from the absence of a cyclotron. It could only rely on natural neutron sources, which—measured against the energy of the fission neutrons—either delivered energies that were far too high or too low and with a large bandwidth that further reduced the significance of the measurements.

Two cyclotrons potentially became available in occupied countries. The cyclotron in Paris was under construction when the German army occupied Paris. Joliot's *Laboratoire de Chimie Nucléaire* was confiscated by the HWA in July 1940. In September 1940, the young physicist Wolfgang Gentner, who had already gained experience with cyclotrons in Berkeley, California, in 1939, was given the task to put the cyclotron into operation, which succeeded in the winter of 1941/42. However, Gentner had reached an agreement with Joliot that prevented the accelerator from being used for military purposes [34]. A technician made sure that the cyclotron was always defective when a member of the Uranverein wanted to use it. Because of his 'too-friendly attitude', Gentner was ordered back to Heidelberg after being denunciated, but his successor, Wolfgang Riezler, continued in the same spirit [35].

Another modern cyclotron had been donated to Bohr's institute in Denmark by the Rockefeller Foundation. It would have been ideal to produce and investigate 'element 94', but it was never seized. Plans to bring the device to Berlin were prevented by Heisenberg, who was thus protecting his fatherly friend Bohr and the institute where he had spent a fruitful year in 1924/25. After Bohr's escape to Sweden and then to the UK, the German Wehrmacht occupied his institute. An expert commission led by Heisenberg concluded in early 1942 that the institute was not used for war-relevant purposes and should be given back to the Danish scientists [36].

When, in June 1942, the new armament minister Albert Speer offered "to exercise all his powers in order to build equally large or even bigger cyclotrons as in the USA", Heisenberg declined because of a lack of experience ([13], p. 240), a completely nonsensical answer in view of the work of Bothe's group and, in particular, Gentner's experience from his stay in Berkeley in 1939 and his work in Paris.

In the USA, it was well known that cyclotrons were indispensable for studying 'element 94'. That was why the US Army destroyed the four Japanese cyclotrons as soon as they could in 1945 [37]. It was one of the greatest oddities of the German Uranium Project that there was no cyclotron available throughout most of the war. It would have been logical if the author of the introduction to the 1942 HWA report that raised hopes regarding element 94, most probably Diebner, had pushed the early construction of cyclotrons. Instead, the total absence of any work on element 94 suggests that there was an implicit agreement not to produce any knowledge that might give rise to a political initiative for a larger project. If that was the case, then it was a sacrifice, because it meant a loss of international competitiveness for German nuclear physics.

Peculiarity 5: The German scientists knew that a reactor would produce not only power but, as a by-product, a new fissile and long-living element. A potentially easier way to the bomb had opened up. It would have been logical to start new activities immediately, if only out of sheer curiosity. However, no cyclotrons were built and those available not used.

- *Would the Production of a Plutonium Bomb Have Been Possible?*

From an economic point of view, yes. An approximate cost of a plutonium bomb could be derived from the Manhattan Project. The capacity of reactors and reprocessing plants built in Hanford allowed the production of 36 bombs per year after an investment of 600 million dollars. Together with the budget of Los Alamos, one could estimate a cost of around 20 million dollars for the first bombs, corresponding to 100 million RM in Germany. The building of several bombs, therefore, would have cost only a fraction of the total cost of the rocket project (2 billion RM). Technically, Heisenberg had argued that a plutonium bomb was out of reach because he and his colleagues would first have to learn how to build and operate power reactors. This was not necessarily true, given the experience in the US. Compared to the time needed for these steps in the Manhattan Project, the Germans could have spent two years more for building much smaller facilities. The installations would not have been conspicuous in terms of construction and size. One cannot exclude that a larger project with many scientists and engineers would have allowed the production of a few plutonium bombs before the end of the war ([2], pp. 9–10).

This assessment was shared by the best experts at the time: the Jewish-German physicists who formed the core of the group working on bomb development at Los Alamos. They knew both the problems and the capabilities of their former colleagues who had remained in Germany. They were convinced that they could match them with a smaller programme. Eugene Wigner, in particular, warned that the Germans might have used their time advantage and, after building a 100 MW reactor, could have completed six plutonium bombs before the end of 1942 ([38], p. 412).

The HWA report revealed that there was absolutely no work done on bomb physics or on plutonium. It showed clearly that even the most basic requirement for a bomb, to use fast neutrons, was not yet recognised ([3], p. 20). The ignorance of the German scientists was confirmed by the Farm Hall transcripts. When Heisenberg tried to understand the Hiroshima bomb at Farm Hall, his attempt to calculate the critical mass ended in a disaster, demonstrating that he had never made the calculation before ([29], pp. 129-130). All experts on the physics of the bomb who wrote books or articles about the Uranium Project: Samuel Goudsmit, Hans Bethe, Edward Teller, Rudolf Peierls, Paul Rosbaud, Jeremy Bernstein, Cameron Reed, and Klaas Landsman agree that even the most basic problems of building an atomic bomb were not known in Germany ([2], p. 19).

Peculiarity 6: Given the possibility of an early start, one cannot exclude that a few plutonium bombs could have been ready in time to influence the outcome of the war. However, no work on the physics of a bomb and on plutonium was done.

- *How Did the Prediction of the Generation of Plutonium in a Reactor Change the Programme?*

The answer can best be derived from the HWA report of February 1942. The actual technical report (chapter II) based on 135 secret reports and four patent applications dealt only with uranium and did not even mention element 94 at all. Only an annex referred to the prediction by von Weizsäcker, corrected for the findings of McMillan and Abelson without quoting them. There was not even one reference to a secret report on element 94 more than 1 1/2 years after its prediction. Equally, the brief chapter on a uranium explosive did not refer to even a single secret report.

Without a cyclotron, only element 93 could be analysed in Germany. Kurt Starke from Hahn's institute succeeded in detecting it with the help of the recoil method, which can be used to separate radioactive isotopes [39]. Strangely enough, this single, yet very preliminary, step towards plutonium was cleared for publication in 1942 [40]. Somewhat later, Hahn and Straßmann were allowed to publish their work on the separation of element 93 [41,42].

It is interesting to note that, in the discussions at Farm Hall, after being informed about the Hiroshima bombing, only Heisenberg, von Weizsäcker, and Diebner spoke of '94', while Hahn and many others referred consistently to '93'.

Peculiarity 7: The German scientists did not even try to study element 94. It remained a phantom.

- *Was Uranium a Limiting Factor for the Nuclear Programme?*

Since 1839, limited amounts of radium and uranium had been mined in Germany at the average rate of 1 ton per year [43]. With the annexation of Sudetenland in 1938, the Auer-Gesellschaft, whose Jewish owners had been expropriated started to exploit the mines in Czechoslovakia—in particular, the one in Joachimsthal—and treat the ores at their facilities in Oranienburg, close to Berlin. In the period of 1939–1945, production was on the order of 10 tonnes of uranium per year, and no efforts were made to increase it. During the war, Auer produced 30 tonnes of refined uranium oxides and forwarded enough to Degussa for producing 5 tonnes of uranium metal for the research programme. All uranium used by the Uranium Club came from Czech mines. However, that was far from all the uranium in Germany during the war. Ninety percent came from the Union Minière du Haut Katanga in Belgium after that country had been invaded in May 1940. At the time of the invasion, the company had thousands of tonnes of uranium concentrates containing 844 tonnes of U_3O_8 [44]. Of that, 370 tonnes of U_3O_8 were shipped to Germany during the war. None of that was used for the nuclear research programme. The lack of uranium was an unnecessary hindrance to the experiments.

Peculiarity 8: There was plenty of uranium available in Germany but not enough for the Uranium Club.

- *Was the Proof of a Sustainable Chain Reaction a Difficult Task?*

Based on a number of preliminary studies similar to, yet larger than in Germany, Enrico Fermi set up the experiment in Chicago within two weeks. He engaged students in building a pile using 360 tonnes of graphite and 50 tonnes of uranium that were needed to establish a self-sustaining reaction. Apart from collecting and moving large amounts of material, there were no special difficulties [45]. Such an experiment would have been possible in Germany too.

In the spring of 1940, the capable and eager physical chemist Paul Harteck, successor of the famous Otto Stern in Hamburg who had to leave Germany in 1933, had a brighter idea than Fermi. He decided to use carbon dioxide ice as a moderator, which was not a good choice for a heat-producing reactor but ideal for a zero-power experiment because it was known that, for lower energies, the fission rate of U 235 is inversely proportional to the velocity of the neutrons. With the cold ice, much smaller quantities would suffice for the experiment. Thanks to his good contacts with the chemical industry, he managed to obtain 15 tonnes of CO_2 ice as a loan. That meant that there was no financial problem for the experiment. A recent calculation has shown that, depending on geometries, 12–15 and up to 20 tonnes of uranium oxide would have been sufficient to reach criticality. Harteck urged Diebner and Heisenberg to lend to him all available uranium, but the response was reluctant. They eventually agreed to send only 185 kg ([6], p. 69). The experiment failed. Wirtz noted after the war that Harteck would have most probably demonstrated neutron multiplication and that this would have moved the whole project onto another track during the war. He found it "difficult to understand today why, at that time, the relatively large stocks of uranium that were available, and which, moreover, could have been supplemented by [un]purified and still relatively usable ones were not made available in the quantity desired by Harteck" ([28], p. 37). (Impurities in uranium do not lead to neutron losses as some trace elements in the moderator do.)

Even if there were difficulties in finding enough uranium oxide at this early stage, Harteck's experiment could have been carried out successfully later after better preparation.

Peculiarity 9: The condition for a reactor and explosives development programme formulated by the HWA could have been reached early on, fast, and at almost no cost.

- *Why Did the Germans Decide against Carbon as a Moderator?*

As Heisenberg found out, only two substances can be used as a moderator for a reactor with natural uranium: clean carbon (in the form of graphite) and heavy water. Fermi used graphite because it was an established industrial product. Heavy water, which must be produced by an isotope separation process, replaced carbon later, until the enrichment of U 235 allowed for the use of plain water as a moderator in much more compact reactors. However, in Germany, shortly after the failure of Harteck's experiment, the HWA decided to use heavy water instead of carbon in the reactor experiments. There is no document mentioning this important decision. Referring to publications of Heisenberg, Bothe, and Höcker, the HWA report simply stated that carbon "was found not suitable" ([3], p. 21), although they do not justify such a clear statement. In the detailed technical chapter, the report added "Since it is practically impossible to produce coal with a higher degree of purity than the one used, it is unlikely to be considered as a braking substance" ([3], p. 88). Contradicting this statement was a 1947 exchange of letters between Heisenberg and the Siemens Company, which stated that the production of even purer graphite would not have been a problem; so far, the purity had been sufficient for all applications [46], ([28], p. 35). In his report about the Uranium Project in 1946 before the international science community, Heisenberg stated cautiously "Whether the reason [. . .] was the incomplete attention to chemical impurities [. . .] or deficits of the theory, is difficult to decide for the time being" [47]. The reactor expert Lothar Koester analysed the decision in 1980 in detail and concluded that, even with Bothe's unexpectedly high value for neutron absorption, the experiment could have been done with a larger amount of graphite and that the German scientists had the knowledge to draw this conclusion [48].

The HWA relied on Norway, occupied since April 1942, for its import of heavy water. However, given the small capacity in Norway and the danger of sabotage, the HWA paid a high price in the form of a two-year delay of the programme. Harteck's experiment was not repeated with larger amounts of uranium, although it was an opportunity to check the suitability of carbon at almost no cost. Harteck opposed the decision taken: "People said that carbon would not work, but I really didn't believe them" ([5], p. 103). Instead, he was persuaded by Diebner to begin working on uranium enrichment.

Peculiarity 10: The decision against carbon and in favour of heavy water was not scientifically compelling and resulted in a two-year delay in the ability to demonstrate a chain reaction. The justification for this decision was contradictory and implausible. It prevented a repetition of Harteck's experiment with more uranium that would have demonstrated neutron multiplication very early.

- *What Was the Consequence of the Decision against Carbon as a Moderator for the Reactor Experiments?*

Altogether, 22 experiments were performed during the years 1940–1945. Ten tests were made at the KWI-P in Berlin and the last one at Haigerloch after the evacuation from Berlin. The others were at Leipzig (four), Hamburg (one), and the Gottow facilities of the HWA (six) ([2], p.10).

As long as no heavy water was available, and because carbon was excluded, the German physicists used paraffin as a moderator, as Hahn had done in his famous fission experiment. It was clear that a self-sustaining chain reaction was unattainable. Because the absorption of neutrons was stronger than the production by fission processes, the neutron balance was negative. However, it was nevertheless sensitive to changes in the reactor design.

The task of the reactor designer is to select the spatial distribution of uranium and moderator in such a way that the neutrons produced by fission enter the moderator as quickly as possible and only return into the uranium to cause new fissions after they have slowed down. Fermi first used cylinders and, later, in power reactors, long rods of uranium. Heisenberg opted for plate stacks, and Diebner devised a system of cube chains. Fermi's solution was the best. Diebner's solution was less good but still better than Heisenberg's, who, nevertheless, held on to his plates for a long time—not out of stubbornness, as often assumed, but because, with plates, calculations were easier. For Heisenberg, an experiment was only of interest as an answer to a theory. Stepwise improving his design, Heisenberg could halve the negative neutron balance twice. Also, the HWA experiments made good progress. Thus, the subcritical experiments served very well for the improvement of the reactor designs.

The proclaimed main objective of the programme, the demonstration of a chain reaction, had to wait for sufficient deliveries of heavy water. As a consequence of the decision for a liquid moderator, there was a need for a vessel made from material with low neutron absorption. A sufficiently large vessel could have been ordered and produced while waiting for the heavy water delivery. The HWA could have easily used its powers, proclaiming an issue of national importance, to take all incoming heavy water for a central experiment. It had done that before. "If the entire supplies of uranium and heavy water had been combined into a larger experiment, it would have been possible to build a critical multiplying arrangement", Wirtz noted ([28], p. 49). Such a joint experiment was never even discussed.

Instead, the incoming heavy water was again distributed among the three parallel lines of experiments. Wirtz admitted after the war that, in reality, they were far from achieving a self-supporting machine. "In Germany, only subcritical assemblies have been experimented with" ([20], p. 3). "If the critical size had been exceeded, a number of new problems would have arisen, such as automatic burner control, heat removal, corrosion protection of the uranium, treatment of the contaminated heavy water, radiation protection, etc., the solution of which was partly in preparation and which would have required a considerable expansion of the technical apparatus. This could not be done" ([20], p. 7). This was confirmed by the construction of the experiments, which were not equipped with control rods and had no provisions for heat removal and radiation protection.

In Leipzig, between March and May 1942, Döpel and Heisenberg observed an increase of 1% of the neutron flux from induced fissions for the first time. At the same time, similar experiments in the US achieved higher multiplication rates; the German scientists had lost their important advantage of time.

From a scientific point, the 1% increase observed in the Leipzig experiment was already sufficient proof of the possibility of a chain reaction. In their report about the successful experiment, Döpel and Heisenberg stated clearly "The simple enlargement of the layer arrangement described here would [. . .] result in a uranium burner from which energy [...] can be extracted" [49]. However, this breakthrough had no consequences for the programme. In retrospect, Wirtz was surprised that the scientific assessment of the importance of the experimental result was not communicated in Germany ([20], p. 5).

After the set-up in Leipzig was destroyed by a fire in June 1942, Heisenberg and Diebner continued their parallel experiments but with increasingly better cooperation. Heisenberg's colleague Höcker even found a way to calculate Diebner's cube arrangement by simplifying it as spheres. The design of the experiments showed that the German scientists did not intend to go beyond subcritical assemblies. That was intelligent, because the experiments gave the same answers but avoided the problem of controlling the reaction and tuning it to a low power in order to avoid the build-up of dangerous radiation.

This statement was confirmed by the design of the experiments. Even those with heavy water were not equipped with control rods and would have been very difficult to handle after becoming radioactive [50]. The experimenter would not have survived if criticality had been reached. At Haigerloch, Heisenberg improved the set-up by adding a carbon reflector, but he would still have needed more heavy water, more uranium fuel, and more time to build a larger reactor [51].

Most previous authors have seen the German physicists as having failed because they did not achieve a chain reaction, but this is a mistake. In fact, Heisenberg and Diebner simply skipped the unnecessary condition and started reactor development as early as 1940, using subcritical experiments to improve the reactor design. Especially in the first phase of reactor development, it is possible to make the same progress with subcritical arrangements as with critical ones but without the effort required to control the reactor and without a build-up of radioactivity. What was long considered a futile attempt to achieve a chain reaction was actually an intelligent and effective way of learning how to achieve the same neutron balance with less material. That was confirmed in a report by the reactor physicists Alwin M. Weinberg and Lothar W. Nordheim, who participated in the Manhattan Project. Their often-cited conclusion "Generally we could say their approach was in no way inferior to ours, in some respect it was superior" [52] was balsam not only for the souls of the German scientists but also for those of their American colleagues, who liked the idea that they had indeed a capable competitor in the presumed race for the bomb. In the enthusiasm over the praise, however, it was overlooked that it referred exclusively to some aspects of reactor physics; there was no mention of the bomb, the state of enrichment processes, or the level of nuclear physics knowledge. It would not be justified to dismiss Weinberg's and Nordheim's report as a whitewash in their own interests. Both were serious scientists who later enjoyed great renown, but they seem to have missed several severe deficiencies. Neither Heisenberg's plates nor Diebner's cubes were adequate solutions to the geometry of a reactor (Heisenberg's plates were easy to calculate but not ideal. Because of the lateral extension, too many neutrons remained in the uranium. Diebner's cubes were slightly better but mounted too narrowly. They should have had a distance of 11 cm, which allows neutrons to travel until cooled down to room temperature (see ref. [52])). The reasons for the poor results of the Uranium Project were the 'under-critical' workforce, the budgetary limitations, the long waiting time for the manufacture of components, and the weak technical infrastructure but, above all, the missing will to succeed.

Peculiarity 11: After Harteck's thwarted attempt to demonstrate a self-sustaining chain reaction, neither Heisenberg nor Diebner ever tried to do it but achieved reasonably good progress in reactor design to the extent possible within the limited resources without any effort to reach the proclaimed goal.

- *Was Werner Heisenberg the Central Person in the Programme?*

Nobel Prize winner Heisenberg was the most prominent German physicist (and the one the American scientists thought of, first and foremost), certainly after a quarter of all

German physics professors had to leave in 1933. His role in the programme was ambiguous. Basically, he was a theoretical physicist who provided the theoretical basis for the reactor experiments, but he was not interested in reactor physics. While the modelling of the processes in a reactor is still a field of scientific improvement even today, he seemed to have never cared to improve his reactor theory. Otherwise, he would have detected his rather obvious mistakes. He supervised several reactor efforts in Leipzig and Berlin, but the work was primarily done by others. Moreover, he travelled extensively, giving lectures in various countries.

Heisenberg's publications during World War II revealed his real scientific interests: a series of three long papers on the observables for a theory of elementary particles appeared in 'Zeitschrift für Physik' in 1943 and 1944 [53]. In 1941 and 1942, he organised a colloquium on cosmic rays and published its proceedings in a book in 1943 [54]. In the Soviet Union, these activities were seen as an indication that Germany was not working on the atomic bomb [55].

An interesting detail allows us to shed light on Heisenberg's ambivalent role in the Uranium Club. His paper of December 1939 contained two errors. This is not surprising, as even a genius cannot get everything right when entering new territory. It is just strange that he did not notice them in the following years. The first error was his misconception of a bomb as an exploding reactor. The second mistake was his prediction that a reactor would automatically stabilise at a temperature that depended only on the degree of enrichment of the U 235 because heating leads to increased absorption of the decelerated neutrons in the uranium. This would be true if the neutrons would still be in the uranium during cooling, but they should be in the moderator [50]. Moreover, a reactor cannot stabilise itself, because it needs a surplus of uranium in order to replace the fissioned U 235 nuclei. The influence of that surplus on reactivity must be compensated by control rods. Heisenberg's mistake, which would have had life-threatening consequences in the case of success, remained undetected until the end of the war. Apparently, it was Heisenberg's nimbus that prevented his colleagues from questioning his predictions and his own disinterest in the matter to do it himself.

Formally, Heisenberg had no special role in the programme, but informally, he was the intellectual spokesman, even though the decisions were made elsewhere. In all official meetings, he introduced the subject, often together with Hahn, and most of the questions were addressed to him. Together with his trusted co-workers, he had the monopoly on talking about a possible explosive. He had convinced himself early on that developing a bomb would be too large a challenge to be mastered during the war, and he defended his judgment against upcoming new developments like the prediction of plutonium.

His repeated statements that he wanted to avoid a large military programme were confirmed by Paul Rosbaud, the well-connected scientific editor of the journal *Die Naturwissenschaften* during the war. Just like his brother Hans, who became a famous conductor, Rosbaud lived unchallenged in Germany thanks to a forged 'Aryan certificate'. In 1945, he told the ALSOS mission, the US intelligence effort that investigated at the end of the war how much progress German scientists had made, that Heisenberg belonged to a group of German scientists who, at the beginning of the war, wrote to a few colleague scientists abroad that they would restrict themselves to basic research ([10], p. 115). (One of the recipients was Delft Professor W. G. Burgers. In what remains of his archive, the letter was not to be found. To our knowledge, he has never publicly spoken on the issue.) Heisenberg confirmed it after the war: "We had to avoid being committed to making a big effort towards an atomic bomb. What we wanted was to [...] get on with our reactor project, but no more than that. We were very much afraid that otherwise someone would say, "Now let's go for the atomic bomb" ([5], p.33).

Peculiarity 12: The intellectual leader of the Uranium Project, Heisenberg, had no scientific interest in the project and spent most of his time on other subjects.

Part 2: Evaluation of the Peculiarities and Conclusions

We have observed that the Uranium Project was characterised by an amazingly large number of peculiarities: a wartime programme without relevance for the war, poorly coordinated for the most important first phase, led by a military authority although of an academic nature, with a proclaimed goal that was not pursued, with half-hearted approaches and suppressed curiosity, not equipped with modern standard experimental facilities, with dubious decisions and contradictory explanations, headed by a top scientist who was not interested in the project, and with understandable beginner's mistakes that remained undiscovered for years. The Uranium Project was, in many aspects, the opposite of what one would expect from German efficiency.

Why was the programme conducted at all during the war when it soon turned out that there was no prospect of a result that could be used for warfare? Aware of the enormous potential of the newly discovered energy source, but also of the many potential obstacles along the way and the time it might take, and without any political guidance, officials found a compromise by carrying out a modest programme of exploring the possibilities and the problems to be solved despite the war, just to be on the safe side.

For the question of what was done and what was not done, we can firmly conclude that no work on a nuclear weapon, not even voluntarily out of sheer curiosity, was done in Germany and that there is no proof whatsoever that any of the researchers had a notion of how such a weapon would work. They shied away from that. The number of scientists involved in the Uranium Project was very small. Without technical and industrial support, they could only achieve limited progress. The focus of the work was on the first steps of building a reactor and the procurement and minimisation of the materials needed. They knew that a by-product of a power reactor would create a powerful explosive that might be easier to get than enriched uranium, but they never tried to produce even micrograms of that substance. They did not have one of the most important research tools for nuclear physics, a cyclotron. For the German scientists, element 94 remained a phantom. That is an important fact in view of the very small but not totally impossible outcome that an ambitious nuclear project with an early start might have led to the construction of a few bombs before the end of the war.

Moreover, we conclude that, to varying degrees, the scientists involved seemed not eager to make progress and demonstrated a definite lack of ambition or chose a deliberate step-by-step approach. Although there was plenty of uranium available at least after May 1940, it was not made available in sufficient amounts for the experiments. The dubious decision to use heavy water from occupied Norway instead of clean carbon as a moderator deprived the Uranium Project of its most important advantage against the Manhattan Project, its early start, and also led to shortages, because there was no heavy water produced in Germany for economic reasons. That, in combination with the war conditions and the lack of political attention in Nazi circles, explains, to a large degree, the course of events in this area in Germany during the Second World War. Nazi hubris too, and lack of interest in 'Jewish' science like quantum mechanics, which included nuclear physics, are part of the explanation.

These deficiencies alone cannot explain the large number of peculiarities and their significance. And we have to realise that all the peculiarities had the same effect: in one way or another, they led to a delay in the programme. Opportunities to achieve the proclaimed goal remained unused, and the necessary facilities to take the easier route to the bomb via plutonium were not created; even curiosity about it was curbed. The fact that this happened so uniformly and consistently cannot be a coincidence. There must have been a strategy.

Heisenberg not only often said that he wanted to avoid a bomb development programme. All his deeds and omissions, the plausible and the enigmatic, helped to secure this goal. Personally, he was only interested in fundamental research, and he had convinced himself that a bomb programme would require such an intense effort over several years that Germany would have lost the war sooner. Protecting Germany from a useless strain on manpower and the economy was no sabotage. He even stated that the Americans "would have won the war against Germany earlier if [they] had made no atomic bombs [. . .] because then [they] would have put this whole effort into aeroplanes and tanks and whatever else, and the war would have been ended earlier”. When asked about “some resentments among the scientists in Chicago because they didn’t control the project anymore after they had proved the principle”, Heisenberg responded “That was also one of the main points we discussed especially in a small group including Weizsäcker, Wirtz and myself. We felt it very important that these things must remain in our hands, then we could always keep control of what goes on. That, we did achieve” ([5], pp. 33–35). Heisenberg also admitted after the war that he opposed the “big experiment” planned by Harteck in the spring of 1940 with “twenty tons of dirty uranium oxide”, revealing that he knew perfectly well what Harteck had needed to succeed. He disliked that Harteck would “do the whole project at once” ([5], pp. 29–30), provoking a much larger bomb development project. The author of the famous play Copenhagen, Michael Frayn, found an apt formulation for Heisenberg’s strategy that explained all his deeds and omissions: “If he had withheld the fatal knowledge [...], it was from himself” [56].

Other members of the Uranium Club may have had different opinions, but the dispersed research locations, the ban on contact, the lack of opportunities for discussion, the intimidation of the scientists by the totalitarian regime, and the mistrust among each other did not allow for a discussion on the path to be taken. And there was another reason, which also made many other scientists exercise restraint: they had to fear being held accountable for missing unrealistically ambitious goals with insufficient means. This was also expressed by Esau, who headed the project before and after Diebner. He warned the scientists not to talk too much about a possible bomb if they did not want to spend the next two years behind barbed wire. “If two years later there is still no bomb, you are lost. Is that what you want?” ([6], p. 147) As a result, no one from the Uranverein contradicted Heisenberg’s strategy during the war.

What was the position of the HWA in light of these peculiarities? To some extent, the HWA must have shared Heisenberg’s goal, despite all the differences in their tasks and motivations—namely, not to create the conditions for a political decision to start an ambitious bomb development programme by foregoing successes that would have been visible even to laymen.

Wirtz attributed the peculiarities to the weakness of the persons in charge and to rivalries, especially between Heisenberg and Diebner. As a reason for blocking Harteck’s experiment, Wirtz considered it “a possibility” that Diebner “wanted to see these experiments done in his own field” ([28], p. 147 and [10], p. 324).

However, these explanations do not sufficiently explain the oddities. For one thing, the HWA was recognised as successful in other areas. Leeb emphasised that it made high demands on its employees. Mistakes and weaknesses were not tolerated. For another, the explanation fails to recognise the sophisticated functioning of German public administration. A civil servant of Diebner’s level could act within a well-defined and limited responsibility and had to obey instructions. Important or unusual matters must be discussed with superiors, especially with the head of a department who attended the regular meetings with the minister or, in this case, the head of the HWA. To the members of the Uranium Club, Diebner seemed to act alone, but he was integrated into a strict hierarchy and had little leeway for his own actions. Preventing a project that might lead to an early breakthrough out of selfishness carried a high risk of being dismissed or accused of sabotage. Whatever Diebner announced was a decision of the HWA.

A third contradiction to this explanation is the fact that at no time was there a joint experiment planned that, according to Wirtz, would have led to preparing a vessel that could have taken all the heavy water that had arrived by mid-1942. This would have been unkind to Heisenberg but still lenient compared to the HWA’s behaviour towards the Göttingen nuclear physicists. Diebner would also have secured an entry into the eternal annals of physics in place of Fermi for being the first to bring about a nuclear chain reaction. And we know that he must have seen it that way, because the Farm Hall transcripts show that the German physicists firmly believed they were far ahead of the rest of the world until the news of the atomic bomb.

For these reasons, Wirtz's explanation cannot be a convincing explanation for the strange decisions. We must extend the question to whether the HWA also wanted to avoid a bomb programme.

What do we know about the intentions of the HWA? In April 1940, Leeb took over the leadership after the dramatic suicide of his predecessor. While Becker was a man of research who himself chaired the RFR, Leeb presented himself in his memoirs as a professional soldier who, even after the war, focused on the amateurish interference in matters of strategy and armaments of the National Socialist leaders rather than on their crimes. The fact that he did not think any differently after the war is testimony to the steadfastness of his statements.

From an HWA perspective, the Uranium Project was of limited importance. In Becker's biography, it is not mentioned at all [57], while Leeb devoted only a few lines to it, concluding that an atomic bomb would not become available during the war ([19], p. 23). Consequently, a bomb programme would be a waste of scarce resources that would lead to a neglect of other, more urgent innovations. A good reason for hesitation.

In his report, Leeb pointed to hollow charges as a particularly successful work of the research department. The German scientists never discovered that mastering this technology would have been an important advantage in building plutonium bombs. In view of Becker's fate, it is not surprising that the problem of external influence on the work of the HWA became the defining element in Leeb's memoirs. He described the ideal process of decision-making as a dialogue between technicians and soldiers, whereby the latter would have to tip the balance in the end. However, he saw with concern that this principle was at risk. "At the High Command of the Wehrmacht, growing distrust, growing direct influence on technical matters is becoming apparent. The 'revolutionary' element was increasingly brought into the cooperation. [. . .] The expert [is] suspected, dilettantism is raised on the shield. The influence of the Party side is constantly growing, the soldier must fight more than ever against the encroachments of the Party" ([19], p. 11).

Therefore, it would be reasonable to assume that Leeb was afraid of possible external influences that might have forced the HWA to start a bomb development programme with a negative effect on more important work of the HWA. He therefore readily agreed with Erich Schumann's proposal in December 1941 to hand back the coordination of the Uranium Project to the Research Council. If one accepts that the HWA wanted to avoid a bomb development programme, then the many peculiarities of the HWA's behaviour take on a new meaning. Specifically, the demonstration of a chain reaction was not a necessary goal but was seen by laymen as a prerequisite for the expansion of the programme. If indeed the setting of this goal defended the HWA against political influences, it was phenomenally successful. It took 75 years to recognise it.

Our new interpretation offers a new reasoning for the 1939 seizing of the project by the HWA and dropping it in 1942. That the HWA let the RFR start the programme and seized it only three months later, on the eve of the war, may have had the rationale that a military authority would be better suited to solving the most important problem of providing the necessary materials. That issue was not yet fully resolved when the HWA withdrew in 1942, although the military continued their own activities and handed over the coordination in an orderly manner to the RFR, which continued it unchanged. In view of these inconsistencies, one cannot rule out the possibility that the temporary responsibility of the HWA had different or additional reasons. At the time of entry, an atomic bomb appeared to be quite possible on the basis of Flügge's claims; the HWA might have preferred to decide whether or not to start a bomb development programme itself, an important decision that should not be left to a group of military amateurs. After Heisenberg's verdict of the unattainability of the uranium bomb in December 1939, given the difficulties of enriching uranium, they might have continued in order to avoid that an expensive, yet hopeless, project would be started. That could also have been the reason for blocking any work on element 94. In spring 1942, when it was too late to start a large programme, the HWA gave the programme back to the RFR, as it was seen as atypical and irrelevant to the war effort. This new interpretation cannot be proven, because no internal documents of the HWA have been preserved, but it is the only reason that can explain the development of the Uranium Project without contradictions.

According to the common assessment, the Uranium Project was a failure due to the expulsion of Jewish scientists, the National Socialists' lack of appreciation for science, fighting against modern physics with 'German physics', weak coordination by overtaxed functionaries, poor cooperation, and a lack of trust among the scientists due to political repercussions, as well as the wartime economy of scarcity. Although all these factors were important, the study of the peculiarities has shown that they are not sufficient explanations. The peculiarities reveal that the programme was by no means a complete failure. The declared goal, the demonstration of a chain reaction, was not missed but intentionally delayed in order to not present a visible success. The first steps into reactor design and technology were made easier, safer, and cheaper with subcritical arrangements. Nordheim's and Weinberg's praise showed that the German scientists were quite successful within their limitations.

The new interpretation derived from the investigation of the many peculiarities of the programme, if we accept it, gives a surprising but inherently logical answer. The Uranium Project was a modest effort during the war but without any prospect to contribute to warfare, performed just to not miss the boat. The German scientists achieved successes within the framework of their very modest possibilities, especially in the reactor experiments, by cleverly limiting themselves to subcritical experiments. The absence of work on the bomb was reasonable based on the assessment that its realisation during the war did not seem promising. Heisenberg convinced the HWA that a bomb development would be out of reach during the war and that one should not awaken political interest in a bomb by writing progress reports. For this reason, he and his colleagues also avoided any investigation into the 'open road to the bomb' that the discovery of the creation of a new fissile material in a reactor opened.

In physics, a theory is considered valid if it explains all observed phenomena. This principle cannot be fully applied to historical problems, because the reactions of human beings do not have to be logical or reproducible. Hence, we consider our interpretation as an offer to look at the Uranium Project in a new light.

The most important reason that the nuclear programme, of all things, became one of the few harmless chapters of the Third Reich is that the National Socialist leadership did not recognise and seize the opportunity to become the first nuclear power on earth. Had Hitler demanded a project to develop the bomb, the Uranium Project would have been different in character and size, and perhaps history would have taken a different course. It is a comforting thought that the narrowness and complacency of the Nazi leaders contributed to their downfall. To a certain extent, the peculiarities of the project were a result of the weaknesses of the regime.

After the war, none of the German scientists boasted of having prevented Hitler's atomic bomb or avoided working on it for ethical reasons. When Heisenberg repeated in 1968 that he "did not want to get into this bomb business", he added explicitly "I wouldn't like to idealize this; we did it also for our personal safety" ([5], p. 34). He often underlined that not building atomic bombs in Germany was a very sensible decision, because the necessary effort would have been useless and would have weakened Germany's military strength. Were the physicists spared the moral decision?

Not necessarily. The assessment that the development of an atom bomb was not promising did, in no way, preclude its being ordered—Hitler's decisions were rarely rational. They could have been forced, but in a completely new field of science, as in the case of nuclear physics, it is not easy to force scientists to make discoveries they do not want to make.

If we look at the entire development of science in the Third Reich, we see that the physicists did have a choice. Research into the history of the Kaiser Wilhelm Institutes under National Socialism has revealed shocking examples of the suspension of normal ethical standards, for example, in inhumane medical experiments. The nuclear physicists did not succumb to such temptation. They did not try to exploit the National Socialists' lack of moral restraint. None of the members of the Uranium Club advocated for starting a bomb project, nor did any of them attempt to gain favour with the regime or to attain a position of importance.

That they did not talk about their refusal to work on the bomb after the war has good reasons. They had to be concerned that it would be interpreted as treason in post-war Germany. At Farm Hall, Gerlach warned the detained scientists "When we get back to Germany we [. . .] will be looked upon as the ones who have sabotaged everything. We won't remain alive long there" ([29], p.125). Gerlach's warning was not unfounded. In the decades after the war, the question to what extent its end was more a liberation than a defeat was still a sensitive topic

The German scientists' refusal to work on the bomb was sufficiently justifiable with the insight that such an expensive development for Germany during the war was futile and that the possible failure of an overambitious project could become dangerous for themselves. However, one cannot rule out that responsible scientists also had ethical motives for their inactions behind their protective claims.

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References

1. Popp, M. Misinterpreted Documents and Ignored Physical Facts: The History of 'Hitler's Atomic Bomb' needs to be corrected. *Berichte Wiss.* **2016**, *39*, 265–282. [CrossRef] [PubMed]
2. Popp, M. Why Hitler did not have Atomic Bombs. *J. Nucl. Eng.* **2021**, *2*, 9–27. [CrossRef]
3. Heereswaffenamt. *Energiegewinnung aus Uran, Ergebnisse der vom Heereswaffenamt Veranlaßten Forschungsarbeiten zur Nutzbarmachung von Atomkernenergien, Herausgegeben Anlässlich der Zweiten Tagung der Arbeitsgemeinschaft vom 26. Bis 28. Februar 1942 in Berlin*; AMPG-I-34-105; Archiv der MPG: Berlin, Germany, 1942.
4. Archiv der Max-Planck-Gesellschaft (AMPG). Übersichten zum Deutschen Uranprojekt—Kernenergieforschung in Deutschland von 1939 bis 1945. Available online: https://www.archiv-berlin.mpg.de/83592/uranprojekt_uebersichten.pdf (accessed on 24 April 2023).
5. Ermenc, J. *Atomic Bomb Scientists: Memoirs*; Meckler: London, UK, 1989.
6. Irving, D. *The Virus House*; William Kimber & Co.: London, UK, 1967.
7. Flügge, S. Kann der Energieinhalt der Atomkerne technisch nutzbar gemacht werden? *Naturwiss* **1939**, *27*, 402–410. [CrossRef]
8. Sime, R. *Lise Meitner—A Life in Physics*; California Press: Berkeley, CA, USA, 1997; p. 277, (which contains Lise Meitner's letter to Otto Hahn of 12 July 1939).
9. Hentschel, K.; Hentschel, A. (Eds.) *Physics and National Socialism—An Anthology of Primary Sources*; Birkhäuser: Basel, Switzerland, 1996; pp. 197–206, (which reproduces Flügge, S. Die Ausnutzung der Atomenergie, Deutsche Allgemeine Zeitung, 1939, 327, August 15, Supplement).
10. Powers, T. *Heisenberg's War*; Alfred A. Knopf: New York, NY, USA, 1993; p. VII.
11. List of Personnel of the KWI for Physics (15 June 1941), AMPG I.Abt. Rep. 34—Nr. 173-3. 15 June.

12. Bagge, E.; Diebner, K.; Jay, K. *Von der Uranspaltung bis Calder Hall*; Rowohlt: Berlin, Germany, 1957; p. 28.
13. Speer, A. *Erinnerungen*; Propyläen: Berlin, Germany, 1969; p. 240.
14. Heisenberg, W. *Die Möglichkeit der Technischen Energiegewinnung aus der Uranspaltung*; Deutsches Museum Archiv, FA 002-461; Springer: Berlin/Heidelberg, Germany, 1939.
15. Heisenberg, W. *Bericht über die Möglichkeit der Technischen Energiegewinnung aus der Uranspaltung*; Deutsches Museum Archiv, FA 002-474; Springer: Berlin/Heidelberg, Germany, 1940.
16. Heisenberg, W. *Der Teil und das Ganze*; Piper: München, Germany, 1969; p. 137.
17. Müller, P.O. *Eine Bedingung für die Verwendbarkeit von Uran als Sprengstoff*; Deutsches Museum Archiv, FA002-482; Deutsches Museum: München, Germany, 1940.
18. Kernbauer, A.; Popp, M. *Paul Otto Müller—Schrödingers Talentierter Schüler*; Publikationen aus dem Archiv der Universität Graz: Graz, Austria, 2020.
19. Leeb, E. Aus der Rüstung des Dritten Reiches. In *Das Heereswaffenamt 1938–1945, Wehrtechnische Monatshefte*; Mittler: Berlin, Germany, 1958; p. 23.
20. Wirtz, K. *Über die Deutschen Arbeiten zur Konstruktion eines Uranbrenners*, Badisches Generallandesarchiv Karlsruhe, 69 KfK-INR 325. 1947; 5.
21. Bothe, W.; Flügge, S. (Eds.) *FIAT Review of German Science 1939–1945, Nuclear Physics and Cosmic Rays, Part 1 and 2*; Office of Military Government for Germany: Wiesbaden, Germany, 1948.
22. Schaaf, M. (Ed.) *Heisenberg, Hitler und die Bombe*; Diepholz: Berlin, Germany, 2001; p. 37, (Schaaf, M. Heisenberg war an der Bombe nicht interessiert—Ein Gespräch mit Willibald Jentschke).
23. Bastian, T. *Forschung unterm Hakenkreuz*; Militzke: Leipzig, Germany, 2005.
24. Nagel, G. *Wissenschaft für den Krieg*; Steiner: Stuttgart, Germany, 2012.
25. Available online: https://de.wikipedia.org/wiki/Heereswaffenamt (accessed on 14 February 2023).
26. Available online: https://de.wikipedia.org/wiki/Erwin_Rommel#Suizid_und_Staatsbegräbnis (accessed on 13 February 2023).
27. Popp, M. *Werner Heisenberg und das Deutsche Uranprojekt im Dritten Reich, Quanten 6*; Hirzel: Stuttgart, Germany, 2018; pp. 9–67.
28. Wirtz, K. *Im Umkreis der Physik*; Kernforschungszentrum: Karlsruhe, Germany, 1988.
29. Bernstein, J. *Hitler's Uranium Club*; Copernicus: New York, USA, 2001.
30. Von Weizsäcker, C.-F. Eine Möglichkeit der Energiegewinnung aus U 238. AMPG I-34-120. 1940.
31. McMillan, E.; Abelson, P.H. Radioactive Element 93. *Phys.Rev.* **1940**, *57*, 1185–1186. [CrossRef]
32. Max-Planck-Institut für Kernphysik. *Von Kernphysik und Kosmochemie zu Quantendynamik und Astroteilchenphysik*; MPI Kernphysik: Heidelberg, Germany, 2008; p. 2.
33. *Collection of Documents on the Planned Leipzig Cyclotron*, AMPG I-34-80.
34. Goldsmith, M. *Frédéric Joliot-Curie*; Lawrence and Wishart: London, UK, 1976; p. 100.
35. Dahl, P.F. *Heavy Water and the Wartime Race for Nuclear Energy*; Institute of Physics: Bristol, UK, 1999; pp. 144–145.
36. Schwarz, S. The Occupation of Niels Bohr's Institute: 6 December 1943–3 February 1944. *Phys. Perspect.* **2021**, *23*, 49–82. [CrossRef]
37. Low, M.F. Accelerators and politics in postwar Japan. *Hist. Stud. Phys. Biol. Sci.* **2006**, *36*, 275–296. [CrossRef]
38. Rhodes, R. *The Making of the Atomic Bomb*; Simon & Schuster: New York, NY, USA, 1986; p. 412.
39. Starke, K. Abtrennung des Elements 93. Deutsches Museum Archiv, FA002-555.
40. Starke, K. Abtrennung des Elements 93. *Naturwiss* **1942**, *30*, 107. [CrossRef]
41. Hahn, O.; Straßmann, F. *Zur Frage Nach der Entstehung des 2,3-Tage Isotops des Elements 93 aus Uran; Deutsches Museum Archiv, FA002-735*; Deutsches Museum: München, Germany; pp. 81–93.
42. Hahn, O.; Straßmann, F. Über die Isolierung und einige Eigenschaften des Elements 93. *Naturwiss* **1942**, *30*, 256–260.
43. Available online: https://de.wikipedia.org/wiki/Uranbergbau#Deutschland (accessed on 12 November 2022).
44. Brion, R.; Moreau, J.L. *De La Mine a Mars*; Editions Lannoo: Tielt, Belgium, 2006; p. 227.
45. Available online: https://en.wikipedia.org/wiki/Chicago_Pile-1 (accessed on 9 March 2023).
46. Müller, W.D. *Die Geschichte der Kernenergie in der Bundesrepublik Deutschland*; Schäffer: Stuttgart, Germany, 1990; p. 569, (letter by Erich Höhne to Werner Heisenberg, 9 September 1947).
47. Heisenberg, W. Über die Arbeiten zur technischen Ausnutzung der Atomkernenergie in Deutschland. *Naturwiss* **1946**, *11*, 325–329. [CrossRef]
48. Koester, l. Zum unvollendeten ersten deutschen Kernreaktor. *Naturwiss* **1980**, *12*, 573–575. [CrossRef]
49. Döpel, R.; Döpel, K.; Heisenberg, W. *Der Experimentelle Nachweis einer Neutronenvermehrung in eine Kugel-Schichten-System aus D2O und Uranmetall*; Deutsches Museum Archiv, FAA 002-588; Deutsches Museum: München, Germany; p. 13.
50. Reed, B.C. An inter-country comparison of nuclear pile development during World War II. *Eur. Phys. J.* **2021**, *46*, 15. [CrossRef]
51. Grasso, G.; Oppici, C.; Rocchi, F.; Sumini, M. A Neutronics Study of the 1945 Haigerloch B-VIII Nuclear Reactor. *Phys. Perspect.* **2009**, *11*, 318–335. [CrossRef]
52. Weinberg, A.M.; Nordheim, L.W. *Note to A. H. Compton, 8 November 1945*; Deutsches Museum Archiv, FA002-765; Deutsches Museum: München, Germany; p. 2.
53. Heisenberg, W. Die beobachtbaren Größen in der Theorie der Elementarteilchen, part 1. *Z. Physik* **1943**, *120*, 513–538, Part 2, *Z. Physik* **1943**, *120*, 673–702; Part 3, *Z. Physik* **1944**, *123*, 93–112. [CrossRef]
54. Heisenberg, W. (Ed.) *Vorträge über Kosmische Strahlung*; Springer: Berlin, Germany, 1943.

55. Kleint, C.; Rechenberg, H.; Wiemers, G. (Eds.) Werner Heisenberg 1901–1976. Beiträge, Berichte, Briefe. Festschrift zu Seinem 100. Geburtstag. (the contribution von Weizsäcker, C.F. Gespräche mit Feinberg in Moskau—März 1987); Sächsische Akademie der Wissenschaften: Leipzig, Germany, 2005; p. 179.
56. Frayn, M. *Kopenhagen*; Wallstein: Göttingen, Germany, 2001; p. 123.
57. Vom Bruch, R.; Kaderas, B. (Eds.) *Wissenschaften und Wissenschaftspolitik*; Steiner: Stuttgart, Germany, 2002; pp. 263–281, (the chapter by Ciesla, B. Ein Meister deutscher Waffentechnik. General-Professor Karl Becker zwischen Militär und Wissenschaft (1918–1940).