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## Chapter 18

# FROM URANIUM PUZZLE TO HIROSHIMA

### The Road to Fission

As soon as the neutron was discovered, physicists realized that the new particle, owing to its lack of electrical charge, might be used as an effective projectile in nuclear reactions. The earliest reported nuclear transmutations of 1932–34 made use of fast neutrons impinging on light target nuclei such as aluminium. The results were $(n, \alpha)$, $(n, p)$ and $(n, \gamma)$ processes, that is, the expulsion of either alpha particles, protons, or gamma radiation. At that time, Fermi and his group in Rome began a systematic study of neutron reactions with all the elements of the periodic system, from hydrogen onward. For a neutron source, they used a sealed glass tube containing beryllium powder and radon. In the course of this work, the Italian scientists discovered—purely accidentally—that neutrons that had passed through paraffin, wood, or water were much more effective in producing radioactive isotopes. They concluded that the neutrons had been slowed down by collisions with hydrogen nuclei. Further experiments confirmed that slow neutrons were more easily captured than fast neutrons. When the Italians bombarded uranium with slow neutrons, they were able to identify several beta-emitting products, one of them with a half-life of 13 minutes. Fermi, Franco Rasetti, and Oscar D'Agostino found that the activity could not be due to isotopes between uranium and lead, and that this negative evidence "suggests the possibility that the atomic number of the element may be greater than 92" (Wohlfarth 1979, 58) The announcement made headlines in the press, and in Italy it was celebrated as a great triumph of fascist culture. Although disturbed about the publicity, Fermi believed that he had manufactured the first transuranium elements. As late as December 1938, in his Nobel lecture in Stockholm, he spoke confidently about "ausonium" and "hesperium," the names used in Rome for elements 93 and 94.

The 1934 Rome announcement caused Otto Hahn and Lise Meitner at the Kaiser Wilhelm Institute of Chemistry in Berlin to engage in similar work. The institute, which had been founded in 1912, was funded at the time mainly by the chemical industry, directly or indirectly by the I. G. Farben Company, Germany's giant chemical corporation. Meitner and Hahn at first believed that they too had found transuranic elements and reported in 1935 that "it seems very probable that the 13 and 90 minute activities are elements beyond number 92" (Graetzer and Anderson 1971, 24). On the other hand, Fermi's results were criticized by Ida Noddack (née Tacke), a German chemist who, together with her later husband Walter Noddack, had discovered the element rhenium in 1925. Ida Noddack found Fermi's conclusions completely unwarranted and denied that transuranic elements had been produced. After all, she argued, almost nothing was known about neutron-induced nuclear reactions, so why assume that the product belonged to the end of the periodic table? "It is conceivable," she wrote, "that in the bombardment of heavy nuclei with neutrons, these nuclei break up into several large fragments that are actually isotopes of known elements, but are not neighbors of the irradiated elements" (Wohlfarth 1979, 63). Noddack's anticipation of nuclear fission made no impact at all on the course of events. Although published in a chemistry journal (the *Zeitschrift für angewandte Chemie*), it was known to both Fermi and Hahn and Meitner, but they did not take the suggestion seriously. Not only was Noddack's paper highly critical and her suggestion speculative, but the author's scientific reputation was also somewhat undermined because of her controversial claim to have discovered element 43 (which she called masurium and is now known as technetium, first produced in 1937 by E. Segré and Carlo Perrier). Noddack was not "rehabilitated" as a precursor of the fission hypothesis until the 1990s.

From 1935, the centers of uranium research moved from Rome to Berlin and Paris, with the two groups entering what can be better described as a rivalry than a cooperation. Although the Paris and Berlin groups were by far the most important, they were not the only ones interested in neutron-irradiated uranium. For example, in Berkeley, Philip Abelson tried to identify the supposedly transuranic products by means of the tested and precise x-ray spectroscopic method. However, in looking for atomic numbers larger than 92, Abelson failed to interpret his x-ray lines correctly. When the fission hypothesis became known, Abelson quickly found evidence for tellurium and thus confirmed the hypothesis. In Berlin, Hahn and Meitner made numerous experiments, suggested elaborate decay schemes, and thought of a variety of hypotheses in order to clarify what happened when uranium was bombarded with neutrons. After two years of strenous work, their main conclusion was disappointing, namely, that irradiated uranium produced complex products of an unknown nature, probably including some transuranic isotopes. Yet not all their work was in vain. One of their hypotheses was that the uranium products were isomers of uranium, that is, isotopes with different half-lives but with the same number of protons and neutrons. At that time, nuclear isomerism was not generally accepted, the only known (and controversial) case being the "uranium Z" that Hahn had reported as a protactinium isomer in 1921.

The work of Hahn and Meitner proved the existence of isomers but did not solve the uranium puzzle. In Paris, Irène Joliot-Curie worked on the same problem but adopted a somewhat different approach. In 1937, together with Pavel Savitch, a Yugoslavian physicist working in Paris, she reported a substance with a half-life of 3.5 hours in irradiated uranium. At first they thought it was thorium, but after more work they concluded in October 1938 that it followed lanthanum in chemical separations and was therefore possibly actinium—although "on the whole the properties of R 3.5 hr are those of lanthanum." Then, in the third round, they suggested that the 3.5-hour substance could not be an actinium isotope, but was probably a new transuranic element. Had they suggested that the close chemical similarity with lanthanum was evidence of a lanthanum isotope with half-life 3.5 hours, they might have discovered fission. But they did not. The results of Curie and Savitch puzzled the Berlin team, which from 1935 had been extended with the inclusion of Friedrich Strassmann, an analytical chemist. While pondering how to understand the Paris experiments, Meitner decided in July 1938 to leave Germany; it was now left to Hahn and Strassmann to find a solution. However, they communicated by mail with Meitner, who unofficially still belonged to the Berlin group.

It was the attempt to explain the Curie-Savitch results that led Hahn and Strassmann to fission. Among the activities resulting from the bombardment of uranium with neutrons, they found one that was precipitated with barium and therefore concluded that it was probably a new radium isotope. It seemed to them that the lanthanum-like isotope might be actinium, created from the radium by beta decay. But could radium be produced from uranium by the emittance of two alpha particles? Bohr, Meitner, and other theorists said no, and Hahn and Strassmann returned to the laboratory. In early December 1938 they began to realize that what they had thought of as radium behaved very much like barium, much more than would be expected from the chemical similarity of the two elements. If so, the Curie-Savitch substance might be lanthanum, produced by beta-active barium. By December 18, 1938, they had experimental evidence that what behaved like barium was, in all likelihood, barium. But it seemed incredible that uranium could turn into a much lighter element, and Hahn did not easily draw the conclusion. "Perhaps you can propose some kind of fantastic explanation," he wrote to Meitner on December 19. "We ourselves know that [uranium] *cannot* really burst apart into barium" (Weart 1983, 112). Even in Hahn's and Strassmann's paper of January 6, 1939, the two authors avoided a definite statement that barium had been produced by neutron-irradiated uranium. "As chemists," they wrote, "we should replace the symbols Ra, Ac and Th . . . in [our] scheme . . . by Ba, La, and Ce. . . . [But] as nuclear chemists, closely associated with physics, we cannot decide to take this step in contradiction to all previous experience in nuclear physics" (Wohlfarth 1979, 58). But they now glimpsed a possible explanation. They had found among the supposed transuranium elements one that resembled rhenium. If "radium" was barium, then the "transuranic rhenium" might be a lower homologue of rhenium, that is, element 43 or masurium (Ma). As Hahn and Strassmann remarked: "The sum of the mass numbers Ba + Ma, thus e.g. 138 + 101, gives 239!"

The insight that the uranium nucleus may split when capturing a slow neutron was first reached by Meitner and her nephew Otto Frisch, both refugees from the Third Reich. Frisch worked with Bohr in Copenhagen and his aunt was in Stockholm, where she had a position at Manne Siegbahn's institute. When the two met in late December 1938 to spend the Christmas holidays at Kungälv near Gothenburg, they had not yet received a copy of Hahn and Strassmann's paper. But they knew about its results and tried to figure out what had happened in the uranium nucleus in the Berlin laboratory. Frisch recalled: "We walked up and down in the snow, I on skis and she on foot . . . and gradually the idea took shape that this was no chipping or cracking of the nucleus but rather a process to be explained by Bohr's idea that the nucleus was like a liquid drop; such a drop might elongate and divide itself" (Frisch and Wheeler 1967, 276). The liquid drop model of the nucleus went back to work performed by Gamow in 1929 and during the following decade, it was developed by Bohr, von Weizsäcker, and others. Bohr's 1936 version, known as the compound nucleus, was particularly important and well suited to illuminate the mechanism of neutron reactions. The theory of the compound nucleus was well known to Frisch, who realized that it might provide an explanation of the Hahn-Strassmann anomaly. The splitting process was termed "fission," a name suggested to Frisch by an American biologist working at Bohr's institute. Meitner and Frisch reported their fission hypothesis in a letter to *Nature* on January 16, 1939. The hypothesis was that the uranium nucleus "after neutron capture, divides itself into two nuclei of roughly equal size." Moreover, the fission would be a violent process: "These two nuclei will repel each other and should gain a total kinetic energy of about 200 MeV, as calculated from nuclear radius and charge. This amount of energy may actually be expected to be available from the difference in packing fraction between uranium and the elements in the middle of the periodic system" (Graetzer and Anderson 1971, 52). Meitner and Frisch also used the occasion to suggest that thorium underwent fission in a manner similar to uranium. They had privately suggested to Hahn and Strassmann to look for radioactive inert gases (krypton and xenon) as fission products, and when Strassmann found the gases, the fission hypothesis was substantiated. It is notable that the discovery of fission, one of the most important discoveries in twentieth-century physics, was made by two chemists working at a chemical laboratory, and not by nuclear physicists. Indeed, the discovery took the physics community by surprise. Not even the Berlin physicists were aware that something highly interesting was going on at the Kaiser Wilhelm Institute of Chemistry.

## MORE THAN MOONSHINE

News about the splitting of the uranium nucleus spread rapidly in the international physics community. The route started in Copenhagen, where Frisch had discussed the matter with Bohr, who was preparing to leave for the United States. Bohr was greatly surprised, but immediately accepted the fission hypothesis. He was, Frisch wrote to Meitner on January 3, 1939, "only astonished that he had not thought earlier of this possibility, which follows so directly from the present conceptions of nuclear structure," that is, the model of the compound nucleus (Stuewer 1994, 78). Bohr and his collaborator, Léon Rosenfeld, arrived in New York on January 16, 1939, and Rosenfeld went straight to Princeton, where he discussed the conclusions obtained in Germany, Sweden, and Denmark. The announcement, made before Meitner and Frisch's paper had appeared, caused a sensation. Fermi, John Wheeler, and other American physicists immediately started working on the fission process. In late January 1939, Bohr attended the Fifth Washington Conference on Theoretical Physics, where he and Fermi discussed the new type of process and Bohr explained it qualitatively from the point of view of the liquid drop model. "The whole matter was quite unexpected news to all present," three American physicists reported in the February 15 issue of *Physical Review*. Fission was still a hypothesis, and the first phase of work, in both Europe and America, was concerned with verifying the suggestion of Meitner and Frisch. Using different methods, this was done within one or two months, first by Frisch in Copenhagen, who used an oscillograph to record the electrical pulses produced by the fission fragments in an ionization chamber. Shortly afterward, the Berkeley physicists Dale Corson and R. Thornton produced the first visual proof of fission by means of a cloud chamber photograph.

By the end of February, there was no longer any doubt about the reality of uranium fission, and a second phase, dealing with the possibility of a self-sustained chain reaction, had its beginning. The concept of a chain reaction had not occurred to either Frisch or Meitner. The possibility seems to have been first suggested to Frisch by Christian Møller in Copenhagen, but at first Frisch did not take it seriously. After all, there was as yet no indication of secondary neutrons. Yet, it was realized early on that if a fission reaction did not result merely in two nuclear fragments, but also in one or more neutrons, a chain reaction might be a possibility. John Dunning, a physicist at Columbia University, was among the very first to confirm the Meitner-Frisch fission hypothesis, which he did on January 25, 1939. As he wrote in his laboratory notebook from that date: "Believe we have observed new phenomenon of far-reaching consequences. . . . Here is real atomic energy! . . . *Secondary neutrons are highly important*! If emitted would give possibility of a self perpetuating neutron reaction which I have considered since 1932–35 as a main hope of 'burning' materials with slow neutrons and release atomic energy" (Badash, Hodes, and Tiddens 1986, 210; emphasis in original). The energy release per fission process, correctly estimated by Meitner and Frisch to be about 200 MeV, was measured by physicists at Columbia University and Princeton University in the spring of 1939. Both groups found that the two fission fragments had unequal masses and that the kinetic energy of the fragments was close to 175 MeV. That left about 25 MeV for other products, including extra neutrons. That such neutrons were produced was first shown in March 1939 by Frédéric Joliot and his collaborators Hans von Halban and Lev Kowarski, and slightly later by two American groups. The French physicists concluded that an average number of 3.5 neutrons were liberated per fission, a figure that was soon corrected to about 2.4. The important thing was that extra neutrons were produced in a number that might make a chain reaction possible. From theoretical considerations, however, Bohr suspected that the much more common uranium-238 isotope would not fission with slow neutrons, but only the rare (0.7 percent) uranium-235 isotope would. Bohr published his suggestion in a brief note on February 15. It was soon substantiated by further theoretical arguments, but lacked experimental confirmation. Only in March 1940 did experiments prove that Bohr was right. The new knowledge seemed to imply that any practical application of fission energy would be extremely difficult and costly.

In 1939, speculations about subatomic energy were far from new. Ever since the discovery of radioactivity, many people, scientists as well as nonscientists, had suggested that a new and powerful source of energy lay hidden in the interior of the atom. In 1903 Soddy described the earth dramatically as a "storehouse stuffed with explosives, inconceivably more powerful than any we know of, and possibly only awaiting a suitable detonator to cause the earth to revert to chaos." Eleven years later, in his book *The World Set Free*, the novelist H. G. Wells wrote about powerful atomic bombs. All the loose talk about atomic energy used for either peaceful or military purposes annoyed Rutherford. In 1933, in an address before the British Association, he said that "any one who says that with the means at present at our disposal and with our present knowledge we can utilize atomic energy is talking moonshine." Three years later, Bohr referred to "the much discussed problem of releasing the nuclear energy for practical purposes," concluding that "the more our knowledge of nuclear reactions advances the remoter this goal seems to become" (Rhodes 1986, 227). The discovery of fission did not make Bohr change his cautious attitude. In an address of December 6, 1939 to a Danish audience, he reviewed the latest developments in nuclear physics, including the great energy released in uranium fission. "One can understand what terrifying perspectives we would face if substantial amounts of uranium and thorium could be made to explode," he said. But there was no reason to worry: "A closer consideration shows that there is no cause for alarm in this respect, although one can hardly say with certainty that any large-scale release of atomic energy is entirely ruled out." He had in mind the difficulties of separating the two uranium isotopes. Yet, other physicists were quick to speculate about a possible uranium bomb. In February 1939, Oppenheimer wrote to Uhlenbeck: "I think it really not too improbable that a ten cm cube of uranium deuteride (one should have something to slow the neutrons without capturing them) might very well blow itself to hell" (Smith and Weiner 1980, 209).

By the end of 1939, more than 100 papers on fission had been published, and a large amount of knowledge accumulated by physicists in Europe and America. Review articles published in Germany, England, and the United States summarized the knowledge. An early review by Norman Feather of Cambridge University, completed in May 1939, concluded that "the possibility of a cumulative process of exothermic disintegration has to be considered" (Graetzer and Anderson 1971,.79). And a German review by Siegfried Flügge, titled "Can the Energy Content of Atomic Nuclei Be Made Technically Useful?" answered the question affirmatively, concluding that an "atomic machine" was indeed possible. Atomic energy was not yet a reality, but it was definitely not moonshine either. The theoretical understanding was still incomplete, but with Bohr's and Wheeler's detailed, semiempirical theory of September 1939, a foundation for further understanding was laid. The Bohr-Wheeler paper appeared in *Physical Review* on September 1, 1939, the same day as World War II began.

Although the possibility of a uranium bomb was not explicitly discussed during the first hectic weeks of 1939, physicists realized that fission research might some day lead to a bomb, and possibly first a German one. Many of the nuclear physicists in the United States who took up the study of fission were recent emigrants from Central Europe, and they were worried about the situation from an early date. Among them was Leo Szilard, the visionary Hungarian refugee who had worked in England and now lived in the United States, where he eagerly followed the work on fission. As early as 1934, Szilard had conceived the idea of a neutron chain reaction that might possibly lead to a violent explosion, but he had thought of using beryllium, not uranium. In order to prevent the Germans from producing a uranium bomb, Szilard suggested to his fellow physicists in February 1939 that they keep all uranium research secret. Szilard's unusual suggestion was met with skepticism, although several of the American emigrant physicists supported the idea. But there were also physicists who opposed it, because of priority reasons, because they found it unrealistic, or because they objected to the very idea of secrecy, so foreign to the ideals of science; and many found the possibility of a bomb so remote that they saw no point in even discussing it.

Nevertheless, Szilard was persistent and after some argument, most of the leading physicists were willing to support the secrecy plan. But not all physicists: Joliot and his group in Paris were unwilling to stop publishing and for this reason, among others, Szilard's idea could not be realized immediately. Physics journals continued to carry articles on fission throughout 1939, available to anyone who could understand them.

With the declaration of war the situation changed, however, and from 1940, physicists in Britain and the United States agreed to stop all publications of possible relevance to the use of atomic energy. In England, there already was a publication stop, and in April 1940 Gregory Breit became the chairman of an American censorship committee on uranium research. It is remarkable not only that the physicists agreed on such a drastic measure, but also that they did it purely voluntarily, without any pressure from their governments, and that they actually succeeded in keeping Western uranium research a secret to both German and, for a while, Soviet scientists. One of the last uranium papers that appeared in *Physical Review* was Edwin McMillan's and Abelson's announcement of the discovery of "Radioactive Element 93" (neptunium), which appeared in June 1940. The next, and much more important transuranic element, plutonium, was first produced by the Berkeley chemist Glenn Seaborg and his collaborators in 1941, but at that time the publication stop was effective. Seaborg's Nobel prize-rewarded discovery was first made public in 1946, when it appeared with the footnote "This letter was received for publication on the date indicated [January 28, 1941] but was voluntarily withheld from publication until the end of the war." Many of these footnotes were appended to papers in the 1946 issues of *Physical Review*.

The first serious attempt to explore the possibility of an atomic bomb took place in England, not in the United States. In March 1940, Frisch and Peierls made a quick study of how a uranium "superbomb" could be constructed in principle and how it would work. They estimated that a mass of one kilogram of metallic uranium-235 would be sufficient for a bomb and that "the energy liberated by a 5 kg bomb would be equivalent to that of several thousand tons of dynamite" and the radiation from it to "a hundred tons of radium." Apart from outlining the mechanism of the bomb, they also mentioned some of the political, ethical, and military aspects of the "practically irressistible" superbomb which, they feared, the Germans were already in the process of developing. Among these aspects were that "[o]wing to the spread of radioactive substances with the wind, the bomb could probably not be used without killing large numbers of civilians, and this may make it unsuitable as a weapon for use by this country" (Serber 1992, 81 and 86). As a result of the Frisch-Peierls memorandum, a British committee, named MAUD, was formed to work on the superbomb. The physicists associated with the committee considered various problems, in particular methods of isotope separation, the possible production of plutonium, and the neutron loss and multiplication in different volumes of uranium. Many of Britain's leading physicists were involved, including refugees like Frisch, Peierls, Kemmer, Simon, Kuhn, Kurti, and Klaus Fuchs. They were joined by Halban and Kowarski, who had fled Paris after the fall of France. The MAUD committee wrote its final report in the summer of 1941, concluding that an atomic bomb was feasible but also that a much larger organization was necessary for this work. The project would be huge and probably much too expensive for Britain alone. At that time, there was little cooperation between British and American physicists working with nuclear energy. Information about the British work in 1941 was passed on to Moscow by Fuchs, the German refugee communist physicist. Later during the war, Fuchs became a central figure in the Soviet network of agents who informed Moscow of the progress taking place in the American bomb project.

## Toward the Bomb

The development of the American bomb program, generally known as the Manhattan Project, is well known and has often been described in detail. We shall merely recall the essential steps in the program that, following tradition, started with the letter that Einstein wrote to President Roosevelt in the summer of 1939. The famous letter was actually drafted by Szilard after consultations with Wigner and Teller. Einstein told the president of the United States that he had reasons to believe that "the element uranium may be turned into a new and important source of energy in the immediate future. . . . Now it appears almost certain that this [chain reaction] could be achieved in the immediate future." Furthermore, "[t]his new phenomenon would also lead to the construction of bombs, and it is conceivable—though much less certain—that extremely powerful bombs of a new type may thus be constructed. A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with the surrounding territory. However, such bombs might very well prove to be too heavy for transportation by air" (Graetzer and Anderson 1971, 93). The Germans were possibly already working along this line—Einstein mentioned von Weizsäcker specifically—and for this reason, Einstein advised Roosevelt to take action. The letter had no immediate effect except that the president appointed an Advisory Committee on Uranium. About a year later, the United States began in earnest to prepare itself for war, which included the foundation of a National Defense Research Committee (NDRC) chaired by the engineer and physicist Vannevar Bush. The Uranium Committee was redefined as a subcommittee of the NDRC. In 1941, the NDRC was absorbed into a larger and more efficient organization, the Office of Scientific

Research and Development (OSRD), again headed by Bush. At that time, American physicists, chemists, and engineers had started working on uranium chain reactions, but were still only in an exploratory stage. The work was experimental as well as theoretical and involved, among other things, a general theory of controlled chain reactions developed by Fermi, Wigner, Wheeler, and others. A report prepared by Lawrence emphasized the possibility of using plutonium as a bomb material. "If large amounts of element 94 were available," wrote Lawrence, "it is likely that a chain reaction with fast neutrons could be produced. In such a reaction the energy would be released at an explosive rate which might be described as a 'super bomb'" (Smyth 1945, 65).

After the Japanese attack on Pearl Harbor, the nuclear program was expanded vastly and large sums of government money were allocated to research related to the future atomic bomb. A Metallurgical Laboratory, or Met Lab, was created at the University of Chicago, headed by Arthur Compton and with Fermi as leader of the experimental nuclear physics group. The goal was now clear: to construct an atomic bomb based on either uranium-235 or plutonium. Compton decided that a first step had to be a slow chain reaction, followed by a uranium reactor to produce plutonium, and then a bomb based on the plutonium produced from the reactor. According to his plan, the reactor should be ready by January 1943 and the bomb two years later. Work progressed satisfactorily, and theoretical and experimental studies indicated that a bomb with an energy corresponding to at least two kilotons of TNT could be ready at the scheduled time. But it would require an enormous investment and an organization of a scale and complexity that only the army could take care of. In early 1943, all the efforts were put together under a new military organization, code-named Manhattan Engineer District and headed by Brigadier General Leslie Groves.

The first step in the expanded program was to make a primitive reactor in order to determine if a chain reaction in uranium was possible. This was the work of Fermi and his collaborators at the University of Chicago, who used pure graphite bricks as moderators for the neutrons produced by naturally occurring, but high-quality, uranium. The Chicago "pile" known as CP-1 consumed 385 tons of graphite, 6 tons of pure uranium metal, and 34 tons of uranium oxide. The critical level, where the multiplication factor exceeds one, was obtained quickly and without major problems on December 2, 1942. Fermi was enthusiastic, not only because the work had succeeded but also because the pile was so easy to control with the neutron-absorbing cadmium rods. "To operate a pile is as easy as to keep a car running on a straight road by adjusting the steering wheel when the car tends to shift right or left," he wrote. That this first case of controllable nuclear energy produced only a minute amount of power was irrelevant, for its purpose was not to produce either heat or electricity. CP-1 was meant as a prototype of a plutonium generator. Its success made OSRD confident that an atomic bomb could be produced in time to be used in the war and implied the need for another upscaling of the project. In late December 1942, Roosevelt approved Bush's plan for using $250 million for factories producing uranium-235 and plutonium. It was uncertain how much plutonium could be produced and how quickly, and for this reason it was decided to go ahead with both types of fission materials. The most formidable problem with the uranium bomb was the separation of uranium-235 from natural uranium. Several methods were considered, and the gaseous diffusion method, where the gas uranium hexafluoride flows through a system of porous barriers, was found to be most practical. Another possibility, proposed by Lawrence, was electromagnetic separation by means of huge electromagnets or "calutrons," and this method also met approval. In 1944, plants using these two methods were supplemented with a thermal diffusion plant. The three methods were integrated in the huge factory system built at Oak Ridge, Tennessee, which started producing uranium-235 in April 1945. Oak Ridge included a pilot uranium reactor, but the three large water-cooled reactors that were to produce material for the plutonium bomb were built at Hanford, Washington.

The design of the atomic bomb was the job of the large group of physicists that began to be assembled at Los Alamos, New Mexico, in the spring of 1943. With J. Robert Oppenheimer as director of the laboratory, seven divisions were established, among them a theoretical division under Bethe, an experimental division under Robert Wilson, a bomb physics division under Robert Bacher, and an explosives division under George Kistiakowsky. Oppenheimer had not previously been engaged in uranium research and was considered a security risk by some military groups because of his occasional flirtations with communism some years earlier. But Groves had confidence in Oppenheimer's abilities as a leader, and his intuition proved right. Oppenheimer, respected by both the physicists and the army generals, was just the right man for an impossible job. The physicists arriving at Los Alamos were given the necessary technical background in a five-lecture course on "How to build an atomic bomb" by Robert Serber, a theorist and colleague of Oppenheimer. Just in case someone did not already know, Serber started the course with pointing out, "The object of the project is to produce a *practical military weapon* in the form of a bomb in which the energy is released by a fast neutron chain reaction in one or more of the materials known to show nuclear fission" (Serber 1992, 3). In an atomic bomb the neutrons have to be fast, not slow as in a reactor, and it was not known if a chain reaction could, in fact, occur with fast neutrons. Early experiments in Los Alamos proved that it could, and indicated the critical size of the bomb.

Another of the important problems studied by the physicists in the New Mexican desert was how to assemble a critical mass of fissionable material from two subcritical masses. One of the methods, a more sophisticated version of the one included in the Frisch-Peierls memorandum, was to shoot one of the subcritical masses into the other. Another method was proposed by Seth Neddermeyer, namely to surround a spherical subcritical mass with a chemical explosive and then "implode" it into a much smaller and denser mass; although of the same mass, the higher density would make it supercritical. The implosion method was untested and much more complex than the gun method, but it turned out that only the implosion method could be used in a plutonium bomb. The physicists discovered that plutonium-240, inevitably occurring with the ordinary plutonium-239, would undergo spontaneous fission and as a result produce too many neutrons for the gun method to work. (Spontaneous fission in uranium had been discovered by two Soviet physicists, Georgii Flerov and Konstantin Petrzhak, and reported in *Physical Review* in 1940.) Rather than relying on one type of bomb, a plutonium-implosion bomb or a uranium-gun bomb, it was decided to develop both kinds at the same time. The final stage of the bomb project took place in the summer of 1945, after the unconditional surrender of Germany and the end of the European war. The bomb had been planned to be used against the Third Reich, but the new situation changed nothing in the pace of the Manhattan Project. There were still the Japanese left, and the momentum of the giant project seemed beyond control.

Who were the physicists working in the Manhattan Project? It would perhaps be easier to list those who did not, for it included most of the Western world's most brilliant physicists, from legendary figures like Bohr to young up-and-coming physicists like Richard Feynman. Both extremes were equally valuable to the project. About Bohr, officially "Dr. Baker," Oppenheimer reported to Groves in early 1944 that "Dr. Baker concerned himself primarily with the correlation and interpretation of the many new data on nuclear fission and related topics . . . [but] very little with the engineering problems of our program although he is of course aware of their importance and their difficulty." About Feynman, Oppenheimer wrote later in 1944 that he "[is] not only an extremely brilliant theorist, but a man of the greatest robustness, responsibility and warmth, a brilliant and lucid teacher, and an untiring worker" (Smith and Weiner 1980, 270 and 276).

A large part of the most active of the Manhattan researchers were physicists who only a few years earlier had resided in Europe. Take a look at table 17.2 and one will find many of the bomb project's central figures. One of them was James Franck, the German Nobel laureate of 1925, who had left the country in 1935 and now worked in Chicago. He was one of the few scientists working in the bomb project who warned against its political and ethical consequences at an early stage. On June 11, 1945, before the explosion of the first atomic bomb, he and six of his Chicago colleagues wrote a report to the Secretary of War in which they explained their position. "Scientists have often before been accused of providing new weapons for the mutual destruction of nations, instead of improving their well-being," Franck wrote. "We feel compelled to take a more active stand now because the success which we have achieved in the development of nuclear power is fraught with infinitely greater dangers than were all the inventions of the past." Echoing many of Bohr's arguments, Franck looked to the future and warned against a nuclear armaments race, for "nuclear bombs cannot possibly remain a 'secret weapon' at the exclusive disposal of this country for more than a few years." More specifically, the Franck report advised that the American bomb be "first revealed to the world by a demonstration in an appropriately selected uninhabitated area" (Graetzer and Anderson 1971, 104). But this was not what happened. Franck and his small group had no sympathy among the military leaders and their worried attitude was far from shared by the majority of physicists working in the Manhattan Project. Most physicists had no moral objections against working with a weapon of mass destruction. There were some who refused, but the general attitude was that the work was justified in view of the war situation and the possibility that Hitler might get the bomb first. In addition, many of the physicists were simply "intrigued with a fascinating and difficult scientific and engineering problem," as David Anderson recalled some forty years later (Badash, Hodes and Tiddens 1986, 222).

## The Death of Two Cities

"The most striking impression was that of an overwhelmingly bright light. . . . I was flabbergasted by the new spectacle. We saw the whole sky flash with unbelievable brightness in spite of the very dark glasses we wore. . . . I believe that for a moment I thought the explosion might set fire to the atmosphere and thus finish the earth, even though I knew that this was not possible" (Rhodes 1986, 673). This was the impression of Emilio Segré when he witnessed the first nuclear explosion in history, the so-called Trinity test in the Alamogordo desert on July 16, 1945 at 5:30 A.M. The bomb, placed atop a 100-feet steel tower, was of the plutonium-implosion type. The test was completely successful, with its energy corresponding to about 18 kilotons of TNT, which was more than most of the physicists expected. This, and not the worries of Franck, was what mattered to the physicists who observed the spectacular phenomenon. "Naturally, we were very jubilant over the outcome of the experiment," Victor Weisskopf recalled. "We turned to one another and offered congratulations, for the first few minutes. Then, there was a chill, which was not the morning cold. . . ." (ibid., 675). The plutonium bomb worked beautifully and there was enough material for the uranium bomb, which was better understood and therefore did not need a test. The time had come for real action, and that, it turned out, meant the destruction of Japanese cities. Up to that time, the development of the bomb had been left to the physicists, whose voices were also important in the political and military discussions. But it was the military and political leaders, of course, who decided what the bombs should be used for. There was much discussion among the physicists in Los Alamos, Berkeley, Chicago, and elsewhere, but they did not form a united front and on the whole, they were not inclined to disagree with the military leaders. The scientific advisory group, consisting of Compton, Fermi, Lawrence, and Oppenheimer, saw no acceptable alternative to letting the bombs explode over densely populated areas in Japan. At any rate, Truman, the new president, decided that the bombs should be dropped over Japan as soon as possible and according to the judgment of the generals. The wisdom of this decision has been the subject of endless discussion, but the important thing is that it was taken. At this stage, in early July, the physicists did not have much to say about the creatures they had constructed.

The first creature was "Little Boy," a ten-thousand-pound uranium bomb about ten feet long and thirty inches in diameter. Carried on the B-29 bomber "Enola Gay," it was brought to explosion over the city of Hiroshima on August 6, 1945 at 8:16 A.M. local time, about 2,000 feet aboveground. It did what it was designed to do, except that it did not force the Japanese to accede to the demanded unconditional surrender. Three days later, "Fat Man" took over. Dropped over Nagasaki from another B-29 (named "Bock's Car"), the plutonium bomb exploded at 11:02 A.M., at approximately the same height above the ground. It also did what it was designed to do (see table 18.1), and five days later the Japanese government and its emperor capitulated. World War II was over.

The $2 billion bomb project was the largest research project in history, involving more scientists and money than any previous or subsequent project. To most people, it proved dramatically that science, and physics in particular, was able to win wars and change the course of history. In reality, the Manhattan Project was completed too late to be of decisive importance with regard to the war. The real significance of the atomic bomb was political rather than military, and became clear only after the end of the war. From a military point of view, the less publicized research on radar, the other main area of allied military physics, was much more important.

TABLE 18.1
Data on the Two Nuclear Bombs and Their Consequences

| | *Hiroshima* | *Nagasaki* |
|---|---|---|
| Bomb type | uranium - gun | plutonium - implosion |
| Weight of bomb (tons) | 4 | 4.5 |
| Explosive power (kilotons TNT) | 12.5 | 22 |
| Population | 285,000 | 270,000 |
| Totally destroyed buildings | 54,000 | 14,000 |
| Dead in initial phase | 105,000 | 65,000 |
| Wounded | 75,000 | 40,000 |
| Area totally destroyed (square kilometers) | 13 | 6.7 |
| Deaths per square kilometers | 8,100 | 9,700 |

*Note:* Based on figures given in *The Impact of the A-Bomb* (Tokyo: Iwanami Shoten, 1985).

Only the gargantuan American project succeeded in developing an atomic bomb, but militarily oriented nuclear research was pursued in other countries as well. In Japan, the navy established a uranium program with the purpose of developing a nuclear reactor for driving warships, but it withdrew from the project because it seemed too costly and too uncertain. At Tokyo University, a group under Nishina explored the possibilities of uranium-235 separation in order to make a bomb, but progress was slow. In the Soviet Union, a Commission on the Uranium Problem was created in August 1940 under the Academy of Sciences. The task of the committee was to study the possibility of using the energy from a chain reaction in uranium. The leading Soviet physicists working in the commission, Iulii Khariton, Igor Zel'dovich, Flerov, and Igor Kurchatov, duplicated independently the work of Frisch and Peierls in England. In early 1941, they calculated the critical mass of uranium-235 and found it to be about ten kilograms. They even included in their calculations a heavy neutron reflector. A little later, Kurchatov realized the importance of plutonium and emphasized that using the new element might be the best way to build a bomb. The Russian equivalent of Los Alamos, although much smaller, was "Laboratory No. 2," established for Kurchatov in the spring of 1943. A year later, it included seventy-four people, of whom twenty-five were scientists (about 2,000 people worked at Los Alamos). The Soviet bomb project was seriously hampered by lack of material, especially pure uranium and graphite. On the other hand, the Russians had the advantage of being informed about the secret American project by Fuchs, the German refugee physicist who worked in Los Alamos. Yet, by August 1945, the Russians were not even close to having an atomic bomb. They obtained their first micrograms of plutonium in August 1944, from the Leningrad cyclotron, and the first Soviet reactor (named F-1) went critical in the last days of 1946.

The German efforts, so much feared by the physicists in Britain and America, started at an early date. Already in the spring of 1939—right after the Paris announcement of secondary neutrons—German physicists pointed out the potential military applications of uranium physics and the *Uranverein* (Uranium Society) started a series of meetings. The group included Flügge, Paul Harteck, Fritz Bopp, Heisenberg, von Weizsäcker, and Walther Gehrlach, among others. Several nuclear research teams were established, at the University of Leipzig, the University of Hamburg, the Kaiser Wilhelm Institute of Physics in Berlin, and elsewhere. The general aim of the Uranverein was to study the possibility of using nuclear energy, primarily in the form of a reactor that could be used to drive submarines or even airplanes. Atomic bombs were initially on the agenda, but were not given high priority. However, the physicists were well aware of the possibility of a bomb. In a talk given in February 1942, Heisenberg mentioned that the isolation of uranium-235 would "lead to an explosive of unimaginable potency" and that a uranium machine "can also lead to the production of an incredibly powerful explosive" (Hentschel 1996, 300). During the first two years of the war, uranium research in Germany was on an equal level with that in Britain and America, but after 1942 progress declined and the military lost some of its interest in the project. Unaware of the progress taking place in the United States, Heisenberg and his fellow uranium researchers concentrated on producing a reactor. When the war ended and Heisenberg was taken prisoner, the primitive machine had still not operated at the critical level. Heisenberg, Hahn, and the other German physicists who were interned at Farm Hall in England were greatly surprised when they learned about the dropping of "Little Boy" over Hiroshima.

One can easily get the impression that the entire physics community was preoccupied with military science during the war years and that they had neither the time nor the desire to do pure science. That was far from the case, however. Although the amount of ordinary academic physics was greatly reduced between 1940 and 1945, there were still physicists working in areas of pure physics and producing a substantial output of papers in this category. Dirac refused to join the Manhattan Project and continued working on problems of quantum electrodynamics (but he also did work related to the British bomb project). Born stayed away from military physics. "I continue my work undisturbed," he wrote to Einstein in the spring of 1940. "Soon my department [in Edinburgh] will be the only spot in Great Britain where theoretical work is still done" (Kragh 1990, 159). At the new Dublin Institute for Advanced Studies in neutral Ireland, Schrödinger gave lectures and arranged colloquia on theoretical subjects, with participants including Heitler, Dirac, Eddington, and Born. Even Heisenberg, working hard with the uranium machine, found time to concentrate on pure theory. The last paper appearing in the *Zeitschrift für Physik* before the end of the war was the third part of Heisenberg's "The Observable Quantities in the Theory of Elementary Particles," a paper as remote from military applications as one can imagine. It was submitted on May 12, 1944. Japanese theorists, including Tomonaga and Sakata, likewise worked on foundational problems in the midst of the war. Tomonaga's important work, "On a Relativistic Reformulation of Quantum Field Theory," appeared (in Japanese) in 1943.

This is not to deny that the war had a very serious impact on academic physics, qualitatively as well as quantitatively. World physics survived in

TABLE 18.2
Number of Papers in Physics, All Journals, and Pages
in the Volumes of *Physical Review* and *Philosophical Magazine*

| *Year* | *1938* | *1939* | *1940* | *1941* | *1942* | *1943* | *1944* | *1945* | *1946* | *1947* | *1948* | *1949* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Papers | 5081 | 4705 | 3230 | 2737 | 3152 | 2968 | 2687 | 3148 | 3273 | 3765 | 4088 | 7500 |
| *Phys. Rev.* | 2965 | 2914 | 1677 | 1041 | 1008 | 428 | 417 | 945 | 1517 | 2240 | 2307 | 2275 |
| *Phil. Mag.* | 2237 | 1478 | 1130 | 1026 | 910 | 851 | 855 | 875 | 884 | 913 | 1008 | 1278 |

1944, but at a low level. The decrease in physics publications and the slow recovery after the peace is illustrated by the number of papers abstracted by *Physics Abstract* and the number of pages in two leading physics journals (table 18.2). The decline is visually displayed in figures 18.1 and 18.2, referring to the situation in Great Britain.

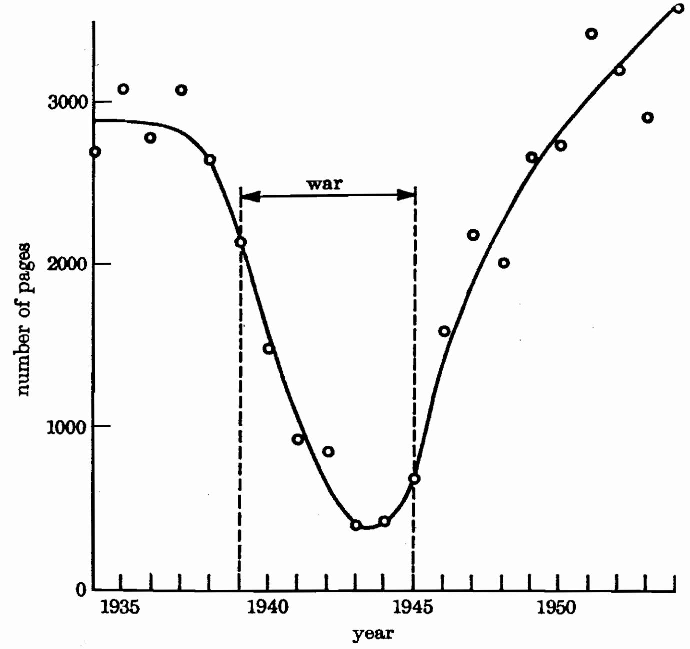

Figure 18.1. The number of pages published in each calendar year in the *Proceedings of the Royal Society of London*, section A. Allowance has been made for changes in the printed area of each page by normalizing to the format used in 1955. *Source:* E. Bullard, "The effect of World War II on the development of knowledge in the physical sciences," *Proceedings of the Royal Society A* 342 (1975): 519–36. Permission by The Royal Society.

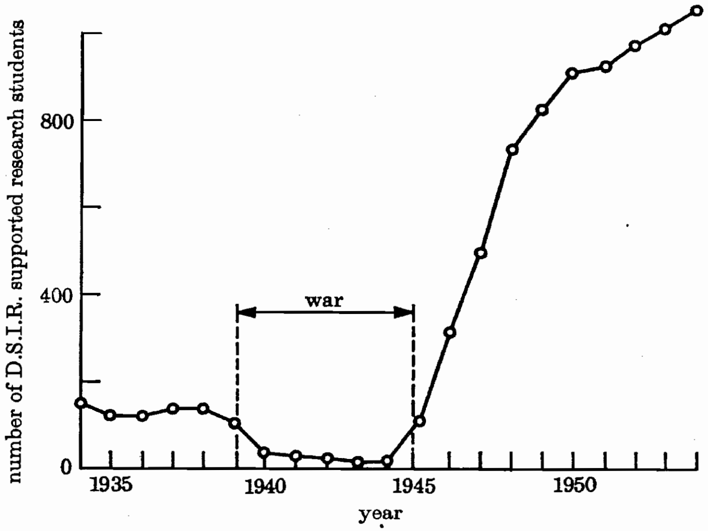

Figure 18.2. Total number of research students, most of them in the physical sciences, supported by DSIR (Department of Scientific and Industrial Research) each year. *Source:* E. Bullard, "The effect of World War II on the development of knowledge in the physical sciences," *Proceedings of the Royal Society A* 342 (1975): 519–36. Permission by The Royal Society.