Bo Lindell

The history of radiation, radioactivity, and radiological protection

PART 2. THE 1940s

THE SWORD OF DAMOCLES

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The History of Radiation, Radioactivity, and Radiological Protection

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Translated by Helen Johnson

through Snabböversättare Sverige AB

Translator’s note: Any views expressed herein on any subject have, to the best of my knowledge, been expressed in the manner in which they are expressed in the original text and are not necessarily my own.

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FOREWORD

THE SWORD OF DAMOCLES is the second part of my series on the history of radiation, radioactivity and radiological protection, i.e., a direct continuation of *Pandora's Box*, 1996 (*English translation, 2019*). It is presumed that the reader of *The Sword of Damocles* has read *Pandora's Box* since the information contained in the latter is essential to the comprehension of the story as it continues. *Pandora's Box* consisted predominantly of science and the story concerned the important discoveries and theories which culminated with Otto Hahn splitting the nucleus of a uranium atom. In this book, the scientists and discoverers play a less significant role with the representatives of technology, politics and military interests playing the primary roles. (*Readers may also wish to refer to Prof. Lindell's foreword to the English translation of the book series, see Pandora's Box*).

My initial intention was to write a trilogy, and I had intended to conclude *The Sword of Damocles* with the start of the 1970s when the nuclear weapons debate was replaced by a nuclear power debate. However, I soon found that there was too much material and that so much happened in the field in the 1940s that I had to limit my account to this decade.

The content of *The Sword of Damocles* is principally an account of the endeavours to produce an atomic bomb during the Second World War and the use of the bombs on Hiroshima and Nagasaki. This describes military and political events which, at first glance, may seem to be somewhat removed from a history of radiation. I would still maintain that they are of great importance to the understanding of the continued development, particularly the use of nuclear reactors for civil purposes and for the impending nuclear power discussion. The trauma of the bombings in Japan has had a major influence on the development, and the radioactive environmental contamination from the atmospheric nuclear weapon tests has contributed to form the picture that people have of the risk of radiation and radioactive substances.

These world politics events are interspersed with descriptions of events relating more obviously to radiation, such as the development of Rolf Sievert's institute and radiophysics in Sweden, the appearance of the first Swedish radiation protection law, the debated 'radiological holidays' and the infancy of Swedish atomic energy. As an aside from the field of radiation, but significant because it affected Sievert's work, we have the account of achievements by physicists during the war years at the Military Physics Institute organised by Sievert, a forerunner to the Swedish National Defence Research Institute (FOA). The final chapter involves a change to the style because that is where I recall events in which I was involved in myself, so it uses a more subjective style; I hope the reader understands this.

I am no historian. The reader therefore cannot expect any new points of view on important matters such as whether Heisenberg really did want to produce nuclear weapons and whether the main motive for using the atomic bomb on Hiroshima and Nagasaki really was to avoid a worse catastrophe, i.e., a continuation of the war, etc. I am unable to answer such questions myself and can do no more than give an account of what others have said.

So, the book can principally be seen as something to guide you through the most widely available literature, a reference book if you will. There is no outline of the field from a Swedish point of view. Much has been written in English, but it concerns mainly special issues and there is no summary, especially not of the course of events seen through the eyes of someone who is interested in radiation protection. Many of the authors of the reference literature have done extensive research, the results of which have been useful to me. I have tried to avoid plagiarism and have stated the sources of direct quotations, but have gratefully utilised the factual information that has been made available through research done by other people.

I am also grateful on this occasion for the help I have received from many colleagues at the Radiation Protection Institute, and particularly for the support from its Director-General, Lars-Erik Holm. The

Radiation Protection Institute has helped to finance the publication of the book. Generous contributions have also come from the Riksbankens Jubileumsfond (the Swedish Foundation for Humanities and Social Sciences) as well as the Marcus and Amalia Wallenberg Foundation. The book could not have been published without these contributions.

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Sollentuna, October 1999
Bo Lindell

1. SZILARD'S BOMB

I have not made my escape to America and I have no plans to revolutionise steam technology.

THESE WERE the words of *Lise Meitner* (1878-1968) when she was interviewed by the writer with the pseudonym '-x' from *Svenska Dagbladet* in her study at the Nobel Institute for Physics in Frescati outside Stockholm in May 1940. The Second World War had been ongoing for eight months. Denmark and Norway were occupied by the Germans and the news travelled down many a winding road. A telegram from New York via London had maintained that Professors *Otto Hahn* (1879-1968), Lise Meitner and *Niels Bohr* (1885-1962) had been in the United States for several months and, while there, succeeded in solving the problem of extracting energy through splitting the atomic nuclei of uranium. The energy that was released in this way was said to be five million times greater than when combusting the equivalent weight in carbon. But unfortunately, Professor Meitner completely denied this. The question from *Svenska Dagbladet's* less-than-happy interviewer was:

> 'But the message surely can't just have been completely made up. There must be a grain of truth in all this quite detailed information!'
>
> 'Probably. But all information that can be checked is incorrect. Professor Bohr was in Princeton for a few months last year but returned to Copenhagen as early as spring 1939; Professor Hahn is still head of the Kaiser Wilhelm Institute for Chemistry in Berlin-Dahlem, and I've been working at Professor Siegbahn's institute in Stockholm for a few years myself with a break for a short stay in Copenhagen,' answers Dr. Meitner. 'I can therefore only hazard a guess as to what may lie behind the American message. That Professor who was mentioned, J.R. Dunning[*] of Columbia University, is a distinguished physicist, and it's quite likely that he and his colleagues succeeded in producing a small quantity of the uranium isotope in question with the atomic weight 235 in pure or almost pure form. It can be a matter of no more than an extraordinarily small quantity, which is nonetheless sufficient for certain important experiments.'
>
> 'The discovery that the splitting of uranium when bombarded with neutrons was made two years ago in Berlin at Hahn's laboratory while I was still working there. We initially thought that the products were 'transuranic elements', i.e., new elements heavier than uranium. However, it wasn't long before the heavy atomic nucleus was shown to have been fissioned almost straight down the middle and gradually created a large number of substances with far smaller atomic weights. Nothing like it had ever been observed before and the case was extremely puzzling, but it was explained by Bohr, who theorised that it was not the heaviest isotope (238) but a relatively rare one with the weight 235 that was responsible for the remarkable fissioning effect of the bombardment.[†] Bohr's hypothesis, which was based on a purely theoretical argument, was confirmed through a number of experiments in Copenhagen by Dr. Frisch and myself, but full confirmation can only be obtained if you manage to isolate the unstable isotope. And that may be what the American physicists with their substantial instrumental resources have now achieved.' (*Svenska Dagbladet*, 8 May 1940.)

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[*] John R. Dunning (1907-1975) was an experimental physicist at Columbia University, best known for having, unlike *Enrico Fermi* (1901-1954), accepted Bohr's theory from the start that uranium-235 rather than uranium-238 (i.e., the "common uranium") was the nuclide that was easy to split. Thanks to Dunning's support, early endeavours concentrated on the isotope separation of uranium-235.

[†] Natural uranium consists of 99.3 % uranium-238 and 0.7 % uranium-235.

Lise Meitner's response to *Svenska Dagbladet's* reporter is a good introduction to the story I am going to tell in this volume. In December 1938, Otto Hahn and *Fritz Strassmann* (1902-1980) had irradiated uranium with neutrons and then performed chemical analyses to demonstrate the new, heavy atomic nuclei they expected to find. However, there were no clear traces of these; instead, there were the unknown substances which had the same chemical properties as barium and lanthanum. Following further analyses and much thinking, they as chemists were convinced that these substances had to be barium and lanthanum.

To them, this conclusion seemed to conflict with all reason and all scientific experience to such an extent that they hesitated to publish the result. Hahn corresponded with his former colleague Lise Meitner who moved to Sweden to escape Hitler's persecution of the Jews. Meitner spent Christmas 1938 with her nephew *Otto Robert Frisch* (1904-1979) in Kungälv where they read Hahn's anxious letter together and, following initial spontaneous doubts, concluded that Hahn and Strassmann must have managed to fission a nucleus and produce barium and lanthanum as the products of that split, i.e., halves of uranium atoms. At this time, Frisch was working for Niels Bohr in Copenhagen, and he returned there and told Bohr of what he and his aunt had concluded. Bohr shouted: 'Oh, what idiots we've been! This is absolutely wonderful!'

A few days later, Bohr travelled to the United States on Swedish American Line's 'Drottningholm' and told his American colleagues about the fission of the nucleus, a story which would have different resulting effects. People everywhere began to realise that the large quantities of energy that could be released by splitting uranium could be used for practical energy extraction. Some were thinking of peaceful energy production while others were looking at the possibility of a new weapon of mass destruction. Lise Meitner thought about the peaceful use, but she told *Svenska Dagbladet's* reporter: 'But this result is at the moment a utopia that goes way beyond the borders of what is possible in practice.'

But developments proved that she had been wrong. And, worse still, with the world situation as tense as it was, the development of the 'atomic bomb' was unavoidable. However, this was understood by only a few people when the Second World War broke out in autumn 1939, and not even these few could be sure who would be first to produce the new weapon. Nor could they imagine just how destructive it would turn out to be. Had someone placed a bet, the odds would have pointed to Nazi Germany and its reputation as a prominent nation within science and technology, with its industry under State control and with a brutal government led by a dictator who dreamt of world domination and loved surprises.

However, when the war began, the Nazis' persecution of the Jews and brutality had caused many prominent scientists to take flight. Continental Europe was no longer safe; safety was largely to be had on the other side of the Atlantic, but to a certain extent already on the other side of the English Channel. There were many renowned refugees from Hungary, including *George de Hevesy* (1885-1966), *John von Neumann* (1903-1957), *Leo Szilard* (1898-1964), *Edward Teller* (1908-2003) and *Eugene Wigner* (1902-1995). From Austria there were Lise Meitner and her nephew Otto Robert Frisch. From Italy Enrico Fermi and *Emilio Segrè* (1905-1989). And from Germany *Max Born* (1882-1970), *Richard Courant* (1888-1972), *Albert Einstein* (1879-1955) and Austrian-born *Erwin Schrödinger* (1887-1961).

There were so many renowned emigrants from Hungary that, according to Otto Frisch, German physicist *Fritz Houtermans*[*] launched the theory that 'these people were really visitors from Mars; for them it was difficult to speak without an accent that would give them away and therefore they chose to pretend to be Hungarians whose inability to speak any language without accent is well known…'.

When Hitler's troops marched into Austria on 12 March 1938, there was no doubt that the 'Third Reich' was a threat to world peace. For nuclear physics, the annexation meant that Lise Meitner's Austrian passport became invalid. With the help of *Peter Debye* (1884-1966), the Dutch representative of the Kaiser Wilhelm Institute for Physics, she fled the country through Holland and ended up in

[*] Born in 1903, grew up in Vienna but was born in Gdansk when it was called Danzig, son of a Dutch banker and a mother who was half Jewish.

Sweden, which is when she and Otto Robert Frisch interpreted Otto Hahn's surprising analysis result over Christmas in 1938.

On 16 January 1939, Niels Bohr arrived in New York and the news spread among his American colleagues, many of whom were refugees from Europe (I have already given an account of this in the first part of my story, Pandora's Box). Two of the physicists in New York were particularly worked up about the news. One was Enrico Fermi who had just come to America via Stockholm where he was awarded the Nobel Prize in Physics in December 1938 for his research into neutron-induced nuclear reactions. The second was Leo Szilard, who had already been in New York for one year and was extremely anxious for someone to find that splitting uranium and releasing neutrons could generate a chain reaction. If this were the case, he thought that it must be kept secret from the Germans at all costs.

The possibility of a chain reaction was understood pretty instantly by several physicists. This could be possible if neutrons were released when the nucleus was fissioned, which could in turn lead to the fission of even more atomic nuclei. This would continue with the number of neutrons released being at least the same as the number of neutrons that helped to fission the nucleus or that were lost through absorption by non-fissile material. If this were the case, it could be a matter of neutron multiplication, and the number of nuclear fissions could increase like an avalanche if the process were not stopped.

If the process could be controlled so that the number of new and used neutrons remained equal, this would result in an unparalleled source of energy for peaceful use. If on the other hand the number of new neutrons were increased, there would be a chain reaction which could release a colossal amount of energy, possibly within a short space of time. You would then have a bomb.

The possibility of an atomic bomb was discussed by Leo Szilard and his colleagues in New York and Washington DC in January 1939 when they heard about Bohr's information. This was something that Szilard had been thinking about since 1933 and a possibility for which he had applied for a patent which was secretly stored by the British Admiralty. How it came to that is a story worth telling.

Leo Szilard was born in Budapest in 1898. His father was an engineer and the family was well-to-do. Leo had governesses at home to teach him German and French. He went to the top secondary school in Budapest and had two interests as a youngster: physics and politics. He was sixteen when the First World War broke out, and believed that the war ought to lead to both Austria-Germany and Russia losing, the only apparent obstacle being that they were fighting on different sides.

He left secondary school in 1916 with the Eötvö Prize[*], a Hungarian national prize in mathematics. He wanted to study physics but there was no future for physicists in Hungary other than as a high school teacher, and nor were there any good university physics courses. He considered chemistry but that looked no more likely to provide a better future or any income. He therefore began studying electrotechnology at the Royal Technical University in Budapest instead, but he was called up to the Hungarian army and, since he had finished his high school education, he was made Officer Aspirant in the Cavalry.

After a while, he became ill with the epidemic influenza which was called the 'Spanish flu' and was granted leave to go home. He then succeeded in using connections to get signed off. His regiment was sent to the front and most of his pals died. He had been lucky. In 1919, *Bela Kun* (1986-1939) came to power and Szilard found this an appropriate time to leave the now communist Hungary to continue his studies abroad. He had just received his passport when Kun was overthrown and Admiral *Miklós Horthy* (1868-1957) became head of the nation. He had to change his passport and left Hungary at Christmas in 1919 to move to Berlin. He registered with the University of Technology there but in Berlin, unlike Budapest, there were also opportunities to study physics at the university. This is where Einstein, *Max Planck* (1858-1947) and *Max von Laue* (1879-1960) were, as well as the Kaiser Wilhelm Institute for Physics and Chemistry in Berlin-Dahlem.

Szilard's interest in his technical studies waned and he left the University of Technology in 1921 to continue his studies at the university. His Hungarian friend Eugene Wigner was still at the University of Technology studying chemistry. Wigner later came to Göttingen to study physics under Max Born, as did so many other physicists, including *Werner Heisenberg* (1901-1976), *Wolfgang Pauli* (1900-1958),

[*] After Baron *Lordnt von Eötvös* (1848-1919), a well-known Hungarian physicist.

John von Neumann and later on Edward Teller. The most prominent professors were limited to accepting only a few students on their courses, but Szilard was noticed by Einstein, Planck and von Laue.

Szilard had already asked von Laue for a subject for a doctoral thesis in 1921, and von Laue may have been testing Szilard when he suggested a relativity theory problem. However, when Szilard began attacking it around Christmas time, his thoughts moved on to other questions and he began working with something completely different. He certainly created an original manuscript, but he dared not show it to von Laue since it was not what the latter had asked for. Instead, he consulted Einstein, who said: ‘That’s impossible. This is something that cannot be done.’ And Szilard’s response was: ‘Well, yes, but I did it.’

Following a brief discussion, Einstein realised that Szilard was right. He had proven that the random movement of the gas molecules at thermal equilibrium could be derived in a classical way with no limiting atom model. Szilard then went to von Laue, who accepted the work as a doctoral thesis.

Post-war Berlin was a unique metropolis where the 1923 inflation meant that bank notes were given bigger denominations: thousand Mark, ten thousand, hundred thousand, millions, billions... and life was difficult, particularly for Szilard who was interested in café life and discussions. But he has summarised his memories of this period in just a few words: ‘Berlin at that time lived in the heyday of physics.’ Szilard was researching x-ray crystallography at the Kaiser Wilhelm Institute for Chemistry until 1925 when he was taken on as a private lecturer. Alone or together with Einstein, he applied for 29 patents between 1924 and 1934, most of which concerned new ideas for refrigerators. For that reason, he became consultant to the major electrotechnical company AEG (Allgemeine Electrizitäts-Aktiengesellschaft) in Berlin.

On 5 January 1929, Szilard applied for a patent for a cyclotron-like accelerator. *Ernest Lawrence* (1901-1958) in California had the same idea but not until April 1929, and actually also succeeded in building a cyclotron in the following year, for which he won the Nobel Prize in Physics in 1939.[*]

In 1929, Szilard travelled to England to meet author *H.G. Wells* (1866-1946) as he had been influenced by Wells’ work *The Open Conspiracy*. He hoped to obtain the central European right to Wells’ books.

Szilard was an original person to say the least. The author and physicist *C.P. Snow* (1905-1980)’s words describe him as follows: ‘He had a powerful ego and invulnerable egocentricity: but he projected the force of that personality outward, with beneficent intention toward his fellow creatures.’ The French microbiologist *Jacques Monod* (1910-1976) has written that Szilard was ‘as generous with his ideas as a Maori chief with his wives’. The author *Richard Rhodes*’ opinion was: ‘To the end of his life he made dull men uncomfortable and vain men mad.’. His colleague Edward Teller has written that ‘Leo Szilard [...]never would be caught saying or doing anything that was expected of him.’.

Influenced by H.G. Wells, Szilard began to become more and more agitated about the future of the world. This led him to propose ‘Der Bund’ (‘The Alliance’) which would gather the ‘best’ individuals at an early age to train them in independent thinking to produce a class of intellectual leadership with inner cohesion, an elite that could renew itself. Szilard’s thinking was that ‘It was not supposed to be something like a political party, but rather it was supposed to represent the state’. He would end up toying with the idea of The Alliance all his life and covertly discuss it in his final book, *The Voice of the Dolphins and Other Stories* in 1961.

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[*] A cyclotron is a particle accelerator consisting of a vacuum chamber in a strongly magnetic field that is perpendicular to the motion path of the accelerating particles. The particles, which consist of ionised atoms, are then deflected so that they would have moved in a circular orbit had they had the same kinetic energy all the time. The particles move between two flat, parallel, circular-shaped electrodes that are each divided into two D-shaped halves separated by an aperture. Above the aperture is an electrical potential difference which increases the velocity of the particles each time they pass the aperture. When they have travelled one additional half a revolution, the field between the electrode halves has changed direction so that they are once again accelerated to a faster speed and, after N revolutions, they have been accelerated to the energy 2NE if E is the added energy given to the particles when they pass the aperture. The orbital of the particles in the classical cyclotron is independent of their speed, which means that the change in potential can take place at a constant frequency. On the other hand, the orbital radius of the particles increases with their speed so that they will illustrate a spiral-shaped path with an even larger radius. At high particle speeds, corresponding to velocities of more than 10-15 MeV for protons, the relativistic mass increase cannot be neglected and the orbital is no longer constant. This can be compensated for with the help of a synchronous change to the potential change frequency. The accelerator is then called a synchrocyclotron but can then only accelerate “bursts” of particles. The greater energy that can be obtained with a synchrocyclotron is therefore obtained at the price of a lower ion flow.

In 1932, Szilard took notice of H.G. Wells' book from 1914, *The World Set Free*. Szilard has written about the book: 'Wells describes the liberation of atomic energy on a large scale for industrial purposes, the development of atomic bombs, and a world war which was apparently fought by an alliance of England, France, and perhaps including America, against Germany and Austria, the powers located in the central part of Europe. He places this war in the year 1956, and in this war the major cities of the world are all destroyed by atomic bombs'.

However, Szilard was influenced even more by what a friend said to him regarding the need to save the world:

> He said that Man has a heroic streak in himself. Man is not satisfied with a happy idyllic life: he has the need to fight and to encounter danger. And he concluded that what mankind must do to save itself is to launch an enterprise aimed at leaving the earth. On this task he thought the energies of mankind could be concentrated and the need for heroism could be satisfied. I remember very well my own reaction. I told him that this was somewhat new to me, and that I really didn't know whether I would agree with him. The only thing I could say was this: that if I came to the conclusion that this was what mankind needed, if I wanted to contribute something to save mankind, then I would probably go into nuclear physics, because only through the liberation of atomic energy could we obtain the means which would enable man not only to leave the earth but to leave the solar system.

In 1932, Szilard moved to 'Harnack House' at the Kaiser Wilhelm Institute in Berlin-Dahlem, a building paid for by German industry and intended for visiting scientists, a sort of University club. Szilard was used to living in rented rooms with whatever fitted into a couple of suitcases as his only belongings. His suitcases were packed but their contents were accessible. He always carried the keys with him – if anything happened, all he had to do was turn them and leave, he would say. In Dahlem, he visited Lise Meitner and asked if he could do nuclear physics experiments on her premises, but Hitler came to power on 30 January 1933, and on 27 February he burned down the parliamentary building for which the Nazis blamed the Jews. Szilard was pessimistic. His Hungarian friends reassured him, saying that 'they all thought that civilized Germans would not stand for anything really rough happening'. But on l April, Jews were abused on the streets of Berlin, so Szilard turned the keys of his suitcases and carried them to the railway station where he bought a ticket to Vienna. The train was almost empty. The next day, the same train was overflowing, and the Nazis were bullying all those who wanted to leave the country. Szilard said afterwards: 'This just goes to show that if you want to succeed in this world you don't have to be much cleverer than other people, you just have to be one day earlier.'

Szilard continued his escape, and in May he came to England and began frantically looking for places of refuge for other fleeing scientists. In September 1933, the British Association for the Advancement of Science held its annual meeting. The newspapers referred to a lecture by *Lord Rutherford* (1871-1937) on nuclear fission. The first nuclear fission by human beings was performed in 1932 by *John Cockcroft* (1897-1967) and *Ernest Walton* (1903-1995) when they had accelerated the nuclei of hydrogen atoms (protons) to sufficiently high energies that they succeeded in using them as projectiles to split the nuclei of lithium atoms so that these disintegrated into two helium atom nuclei. The disintegration developed an extremely large amount of energy (in atomic terms), but Rutherford had explained to those who were hoping that the discovery could be used for the purpose of practical energy production that for each hit that led to nuclear fission, you were forced to waste energy on the acceleration of protons that never hit. There was therefore no positive net energy yield. Rutherford had now repeated this view and his conclusion was described in the newspaper reference with the sentence: 'It was a very poor and inefficient way of producing energy, and anyone who looked for a source of power in the transformation of the atoms was talking moonshine'.

The views of experts who said that something was impossible always irritated Szilard. Had Rutherford forgotten that in the same year that Cockcroft and Walton had fissioned an atom, another of his colleagues, *James Chadwick* (1891-1974), had demonstrated the existence of the neutron? If you used neutrons instead of protons as projectiles, thought Szilard, you did not need to waste the projectiles in the same way. The neutrons have no electric charge to repel them from the atomic nuclei. They would therefore have no difficulty in reaching the atomic nuclei which would be fissioned. And if when they were fissioned a few new neutrons were formed, which in turn could fission other atomic nuclei, you would obtain a chain reaction, 1, 2, 4, 8, 16, 32, 64, 128, 256 ... until such time as an enormous number of nuclei were fissioned. It was simply a matter of finding the right element to fission. However, Szilard remembers: ‘I didn’t see at the moment just how one would go about finding such an element, or what experiments would be needed, but the idea never left me. In certain circumstances it might be possible to set up a nuclear chain reaction, liberate energy on an industrial scale, and construct atomic bombs’.

Szilard continued to ruminate about this possibility. At the same time, he looked for someone who could finance his research. In spring 1934, he wrote to *Sir Hugo Hirst* (1863-1943), founder of the British General Electric Company. He enclosed a copy of The World Set Free and complained that scientists such as Rutherford considered the practical use of atomic energy to be impossible. He wrote: ‘I have reason to believe that insofar as the industrial applications of the present discoveries in physics are concerned, the forecast of the writers may prove to be more accurate than the forecast of the scientists’.

On 28 June and 4 July 1934, Szilard applied for new patents concerning the ‘the liberation of nuclear energy for power production and other purposes’. He described a nuclear reaction based on neutrons and also the need for a certain minimum mass, the ‘critical mass’, of the fissile material. He also proposed that the critical mass could be reduced if you surrounded the fissile material with ‘some cheap heavy metal, for instance lead’ which would reflect back neutrons that would otherwise have been lost (a device that would later be called a ‘tamper’ from the English verb ‘to tamp’, e.g., a blast hole, and comes from the French word ‘tampon’). He continued with ‘If the thickness is larger than the critical value [...], I can produce an explosion’. Szilard had thus given a fair description of the principle for an atomic bomb, but he still did not know which of the atoms the fissile one could be. To all appearances, uranium did not occur to him at all, but he was thinking primarily of the light element beryllium. But it was obvious that Szilard was convinced that the atomic bomb would become a reality sooner or later.

For experiments, Szilard needed a source of radiation and he thought he would be able to find one at a hospital. He approached the head of physics at Saint Bartholomew’s Hospital in London (close to Saint Paul's Cathedral) who agreed to allow him to borrow radium for experiments occasionally on condition that he took one of the medical physicists as a colleague. The person in question was *T.A. Chalmers*.

That same spring, Szilard had asked Rutherford if he could work at the Cavendish Laboratory in Cambridge, but Rutherford had declined. He was now able to experiment at Saint Bartholomew’s. He has written:

> These experiments established me as a nuclear physicist, not in the eyes of Cambridge, but in the eyes of Oxford. I had never done work in nuclear physics before, but Oxford considered me an expert. […] Cambridge […] would never had made this mistake. For them I was just an upstart who might make all sorts of observations, but these observations could not be regarded as discoveries until they had been repeated at Cambridge and confirmed.

Rutherford was clearly not Szilard’s favourite person.

However, beryllium was not a good candidate for a fissile substance. The fact that Szilard had thought this was because he had erroneous information on the mass of the helium atom. Stable beryllium can be seen as two helium atoms that are united by a neutron. *Francis Aston* (1877-1945), the inventor of the mass spectrograph, had used his instrument to determine the mass of helium, but had obtained incorrect results, which was subsequently proven by Rutherford. Beryllium therefore had no such surplus mass as Szilard had believed and was therefore too stable to be easy to split. Which other candidates were there? Szilard answered the question in a new patent application on 9 April 1935: ‘Other examples for elements from which neutrons can liberate multiple neutrons are uranium and bromine’. This was very probably a pure guess.

Szilard’s many contacts in England included *F.A. Lindemann* (Lord Cherwell, 1885-1957), who was head of the Clarendon Laboratory at Oxford and who had been a close friend of Winston Churchill since 1921. Lindemann helped Szilard to gain a foothold at Oxford. On 3 June 1935, Szilard wrote to Lindemann about the possibility of releasing nuclear energy in the near future: ‘[...] it is certainly less bold to expect this achievement in the immediate future than to believe the opposite'. Szilard thought that it would be bad if someone in Germany was the first to obtain a chain reaction and that this was necessary to influence the development.

English scientists did not trust Szilard due to his many patents. His patent applications were seen as an attempt to achieve personal gain in conflict with the tradition of research results being public property. Szilard explained to Lindemann that the patents were needed to be able to stop the abuse of the usufruct of 'double neutrons', i.e., the neutron multiplication that was necessary for a chain reaction. However, the very existence of the patent applications could suggest possible enemies. Lindemann advised Szilard to keep the patents secret by offering them to a British authority. Szilard offered them to the War Department, which rejected the offer on 8 October 1935, saying that 'there appears to be no reason to keep the specification secret so far as the War Department is concerned'. In February 1936, Szilard was assisted by Lindemann, who wrote to the Admiralty, arguing that 'even if the chances were a hundred to one against it seems to me it might be worth keeping the thing secret as it is not going to cost the Government anything'. Lindemann enclosed a letter from Szilard in which the latter wrote that the patents contained information on explosives that were many thousands of times more powerful than ordinary bombs and that he was concerned about 'the disasters which could be caused by their use on the part of certain Powers which might attack this country'. The Admiralty agreed to keep the patents in secure storage.

Rutherford's conviction that the liberation of nuclear energy would cost a greater energy input than the one that was gained was often quoted. It is known that Rutherford met Bohr and Heisenberg in Copenhagen in spring 1936 and then repeated his view that 'all those who speak of the technical exploitation of nuclear energy are talking moonshine'. However, it is also known that on one occasion, Rutherford told the Cabinet Secretary *Sir Maurice Hankey* (born in 1877), one of the United Kingdom's most prominent public officials, that the nuclear physics research at the Cavendish Laboratory could become very significant to the army and that the government ought to keep an eye on the development. It is not impossible that Rutherford believed more than he let on and that the pessimism he demonstrated was feigned owing to mistrust of the Germans' views.

Szilard became more and more certain that the world war would come, and he felt uncertain, although Lindemann had succeeded in arranging a grant for him to do research at Oxford from 1935. In March 1936, he wrote to *Gertrud Weiss* in Vienna, a woman who would later become his wife, saying that she ought to emigrate to America. He wrote to a friend in England, saying that he intended to remain in Oxford until one year before the war but then travel to New York. His friend wondered how Szilard could know when the one year *before* the war would be - but Szilard seemed to know.

In August 1937, he wrote a letter to financier *Lewis Strauss* in New York to establish his interest in supporting the development of a shock generator for the production of radioactive nuclides.* Szilard reckoned it was possible to achieve sufficiently high voltages to accelerate charged particles to projectiles for nuclear reactions. The idea was not realistic, but it gave Szilard the option of contacting the man who would later end up supporting him financially.

On 2 January 1938, Szilard arrived in New York, but not with the intention of staying there - there was more than a year to go until the war. With the support of Strauss, he experimented with all sorts of ideas in addition to the shock generator, including killing trichinae and insect larvae with ionising radiation, pioneering work regarding the irradiation of foods (the later research into the irradiation of foods was not started until the 1950s when the search got underway for ways to use radioactive waste from nuclear reactors). He utilised his medical physicist contacts and, in April 1938, had things like cigars irradiated at Montefiori Hospital with the help of *Dr. Maurice Lenz*, who ensured that they were exposed to approximately 1000 röntgen (corresponding to an absorbed dose of just under 10 gray) from an x-ray apparatus. The intention was to test whether the cigars deteriorated by being irradiated, which they did not seem to have done - 10 gray is a low dose in this context.

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* The idea of a shock generator is that you charge up N lots of electrical condensers, parallel-coupled through a high-ohm resistance, to the voltage of V volt to earth and then suddenly connect them in series through discharges over a spark gap to obtain a current impulse at a voltage of NxV volt.

In September 1938, Szilard travelled to the University of Illinois in Champaign, 200 km south of Chicago, to visit a colleague from England, *Maurice Goldhaber*, who had just come to the United States and taken up a Professorship in Physics at the university. In 1937, Goldhaber and Szilard had begun a series of experiments with indium as a possible fissile element, and Szilard wanted to resume the cooperation. Szilard's visit took place at the same time as the Munich crisis over (the former) Czechoslovakia, and the danger of war in Europe appeared to be imminent. Goldhaber purchased a radio so that he and Szilard could follow the development. Hitler demanded the Sudetenland from Czechoslovakia and had stated 28 September as the deadline before his troops would march in. Chamberlain, Daladier, Hitler and Mussolini met in Munich at the last moment. Hitler assured them that this was his last territorial demand in Europe. Chamberlain and Daladier gave way. Churchill has described the British and French governments at this time as 'two overripe melons crushed together; whereas what was needed was a gleam of steel'. Chamberlain returned triumphantly to London and pronounced the notorious words 'peace for our time'.

Szilard listened to the radio and knew that the time had come. There was now one year before the war and he would now act on his decision to leave Europe for good. He sent the following telegram to Lindemann in Oxford:

> HAVE ON ACCOUNT OF INTERNATIONAL SITUATION WITH GREAT REGRET POSTPONED MY SAILING FOR AN INDEFINITE PERIOD STOP WOULD BE VERY GRATEFUL IF YOU COULD CONSIDER ABSENCE AS LEAVE WITHOUT PAY STOP WRITING STOP PLEASE COMMUNICATE MY SINCERELY FELT GOOD WISHES TO ALL IN THESE DAYS OF GRAVE DECISIONS
>
> SZILARD

At the end of the year, Szilard was living in New York at the King's Crown hotel by West 116th Street right opposite Columbia University and close to Harlem. He had begun to doubt whether it would be possible to achieve chain reactions from nuclear fission and wrote a dejected letter to the British Admiralty: '[…]it does not now seem necessary to maintain the patent […] nor would the waiving of the secrecy of this patent serve any useful purpose. I beg therefore to suggest that the patent be withdrawn altogether'.

2. THE EXTRA NEUTRONS

SZILARD WOULD MEET Enrico Fermi in January 1939 at the King's Crown. In December 1938, Fermi had been in Stockholm with his family and received that year's Nobel Prize in Physics. He had no intention of returning to Italy since his wife was Jewish and it would have been a risk to her life had they returned. Instead, they went to New York where they arrived on 2 January. They temporarily put up at the same hotel where Szilard was living. Two weeks later, on Monday 16 January, Swedish America Line's 'Drottningholm' called at Manhattan harbour, bringing with her Niels Bohr and his colleague *Leon Rosenfeld* (1904-1974). The Fermi family stood waiting on the quay alongside the young *John Wheeler* (1911-2008), an American physicist who had previously worked for Bohr in Copenhagen.

Bohr brought the news that Lise Meitner and Otto Robert Frisch had confirmed that Otto Hahn's and Fritz Strassmann's unexpected find of an element which had the properties of barium was actually barium and that it had been formed when the uranium atom was split when irradiated with neutrons.

On the Monday afternoon of 6 January, John Wheeler accompanied Rosenfeld to Princeton, the renowned university in New Jersey, a one-and-a-half hour's train journey from Manhattan on the way towards Philadelphia. And in the evening, Rosenfeld told the physicists in Princeton of Hahn's and Strassmann's discovery and of Meitner's and Frisch's interpretation.

On the Tuesday, Niels Bohr came to Princeton having spent his Monday evening with the Fermi couple. Unlike Rosenfeld, Bohr had said nothing about the discovery because he had promised Frisch that he would wait until Hahn's and Strassmann's report was published in *Naturwissenschaften* and the article that Frisch and Meitner had written had been sent to *Nature*. Therefore, unlike the physicists in Princeton, Fermi still knew nothing of the possibility of nuclear fission. Over the next few days, Bohr quickly wrote his own letter to *Nature* on the theoretical considerations that he and Rosenfeld had discussed on the boat journey. They had searched for the answer to the question as to why the uranium atom had not disintegrated into small particles rather than two approximately equal parts when it was hit by a neutron.

At the end of the week, some of the scientists returned from Princeton to New York. One of these was *Isidor Isaac Rabi* (1898-1988), a Polish-born physicist who had studied in Germany and was now Professor of Physics at Columbia University. He was in the company of *Willis Lamb* (born in 1913), who was also from Columbia University. Both would go on to receive the Nobel Prize in Physics. It was these two who told Fermi what Bohr had said.

The news came as a shock to Fermi. It suddenly made it clear to him exactly what he had been doing while he had been searching for transuraniums and the elusive element that he had called 'element 93'. But, what was worse, he had received the Nobel Prize in Physics in the previous month for his discoveries. His Nobel lecture, which had not yet left the press, was suddenly out of date and had to be revised. Fermi added a footnote: 'The discovery by Hahn and Strassmann [...] makes it necessary to re-examine all the problems of the transuranic elements, as many of them might be found to be products of a splitting of uranium'.

Another physicist got to hear the news in his sickroom in Princeton. It was Szilard's compatriot Eugene Wigner, who was Professor of Physics at Princeton University but who had not been able to catch Rosenfeld and Bohr because he had been admitted to hospital for jaundice. Szilard himself did not hear the news until he visited Wigner at the hospital. His account is as follows:

> Wigner told me of Hahn's discovery. Hahn found that uranium breaks into two parts when it absorbs a neutron [...] When I heard this I immediately saw that these fragments, being heavier than corresponds to their charge, must emit neutrons, and if enough neutrons are emitted [...] then it should be, of course, possible to sustain a chain reaction. All the things which H. G. Wells predicted appeared suddenly real to me.

Szilard, who was moving in and out of rooms with his suitcases, was able to borrow Wigner's apartment. He visited Princeton University, but a downpour soaked him and caused him a severe cold so he knew that he ought really to stay indoors, although he would have preferred to have travelled back to New York to warn Fermi. He also thought that he needed to warn *Frédéric Joliot* (1900-1958) in Paris. These two impatient experimentalists could be expected to demonstrate the extra neutrons and, if they did, it was important that the Germans did not get to hear about it. Therefore, in spite of his cold, Szilard returned to New York and the King's Crown. Because he had a high temperature, he did not dare to look up Fermi. Instead, he started writing a warning letter to Lewis Strauss since he had now also read Hahn's and Strassmann's report in *Naturwissenschaften*. The letter began:

> I feel I ought to let you know of a very sensational new development in nuclear physics. In a paper […] Hahn reports that he finds when bombarding uranium with neutrons the uranium breaking up. […] This is entirely unexpected and exciting news for the average physicist.

After having said that the discovery made it theoretically possible to extract power from nuclear energy, but not believing that this would ever be economically viable (maybe he was thinking of car and boat engines), Szilard continued:

> I see […] possibilities in another direction. These might lead to large-scale production of energy and radioactive elements, unfortunately also perhaps to atomic bombs. This new discovery revives all the hopes and fears in this respect which I had in 1934 and 1935, and which I have as good as abandoned in the course of the last two years. At present I am running a high temperature and am therefore confined to my four walls, but perhaps I can tell you more about these new developments some other time.

Szilard was thus forced to remain inactive as regards his decision to look up Fermi, but Fermi was even more active. As Szilard had guessed, Fermi was anxious to start experimenting with nuclear fission as soon as possible. He had therefore gone to Pupin Hall*, a thirteen-storey building at Columbia University intended for the physics institutes. On the basement floor, the physicists had built a cyclotron. The physicists whom Fermi approached were neutron expert John R. Dunning and the latter's assistant, *Herbert Anderson* (1914-1988). The experiment that the three men planned was principally the same as the one that Frisch had carried out when he returned to Copenhagen following the discussion with Lise Meitner over Christmas in 1938. Frisch had placed a neutron source and uranium in an ion chamber and registered the powerful current pulses owing to the nuclear fission using an oscilloscope.

In the meantime, Bohr was also looking for Fermi, who had not been in Princeton, to tell him about the remarkable discovery, not knowing that Fermi had already heard about it from Rabi and Lamb. But Bohr did not succeed in finding him and was forced to take the train to Washington DC where he would pay a visit to a physics conference that had been organised by *George Gamow* (1904-1968) and Edward Teller.

Teller, who would go on to be called 'the father of the hydrogen bomb', was another one of the prominent Jewish emigrants from Hungary. His father was a lawyer, but the family had no wealth to speak of. Edward, who was born in 1908, went to school in Budapest during Bela Kun's attack on the bourgeoisie and went to high school under Horthy's anti-Semitic regime. His father gave him two pieces of advice: (1) he ought to emigrate to a more suitable country as soon as he became an adult, and (2) since he belonged to an unpopular minority, he would be obliged to be above average in order to be accepted.

Following an anxious childhood full of horror, Edward left Hungary at the age of seventeen and registered at the one-hundred-year-old technical university in Karlsruhe. He moved from there to Munich

* Named after *Michael Pupin* (1858-1935), a Hungarian-American technologist who in 1901 became Professor of Electrotechnology at Columbia University. Pupin invented the Pupin spools which, when incorporated into telephone cables, revolutionised the development of long-distance telephony.

in 1928 to study with *Arnold Sommerfeld* (1868-1951), the man who educated a significant number of the pre-war physicists, including von Laue, Pauli and Heisenberg. When Teller came to Munich, Sommerfeld was also supervising chemist *Linus Pauling* (1901-1994) and physicists *Hans Bethe* (1906-2005) and Polish-born I.I. Rabi. Bethe was an energetic young man with blue eyes and an infectious laugh, but his mother was Jewish, and he would therefore be affected by the Nazis' 're-establishment' of the university services.*

In Munich, Teller ended up beneath a tram and had to have his right foot amputated. To top it all, Sommerfeld was temporarily away from Munich on a round-the-world trip to celebrate his 60th birthday. Teller decided to leave Munich and moved to Leipzig to continue his studies with Heisenberg, where he graduated with a doctorate.

He then spent some time in Göttingen, but when the situation deteriorated even further for Jews in Germany, he applied for a Rockefeller grant so that he could work with Bohr in Copenhagen. He stayed there for eight months, and in February 1934 he married his childhood sweetheart, *Mici Harkanyi*. Through Bohr he met George Gamow who, along with his wife *Rho*, succeeded in escaping the Soviet Union by participating in a Solvay Conference in Brussels.

From Copenhagen, Edward and Mici moved to England in summer 1934 with a view to settling there. However, in January 1935, Gamow came up with an offer: he wanted the Tellers to accompany him and Rho to the United States and was able to promise Edward a Professorship at George Washington University. This was too tempting an offer for the young Teller, who was not yet 27. The two young couples boarded the boat to America in August 1935.

It just so happened that Teller was in situ and, along with Gamow, was responsible for the physicists' conference in Washington DC that Niels Bohr was on his way to on Wednesday 25 January 1939†. The actual subject of the conference was low temperatures, but Bohr expected to find the prominent physicists in the United States gathered there. As soon as Bohr had arrived, he sought out Gamow and told him the news of Hahn's nuclear fission. Gamow in turn rang Teller and said: 'Bohr has just come in. He has gone crazy. He says a neutron can split uranium'.

On the same afternoon, Fermi in New York, along with Dunning and Anderson, tried to use the cyclotron in Pupin Hall for their experiment but it did not work. They were forced to discontinue their experiment since Fermi was also going to Washington DC. However, they did manage to ascertain that there was a radon-beryllium preparation that could be used as a neutron source the next day.

In Washington DC, Teller started thinking. He was quite sure that Niels Bohr could not possibly be mad - he must have meant what he had sad. Teller remembers:

> The next day, as Bohr talked at the conference about a subject very different from low temperatures, I knew what was coming. He explained in detail that when a neutron hits the nucleus of a uranium atom, the entire nucleus is split into two pieces, and the two fragments are forced away from each other with a tremendous velocity.
>
> Although I had been prepared for Bohr's description of fission, I was completely unprepared for the reaction of one scientist at the conference. Obviously concerned, he took me aside: 'Let's be careful. Let's not talk about this too much.' I agreed and concentrated on returning the conference to the subject of low temperatures.

Another memory from the same conference has been supplied by *Richard Roberts* (1910-1980), a physicist who worked at one of the Carnegie Institution's research laboratories in Washington DC, the Department of Terrestrial Magnetism (DTM):

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* In spring 1933, Bethe got a job with Hans Geiger at the University of Tübingen, but he was soon forced to leave it when Geiger explained in a short letter that the job was over. Bethe emigrated to the USA in 1935 to start a job at Cornell University.

† The Solvay Conferences were arranged every three years at the Solvay Institute for Physics and Chemistry in Brussels. The Institute was founded in the 1890s by Belgian industrialist *Ernest Solvay* (1838-1922), who is known as the inventor of the Solvay process for the production of soda.

> The […] conference […] was on the topic of low temperatures and I was not eager to attend. However, I went down to sit in the back row of the meeting. […] Bohr and Fermi arrived and Bohr proceeded to reveal his news concerning the Hahn and Strassmann experiments. […] He also told of Meitner's interpretation that the uranium had split. As usual he mumbled and rambled so there was little in his talk beyond the bare facts. Fermi then took over and gave his usual elegant presentation including all the implications.

Fermi also told how Meitner's theory could be confirmed and described the experiment he intended to perform with Dunning and Anderson, without knowing that Frisch had already carried out the same experiment in Copenhagen. Bohr, who did not know whether Frisch had yet published his results, did not dare to say anything, but Roberts and a colleague rapidly left the conference. If nobody else had yet carried out the experiment, they could be the first to do so.

As Bohr had expected, there was a distinguished gathering of physicists at the conference on 26 January, particularly Bethe, Bohr, Fermi, Gamow, Rabi and Teller. It is remarkable that all of these came from Europe. The physicists from the west coast were absent, however - they had probably had difficulties getting their travel paid for. And one other European had registered to be there, but Szilard was lying in his hotel room in New York with a high temperature. But not for long. That very same day he dragged himself to the nearest telegraph office to telegraph the British Admiralty: 'KINDLY DISREGARD MY RECENT LETTER STOP WRITING'.'

At the Carnegie Institution's Department of Terrestrial Magnetism (DTM) there was a new building called the 'Atomic Physics Observatory'. The facility was located in the Chevy Chase area north of Washington DC, a suburb of distinguished villas. In situ was a 5-million-volt Van de Graaff generator which was enclosed in an enormous pressure tank to reduce the risk of electrical flashover. The man who was responsible for the facility was a physicist by the name of *Merle Tuve* (1901-1982). When he had originally requested planning permission for the large building that housed the high-voltage generator, he had been refused because they did not want any technical plants in the area, so he called the building an 'observatory' and it went ahead without problems.

On Saturday 28 January 1939, after a number of technical problems, Roberts and his assistant succeeded in irradiating uranium with neutrons and in using an ion chamber to produce the extremely powerful current pulses that could be obtained only from the energy-rich nuclear fragments following the splitting of uranium. Roberts reported the results to Tuve in the afternoon, unaware that Otto Frisch had already performed the same experiment, and Tuve rang Bohr and Fermi. They both came out to the 'observatory' in the evening along with a number of other physicists to celebrate what everyone except for Bohr believed was the first physical confirmation of the nuclear fission phenomenon. A photograph of the group was taken in the irradiation room beneath the accelerator. Fermi rushed back to New York and Columbia University's Pupin Hall with a list of continued experiments that he was eager to get started on.

The news of nuclear fission had now also reached the west coast of America. The late Nobel Prize winner *Luis Alvarez* (1911-1988) read about it in the *San Francisco Chronicle* while sitting in a hairdresser's salon. He hurried to the university's radiation laboratory where one of his students, *Phil Abelson* (1913-2004), was irradiating uranium with neutrons in the hope of transforming it into a heavier element. Abelson has described that morning:

> About 9:30 a.m. I heard the sound of running footsteps outside, and immediately afterward Alvarez burst into the laboratory. […] When [he] told me the news, I almost went numb as I realized that I had come close but had missed a great discovery. […] For nearly 24 hours I remained numb, not functioning very well. The next morning I was back to normal with a plan to proceed.

The next day, Abelson was able to demonstrate a new fission product - so one of the nuclear fragments did not always have to consist of barium. Abelson found iodine, which he could demonstrate was a daughter product of radioactive tellurium, and that in this case the atom fragments had been tellurium (atomic number 52) and zirconium (atomic number 40). He ascertained that 52 + 40 = 92, which is the atomic number of uranium.

Alvarez also visited *Robert Oppenheimer* (1904-1967), the physicist who would go on to be called ‘the father of the atomic bomb’, and told him the news. Oppenheimer declared first of all that it was physically impossible, but soon changed his mind when he witnessed the experiment for himself. Alvarez says:

> When I invited him over to look at the oscilloscope later, when we saw the big pulses, I would say that in less than fifteen minutes Robert had decided that this was indeed a real effect and […] he had decided that some neutrons would probably boil off in the reaction, and that you could make bombs and generate power, all inside of a few minutes.

Oppenheimer told another colleague that he did not consider it unlikely that a 10-cm cube of uranium with heavy hydrogen (deuterium) there to slow down the neutrons could ‘blow itself to hell’. In just a few days, Oppenheimer’s doctoral students began sketching atomic bombs on the blackboard in the seminar room. However, as Oppenheimer immediately realised, everything depended on whether nuclear fission generated a surplus of new neutrons. And no-one had realised it as early as Szilard.

When in their home in Washington DC after the end of the conference, Edward and Mici Teller were ‘ready to collapse under the strain of our social-scientific burdens as hostess and host of the meeting’, the telephone rang. It was Szilard, who had recovered from his cold. ‘I’m at the railroad station,’ he said. ‘Can you pick me up?’. Mici protested. ‘We are both much too tired’, she told Edward. ‘He must go to a hotel.’ But they drove to the station and met Szilard, who asked Teller if he had heard Bohr’s lecture on nuclear fission, and did he realise what it meant?

The actual fission process could perhaps generate more neutrons, said Szilard. They could in turn split other nuclei. This could produce a chain reaction that released a fantastic amount of energy. We need to find out whether more neutrons are created, concluded Szilard. ‘Hitler’s success could depend on it!’

Szilard waited anxiously to see whether Fermi would demonstrate the extra neutrons. If Fermi could do it, so could the Germans. What would happen when the eager Fermi published his observations? And, in France, the equally eager experimentalist Frédéric Joliot was doubtless also on the way towards publishing something. The German scientists would receive a great deal of free information and get way too good a head start towards the atomic bomb which Szilard was sure they would do their best to produce. He told Rabi of his fears, Rabi being a Professor at Columbia University and someone who ought to be able to influence Fermi. Ask Fermi, he said, to jolly well hurry up with his experiments but not to publish anything. Fermi’s response to Rabi with a shake of the head was a monosyllabic ‘Nuts!’.

Rabi asked Fermi to explain himself, whereupon Fermi’s response was that there was a very remote possibility of some extra neutrons being created but, if that did happen, there could be a chain reaction.

> ‘What do you mean by “remote possibility”?’ asked Rabi.
>
> ‘Well, ten per cent’, responded Fermi.
>
> ‘Ten per cent is not a remote possibility’, said Rabi, horrified, ‘if it means that we may die of it. If I have pneumonia and the doctor tells me that there is a remote possibility that I might die, and it’s ten percent, I get excited about it!’

Szilard and two of his colleagues, *Victor Weisskopf* (1908-2002) and Eugene Wigner tried in vain to stop the publication of papers that told of secondary neutrons. On 2 February 1939, Szilard sent a letter to Joliot in Paris and begged for restraint. He wrote: ‘Obviously if more than one neutron was liberated [in the disintegration of uranium], a sort of chain reaction would be possible. In certain circumstances this might then lead to the construction of bombs which would be extremely dangerous in general and particularly in the hands of certain governments’.

But Joliot and his colleagues did not want to abandon the possibility of publishing and thereby securing priority regarding the results of the experiments they were doing.

In New York, Fermi, whose Nobel Prize had afforded him quite some status, arranged laboratory space for Szilard and a Canadian physicist, *Walter Zinn* (1906-2000), with whom Fermi had become acquainted at Pupin Hall. Szilard was accepted as a guest researcher for a three-month period. So, two groups ended up at Pupin Hall searching for the extra neutrons at the same time. Fermi and Anderson, with the help of an arrangement consisting of a large water tank into which uranium and a neutron source were lowered, and Szilard and Zinn with an assembly consisting of paraffin around a neutron source in the form of a beryllium cylinder in which there would be a radium capsule. The immediate difficulties were that Fermi's radon-beryllium preparation proved to be unsuitable, and Szilard's radium preparation did not yet exist. He needed a couple of grammes, and radium was expensive.

Meanwhile, Bohr was still thinking in Princeton where he and Rosenfeld intended to stay for a few months. On 5 February 1939, they were joined in the university's club room by *George Placzek*, an exiled Czech physicist who had just come to the United States from Copenhagen. When Bohr said that all stories of transuraniums following the irradiation of uranium with neutrons had now finally been put paid to because it had probably been a question of fission products such as barium and lanthanum all along, Placzek protested. Some neutrons would always be able to generate transuraniums, he thought. But above all, he had doubts about Bohr's drop model for the atomic nucleus. Frisch had already shown in his initial experiment in Copenhagen that not only uranium but also thorium could be split with neutrons, but only if neutrons were 'fast', i.e., had high kinetic energy of the magnitude of millions of electron volts. Slow neutrons could not split thorium. Uranium, on the other hand, could be split by both fast and slow neutrons. How would Bohr explain the difference?

Bohr sat silently for a while, but appeared to come up with an idea and rushed headlong out of the club room without a word to the surprised Placzek. Rosenfeld followed Bohr to the university's building for advanced studies where Einstein had let Bohr use a room. It was actually Einstein's own work room, but he had found it too big and had withdrawn to a smaller room intended for secretaries. Bohr drew a few curves on the blackboard for Rosenfeld. Thorium and ordinary uranium, i.e., uranium-238, ought theoretically to behave in the same way, said Bohr. So, it was not uranium-238 that was split by the slow neutrons. What could it be then? Well, the natural uranium does not consist solely of uranium-238, but 0.7 % consists of another uranium isotope, uranium-235. Thorium consists of thorium-232. Uranium-238 and thorium-232 both have equal mass numbers. Uranium-235 on the other hand has an odd mass number and can be expected to react differently, which would explain why it could also be split by slow neutrons.

Bohr quickly wrote a paper on the significance of uranium-235 and sent it to *Physical Review* on 7 February 1939. He would write a more in-depth account over the next couple of months along with the young John Wheeler. Bohr's thinking difficulties were accompanied by intense use of the blackboard in Einstein's room and frenetic pipe-filling. However, the other physicists in Princeton and at Columbia University were not that impressed. Fermi refused to believe that uranium-238 was not the principal object of fission.

Roberts and his colleagues at DTM in Washington DC were very active in February and were able to report their investigations into neutrons that were released during nuclear fission in *Physical Review*. They had found that a small number of neutrons (less than 1%) were not given off immediately, but only after a few seconds. The significance of these delayed neutrons would not become evident until later on.

On 24 February, a journalist, *William Laurence* (1888-1977) from the *New York Times* paid a visit to a meeting of the American Physical Society at Columbia University. He ended up sitting next to John Dunning. When Fermi mentioned a 'chain reaction', Laurence Dunning asked how much more energy could be obtained from splitting uranium compared with TNT, and Dunning's response was 'more than a factor of 20 million'. How long would it take for a chain reaction to spread through one kilo of uranium, wondered Laurence. One millionth of a second was the answer he heard but Dunning emphasised that it had to be uranium-235 and that one kilo of uranium-235 was beyond their reach. After the meeting, Laurence succeeded in detaining Niels Bohr and Enrico Fermi for an informal chat about nuclear fission. To their dismay, he asked whether one kilo of uranium-235 could be used for an atomic bomb. Bohr and Fermi did not know how to respond. 'We must not jump to hasty conclusions', said Fermi after a while - 'it will take many years.'

> ‘How many?’
> ‘At least twenty-five, possibly fifty years’, answered Fermi.
> ‘Supposing Hitler decides that this may be the very weapon he needs to conquer the world?’, Laurence persisted. ‘How long then?’

After the war, Bohr and Fermi told Laurence that his questions had surprised and scared them. The secret was on the way to becoming public property. Laurence discussed the matter with his wife when he came home after the meeting and they took an evening walk to take their dog out (who, to crown it all, was called Einstein). The atomic bomb was a possibility. Would Hitler get one?

The most prominent scientists had different approaches to the fears. Fermi thought that there was no point worrying themselves for no reason. The important thing was to experiment. If the experiment indicated that there really was cause for concern, they would tackle the problem then.

Szilard worked on the basis that Hitler was in the process of procuring nuclear weapons and that the Americans would have to act accordingly. Bohr did not want to accept the danger. He devoted his energy to endeavouring to show that nuclear weapons were impossible. He worked on the basis of his assumption that a bomb had to consist of uranium-235. How could you produce kilos of pure uranium-235? Well, theoretically speaking, you *could* make an atomic bomb but in practice, the necessary separation of uranium-235 from uranium-238 would take up all of the country’s resources.

They would be obliged to convert the whole of the United States into one single enormous factory, said Bohr. The secrecy aspect was therefore unnecessary and most of the information had already leaked out.

Eugene Wigner on the other hand was influenced by Szilard’s approach and therefore agreed that it was important to get the American government to react. But as yet it was mainly just a notion and the danger existed only if a surplus of neutrons appeared as feared.

At the start of March 1939, Szilard actually got hold of the radium he needed and arranged the necessary experiment with Zinn. When everything was set up, all they needed to do was press a button and look at the oscilloscope screen. When it lit up, this meant that extra neutrons had been emitted. It lit up time after time and the two scientists sat for a while looking at the beams of light. ‘Then we switched everything off and went home’, Szilard wrote later. He then rang Teller in Washington DC. Teller recollects it as follows:

> I was at my piano, attempting with the collaboration of a friend and his violin to make Mozart sound like Mozart, when the telephone rang. It was Szilard, calling from New York. He spoke to me in Hungarian, and he said only one thing: ‘I have found the neutrons.’ I was unhappy about those neutrons. They presented, to me, an inescapable challenge. I guessed, then, that I would be unable to continue playing with theories.

On 16 March 1939, Hitler declared that Czechoslovakia had been occupied and there were now few who doubted his ambitions as a conqueror. Szilard and Wigner certainly did not at any rate when the two Hungarians and the Italian Fermi met the Dean of the Physics Section at Columbia University, *George Pegram* (1876-1958), on the same day at his office at the university. Pegram, then a strong man in his sixties, was the person who had made it possible for Fermi and Rabi to come to Columbia.

Above all, Szilard and Wigner wanted to hold restrained discussions on publications. Fermi had written a report on his observations of secondary neutrons and wanted to publish it with priority rights in mind. Szilard also had a report on his results which was ready for printing. Wigner also wanted Pegram’s help with getting a warning out to the government.

Fermi was going to travel to Washington DC in the afternoon anyway to give a lecture there in the evening, so he should be able to meet one significant person or another the next day. Pegram said that he knew *Charles Edison* (1890-1969), the Assistant Secretary of the Navy, and tried to get him on the telephone. However, Edison had gone away and Pegram was instead put onto *Admiral Stanford Hooper* (1884-1955), who was technical adviser to the Assistant Secretary of the Navy. Hooper promised to meet Fermi.

Before Fermi left, he and Szilard agreed on a compromise concerning the publication of the articles. They would both post their articles to *Physical Review* but ask the editor to keep them aside until the secrecy issue had definitively been solved. After having made this arrangement, Fermi went to Washington DC while Szilard and Wigner went to Princeton to meet Bohr, who was expecting them in Wigner's work room along with Rosenfeld and Wheeler, as well as Teller who had come up from Washington DC. Apart from Fermi, Bohr was the strongest objector to scientific secrecy, and the two Hungarians realised that they had to convince him to manage to keep dangerous information away from the German scientists. Szilard told him of his observations regarding an increase in the number of neutrons during nuclear fission. Some of the physicists drew very pessimistic conclusions, but Bohr was reluctant and repeated that it would be practically impossible to produce an atomic bomb because he could see no prospect of isolating sufficient quantities of uranium-235.

Fermi met Admiral Hooper in Washington DC the following day, Friday 17 March. The latter was in the company of some army officers and a few civil scientists linked to the Navy's research laboratory. Fermi gave them an hour's lecture on nuclear fission and on his results from the search for secondary neutrons. But everything was too abstract and vague for the military personnel. Only one civil physicist, *Ross Gunn* (1897-1966), realised the importance of what had been said. This first contact between nuclear physicists and the military therefore ended in empty, polite phrases and lead to no important action other than Ross Gunn seemingly having received 2000 dollars for nuclear fission research.

On the same day, Szilard and Teller continued from Princeton to Washington DC to meet Fermi over the weekend and continue the secrecy discussion. The two Hungarians begged Fermi not to have his article published in *Physical Review*. Fermi gave up in the end and said that, in the name of democracy, he had to accept being outvoted two to one. But this secrecy arrangement was already a thing of the past the following day, Saturday 18 March 1939, when *Nature* published a paper by Frédéric Joliot, *Hans von Halban* (1877-1947) and *Lew Kowarski* (1907-1979) on the presence of a surplus of neutrons following the splitting of uranium with neutrons. The protected secrecy was now in the hands of the public.

3. NAPOLEON’S SUCCESSOR

IN EUROPE, not that many people had drawn Szilard’s conclusions from Otto Hahn’s barium find and Lise Meitner’s interpretation that nuclear fission had occurred. Neither Otto Robert Frisch nor Meitner appears to have immediately wondered about the possibility of a chain reaction, and nor had Hahn himself continued to do research in that direction. Frédéric Joliot and his colleagues in Paris did though.

Frédéric Joliot-Curie is the name given to him in many reference books, but it was a name that he never officially took. He and his wife *Irène Curie* (1897-1956), Marie Curie’s daughter, did on the other hand sometimes sign their joint articles as ‘I. and F. Joliot-Curie’. While Irène was primarily a chemist, Frédéric was primarily a physicist. He had been Marie Curie’s assistant before becoming a Professor at College de France in 1937, the 400-year-old technical college in Paris that was completely independent from the university. That same year, Irène became a Professor at the Sorbonne.

Joliot was a prominent experimentalist and, in 1937-1938, was responsible for designing the first cyclotron to be built in Europe. The Joliot-Curie couple had been affected by a number of miscalculations, which meant that they had missed out on major discoveries like the neutron and nuclear fission. But they had also enjoyed successes by producing the first artificial radioactive substance in 1934, the short-lived phosphorus-30. Hahn-Meitner’s discoveries would naturally spur them on, particularly the physicist Joliot, to continue to experiment with irradiating uranium with neutrons.

So, Joliot, like Fermi and Szilard, demonstrated the surplus of neutrons during nuclear fission and drew the important conclusion that a chain reaction therefore ought to be possible. He had help with this work from the Austrian Hans von Halban and the big Russian Lew Kowarski.

Together, they published two articles in *Nature* about their results and conclusions. The first came on 18 March 1939, the day after Fermi, Szilard and Teller had agreed to keep the same discovery secret in order to prevent it from being misused by the German scientists. On 5 April, the Parisian scientists telegraphed Szilard to say that his appeal for secrecy had come too late and that the rumour of the American experiment had necessitated further work on their part. The second article on ‘the number of neutrons liberated when splitting the nucleus of uranium’ was published in *Nature* on 22 April. Joliot and his colleagues thought they had found an average of 3.5 neutrons per instance of nuclear fission (the correct number is 2.5). They also wrote that if a sufficient quantity of uranium were lowered into a suitable moderator, i.e., a substance that slows down the speed of the neutrons without way over-absorbing them, the fission process would maintain itself through a chain reaction. Joliot, von Halban and Kowarski later submitted an application for a world patent on a device to extract ‘atomic energy’, unaware of Szilard’s earlier patent.

The *Nature* article of 22 April brought about the exact situation that Szilard and his colleagues had been afraid of: it was noticed immediately by German scientists. On 24 April, the well-known Professor *Georg Joos* (1894-1959) wrote a letter to the National Ministry for Science, Education and Public Education and told them about the article. It was this unknown ministry under the Minister for Education *Bernhard Rust* (1883-1945) which took over the responsibility for all education when Hitler came to power in 1933. It had previously been local authorities and, as regards the university, the federal states which were responsible. Following the reorganisation, the Nazis were able to control education with an iron fist and remove all ‘inconvenient’ teachers and professors.

In 1934, Joos had succeeded *James Franck* (1882-1964) as Professor of Physics at the University of Göttingen after Franck had left his job in protest against the Nazi race laws.* The Nazi crackdown on education later also became too heavy for Joos, who left his Professorship in 1941 to start working with Zeiss in Jena. After the war, he told Arnold Sommerfeld that he had scarcely gained anything by doing this because the Nazis were just as much of a problem for him in Jena. Joos was one of those scientists who had no sympathy for the Nazis but who felt a sense of responsibility for his country.

Bernhard Rust, who was part of Hitler's government as head of the National Ministry for Science, Education and Public Education, was directly responsible for the standardisation of education (he committed suicide in 1945). Rust's private secretary was SS Brigadeführer Ministerial-Director *Professor Doctor Rudolf Mentzel*, who was also head of 'the National Research Council' and thereby in practice of all university research in Germany. The German scientists called him the 'culture sergeant'. Under Mentzel, a Nazi physicist with, given the context, the unbelievable name of *Abraham Esau* (born in 1884) would end up being responsible for managing the research in physics. Esau was Professor of Technical Physics in Jena and Principal of the university there during the 1930s. He was an expert and pioneer when it came to microwave radiation and his previous statements on 'death rays' during the 1930s had made him famous.

Another Nazi physicist was *Johannes Stark* (1874-1957), who had won the Nobel Prize in Physics in 1919 for his 'discovery of the Doppler effect in canal rays and of the splitting of spectral lines in electric fields'. In the following year, he had become Professor of Physics at Würzburg, but left the Professorship in 1922 to live as a private researcher; maybe the Nobel Prize made this possible. He became active within the national socialist movement at an early stage, and when Hitler came to power in 1933, Stark became a representative of the Reich Physical Technical Institute (PTR), a position that he held until 1939. Stark was the one who, along with *Philipp Lenard* (1862-1947), had aimed such rancorous attacks at Heisenberg in 1937 that the latter's life had been in danger.

In whose hands was the actual power? The National Research Council is said to have had a link to the PTR which was formally controlled by Stark. But the head of the Council, Rudolf Mentzel, who reported directly to the Minister for Education Rust, appears to have had more influence, and Esau thereby was also in a strong position where physics was concerned – in spite of the fact that the country's most prominent physicist was considered to be Heisenberg in Leipzig. This paved the way for rivalry and antagonism, particularly since the best physicists visited to Heisenberg and looked down on Esau.

This was what formed the circumstances for the reaction to Joos' letter to the Ministry of Education. The letter resulted in the Ministry commissioning Professor Esau to call a physicists' meeting on 29 April 1939 to discuss the implications of the French research group's article in *Nature*. The meeting took place in secret on the Ministry's premises at Unter den Linden. Those present included Esau as chairman and, as well as the letter-writer Joos, *Walther Bothe* (1891-1957), *Hans Geiger* (1882-1945), *Wilhelm Hanle* (born in 1901), *Gerhard Hoffmann*, *Joseph Mattauch* (1895-1976) and Esau's assistant, *Dr. Wilhelm Dames*.

Mattauch came from the Kaiser Wilhelm Institute for Chemistry in Berlin where Otto Hahn was in charge. He had succeeded Lise Meitner there when she was forced to flee from Germany. Hahn, like the remaining established physicists Bothe, Heisenberg and *Carl Friedrich von Weizsäcker* (1912-2007), was a patriot but not a Nazi. They compromised with the system and stayed in Germany, justifying this by saying that they could be of use to their country and that if they remained in important posts, they could prevent good-for-nothing Nazis from coming to power and helping to bring about the downfall of German science.

Heisenberg, who until June 1938 risked being murdered by the Nazis following attacks from Stark and Lenard but was then saved by intervention on the part of Himmler, whose mother knew Heisenberg's mother, was still an unwanted guest on the Ministry for Education's premises and, along with Hahn, was noticeably absent from the meeting on 29 April. Wilhelm Dames emphasised the importance of secrecy

* Franck had shared the 1925 Nobel Prize in Physics with *Gustav Hertz* (1887-1975, the nephew of *Heinrich Hertz*, the person who discovered radio waves). Franck moved from Germany to Copenhagen and then to the USA where he was Professor of Physical Chemistry at the University of Chicago from 1938-1947.

as Szilard had earlier in the United States, but now in the opposite direction. He criticised the absent Hahn because the latter had allowed the publication of his discovery at the start of the year, but Mattauch protested and defended Hahn.

Esau suggested that the leading physicists in Germany be brought together to put body and soul into building a nuclear reactor and that all available uranium ought to be bought up immediately for that purpose. Esau's eagerness and Dames' secrecy requirement scared Mattauch, who later discussed the conceivable consequences with his colleague *Siegfried Flügge* and Heisenberg's good friend Carl Friedrich von Weizsäcker. Nothing else happened other than a ban being placed on the export of uranium.

What Esau did not know was that another letter had been sent on the same day that Joos wrote to the Ministry for Education. The letter was addressed to the War Department and was sent by two young scientists in Hamburg, *Paul Harteck* (1902-1985) and *Wilhelm Groth*. Harteck was a physical chemist and had previously worked with Rutherford at the Cavendish Laboratory. The two scientists wrote the following in the letter:

> We take the liberty of calling to your attention the newest developments in nuclear physics which, in our opinion, will probably make it possible to make an explosive many orders of magnitude more powerful than the conventional ones [...] The country which first makes use of it has an unsurpassable advantage over the others.

The letter was forwarded to the Army Supply Administration's head of research, *Professor Erich Schumann*, a man holding the rank of General who was also the scientific adviser to *General Wilhelm Keitel* (after 1940, General Field Marshal). Schumann in turn handed over the letter to the army's explosives expert, the 34-year-old physicist *Kurt Diebner* (1905-1964). Diebner asked the advice of Hans Geiger, who was Professor in Experimental Physics in Berlin. Geiger advised him to examine the case more closely. This led to the creation of a special section for nuclear physics within the Army Supply Administration, with Diebner as head. Diebner also received funds to set up a laboratory at Kummersdorf, 40 kilometres south of Berlin, where the army had previously carried out experiments with explosives for rockets.* All this happened in great secrecy. So, Germany initially had two independent groups doing research into nuclear fission. Esau's group had the knowledge and Diebner's group had the military resources, which would soon give him the advantage.

In February or March, Carl Friedrich von Weizsäcker had listened to a talk on nuclear fission by Hahn. Hahn had said that the Joliot-Curie couple in Paris had observed secondary neutrons when splitting uranium nuclei. Weizsäcker understood what this meant - a chain reaction would be possible and thereby also an atomic bomb. After the physicists' meeting convened by Esau at the Ministry for Education on 29 April, Weizsäcker was able to recognise Mattauch's concern that the authorities had already started thinking about such a bomb.

Weizsäcker, who had previously been Heisenberg's best friend, had scarcely spoken to him during 1936-1939 and had moved from Heisenberg's institute in Leipzig to the Kaiser Wilhelm Institute for Physics in Berlin-Dahlem. The reason for the disagreement was that Weizsäcker had fallen in love with Heisenberg's sister Adelheid and that something had come between them. Following Hahn's discovery, however, the friends put it all behind them and were able to discuss the possible consequences of the discovery.

In summer 1939, the Kaiser Wilhelm Institute for Physics was still managed by Dutchman *Peter Debye* (1884-1966), the man who helped Lise Meitner to flee Germany. Debye, like Heisenberg, had been a candidate for Sommerfeld's Professorship in Munich, but both of them had been opposed by Lenard and Stark. Debye had been able to secure the prestigious job as head of the Kaiser Wilhelm Institute for Physics because the Kaiser Wilhelm Institute was not state-owned but was run by the private

* The laboratory is usually named after the closer but smaller place called Gottow, which is missing from many normal tourist maps. It lies 8 km east of Luckenwalde, a small industrial town which is on most maps.

Kaiser Wilhelm Gesellschaft, whose President in 1930-1937 had been Max Planck who had listened to neither Lenard nor Stark.

It was not just in Germany that the news of a surplus of neutrons when splitting uranium had aroused interest and led to the thoughts of the possibility of an atomic bomb. In England, the old *J.J. Thomson's* son *George P. Thomson* (1892-1975), also a Nobel Prize winner (1937), decided to do his own experiments. Thomson was Professor of Physics at the Imperial College for Science and Technology in London, but his institute did not have the required resources. He needed one tonne of uranium and approached the military authorities with a request for assistance, evidently thinking of the possibility of an atomic bomb. Thomson's request was sent back to Imperial College of which *Sir Henry Tizard* (1885-1959) was Principal but also chairman of a committee for the scientific review of the Air Force.

Tizard would play an important role during the impending war. His dealings with Lord Cherwell (F.A. Lindemann, Szilard's protector) have been described as lively by C.P. Snow in the book called *Science and Government* (1962). According to Snow, Tizard was 'the best scientific mind that in England has ever applied itself to war'. Tizard himself has written the following about the war: 'I wonder if the part that scientists have played will ever be faithfully and fully recorded. Probably not'.

Snow describes Tizard as follows:

> What was he like? Physically he did not alter much from middle age, when I first met him, until he died in 1959. He was English of the English. His whole appearance, build, and manner were something one does not often see outside England, or even outside the English professional class from which he sprang. He was not pretty.
>
> There were times when he looked like a highly intelligent and sensitive frog. His hair, what was left of it, was reddish. His face was unusually wide across the jaw line. But his expression was transfigured by his eyes, which were transparent light blue, sparkling with dash and interest.

Tizard called Thomson over to him, who was a little embarrassed about what he had written, and was asked to state his intentions. Not yet believing in the possibility of a bomb, Tizard wrote to the Air Force's head of research *David Pye* on 9 May and supported Thomson with reference to the fact that so many accounts had been given to government members that something had to be done. 'I do not agree with these representations, but now so many people are talking about the subject as a result it is, I think, wise to get ahead'.

Tizard did more than that. On 10 May, he had a conversation with *Edgar Sengier*, a Belgian who was director of the subsidiary of *Union Minière* which ran the Shinkolobwe mine in Upper Katanga in the former Belgian Congo, the world's largest producer of uranium ore at the time. Tizard requested an option to buy all the uranium ore from Shinkolobwe but Sengier refused. Tizard found out that there were also thousands of tonnes of uranium in Belgium but was not yet sufficiently convinced of the possibility of a bomb to dare to suggest that the British government immediately buy up the Belgian uranium. He made do with warning Sengier of what could happen if the uranium fell into the wrong hands: 'Be careful and never forget that you have in your hands something that may mean a catastrophe for your country and mine if this material were to fall into the hands of a possible enemy'.

Despite being unwilling to meet Tizard, Sengier was friendly, however. He said he was willing to tell the Englishmen if any surprising demand for uranium were made. When this did not happen, the British Admiralty said the following (according to Margaret Gowing):

> [...] the fact that other people have apparently not already started buying up stocks of uranium must not be ascribed to ignorance but either to the fact that foreign nations have limited funds to gamble with or that they have decided that the possibility of developing an explosive of unprecedented power from uranium is so remote as to be negligible.

There were others in the United Kingdom besides G.P. Thomson who were searching for secondary neutrons in April 1939. That month, the young Pole *Joseph Rotblat* (1908-2005) came to the University of Liverpool and began experimenting. Like so many others, he now found the extra neutrons and drew the obvious conclusion. He, like Szilard, was weighed down by the fear that the Nazis would also draw the same conclusion and start developing an atomic bomb. He began to do calculations for a bomb but when he visited Poland just before the war broke out and met his old physics Professor there and asked him for advice, the answer he got was: 'This is something no scientist should do!'

But the interest in the possibility of a bomb was so widespread that coordination problems began to arise. In Bristol there was a Professor Tyndall* at the university, who was involved in a government committee on defence against chemical warfare. Tyndall wrote a memorandum for the committee on the possibility of producing an atomic bomb. He described the possibility of triggering an explosion by rapidly fusing parts of a critical mass, but he believed that this was several tonnes.

When Tyndall's memorandum became known, it led to a government decision that the uranium research must not be spread among too many groups. The majority of scientists, including Tyndall and the committee on chemical warfare, were advised against concentrating on uranium research. It would be centralised under the Air Force through Tizard's committee for the scientific review of the Air Force. The principal experiment would be executed by George Thomson, while Professor Oliphant† at the University of Birmingham would research the production of metallic uranium. In July 1939, the Air Ministry reported that everything that could be done had now been done and that they first had to wait for the outcome of Professor Thomson's experiment.

They were still discussing the significance of uranium-235 in the United States in April 1939. Fermi was still not convinced by Bohr's argument that this nuclide was the main thing responsible for the fission process. Bohr was totally convinced but did not think that it had any practical consequence since he did not believe that it would be possible to separate sufficient quantities of uranium-235. But there were several views. On 29 April, an article in the *New York Times* concerned a meeting of the American Physical Society:

> Tempers and temperatures increased visibly today among members of the American Physical Society as they closed their Spring meeting with arguments over the probability of some scientist blowing up a sizable portion of the earth with a tiny bit of uranium, the element which produces radium.
>
> Dr. Niels Bohr of Copenhagen, a colleague of Dr. Albert Einstein at the Institute for Advanced Study, Princeton, N.J., declared that bombardment of a small amount of the pure Isotope U235 of uranium with slow neutron particles of atoms would start a 'chain reaction' or atomic explosion sufficiently great to blow up a laboratory and the surrounding country for many miles.
>
> Many physicists declared, however, that it would be difficult, if not impossible, to separate Isotope 235 from the more abundant Isotope 238. The Isotope 235 is only 1 per cent of the uranium element.
>
> Dr. L. Onsager of Yale University described, however, a new apparatus in which, according to his calculations, the isotopes of elements can be separated in gaseous forms in tubes which are cooled on one side and heated to high temperatures on the other.
>
> Other physicists argued that such a process would be almost prohibitively expensive and that the yield of Isotope 235 would be infinitesimally small. Nevertheless, they pointed out that, if Dr. Onsager's process of separation should work, the creation of a nuclear explosion which would wreck as large an area as New York City would be comparatively easy. A single neutron particle, striking the nucleus of a uranium atom, they declared, would be sufficient to set off a chain reaction of millions of other atoms.

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* Not to be confused with *John Tyndall* of the 1800s (1820-1893), the Irish physicist who is known for the Tyndall Effect. This refers to the scattering of light in a gas or a liquid in which small particles are suspended. The scattering is dependent upon wavelength and is at its strongest for light with short waves. This is the reason why the sky appears to be blue.

† *Marcus ("Mark") Oliphant*, born in 1901, was an Australian physicist who was Professor in Birmingham where he founded a laboratory for nuclear physics and led development work for cavity magnetron for the generation of the microwaves that were required for the big British invention, *radar*.

Lars Onsager (1903-1976) who was mentioned in the article was a Norwegian-American chemist who would become Professor at Yale in 1945 and who won the Nobel Prize in Chemistry in 1968. The process he referred to, isotope separation through thermal diffusion, had been attempted by *Klaus Clusius* and *Gerhard Dickel* in Germany in 1938. They had built a vertical cylindrical pipe in the middle of which, like the axis of a cylinder, a wire was heated to bring about a temperature difference of 600 °C between the wire and the cylinder. The device was based on a theory developed by Englishman *Sydney Chapman* (1888-1970) and the Swede *David Enskog* (1884-1947) in the 1910s, and the intention was that different heavy gas particles would diffuse to different extents. The difference in temperature created a current so that the heated gas from the substance being examined rose next to the heated wire but fell on the cold side of the pipe where the heavier gas particles were concentrated.

David Enskog's input is interesting, particularly to me, since he taught me mathematical physics at the Royal Institute of Technology in Stockholm in the 1940s. He was born as number five out of twelve siblings to a poor and strictly religious family in Västra Ämtervik in Värmland. After going to high school, he started working as an assistant in a pharmacy in Sunne where the pharmacist noticed his aptitude and helped him to continue his studies. He quickly caught up on the years he had lost through his initial lack of concentration on the leaving certificate and became a student in 1903. He then continued his studies at Uppsala University where he was able to begin his licentiate studies in 1907, and in 1911 had a licentiate thesis approved on the subject of 'Studies of the diffusion of water vapour at different pressures'.

This exhausted his financial options based on loans and study grants and, since he also wanted to start a family, he was forced to discontinue his studies and go and earn a living, which he did as a high school teacher.

However, David Enskog did not abandon the idea of a doctoral thesis and, since his licentiate thesis had concerned kinetic gas theory, he continued along the same lines. At the time, there was a big, unsolved problem within the field, namely finding a general solution to Maxwell-Boltzmann's equation, a differential equation that describes the way in which the likelihood of finding gas particles with a given kinetic energy (the product of mass and velocity) varies in time and space with regard to the fact that the particles collide with one another.

Since Enskog had studied part of a paper from 1912 by German mathematician *David Hilbert* (1862-1943) from Heidelberg, he realised how he could derive the solution by using a system of integral equations. In 1917, he was ready to defend a thesis in which he gave the general solution. The thesis was presented at the Institute for Mechanics and Mathematical Physics in Uppsala to *Professor Carl Wilhelm Oseen* (1879-1944), a man with a very critical disposition when it came to mathematical presentations. Oseen was the first opponent for the defence and thought that Enskog's presentation was dull and difficult to understand, so Enskog was given poor marks that did not suffice to earn him an assistant professorship, although he was now able to get a permanent post as a senior master (in Gävle).

This was additional proof of the word of the Bible that no-one is a prophet in his own country. Outside Sweden, Enskog's thesis was widely acclaimed. Arnold Sommerfeld wrote to him, saying 'As far as I can see, you have really succeeded with what Hilbert was referring to'. The English mathematical physicist Sydney Chapman, who had received Enskog's thesis, had arrived at the same result independently from him, but had written to Enskog in 1917 saying that he no longer intended to continue attempting to work out any rigorous proof of his result since this had been more intuitive than 'your own elegant analysis'. He also wrote about 'the rigorous foundation which you have built up on the Boltzmann integral equation' and 'before I read your thesis, I did not realise how [such a basis] could lead to such a simple, general solution'.

When it was later said that Chapman had found the solution before Enskog, Chapman wrote to the latter, saying that 'we are entitled to roughly equal credit' and that 'so far as the theory is associated with our names at all, I think it should be on an equal basis, and I will push for this in England as far as I can'.

In 1929, Enskog applied for two vacant Professorships in Stockholm. The first was at Stockholm University College and was in Mechanics and Mathematical Physics. This was where expert Bohr's pupil *Oskar Klein* (1894-1947) was recommended, who was given the Professorship. The other was at the Royal College of Technology (KTH) and was for Mechanics and Mathematical Physics. Competitors here primarily included *Hilding Faxén* (1892-1970), although he made less of an impression in the test lectures. Of the three experts, one placed Enskog and another placed Faxén at the top of the list. The third expert was C.W. Oseen, who could not choose between Enskog and Faxén, placing them both at the top of the list.

Luckily enough for Enskog, Sydney Chapman just happened to be visiting Stockholm, which is where he met Enskog for the first time. Chapman succeeded in convincing three influential Professors at KTH of Enskog's advantages: physicist *Gudmund Borelius* (1889-1985), electrotechnologist *Henning Pleijel* (1873-1962) and mathematician *Johannes Malmqvist* (1882-1952). Enskog got the Professorship.

David Enskog was a modest man, but his elegant lectures were difficult to follow since he took it for granted that his pupils understood matters as well as he did. The series of lectures that I attended in 1947 ended in tragedy. During the last lectures, Enskog occasionally went quiet and just stared out at us as if he had completely forgotten why he was standing there. We realised that the mild-mannered Professor with his trim, white moustache was seriously ill.

And this was as far as the basis for gas diffusion technology for enriching uranium-235 got. But there were also other enrichment possibilities. Fermi's colleague at Columbia University, John Dunning, would give thought to the mass spectrograph.

The mass spectrograph was an invention by *Francis William Aston* (1877-1945). Aston initially studied to be a chemist but changed to physics owing to the options that presented themselves with Wilhelm Conrad Röntgen's discovery. Following an adventurous youth with round-the-world trips and sports achievements, Aston came to the Cavendish Laboratory in 1910 when J.J. Thomson was the manager. One of the problems that were occupying Thomson's thoughts was whether the noble gas neon consisted of several isotopes with different atomic masses. After a while, Aston was able to say that he had succeeded in separating two such isotopes using diffusion through pipeclay, but he did not succeed in completely convincing Thomson. The First World War occurred in the meantime and Aston was given research assignments for the Air Force. During that time, he continued to think about Thomson's problem and the conditions for finding the answer by experimenting with discharge tubes.

At the time, Thomson was investigating ion beams in discharge tubes. The usual thing had been to study cathode rays, i.e., electrons, through dilute gases. Thomson instead created rays from the ions that were generated when the atoms of the gases were ionised or when ionised atoms left the anode of the discharge tube when this was covered with a thin layer of the substance that was being experimented with.

When ion beams pass through electric or magnetic fields, they are deflected. The size of the deflection in a magnetic field is dependent on the velocity of the particles and therefore on their mass. The deflection in an electric field is dependent on the electric charge of the particles. By observing the deflections, Thomson was therefore able to draw conclusions on differences in atomic masses for particles with the same electric charge. Thomson's result indicated that the noble gas neon consisted of two different isotopes with different masses. But Aston wanted to refine the method.

When Aston returned to Cambridge and the Cavendish Laboratory after the war in 1918, he had decided which instrument he wanted to build. It was a vacuum chamber in which he could create ions of different atomic species and then, using two narrow apertures, create sharply-defined ion beams in which the ions were accelerated - firstly through an electric field which separated the beams of different electric charges and then through a magnetic field which further separated the beam of ions with different masses. The ion beams then encountered a photographic film which was blackened at various distances from a central line, all according to the extent to which the ion beams had been deflected. The device was similar to a spectrograph, but here it was not a question of light of different wavelengths, but of particles with different masses. Aston therefore called his apparatus a *mass spectrograph*. He won the Nobel Prize in Chemistry in 1922 for this invention and for his use thereof. Using the mass spectrograph, Aston demonstrated more than 200 isotopes of the naturally-occurring elements.

Aston was able to rapidly confirm that neon consisted principally of the isotopes neon-20 and neon-22 and that it was their reciprocal proportions which gave neon the atomic weight of 20.2 rather than exactly 20. This was before 1932, and Chadwick had not yet discovered the neutron. It was therefore thought possible that all atomic nuclei were made up of hydrogen nuclei, i.e., protons (as Dr. William

Prout had once proposed*). Thiswas a natural thing to believe since initially, hydrogen did not appear to have an isotope. That being the case, why did other atomic nuclei not have masses that were exact multiples of that of hydrogen? Aston observed that a helium atom appeared to lose almost 1 % of the mass that would have been expected from four protons.

ATOMIC NUMBERS, MASS NUMBERS, NUCLIDES AND ISOTOPES

YOU CAN designate an atomic species (i.e., a nuclide) ${}^{A}_{Z}X$ where X is the chemical symbol (i.e., H, C, Cl, Ag or U, etc.), A is the mass number, i.e., the number of protons and neutrons in the atomic nucleus, and Z is the atomic number, i.e., the number of protons in the nucleus. The number of protons is what determines the electric charge of the nucleus and thereby its chemical properties and its position in the periodic table of elements. Since the atomic number Z and the chemical symbol for an element provides one and the same piece of information, you usually omit Z from the designation. Nuclides with the same atomic number but different mass numbers are called isotopes of the element concerned. We now know that neon, whose chemical symbol is Ne, consists of the three isotopes ${}^{20}Ne$ (90.51%), ${}^{21}Ne$ (0.27%) and ${}^{22}Ne$ (9.22%).

If the hydrogen atoms could be merged into helium atoms in a glass of water, said Aston, you would, on the basis of Einstein's link between energy and mass ($E = m_0c^2$), expect sufficient energy to be released to run an ocean liner back and forth across the Atlantic at full speed. As early as 1936, Aston prophesised:

> There are those about us who say that such research should be stopped by law, alleging that man's destructive powers are already large enough. So, no doubt, the more elderly and ape-like of our prehistoric ancestors objected to the innovation of cooked food and pointed out the grave dangers attending the use of the newly discovered agency, fire. Personally I think there is no doubt that sub-atomic energy is available all around us, and that one day man will release and control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbor.

However, it was not Aston who would investigate the isotopes of uranium. Aston had observed only uranium-238 with his mass spectrograph, but in 1935, a physicist at the University of Chicago, *Arthur Jeffrey Dempster* (1886-1950), found that uranium also had a lighter isotope. Dempster has recounted the way in which this was demonstrated in a mass spectrograph: 'It was found that just a few seconds of exposure were sufficient for the main component at 238, which Dr. Aston had reported, but on exposures a faint companion of mass number 235 was also present'. Later on, *Alfred Nier* (1911-1994) at the University of Minnesota was able to demonstrate that the isotope uranium-235 consisted of 0.7 % of the mass of uranium.

In April 1939, John R. Dunning, who was convinced that an atomic bomb required pure uranium-235, wanted proof that Bohr was right in the dispute with Fermi who was sticking to uranium-238. Initially, very small quantities of uranium-235 ought to be enough to show who was right. It ought to be possible to separate sufficient quantities using a mass spectrographer. The one that seemed the most suitable was the one that was used by Nier in Minneapolis. Dunning appealed to Nier in a letter:

> There is one line of attack that deserves strong effort, and that is where we need your cooperation. […] It is of the utmost importance to get some uranium isotopes separated in enough quantities for a real test. If you could separate effectively even tiny amounts

* See Book I: Pandora's Box, p. 285 onwards.

of the two main isotopes, there is a good chance that Booth* and I could demonstrate, by bombarding them with the cyclotron, which isotope is responsible. There is no other way to settle this business. If we could all cooperate and you aid us by separating some samples, then we could, by combining forces, settle the whole matter.

FUSION AND FISSION

THE SUBSTANTIAL quantities of energy that would be released were light atomic nuclei to be merged to form heavier ones (and that we now call fusion) correspond to the substantial energy, the binding energy (E_B), which must be supplied to split them up. This means that the forces holding the particles together are very strong. Since these forces always exist in a composite atomic nucleus, the mass of the atomic nucleus will be less (almost 1% less) than the composite mass of the protons and neutrons of which it is made up. This difference in mass is called the *mass defect* and, according to Einstein's formula, ought to be the binding energy divided by the square of the speed of light. It may often be more appropriate to state the masses in measurements of energy rather than in measurements of mass.
The mass of the proton is 1.673 10^{-27} kilogrammes, which corresponds to an energy of 938.3 million electron volts (MeV). The mass of the neutron is slightly greater, 1.675 10^{-27}, which corresponds to 939.6 MeV.
Protons and neutrons are commonly called nucleons. Of particular interest is the mass defect per nucleon. It is at its greatest for medium-weight elements such as manganese, iron, copper, nickel and zinc, for which it is approximately 8.7 MeV, and then falls as mass numbers increase so that it is approximately 7.6 MeV for the isotopes of uranium. For light atomic nuclei it is even smaller, just l MeV for deuterium (heavy hydrogen), just over 7 MeV for helium and 5.3 MeV for lithium-6. This means that nuclear energy can be extracted either by merging light atomic nuclei (fusion) or splitting heavy atomic nuclei (fission). The fission products arising in the latter case have mass numbers between 95 and 150 and binding energies of up to 8.5 MeV. During the splitting of uranium, you therefore extract approximately 0.9 MeV per nucleon, i.e., during the fission of uranium-235 approximately 0.9 times 235 MeV per split atomic nucleus (Lise Meitner and Otto Frisch estimated the energy at approximately 200 MeV when they made a rough calculation at Christmas in 1938).
The individual hydrogen atom consists of a single proton and therefore has no binding energy. Nor can a heavier atomic nucleus be built up only from protons – neutrons are also needed, so the smallest atomic nucleus that can be used for fusion is deuterium ('heavy hydrogen'), which consists of one proton and one neutron.

Not until February 1940 was Nier able to send Dunning separated uranium-235 for analysis.

The physicist whom everyone feared could develop a German nuclear weapon, Werner Heisenberg, was travelling around to different universities in the United States in the summer of 1939. In Berkeley, he visited Robert Oppenheimer at the University of California. At the University of Chicago, he discussed the possibility of a second world war with the 1927 Nobel Prize winner for Physics, *Arthur Compton* (1892-1962), and wondered if America would attack if this were the case, to which Compton replied yes. At the University of Rochester, he met Victor Weisskopf and Hans Bethe, who asked him whether he believed that Germany would win if war broke out. 'Yes,' answered Heisenberg, 'I believe the Nazis will win.'

Isidor Isaac Rabi, who had been given his Professorship at Columbia University thanks to strong recommendations from Heisenberg, asked him to remain in New York. Eugene Wigner asked Heisenberg to stay at Princeton, but he declined. According to Rabi, he said: 'It is clear there will be a war, it is clear that Germany will lose it. But I am a German, I have to try to save the young physicists who work with me, and it's important to be there after the war is over to re-establish physics and to see that the right people get jobs at the right universities.' The details of Heisenberg's assumptions regarding the outcome of a war vary and his opinion may have changed while he was travelling.

* Dunning's assistant, Eugene T. Booth.

At the end of July 1939, Heisenberg visited the University of Michigan in Ann Arbor and stayed with a friend of fourteen years, *Samuel Goudsmit* (1902-1978), a Dutch physicist who would go on to be tasked with investigating what had happened to the atomic bomb research in Germany during the war. Goudsmit also asked him to stay in the United States, but Heisenberg gave the same reason as before for being in Germany if a war broke out. Goudsmit perceived Heisenberg's argument as an expression of self-righteousness. Heisenberg also used another reason for travelling home to Germany. If he stayed in the United States, his assistance might be requested with a bomb project aimed at his own country. At the start of August 1939, Heisenberg returned home on the ocean liner *Europa*, leaving colleagues who feared the worst - Heisenberg's bomb.

At home in Germany, a further discovery had been made in spring 1939. In June, Mattauch's colleague Siegfried Flügge published an article in *Naturwissenschaften* entitled (English translation) 'Can the energy content of the atomic nucleus be utilised?' In the article, he proposed the use of *cadmium* (an element that very effectively absorbs neutrons) to control the chain reaction so that it did not run away. The implication that 'running away' would signify a bomb was clear. Flügge also wrote that the energy released from one cubic metre of uranium oxide would be enough to blow one cubic kilometre of water 27 kilometres into the air. When Szilard and Wigner read the article in New York where Wigner was visiting from Princeton, they were surprised at the development in Germany. What would happen if the Germans got hold of the substantial quantities of uranium in Belgium? According to what Flügge said later, that was a concern which the article was intended to create. Mattauch and Weizsäcker said they had encouraged him to publish it to show the surrounding world what could happen in Germany were the Nazis to gain control of the scientists.

Flügge's article was also noted in Sweden. *Professor Hans Pettersson* (1888-1966) wrote about the possibility of extracting nuclear energy in two articles in *Göteborgs Handelstidning* (on 8 and 10 July 1939). Hans Pettersson was a prominent ocean researcher and physicist, Professor of Oceanography at Gothenburg University. He was particularly interested in radium and had worked with Marie Curie in Paris. The words he wrote in GHT in summer 1939 were close to a prophecy:

> So, three tonnes of uranium conceal within their atomic nuclei as much energy as one million of prime coal when combusted. If it were possible to [...] use the development of energy for technical purposes, all of the factories and means of transport throughout the world could be run by sacrificing a few thousand tonnes of uranium per year. But it is not only a matter of accelerating the decay of the atomic nuclei. You also have to be able to control the tempo, for if the transformation were to be explosive, within the space of a few fractions of a second, a disaster would occur against which the world war's worst drumfire will look like an innocent firework display.
>
> Ten years or so ago, the thought of using the energy store of the atomic nucleus still appeared to be absurd. [...] But time flies by in our century and the clearly demonstrated impossibilities do sometimes have a shorter lifetime than a normal truth according to Ibsen. In the German magazine *Die Naturwissenschaften* for 9 June this year, we are surprised by an article, written by a renowned physicist at the Kaiser Wilhelm Institute for Chemistry in Berlin-Dahlem, Flügge, with the heretical-sounding title of 'Kann der Energieinhalt der Atomkerns technisch nutzbargemacht warden (Can the energy content of the atomic nucleus be utilised)?'
>
> [...] It is shown that a spontaneous transformation of uranium atoms ought to be possible under certain conditions if you ensure that the neutrons that are formed as by-products of nuclear fission remain within the reaction mass as far as possible until they have been slowed down to a minute fraction of the initial speed, i.e., down to speed of a few thousandths of a metre per second. However, the great difficulty would be mastering the spirits thus evoked, i.e., preventing the nuclear reaction from taking place quickly enough to form an explosion where the enormous amount of energy available would be released in a fraction of a second.
>
> A brilliant idea to solve this problem has recently been put forward by the discoverer of this special nuclear reaction, Otto Hahn at the Kaiser Wilhelm Institute in Dahlem. There is an element, cadmium, whose atomic nuclei have the remarkable property of

> being able to stop neutrons at high speed to an extent that is one hundred to a thousand-fold times greater than that of other elements.
>
> [...] In our century, vastly over-filled with technology, the time difference between the laboratory experiment and the technical facility is far shorter. It is conceivable that the intensive work now being put in by some of the world's sharpest brains* and in some of the best-equipped research laboratories on unlocking the infinitesimally small safes, the atomic nuclei, in which nature has deposited her most substantial energy reserves, may succeed in facilitating uranium machines and maybe also thorium machines. [...]
>
> If this dream were to be realised, the relatively rare layers of uranium and thorium would immediately undergo extreme exploitation. There would be a fight for the uranium, with the intensity of that fight doubtless exceeding the current fight over oil. Radium, which had up until now been the purpose of processing the uranium find, would be a future by-product whose value would be a long way off the energy value of the 3 million times greater quantity of uranium from which it was extracted.
>
> But the realisation of the uranium machine also opens up other more dismal perspectives. [...] Without cadmium as a regulator, the mixture of uranium oxide and water could be used as fire bombs, compared with which today's thermite bombs would be as effective as if they came from the stone age. Without lapsing into horror stories, you must admit that the solution of the problem of the nuclear machine and the atomic bomb in these particular times could easily lead to a world disaster.
>
> As we know, Napoleon dismissed the project manager who proposed to him the use of steam to create an armada sufficient to conquer the noble Albion as a dreamer. The fear is that his present-day successors would waste no time seizing the thunderclap which today's nuclear research threatens to present to them on a plate. And if they do, our civilisation, born of carbon, may perish from uranium.

If by 'Napoleon's present-day successors' Hans Pettersson was thinking of Hitler, the prediction was wrong. If on the other hand President Roosevelt could also be included, Pettersson really was a visionary. Ninety-three days later, Roosevelt listened to a proposal for an atomic bomb and, funnily enough, a proposal that was put forward with reference to Napoleon's failure to seize the opportunity. What was meant to be a secret during the 1940s was on everyone's lips by the summer of 1939.

* Here, text is also missing from the original article.

4. THE LETTER FROM EINSTEIN

IN JUNE 1939, Szilard's temporary laboratory privileges at Columbia University came to an end and he applied himself fully to thinking activities and discussions, primarily with his compatriot Wigner but also with Fermi. From among the results, it can be mentioned that at the start of July he the proposed the possibility of using graphite as a moderator to slow down the fast neutrons in the endeavour to achieve a chain reaction of uranium fission.

Fermi, who for the time being had grown tired of his uranium experiments, was in Ann Arbor where a Summer School was being held in theoretical physics. From there he corresponded with Szilard and wrote that he had had the same idea himself. They discussed the benefits of arranging uranium and graphite in a number of layers. Fermi estimated that the required quantities were 39 tonnes of graphite and 600 kg of uranium if you mixed the components. Szilard did a more complicated calculation for a lattice (the English word 'lattice' originally designated a trellis or chequered pattern in a leaded window) of small uranium spheres in a block of graphite and arrived at quantities of 50 tonnes of graphite and 5 tonnes of uranium.

Neither Szilard nor Wigner had given up hope of being able to convince the American government of the threat of a German atomic bomb. However, until June 1939, they had very little to go on other than their instinctive fears. Little was known in the United States about what was going on in Germany other than the fact that there were plenty of relevant experts and that Heisenberg would soon be on his way home to his family and acquaintances. There was Otto Hahn with competent colleagues at the Kaiser Wilhelm Institute for Chemistry, von Weizsäcker at the Kaiser Wilhelm Institute for physics, Clusius and Dickel who had proof that they could separate isotopes, a successful industry and a powerful government with no scruples. But there was no direct proof that the Germans were developing an atomic bomb if you discount the ominous ban on the export of uranium.

In June, Flügge's article changed the situation. There was now no doubt that the Germans had the knowledge and knew that atomic bombs were possible. Szilard and Wigner became more and more resolved to intervene in some way. Above all, Wigner wanted the American government to be contacted - but how? They discussed various means. One of the people they asked was *Gustav Stolper*, a German immigrant who had been an MP in Berlin. Stolper promised to come up with some possibilities.

The action that Szilard and Wigner thought was the most urgent was to prevent the Germans from getting at the Belgian uranium. Szilard suggested that they should contact Einstein since the latter knew and was in contact with the Belgian Queen Mother *Elisabeth* (1876-1965), who became very popular during the First World War for her efforts for refugees and the wounded. They rang Einstein's work room in Princeton first of all but found out that he was spending the summer on Long Island. The two Hungarians decided to visit him in his summer house and took themselves off on a drive which proved to be both long and difficult in the summer heat. It was Sunday 16 July.

Long Island does have areas that are part of New York city, but the island is very long and stretches approximately 200 km away from the city. Einstein's summer house lay almost at the far end where the island divides like a gaping crocodile with two 50-kilometre peninsulas like jaws. The peninsulas surround Peconic Bay, almost forming a lake, 30 km in length, until the tips resembling the crocodile's teeth almost bite together. Just outside there is the open Gardiner's Bay. Peconic Bay is through a narrow cape, Nassau Point, divided into Great and Little Peconic Bay. Einstein was on the cape, but it was not easy to get there. Szilard and Wigner wandered around for a long time, well into the afternoon, until they found a small boy, seven to eight years of age, who knew who Einstein was and where he was living.

Two years previously, C.P. Snow had visited Einstein at the same summer house. His description of the environment and Einstein is worth reproducing:

> He came into the sitting room a minute or two after we arrived. There was no furniture apart from some garden chairs and a small table. The window looked out on the water, but the shutters were half closed to keep out the heat. The humidity was very high.
>
> At close quarters, Einstein’s head was as I had imagined it: magnificent, with a humanizing touch of the comic. Great furrowed forehead; aureole of white hair; enormous bulging chocolate eyes. I can’t guess what I should have expected from such a face if I hadn’t known. A shrewd Swiss once said it had the brightness of a good artisan’s countenance, that he looked like a reliable old-fashioned watchmaker in a small town who perhaps collected butterflies on a Sunday.
>
> What did surprise me was his physique. He had come in from sailing and was wearing nothing but a pair of shorts. It was a massive body, very muscular: he was running fat round the midriff and in the upper arms, rather like a footballer in middle-age, but he was still an unusually strong man. He was cordial, simple, utterly unshy. The large eyes looked at me, as though he was thinking: what had I come for, what did I want to talk about?
>
> […] The hours went on. I have a hazy memory that several people drifted in and out of the room, but I do not remember who they were. Stifling heat. There appeared to be no settled time for meals. He was already, I think, eating very little, but he was still smoking his pipe. Trays of open sandwiches – various kinds of wurst, cheese, cucumber – came in every now and then. It was all casual and Central European. We drank nothing but soda water.

It was doubtless the same when Szilard and Wigner arrived on the Sunday afternoon. When they settled themselves down and had Einstein’s attention, they told him about the neutron experiment in Columbia and of Szilard’s calculations regarding the possibility of achieving a chain reaction in an assembly of uranium and graphite. To their surprise, Einstein had not yet heard anything about the possibility of a chain reaction. ‘Daran habe ich gar nicht gedacht!’ [‘I haven’t even thought about it!’], he said.

Szilard and Wigner had no difficulties convincing Einstein that something had to be done to prevent the Germans producing an atomic bomb, but Einstein did not want to write to Elisabeth, the Queen Mother. He was on the other hand willing to contact a member of the Belgian government whom he knew. Wigner thought that if this was what they were going to do, they had to inform the American government first. He suggested that Einstein’s letter ought to be sent through the Ministry for Foreign Affairs with an accompanying letter.

When they had all agreed, Einstein dictated a letter in German which was now intended for the Belgian ambassador through the Ministry for Foreign Affairs, and Wigner wrote down what he said, to then translate it into English when he returned to Princeton. While Wigner went off to Princeton, Szilard returned to the King's Crown, the hotel in Manhattan which was his home. There lay a message from Gustav Stolper.

Stolper had been looking for a suitable person to convey the message from Szilard and Wigner to the American government. He had now found a man who was also willing to convey the message directly to the President. The man’s name was *Alexander Sachs* (1893-1973).

Sachs was an immigrant from Russia and had come to the United States at the age of elven. He had a university degree in biology from Columbia University and had then studied philosophy, legal science and sociology at Harvard. In 1932, he had assisted with Franklin Roosevelt’s election campaign and then spent three years working for the National Recovery Administration (NRA), a committee assembled by Roosevelt with the task of reconstructing American industry with the support of the proxies that the President had been given through a special law, the National Industrial Recovery Act (NIRA), a result of Roosevelt’s New Deal. Sachs had then become Deputy MD of Lehman Corporation, a financial company that was close to Roosevelt. It appeared that Sachs was one of the people whom Roosevelt summoned for discussions and advice from time to time.

Szilard got to meet Sachs, who listened to him and promised to speak to the President. This was something new to Szilard whose voice, up until then, had fallen on deaf ears with the exception of the visit to Einstein. Extremely satisfied, he wrote to Einstein about the conversation:

[Sachs] took the position, and completely convinced me, that these were matters which first and foremost concerned the White House and that the best thing to do, also from the practical point of view, was to inform Roosevelt. He said that if we gave him a statement, he would make sure it reached Roosevelt in person.

Einstein and Szilard

Szilard had spoken to Sachs at the start of the week following the visit to Einstein. He had received Wigner's translation of Einstein's letter that had been dictated in German but was now unable to discuss the new situation with Wigner since the latter had gone on holiday to California. Instead, he wrote another letter, intended for Roosevelt from Einstein, and posted it to Einstein along with an accompanying letter in which he said he was willing to come out to Nassau Point and discuss what should be done. He wrote the draft of the letter to Roosevelt in German since Einstein's English may not have been adequate for the necessary distinctions between meanings.

Well, Einstein wanted to meet Szilard again. The previous time, Wigner had acted as chauffeur since Szilard had never learned to drive. This time, he asked his other compatriot, Teller, for help. Teller wrote the following about this:

> Later that summer I was given my first atomic assignment. I was drafted as chauffeur for Szilard, who never had descended to the mechanical skill of driving a car. He had an appointment with Albert Einstein at Peconic Bay, N.Y., that was to have a profound effect on the future of the United States. It was August 2, 1939, and during their meeting Szilard and Einstein discussed a letter addressed to 'F.D. Roosevelt' at the White House.

Teller drove Szilard in his big 1935 Plymouth (although he stated the wrong day in his account). The 2nd August was a Wednesday, and, to all appearances, the visit took place on Sunday 30 July. Einstein received them without ceremony wearing his slippers. Together with Szilard, he dictated a third version of a warning letter, Teller writing it down this time. However, Einstein was not sure whether Sachs was the right man to convey the letter to Roosevelt. Therefore, when Szilard came back to New York, he visited Sachs and told him about the visit to Einstein and of Einstein's hesitation. Sachs, who appeared anxious to take on the assignment, suggested alternative names, including financier *Bernhard Baruch* (1870-1965), President of the MIT *Karl Compton* (1887-1954, Arthur Compton's brother) and pilot veteran *Charles Lindberg* (1902-1974, whom Roosevelt detested).

On 2 August, Szilard wrote to Einstein and told him about Sachs' suggestion and that Sachs had proposed a slightly longer version of the letter to Roosevelt. Szilard sent along two versions, one long and one shorter letter. Einstein accepted Sachs and signed both of the letters and sent them back to Szilard but prescribed the longer version. Szilard gave this to Sachs on 15 August, whereupon Sachs waited for an appropriate occasion on which to speak to President Roosevelt. He thought he would need an hour to give him time to read out the content and discuss it with Roosevelt. Simply giving it to Roosevelt would be a waste of time, thought Sachs. A few years later, he said:

> Our social system is such that any public figure [is] punch-drunk with printer's ink. […] This was a matter that the Commander in Chief and the head of the Nation must know. I could only do it if I could see him for a long stretch and read the material so it came in by way of the ear and not as a soft mascara on the eye.

It was not until October that Sachs got such an opportunity. Roosevelt had other things to think about in September when the Second World War was triggered by Hitler's invasion of Poland and the declaration of war from England and France on 3 September.

People also had other things to think about in England, but a government initiative had finally been taken as a consequence of George Thomson's conspicuous request for a tonne of uranium and Tizard's support for the proposal. The government had asked Lord Hankey, Minister without Portfolio, to examine the possibility of creating an atomic bomb, the same Maurice Hankey whom Rutherford had told, just before his death, that the government ought to keep an eye on the development.

Lord Hankey delegated the task to *Sir Edward Appleton* (1892-1965), a Professor of Physics at Cambridge who won the Nobel Prize in Physics in 1947 for his work with research into atmospheric physics. Sir Edward was given the task because he was secretary of the British Research Council for Natural Science and Technology. His input consisted of finding the right man to take on the task wholeheartedly. He chose James Chadwick in Liverpool, who gave thought to the problem until December. It was not a new problem for Chadwick. He had noticed the young Pole Joseph Rotblat

and, during the summer, had been able to look at the latter's thoughts on the possibility or the threat of an atomic bomb.

On 5 December 1939, Chadwick reported to Appleton and said that bringing together sufficient quantities of uranium would doubtless create a chain reaction and an incredible explosion, but that it was difficult to say how much. One tonne might be enough, but thirty or forty might be needed. Appleton reported this to Lord Hankey, who apparently said that people could remain calm, saying 'I gather that we may sleep fairly comfortably in our beds.' Six months passed before he changed his mind. During that time, Niels Bohr had visited England in summer 1939 when he returned from the United States. At the time, Churchill was also worried about the rumours of a German atomic bomb – not because he was afraid of the bomb but because he was afraid that the Prime Minister, Neville Chamberlain, would be influenced by an even more powerful German weapon and therefore hesitate with regard to restraint concerning the Poland issue.

Churchill's good friend and Szilard's previous protector F.A. Lindemann discussed bomb possibilities with Bohr, who maintained that a bomb required uranium-235 and that it was not practically viable to separate sufficient quantities of uranium-235. Lindemann told Churchill that the atomic bomb was, practically speaking, an impossibility. Churchill, in turn, was anxious to make this message generally known and wrote the following summary in a letter to the Secretary of War, *Sir Kingsley Wood*:

> It is essential to realise that there is no danger that this discovery, however great its scientific interest, and perhaps ultimately its practical importance, will lead to results capable of being put into operation on a large scale for several years. […].
>
> First, the best authorities hold that only a minor constituent of Uranium is effective in these processes, and that it will be necessary to extract this before large-scale results are possible. This will be a matter of many years. Secondly, the chain process can take place only if the Uranium is concentrated in a large mass. As soon as the energy develops it will explode with a mild detonation before any really violent effects can be produced. […] Thirdly, these experiments cannot be carried out on a small scale. If they had been successfully done on a big scale […] it would be impossible to keep them secret. Fourthly, only a comparative small amount of Uranium in the territories of what used to be Czechoslovakia is under the control of Berlin.
>
> For all these reasons the fear that this new discovery has provided the Nazis with some sinister, new, secret explosive with which to destroy their enemies is clearly without foundation. Dark hints will no doubt be dropped and terrifying whispers will be assiduously circulated, but it is to be hoped that nobody will be taken in by them.

So, before the war broke out, and for a fair time afterwards, the authorities in England failed to take the atomic bomb seriously. A patent had been taken out on it in France. Endeavours were underway to produce it in Germany.

On 23 August 1939, the German-Soviet Non-aggression Pact was signed, forbidding the invasion of Poland. The attack came on 1 September and impressed the world with its speed. On 3 September, the Second World War was a fact, and many dreaded what would happen next.

Heisenberg was now back in Germany. He had expected to be called up for the annual military service refresher course with the alpine hunters, but the war came in the meantime. When it broke out, Heisenberg was with his wife in Urfeld, 20 km north-east of Garmisch-Partenkirchen in the Bavarian alps. From there he wrote to his old Professor Arnold Sommerfeld in whose footsteps he still hoped to be able to follow in spite of aversion on the part of the Nazis:

> [I am expecting] call-up any day now, which strangely enough has not yet come through. My family will remain here in the mountains until the war is over. The question of your professorship will now also remain undecided until the mastery of Europe has been decided. Let us hope that the path to this does not cost too many human lives!

Heisenberg was not the only physicist who was waiting impatiently and anxiously to be called up. His colleague *Erich Bagge* (1912-1996) in Leipzig had been ordered to go to the War Department in Berlin and was convinced that he would be sent to the front line as a common soldier. However, to his surprise, he was met by two people whom he recognised as employees of the Army Supply Administration's research department and then got to meet Kurt Diebner who had been tasked by the Army Supply Administration to research the extent to which the new nuclear physics discoveries could be used for military purposes before the war was over, something which was as yet unknown to Bagge.

Bagge of course gave a sigh of relief that he would not be sent to the front line. Instead, Diebner gave him the task of setting up a meeting at the War Department with a number of physicists who would be summoned there to discuss the development within nuclear physics. The list of names that Diebner gave Bagge showed a good selection (Diebner was a well-informed physicist) but the list included only experimental physicists. That was the intention, said Diebner, as the main point was to experiment.

Bagge still thought that a few theorists ought to be included and suggested Heisenberg, which did not go down well with Diebner. Heisenberg had been critical of Diebner's work when the latter had asked to be declared competent to be an assistant professor a few years previously. Diebner nonetheless consulted Bothe and Gerhard Hoffmann, who shared his doubt about Heisenberg. Heisenberg was therefore not summoned to the meeting.

The meeting with experimental physicists took place at the War Department at Hardenberg Strasse no. 12 on Saturday 16 September 1939. Those present in addition to Diebner and Bagge were well-known physicists and chemists such as Bothe, Flügge, Geiger, Harteck, Hoffmann and Mattauch, but above all Otto Hahn, the inventor of nuclear fission. They were all equally concerned at the prospect of being summoned there as Bagge had been one week previously, and they thought they had simply been called up for military service.

The meeting was opened by *H. Basche*, Diebner's line manager within the Army Supply Administration. Basche gave an account of what the German intelligence service had obtained on the nuclear physics research abroad, which could not exactly be anything new since everything had already been published in accessible scientific journals. Basche emphasised the possibility of using nuclear energy to produce electrical power. Diebner added that it could also be possible to produce weapons.

Otto Hahn was left the victim of conflicting emotions. Weizsäcker had already told him that the military were interested in the possibility of atomic bombs. Hahn had replied that if his discovery led to that possibility becoming a reality, he would take his own life. Weizsäcker had put his mind at rest, saying that research which interested the military might save many scientists from frontline service and keep the scientific institutes intact. It was a matter of doing the research at a suitable speed.

The fact that many of the German scientists entertained this hope of doing research to appear important and save the lives of men but not doing enough to give the Nazis an atomic bomb has been disputed. A widespread view, perhaps first offered by Samuel Goudsmit in his account of German nuclear weapon research, *Alsos* from 1947, has been that they wanted to but were unable to. Goudsmit, who had previously been a very good friend of Heisenberg, could never forgive the fact that Heisenberg's intervention was not strong enough to prevent Goudsmit's parents from being arrested as Jews in Holland and being sent to Auschwitz, where they were murdered on 11 February 1943.

In *Alsos*, Goudsmit is almost savage towards Heisenberg and taunts him for having failed with the German atomic bomb. Goudsmit also omits to quote some statements that place the German scientists in another light. Max von Laue, for example, whose integrity and honesty were doubted by no-one, has written that a German bomb never came to fruition 'as no one of us wanted to lay such a weapon in the hands of Hitler'. And von Weizsäcker has said that the German scientists did not succeed with the bomb since they never actually wanted to succeed. But these statements have also been contested. However, it *is* possible that they never believed that the project was viable, but in any case wanted to be 'getting on with something' that the government thought was sufficiently important for them to avoid being sent to the front line.

At the meeting of 16 September, Otto Hahn was also able to clearly see the difficulties that had to be overcome. He knew of Bohr's and Wheeler's paper, *The Mechanism of Nuclear Fission*, in the latest edition of *Physical Review* on uranium-235 and that this isotope had to be separated from the rest of the uranium if you wanted to make a bomb. As a chemist, he thought that this involved an almost impossible task. In a more optimistic frame of mind was Paul Harteck, who had experience of separating isotopes through thermal diffusion. The crucial words came from Hans Geiger, who said that if there was a possibility of extracting energy from uranium, yes, if just the mere hint of a possibility, it had to be researched.

Thereafter, Bagge repeated his proposal that Heisenberg must be included in the continued work or else there was a risk of him being called up for military service and killed. Since a number of his friends were now present, there were no longer any protests and Diebner accepted the proposal. Heisenberg thus accepted a summons to the Army Supply Administration on 20 September.

After this first meeting, Schumann, head of military research, recommended to a General by the name of Becker, whose task was to keep an eye on what the scientists got up to, that a separate 'Arbeitsgemeinschaft Kernphysik' [nuclear physics working group] ought to be set up with Diebner as head. And this is what happened. After that, all research results concerning nuclear physics were classified as secret in Germany.

The next meeting of the working group, which now also included Heisenberg, took place on 26 September, two days before the German-Soviet friendship pact which enabled Russia to act at will in the Baltic States and against Finland. Although just one week had passed since the previous meeting, the scientists had been active. Heisenberg began by introducing an overview of the problems.

The nuclear energy could be extracted in two different ways, he said. The first involved building a 'uranium burner' in which you could split uranium in a chain reaction with the neutrons released from the previous nuclear fission. In order to achieve the fission, the neutrons had to be slowed down or else they would not be captured by the atomic nuclei of uranium-235 which were fissile. They must be slowed rapidly or else the neutrons would move for a while at a very high ‘resonance energy’, which would mean that they would be inclined to capture by uranium-238 but unfortunately without achieving any nuclear fission. If this were the case, they would be unnecessarily lost. The solution to the problem was to distribute the uranium in a substance that rapidly slowed down the neutrons without capturing them, i.e., a moderator.

The second way would be to create an explosive energy, a bomb, but this would not be possible with the normal uranium, i.e., uranium-238. The only way would be to isolate uranium-235 using a separation method.

Harteck had access to a Clusius-Dickel apparatus for isotope separation using thermal diffusion and had previously used it to separate xenon and mercury isotopes. He had concluded that the only viable way of obtaining uranium in gaseous form was to use uranium hexafluoride (UF6), which is a very unpleasant gas to work with since it easily causes corrosion and requires working temperatures of more than 50°C.

Harteck had also ideas of how a ‘uranium machine’ could be built by alternating layers of uranium and a moderator. He had with him a work document entitled ‘A way of layering uranium and heavy water to prevent resonance absorption in uranium-238’. It was *Dr. Hans Suess*, grandson of the famous Austrian geologist *Eduard Suess* (1831-1914), who had proposed heavy water. The possibility of simplifying the construction by using graphite instead was discussed at the meeting. It is remarkable that Harteck headed directly for layering uranium and moderator, the same idea conceived by Fermi and Szilard during the summer, rather than proposing a reactor where the two substances were homogenously mixed as Fermi had first imagined.

Diebner and Bagge had drawn up a plan for the continuation of the work, written down in a work document called ‘Preparatory work plan to start experiments on the usefulness of nuclear fission’. Heisenberg would lead the theoretical work. Bagge would measure the capacity of the heavy hydrogen to slow down neutrons. Harteck would continue with the endeavours to separate uranium-235 isotopes.

The Army Supply Administration wanted to concentrate all this research in Berlin-Dahlem and take over the Kaiser Wilhelm Institute for Physics there. Heisenberg protested, saying that scientists would be more effective in their home environments. He did not want to leave his institute in Leipzig. Reluctantly, Diebner had to give way but the military took over the Kaiser Wilhelm Institute for Physics in October 1939 and issued an ultimatum to its manager, Dutchman Peter Debye: become German or disappear! Debye found a way of compromising; he obtained leave of absence until the end of the war to travel to the United States and teach at Cornell University. During that time, Carl Friedrich von Weizsäcker would act as the manager.

Before Debye left Germany in February 1940, he met *Warren Weaver* (1894-1978) in Berlin, who was head of the Rockefeller Foundation’s Natural Science Department. He told him what had happened, and that the army had taken over the Kaiser Wilhelm Institute for Physics to develop an ‘invincible offensive weapon’. But scientists at the Institute had set their sights on something else, said Debye. In his minutes of the meeting, Weaver wrote:

> With D they consider it altogether improbable that they will be able to accomplish any of the purposes the Army has in mind; but, in the meantime, they will have a splendid opportunity to carry on some fundamental research in nuclear physics. On the whole D is inclined to consider the situation a good joke on the German army.

In October 1939, German nuclear physics research was under the control of the army and had all the support that could be expected at this stage. The United States did not have the equivalent situation until August 1942. The German scientists had a big lead but not much in the way of motivation.

The Ministry for Education’s initiative in gathering together scientists under the protection of Abraham Esau had led to no progress other than the ban on the export of uranium and the fact that Esau, with the help of the Ministry of Economic Affairs, succeeded in sequestering all available uranium for the ‘Physikalisch-Technische Reichsanstalt’. Esau had had the green light from the Nazi regime, but this was not an adequate recommendation for entry to the Army Supply Administration’s ‘Arbeitsgemeinschaft’. Military scientists in Dahlem instead demanded to take over Esau’s uranium.

Esau appealed to his boss at the Ministry for Education, Professor Mentzel, and reminded him that the original initiative for the uranium research had been taken by himself and not by the military. To Esau's surprise, Mentzel's response was that the army had been doing uranium research 'for years' and that Esau had got the idea from them. The astounded Esau then wrote to General Becker and pointed out that the uranium problem as such had not even been known for a year as yet. If the War Department sequestered the uranium that belonged to the PTR, this would damage an activity that was already well planned, not to mention in a situation when everyone's cooperation was needed. However, General Becker, who was aware of what was really going on, did not back down and the indignant Esau had to hand over his uranium reserves to the Dahlem Institute.

The military scientists could not be accused of being idle. I.G. Farben was asked to deliver at least one litre of uranium hexafluoride, and within two weeks received 100 g uranium to work with for that very purpose. Harteck immediately received 6 000 Reichsmark to plan a larger-scale copy of the Clusius-Dickel apparatus which he already had at his disposal, and would later receive more money for this purpose. *Deutsche Gasglühlicht Auergesellschaft* ('Degea', the Auer gas-glowlight company) in Oranienburg, 25 kilometres north-west of Berlin won a contract for the production of extra-pure uranium oxide. Oranienburg also had a concentration camp and the option of using slave labour.

Auer/Degea had a great deal of experience of handling natural radioactive substances. Thorium was produced from monazite sand for use in glow mantles for gas lanterns, and substantial interest was shown in radioactive luminescent paint. The company had an important laboratory activity which was run by the 38-year-old *Dr. Nikolaus Riehl* (1901-1990) who had studied under Hahn and Meitner in Berlin.

When the Germans had occupied (the former) Czechoslovakia, Auer/Degea had gained influence over the uranium mines in Jachymov (Joachimsthal). The primary interest was in producing radium and, as for the Belgians, a by-product of producing radium was uranium. Riehl had understood the importance of uranium at an early stage and had arranged a contract with the War Department regarding the production of pure uranium oxide. He established a factory in Oranienburg with a production capacity of one tonne per month for that purpose. The first delivery would take place in January 1940.

Alongside the practical measures, Heisenberg wondered how a nuclear reactor ought to be constructed. The most obvious problem was how the chain reaction could be kept under control, assuming that it would actually get going. One morning at the end of October 1939, he thought he had arrived at a method and waited eagerly for Bagge, who was now also staying with him in Leipzig. The capacity of the uranium to capture neutrons in such a way that led to nuclear fission ought to be dependent upon temperature.* If the development of energy increased, the temperature would rise but the chain reaction would also be slowed down. The reactor would therefore be able to control itself. Heisenberg found this so remarkable that he sat down to write a couple of papers on the theory of the control of a reactor. The first was finished in November 1939 and the second in February 1940. These two papers formed the basis for the continued German reactor research during the war.

Heisenberg now thought that a functioning 'nuclear machine' would be obtained if 1.2 tonnes of uranium were to be combined with l tonne of heavy water in a spherical container surrounded by normal water to reflect back escaping neutrons. He believed that such a device would automatically adjust itself to a temperature of 800°C, which obviously required a pressure tank. He also believed that Harteck's proposal to layer the uranium and the moderator would make it possible to reduce the size of the reactor.

On 6 December 1939, Professor Heisenberg summarised the situation in a letter to the War Department:

> SUMMARY: The fission process in uranium discovered by Hahn and Strassmann can, as far as we now know, also be used for the large-scale production of energy. The most reliable method of building a machine suitable for doing so consists in the enrichment of the isotope U235. The greater the enrichment, the smaller the machine can be made. The enrichment of uranium-235 is the only way of making the volume of

* In reality, it is not just the capacity of the uranium to capture neutrons that leads to the dependency on temperature but also other phenomena such as a change in the density of the moderator with the temperature.

> the machine smaller than one cubic metre. It is, furthermore, the only method of producing an explosive which surpasses the explosive power of the strongest up to now.
>
> However, ordinary uranium can also be used for the production of energy without the enrichment of uranium-235 on condition that the uranium is used along with another substance that slows down neutrons from the uranium without absorbing them. Water is not suitable for this. On the other hand, based on current knowledge, heavy water or very pure graphite could be used. Very small contaminations can prevent the development of energy.

This was a fairly complete description of the position of German atomic energy at the end of 1939 and as regards the overall situation for atomic energy throughout the world. The knowledge was no greater in the United States. The German scientists had promises of sufficient quantities of uranium within a few months but the moderator was lacking: the heavy water or the pure graphite. At the start of 1940, accurate measurements were therefore made of the properties of these substances when they were hit by neutrons. To what extent did they absorb neutrons rather than simply slowing them down?

Graphite was the material that was available immediately because it was easily accessible - the only problem was obtaining it free from contaminants, primarily boron which, as with cadmium, is very inclined to capture neutrons. Professor Walther Bothe in Heidelberg undertook to measure the diffusion length for thermal neutrons (i.e., neutrons that have a kinetic energy of approximately 0.025 electron volts at room temperature[*] in pure graphite. The diffusion length is the average distance that a thermal neutron manages to move in a material before being captured by an atomic nucleus. Bothe found that the graphite he first worked with was not pure enough. He eventually measured diffusion lengths of around 60 cm but hoped to find over 70 cm when he obtained sufficiently pure graphite.

Bothe's preliminary measurement results caused Heisenberg some uncertainty, and in a new report in February 1940 he expressed doubt regarding the suitability of graphite as a moderator. However, Bothe continued his measurements, now with some degree of pessimism, and obtained more and more extraordinary results. Not until January 1941 was he able to submit a final report. The purest graphite he had experimented with (supplied by Siemens) had certainly not produced the anticipated diffusion length of 70 cm, only that of 35 cm. Bothe concluded that graphite was not a suitable moderator.

This was a mistake that would be of controversial importance to the German atomic energy programme. Bothe's graphite was probably not completely pure, possibly contaminated with boron. Equally erroneous results, maybe for the same reason, were obtained in Cambridge in England where von Halban and Kowarski were now doing research, but they were able to compare their results with Fermi's measurement results in New York. Fermi had evidently been able to get hold of genuinely pure graphite and had found that it was useable. The ever-present, anxious Szilard realised how important it was for this conclusion not to reach the German scientists. He has recalled a dispute with Fermi:

> When [Fermi] finished his [absorption] measurement the question of secrecy again came up. I went to his office and said that now that we had this value perhaps the value ought not to be made public. And this time Fermi really lost his temper; he really thought this was absurd. There was nothing much more I could say, but next time when I dropped in his office he told me that Pegram[†] had come to see him, and Pegram thought that this value should not be published. From that point the secrecy was on.

While so much nuclear physics activity had taken place in Germany after the war had broken out, nothing tangible had happened in the United States during September 1939. Szilard became even more anxious and wondered what Sachs was up to. At the end of the month, he and Wigner visited the financier and found that Einstein's letter had not yet been given to President Roosevelt. Sachs explained that he

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[*] You might think that only such neutrons would be referred to as "slow", but slow neutrons usually refer to all neutrons with a kinetic energy of less than 100 eV, as opposed to fast neutrons with a significantly higher kinetic energies, greater than 0.1 MeV. The fission of uranium nuclei releases neutrons, the majority of which have energies in the range of 1-4 MeV.

[†] Dean of the Physics Section (see Chapter 2).

had spoken to the President's secretary but had heard that Roosevelt was so busy with work that it would be best to delay any visit. However, according to Sachs, the visit would take place within one week.

Szilard and Wigner were disappointed, and Szilard wrote to Einstein:

> There is a distinct possibility that Sachs will be of no use to us. If this is the case, we must put the matter in someone else's hands. Wigner and I have decided to accord Sachs ten days' grace. Then I will write you again to let you know how matters stand.

But Sachs did act within those ten days. He went to Washington DC and in the afternoon of Wednesday 11 October, he arrived at the White House and was ushered into the Oval Office to see the President. Roosevelt greeted him warmly: 'Alex,' he said, 'what are you up to?'

Sachs began – as a remarkable coincidence – to tell the same story that Hans Pettersson had told in *Göteborgs Handelstidning* three months previously. The way in which Napoleon had rejected the inventor Robert Fulton's proposal to use steam to help create an armada that could conquer the English fleet. Napoleon's scornful reply was: 'Ships without sails? Bah! Away with your visionists!' Roosevelt was in a good mood and laughed.

Sachs continued by saying that he was now in the same situation as when Fulton asked for the Kaiser's attention. The President held up a disapproving hand and then wrote a few lines on a piece of paper that he passed to an aide he had summoned.

After a short while, the aide returned with a well-packaged bottle. Roosevelt opened it and explained that it had been in the family for many years, having been kept for a suitable occasion. This was evidently the occasion. It was a bottle of Napoleonic brandy. Roosevelt poured out two glasses and passed one of them to Sachs, toasted him and leaned back to listen.

Sachs had with him Einstein's letter but did not read it aloud, and initially did not hand it over to Roosevelt either. Instead, he read out an 800-word summary which he had written himself and which he believed the President would be more easily able to understand. He also quoted Aston's statement from 1936. He took his time in particular with Aston's final sentences:

> Personally I think there is no doubt that sub-atomic energy is available all around us, and that one day man will release and control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbor.
>
> 'Alex', said Roosevelt, 'what you are after is to see that the Nazis don't blow us up.'
>
> 'Precisely!', said Sachs.

The President then took Einstein's letter and Sachs' notes and called his chief aide, brigadier Edwin ('Pa') Watson. He gave him the documents with the words: 'Pa – this requires action!'

5. SIEVERT AND MILITARY PHYSICS

THE CURIOSITY and desire to experiment with the nuclear fission phenomenon that was demonstrated by so many physicists in England, France, Germany and the United States during 1939 was not thought to exist in Sweden. With few exceptions, the established Swedish physicists were university lecturers who were short on resources and burdened with the after-effects of a stale, old academic system that did not encourage flexibility and new thinking.* The main exceptions, *Manne Siegbahn* (1886-1978) and *Rolf Sievert* (1896-1966), had carved out new careers for themselves. Siegbahn realised the importance of nuclear physics and proceeded to take a number of initiatives, including towards the construction of the first Swedish cyclotron. Otherwise, Sweden had scarcely any real nuclear physics before 1945. There was of course a certain Lise Meitner, a refugee under Siegbahn, but she was unhappy and isolated and had no opportunities to take initiative. There were also prominent theorists such as Oskar Klein, David Enskog and Carl Wilhelm Oseen, but they were also remarkably isolated and were not afforded the esteem that they were due, although Oseen had been head of the Theoretical Physics Department of the Academy of Sciences' Nobel Institute since 1933.

The lack of theoretical physicists in Sweden was pointed out by Wolfgang Pauli in a letter to Oskar Klein in 1930. Pauli wrote:

> [...] I hope that you will now fulfil the words 'Go now and *teach* the people'. Your great pedagogical talents have always been one of your greatest strengths and a broad field of application awaits it in Sweden. Up to now there has scarcely been any theoretical physics in Sweden of course [...]. something which is unreasonable compared with experimental physics which is so brilliantly represented by Siegbahn and Hulthén.†

Klein made many remarkable contributions to theoretical physics. In spite of his in-depth knowledge, however, he was an atomic physicist rather than a nuclear physicist. Within the field of radiation, he is best known for the *Klein-Nishina formula*, an expression which mathematically shows the way in which electromagnetic waves are scattered towards electrons and which was worked out in cooperation with Japanese physicist *Yoshio Nishina* (1890-1951) when they both did research under Bohr in Copenhagen. Using the Klein-Nishina formula can help us to calculate the intensity of the scattered radiation in different directions. With an increase in quantum energy in the incident radiation, the radiation is scattered in more of a forward direction.

In spring 1938, Rolf Sievert and his colleagues had moved from the old premises on Fjällgatan in Södermalm into the new building on the hill above the southern gates of Haga Park next to the new *Karolinska Sjukhuset* [Karolinska Hospital] building which would be inaugurated two years down the line. Sievert's Institute of Radiophysics was one of the three buildings that constituted King Gustaf V's Jubilee Clinic in Stockholm and had been erected by means of a grant from the Jubilee Fund that was set up in 1928 with funds collected when Gustaf V reached the age of seventy. The other two buildings consisted of Radiumhemmet's big new building and the smaller Radiopathological Institute closer to Radiumhemmet, linked with the latter by means of a corridor bridge.

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* Even though university rules emphasised the importance of scientific competence among the teachers as early as 1870, it was not until 1908 that requirements were set stating that education would be based on scientific research, and research was not named as an independent objective until 1916.

† *Erik Hulthén* (1891-1972) was Professor of physics at Stockholm University College from 1928-1959.

Up on the hill between Haga Park and Karlberg, there had long been a type of amusement park that included things like a rifle range and an open-air dance floor. When Karolinska Sjukhuset was inaugurated, you could still see the hollowed-out block of stone behind the hospital in the park which had lain behind the rifle range target for many years, a demonstration of a three-dimensional 'Gaussian curve' with the largest hollow behind the middle of the target.

The earliest within this area but for a long time separated from Karolinska Sjukhuset was *Eugeniahemmet*, which was established in 1879 under the Eugenia Home Foundation with the support of Princess Eugenia.* Its task was to 'offer housing, care and maintenance to the poor and terminally ill and crippled (but not children suffering from idiocy) from the capital and the provinces. Not only that: were such protégés who had been taken in to survive childhood, 'to continue to provide as required by Christian compassion and as permitted by the foundation's funds'. The operation was run until 1971 when the nursing tasks were taken over by Karolinska Sjukhuset.

The Institute of Radiophysics was *Sievert's* institute way beyond his being its manager. In the three-storey building, the uppermost floor consisted largely of Sievert's living quarters, a patriarchal arrangement that was relatively rare even in those times. In the other premises there were instruments and equipment which had sometimes been procured with research grants or funds from the Jubilee Fund but sometimes also with Sievert's private funds. The library, which was on the upper floor next to his living quarters, was altogether Sievert's private one and well-equipped with the big radiological journals and specialist literature of the time. There were also photographic and cultural history magazines. The library was far better stocked than could be expected of an institute of the small format that Sievert's still had. There were not many permanent employees who had had a university education: biologist *Arne Forsberg* (1904-1975) and physicists *Sven Benner* (1900-1986) and *Robert Thoraeus* (1895-1970).

Sievert had been married for a second time to Astrid Östergren since 1932, a small, energetic woman, apparently insignificant but with a firm hand over house, home and the many children, four of whom Sievert had with his first wife, *Ingrid Sandberg* (whom he married in 1918), and three with Astrid.

In 1928, Sievert had sold all of his shares in *Sieverts Kabelverk* founded by his father and had then bought the Tvartorp manor near Rejmyre in Östergötland. Much of the practical running of Tvartorp was taken care of by his wife Astrid. Sievert had a large household and he did not seem to see any clearly-defined borders between the private household and the scientific activity – everything was a large family and the employees at the Institute were seen as a cross between children and distant relatives or faithful old servants.

The patriarchal hierarchy was maintained with a mixture of goodwill, friendliness and the sound of thunder when all was not going to plan. Sievert was careful not to get onto first name terms with anyone who neither had an academic qualification nor was a long-term member of staff. 'If you become too familiar, you risk being used,' he used to say, 'and that is to the detriment of both respect and friendship.' But Sievert had the deep interest of a good patriarch in the welfare of the employees and often intervened to provide assistance where necessary.

Sievert had become an assistant professor on the basis of his doctoral thesis, and his inputs into Radiumhemmet in the 1920 and 1930s had won him respect and affection among the radiologists, and primarily from the doyen of Swedish radiology *Gösta Forssell* (1876-1950) and from the short-statured head of Radiumhemmet, *Elis Berven* (1885-1966), who was sometimes called 'Sville' by irreverent interns since he was born with the very common family name Svensson, and sometimes 'the pastor' because of the way he preached to them as to how they should treat the patients.

Following the establishment of international contacts during the international radiology congress in Stockholm in 1928 and the defence of Sievert's and Thoraeus' theses in 1932, the main interest of the Institute of Radiophysics had been in medical physics at Radiumhemmet. This had led to new condenser chamber designs and 'radium cannons'. Sievert's correspondence in the early 1930s shows that the radium cannons aroused international interest and that enquiries came from various directions regarding the possibility of ordering similar 'cannons'.

* Eugenia or Eugenie (1830-1889) was the only daughter of King Oskar I and Queen Josefina.

The other main person in the Swedish military physics adventure is Karl Manne Georg Siegbahn, who was born in Örebro in 1886 and was thus ten years older than Sievert. Manne's father was a railway station inspector and the family moved to new places time after time. Manne was therefore lodging in Stockholm as a high school student where he went to Norra Real. When his father retired and moved to Lund, Manne went with him and began studying physics at the university in spring 1906.

The internationally best-known Swedish physicist at the turn of the century, *Janne Rydberg* (1854-1919), had missed out on the Professorship in Physics that become available in Lund in 1897. Although Rydberg had been placed at the top of the list by the experts and recommended by both the Senate and the Chancellor, in 1900, the government let the Professorship in Physics go to mathematician *Victor Bäcklund* (1845-1922). However, the year after that, Rydberg got a newly-established Professorship in Experimental Physics and simultaneously became the head of the Institute for Physics. So, when Manne Siegbahn came to Lund, there were two professors there, a theorist and an experimentalist.

In the entrance hall of Radiumhemmet: Professors James Heyman and Elis Berven.
In the background to the left: Robert Jaeger.

Siegbahn soon became assistant to Janne Rydberg, who was loaded down with educational obligations and did not have much time left for research. Manne had already defended his thesis at the age of 25 since he had also studied at the Universities in Göttingen, Munich, Paris and Berlin during the summer holidays. He took an interest in x-ray spectrometry under Sommerfeld in Munich.

Since Janne Rydberg suffered ill-health, Siegbahn often had to deputise for him and became the acting manager of the Institute on a more formal basis in 1915. When Rydberg died in 1919, Siegbahn was called upon to succeed him. His assistant was Gudmund Borelius, who was given the main responsibility for the teaching since Siegbahn preferred to devote himself to research, which he did with great success. He built on *Henry Moseley's* (1887-1915) experiments with x-ray spectra but improved the technology and the measurement apparatus. Siegbahn discovered the M series* in 1916 in addition to investigations into the K and L series.

In addition to Borelius, Siegbahn's colleagues in Lund included a few future Professors: Erik Hulthén and *Axel Lindh* (1888-1960) as well as *Wilhelm Stenström* (1891-1973), a Swedish physicist who later succeeded in becoming head of the medical radiation treatment at a university clinic in Minneapolis, which was no mean achievement.

In 1922, Gudmund Borelius took up the Professorship in Physics at the Royal Institute of Technology in Stockholm and in the same year, Siegbahn was called up on to succeed the Professor of Physics *Gustaf Granquist* (1866-1922) in Uppsala. He accepted, thinking there would be better research resources. The following is said in a biography about Siegbahn's arrival in Uppsala:†

> His entry into the patriarchal environment in Uppsala was a shock for the older-school men but was welcomed by the doctoral students. Siegbahn was irreverent towards the traditions and conducted himself with a more relaxed style of work.
>
> At the Uppsala Institute, the rock-solid tradition from the German universities of the 1800s had been predominant. This also brought with it respect, not to mention deference, for the Institute's collection of instruments. The instruments were primarily intended for demonstrations and lectures. They were therefore often designed as elegant gadgets and were made of mahogany and polished brass.
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> In this respect, Siegbahn also represented a radical new approach where the instruments were seen primarily as tools for research. This outlook also included the collection of old instruments; if there was a need to use parts from old apparatuses in new designs, the old instruments were unceremoniously taken apart.

In Uppsala (during 1923-1937), Siegbahn came by many good colleagues, a number of whom will appear as the story goes on, such as *Hannes Alfven* (1908-1996), *Bengt Edlén* (1906-1993), *Sten von Friesen* (1907-1996), *Erik Ingelstam* (1909-1988), *Torsten Magnusson* (1907-1987), *Per Ohlin* (1910-1974) and Robert Thoraeus. In 1924, Siegbahn published his famous manual, *Spektroskopie der Röntgenstrahlen* and won the Nobel Prize in Physics in 1925.

In the mid-1930s, the Academy of Sciences started a research institute for experimental physics in Frescati north of Stockholm, opposite the Academy building on the other side of Norrtäljevägen. The research institute would be a part of a Nobel institute of the type set up by the prize-giving organisations to support the Nobel Foundation's activities. The formal name was the Academy of Sciences Research Institute for Physics but since it also functioned as a department of a Nobel institute, many preferred the shorter name of the 'Nobel Institute'.‡

Nobel institutes were already mentioned in Alfred Nobel's last will and testament. Their task would be to assist in the assessment of prizeworthy research achievements. Following negotiations between the executors of the will and the Academy of Sciences, the Academy received a contribution of 600 000 Swedish kronor to set up a Nobel Institute. The first plans were for a new building to house a department for research in physical chemistry. This was because in 1904, *Svante Arrhenius* (1859-1927), who was

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* In order to understand the designations 'K', 'L' and 'M', the easiest way is to look at the much simplified atomic model that describes an atom as a positively-charged nucleus surrounded by electrons in 'shells' at specified distances corresponding to defined energy levels. If an electron is knocked out of one of the inner shells when energy is supplied to the atom, the space is filled by an electron that 'falls down' from a more energy-rich shell. The atom thereby loses energy which radiates out in the form of characteristic x rays. This radiation is named after the energy levels between which the energy loss takes place. If an electron 'falls' to the innermost (containing the least energy) shell (K), the radiation is called K-radiation. If it falls to the second innermost (L) it is called L-radiation, etc. If you draw a curve of the way in which the x rays absorption capacity of a substance changes with the wavelength of the radiation (or quantum energy), the curve is shown to have cuts (absorption edges) at the wavelengths corresponding to the binding energies of the electrons in the various K, L, M, etc. shells.

† Olle Edqvist in *Kosmos*, 1987.

‡ The Nobel Institute was nationalised in 1964 and was then named the Research Institute for Atomic Physics (AFI). In 1988, the name was changed to the Manne Siegbahn Institute for Physics (MSI). The MSI was replaced on 1 July 1993 by the Manne Siegbahn Laboratory (MSL). The many different names have caused a great deal of confusion.

Professor of Physics at what was then the Stockholm University College, had received an attractive offer of a post in Berlin. Arrhenius had said that he would prefer to stay in Sweden if he received a corresponding offer. The Academy of Sciences then set up a Nobel Institute for Physical Chemistry with the support of the industry. Arrhenius was invited to take charge of the Institute on a Professor's pay.

In 1927, Arrhenius stepped down from being the manager of the Institute at the age of 68 and died a few months later. Since the first Nobel institute had been founded on Arrhenius' reputation, the way in which the Academy would act in the years to come was not clear. However, in 1933, a Department for Theoretical Physics was set up at the Academy's Nobel Institute with O.W. Oseen in charge. Oseen moved into Arrhenius' villa, which had been converted into an official residence.

This was the position from which Manne Siegbahn started recommending a research institute for experimental physics as well. Following much hesitation, The Academy of Sciences undertook to pay for the erection of the new Institute. The Wallenberg Foundation promised to contribute 55 000 Swedish kronor per year to cover the running costs, and His Majesty undertook to also contribute 48 000 kronor per year until 1951. Siegbahn became head of the Institute and was given a personal Professorship on 1 July 1937. He would remain in service until 1964 when the Institute was nationalised. He was then 78 years of age.

Since the Nobel Institute was not nationalised but was owned by the Academy of Sciences, Siegbahn was able to act on a fairly independent basis and run the activities more or less as a private company. He attracted a large number of Swedish physicists, often on quite uncertain temporary appointments. The Nobel Institute came to be near enough the only institute in Sweden where physicists were educated in modern physics, and certainly not by means of courses but through its activities, its research environment and Siegbahn's supervision. Siegbahn's previous operations in Lund and Uppsala meant that many of the country's physics professors and research administrators - including Tage Erlander - were also his pupils.

The first major research instrument at Siegbahn's new institute was a cyclotron*, which was constructed in 1937-1939 by Sten von Friesen. The basic principle for multiple acceleration had been stated by the Swede *Gustaf Ising* (1883-1960) as early as 1924. The American *Ernest Orlando Lawrence* (1901-1958) in California was responsible for the first practical design of a cyclotron, an enterprise for which he won the Nobel Prize in Physics 1939, justified by:

> [...]for the invention and development of the cyclotron and for results obtained with it, especially with regard to artificial radioactive elements.

The cyclotron became a powerful instrument for generating nuclear reactions. The early nuclear scientists such as Fermi, Irène Curie, Frédéric Joliot, Otto Hahn and Lise Meitner had bombarded atomic nuclei with neutrons in the hope of forming heavier atomic nuclei. When they irradiated uranium with neutrons, they had all achieved nuclear fission but only Hahn and Meitner had understood that fact. Other physicists such as Rutherford and his colleagues Cockcroft and Walton had previously used charged particles as projectiles, alpha particles from radium or polonium and accelerated protons. However, the accelerators that had been available had not been based on multiple acceleration and had therefore not led to particularly high acceleration energies. The cyclotron made it possible to use ions of heavier substances and accelerate them to high energies. The Nobel Institute's first cyclotron could accelerate particles to an energy of seven million electron volts (7 MeV).

At the same time, cyclotrons were built in several other places in Europe. This is what Sten von Friesen, the Stockholm cyclotron designer, had to say:

> While we were working here in Stockholm, cyclotrons were being built in Copenhagen, Paris, Liverpool and Cambridge. Jackson Laslett and Otto Robert Frisch worked in Copenhagen, Bernard Kinsey and Harald Walker in Liverpool, and Donald Hurst in Cambridge. When we longed for some change, we arranged to meet at the

* See note on p. 17.

various construction sites to discuss things such as ion sources, oscillators, vacuum problems, safety systems, etc. This was very useful and pleasant for the participants.

The cyclotron in Copenhagen appears to have been the one that was first put to use. von Friesen has said the following about starting up the cyclotron at Siegbahn's institute:

> Finally, the day arrived when it was time to try starting the whole thing up! The last thing I had built was a comprehensive safety system to prevent someone attempting to run it with insufficient vacuum, insufficient cooling, without access to the hall being blocked, etc., etc. I fetched Torsten Magnusson who wanted to be there at the first formal attempt to create a ray. [...] I pressed the start button and nothing happened! 'It's the safety system. It's too big and complicated. It can't possibly function,' said Torsten and disappeared into his own premises. It turned out that the system was so comprehensive that it was in principle impossible to start it electrically. However, I did eventually find that if I mechanically pressed down a certain relay using a peg, the whole thing got going. The problem was solved by redesigning it, followed by adjustments and improvements.
>
> In the afternoon of 13 October 1939, I succeeded in getting the ray for the first time and in the morning of 14 October, I was called to military service and travelled to Boden and Haapakylä by Torne älv [...].

At the time, just before the Second World War, Lise Meitner was in Sweden as a refugee and as a guest researcher with Siegbahn, but the relationship between her and Siegbahn was not the best, partly because Meitner was displeased to have been excluded from the Kaiser Wilhelm Institute for Chemistry and the contact with Hahn and Strassmann, and partly because she lacked the resources she had had and felt like a swan on a duck pond, and finally because she felt that she had not been afforded the esteem she had earned for her help with the discovery of nuclear fission. As for Siegbahn, he was almost embarrassed by her presence. Maybe he had a bad conscience about not being able to offer her better research resources, but he was no doubt also irritated by the attention she aroused among visitors to the Nobel Institute. When Siegbahn really wanted to demonstrate and discuss his own research projects, the visitors seemed primarily interested in meeting the famous Lise Meitner.

Sievert's main interest at this time was to formulate the first Swedish radiation protection law on which Swedish Parliament would make a decision in 1941. A reorganisation of his laboratory was suggested in 1939. The laboratory started to receive many questions from doctors and county councils throughout Sweden on the dimensioning of radiation protection walls and advice on suitable equipment. A rough estimate already showed that it would not be possible to find enough time to cope with giving this advice and doing the medical physics work for Radiumhemmet. Nor would the grants from the Jubilee Fund and the Cancer Society be enough.

Sievert's solution to this problem was ingeniously simple. If the activities were required by law, fees could be charged for the advice.[*] Together with the then assessor of the civil and criminal appeal court, later the County Governor of the County of Gotland *Martin Wahlbäck* (1901-1985), Sievert sat in the evenings and, as he himself has described it, 'laboriously put together a proposal' for a radiation protection act. Sievert and Wahlbeck also had help from the Assistant Secretary *Erik Björkquist* (born in 1901) of the Ministry of Health and Social Affairs (Björkquist became Director General of the Medical Board in 1947). Thoraeus and Benner helped to work out which governmental supervisory operations should be proposed. Everything was ready in 1939, but too late to be put before the 1940 Parliament. However, the proposal was adopted in its entirety in 1941 with minor amendments.

On 1 September 1939, the German army invaded Poland. Already before the war broke out, the National Commission for Economic Defence Preparedness had given the Swedish Academy of Sciences (the IVA) the task of making an inventory of Sweden's resources in laboratories for physical, chemical

[*] A previous piece of legislation had been drawn up by the Medical Board in 1932 and submitted to His Majesty but, following a six-year consideration period, the Social Affairs and Health Minister said no, bearing in mind the costs.

and technical research. The IVA had sent out a questionnaire for this purpose. *Edy Velander* (1894-1961), engineer (in electrotechnology), deputised for the IVA's Managing Director and founder, Councillor *Axel Enström* (1875-1948), and would succeed Enström as Managing Director in 1941. Velander envisaged a cooperation between the IVA and the scientific institutes through an activity in which the IVA would provide technical and scientific advice while the heads of the institutes for physics and chemistry would each form physical science and chemical science committees.

The equivalent of the IVA in the area of general science, the Royal Swedish Academy of Sciences (KVA), had taken a number of national committees under its wing. In 1939, many of Sweden's physicists were members of the National Committee for Physics of which Manne Siegbahn was chairman. The KVA's National Committee considered itself better able to coordinate the physicists than the IVA. Sievert, who was asked by the IVA to act as the contact person for the physicists, acted quickly in consultation with Siegbahn. On 4 September, he had already produced a draft for a physical science commission consisting of the heads of Sweden's institutes for experimental physics. There were ten people, including Manne Siegbahn and Sievert himself. The other eight were:

Professor Gudmund Borelius	The Royal Institute of Technology (KTH)
Lecturer Helmer Bäckström	The Royal Institute of Technology
Professor Erik Hulthén	Stockholm University College
Professor Erik Ingelstam	Chalmers University of Technology
Professor Gustaf Ising	The Geophysical laboratory in Djursholm
Professor John Koch	The University of Lund
Professor Axel Lindh	Uppsala University
Professor Harald Norinder	Uppsala University

Borelius has already been mentioned. He had been Professor of Physics at KTH since 1922 and had taken the initiative towards a separate department for technical physics there. *Helmer Bäckström* (1891-1964) was head of the photographic laboratory at KTH and a prominent figure within Swedish photography. Erik Hulthén had worked with Bohr in Copenhagen from 1927-1928 and was then Professor and head of Stockholm University College's Institute for Physics in 1928.

Erik Ingelstam was the youngest in the group at just 30 years of age. Although Sievert called him Professor, Ingelstam still had no professorship. He worked temporarily as head of the Institute for Physics at the Chalmers University of Technology in Gothenburg pending the provision of a permanent manager. Gustaf Ising had held the Professorship in Physics at Stockholm University College from 1920-1928 until it went to Erik Hulthén. Ising was a clever designer of electrical measurement instruments and had gained the title of Professor in 1934. From 1936, he ran his own geophysical laboratory in Djursholm. *John Koch* (1878-1950) was the oldest in the group at the age of 61. He had been Professor of Physics at Chalmers from 1914-1924 and had then succeeded Manne Siegbahn as Professor in Lund. Axel Lindh had also succeeded Siegbahn, but in Uppsala in 1937 after five years as Professor of Physics at Chalmers. *Harald Norinder* (1888-1969) eventually became known as the 'thunderstorm professor' and was then in 1932 Professor of Electricity at Uppsala University and head of department at the Institute for High Tension Research.

On 5 September, Sievert wrote to these physicists in his capacity as contact person for the IVA, referring to the IVA's assignment and requesting details of materials and personnel resources. It is interesting to note that it had not occurred to Sievert and Siegbahn that the theoretical aspect of physics could be of any interest. What they were primarily interested in was laboratory resources. Sievert wrote:

> At the request of the Swedish Academy of Engineering Sciences, which has been asked by the National Commission for Economic Defence Preparedness to produce an inventory of Sweden's resources in respect of physical and chemical laboratories as quickly as possible, I hereby provide you with a form for you to fill in, to provide details concerning the institute of which you are in charge.
>
> The details refer to scientists, students or other people at the institute, whom you consider to be of importance to the execution of scientific work in the event of war according to the enclosed memorandum.

Please send all details to the above address.
Yours sincerely,
ROLF SIEVERT

Sievert did not want to write ‘Yours sincerely’ to two of the addressees, Ingelstam and Koch, and left it at ‘Yours faithfully’. The fact that this applied to the youngest and the oldest may be a sign of the generation gaps that prevented familiarity.

Two days later, Edy Velander from the IVA wrote the following to Manne Siegbahn regarding ‘the coordination of research’:

> The onset of crisis has meant that the investigation concerning the coordination of technical and scientific research that has been ongoing for some time must now be intensified to at least a provisional solution. For such a purpose, the National Commission for Economic Defence Preparedness has asked the Swedish Academy of Engineering Sciences to start by making a quick inventory of Sweden’s resources in the form of scientific and technical-scientific laboratories and technical testing departments. A questionnaire has been sent out and I am pleased to ascertain that the laboratories have generally without hesitation made themselves available, which is a part of our common endeavour to make available all of the country’s resources to strengthen our wartime preparedness.
>
> The Academy then intends to convene a conference with the leading laboratory managers and other technical and scientific scientists to discuss current problems and deliberate the ways in which the laboratories could most effectively be used for the common goal. However, prior to this, the Academy’s management finds it particularly urgent to discuss these matters in a core group of people. The Academy has taken the liberty of assuming that it can count on your kind assistance with this objective and hereby invites you to a meeting that will take place at the Swedish Academy of Engineering Sciences, Grevturegatan 14 on Monday 11 September at 10.00.
>
> Should you be prevented from participating, we would be very grateful for a telephone conference with the Academy’s Managing Director, Councillor A.F. Enström, or the undersigned, both of whom can be reached on 62 28 98 or 62 35 35.
>
> *Stockholm, 7 September 1939*
> E. VELANDER
> Deputy Managing Director

The result of the meeting to which Manne Siegbahn and a few other scientists were called and the continued actions within the IVA led to a proposal that was submitted to the heads of the Ministries of Defence, Trade, and National Economy on 2 December 1939 with a proposal that His Majesty might like to appoint through the Department of Trade a chairman of a central committee for the Research Preparedness Organisation (FBO). His Majesty did so on 30 December by appointing the former Director General of the *Vattenfall* board, former Cabinet Minister *Gösta Malm* (1873-1965), to spend 1940 and 1941 as the chairman of this committee. A work programme for the FBO was not established until February 1940.

Sievert was more energetic. Neither he nor Siegbahn was enthusiastic about a cooperation that was directed by the IVA and industrial interests. They wanted a more independent cooperation between Sweden’s physicists. A ‘physicists’ executive committee’ was formed consisting of Sievert as the executive member and physicists Hannes Alfven, Erik Bäcklin, Helmer Bäckström, Carl Hugo Johansson, Gustaf Ising, Harald Norinder and John Tandberg. Sievert appointed Sven Benner as scientific secretary. The executive committee met practically every week over the next few months and was given reports by military experts on the physical problems encountered by the Defence.

Erik Bäcklin (1893-1947) was Assistant Professor in Physics at Uppsala and had defended his thesis on determining the wavelength of x rays in 1928. *Carl Hugo Johansson* (1898-1982) was a lecturer at KTH and one of Borelius’ favourite students; he went on to become head of Nitroglycerin AB’s detonics laboratory. *John Tandberg* (1896-1968), who went on to become a Professor in 1945, was a researcher at AB Electrolux and was a versatile man. He was a popular radio lecturer and had published works on angle trisection and on Esaias Tegner's interest in physics. He was also a lover of practical jokes, which often left his colleagues unsure as to what was seriously meant and what was a big joke.

At the end of November 1939, the Russians attacked Finland and the Finnish winter war began, lasting until 12 March 1940. It suddenly brought the World War within touching distance and made it appear more threatening than when the conflicts had taken place further away. Sievert's executive committee therefore took its assignment extremely seriously. The demonstrations to the committee included detonator designs (12 December 1939), problems within the submarine forces (18 December), the Home Guard's mining (30 January 1940), 'experiences in Finland of landmines and other devices to combat tanks' (27 February) and the problems of the military engineers (12 March).

In order to prevent the necessary contacts with military experts becoming too much of an overload for the latter, on 12 January 1940, Sievert asked a number of contacts to state the times when contacts would be the least disruptive. The experts he wrote to were *Captain Harald Jentzen* (1902-1984) of the Army Administration; Air Force Director *Peter Koch* (born in 1886) of the Air Force; Captain *Edward Malm* (1899-1983) of the Army Supply Administration ; Commander *Henning Muhl* (1886-1960), inspector for the minesweeping service; Captain *G. G:son Sjöberg* at Blockhusudden; Captain *Hugo Svenow* (1904-1988) of the Air Force; and Marine Engineer *B.T. Swenzén* (1894-1965) of the Marine Administration. It was a good selection. The contact persons reached the tops of the ranks, three finally becoming generals.

On 1 February 1940, the central committee for the Research Preparedness Organisation met for the first time on the Swedish Association of Technologists' premises at Brunkebergstorg with Gösta Malm as chairman. The central committee was composed of highly esteemed people. There were Director Generals of the Swedish National Board of Trade, the Swedish National Board of Public Building, the Swedish Telecoms Agency's board and the board of the power company Vattenfall. The Academy of Sciences was represented by Professors Manne Siegbahn, *The Svedberg* (1884-1971) and *Arne Westgren* (1889-1975), Professor of Chemistry at Uppsala. The Swedish Academy of Sciences had five representatives, including Councillor Enström and Dr. of Philosophy *Ragnar Liljeblad* (1885-1967), Asea's strongman. Edy Velander was not a member but had been invited to participate by the chairman. Finally, there were representatives of the Swedish Association of Technologists, the Royal Institute of Technology, Chalmers University of Technology, the Swedish Cooperative Union with its MD *Albin Johansson* (1886-1968) and the Confederation of Swedish Enterprises. An illustrious crowd but scarcely energetic. And only men.

It was still the age of male domination. Women had long been excluded from the majority of official contexts, not due to gratuitous gender discrimination but because of requirements set by laws and regulations. They had certainly been able to take the school leaving certificate for a long time but, until 1923, only as private external candidates. From 1873, women had the right to study at university, but until 1909 they were excluded by law from secondary education teaching posts. Not until 1918 were unmarried women given the statutory right to said teaching posts and only with the Competency Act of 1923 could they obtain higher academic posts such as Assistant or Full Professor. Not enough time had yet passed to distinguish the year 1940 from the period of statutory discrimination.

Two female physicists had successfully busied themselves with radiation and radioactivity early on. One was *Eva von Bahr-Bergius* (1874-1962), Lise Meitner's female friend who became a lecturer in experimental physics in 1909 and taught at Brunnsvik's folk high school from 1914-1927. The other was *Eva Ramstedt* (1879-1974), who defended her thesis in 1910 and then studied under Marie Curie from 1910-1911. She became an assistant professor in radiology at Stockholm University College in 1915 and later became Assistant Professor and acting Full Professor in Physics there until 1928. She was also a lecturer at the Comprehensive School Training College in Stockholm from 1925-1932. But none of these women had a place among the authoritative men who on 1 February 1940 attempted to coordinate the Swedish research done in the interests of the Defence and to establish a work programme for the organisation through which this would take place.

While awaiting the meeting of that particular day, the preparedness organisation had acted through a provisional organisation committee consisting of Edy Velander, *Waloddi Weibull* (1887-1979), Professor of Machinery Parts at KTH, and Colonel *Sven Thorén* (1881-1963). These gave an account of the activities so far. The inventory of laboratory resources had brought in information from 347 laboratories, 82 of which could be seen as actual research laboratories with a total of 260 academics. The main contact persons had previously been appointed for the following branches of science:

Physics	Assistant Professor Rolf Sievert
physical chemistry	Professor Gunnar Hägg
biological chemistry	Professor Karl Myrbäck
technical physics	Professor Folke Odqvist
technical chemistry	Professors Arvid Hedvall and Bror Holmberg

The FBO's activities and the preparedness of the physicists had already begun to diverge before the meeting at the Swedish Association of Technologists. In January 1940, the physicists' own preparedness organisation had been formally placed under the National Committee for Physics.

The FBO was too unwieldy an organisation and was led by too many bigwigs who had too many other things to do. The inputs of the physicists were led by an inventive and energetic Sievert who, after a while, said contentedly to Siegbahn: 'See what you can do with a little enterprising spirit and your father's money!'

Increasingly references to the FBO therefore soon became rarer and the National Committee for Physics was referred to far more regularly as the umbrella organisation for physicists, with Siegbahn at the top and Sievert as deputy manager. Sievert led the physicists' work committee with a firm hand and took plenty of initiative while keeping in close contact with Sweden's managers of institutes for experimental physics.

Sievert was careful to emphasise the independence of the physicists. When the director of the national defence's chemical establishment, Major *Torsten Schmidt* (1899-1996), later a key figure in defence research, sent over a report on gas protection on 29 March, saying that it had been drawn up 'in connection with the Research Preparedness Organisation', Sievert's response was:

> [...] in order to avoid any misunderstandings, however, I should point out that the work did not devolve through the Research Preparedness Organisation but through the special physicists' organisation which, through the accommodation of the National Defence's chemical establishment, has been given the opportunity of looking at the establishment and gaining a few problems through the same. [...]

In spring 1940, Sievert requisitioned various materials for experiments and studies. *Åkers styckebruk* sent him trinitrotoluene for experiments with 'explosions by means of radio signals'; from Captain Jentzen he received a wing grenade and from the Army Supply Administration's armoury department he was able to borrow '1 landmine, not charged'. The Institute of Radiophysics' building was full of strange people observing strict secrecy.

On 28 March, the physicists' work committee met at Sievert's premises to discuss the possibility of setting up a military physics institute, a forerunner to the Defence Research Establishment and a number of practical problems accumulated at the same time. Physicists wanted to defer military service and anticipated that Sievert would support them. Physics institutes undertook tasks for which they had inadequate funds. Younger physicists took initiative without ensuring that they had formal support.

On 9 April 1940, the Germans invaded Norway. There was a great risk that Sweden would also be dragged into the war. Surprisingly, this dramatic occurrence is not reflected in Sievert's document on the military physics work.

Assistant Professor *Nils Ryde* (1906-1997) at John Koch's institute in Lund wrote to Sievert in June 1940, saying that he had directly contacted the Army Administration about the development of telephony with infrared light and that experiments were being carried out by research assistants *Lennart Minnhagen* (1913-2005) and *Eve Staffansson*.

On 1 July 1940, the physicists in Uppsala and Stockholm gave an account of the results they had so far at a demonstration before the Minister of Defence *Per Edvin Sköld* (1891-1972) and Supreme Commander of the Armed Forces *Olof Thörnell* (1877-1977). Sköld and Thörnell were positive and thought that the operation ought to continue. Sköld thought that the National Committee ought to request a grant to continue its research. The positive attitude led to the National Committee receiving a grant of 50 000 Swedish kronor to be allocated as necessary.

That same year, *Erik Rudberg* (1902-1980) was appointed as permanent Professor of Physics at Chalmers University of Technology and Erik Ingelstam's temporary post ceased. Rudberg immediately showed great interest in the physicists' preparedness work and wrote to Sievert saying he thought that the latter's account at a meeting of the National Committee for Physics was impressive. Ingelstam stayed at Chalmers with the help of an assistant professor's scholarship in physics to do technical scientific research.

In addition to the plans to create a military physics institute, Sievert began exploring the possibility of gaining access to a test bed for military physics experiments, including shaped charges. He therefore requisitioned a map of Greater Stockholm on a scale of 1:20 000 to search for a suitable area. On 28 August 1940, the physicists' work committee met to discuss the need for a military physics institute and a test bed.

On 18 August, the daily press carried an announcement regarding a new secret English weapon, an 'aircraft trap'. A German bomber was already purported to have been captured and brought down. *Aftonbladet* showed the following the next week:

> Parachute grenade
> air barrage
> against air strike
> remarkable invention made
> in America
>
> LONDON, Tuesday (AB)
>
> A new defence weapon has now been invented which will probably render the balloon barrage around London superfluous. It is a 'parachute grenade' which is said to be particularly effective in the event of an air strike.
>
> This grenade can be fired from a normal cannon and, at a height of around 8 000 m, releases a several-hundred-metre-long steel band that is fixed to a parachute. The intention is for the steel band to become entangled in the propellers of the attacking aircraft and military experts in America, where it was invented, consider it to be very important.

And so did Sievert. On 6 September, *Aktiebolaget Svenska Stållinor* confirmed an order of 1 000 metres of top-class piano wire of 2.5 mm in diameter. Sievert wasted no time. On 18 September, he ordered a further 2 000 metres of piano wire to be sent to Hemlin's mechanical workshop at Malmskillnadsgatan 34 B. This was the workshop that Sievert used for a long time to realise his design ideas. He also ordered 40 mm diameter steel pipe from *SEE Fabriks Aktiebolag* in Sandviken and low-smoke gunpowder from *Bofors Nobelkrut*. To all appearances, he wanted to experiment with rocket-borne steel wires.

As it became clearer that it was the National Committee for Physics rather than the Research Preparedness Organisation (FBO) that was responsible for the physicists' inputs, the organisation was changed. The National Committee's work committee - not to be confused with the first 'physicists' work committee' consisting of Siegbahn, Sievert and Professors Borelius, Hulthén, Ising and Lindh – increased in importance. In autumn 1940, the original contact group of eight Professors became known as 'The Swedish National Committee's Preparedness Committee for Physics', with Sievert as chairman and Sven Benner as secretary. It had grown by including the members of the first work committee and also a number of additional physicists, including Sten von Friesen and Torsten Magnusson from Siegbahn's Nobel Institute and Ph. Lic. *Matts Helde* (1910-1999) from Stockholm University College. A total of twenty people were called to the Committee's meeting under Sievert in autumn 1940.

Concretisation of the plans for a more permanent organisation and a test facility began. Sievert had found a suitable area of land by Grindsjön in Södertörn south of Stockholm in a fairly isolated position. In September 1940, he and Benner wrote to the head of the Air Force:

> The Preparedness committee within the Swedish National Committee for Physics hereby respectfully requests the Air Force's permission to initiate investigations into possibilities of arranging a provisional take-off and landing ground for landplanes in the immediate vicinity of Grindsjön in Södertörn. The National Committee's Preparedness Committee is preparing a proposal for a test station for the military physics work at Grindsjön [...]

Sievert submitted the letter on 24 September 1940 to Captain Svenow at the Swedish Air Force Administration's material department together with an accompanying letter with following warning:

> I would be particularly grateful to you if, at the same time as submitting this letter, you would verbally inform those who are involved with the testing that it is important for the landowners not to find out about the plans for a test station, for were they to do so, the price of the land would probably strongly increase.

The warning referred to the land that would be used as an airfield. The price of the land for the test station by Grindsjön did not worry Sievert. The land was owned by *Olof Arrhenius*, PhD (1895-1977), botanist and agricultural chemist, private scientists and son of Svante Arrhenius. Sievert was convinced be would be able to strike a beneficial deal with Arrhenius for the greater good. He was equally convinced that he could persuade Per Edvin Sköld that the State ought to invest money in the project. Sievert was very good when it came to persuasion.

On 5 October 1940, the National Committee for Physics approved a draft of a letter to the Minister of Defence with a request for 570 000 Swedish kronor to establish a provisional military physics institute which also comprised a test station. At the end of October, Sievert visited Council of War member *Uno Brunskog* (1895-1970) of the Army Supply Administration, Major General *Arthur Örnberg* (1883-1967) of the Swedish Air Force Administration, Colonel *Birger Hedqvist* (1894-1964) of the Army Supply Administration, and *Commander Granström* of the Naval Administration, to hand over the report by the National Committee on a military physics institute. The mills of the State then slowly began to grind.

In October 1940, Erik Rudberg and Sievert wrote to one another about the aim of the military physics research. Rudberg thought that the industrial and technical operations that had previously already taken place in Sweden had been underestimated:

> [...] Bofors has been working here for quite some time, with its own test sites and with products that hold their own on international comparison. The mistake is of course that this asset was not reserved in time for our national defence but was essentially used as an export industry. That is the old story.
>
> I really cannot believe that the principal difference you wish to draw between academic physics and technical physics research for military assignments is that real or tangible. It is true that there is usually a large gap between peaceful academic research - concerning the quantum theory treatment of the electromagnetic field, spectra, molecular, atomic and nuclear problems, for example – and technical physical research. But it is quite a different matter where military assignments are concerned. Your own copious and impressive list of problems that have been looked at by the physicists' preparedness committee is a superb example. Or are not the radio, high frequency or amplifier problems very close to those handled at L.M. Ericsson's, Aga-Baltics or *Svenska Radiobolaget's* [Swedish Radio's] laboratories? The radiation and illumination problems are basically not much different from those that Luma's laboratories prepared themselves to process. And if it concerns firearms, it is probably easy enough to get involved in assignments such as those that Svenska Kullager's laboratory in Gothenburg or C.W. Johansson's engineers in Eskilstuna could do a lot about. The credible thing that the physicists could have given the Swedish Defence so far has not been given due to their own knowledge of the newest physics - concealed from the technical scientists and not yet preached at the technical universities! – but the contributions from the physicists probably have the same explanation as the contributions from more technical people: capable people who have dedicated themselves to the task!

Sievert did not really comprehend Rudberg's apprehensions. This was probably because he and Rudberg, probably unconsciously, had completely different ideas of research in a targeted context. The problem has been described in a paper by Hans Weinberger*, who thinks that Sievert preferred a 'linear model' for cause and effect. It could also be said that Sievert advocated a 'vertical' model and Rudberg a 'horizontal', or that Sievert's model was hierarchical. Sievert saw the physicist as a source of ideas and the person who was able to ascertain through experiments whether or not an idea was viable. Then it would be time for the technologist to put the idea into practice and for industry to take care of production. Rudberg saw a need for all parties to work together simultaneously. Sievert and Rudberg were talking at cross purposes. Sievert's response to Rudberg's letter highlights this.

> [...] In consideration of what you have written, I have repeatedly read the sections of the report to which your observations refer and, pardon me for saying so, cannot actually consider them to be justified. The need for cooperation with representatives for technical research and production ought really to be clearly shown by the work assignment, as pointed out on pages 11 and 12, with particular emphasis placed on the arrangements for cooperation between physicists and representatives of technical research and the interplay between specialist designers and physicists. As regards discussion meetings, the need for physicists to be informed of the technical viewpoints on the problems has also been pointed out.
>
> So, I agree with you that the need for cooperation between physicists and engineers is crucial and important for both parties.
>
> Nothing would please me more than to be able to include the civil engineer alongside the physicist in the proposal rather than as a subordinate to him. However, I do think that a proposal which would involve technical research to the equivalent extent of physical research ought not, at least for the moment, to come from the National Committee for Physics, but that this ought to wait until a later date when there may be a chance of a central physical-technical institute.
>
> In my view, one of the difficulties when organising such a major establishment will be the coordination of physical and technical research since the unfortunate general view of the engineers is that the work of physicists has no direct connection with the necessities of practical life, and maybe also a certain tendency among the physicists to underestimate the technical research. As you may be aware, the work within my special area has been mainly technical physics. During my studies, I spent a year at the Royal Institute of Technology, although I cannot profess to have obtained any noteworthy knowledge from that (which was largely due to my own actions). I also have some experience of the industry from my younger days.
>
> I would be very pleased to be able to participate in the organisation of good cooperation between physicists and technicians. Personally, I am of the opinion that if you compare a technically orientated physicist and a physically orientated engineer, the personal qualifications are what mainly determine which one should be seen to have the greater capacity for the creation of new operations. Where working out details in a new method or a new design, anyone with any sense always relies on specialists.
>
> Finally, I would like to point out one thing that I feel it is unwise to include in the report, i.e., the fact that there is now already in several cases an absolute demand for the military work of the physicists to be more or less completely handed over to technical specialists to be developed in such a form as to enable production. [...]

Erik Ingelstam, who no longer had a permanent job after Rudberg started the Professorship in Physics at Chalmers, began to show great interest in helping with the planned military physics institute. In a letter to Sievert of 8 December 1940, he began to banter about the FBO's operations:

* Physics in Uniform: The Swedish Institute of Military Physics, 1939-1945, *Center on the Periphery, Historical Aspects of 20th-Century Swedish Physics* (Ed. Svante Lindqvist), Science History Publications, USA (1993).

> [...] The FBO is a very odd establishment. There was a great deal of interesting things to observe here at the big assembly for those who wanted to penetrate realities (or rather the lack of such) behind all pompous phrases. Yesterday, I heard a funny story about an FBO case from Professor Larsson[*] here. Hedvall[†] and some of his lads were to extract fuel products from wood and Velander and accomplices had been informed and in turn done a good bit of bragging about results, the IVA having given a grant, etc. The result was a very combustible extract and was considered to be very good. Larsson had received samples for analysis. They were approximately 250 g acetone + some g of certain prod. of acetone and calcium carbide, which were used – nothing more! Nothing from wood. Approximately 20 kg of acetone had been used in this test, so this method of solving our fuel question seems a little strange. [...]

Unfortunately, many sources of irritation and practical difficulties disrupted Sievert's work. These included 'domestic policy' conflicts, i.e., friction between an institute manager and his closest colleagues. The most notable such conflicts came between Koch and Ryde in Lund but also between Rudberg and Ingelstam in Gothenburg, although they were more good-natured in the latter case. In Lund, Koch thought that Ryde was being too arbitrary and was causing the institute expenses without consulting the manager, and Ryde complained in turn that Koch opposed him.

In the proposed budget for 1941, the head of the Ministry of Defence granted the funds that had been requested for a military physics institute (MFI) and for a test station. The secretariat of the new Institute was located in the Radio Physics Institute's building. The National Committee's work committee suggested setting up a board for the MFI consisting of Professors Siegbahn, Hulthén and Lindh and an executive member, with Sievert chosen as the latter. The board would also include a representative of the Defence Supreme Commander's staff. For as long as Sievert led the operation as an executive member, he would also function as head of secretariat.

The physicists who gave Sievert the most help with the practical problems when setting up the MFI, and primarily with the addition of the new test station by Grindsjön, were Sten von Friesen, Erik Ingelstam and Torsten Magnusson. Sievert drew outlines of the buildings and engaged architect *Sven Malm* (1902-1983), as he would on several subsequent occasions. On 6 February 1941, he sent Malm's drawings to von Friesen, Ingelstam and Magnusson and soon received their comments and proposed changes to details.

In Lund, Ryde continued his experiments with modulating light bulbs, but he had worries about the help he had counted on. On 17 March he appealed to Sievert:

> [...] I had succeeded in interesting a clever young physicist, fil.mag. [MSc] Kurt Lidén[‡] in the case and we had just begun a number of series of experiments when the lad was called up last Saturday just like that. He had previously been called up for 1 year and released last Christmas, so I had not expected them to take him from me again for some time. But, despite any denials, I suppose the military situation has reached crisis point. [...] Do you think it would be possible to have him released?

On 28 March, Sievert eventually negotiated with the Army Supply Administration's fortification board to buy land by Grindsjön from Dr. Olof Arrhenius. However, in April 1941, Arrhenius donated 130 hectares of land for the MFI's activity. Sievert had now achieved his goals. The MFI had become a reality, as would the test station by Grindsjön in the near future. The restless Sievert thereby began to lose some of his interest in the military physics operations; he had many other plans and had now been forced to neglect his Institute for Radiophysics for a long time. The arrangement with Sievert as both

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[*] *Erik Larsson* (1899-1985), Professor of Organic Chemistry at Chalmers from 1936-1951.

[†] *Arvid Hedvall* (1888-1974), Professor in Chemical Technology at Chalmers from 1929-1946.

[‡] *Kurt Lidén* (1915-1987) became Professor of Medical Radiophysics at the University of Lund in 1964 and made substantial achievements within radiophysics and radioecology.

executive member of the MFI's board and head of its secretariat was a problematic burden and Sievert made it understood that he did not want to act as either manager or head of secretariat for the foreseeable future. There were primarily two people who could conceivably succeed him as manager: Erik Ingelstam and Torsten Magnusson.

Magnusson became head of secretariat for the MFI in 1941 and its manager in 1944. The pressure on Sievert was thereby relieved and he could set about dealing with the consequences of the parliamentary decision regarding the first Swedish Radiation Protection Act on 6 June 1941. That same year, he became Professor of Radiophysics at *Karolinska Institutet* (the Karolinska medical university – co-located with *Karolinska Sjukhuset*, the Karolinska university hospital). As a physicist among the doctors he was discriminated against – he was only entitled to participate in the staff meetings if they concerned matters that directly involved his Institute.

The arrival of the Radiation Protection Act brought Sievert greater resources. The Medical Board became the supervisory authority and gave permits for the use of radiation sources, but the supervisory operations would be run by officials at Sievert's Institute of Radiophysics, thereby making the 'Institute of Radiophysics' a peculiar hybrid. It was insignificant as a university institute; the link to Karolinska Institutet was weak in terms of work and finance. As a research institute, it became more and more important due to Sievert's unique ability to squeeze grants from various places. But this research institute was not a university institute in the normal sense because Sievert strove for independence and was able to indicate that the instruction that accompanied the Radiation Protection Act decreed that his institute would run scientific research within radiophysics and biophysics. This meant that the Institute of Radiophysics became an independent State institution, and the radiation protection inspectors who were employed for the supervisory operations in accordance with the law were State officials with no link to the university activities. The organisation was further complicated through the fact that Sievert's institute was still a part of the Jubilee Clinic, with responsibility for medical physics at Radiumhemmet. And finally, when the Act came into force, the Institute of Radiophysics accommodated the secretariat of the MFI. This virtually incomprehensible organisational mish-mash suited Sievert very well in that he could play off one aspect of the operations against the other as necessary.

The 1941 Radiation Protection Act meant that permission was required for all use of ionising radiation and possession of the radioactive substances - a great step towards adequate radiological protection. The supervisory operations were organised into three departments: a department 'A' for checking x-ray installations for medical diagnostics and for technical and scientific purposes, a department 'B' for checking x-ray treatment equipment and a department 'C' for checking the radioactive substances.

Department 'B' was a direct continuation of the ambulant measurement activities in which Sievert had taken initiative in the 1920s and for which he had employed Robert Thoraeus and engineer *Paul Haglund* (1904-1985). It was therefore natural for Thoraeus to become head of this department. It was also natural for Sven Benner to become the manager of department 'C'.

The supervision of the x-ray diagnostics, including the x-ray equipment at Sweden's dentists, was on the other hand something new and also the most comprehensive supervisory operation. Sievert employed licentiate Matts Helde as manager of department 'A', whom he had got to know while working with the MFI. Sometime later, *Lars Lorentzon* (1905-1980), *Thor Wahlberg* (born in 1914) and *Lars-Eric Larsson* (1920-1997) were employed as radiation protection inspectors under Helde.

In 1941, the first extension of the Institute of Radiophysics took place, a narrow off-shoot on two floors to the east. The bottom floor had a laboratory for packing radium preparations which had a separate entrance from outside in order not to risk contaminating the main building with radon. Half a flight of stairs up, with an entrance from the staircase, a corridor with instrument cabinets led to a room 5 x 11 metres in size into which Sievert's library was moved. In the middle of the floor stood a line of black tables and chairs for the meetings (Sievert was totally colour blind and often chose black in order to be sure of having the same visual impression as others). Therefore, as well as being a library, the room was the institute's first conference room and above all, in the 1950s, became its central point where the supervisory meetings were held. The building was demolished in the early summer of 1997.

In this chapter, I have gone into detail in describing the establishment of the MFI because Sievert's initiative and work to coordinate the preparedness inputs of the physicists was to the considerable detriment of his operations with radiophysics and radiation protection. The continued operations within the MFI between 1941 and 1945 fall outside the framework of this account. The work at Grindsjön, where the facility was managed by physicist *Bengt Grabe* (1912-2011), was dominated during these years by tests using missiles and shaped charges. Due to the necessary secrecy, many good, if untrue, stories circulated about what happened. One that was often told concerned rabbits that were placed in cages around an explosive charge to see whether they would survive. The story goes that the number of rabbits was greater following the explosion than before it – that is to say that one rabbit was pregnant.

What might be considered remarkable is that none of the many scientists (several of whom were of Nobel Prize winner class such as Alfven, Siegbahn and The Svedberg) who wondered how physics could be used for the Defence's purposes appear to have thought of experimenting with nuclear fission. What had happened to people's curiosity?

A few people outside the circle of 'military physicists' had wondered, though. I have already mentioned Hans Pettersson and his prophetic article in *Göteborgs Handelstidning* in 1939. Tage Erlander's confidant, Professor of Theoretical Physics at the University of Lund, *Torsten Gustafson* (1904-1987), also had considered it. In the anti-Nazi book entitled *Tidsspegel*, published by ten Lund Professors in 1942 three years before the atomic bomb, he wrote about the discovery in 1938 of

> an enormous, energy-rich nuclear process, known as nuclear fission, using uranium and some other substances. The research into this nuclear process led to possibilities for fundamentally important information on the forces of nature. Hopes could also be entertained regarding the possibility of finding new, incredibly rich energy sources to serve society. However, there was also a glimpse of the terrible possibility of designing a bomb that was ten million times more powerful than one loaded with normal explosives. In this situation, it was impossible for the physicist to stop researching nuclear fission. He would certainly for a brief, particularly critical period be able to desist from expanding his knowledge on this area of nature, although this also involved a certain level of risk during other nuclear experiments of blowing up himself and his institute due to lack of knowledge. He could also give up searching for new energy sources, but he must, purely from a self-preservation point of view, look at whether or not such an blast effect could be provoked in order to have the possibility of combating any evil were any amoral elements of society or among the governments to succeed in designing a similar weapon to use terror to usurp power.

This was also a prophetic statement, but no Swedish experimental physicist appeared prepared to research 'such a blast effect'. They contented themselves with looking at possibilities of shaped charges using normal explosives. Torsten Gustafson had the vision but he was no experimental physicist. The only interested experimental physicist in Sweden at this time was Lise Meitner, but she lacked resources

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6. THE YEAR OFF, 1940

WHEN President Roosevelt handed over Einstein’s letter and Alexander Sachs’ observations to Brigadier General ‘Pa’ Watson on 11 October 1939 with an order for ‘immediate action’, the result was something of the mountain becoming a molehill. Arthur Compton has said that the immigrant European physicists were accustomed to research being supported by government grants, whereas this was not how it was in the United States where private sponsors were more common. ‘It was simply not in our tradition,’ thought Compton, ‘to use government funds to deal with a request for support for research projects that could definitely be considered to be important but which still involved research.’

What happened was that the President set up an Advisory Committee for uranium matters (the Advisory Committee on Uranium) consisting of an officer from each of the army and the navy, and the head of the National Bureau of Standards, *Dr. Lyman J. Briggs*. The committee met at the end of October along with Szilard, Teller, and Wigner and submitted a report to Roosevelt on 1 November, whose summary said that: ‘We believe that this investigation is worthy of direct financial support by the government’. A sum of USD 6 000 was consequently awarded for the study of the splitting of uranium from 1 November 1939 to 31 October 1940, a ridiculously small sum.

In his book about the Manhattan Project, Compton has written that the establishment of the ‘Uranium Committee’ delayed rather than accelerated the uranium research since the existence of the committee gave the world of research an impression that the uranium issue was now in safe hands. But not much happened and Szilard became more and more impatient.

However, something did happen in February 1940. Alfred Nier at the University of Minnesota succeeded in separating microscopic quantities of uranium-235 from the uranium that Dunning sent him. The separated uranium, collected on a piece of nickel foil, was sent by express letter to Columbia University in New York,

where it was irradiated with neutrons. The experiment confirmed Bohr’s theory that it was uranium-235 and not uranium-238 which was responsible for nuclear fission using slow neutrons.

Even though not much happened in the United States at the start of 1940, Frédéric Joliot was more active than ever in France. At the start of May 1939, he and von Halban and Kowarski had already registered three patents with *Caisse Nationale de la Recherche Scientifique* (CNRS) as patent holder (it was the national research fund, CNRS, that supported Joliot’s experiments). Two of the patents concerned the production of energy and the third one referred to an atomic bomb. However, the aim now was to design a reactor for the production of heat, primarily considering the possibility of running submarines on nuclear energy. The efforts received extra support from armaments minister *Raoul Dautry* (born in 1880) after the war had broken out in September 1939.

However, the possibility of using nuclear energy for a bomb was not considered to be realistic in spite of the patent. Physicist *Francis Perrin*, who had now joined Joliot’s group, estimated that 40 tonnes of uranium would be needed to achieve the critical mass that was required for a chain reaction with the fast neutrons released when uranium was split. It is easy to understand that such a critical mass exists if you consider that the surface of a sphere increases less than the volume of the sphere when the sphere is enlarged. If a uranium sphere is enlarged, more neutrons will find uranium nuclei to split inside the sphere, while the number that pass the surface of the sphere and thereby disappear from the sphere does not increase to a corresponding extent. The large quantity of uranium-238 needed made it clear that it was not possible to produce a bomb from natural uranium.

Instead, the French group experimented with combining the uranium with a suitable moderator to rapidly slow down neutrons to the low energies that can split uranium-235. It was assumed that the uranium could be obtained from what was then the Belgian Congo through *Union Minière du Haut*

Katanga. It was understood early on that the moderators that were possible in practice was graphite and heavy water. Joliot chose heavy water since he had difficulties finding sufficient pure graphite.

In Germany, they also wanted to use heavy water. Bothe's negative results with graphite showed that the it needed to be so pure that heavy water was judged to be cheaper. On 15 January 1940, Harteck wrote to Heisenberg about the need for heavy water, and later to the War Department that either heavy water production had to begin, or they had to secure heavy water from Norway.

The heavy water had no commercial use as yet and fetched a price of half a US dollar per gramme. It was a by-product of the production of ammonia, and the only major producer was *Norsk Hydro*, a company in which a significant shareholding belonged to *Banque de Paris et des Pays-Bas*. However, German companies such as *I.G. Farben* also had shares in Norsk Hydro. At the start of 1940, I.G. Farben heard that the War Department was interested in heavy water. Representatives of I.G. Farben contacted the Norwegians and offered to buy out Norsk Hydro's stocks of heavy water, also suggesting that the production rate ought to be increased. The Norwegians asked what the Germans intended to use the heavy water for, and the Germans refused to answer. That being the case, Norsk Hydro refused to sell the heavy water in February 1940.

When the French discovered this, Joliot asked the armaments minister Dautry for help. Dautry asked a man by the name of *Jacques Allier* to secretly attempt to get hold of the heavy water. Allier had recently been recruited to *Deuxième Bureau*, the military intelligence service, but had previously worked for Banque de Paris et des Pays-Bas. Allier succeeded in winning the confidence of the Norwegians and the water, a total of 185 litres in 26 drums, was secretly transported to Paris.

Deuxième Bureau insisted that secrecy was important. Demands were made of Joliot to isolate his two foreign colleagues, von Halban and Kowarski, for the time that the assignment took. Joliot sent them on holiday under supervision on islands that were not easily accessible and from which they returned to Paris when the heavy water has successfully arrived there.

However, Joliot's use of the heavy water had to be interrupted when the Germans occupied France. The water was then concealed in the prison in Riom, which later became notorious as a site of internment for the accused in the legal action brought by the Vichy government against leading servicemen and politicians of the Third Reich in 1942.

In Germany, Mattauch had discussed his distaste regarding the goings on not just with Weizsäcker and Flügge but also with *Paul Rosbaud*, the editor at *Springer Verlag* who helped Hahn and Strassmann to rapidly get their report into *Naturwissenschaften*. Rosbaud did not believe that it would be possible to produce an atomic bomb in a short enough time for it to have any immediate significance, but recounted Mattauch's fears to one of his acquaintances, a British metallurgist who was on his way home to Cambridge. The latter in turn reported to John Cockcroft, the man who had generated the first experimental nuclear reaction together with Ernest Walton in 1932. More and more scientists in England became aware of what could happen.

One of these scientists was Otto Robert Frisch, Lise Meitner's nephew. Faced with the threat of war in 1939, he had no longer felt safe with Bohr in Copenhagen. He convinced Professor Mark Oliphant to let him come to the University of Birmingham an Assistant Lecturer.

At the request of the British Chemical Society, Frisch wrote a general article on the development of nuclear physics in which he was influenced by Bohr's opinion that the fission of uranium-235 required slow neutrons. The fast neutrons that come from nuclear fission would only lose a little energy in each collision with the heavy nuclei of uranium-238, similar to a small, light ball bouncing back with almost the same amount of energy after colliding with a larger, heavy ball. The fast neutrons would therefore just gradually lose energy and, *en route* to low energies, pass a stage with a kinetic energy ('the resonance energy') of approximately 7 electron volts, which would mean that they would be easily captured by a uranium nucleus and generate a nuclear reaction – but not nuclear fission except in rare cases. Frisch later wrote that 'That process would take times of the order of a sizeable part of a millisecond […], and for the whole chain reaction to develop would take several milliseconds; once the material got hot enough to vaporize, it would begin to expand and the reaction would be stopped before it got much further. So the thing might blow up like a pile of gunpowder, but no worse, and that wasn't worth the trouble'.

The quantity of uranium needed to maintain a chain reaction was also calculated to be very substantial. In Paris in May 1939, Joliot's colleague Francis Perrin had given a formula to calculate this 'critical mass'.

German physicist *Rudolf Peierls* (1907-1995), who ended up as a refugee in Birmingham at the same time as Frisch, had improved Perrin's formula and gave an account of this in a paper that was published by the Cambridge Philosophical Society in October 1939. Based on known physical data on the likelihood ('the cross section') of a uranium-238 nucleus being split, Peierls, like Perrin, found that tonnes of natural uranium would be required to maintain a chain reaction. There was thus no cause to entertain the fear of 'atomic bombs'.

HEAVY WATER

WATER CONSISTS OF hydrogen (H) and oxygen (O) and has the chemical formula H_2O. Hydrogen has three isotopes, ^1H, ^2H and ^3H. The normal hydrogen (^1H) has an atomic nucleus consisting of just one proton; 'proton' is another name for the nucleus of the hydrogen. The double-weight hydrogen (^2H) is called deuterium. It has two nuclear particles, one proton and one neutron. The heaviest hydrogen (^3H) is called tritium. It consists of one proton and two neutrons. All three hydrogen isotopes can be found in water. There are thus five possibilities for water that is heavier than H_2O, i.e., DHO, D_2O, THO, T_2O and DTO. Since the hydrogen in nature consists of only 1 deuterium atom in 6 700 atoms of normal hydrogen, the naturally-occurring heavy water is rarely D_2O and usually DHO.

However, the heavy water that is of interest as a moderator in nuclear reactors is primarily D_2O, which is the substance that is usually referred to when talking about 'heavy water'. It can be produced through the electrolysis of water (normal hydrogen is released more easily than deuterium), but since deuterium is so rare, the process is pretty energy-intensive in that such large quantities of water have to be electrolysed. Another process is a chemical exchange reaction between gaseous hydrogen sulphide and water. It requires more than 40 tonnes of water and 135 tonnes of hydrogen sulphide to produce one tonne of D_2O.

Deuterium was discovered in 1931 by *Harold Urey* (1893-1981) and his colleagues.

Tritium, as opposed to the other hydrogen isotopes, is radioactive and disintegrates with a half-life of 12.4 years during the emission of a very low-energy beta radiation which is incapable of penetrating even one thousandth of a millimetre of body tissue. Although tritium is thus unstable, it does occur in nature. It is formed in the upper atmosphere through the influence of cosmic radiation and reaches the surface of the Earth as triturated water. The natural occurrence is as low as one tritium atom per 10^{18} (!) hydrogen atoms, however. The test explosions of hydrogen bombs have strongly increased quantities of tritium in nature, however.

Tritium was first produced in 1934 by Ernst Rutherford and his colleagues (including Paul Harteck). Its radioactivity was demonstrated in 1939 by Luis Alvarez and *R. Cornog*.

Willard Libby (1908-1980), creators of the carbon-14 method, showed in 1946 that tritium was formed by cosmic radiation, and he showed in 1951 that it is found in rainwater.

However, in February 1940, Frisch began to wonder whether the previous conclusions were correct. There were four logical possibilities for a chain reaction in uranium, i.e.

(1) the fission of uranium-238 with slow neutrons
(2) " fast neutrons
(3) the fission of uranium-235 with slow neutrons
(4) " fast neutrons

Niels Bohr had shown that (1) could be excluded. The alternative (2) was excluded in practice since the resonance capture of neutrons without any nuclear fission would compete too strongly. Alternatives (3) and (4) remained.

Alternative (3) was the most obvious but required neutrons to be slowed down faster than through innumerable small energy losses in collisions with uranium nuclei. In this respect, Joliot, von Halban and Kowarski had already stated the solution, i.e., to mix the uranium with a substance that had light atomic nuclei, i.e., a moderator. In the same way as a light ball loses a lot of its energy if it bounces against another light ball rather than a big, heavy ball, neutrons would lose large amounts of their kinetic energy each time they collided with one of the moderator's light atomic nuclei. They would therefore not need to stay for long in the energy range where they risked resonance capture of the uranium nuclei, but would survive until they became slow enough to be able to split uranium-235 nuclei.

The use of a moderator looked as though it could be practically possible if you wanted to create a controllable chain reaction in a nuclear reactor, but it would still be too slow for use in a bomb. The heat that developed would have time to vaporise the material and scatter it so that the process would be interrupted early.

In order to create a bomb, a chain reaction that took place within the space of less than a microsecond was needed. Could alternative (4) allow this possibility? At the start of 1940, Frisch had come to the conclusion that it might not be impossible to separate uranium-235 from uranium-238. He had experimented with a Clusius tube himself in Birmingham.

Frisch therefore began to study the possibility of making a bomb from pure uranium-235, something which no-one, not even Szilard, had yet thought of. He consulted Peierls and wanted to use the latter's formula to calculate the critical mass of uranium-235. This meant that he needed to have information on the fission cross section* for the splitting of uranium-235 with fast neutrons, but this was not yet known because no-one had ever had access to pure uranium-235.

The fission cross section for uranium (the value that Peierls had used when he calculated the critical mass of uranium-238 was 10^{-24} cm^2 (1 barn). Where uranium-235 was concerned, Peierls thought that each neutron that collided with the atomic nucleus ought to create a powerful effect. The likelihood of collision, the purely geometrical cross section, was then calculated for the whole of the cross section of the nucleus, 10^{-23} cm^2 (10 barn). Frisch put this value into Peierl's formula 'just sort of playfully' and was astounded by the result: according to this calculation, the critical mass of uranium-235 was only less than a kilogramme! A volume less than a golf ball!

Bearing in mind the rough estimate, the calculation was good. In the following year, Merle Tuve's group at the DTM (the Carnegie Institution's Department of Terrestrial Magnetism outside Washington DC) took measurements of the fission cross section for uranium-235 and was able to send better values to England. It was shown that the critical mass (without tamper to return some neutrons that would otherwise have been lost) was probably approximately 8 kg and with tamper around 4 kg. The question remained as to whether the chain reaction would be fast enough or whether the uranium would be vaporised and expand in time to interrupt the reaction. Frisch made a calculation whose result was 4 microseconds, i.e., much faster than the few milliseconds that he had estimated for the chain reaction with slow neutrons where it took time for the neutrons to be slowed down.

Frisch and Peierls informed Oliphant of the result of the calculation and were asked to write a report. The first section of this was headed 'On the construction of a "superbomb"; based on a nuclear chain reaction in uranium'. The bomb would have a power corresponding to many thousands of tonnes of dynamite, they calculated. In another section of the report, they wrote (according to Rhodes):

> 1. As a weapon, the super-bomb would be practically irresistible. There is no material or structure that could be expected to resist the force of the explosion [...]
> 2. Owing to the spreading 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 [...]
> 3. [...]It is quite conceivable that Germany is, in fact, developing this weapon [...]

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* In nuclear physics, a "cross section" is a measurement of the likelihood of a reaction. The cross section is usually stated as the imaginary surface that an incident particle must collide with for the reaction to take place. The surface area is stated using the unit "barn" (barn[wall], after a joke name) where 1 barn = 10^{-24} cm^2.

4. If one works on the assumption that Germany is, or will be, in the possession of this weapon, it must be realised that no shelters are available that would be effective and could be used on a large scale. The most effective reply would be a counter-threat with a similar weapon.

And so, in March 1940, it was known in England that a bomb consisting of uranium-235 would have destructive impacts and that these impacts would also affect civilians and make the bomb a terror weapon against which only a power balance based on the terror weapon could provide protection. This conclusion worried James Chadwick, the man who had discovered the neutron, the particle that made all of this possible. In Liverpool, Chadwick was now in the company of not only Joseph Rotblat but also Otto Robert Frisch, who went there in 1940 from Birmingham. But these two were foreigners. In an interview in 1969 (reproduced by Rhodes), Chadwick said:

> I remember the spring of 1941 to this day. I realized then that a nuclear bomb was not only possible – it was inevitable. Sooner or later these ideas would not seem peculiar to us. Everybody would think about them before long, and some country would put them into action. And I had nobody to talk to. You see, the chief people in the laboratory were Frisch and Rotblat. However high my opinion of them was, they were not citizens of this country, and the others were quite young boys. And there was nobody to talk to about it. I had many sleepless nights. But I did realize how very very serious it could be. And I had then to start taking sleeping pills. It was the only remedy. I've never stopped since then. It's 28 years, and I don't think I've skipped them a single night in all those 28 years.

On the other hand, Lord Hankey had felt justified in being able to sleep peacefully and had sent Appleton-Chadwick’s correspondence with the previous negative conclusions on the possibility of an atomic bomb to the War Cabinet, which had also received information that Thomson’s attempt to create a chain reaction had failed and that Oliphant had attempted to produce metallic uranium. The War Cabinet Secretariat felt that the situation was confused and wondered whether the various scientists really were aware of one another’s endeavours. They therefore asked Appleton to see to the situation again, this time in consultation with the Ministry of Air Defence.

In April 1940, Thomson reported his experiments and suggested that either heavy water ought to be used as a moderator or uranium-235 ought to be enriched so that ordinary water could be used. Thomson’s report went to David Pye, the head of the Air Force’s research. However, at the same time, Frisch and Peierl’s sensational report had also found its way from Birmingham. When they gave it to Oliphant in March, he had forwarded it to Tizard, who forwarded it to Thomson, who was still formally responsible for uranium research.

When Thomson read the report, he immediately realised its substantial significance. He asked permission of Tizard’s committee, which was now called ‘The Committee on the Scientific Survey of Air Warfare’, to discuss it with both Oliphant and Cockcroft. The possibility of an atomic bomb now seemed to have become realistic.

A visit to London by Jacques Allier, the man who had negotiated with the Norwegians for the heavy water on behalf of France, also increased the interest of the British in uranium research. Allier told Thomson, Oliphant and Cockcroft about the French uranium research and the importance of the heavy water from Rjukan* but that the Germans had also attempted to get hold of it.

The Englishmen asked Professor *A. W. Hill,* British scientific attaché in Washington DC, to find out what the Americans were up to. Hill spoke to scientists at the Carnegie Institution, who now – in spring 1940 - told him that practical applications in the interests of the military might well occur but not for a long time. In their opinion, the British ought to have more important things to be getting on with during the war. And if anything important happened, they promised it would be reported. Despite this lack of

* Rjukan is a small Norwegian place by the River Måna in Telemark. This is where Norsk Hydro had built a hydropower works and an ammonium factory since 1907. The plant for the production of heavy water was by the river in Vemork, 5 kilometres west of Rjukan.

encouragement, the British continued. A sub-committee for uranium issues was formed under Tizard's committee.

In the United States, there was no greater encouragement for those who, like Szilard, were eager to prevent a German advantage with atomic bombs. Szilard complained: 'I had assumed that once we had demonstrated that in the fission of uranium neutrons are emitted, there would be no difficulty in getting people interested; but I was wrong.'

An important decision had nonetheless been made at the start of 1940. The Uranium Committee had decided to use its 6 000 dollars to support Enrico Fermi's research into neutron absorption in graphite for the purpose of establishing whether graphite was a useable moderator. The same research was taking place in Germany, but it produced a negative result there due to contaminants in the graphite.

Szilard was convinced that an experiment using graphite rather than water as a moderator would produce a chain reaction. He was now very impatient. In the end, he approached Einstein once more, who was now in Princeton. Together, they formulated a new letter from Einstein to President Roosevelt and sent it to Alexander Sachs, requesting his help for a second time. The letter referred to information that the German military had now taken over the Kaiser Wilhelm Institute for Physics in Berlin-Dahlem and was concentrating heavily on uranium research. The efforts in the United States ought to be intensified.

Sachs forwarded the letter to 'Pa' Watson to be passed to the President. However, before Watson did this, he contacted the Uranium Committee to ask what the situation was. He received a reply from one of the committee's military explosives experts, Lieutenant Commander *Keith Adamson*, who said that no more could be done until the results of the research that was ongoing at Columbia University under the leadership of Professor Fermi were known. Roosevelt wrote to Sachs, telling him that the case would best be dealt with by the Uranium Committee but that, owing to Einstein's letter, he had summoned the committee to a meeting on the afternoon of 27 April. He invited Sachs to participate in the meeting along with Fermi, Pegram, Szilard and Wigner.

In spite of the reminders from those who were given special invitations, the Uranium Committee stuck to its guns. No more could be done until it was known which moderator was suitable. Fermi was now the key person at Columbia University, and he was able to purchase the graphite that was needed courtesy of the grant from the Uranium Committee. When making the purchase, he followed Szilard's advice about where he could find the purest graphite, a total of five tonnes, which were now delivered to the Pupin laboratory. Herbert Anderson has described the situation:[*]

> Fermi returned to the chain reaction problem with enthusiasm. This was the kind of physics he liked best. Together we stacked the graphite bricks in a neat pile. We cut narrow slots in some of the bricks for the rhodium foil detectors we wanted to insert, and soon we were ready to make measurements.
>
> [...]
>
> A precise schedule was followed for each measurement. With the rhodium in place in the graphite, the source was inserted in its position inside the pile and removed after a one-minute exposure. To get the rhodium foil under the Geiger counter in the allotted 20 seconds (because its induced half-life is only 44 seconds) took coordination and some fast legwork. The division of labor was typical. I removed the source on signal; Fermi, stopwatch in hand, grabbed the rhodium and raced down the hall at top speed. He had just enough time to place the foil carefully into position, close the lead shield and, at the prescribed moment, start the count. Then with obvious satisfaction at seeing everything go right, he would watch the flashing lights on the scaler, tapping his fingers on the bench in time with the clicking of the register. Such a display of the phenomenon of radioactivity never failed to delight him.

[*] The quote is from Rhodes (Rhodes, 1986).

The result of Fermi's experiment showed that the graphite ought to be a useable moderator and they now had the green light to physically build a uranium pile with graphite and natural uranium. But there was still a lack of resources and backing.

In Sweden, Lise Meitner was in anguish over the limited possibilities of research. The thing she primarily wanted to succeed with was demonstrating the mysterious 'element 93' which had eluded Fermi in Rome for so long. The discovery of nuclear fission had led to all of the substances that were observed following the irradiation of uranium with neutrons being presumed to be fission products. In February 1939, George Placzek had been indignant at Bohr's statement that they were now 'finally clear of all stories of transuraniums'. Placzek had thought that some neutrons could always create transuraniums. Lise Meitner agreed.

Otto Hahn and Lise Meitner had already found a radioactive substance with a half-life of 23 minutes in 1936 following the irradiation of uranium with neutrons. The year after, Meitner, along with Hahn and Strassmann, had shown that the new substance was a uranium isotope, uranium-239. This had arisen according to the following formula:

$$\frac{1}{0}\mathrm{n} + \frac{238}{92}\mathrm{U} \rightarrow \frac{239}{92}$$

The new uranium disintegrated during the emission of beta radiation, i.e., electrons. It could then be expected to give rise to an atomic nucleus with a loss of a negative charge, i.e., with the addition of a positive charge and thus with atomic number 93:

$$\frac{239}{92}\mathrm{U} \rightarrow \frac{239}{93}? + \frac{0}{-1}e$$

Lise Meitner thought that it had to be possible to demonstrate the question-mark substance, 'element 93'. In 1939, *Edwin McMillan* (1907-1991) had almost discovered it when he irradiated uranium with neutrons using the cyclotron in Berkeley. He found a radioactive substance with a half-life of 2.3 days. Emilia Segrè, who cooperated with McMillan, found that this new substance had the same chemical properties as the rare earths. Since these are close to barium and lanthanum in the periodic table, Segrè assumed that the new substance was a fission product.

However, Lise Meitner was sure that the 2.3-day substance was the 'element 93' they were looking for. McMillan and Segrè had irradiated thin foils of uranium with neutrons. The new substance lay embedded in the uranium. Meitner saw this as evidence that it could not be a fission product; the recoil at the time of nuclear fission would have knocked the new nuclei out of the uranium foils.

However, in order to be able to carry out the experiment that would demonstrate 'element 93', Lise Meitner needed a strong neutron source. No such thing was to be had at Siegbahn's institute; the new cyclotron had been started up to test it in October 1939 but was certainly not yet fully operational. Meitner was extremely displeased with her situation. Having just come to the Nobel Institute, she had written to Hahn when she had not received the help that she had expected (this and the following quotation are from Sime's book about Lise Meitner):

> Dear Otto! […] Perhaps in principle [Siegbahn] does not dislike that it worked out this way, he would much rather use the institute money for mechanics and big machines than for an academic person and scientific problems. In the whole big institute, only 5 academic people are employed, and they too work almost entirely on equipment problems. Scientifically I am completely isolated, for months I speak with no one about physics, sit alone in my room and try to keep myself busy. You cannot call it 'work'.

In January 1940, she wrote to Hahn that her work was

> …as meaningless as ever. Just to keep busy, I did some experiments with the rare earths. I am not very interested in it, but one must do something. All day long I sit alone in my workroom, and for days I do not speak a word. In this way one has plenty of time to think over the world situation, a good method for staying slender.

Since Lise Meitner had waited for a long time for the Nobel Institute's cyclotron to be commissioned to no avail, in April 1940 she decided to travel to Copenhagen to carry out her experiments using Bohr's cyclotron. She travelled on 8 April and arrived in the evening before the German invasion. She stayed with Bohr for three weeks. After returning to Stockholm, she wrote the following to her nephew Otto Robert, who had left Copenhagen to lecture in Birmingham and ad already got around to making the important estimation of the critical mass of uranium-235:

> [...] I had come to C[openhagen] just 12 hours before the big event took place when no-one had the slightest suspicion that such a thing could happen. [...] We were awoken by the rumble from numerous aircraft at around quarter to six in the morning and there was nothing you could do apart from wait to see what would happen next. The central post office, the newspaper editors, the radio station and police stations were occupied almost immediately but you saw only a few soldiers on the streets and they were all speaking – *mirabile dictu* - Danish. They were not doing anything official. No great everyday life difficulties arose while I was there. The scientific work continued as usual, apart from the fact that all types of meeting (including 'doctoral thesis defences') have been banned. [...] Niels and Margrethe were obviously very unhappy about the events but he does not intend giving up his work, in spite of having received three offers from other places over the first two days. [...] I cannot drop this subject without mentioning how wonderful Niels was the whole time.

Bohr was not out of danger. He would be counted by the Germans as 'non-Aryan' since his mother was Jewish. There was also a cyclotron at his institute which was valuable to the Germans, and it was well-known that many prominent Jewish physicists worked there. George de Hevesy was still there helping Bohr to remove letters that were too compromising and other documents that could harm someone were they to fall into the hands of the Germans.

These also included the German Nobel Prize winners Max von Laue's and James Franck's gold medals, which they had asked Bohr to store. Since the export of gold was a serious breach of the Nazi laws and because the names of the prize winners were inscribed on the medals, the Nobel medals and other gold medals that von Laue had given to Bohr constituted a threat to the two German scientists. De Hevesy quite literally solved the problem in the way he described in a letter to von Laue after the war (quoted from Sime's book on Meitner):

> After the occupation of Copenhagen, your medals were Bohr's first concern. He was not interested in your medals but in your person. I proposed that we bury the medals, but since your name was engraved on them this did not satisfy Bohr. Dissolving the medals was the only way to make them disappear. I spent the entire first day of occupation with this not very easy task. […] [Later] the Nazis occupied Bohr's institute and searched everything very carefully, especially the vault where your medals had been stored. If they had found your medals in their original state, you would probably have landed in prison and would surely have wished you had never received them.

After the war, the gold was recovered, and the Nobel Foundation had new medals engraved for von Laue and Franck.

Before Lise Meitner returned to Sweden, Bohr asked her to send a telegram from there to a friend in England, physicist *Owen Richardson*, and say that all was well with the Bohr family. The wording of the telegram that Richardson received was somewhat unclear:

> MET NIELS AND MARGRETHE RECENTLY BOTH WELL BUT UNHAPPY ABOUT EVENTS PLEASE INFORM COCKROFT AND MAUD RAY KENT MEITNER

This telegram would have unexpected consequences. It was discussed by the subcommittee for uranium research since Cockcroft suspected that the word 'RAY' carried a hidden message. Frisch and Peierls believed that 'MAUD RAY KENT' could perhaps be an anagram of some important information. It was all so bewildering that the committee later (on 20 June 1940) took the code name 'M.A.U.D.' and became known as the Maud Committee. The explanation did not follow until after the war. Maud Ray, who lived in Kent, had been a nursemaid to the Bohr family. The details of the address in Kent had been dropped from the telegram.

Lise Meitner had been forced to leave Denmark without having managed to demonstrate the ‘element 93’ she had been searching for. On the other hand, Edwin McMillan and Phil Abelson did manage to do this a few weeks later in Berkeley and their article, ‘Radioactive element 93’, was published in *Physical Review* on 27 May 1940. They later called ‘element 93’ neptunium, after the planet Neptune, the planet that is just beyond Uranus. Lise Meitner was mortified to have missed this discovery of ‘element 93’ with its half-life of 2.3 days. She wrote to Otto Hahn:

> I never believed that this activity was a rare earth, because it was too unlikely that a fission product would not fly out by recoil, and I intended to prove it with Niels. Unfortunately nothing came of it, but the fact remains that the first true transuranic (the unavoidable decay product of uranium-239) was found by us [in Berlin].

Meitner’s disappointment is understandable. She had found uranium-239 early on together with Hahn and Strassmann and she had drawn the right conclusion that the substance must have a daughter product with the atomic number 93. She thought that she had the right to the discovery. but she had never actually succeeded in demonstrating neptunium.

For both Lise Meitner and the scientists in Berkeley, the discovery of neptunium was a physical feat worth striving for. For Leo Szilard, the discovery had a more practical meaning, something which he had already understood when he corresponded with Princeton physicist *Louis Turner*. Turner had consulted Szilard about a letter he was intending to write to *Physical Review* with what he feared was too crazy an idea. If it was possible to create transuraniums and these in turn had daughter products that proved to be fissile like uranium-235, it would be easy to enrich the fissile nuclide. It would differ chemically from uranium.

They now had an easily-separable daughter product, neptunium. It was certainly not fissile, but the principle had been shown. It could be that someone would find a fissile nuclide that was not a uranium isotope.

In Germany, von Weizsäcker came to the same conclusion upon reading the article in *Physical Review* with great interest. This opened up new ways of extracting nuclear energy. Due to resonance capture, irradiating uranium with neutrons in a pile of uranium ought also to be able to lead to the formation of fissile transuraniums. The difficulty of enriching uranium-235, which many saw as impractical, could be disregarded. The ‘uranium machine’ ought to be able to produce a bomb material that would be easy to deal with. Weizsäcker wrote to both Heisenberg and Diebner about this possibility.

On 28 April, Debye came from Germany to the United States on enforced leave of absence and reported that the military had taken over the Kaiser Wilhelm Institute for Physics. On 5 May, William Laurence wrote a sensational article in the *New York Times*: ‘Vast Power Source In Atomic Energy Opened by Science’. Laurence described the possibility of separating uranium-235 and its explosive force and the ‘tremendous implications this discovery bears on the on the possible outcome of the European war’. He also wrote that ‘every German scientist in this field, physicists, chemists and engineers […] have been ordered to drop all other researches and devote themselves to this work alone’. The article romanticised Meitner’s flight from Germany and said that she had taken ‘the atomic secret’ with her.

Now, suddenly everyone could read what Szilard and his colleagues had spent so much energy trying to keep secret. As Szilard had feared, the article attracted great attention far beyond the United States. Laurence himself had hoped that the American politicians would wake up and realise the risk of the Germans succeeding in creating an atomic bomb. But no-one in Washington DC appeared to react.

Those who read Laurence’s article included a Professor of History at Yale University, *George Vernadsky* (1887-1973), who was the son of one of the Soviet Union’s most noted scientists, *Vladimir Vernadskij* (1863-1945). The latter had already spoken about visions of atomic energy at a talk for the Academy of Sciences in 1910. He had said: ‘And now in the phenomena of radioactivity new sources of atomic energy are opening up before us, exceeding by millions of times all the sources of energy that the human imagination has envisaged.’

Vladimir was born in St. Petersburg in a well-to-do family; his father was a Professor of Economics. During the 1905 revolution, he was involved in forming the Cadet Party. He was educated as a mineralogist and in 1911, with the support of the government and private funds, he succeeded in starting a comprehensive investigation into the presence of Russian radium. He got the Academy of Sciences to send expeditions to the Urals, the Caucasus and Central Asia for this purpose. During the big revolution, he was against the Bolsheviks but took no active part in the conflicts. Instead, he went to Crimea, which was still beyond the reach of the Bolsheviks and where his son George was Professor. Vladimir became Rector of the university there.

When the Red Army approached Crimea, his son George fled to Constantinople and from there to (the former) Czechoslovakia and finally to the United States and Yale University. However, the students convinced Vladimir not to abandon them, so he stayed with his wife and daughter. He was arrested by the Red Army and sent to Moscow where, thanks to influential friends, he was released and was able to travel to St. Petersburg, which was then re-christened Petrograd.

In Petrograd, Vernadskij and radiochemist *Vitalii Khlopin* (1890-1950) and geologist *Alexander Fersman* (1883-1945) formed the Radium Institute in January 1922 to coordinate all Russian operations with radium. Khlopin became head of its chemical department and Fersman of its geological. A third department, of physics, was headed by *L.V. Mysovskij*. Vernadskij was then already stating that the purpose of the new institute was to work to 'mastering of atomic energy'! In February 1922, Vernadskij wrote (according to David Holloway's book):

> We are approaching a great revolution in the life of humankind, with which none of those it has experienced before can be compared. The time is not far off when man will get atomic energy in his hands, a source of power that will give him the possibility of building his life as he wishes. This could happen in the coming years; it could happen in a hundred years' time. But it is clear that it must be.
>
> Will man be able to use this power, direct it towards good, and not towards self-destruction?
>
> Is he mature enough to be able to use the power that science must inevitably give him?
>
> Scientists ought not to close their eyes to the possible consequences of their scientific work, of scientific progress. They ought to feel responsible for all the consequences of their discoveries. They ought to connect their work with the best organization of all mankind.
>
> Thought and attention ought to be directed to these questions. And there is nothing in the world more powerful than free scientific thought.
>
> Remarkably prophetic and wise words - and that was in 1922!

The main responsibility of looking after the Radium Institute gradually fell more and more to Khlopin, who did not have Vernadskij's visions. During 1922-1926, Vernadskij lived in Paris where he lectured at the Sorbonne. On returning to Russia, he used great integrity in criticising the politicisation of science, a dangerous attitude that required substantial civilian courage. At the start of the 1930s, he was in Moscow where he created a biochemical laboratory. He continued to be the leading figure when it came to radioactivity and investigations into uranium deposits.

When Vernadskij's son George at Yale University read Laurence's article, he immediately saw a confirmation of his father's early prophecies and wasted no time in sending him the article. Vernadskij was obviously very interested and wrote, together with Khlopin, to the Russian Science Academy's Department of Geological and Geographical Sciences that it was now more important than ever to chart the presence of Russian uranium deposits. They wrote that 'Uranium metal, which has found only limited application and has always been regarded as a by-product of the extraction of radium, is now acquiring absolutely exceptional significance'. To his son in the United States, Vernadskij wrote:

> Thank you for the clipping you sent from Washington DC from the New York Times about uranium. This was the first news about this discovery to reach me, and to reach Moscow in general. I quickly set things moving. On 25.VI a 'troika' was formed in the Academy under my chairmanship (Fersman and Khlopin) with the right of cooptation.

> Fersman is in Murmansk – but I started work quickly. We have to make use of the summer and autumn. I did not expect when Soddy first explained clearly the possibility of using intraatomic energy (more than 35 years ago) that I would live to see not only practical discussion of this phenomenon, which has a great future, but also work in this area. I think now that the possibilities that are being opened up for the future here are greater than the application of steam in the XVIIIth century and of electricity in the XIXth.

On 3 May 1940, the Germans conquered Rjukan and demanded an increase in the production of heavy water. In May, they confiscated thousands of tonnes of uranium ore in Belgium and entered France. The construction of a special building called the ‘Virus House’, intended for a German uranium pile, began in Berlin-Dahlem in June. Paris was occupied on 14 June and France capitulated on 22 June. Joliot and his wife Irène Curie decided to stay in Paris and support the resistance movement and ensure that the cyclotron in Paris was not misused, but von Halban and Kowarski were forced to flee. They had 26 containers containing 185 kilos of heavy water which, following a hazardous journey from Bordeaux through the Bay of Biscay and a Channel crossing, finally reached England and eventually secure storage at Windsor Castle. In England, the Maud Committee discussed whether von Halban and Kowarski were doing research within their fields of interest. The Englishmen had now concentrated fully on producing a bomb using uranium-235. Joliot’s research in Paris had been based on getting a uranium pile to become critical bearing in mind the possibility of obtaining an effective source of energy, not a bomb in the first instance. However, von Halban and Kowarski had intimated that resonance capture of neutrons in uranium-238 could perhaps lead to the formation of substances that would be fissile with slow neutrons. The as yet undiscovered element plutonium was thus anticipated by scientists in the United States and in Germany, France and England. In England, the conclusion was that von Halban and Kowarski ought to stay, and in August 1940 the two scientists were sent to the Cavendish Laboratory in Cambridge.

On 12 July 1940 in Russia, Vernadskij and Khlopin wrote to the deputy head of the government *Nikolaj Bulganin* (1895-1975) about the possibilities of nuclear energy; this was the first time the government of the Soviet Union had been informed. The Soviet Academy of Science’s presiding committee met on 16 July to discuss Vernadskij’s proposal. The presiding committee asked Vernadskij to write a memorandum to the government on the importance of atomic energy. The government approved the establishment of a uranium commission linked to the Academy’s presiding committee.

Vernadskij, who was now 77, declined the assignment of being chairman of the Commission, not due to age problems but because he had too many irons in the fire. Khlopin became chairman of the Commission at his proposal.

The Uranium Commission immediately sprang into action. Khlopin referred to known research results and thought that if it proved difficult to get a chain reaction by splitting uranium-238, there was no doubt that it could be achieved if the uranium were enriched with uranium-235.

The Academy of Science’s presiding committee asked the Uranium Commission to provide a research plan before 20 September 1940. But a proposal was already put forward on 29 August by the person who would become the leader of the Soviet nuclear weapons research, the energetic physicist *Igor Kurchatov* (1903-1960) who, due to his desire to direct his colleagues, had them call him ‘the general’. Together with *Yuli Khariton* (1904 – 1996), *Georgij Flerov* (1913-1990) and *L.I. Rusinov*, he submitted his own research plan to the Academy under the heading ‘On the utilisation of the energy of uranium fission in a chain reaction’.

It is not clear whether Khlopin was aware that the Kurchatov group had submitted its own work plan, in spite of the fact that from April 1939 to October 1940, Kurchatov was manager of the physics department at the Radium Institute where Khlopin then succeeded Vernadskij as head in 1939. There did not appear to be the best of cooperation between Khlopin and Kurchatov. That being the case, on 28 September, the Uranium Commission decided to submit a separate plan drawn up at the Radium Institute by Khlopin and *Alexander Leipunskij* (1903-1972). The plan was approved by the Academy of Science’s presiding committee on 5 October 1940.

The biggest problem for the Russian scientists was the shortage of uranium, despite Vernadskij’s prolonged endeavours to get the authorities interested in the mining of uranium. They initially started to believe in the possibility of obtaining a chain reaction in natural uranium using ordinary water as a moderator. At this early stage of research, it was the possibility of producing energy that the scientists were thinking of rather than a bomb.

Another problem was the antagonism between the two dominant groups. The first group originated from the Radium Institute and dominated the Uranium Commission. The leading scientists were Vernadskij and Khlopin and the emphasis was on chemistry and geology.

The other group consisted of physicists. The senior man among the physicists was *Abram Joffe* (1880-1960) who was head of Leningrad's Institute for Technical Physics and his pupils included Khariton and Kurchatov. Joffe's scientific qualifications lay within semiconductor physics but his main achievements concerned the building of a physics institute which came to mean a great deal in the education of Russian physicists. Those who had been educated there included Russia's greatest physicist, *Pjotr Kapitsa* (1894-1984), and later *Andrej Sacharov* (1921-1989).

Vernadskij and Joffe had difficulties cooperating. Vernadskij thought that the physicists were not sufficiently interested in radioactivity, although Joffe had built up a vigorous nuclear physics activity during the 1930s. At the Radium Institute there was a cyclotron which came into operation in 1939 but which did not work reliably until 1940. The physicists thought that the cyclotron ought to have been at Joffe's Institute. Kurchatov, who needed access to a cyclotron for his research, was forced to work at the Radium Institute before the physicists finally obtained their own cyclotron.

Vernadskij criticised the physicists for devoting themselves to theoretical nuclear physics when, according to him, the major problems were finding uranium and working out methods for the enrichment of uranium-235. However, the physicists did make important achievements. At the end of 1940, Khariton and *Jakov Zeldovitj* (1914-1987) calculated the critical mass for uranium-235, the same calculation that Frisch and Peierls had previously performed. They obtained an equally surprising result. According to their calculation, no more than 10 kg of uranium-235 would be needed to create an atomic bomb, a better estimation than the first one made by Frisch and Peierls. Their boss, prominent chemist *Nikolaj Semjonov* (1896-1986), reported to the next authority up, which was the technical scientific administration of the People's Commissariat for the Petroleum Industry, that it was now possible to produce a bomb with an unbelievable explosive power and that research ought to be concentrated on this weapon. But Semjonov's report did not make it through the Soviet bureaucracy up to anyone who dared to make decisions. The Soviet Union therefore did not have a Maud Committee as a result of the discovery.

On 20-26 November 1940, the fifth Soviet nuclear physics congress was held in Moscow with approximately 200 participants. Kurchatov held the most important plenary talk and summarised what had been done and the existing possibilities of creating a chain reaction. He said:

> Although the possibility of a nuclear chain reaction has now been established in principle, tremendous practical difficulties will arise in any attempt to realize it in any of the systems that have so far been investigated.[…] Perhaps the next few years will reveal other ways of solving this problem, but if this should not be the case, only new and very effective methods for separating the isotopes of uranium and hydrogen will make possible the practical realization of a nuclear chain reaction.

In the United States, William Laurence wrote another enlightening article on the danger of an atomic bomb, 'The Atom Gives Up', this time in the *Saturday Evening Post* on 7 September 1940, but with no noteworthy consequences. In December, Bohr held a talk on atomic bombs, but he still did not believe they were actually possible in practice.

In Berkeley, McMillan and his colleagues continued their studies of the decay chain that arises after uranium has been irradiated with neutrons. McMillan and Abelson were able to detect neptunium-239 before Lise Meitner, the isotope of the 'element 93' which she had predicted when she discovered uranium-239 and found that the nuclide disintegrated during the emission of beta radiation:

$$\frac{1}{0}n + \frac{238}{92}U \rightarrow \frac{239}{92}U \rightarrow \frac{239}{93}Np + \frac{0}{-1}e$$

McMillan drew the same conclusion regarding neptunium-239 that Lise Meitner had drawn about uranium-239. Since the neptunium was also unstable, there had to be a daughter product, and since the neptunium also disintegrated during the emission of beta radiation, the daughter product had to be a nuclide with atomic number 94 and mass number 239. McMillan began to search for this daughter product, 'element 94'.

One way of creating a nuclide with atomic number 94 was to irradiate uranium-238 with heavy hydrogen, deuterium, i.e., ^{2}H. However, before McMillan succeeded with his investigation into the postulated new element, he was summoned to the Massachusetts Institute of Technology to participate in radar research. Chemist with Swedish origins *Glenn Seaborg* (1912-1999) was chosen to continue the experiments.

On 14 December 1940, Seaborg and his colleagues irradiated uranium-238 with deuterium and succeeded in producing 'element 94', which in March 1942 they called plutonium after the planet Pluto, which lies beyond Neptune. The irradiation initially formed neptunium-238 plus two released neutrons. The reaction was:

$$\frac{238}{92}U + \frac{2}{1}H \rightarrow \frac{238}{93}Np + 2\frac{1}{0}n$$

The neptunium then disintegrated into plutonium during the emission of beta radiation:

$$\frac{238}{93}Np \rightarrow \frac{238}{94}Pu + \frac{0}{-1}e$$

The element plutonium was thus discovered in December 1940, but the scientists were not satisfied: they also wanted to detect the plutonium isotope that they assumed was formed during the decay of neptunium-239 following the irradiation of uranium-238 with neutrons. They therefore began the search for this isotope. The experiment was carried out by *J.W. Kennedy* (1916-1957), *E. Segrè, A.C. Wahl* and Glenn Seaborg. They irradiated a good kilogramme of a uranium salt with neutrons that had been generated by irradiating beryllium with deuterium which had been accelerated in the cyclotron. The neptunium formed was chemically extracted and cleaned of all traces of uranium. On 28 March 1941, they were able to ascertain that the neptunium had been transformed into half a microgramme of plutonium-239 which was estimated to have a half-life in excess of 20 000 years (it was estimated at 30 000 years; the right value is approximately 24 000 years).

$$\frac{1}{0}n + \frac{238}{92}U \rightarrow \frac{239}{92}U \rightarrow \frac{239}{93}Np + \frac{0}{-1}e$$

$$\frac{239}{93}Np \rightarrow \frac{239}{94}Pu + \frac{0}{-1}e$$

When Seaborg then quickly found that plutonium-239, like uranium-235, was fissile and the military significance of the substance was realised, they began using code names. Element 94 with mass number 239 could be written as 94-239, which was shortened to '49' as an assumed name. Those who worked on the plutonium project later called themselves the 'forty-niners' like the gold diggers in California in 1849.

The detection of neptunium had been published in *Physical Review* but the demonstration of plutonium and its fissibility was kept secret. However, as Lise Meitner had postulated the existence of 'element 93', many of the world's physicists had postulated the existence of an 'element 94' with an isotope that had the mass number 239 as a daughter product of neptunium-239. Although the detection of plutonium was kept secret, its existence was already anticipated.

One of those who were thinking about plutonium as a possible bomb material was the strange Austrian physicist Fritz Houtermans. Houtermans had had his higher education in Göttingen. In 1933, he emigrated to the Soviet Union where he fell victim to Stalin's terror and had to spend a number of years in prison. Following Stalin's and Hitler's friendship pact in 1939, he was exchanged for Russian prisoners in Germany where he was quickly imprisoned by the Gestapo. Thanks to the effort of Max von Laue, he was released and got a job as assistant to Baron *Manfred von Ardenne* (1907-1997).

Von Ardenne was a remarkable German private researcher who had his own electron physics laboratory in Lichterfelde in the south of Berlin. He possessed great energy and enterprise and was full of ideas and suggestions. He researched and designed everything that concerned the electronics of the time: the electronic microscope, radio, TV... and is usually referred to as 'the father of TV' (he took out a patent in 1931). Nuclear fission captured his interest immediately and he succeeded in obtaining research funds from a source as unexpected as the German post office. The nuclear physics research run by von Ardenne was nothing to do with cooperation with others such as Heisenberg or the German military scientists. He experimented with electromagnetic isotope separation in the hope of separating uranium-235.

Under von Ardenne, Houtermans was able to devote himself to theoretical studies of nuclear reactions and to calculate the critical mass of uranium-235. He concluded early on that an 'element 94' (i.e., plutonium) must exist and be fissile and thereby offer the easiest route to an atomic bomb. Houtermans discussed this with Heisenberg and von Weizsäcker. Houtermans said he was afraid that von Ardenne would understand the significance of 'element 94' and win Hitler's support for a weapon which, when there was no longer any need to separate uranium-235, would not be that unrealistic. This would put pressure on Houtermans who, if he failed, would be very much in trouble once again.

A number of sources say that both Heisenberg and von Weizsäcker were well aware of the alternative of basing an atomic bomb on plutonium, yet the possibility was still not mentioned in the official documents on German uranium research. According to some scientists, this supports the theory that Heisenberg was afraid that the Nazis would see the bomb as a viable project. When questioned by the Alsos group after the war, Houtermans said that Heisenberg stated the inverted phrase to 'put the war in the service of science' as a motto.

Houtermans feared that the development would put substantial pressure on the physicists to develop an atomic bomb and then probably with plutonium. He asked a colleague, physicist Fritz Reiche, who emigrated to the United States in spring 1941, to take a message to the American physicists. Reiche recalls Houtermans saying: 'Please remember if you come over […]. We are trying hard, including Heisenberg, to hinder the idea of making a bomb. […] Please say all this; that Heisenberg will not be able to withstand longer the pressure from the government to go very earnestly and seriously into the making of the bomb. And say to them, say they should accelerate, if they have already begun the thing . . . they must hurry.'

When Reiche came to the United States with his family, he went to Princeton where he was invited to live with a colleague, physicist *Rudolf Ladenburg*. He gave Ladenburg Houtermans' message. Ladenburg later wrote a supplement to a letter to the head of NBS, Lyman Briggs. These lines are of particular interest because, at this point in time, Heisenberg had no reason to give his uranium research colleagues a false image of his intent (quote from Thomas Powers):

> It may interest you that a colleague of mine who arrived from Berlin via Lisbon a few days ago, brought the following message: a reliable colleague who is working at a technical research laboratory asked him to let us know that a large number of German physicists are working intensively on the problem of the uranium bomb under the direction of Heisenberg, that Heisenberg himself tries to delay the work as much as possible, fearing the catastrophic results of a success. But he cannot help fulfilling the orders given to him, and if the problem can be solved, it will be solved probably in the near future. So he gave the advice to us to hurry up if U.S.A. will not come too late.

Briggs himself added a few rows to this letter in his response to Ladenburg: 'I am deeply concerned about the contents of your confidential note. If you learn anything further will you please advise me'.

Briggs himself did not let anyone know any more. Houtermans' warning and statement about Heisenberg were placed in an archive drawer. Maybe Briggs thought that they were getting on as well as they could.

At the start of the 1940s, for x rays and gamma radiation from radium there was a dose limit called a 'tolerance dose' which was recommended by the US Advisory Committee on X-Ray and Radium

Protection in 1934. It was 0.1 röntgen per day (approximately equivalent to 250 mSv per year). Six months later, the International Radiation Protection Commission (ICRP), at that time called the International X-ray and Radium Protection Commission, had adopted twice the value, 0.2 röntgen per day, but this recommendation made no great impact.[*] Until the start of the 1940s, all radiation sources had been seen as external. The situation changed with the experience of injuries from radium-226 that was stored in the bone tissue of female luminescent paint workers after they had licked their brushes while applying luminescent paint containing radium to watch dials and instruments.

This meant that in 1940, the American Advisory Committee began an investigation into the way in which radioactive luminescent paint ought to be handled. The result was published in May 1941 in the National Bureau of Standards' Handbook 19, later called NCRP Report No. 5. The Committee recommended a maximum permissible body burden of 0.1 microgrammes of radium. The recommendation was based on a proposal from MIT Professor *Robley Evans*, of which he himself has given an account (quoted from Taylor):

> [...]With the U.S. Military establishment cranking up for World War II, and radium-dialed instruments being produced in profusion, [there was a pressure on] me and others to set standards of safety for the radium-dial industry.
>
> [...]We found that […] rats were several hundred times more resistive to skeletally retained radium, on a µCi/g skeleton basis, than the few human cases which we had studied. We concluded that permissible levels for man had to be determined from observations on man. By 1941 we had studied about 20 persons with measurable body burdens of radium and had found 7 cases with residual body burdens below 0.5 µg and no injuries, whereas persons with about 1.2 µg to 20 µg showed various degrees of injury. Previously suggested tolerance levels had been 10 µg and 1 µg.
>
> [...]noting that we were obliged to make an 'informal judgment' decision, I suggested that we should set the 'tolerance level' for residual radium burden in radium-dial painters at such a level that we would feel perfectly comfortable if our own wife or daughter were the subject. I then asked each committee member individually in turn if he would be contented with 0.1 µg. Unanimously, they all were.

The recommendation of a maximum permissible body burden of 0.1 of a microgramme of radium proved to be remarkably viable and would for a long time form the basis of subsequent limit values for the body burden of other radioactive nuclides that, as with radium, make for the skeleton. The recommendation was the first to apply to the total body burden of a radioactive substance.

The committee also recommended a limit for the concentration of radon in the air at workplaces, 10-11 curie per litre of air (370 becquerel per cubic metre in today's units) as the maximum value, irrespective of time and place. By way of comparison, it is worth mentioning that the Swedish limit value for radon in new buildings is 200 Bq/m^3 as a mean value over a long period and that the ICRP's latest limit value for the exposure of mine workers to radon corresponds to a concentration of approximately 3 000 Bq/m^3 for 2000 hours' work per year. The American Committee got it quite right early on based on rough estimates.

In 1940, the same Committee also observed the risk of hereditary injuries and discussed the idea of lowering the tolerance dose for x-ray and gamma radiation to one fifth of the value from 1934. However, the influential medical physicist *Gioacchino Failla* (1890-1961) had objections. Tolerance doses had previously been estimated on the basis of a safety margin to the radiation doses (*threshold doses*) which had proven necessary to cause what are now called *deterministic* (i.e., unavoidable) injuries such as skin damage. However, hereditary injuries are not deterministic but random (they are therefore now called stochastic). No threshold value for the radiation dose can be counted on for these; even small radiation

[*] It is interesting to note that the American recommendation of a tolerance dose of 0.1 röntgen per day was not originally adopted owing to greater caution than what lay behind the ICRP's double value, but because it was thought that 0.2 give the appearance of a non-existent accuracy in the risk assessment. The greater caution was exercised in1940, however.

doses lead to some, albeit small, risk of injury. There is therefore also no objective tolerance dose. Failla therefore wrote to the committee (quoted from Taylor):

> 0.1 roentgen per day is certainly a safe dose insofar as systemic changes are concerned. If we bring in the genetic criteria then there is no limit at all and 0.02 roentgen per day is just as arbitrary as 0.1 roentgen per day. To be sure, the smaller the dose the less the genetic damage but the possible damage from 0.1 roentgen per day is so slight that one can just as well stop at this point.

Failla continued to explain why he was anxious for the limit value to be lowered:

> In the case of x rays it is a very simple matter to increase the protection, at least in new installations, so as to comply with the new requirement. In the case of radium, however, the new requirement would introduce serious complications in the handling of radium for therapeutic purposes. For these reasons, I am opposed to the [proposed] change in tolerance dose and I hope that something can be done to modify the action of the committee.

When the Advisory Committee met in Cincinnati on 25 September 1941, the decision was to delay any lowering of the tolerance dose. However, bearing in mind that hereditary injuries could occur even at lower radiation doses, the expression ‘tolerance dose’ was thought to be unsuitable and misleading. A decision was made to instead use the term ‘permissible dose’ in the future.

The meeting in Cincinnati was the last meeting for the American Advisory Committee before it split up because the members were given other assignments owing to the war. A final proposal to lower the dose limit was made by *Paul Henshaw* in a circular to the Committee in October 1941. Henshaw proposed a new dose limit of 0.02 röntgen per day (corresponding to approximately 50 mSv per year). He referred to an overview that had recently been published in the *Journal of the National Cancer Institute*. The authors had found that there ought to be no difficulty with working under the more restrictive limit if they really tried. Henshaw also recommended that the Committee ought to make a mental note that hereditary injuries were a new type of radiation injury in that you could not count on a threshold value for the radiation dose. He also pointed out that so little was known about a number of conceivable radiation effects such as changes to the blood, hereditary injuries and cancer (including leukaemia) that the Committee ought to concentrate on the biological aspects of the radiation protection just as much as the physical.

The Committee never got around to considering Henshaw’s proposal. An informal preparatory meeting of some of the members was held in December 1941, a few days before Pearl Harbour. Henshaw’s proposal was then sent to all members on 24 December, but the Committee had already split up by then. The visionary proposal was therefore forgotten owing to the war.

The driving force behind the Advisory Committee, physicist *Lauriston Taylor* (1902 – 2004) from the National Bureau of Standards, the man who had been involved along with Rolf Sievert in forming the Committee in Stockholm in 1928 which would end up being called the ICRP, was busy with military assignments. He was initially occupied by developing proximity fuses, i.e., distance sensing detonators for grenades, and then spent two years in Europe on tactical operations analysis for the Air Force. The ICRP was also split up due to the war - a war which just two of its members survived: Sievert and Taylor.

7. ADMINISTRATORS AND SCIENTISTS

IN SPRING 1941 there was still no actual support for nuclear weapons research from the political and military policy makers in the United States. There certainly was a Uranium Committee headed by NBS director Lyman Briggs, but there were no resources and initiative was lacking. Up until now, it had been the European immigrants such as Fermi, Rabi, Szilard, Teller and Wigner who had taken the actual initiative regarding research that concentrated on an American atomic bomb, but their achievements were limited to shrewd theories and small-scale research. The policy makers showed polite interest but nothing more.

In June 1941 there was a turn-around as a consequence of a number of synergetic factors. Information on the interest of the German military in uranium research had reached the United States. The optimism of the Englishmen regarding an atomic bomb with uranium-235 caught on after Frisch and Peierls had made their spectacular estimate of the critical mass in 1940. The detection of plutonium-239 in March 1941 and the awareness that this nuclide was fissile and could be produced in a uranium pile expanded the possibilities. But it was primarily the contacts and discussions between five American research administrators which led to an increase in research efforts.

So, five Americans took over the initiative from the five immigrants, five Americans who had better connections, greater persuasion capacity, closer contact with politicians and greater empowerment. The five were *Vannevar Bush* (1890-1974), *James Conant* (1893-1978), *Ernest Lawrence* and the brothers Arthur and Karl Compton.

Vannevar Bush, as electronics engineer at Massachusetts Institute of Technology (MIT) had already designed a magnetic submarine detector in 1917 and had then spent a couple of decades developing several ingenious electronic and electromechanical devices. At the end of the 1930s, Bush was Deputy Rector of MIT (where

Karl Compton was Rector) and was given increasing administrative assignments. He left this post in 1939 to become head of the Carnegie Institution of Washington DC[*], the institution that ran the DTM laboratory in the Chevy Chase area outside Washington DC where a substantial amount of important research results on nuclear fission had already been obtained. Bush was therefore at an early stage aware of the uranium research that was being conducted. He also had definite views on the need for cooperation between civil research and military authorities. His submarine detector from 1917 had been effective and many hundreds of them had been made, but it had never been used for military purposes.

James Conant, three years younger than Bush, had been Professor of Organic Chemistry at Harvard University[†] in Cambridge outside Boston. At the start of the 1930s, Conant was considered to be one of the United States' most prominent chemists and had carried out pioneering work with radicals, haemoglobin and chlorophyll. However, like Vannevar Bush, he gradually changed over to research administration and became Rector of Harvard University in 1933.

Arthur Compton would also become a University Rector (for Washington DC University), but not until 1945. In 1941, he was Professor and experimental physicist at the University of Chicago, a post which he held as early as 1923. In 1927 he had won a Nobel Prize in Physics for his discovery of the

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[*] The Carnegie Institution was formed in 1902 by *Andrew Carnegie* (1835-1919), American industrialist and maecenas. Donations from Carnegie made it possible to form the Carnegie Institution in 1911, which rewards people who have risked their own lives to save those of others.

[†] Harvard University was founded in 1636 on the basis of a donation from Priest *John Harvard* in a Boston suburb. Under English influence, this suburb changed its name to Cambridge in 1638. At the end of the 1800s, Harvard was developed into one of the most prominent universities in the USA.

Compton Effect, i.e., that the laws of mechanics can be applied to describe directional changes and energy losses when photons collide with electrons. Compton's discovery was of great significance. Arnold Sommerfeld considered it to be the most important that had been made at the time since it proved that photons existed, which was something that many still held doubts about. It clearly demonstrated the wave-particle duality of electromagnetic radiation.

Karl Compton, Arthur's less well-known brother who was five years older than him, was also Rector of a famous high-level educational establishment, i.e., the Massachusetts Institute of Technology. Like Harvard, the MIT lies in American Cambridge and was founded in 1861.

The five American research administrators.
From the left: Ernest Lawrence, Arthur Compton, Vannevar Bush, James Conant and Karl Compton

Ernest Lawrence had come to the University of Chicago as a doctoral student under Arthur Compton in 1923, the same year as the latter started his professorship. While there, he became a close friend of Compton. In

1930, Lawrence became Professor at the University of California in Berkeley near San Francisco. It was here that he built the first the cyclotron in 1931, something which led him to be awarded the 1939 Nobel Prize in

Physics. In 1936, he became head of the research laboratory, Berkeley Radiation Laboratory, which he had been helping to set up since 193l and which, through access to big accelerators, became a leading nuclear physics laboratory (it is now called the Lawrence Berkeley Laboratory).

ABSORPTION AND SCATTERING GAMMA RADIATION

IF gamma radiation with photons of a certain energy hits a body, the intensity of the radiation (I) will decrease with the depth in the body. One reason is the inverse square law, which says that the intensity decreases in inverse proportion to the square of the distance from a (spot-shaped) radiation source. Another reason is that radiant energy is lost from the primary beam as a consequence of absorption and scattering in the body. If the inverse square law can be ignored (great distance), the primary intensity of the radiation decreases exponentially to the depth (x) in the body:

$$I=I_0e^{-\mu x}$$

The coefficient μ is called the extinction coefficient and covers both the energy that is scattered through secondary photon radiation (μs) and the energy that is absorbed (μa) by being taken over by electrons or by exciting atoms, i.e., $\mu = \mu_s + \mu_a$. The extinction coefficient can also be divided according to the physical processes that take place. If the photon energy of the gamma radiation is less than 1 MeV (one million electron volts), there are two processes that are relevant: the photoelectric effect (designated τ) and the Compton effect (σ). This can also be expressed as $\mu = \tau + \sigma$.

The Compton effect

During the Compton effect (for the discovery of which Arthur Compton won the Nobel Prize in 1927), the energy of the gamma radiation is not absorbed. The radiation is instead scattered in that the electromagnetic wave makes electrons (free or bound to an atom) oscillate whereby the oscillating charge in turn emits radiation. Alternatively, the process can be seen as an impact between a photon and an electron. The electron takes over some of the energy of the photon while the photon is deflected in another direction with reduced energy and thus a lower frequency.

Compton electrons have less energy than photoelectrons. The greater the energy the incident photons have, the more of the scattered radiation is directed forwards. The radiation that is scattered backwards has the lowest photon energy. The Klein-Nishina formula describes the distribution of directions of the scattered radiation.

The photoelectric effect

During the photoelectric effect, the energy of the incident photon, hν, is completely absorbed by an atom (h is Planck's constant and v the frequency of the gamma radiation). The atom immediately disposes of the energy it has collected by throwing out an electron with the kinetic energy E = hν B, where B is the binding energy of the electron. Einstein won the Nobel Prize in Physics in 1921for proving this connection.

When the vacuum after the discarded electron is filled, more energy is released in the form of characteristic x rays. The movement of the discarded (photo)electron is increasingly in a forward direction (i.e., in the direction of the gamma radiation) the greater the energy of the gamma radiation. The characteristic x rays on the other hand is equally likely to go in all directions.

If the absorbent substance has a low atomic number, it is likely that energy will not be given off in the form of characteristic x rays but rather as kinetic energy of an electron, the auger electron, which has low energy and is emitted with the same intensity in all directions.

Pair formation

At photon energies in excess of 1.02 MeV, another absorption process occurs. This means that the photon disappears while an electron pair is formed instead, one positive and one negative electron. The extinctions coefficient for pair formation is usually designated by π.

One way or another, these five men had been involved in defence research activities long before summer 1941. The need for concentrated military research had already been discussed before the war when a new Rector of the American Academy of Sciences would be appointed. It had been said at the time that people must prepare themselves for an impending war and that not only had the new Rector to be a scientist, he must also have the ability to put the research results to practical use. With this in mind, the director of Bell's telephone research laboratories, *Frank B. Jewett* (1879-1949) was chosen as Rector of the Academy.

Once Jewett had been selected for this post, he and Bush, Conant and Karl Compton discussed which research efforts would be needed on behalf of the Defence. Before the war broke out in Europe, it had already been agreed that the nation's research resources ought to be mobilised for this purpose and that the main responsibility lay with the Rector of the Academy of Sciences. So, this was initiative on the part of the scientists of the same type that was taken at almost the same time by Siegbahn and Sievert in Sweden.

After the war had broken out in Europe, the research group decided that Vannevar Bush should take up the matter of military research with President Roosevelt. Bush made this contact the day after Paris had fallen in June 1940.

Bush suggested that the President appoint a new committee to unite the military interests with the Academy of Science's research planning. This took place through the formation of the National Defence Research Council, or NDRC, of which Bush became chairman.

This Council included James Conant and Karl Compton. Conant was responsible for the Department of Chemistry and Explosives. This was the first step towards effective defence research. Briggs' Uranium Committee was brought under the new Research Council which would monitor his work.

At this point, Conant still knew nothing about the possibility of an atomic bomb. At the start of 1941, Bush sent him to England to establish contact between the NDRC and the British government. He ate lunch with Churchill in the shelter beneath number 10 Downing Street and met von Halban in Oxford, and was surprised by all the insinuations about uranium research. At a London club he lunched with F.A. Lindemann who had explained to Churchill one and a half years previously that an atomic bomb was impossible. Now, Lindemann was instead surprised that Conant did not know about the possibility of a bomb. Lindemann said (according to Conant, quoted from Rhodes):

> 'You have left out of consideration the possibility of the construction of a bomb of enormous power.'
>
> I asked: 'How would that be possible?'
>
> 'By first separating uranium-235,' he said, 'and then arranging for the two portions of the element to be brought together suddenly so that the resulting mass would spontaneously undergo a self-sustaining reaction'.

In the United States, Briggs' Uranium Committee was making little progress. Bush then concluded that it was a choice between two decisions. Either the uranium research was of such great importance that much more substantial resources ought to be given to it, or it was so unimportant that it ought not to be allowed to compete with more important activities while the war was on. Bush therefore suggested that Briggs contact Jewett and ask whether the Academy of Sciences ought to establish a committee to look into the importance of uranium research. At an Academy meeting in April 1941, Bush, Briggs and Jewett visited Arthur Compton and convinced him to lead such a committee. Compton became chairman and *William Coolidge* (1873-1975), previously head of the General Electrics research laboratory and designer of the first x-ray tube with a heated cathode, became deputy chairman. The committee also included Ernest Lawrence.

Arthur Compton's committee was quick to produce a report that the Academy of Sciences published on 17 May 1941. The committee had found that none of the members of Briggs' Uranium Committee believed that the splitting of uranium could be of any significance whatsoever to the ongoing war in Europe. The Uranium Committee had concentrated on a possible peaceful use. The work group which had been given the task of examining the possibility of an atomic bomb had scarcely achieved anything.

The work had been considered to be so secret that the members of the group had had virtually no contact with one another.

The report from the Compton Committee had more to say. They believed that the splitting of uranium could become a source of energy to run submarines but that it would take many years. If there were success with a controlled chain reaction, it would be possible to produce quantities of radioactive substances, but this was not expected to be of any significance to the military. The possibility of an atomic bomb was conceivable but so little had been looked into this that the Committee did not think it could recommend any opinion. It would be necessary to separate kilogrammes of uranium-235, and sufficient quantities for a bomb would not be possible before 1945 (!) under any circumstances. The final recommendation was that more ought to be invested in the uranium programme, the proposal being 350 000 dollars over the next six months.

Briggs accepted the Compton Committee's conclusions. However, he thought that the enrichment of uranium-235 was a dubious project and that the first task ought to be to endeavour to get a reactor going with graphite or heavy water as a moderator. The Compton Committee continued to look at the matter for the Academy of Sciences and was boosted by two very competent engineers, one from Westinghouse and one from Bell. At its next meeting, the Committee decided to recommend the creation of a central laboratory to develop a reactor. On the other hand, the Committee could not see how this research could influence the outcome of the war.

These pieces of information and recommendations led to disagreement within Bush's Defence Research Council. Some members thought that uranium research ought to be mothballed for the rest of the war. Others thought that they ought to wait for more information from the physicists on what would be possible. Uncertainty meant that the Defence Research Council made no decision on the proposed central laboratory; on the other hand, it did continue to support the research that was suggested by the Uranium Committee. Uranium research simmered over a low heat.

However, in spring 1941, the consequences of the detection of plutonium-239 in March began to make themselves increasingly evident. Segrè and Seaborg continued their research in Berkeley and in May 1940 determined the fission cross section for slow neutrons when irradiating plutonium-239. On 18 May, they reported that they had found it to be 1.7 times greater than for uranium-235. According to Seaborg (quote from Rhodes), Lawrence was excited about the result:

> We told Lawrence about our definitive demonstration yesterday of the slow neutron fissionability of 94239 and he was quite excited. He immediately phoned the University of Chicago to give the news to Arthur H. Compton. […] Compton made an immediate attempt to phone (unsuccessfully) and then sent a telegram to Vannevar Bush. […] In his telegram Compton indicated that the demonstration […] greatly increases the importance of the fission problem since the available material [i.e., U238 transmuted to plutonium] is thus increased by over 100 times.

On 28 June 1941, a new authority, the Office of Scientific Research and Development (OSRD) was created at the proposal of Vannevar Bush, directly under the President. It would be an umbrella organisation for all research that the government considered to be of importance to the Defence and would also act as a contact organisation for such research in other countries that cooperated with the United States. Bush moved from the less powerful Defence Research Council (NDRC) to become head of the OSRD. He was succeeded as chairman of the NDRC by James Conant.

Briggs' Uranium Committee was incorporated into the NDRC as a special section with Briggs as chairman and George Pegram, Dean of the physics section at Columbia University, as deputy chairman. Conant now got to see some of Compton's reports, but he was not that impressed. Remembering his experiences in London, he was sceptical. He has remembered (according to Rhodes):

> What worried me about Compton's first report, I told Bush, was the assumption that achieving a chain reaction was so important that a large expenditure of both money and manpower was justified. To me, the defense of the free world was in such a dangerous state that only efforts which were likely to yield results within a matter of months or, at most, a year or two were worthy of serious consideration. In that summer of 1941, with

> recollections of what I had seen and heard in England fresh in my mind, I was impatient with the arguments of some of the physicists associated with the Uranium Committee whom I met from time to time. They talked in excited tones about the discovery of a new world in which power from a uranium reactor would revolutionize our industrialized society. These fancies left me cold. I suggested that until Nazi Germany was defeated all our energies should be concentrated on one immediate objective.

However, a number of events during the summer and autumn of 1941 would lead Conant to change his opinion. In his excitement about the possibility of using plutonium-2 39 instead of uranium-235 for a bomb, Lawrence wrote to the Academy of Science's committee about the matter on 1 July (quoted from Compton):

> An extremely important new possibility has been opened for the exploitation of the chain reaction with unseparated isotopes of uranium.
>
> Experiments in the Radiation Laboratory of the University of California have indicated (a) that element 94 [i.e., plutonium] is formed as a result of neutron capture by uranium-238 followed by two successive emissions of electrons (beta transformations), and furthermore (b) that this trans-uranic element is fissile using slow neutrons and therefore presumably behaves like uranium-235.
>
> If this is so, the following three outstanding important possibilities are opened:

1. Uranium-238 would be available for energy production, thus increasing about one hundred-fold the total atomic energy obtainable from a given quantity of uranium.
2. Using element 94, one may envisage preparation of small chain reaction units for power purposes weighing perhaps 100 pounds [approximately 45 kg] instead of a hundred tonnes as probably would be necessary for units using natural uranium.
3. If large amounts of element 94 were available, it is likely that a chain reaction with fast neutrons would be produced. In such a reaction, the energy would be released at an explosive rate which might be described as a 'superbomb'.

When Compton read Lawrence's letter, he realised that the discovery of plutonium would make a uranium pile important to the military; here was an alternative to the enrichment of uranium-235. It was also clear that there was a third alternative. The irradiation of ordinary thorium (thorium-232) with neutrons would also lead to the capture of neutrons followed by two successive beta disintegrations. The end product would be uranium-233 in this case rather than plutonium-239, but its odd mass number indicated that the nuclide would be just as fissile as uranium-235. The Academy of Science's committee had received new information to ponder over.

That same summer, a seemingly insignificant talk was held in New York which would become important in the longer term. It was Edward Teller and Enrico Fermi who were on their way back to the Pupin laboratories from lunch at the University Club and talking about this and that. Suddenly, Fermi began to speculate about whether an atomic bomb could heat up a mass of deuterium enough to set off a thermonuclear reaction, i.e., fusion. Such a procedure would transform the deuterium into helium and facilitate a bomb with a thousand times greater blast effect than that of a fission, and at a much lower cost. A hydrogen bomb.

Fermi often proposed ideas and whims to immediately forget them again, but Teller could not forget the thought that Fermi had aroused - and one day he would realise it.

In England, the Maud Committee had finished its final report on 23 June 1941, the day after Hitler had begun his attack on the (former) Soviet Union ('Operation Barbarossa'). The report was approved by the Committee on 15 July, whereupon the Committee was dissolved. As opposed to the American Academy of Science's two reports, the Maud Report was full of concrete recommendations. It started with the following words (quoted from Gowing's presentation of the report):

> Work to investigate the possibilities of utilizing the atomic energy of uranium for military purposes has been in progress since 1939, and a stage has now been reached when it seems desirable to report progress.

> We should like to emphasise at the beginning of this report that we entered the project with more skepticism than belief, though we felt it was a matter which had to be investigated. As we proceeded we became more and more convinced that release of atomic energy on a large scale is possible and that conditions can be chosen which would make it a very powerful weapon of war. We have now reached the conclusion that it will be possible to make an effective uranium bomb which, containing some 25 lb [approximately 11 kg] of active material, would be equivalent as regards destructive effect to 1,800 tons of T.N.T. and would also release large quantities of radioactive substances, which would make places near to where the bomb exploded dangerous to human life for a long period. The bomb would be composed of an active constituent (referred to in what follows as ^{235}U) present to the extent of about 1 part in 140 in ordinary Uranium. Owing to the very small difference in properties (other than explosives) between this substance and the rest of the Uranium, its extraction is a matter of great difficulty and a plant to produce 2 ¼ lb (1 kg) per day (or 3 bombs per month) is estimated to cost approximately £5,000,000, of which sum a considerable proportion would be spent on engineering, requiring labour of the same highly skilled character as is needed for making turbines.
>
> In spite of this very large expenditure we consider that the destructive effect, both material and moral, is so great that every effort should be made to produce bombs of this kind. As regards the time required, Imperial Chemical Industries* after consultation with Dr Guy of Metropolitan-Vickers, estimate that the material for the first bomb could be ready by the end of 1943. This of course assumes that no major difficulty of an entirely unforeseen character arises. Dr Ferguson of Woolwich estimates that the time required to work out the method of producing high velocities required for fusing […] is 1 – 2 months. As this could be done concurrently with the production of the material no further delay is to be anticipated on this score. Even if the war should end before the bombs are ready the effort would not be wasted, except in the unlikely event of complete disarmament, since no nation would care to risk being caught without a weapon of such decisive possibilities.

The Maud Committee's report contained a number of detailed accounts of different parts of the problem and was full of technical information. The Committee's conclusions and recommendations were:

(i) The Committee considers that the scheme for a uranium bomb is practicable and likely to lead to decisive results in the war.

(ii) It recommends that this work be continued on the highest priority and on the increasing scale necessary to obtain the weapon in the shortest possible time.

(iii) That the present collaboration with America should be continued and extended especially in the region of experimental work.

The way in which the Maud Report would be received would crucially depend on the way in which the British government's two dominant scientific advisers perceived it. Sir Henry Tizard was sceptical and doubted that a bomb could be produced before the war. 'The probability has increased but is still very small,' he said.

On the other hand, F.A. Lindemann, now Lord Cherwell, was positive. And Lindemann had the ear of Churchill, and Earl *Frederik Birkenhead* (1872-1930) had already considered him to have 'greater power than any scientist in history'. Unfortunately, C.P. Snow considered his scientific opinion to be 'unusually poor'. In any case, Lindemann's view on the Maud Report had unexpected consequences. It influenced Churchill and the British government, and this effect was in turn crucial to the American decisions on the atomic bomb.

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* Now the parent company of the world-leading British chemical company I.C.I., which was formed through the merger of four British chemical companies in 1926.

Lindemann undertook to summarise the Maud Report in a memorandum to Churchill. Churchill did not usually want to read reports of more than half a page, but on this occasion Lindemann wrote two and a half and recommended that the Prime Minister invest in the atomic bomb. Churchill read Lindemann's memorandum on 27 August 1941. He then summarised (according to Rhodes) his view to his military advisers:

> Although personally I am quite content with the existing explosives, I feel we must not stand in the path of improvement, and I therefore think that action should be taken in the sense proposed by Lord Cherwell.

On 3 September, the British general staff agreed. The British government's attitude would be of crucial importance to the attitude of the Americans. And so would the visit to the United States of Professor Mark Oliphant during early autumn 1941. His primary assignment was to discuss radar matters with colleagues within the Defence Research Council (NDRC). He was also asked to look into why the Americans had not commented on the Maud Committee's result. Minutes and a draft of the report had secretly been sent to Lyman Briggs to keep the Uranium Committee informed, but the Committee had not been in touch.

Oliphant visited Briggs in Washington DC and found to his dismay that Briggs had put the reports in his document cabinet and never told his committee members about them. Oliphant then asked to be able to participate in the next Uranium Committee meeting. A recently added member at the time, Arthur Compton's colleague *Samuel Allison* (1900-1965), has said the following about his visit to the committee (according to Rhodes):

> [Oliphant] came to a meeting and said 'bomb' in no uncertain terms. He told us we must concentrate every effort on the bomb and said we had no right to work on power plants or anything but the bomb. The bomb would cost twenty-five million dollars, he said, and Britain didn't have the money or the manpower, so it was up to us.

Allison, who had thought that the Uranium Committee's task was to find a power source for submarines, was surprised. Briggs had failed to keep his committee informed.

Oliphant was also surprised about the lack of knowledge on the part of the Americans. He traversed the United States like a whirlwind. In Berkeley, he met Ernest Lawrence and gave an account of the Maud Report, which Lawrence had not seen. In Washington DC, Oliphant invited Conant to lunch, but in New York,

Bush had no more than twenty minutes to spare for talks. Neither Bush nor Conant disclosed any knowledge of the uranium research; they considered it to be too sensitive a matter to discuss freely with a British Professor who had not been officially asked to contact them.

Oliphant also visited Fermi who was still not satisfied with the Bohr-Wheeler theory for nuclear fission. In Schenectady, Oliphant met William Coolidge who reacted strongly to the information that pure uranium-235 could be split using fast neutrons and that this made a very small bomb possible. This information was new to Coolidge, despite the fact that the information was already available in the United States, most of it unfortunately in Briggs' document cabinet. In his capacity as deputy chairman of the Academy of Sciences' committee, Coolidge wrote immediately to Jewett and said that action must be taken on the basis of Oliphant's information.

It did not take long for Lawrence to ring Arthur Compton from Berkeley. Lawrence said that, after having spoken to Oliphant, he now thought it was definitely possible to make an atomic bomb and that such a bomb could influence the outcome of the war, which no-one believed that United States would be able to stay out of. Lawrence wanted to meet Compton to discuss the matter.

An opportunity for this came as early as the following week. The University of Chicago was celebrating its 50th anniversary. Lawrence was invited to accept an honorary doctorate. It just so happened that James Conant had been afforded the same honour. Arthur Compton invited Conant and Lawrence to his home to discuss the atomic bomb issue. Both Compton and Lawrence were well aware of Conant's key role as chairman of the Defence Research Council. Compton says in his memoirs that the meeting between the three constituted the actual starting point of the American nuclear weapons programme. He has described what happened:

> It was a cool September evening. My wife greeted Conant and Lawrence as they came into our home and gave us each a cup of coffee as we gathered around the fireplace. Then she busied herself upstairs so the three of us might talk freely.
>
> Lawrence began by telling briefly of new results from England and how this work was confirmed by experiments in the United States. This work had convinced him of the feasibility of making an atomic bomb using only a few kilograms of fissionable material. He described recent Berkeley experiments, indicating that the bomb could be made equally well either with uranium-235 or with a new chemical element discovered in his laboratory which was soon afterwards named 'plutonium'. [...] Finally he pressed the point that we knew the Nazis were working with a large group of able men on the problem of separating the two forms of uranium and had already made substantial progress. There was every reason to believe that the Nazis saw in the atomic bomb the possibility of a new weapon of decisive importance. If they succeeded first they would have in their hands the control of the world.
>
> Conant was reluctant. As a result of the reports so far received he had concluded that the time had come to drop the support of nuclear research as a subject for wartime study. Every one of our competent research men was needed for work on problems that would bring useful results during the period of the war. We could not afford to spend either our scientific or our industrial effort on an atomic program of highly questionable military value when every ounce of our strength was needed for the nation's defense.
>
> I rallied to Lawrence's support. I confirmed his interpretation of the new scientific results as indicating the practical feasibility of the atomic bomb. I described the rough estimates that had been made of the destructive power of such a weapon. I told of the evidence that the Nazis were making a major effort of the atomic program. They would not do so in the midst of war unless they believed they might succeed. We just could not afford to let the Nazis beat us to the making of atomic weapons. This would be inviting disaster.

The argument began to convince Conant. It was a complete turnaround at the last minute when he had almost decided to mothball the uranium research for the rest of the war. But he realised that half measures simply would not do. It was either stop or full steam ahead. According to Compton:

> 'If this task is as important as you men say,' he remarked, 'we must get going. I have argued with Vannevar Bush that the uranium project be put in wraps for the war period. Now you put before me plans for making a definite, highly effective weapon. If such a weapon can be made, we must make it first. We can't afford not to. But I'm here to tell you, nothing significant will happen on such a job as this unless we get into it with everything we've got.'
>
> He turned to Lawrence.
>
> 'Ernest, you say you are convinced of the importance of these fission bombs. Are you ready to devote the next several years of your life to getting them made?'
>
> Conant had put his finger on the crucial point. The question brought up Lawrence with a start. I can still recall the expression in his eyes as he sat there with his mouth half open. Here was a serious personal decision. Lawrence was at this time working on many things, mostly war projects related to electronics, but also directing a major program of basic science research that was precious to him. He hesitated only a moment: 'If you tell me this is my job, I'll do it.'
>
> Conant turned to me. 'Arthur,' he said, 'it's up to you to call your committee together, look over the evidence as carefully as you can, and get a report on atomic bombs into Bush's hands as quickly as possible. If this matter is as critically important as you men indicate, we mustn't lose a day.'

Conant had now left the door that he had been on the way towards locking ajar, but this did not mean that he was convinced just yet. Having returned to Washington DC after his visit to Chicago, he reported the talk with Lawrence and Compton to Vannevar Bush. He emphasised that he was still sceptical by saying that he 'had been exposed to an involuntary conference'. Bush and Conant thought there was still justification for demanding a third report from the Academy of Sciences and proposing that the Academy's Committee be enlarged by a further two experts. One was *W.K. Lewis*, an industrial chemist who was known for his skill in assessing the possibility of going from laboratory experiment to industrial scale. The other was *George Kistiakowsky* (1900-1982), a Ukrainian who became Professor of Chemistry at Harvard when he emigrated from Berlin where he had defended his thesis at the university in 1925. Kistiakowsky was the NDRC's explosives expert.

Bush was more positive than Conant and had been influenced by Lawrence's and Compton's arguments through the report. But what really convinced Bush was the Maud Report which Conant had officially sent from Professor G.P. Thomson on 3 October, who was now the British contact person in Ottawa.

After having read the Maud Report and without waiting for the third report from the Academy of Science's committee, Bush informed President Roosevelt of this development on Thursday 9 October 1941. There were now no vague warnings like when Sachs had told Roosevelt about the possibility of an atomic bomb almost exactly two years previously. Bush told the President and Deputy President *Henry Wallace* (1888-1965) about the Maud Report and the Englishmen's optimism, that as little as just over ten kilogrammes could be enough for a bomb that could explode with an effect corresponding to a couple of thousand tonnes of TNT, and that the raw material was available in Canada and the Belgian Congo. Bush said that the English scientists who produced the Maud Report had also made recommendations regarding its use right up to the government but that the American scientists ought to stick to the technical matters. It was enough to have him and Conant proposing policies.

Bush may have warned the President of over-eager scientists and the President took the warning *ad notam*. He decided that the policy matters regarding the bomb and the continued programme should be discussed only by a special advisory group, which he determined would consist of Deputy President Wallace, Secretary of War *Henry Stimson* (1867-1950), Army Commander *George Marshall* (1880-1959) and Bush and Conant. The group ended up being called the Top Policy Group.

Bush wasted no time telling the scientists where they stood. Men like Szilard and Lawrence had been problematic in that they had put forward certain views on an atomic bomb policy from time to time. Bush wrote to Frank Jewett and said that he had emphasised to Arthur Compton and his people that they were being asked to report technical matters and that they were not being asked to consider general policy. He continued: 'I think [Lawrence] now understands this, and I am sure that Arthur Compton does, and I think our difficulties in this regard are over.'

Vannevar Bush now had unrestricted power over the atomic bomb research and was responsible only to the President. The scientists had to go along with this and no longer had any say in the policy matters. But Bush was realistic and understood the limitation of the system. He was not in charge of an organisation that could handle a project that ought to grow quickly. He therefore soon advised Roosevelt to find a larger organisation than the OSRD to handle the bomb project.

In reality, Roosevelt had now decided to continue with the bomb project while observing the strictest secrecy and without informing the congress. As Rhodes writes: 'It seemed to be a military decision and he was Commander-in-Chief'. The President told Bush that funds for the project must be taken from special sources intended for special purposes. The impending substantial financial investments were not reflected in any official budget.

The formal situation in October 1941 was that Bush had the political power in his dual role as chairman of the Office of Scientific Research and Development (OSRD) and member of the President's Top Policy Group. The Uranium Committee with Briggs as chairman had the executive function by controlling research at the request of the Defence Research Council (NDRC) of which Conant, also a member of Roosevelt's Policy Group, was chairman. The scientific investigation of the physical conditions for an atomic bomb was prepared by the Academy of Sciences through the committee of which Arthur Compton was chairman.

Roosevelt's outlook increased the pressure on the Academy of Sciences, and the energetic Compton's October days were busy. He visited Fermi at Columbia University in New York and asked him to estimate the critical mass of uranium-235 for a bomb. Fermi stood in front of the blackboard and calculated. He knew all of the formulae and had the latest experimental values of different constants in his head. The answer, according to Compton, was that the critical mass would scarcely be greater than approximately 50 kg. However, when just afterwards Fermi was asked by other colleagues to do the same calculation on paper, he arrived at a probable value of 130 kg and an uncertainty interval of between 20 kg and several tonnes.

In New York, Compton also visited Harold Urey, John Dunning and Eugene Booth to discuss separation methods for uranium-235. He then continued to Princeton to meet Eugene Wigner, who gave him a lecture on the difference between fast and slow neutrons during nuclear fission. Lawrence had said that the cross section of plutonium for fast neutrons was ten times greater than the corresponding cross section for uranium-238, which was crucial regarding the possibility of a bomb. A few months previously, Briggs had been able to look at the information but had not been particularly interested - his interest was aimed at the peaceful production of energy in a reactor and concerned slow neutrons.

Back in Chicago, Compton called Seaborg to hear about the possibility of separating plutonium from fuel irradiated with neutrons in a reactor. Seaborg thought that it would go well and that it would not be difficult to work out remote-operated technology for that purpose.

On 21 October 1941, Arthur Compton summoned his Academy Committee to a meeting to discuss the draft of a report. Lawrence had insisted on taking the young physicist *Robert Oppenheimer* (1904-1967) with him, something which Conant had previously objected to. Many, including Lawrence, were uncertain about Oppenheimer's loyalty; he had said many times that it lay primarily with 'the underdog', i.e., the outcast of society. However, after having participated in the Compton Committee meeting, Oppenheimer wrote to Lawrence (according to Rhodes):

> I […] assure you that there will be no further difficulties at any time with the A.A.S.W.* […] I doubt very much whether anyone will want to start at this time an organization which could in any way embarrass, divide or interfere with the work we have in hand. I have not yet spoken to everyone involved, but all those to whom I have spoken agree with us: so you can forget it.

Several estimates of the critical mass of uranium-235 were given at the meeting. Oppenheimer contributed the value of 100 kg, not far off Fermi's 130 kg. Kistiakowsky emphasised the benefit of being able to create terrible destruction with the help of just one aircraft - he saw it as a great economic gain.

However, when it came to estimates of production time and costs, the committee members disagreed. The engineers from Westinghouse, General Electric and Bell Telephone who had been added to the Committee following pressure from Bush to make its recommendations more realistic, refused to back any estimate. They thought the basis material was too uncertain.

This infuriated Lawrence. The following day, he wrote to Compton:

> In our meeting yesterday, there was a tendency to emphasize the uncertainties, and accordingly the possibility that uranium will not be a factor in the war. This to my mind, is very dangerous. […]
>
> It will not be a calamity if, when we get the answers to the uranium problem, they turn out negative from the military point of view, but if the answers are fantastically positive and we fail to get them first, the results for our country may well be tragic disaster. I feel strongly, therefore, that anyone who hesitates on a vigorous, all-out effort on uranium assumes a grave responsibility.

Compton had made estimates himself and, in the draft he sent to Bush and Jewett for information before the weekend of 1 November, he proposed a time of between three and four years and a total cost,

* A.A.S.W. stands for American Association of Scientific Workers, an organisation of which Arthur Compton was also a member. Like many American trade unions, it was often viewed as suspect left-orientated. Lawrence was one of those who mistrusted political involvement.

on condition that only the alternative with uranium-235 were applied, of a few hundred million dollars. He was later proud of these estimates since the time until the first bomb was three years and eight months and the total cost of the whole atomic bomb project 1500 million dollars (but then both uranium-235 with more than one method and plutonium were included). Bush advised Compton to be careful not to scare the government with his cost estimate.

The new member of the Compton Committee, Kistiakowsky, was the one who finally convinced Conant that the bomb project was realistic and necessary.

Conant had mistrusted physicists, but Kistiakowsky was a chemist.

The most difficult part of Compton's assignment was to calculate the explosive power of the bomb. Compton himself made calculations, helped by Kistiakowsky, and the result tallied with the calculations simultaneously performed by Oppenheimer's group in Berkeley.

On 6 November 1941, Arthur Compton personally submitted the Academy Committee's third report to Vannevar Bush and spent an hour discussing its content. The report was limited to apply to a bomb with uranium-235. It therefore did not mention Fermi's research into a graphite-moderated reactor and nor the possibility of using plutonium as bomb material. It could be seen as a confirmation of the British Maud Committee's conclusions.

Three weeks passed before Bush was able to report to President Roosevelt regarding the Compton Committee's report. He gave the following summary at the time:

> The present report estimates that the bombs will be somewhat less effective than the British computations showed, although still exceedingly powerful. It predicts a longer interval before production could be started. It also estimates total costs much higher than the British figures.

Roosevelt, who had in reality already made a decision, reacted positively and Bush received support for stronger action. The first step was the re-organisation of the Uranium Committee and less important tasks for Briggs. The Committee was separated from the Defence Research Council and placed as a separate section, called the 'S-1' (section l), within the OSRD. It later became an effective committee, the 'S-1 Committee', with Conant as chairman.

The new organisation was announced by Bush and Conant on 6 December when the members of the old Uranium Committee were called to Washington DC for information. Bush and Conant had agreed on assignments for some of the scientists. Harald Urey would test gas diffusion at Columbia University. Lawrence would develop electromagnetic separation in Berkeley. A young research engineer at Standard Oil in New Jersey, *Eger Murphree* (1898-1962), would generally monitor engineering questions, but especially see to centrifugal methods. Compton would be responsible for theoretical studies and designing the actual bomb. It was all about uranium-235.

Compton was not pleased. He thought it was a mistake not to also include the alternative with plutonium, which he and Lawrence strongly recommended. There were several reasons for the Committee's doubts. With limited resources, it was reasonable to concentrate the efforts on one of the alternative possibilities (later, it would be said that all possible routes ought to be explored for the sake of safety). Bush thought that Lawrence talked too much. Conant, as a chemist, thought that too little was known about the properties of plutonium and that the uranium path was safer.

After the meeting on 6 December, Compton accompanied Bush and Conant to eat lunch at the Cosmos Club, the prestigious club for prominent academics. The Cosmos Club would also play a certain role as a meeting place for the ICRP (the International Commission on Radiological Protection) after the war.

Compton now saw an opportunity to speak up for his cause. Would the advantage of chemical separation over isotope separation not counterbalance the uncertainty of the possibility of getting a reactor going? Bush retorted that there were incalculable risks with attempting to start a process that had not yet been tested (i.e., a nuclear reactor) on an industrial scale. Conant added that even if it were possible to produce plutonium, nothing was known of its chemistry. It would take years to get a chemical extraction process to work.

'Seaborg tells me that within six months from the time the plutonium is formed [in the reactor] he can have it available for use in the bomb,' was Compton's comment.

'Glenn Seaborg is a very competent young chemist, but he isn't that good,' said Conant.

In actual fact, says Compton in his memoirs, it took just two months from when the irradiated fuel was removed from the production reactors until metallic plutonium was available for a bomb.

In spite of everything, Compton finally succeeded in convincing Bush and Conant to give him the task of looking more closely at the plutonium alternative. It was a case of hindsight without which a reactor may very well never have been developed within the atomic bomb project, writes Compton, and continues:

> This was Saturday. I went from the luncheon table to my room and wrote out a preliminary plan of attack and an initial operating budget. Though devising the method for building the bomb was my most important assignment, it did not seem to call for first attention. If plutonium was to be made, we needed to get into action at once. Plans for the bomb could be postponed until we were further along toward producing the material from which the bomb could be made. The same afternoon I called on Conant and Briggs in their offices, outlining briefly how I intended to proceed, and got their approval of my initial budget. This amounted, as I recall it, to some $300,000 for the first half year. This figure seemed big to me, accustomed as I was to work on research that needed not more than a few thousand dollars per year. But the estimates were realistic in terms of the job, and I was reassured when I was authorized to go ahead with my plans.

Compton spent the rest of the afternoon on the telephone. He thought the right man to produce a nuclear reactor was Enrico Fermi, who was already getting on with the task at Columbia University. Compton decided to travel to New York the next day, Sunday 7 December. He took the train to New York. When on the way it stopped in Wilmington, the old Swedish town in Delaware, and new passengers came on board, THE NEWS was spreading.

The warning had found its way there. On 27 November, the army's headquarters on Hawaii had received a coded message from the army commander, General Marshall. The wording was (according to Rhodes):

> Negotiations with Japan appear to be terminated to all practical purposes with only the barest possibility that the Japanese Government might come back and offer to continue. Japanese future action unpredictable but hostile action possible at any moment. If hostilities cannot, repeat cannot be avoided the United States desires that Japan commit the first overt act. […] Measures should be carried out so as not, repeat not, to alarm civil population or disclose intent.

The head of the American Pacific Fleet, whose base was Pearl Harbor in Honolulu, received an even clearer warning from Washington DC (also according to Rhodes):

> This dispatch is to be considered a war warning. Negotiations with Japan looking toward stabilization of conditions in the Pacific have ceased and an aggressive move by Japan is expected within the next few days.

And yet still early radar warnings of approaching aircraft were not taken seriously. 43 fighters, 49 bombers, 51 dive bombers and 40 torpedo bombers approached from Japanese aircraft carriers 300 km north of Honolulu and took the American Pacific Fleet in Pearl Harbor completely by surprise in two waves of attack. According to Rhodes, they sank 5 out of 8 battleships, 3 cruisers, 3 destroyers and 4 other vessels, destroyed 292 aircraft that had never been up in the air, 117 of which were bombers, and killed 2 403 Americans and wounded 1 178. The aim had been to destroy the whole of the American Pacific Fleet, but this was not fully achieved. To the disappointment of the Japanese, there were no aircraft carriers in Pearl Harbor.

In the afternoon of the next day, 8 December 1941, President Roosevelt addressed both chambers of the Congress and formulated a declaration of war not just against Japan but also against Germany and

Italy. The United States was ‘properly’ in the war and the atomic bomb project was felt to be important to those involved.

8. THE ITALIAN NAVIGATOR

THE ATTACK ON Pearl Harbour united the nation, and Compton had no difficulty engaging scientists for the plutonium programme when he was able to maintain that it could settle the war. At this time, the planned bomb was seen as just another weapon in the military's arsenal, certainly with an unsurpassed capacity to destruct but still just another weapon. It was obvious that weapons killed people and the capacity of the new weapon to destruct made it no more repugnant to the user than other weapons, just more effective. As Kistiakowsky had said: it would now be possible to use one aircraft to cause the same destruction that would otherwise have required hundreds. We should remember that the atomic bombs that were planned in 1941 had no more than one thousandth of the destruction capacity of the real terror weapon, the superbombs of the 1960s. It was not the actual weapon that was terrible - the terrible thing was the war that others had started, and which now required the defence of the nation.

Arthur Compton had no doubts. He was in many ways a typical American of the time: deeply religious, serious, energetic and full of pride in his country. It was said that ‘Arthur Compton and God were daily companions’. His father was a preacher and Professor of Philosophy and his mother devoted herself to missionary activities and had been American Mother of the year in 1939. However, in spite of his career as a prominent physicist, experimenter, theorist and Nobel Prize winner, there was a strong element of naivety about Arthur. He shocked Bohr by saying that Heisenberg's uncertainty principle could be extended to include people's actions and confirmed the essence of free will but, like Einstein, he thought that the uncertainty principle did not apply to God. Too primitive a philosophy, thought Bohr. In physics, we do not talk about God - we talk about what we can know.

On 20 December 1941, Conant summoned the S-1 section's experts to a meeting in Washington DC to discuss the different possibilities that were available. It was now a question of ‘four horses in a race’, as Compton put it. He then proposed the following four possibilities:

1. Making the bomb of plutonium; or making the bomb of uranium-235 produced using:
2. Electromagnetic separation or
3. Gas diffusion or
4. Centrifuging.

There was one further possibility, i.e., separating uranium-235 from uranium-238 through thermal diffusion, but this was thought to be less promising than the others.

Bush and Conant thought that making the bomb from plutonium as advocated by Lawrence and Compton was the least secure alternative. It meant that a number of completely new problems had to be solved. First, a nuclear reactor (at the time called a uranium pile) had to be built so that a chain reaction could occur. This chain reaction must be controllable so that it did not degenerate into an explosion. Then the theory that nuclear reactions in the reactor led to the formation of plutonium had to be confirmed. Then the formed plutonium had to be chemically separated from the uranium in which it was formed. It also had to be possible to handle and mechanically process the plutonium without any risk. Finally, the actual bomb had to be designed so that the plutonium would explode with the maximum blast effect. The final problem was common to all of the alternatives, however, and also concerned uranium-235.

The plutonium alternative involved a number of completely new problems. The alternatives with uranium-235 also involved major problems, but they were basically not new; they instead concerned technical applications on a large scale of something that was largely already known. The plutonium alternative was a gamble whereas the uranium alternatives were physically secure but very expensive. It might have been reasonable to think that all resources should have been put into one of the alternatives rather than being divided among four, but backing the wrong horse was a risk they did not want to take.

When it came to the separation (or rather the enrichment) of uranium-235, the two main alternatives were electromagnetic separation and a diffusion method. The principle for the electromagnetic separation was well-known and had been applied by Aston in his mass spectrograph. This was the method with which Arthur Dempster had proven the existence of uranium-235 in 1935. Alfred Nier had also used a mass spectrograph when he succeeded in separating sufficient uranium-235 in 1939 to be able to show that Bohr had been right when he said it was *that* isotope that was fissile rather than uranium-238. But the quantities that could be separated in a mass spectrograph were microscopic. Where producing tens of kilogrammes was concerned, the same principle had to be applied on an industrial scale, which involved great expense. Lawrence's group in Berkeley believed that this would be possible within a couple of years, however.

There were several diffusion methods. The one that was usually called gas diffusion involved pressing a uranium compound in gaseous form through a small-pore filter through which the uranium-235 compound could pass faster than the uranium-238 compound. The problem with this method was primarily that the only gaseous uranium compound that could be used was uranium hexafluoride, which is a very corrosive substance that is difficult to handle. It was therefore difficult to find suitable filter materials and use them to produce filters with small enough pores. However, in November 1941, Harold Urey's physicist colleagues John Dunning and Eugene Booth at Columbia University in New York had reported optimism regarding the separation of measurable quantities of uranium-235 using the gas diffusion method (they had definitely succeeded in January 1942).

The other diffusion method was based on the method worked out in Germany by Klaus Clusius and Gerhard Dickel in 1938 (see Chapter 3). Research was already being done into this by Ross Gunn and Philip Abelson at the Naval Research Laboratory in San Francisco, but the Navy was being kept out of it as expressly ordered by President Roosevelt. This 'horse' was thus an outsider in the race and competed outside the organisation of the S-1 section.

Centrifuging was definitely a conceivable method of separating the lighter isotope uranium-235 from the heavier uranium-238. The centrifugal method had been studied by Professor *Jesse Beams* at the University of Virginia, but the experiment had not been going for as long as the ones for the other methods.

On 16 December 1941, just before the S-1 section's experts agreed to back 'four horses', President Roosevelt's Top Policy Group decided on the advice of Vannevar Bush to give the army's engineering troops the task of building the necessary production plants to produce plutonium and uranium-235 in sufficient quantities for atomic bombs. The task fell to *Major General Wilhelm Styer* (1893-1975) who was responsible for the army's material supplies. Bush had begun to see that the atomic bomb project required far greater resources than those available to his OSRD. Groves writes (1962):

> The entry of the United States into World War II caused the abandonment of all projects aimed at developing atomic energy as a source of power and gave added impetus to the efforts to build an atomic bomb. At the same time, Bush and a number of others in policy-making positions began to realize that vital as continued laboratory investigations were, even more pressing problems were developing in the fields of engineering and construction. Although they had created a planning board to cope with problems of this nature, it was fast becoming apparent that a much more powerful organization would be required. It is to their everlasting credit that Bush and his colleagues had the discernment to recognize the limitations of their own organization as well as the moral fortitude to admit them in the national interest. Very few men, confronted with a similar situation, would have done so.

On 17 June 1942, Vannevar Bush reported to the President and said that an atomic bomb was possible and that it could be produced in time to influence the outcome of the war. This was the final crucial stage.

At this time, the decision-makers in the USA and Germany had largely the same decision basis on which to decide whether it was worth investing in an atomic bomb. In Germany, General Schumann assumed that the war would be short and that the atomic bomb would not be produced in time to affect its outcome. He therefore thought it irresponsible to bind substantial resources for this purpose. In the United States, Vannevar Bush made the opposite assessment.

The question as to whether or not the German physicists headed by Heisenberg really did want to have an atomic bomb is therefore academic and moral. It seems as though nothing they could have said would have been able to influence Schumann's decision if they had no burning desire for a bomb such that they would have succeeded in convincing Hitler himself. Which was something they never tried.

The four methods in which the S-1 section decided to invest on 20 December 1941 - largely confirmation of what had been agreed on at the first meeting on 6 December - were:

1. Electromagnetic separation (*Ernest Lawrence* in charge);
2. Gas diffusion (*Harold Urey*);
3. Centrifuging (*Eger Murphree*), and
4. The plutonium alternative (*Arthur Compton*)

The first three projects were already underway; the plutonium project was new. Fermi had certainly spent a long time performing experiments for the purpose of achieving a chain reaction in a reactor, but the justification had now changed from being fundamental physical research aiming to produce energy to being the production of plutonium for a bomb. Much greater resources would be made available in this connection.

Compton's first important decision concerned the site of an initial trial reactor. There were many conceivable places: Columbia University, Princeton, Pittsburgh (Phillips Petroleum), Cleveland and Berkeley. Reactor research was ongoing at Columbia University and in Princeton. Pegram had spoken warmly in favour of Columbia University. Compton would still have preferred the plutonium project to be at the University of Chicago where he himself worked. He had secured support from the Chancellor of the university and was free to move the research there.

When Compton was to make the decision at the start of 1942, he was confined to bed with influenza and fever. A number of advisers had congregated in his bedroom. There was Leo Szilard from Columbia University, Ernest Lawrence from Berkeley, Luis Alvarez from MIT in Cambridge, *Richard Doan* from Phillips Petroleum's research laboratory and *Norman Hilberry* from New York University. All promoted their own places and only Doan supported Compton when the latter promoted Chicago. The exhausted Compton finally made his decision: Chicago. The university there could offer just as good premises as any other, Compton had better control over the situation there, Chicago's location in the middle of the country made it less sensitive to enemy attacks... Compton had many good arguments.

However, Ernst Lawrence was not pleased and objected: 'You'll never get the chain reaction going here. The whole tempo of the University of Chicago is too slow.'

Compton made a bet with Lawrence that a chain reaction would be achieved before the end of the year. They bet a 5-cent cigar on it. In his memoirs from 1956, Compton writes: 'I won the bet, but I haven't yet received the cigar. Maybe the five-cent variety is no longer made'.

Immediately after the decision, when everyone except for Doan had left, Compton rang Fermi and told him what had happened. Fermi immediately agreed to move from Columbia University to Chicago, and Compton had already previously got his boss, Professor Pegram, to agree to the move if, as Pegram would have preferred, the decision was not to keep the research in New York. Fermi's colleague Herbert Anderson immediately came to Chicago to prepare for the move so that the research could continue with the least possible interruption. Compton arranged corresponding preparations with Eugene Wigner in Princeton.

At the University of Chicago, physicists and mathematicians co-existed in one of the most appropriate buildings, Eckhart Hall. To Compton's delight, the mathematicians unanimously agreed to move to lesser premises to prepare room for the now secret plutonium research. In February 1942, the latter was given the code name of 'Metallurgical Project' and the laboratory in Chicago was called the 'Metallurgical Laboratory' (Met. Lab.). The name aroused no interest since the university had long entertained plans to establish a metallography laboratory, and indeed did so after the war. Compton appointed Richard Doan as head of the new laboratory on the basis of the latter's experience of administering industrial research, while Compton himself was head of the actual plutonium project for which the laboratory was working.

Many prominent scientists came to work at the Met. Lab. while this work was progressing. Principally Enrico Fermi but also Eugene Wigner, John Wheeler, Samuel Allison and James Franck and, of course, Leo Szilard. Compton writes: ‘Leo Szilard, a most remarkable genius, undertook to secure pure materials, especially graphite, but in reality was concerned with every part of the enterprise’. Glenn Seaborg was called in from Berkeley to take charge of the studies of the chemistry of the plutonium.

Right from the start, Arthur Compton was aware that the plutonium project could lead to special risks for those involved. He therefore decided to set up a Health Division relating to health matters within the Met. Lab. He recruited San Francisco radiologist Robert S. Stone (1895-1966) as head of the new department.

Stone, who was 47 years old at the time, was a man with very good qualifications. He was born in Canada and had studied biology and physics at the University of Toronto. His studies were interrupted by the First World War, and in 1916 he was sent to Europe with the Canadian expeditionary force. There, he was wounded in the fighting and returned to his studies in Toronto in 1917 in order to graduate in 1919 in the slightly unusual combination of biology and physics. He then went to Peking (now Beijing) to become an anatomy assistant (!) at the medical faculty. When he returned to Toronto in 1921, it was to take an exam in neuroanatomy one year later. He then became licentiate of medicine at the same university in 1924.

So, ‘Bob’ Stone had a very all-round education when he came to the United States in 1925 for a position as a radiologist at a hospital in Detroit. From there, he came to the University of California’s Medical Centre in San Francisco in 1928 as the university hospital’s first radiologist. There was no radiological department at the time - radiology was a sub-department within surgery, but Stone rapidly set up a radiology department and became Professor and its first manager.

Stone was the first within many areas. He was the first to use million-volt apparatuses for radiation treatment (1934) and the first to use neutrons to cure cancer (1938). He was also the first, along with *Joseph Hamilton*, to treat chronic lymphatic leukaemia with an artificial radioactive substance. This occurred in 1936, two years after the Joliot-Curie couple produced such a substance for the first time. Stone and Hamilton used sodium-24 which was produced in Ernest Lawrence’s cyclotron. Stone possessed a unique competence to take care of the health problems of the new atomic age.

Stone set to work on 6 August 1942 and rapidly organised a number of sub-divisions. One of these was the medical section. Stone was lucky enough to find a clever young doctor to manage it, *Dr. Simeon Cantril* (1908-1959). Cantril had worked at the Curie Institute in Paris and had then spent five years as a radiologist in Chicago until, at just thirty years of age, having been employed as head of the cancer clinic at the Swedish

Hospital in Seattle in 1938. In the preparedness situation during the war, he had been called up as a military doctor but it was not difficult for Arthur Compton to convince the military authorities that Cantril was very useful to the Met. Lab.

Another of Stone’s sub-divisions was the physics section. There was already a suitable head in place for this. Compton had previously employed *Ernest O. Wollan* to take radiation measurements and perform calculations for radiation screens around the experimental reactor that Fermi would be building. Wollan became head of the physics section. Since this was a sub-division of Stone’s Health Division, it was natural to use the name Health Physics which, from the secrecy point of view, was also a name that was sufficiently all-encompassing not to arouse any particular curiosity.

Wollan obtained a very competent colleague in his Health Physics section, namely Englishman *Herbert Parker* (1910-1984). Parker was a well-known name within medical physics due to his work in Manchester together with radiologist *Ralston Paterson* (1897-1981) with the standardisation of precise positioning of radium preparations, the Manchester Method. When Cantril started as head of the cancer clinic in Seattle in 1938, he had offered Parker the chance of bring his medical physicist. Parker had accepted, and Rolf Sievert’s guest book shows that the journey to the United States was prepared for with a study visit to Sievert in Stockholm. When Cantril was called to the Met. Lab. in Chicago, Parker decided to go with him.

A third sub-division was the biology section. It was run by *Kenneth Cole* (1900-1984), Professor of Physiology at Columbia University with an educational background in physics.

Stone's division belonged to the university world since the University of Chicago was responsible for the plutonium project. Funds for the operation and overall directions therefore came from Bush's OSRD. During the coming year, however, the military would gradually take over all projects concerning atomic bomb research, but they did not initially get involved in Stone's activities.

On 18 June 1942, General Styer had given *Colonel James Marshall*[*] the task of creating a new project district for the construction of production plants as per the proposals made by Conant from the S-1 section. As manager of the project division, Marshall's title was 'District Engineer'. Marshall in turn selected *Lieutenant Colonel Kenneth Nichols* as his deputy. Nichols has written:

> When I entered his office on Sunday morning, 21 June, Marshall sat at the head of the table with a large folder in front of him. […] I had 20 seconds to register as a volunteer for the job of deputy District Engineer - or be ordered to do the task. I registered as a volunteer.

Marshall immediately informed other military bodies of the assignment, including the army's building section of which *Colonel Leslie Groves* (1896-1970) was the deputy manager. Groves was sceptical but promised to help. He suggested that Marshall engage the construction company Stone & Webster. The project district was initially limited to the construction of the production plants, not for the production of the bombs. The OSRD under Vannevar Bush initially continued to administer the necessary research projects but their money began to run out. Luckily, Colonel Marshall succeeded in arranging 15 million dollars from the engineer troops.

On 25 June 1942, Styer, Marshall and Nichols took part in an S-1 committee meeting (which it was now called since it had become more independent, with Conant as chairman) and concluded that neither the centrifugal nor the diffusion method was ready for development. They therefore initially concentrated on the plutonium project in Chicago and the electromagnetic separation method which was being developed in Berkeley. The head of the engineer troops, *General Eugene Reybold* (1884-1961) wanted the name of the production plants to be 'Laboratories for the Development of Substitute Materials' (DSM), but people like Colonel Groves did not like this, saying that the name could arouse curiosity.

In Chicago, Fermi continued his reactor experiments but had not yet succeeded in obtaining a chain reaction. As soon as there was success with doing so, the construction of an experimental reactor would start, followed by production reactors as soon as possible. An area in the Forest of Argonne outside Chicago was considered suitable for the experimental reactor. Groves suggested an area of land close to Knoxville, Tennessee for the production reactors.

However, things progressed slowly for Colonel Marshall, who was impaired by lack of money despite being an AA-3 priority, and Groves began to doubt the viability of the project. In August 1942, Marshall consulted Groves regarding a draft for a general order to definitively create the new engineer district (the project district) and used the name 'DSM' as General Reybold had proposed. Groves suggested 'Manhattan Engineer District' (MED) instead since Marshall had his office in Manhattan. But at that time, its form of an 'engineer district' clearly showed that the project was still not properly viable.

In September 1942, a change occurred which saved the 'Manhattan Project'. On 17 September, the Secretary of War decided to assign the responsibility for the total atomic bomb investments to Colonel Leslie Groves, who was thereby promoted to Brigadier General. Groves was already a very important person at the time, responsible for all of the army's building projects within the United States: barracks, airfields, harbours, factories, etc. The latest project was the construction of the Pentagon, the big, five-cornered building in Arlington outside Washington DC, intended for the War Department (now the Defence Department) and the headquarters of the various defence services. In his work, he had helped Colonel Marshall just as he helped others who had building plans, but he was far from knowing about

[*] Not to be confused with head of the army, General George Marshall.

the details of the atomic bomb project and the potential scope thereof. To top it all, he was in the process of requesting a transfer to active service during the war. In his memoirs, Groves writes:

> [...]I met Somervell* outside the hearing room [of the congress], and asked him whether he had any objections to my being relieved from my army construction duties. To my great surprise, he told me that I could not leave Washington DC. He went on to say:
>
> 'The Secretary of War has selected you for a very important assignment, and the President has approved the selection.'
>
> 'Where?'
>
> 'Washington.'
>
> 'I don't want to stay in Washington.'
>
> 'If you do the job right,' Somervell said, 'it will win the war.'
>
> My spirits fell as I realized what he had in mind.
>
> 'Oh, that thing,' I said.
>
> Somervell went on: 'You can do it if it can be done. See Styer and he will give you the details.'

Groves was deeply disappointed. He knew the purpose of the project: to use uranium to create an atomic bomb that was expected to have unsurpassed power. But the project was not thought to be particularly important in economic terms – it might involve 100 million dollars. This was certainly a large sum, larger than for any of the other jobs that Groves had monitored, but the sum was less than the army's total costs over a week. Groves writes: '[...] what little I knew of the project had not particularly impressed me, and if I had known the complete picture I would have been still less impressed'.

Having been given additional information by General Styer, Groves informed General Reybold of the assignment he had been given. He then visited Lieutenant Colonel Nichols to get an idea of the situation since Colonel Marshall was on an assignment in the Pacific Ocean. Groves was shocked by the size and degree of difficulty of the project and by its low priority. He and Nichols then turned to Vannevar Bush who, because of some mistake, was not yet aware of Groves' assignment. The talk with Bush was therefore a disappointment to the eager and energetic Groves. Bush did not want to answer the many questions that Groves asked and was suspicious of Groves' alleged assignment. Not until Nichols and Groves had left Bush and the latter could ring General Styer was the mistake cleared up, but it had managed to give Bush an unfavourable impression of Groves. Groves remembers:

> Bush accepted the appointment, although he was quite disturbed. He told Styer that he felt I was too aggressive and might have difficulty with the scientific people. Styer told him that this was quite possible and that the two of them, personally, might have to smooth out a number of difficulties, but that the work would move.

In spite of this unfortunate start, Groves and Bush would cooperate very well and become good friends. Groves maintains that they never once disagreed on any matter throughout the project. Groves asked his secretary *Jean O'Leary* to accompany him on the new assignment, and when Colonel Marshall returned from the Pacific Ocean, Groves announced that the MED would continue as before with Marshall as District Engineer.

When, as Colonel, Groves was given his assignment he was forty-six years old. He was well-educated. He had graduated from the University of Washington in 1914 and studied at MIT for two years before being accepted at West Point, the American army's cadet school with emphasis on academic studies in technical subjects. He graduated from there in 1918 as fourth in his class. He continued his education at the army's technical schools during the 1920s and had been posted in Europe and Central America before being asked to take responsibility for the army's building project.

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* *Lieutenant General Brehon Somervell* (1892-1955), head of the army's maintenance service.

Lieutenant Colonel Kenneth Nichols, the deputy District Engineer who would cooperate closely with Groves, has described him in the following words (quoted from Rhodes):

> […] the biggest sonovabitch I've ever met in my life, but also one of the most capable individuals. He had an ego second to none, he had tireless energy - he was a big man, a heavy man but he never seemed to tire. He had absolute confidence in his decisions and he was absolutely ruthless in how he approached a problem to get it done. But that was the beauty of working for him - that you never had to worry about the decisions being made or what it meant. In fact I've often thought that if I were to have to do my part all over again, I would select Groves as boss. I hated his guts and so did everybody else but we had our form of understanding.

As said, Groves was a big man, being said to have weighed 110 kilos or more when he was given his big assignment. He was putting on weight - his sweet tooth was common knowledge. Rhodes describes him as a man who was 180 centimetres tall ‘with curly chestnut hair, a sparse mustache and sufficient girth to balloon over his webbing belt above and below its brass military buckle’.

Groves is said to have been a fan of boys’ books, which is perhaps shown by what he has written about himself:

When I was a boy, I lived with my father at a number of the Army posts that had sprung up during the Indian wars throughout the western United States. There I came to know many of the old soldiers and scouts who had devoted their active lives to winning the West. And as I listened to the stories of their deeds, I grew somewhat dismayed, wondering what was left for me to do now that the West was won.

But the young Groves need not have been disappointed - he was given plenty to do. It might have been possible to say that he had misfired had it not been for the total secrecy that was quickly ordered and where a need-to-know basis was the dominant principle. No-one was informed of anything unless they needed to be, and when *Harry S. Truman* (1884-1972) became America’s Vice President in 1944, not even he was informed of what was going on. All threads led to Groves who, along with his secretary, the indispensable Jean O’Leary, had the responsibility of knowing details of all secrets which were embraced by no-one else.

It seemed as though Groves loved secrets. The key people who were assisting him all went by code names. Groves himself was often called ‘99’ because Jean O’Leary used to write ‘g.g.’ for ‘General Groves’ in a way which made them look like two nines. Enrico Fermi was called Henry Farmer and Niels Bohr became known as Nicholas Baker.

Groves had already approached the head of the War Production Board *Donald Nelson* on 19 September to demand higher priority than AA-3. Nelson refused at first. Groves then said that if that were the case, he had to recommend to the President that the project be shut down. Nelson was dismayed and changed his mind, whereupon Groves was given the highest possible priority: AAA.

Groves’ first contacts with the scientists were largely successful, with one noteworthy exception - Leo Szilard. There, the result was disastrous. Szilard with his European background, original behaviour and ambition to be part of most things and give advice on everything, aroused Groves’ suspicion. Groves’ description of Szilard was ‘the kind of man that any employer would have fired as a troublemaker’.

The growing conflict between Groves and Szilard can be seen as a conflict between extreme military and individualistic desires. Groves compartmentalised different parts of the operations with partition walls and was strict with operating his need-to-know principle. Szilard strove for total openness and thought that you could never know in advance what new ideas might turn up which could be important to the work.

At the Metallurgical Laboratory, sometime later even Compton thought that Szilard’s curiosity and involvement in work assignments that were not his own was actually disturbing, and he tried to get him to return to New York. Groves saw Szilard as a mischievous stranger and drafted a letter in which he suggested that Szilard be interned for the rest of the war, something which Compton luckily prevented. It went as far as Groves suspecting Szilard of being a German spy, which was of course a completely unreasonable thing to suspect in the eyes of those who knew him.

One reason for the conflict was that Groves did not know of Szilard's previous activities where the latter had been the person who had primarily attempted to get the American authorities to invest in nuclear weapons research. Not until Szilard sent Compton a thick bunch of documents showing the way in which he and Wigner had been responsible for ensuring that President Roosevelt obtained information on the possibility of an atomic bomb between 1939 and 1940 did Groves begin to understand the importance of Szilard. But Szilard had felt misunderstood and underestimated and began a long discussion with the military authorities on patent rights for the bomb, which was a tragicomedy.

However, there were many scientists who greatly appreciated Groves. One example is mentioned by Edward Teller in his introduction to Groves' book on the Manhattan Project. Teller says that on a visit to England after the war he was invited to Sir James Chadwick's home. Sir James was a man of very few words, but his wife was much more of a conversationalist. On one occasion at lunch, she asked Teller about General Groves. Teller's response about Groves was not particularly flattering. Teller says:

> At that point, a miracle occurred. Sir James, who had spoken perhaps twenty words that evening, became talkative to the point of being almost uninterruptible. He told me most emphatically and repeatedly that the atomic bomb project would never have succeeded without General Groves. I pointed out how often Groves had made plain his dislike of the British. Sir James brushed aside my comment. That made no difference. What was important, sir James went on, was that Groves understood the overriding importance of the project better than some of the leading American scientists. Without Groves, he said, the scientists could never have built the bomb.
>
> I have rarely seen anyone - even an ordinarily effusive talker - so insistent on making his point. However, Sir James's tirade carried no trace of reproach for my inappropriate remark about General Groves. At the end of the evening, my host walked me back to my inn. On parting, he told me to remember what he had said as I might 'have need of it'.

One of Groves' first visits concerned the Naval Research Laboratory in San Francisco. This is where he met Ross Gunn and Philip Abelson who were disgruntled that the Navy had been kept out of things so far. They told Groves about their research with thermal diffusion, but this gave Groves a negative impression because they seemed to have too little time and insufficient resources to spend on the assignment.

Abelson continued with the thermal diffusion experiments in any case. The Navy was interested in ship reactors and Abelson realised that reactors with natural uranium-238 would be too bulky. He thought it ought to be possible to make them smaller if the uranium were enriched to uranium-235, so the separation technique was therefore important. But Abelson was isolated from the MED and did not know what was planned on its behalf. Roosevelt had given orders for the Navy not to be involved in the Manhattan Project, which was something that Groves would eventually change.

A group to control the atomic policy at the highest level was now needed more than ever, but the existing Top Policy Group was not effective because not all of its members could give enough time. Secretary of War Stimson therefore proposed a new group. Groves did not want it to be more than three people. The result was that Bush was appointed as chairman of a new policy group, with Conant as deputy. The other two members were *Rear Admiral William R. Purnell* (1886-1955) and General Groves. However, the latter declined the assignment and was replaced a few days later by General Styer.

Groves established his headquarters in Washington DC. In August 1943, Nichols succeeded Colonel Marshall as District Engineer when Marshall was promoted to Brigadier General. Groves later regretted that he had not immediately found someone to replace him should he or Nichols die. Not until the end of 1944 did Stimson oblige him to find a replacement. He ordered Groves: 'I want you to get a Number Two man immediately who can take over your position, and with Nichols' cooperation, carry on in the event that something happens to you. You can have any officer in the Army, no matter who he is, or what duty he is on.'

After having discussed his choice with Lieutenant Colonel Nichols, Groves decided on the Brigadier General of the Air Force, *Thomas Farrell* (1891-1967), a man with whom he had cooperated for a long time.

There soon turned out to be problems procuring the necessary uranium, although there was unexpected luck in this regard. The Managing Director of Union Minière du Haut Katanga, Edgar Sengier, who had previously refused to give the Englishmen the option of the Belgian uranium, had left Brussels in October 1939 for New York. He had then taken care of the company's affairs from there. Facing the threat of a German invasion of the Congo at the end of 1940, Sengier ordered the available ore, 1 250 tonnes, to be shipped to the United States where it was stored on Staten Island. Sengier tried repeatedly to get the American Ministry of Foreign Affairs' officials, including a man by the name of *Thomas Finletter* (1893-1980), to understand the importance of the uranium. However, the Ministry still did not know about the Manhattan Project (it did not find out until just before the Jalta Conference in February 1945) and therefore saw no value in uranium, despite the fact that newspaper articles such as Laurence's ought to have informed them as early as 1940. It was not until September 1942 that Finletter told Lieutenant Colonel Nichols about the uranium.* It was a great and encouraging surprise for Nichols to have the information confirmed when visiting Sengier. Groves writes about the visit:

> […] when Nichols opened the conversation, Sengier was somewhat guarded in his reply, recalling how the State Department had consistently ignored his repeated proddings. After inspecting Nichols' credentials, he said: 'Colonel, will you tell me first if you have come here merely to talk, or to do business?' Nicholas answered diplomatically, as always, that he was there to do business, not to talk. Sengier then really pleased him by saying that it was true that over 1,250 tons of rich ore were stored in some two thousand steel drums in a warehouse on Staten Island.
>
> A delighted Nichols left Sengier's office an hour later carrying with him a sheet of yellow scratch paper on which were written the essentials of an agreement to turn over to us at once all the ore in the Staten Island warehouse and to ship to the United States all the richer uranium ore aboveground in the Belgian Congo.
>
> A simple handshake proffered by Sengier had sealed the bargain. The exact wording of the written contract would be settled later.

The plutonium project at the 'Metallography Laboratory' in Chicago was still theoretical: it was known that plutonium was fissile but it had not yet been shown that plutonium could be formed in a reactor – there was no reactor as yet. Thus far, all of the plutonium had been produced in Lawrence's cyclotron, and even in December 1943 there was no more than two milligrammes. It was vital to Groves' planning to know how much plutonium or uranium-235 was required for a bomb. Groves discussed these problems with Compton's group in Chicago but he was dismayed when the accuracy of the estimated quantities was said to be no better than a factor of ten.

Groves was still impressed by the scientific competence of the Chicago scientists. It appeared simpler to produce a plutonium bomb than one with uranium-235. However, Groves understood that the overall project was too big for Stone & Webster alone and understood that different operators might be needed for each plant. For the tasks in the plutonium project, i.e., the construction of production reactors, the operation thereof and the extraction of plutonium from the reactor fuel, Groves thought that there was only one American company big enough to cope with it all, i.e., Du Pont. The giant Du Pont had originated from a gunpowder factory which was founded in Wilmington† in 1802 by Frenchman *Èleuthère Irénée Du Pant de Nemours* (1771-1834). Du Pont specialised in explosives early on but later in pigments and plaster (nylon, Dacron, Teflon, etc.).

It was not easy to convince Du Pont's managers to enter an area in which they were not experienced. It had not yet been shown that a reactor could function and they wanted to build several of them in spite

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* The fact that the Ministry for Foreign Affairs did not know about the Manhattan Project until the time just before the Jalta meeting is surprising. However, General Groves writes in his memoirs that "it was in accordance both with President Roosevelt's policy of personally conducting international relations and with his disinclination to bring any unnecessary persons into atomic affairs. And it was well known, of course, that there was friction in the higher levels of the Department".

† Wilmington in the state of Delaware got its current name in 1739 but the first settlement was made up of Swedish colonists in 1638 under the name of Fort Christina.

of this. Yet Du Pont accepted the task when it was said that the bomb could shorten the war and save lives. However, one of Du Pont's higher officials complained that the company had been given the most difficult task. Groves' response was that it was the easiest; it was just that they did not yet know how to solve the separation of uranium-235.

But the scientists in Chicago protested. They thought that they could do the project themselves if they were simply allocated a hundred or so engineers. Groves saw how unreasonable this was; in reality, Du Pont would use 45 000 men for the task and 10 000 subcontractors! Some scientists also opposed the fact that men like Bush and Conant controlled the atomic bomb project. They thought that no-one over the age of 40 could understand nuclear physics, but Groves disagreed. He writes in his memoirs: 'This was quite absurd, for it was not and is not extraordinarily difficult for anyone who will apply himself to learning them to understand the basic principles of atomic physics'.

In 1942, Robert Stone's health department carried out some really pioneering work. The plutonium project would require large nuclear reactors in which not only was plutonium formed after being transformed from uranium-238, but also fission products with a radioactivity corresponding to tonnes of radium. Each phase of the work could involve hazards of a type that were not known up to now.

The most acute danger was thought to come from the handling of the huge quantities of uranium. Since uranium could be just as toxic as other heavy metals, work procedures needed to be introduced to reduce the risk of swallowing or inhaling uranium dust. This was not a risk concerning radiation, and it was gradually found that those working on the Manhattan Project actually ran lower risks from radiation and the radioactive substances than from other aspects of the operations.

Routine urine and blood tests were introduced in the hope of being able to detect early signs of radiation injury. However, the normal measurement values were shown to vary too greatly and to be affected by too many factors in addition to radiation, and that these routine tests were not viable for the purpose of giving early warning signs. We would draw the same conclusion in Sweden but not until twenty years later.

Warnings of the risks of radiation therefore had to come from physical measurements. Before the war, there was scarcely any commercial production of radiation detectors. Radiation measurements occurred almost exclusively in connection with the radiation treatment of tumours, and then it was a matter of high radiation doses. The measurement methods were based either on radiochemical changes in chemical dosimeters or on the capacity of the radiation to ionise air. In the latter case, the measurement instrument was an ion chamber, i.e., an isolated volume of air in which an electric current could pass between two electrodes if the air became electrically conductive through ionisation. The current could be measured using a connected instrument but this was difficult since the ion chamber and measurement instrument had to be connected with a cable. At the start of the 1930s, Sievert had made a great achievement through the idea of a condenser chamber. A condenser chamber is an ion chamber in which the electrodes have been designed so that the chamber has sufficient capacity to bind a measurable electric charge. Before the chamber was used, it was connected to a battery to be charged up. It could then be placed at the point of measurement, e.g., on a patient, with no cable in the way. When the radiation ionised the air so that the latter became electrically conductive, this neutralised part of the charge. The chamber could then be taken to a measurement instrument, usually a sensitive electrometer, to measure the residual voltage between the electrodes. The loss of charge could then be calculated and be considered to be in proportion with the radiation dose.

Condenser chambers worked well for measuring the high radiation doses during radiation treatment but it was more difficult to get them to work for measuring low radiation doses during routine measurements of personnel doing radiation work. The ionisation then became so small that leak currents between the chamber's electrodes became significant and disrupted the measurement results, particularly if the condenser chamber was left charged up for a longer period, e.g., a work-day or a working week. The only substance that could keep the chamber's electrodes adequately insulated before insulating plaster became available was amber, and access to amber was limited.

The first protection measurements condenser chamber that was commercially available was sold in 1940 by Victoreen Instrument Co. in Cleveland. It was a variation of the well-known Victoreen R-Meter that was used for x-ray treatments in the 1930s. In Sweden, Sievert's 'Bg-chamber' (amber insulation and graphite electrodes) was used in radiation therapy but only for high radiation doses; Sievert certainly also designed a chamber for low radiation doses but those that were practically viable for use were not available until the 1950s.

Victoreen's system - condenser chamber plus instrument for charging and reading – was called the Minometer. The chamber was not particularly reliable as the result was affected by vibrations and moisture. Herbert Parker therefore ensured that every worker carried two condenser chambers. The error that arose always involved an extra charge loss, which showed an erroneously high dose as a result. Parker therefore introduced the rule that if the two condenser chambers showed different results, the lowest value would be the correct one.

In 1945, the pocket dosimeter started to replace the condenser chamber. It consisted of an ion chamber that still needed to be charged using a special device but which needed no further instrument for reading. An electrometer wire was built into the dosimeter and its output could be read off against a scale using an inbuilt magnifying glass. This form of directly readable condenser chamber was shaped like a cylinder with an eyepiece in one end. It could be carried in a breast pocket like a pen and was often called the pen dosimeter. The carrier could read off the output at any time and any number of times but the pen dosimeter was usually charged once a day.

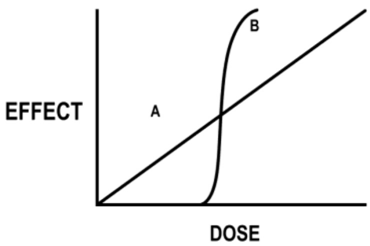
An alternative to the ionisation measurements was to measure the blackening of a photographic film. Film dosimeters offered several benefits. They could be carried for long periods to register the total accumulation of radiation doses. They could be fixed to the identity card that everyone would wear on their chest. If the blackening showed any clear shadows, it could be assumed to originate from a short-term exposure and conclusions could be drawn regarding the direction of the radiation. Films such as dental films cut into a suitable format and enclosed in a protective case were already on the market.

However, the film dosimeters also had weaknesses. The blackening could be completely different from one and the same radiation dose, depending on the type of radiation. This inconvenience could be partially eliminated by putting different types of filter in front of the film. This required considerable development work.

By using the different measurement instruments, it was possible to estimate the radiation doses that the employees had received. The activities were made easier by the fact that there had already been recommendations from the US Advisory Committee on X-ray and Radium Protection* on a 'tolerance dose' of 0.1 röntgen per day from x rays and gamma radiation since 1934. Stone was aware that this value was on shaky ground but it was better than nothing.

The actual term 'tolerance dose' was disliked by Stone, partly because it expressed an opinion and indicated that small radiation doses ought to be tolerated. However, this tallied with the medical experience of the time, which did not indicate that there were any harmful effects from small radiation doses. Stone also thought that caution ought to be exercised and action to be taken as if there were no threshold value below which a radiation dose could not be dangerous. In 1941, the NCRP had already recommended that, as a consequence of the possibility of hereditary injuries, the term ought to be 'permissible dose' rather than 'tolerance dose'.

Stone was not alone where this caution was concerned. In 1945, Cantril and Parker published a report for the American Atomic Energy Commission on the subject of a tolerance dose. In the report, they wrote and illustrated the following link between radiation dose and effect (= impact):



* Now the National Council on Radiation Protection and Measurements, which previously started out as the US Advisory Committee on X-Ray and Radium Protection and later became the National Committee on Radiation Protection and Measurement. The early history of the NCRP and the International Radiation Protection Commission (ICRP) has been given in Pandora's Box

Threshold and non-threshold effects – If one plots a dose-effect graph* for various tissues subjected to radiation, there are, in general, two forms which the graph may take:

Curve A illustrates the non-threshold case, where, as the dose is increased there is a linear increase in the effect. There is no initial threshold of dose which must be exceeded before an effect is obtained. To recognise a non-threshold effect, it must be readily observable or measurable after exposure to minimal amounts of radiation. An example is the influence of radiation upon the germ plasm of lower organisms.

Curve B illustrates a threshold effect. Here the effect is not measurable by present methods until a certain threshold of dose is exceeded. Threshold effects are not linear with dose but assume some form of S curve. The effects of radiation upon the skin and the blood-forming organs are examples. Until the dose reaches or surpasses the threshold, the first signs of skin effect (erythema) or of effect upon the blood-forming organs (as reflected in the circulating blood) are not seen.

The majority of radiation effects are thought to be of the threshold type. It may be that as more delicate indicators are found to measure effects, more of them will be seen to be of the non-threshold type.

The linear connection that Cantril and Parker were thinking of concerned the level of injury rather than the likelihood of injury. It seemed more obvious to make a comparison with a photographic film. Light and other radiation causes photochemical reactions so that silver is precipitates from the silver salts in the film emulsion (bromide, chloride and iodide). The greater the dose (the exposure), the greater the effect on the grains of salt in the emulsion and the blacker the film becomes. In the dose effect connection there is no dose threshold; one single photon can affect a grain but such blackening is then only microscopic.

Similarly, you could picture the radiation damaging or killing cells. A very small radiation dose damages just a few cells but the level of impact in the form of the number of damaged or killed cells can initially be assumed to increase in proportion with the radiation dose so that the dose-effect connection is linear. As long as the radiation dose is small, the damage is random for each given cell. Random injury is now usually called *stochastic* (from the Greek *stokastikos* = comes under conjecture) as opposed to deterministic injuries that are unavoidable if the radiation dose is high enough. At the cellular level, cell death is therefore stochastic while at tissue level it is deterministic and requires the radiation dose to exceed a threshold value which means that enough cells are killed for an observable injury such as erythema to arise.

This change from stochastic to deterministic connection does not take place if one single damaged cell is sufficient for the injury to also be noticed at tissue or individual level. This is the case with radiation-induced changes in a gamete. There, it is sufficient for one changed cell to be part of fertilisation and create a new individual which inherits the primary cell injury. Chance at the cellular level is then reflected as being correspondingly random at individual level.

The capacity of radiation to achieve genetic changes for better and (mainly) for worse was discovered in 1926 by the American geneticist *Hermann Müller* (1890-1967), who won the Nobel Prize in Medicine in 1946 for having proven that mutations can be caused by irradiating chromosomes with x rays.

The consequences of such changes were usually called ‘genetic injuries’. In these cases, it was the gametes that were affected by the primary injury. However, somatic cells (body cells) can also suffer injuries that change the code which programmes the behaviour of the cell. This does not then lead to hereditary injuries to an individual but can instead lead to injuries which either affect the capacity of the cell to divide or lead to the cell ‘forgetting’ its original function and behaving anti-socially at the cellular level. In this case, it is the genetic code of the somatic cell which has been changed. If this is the case, the randomness that applies at the cellular level can be reflected in a randomness of the injury that is finally manifested, as in the case of radiation-induced cancer. It is therefore also possible to refer to

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* A *dose-effect curve* shows the link between the radiation dose and the *level* of injury. *Dose-response curves* show the link between the radiation dose and the likelihood of injury (e.g., cancer) but the expression dose-effect curve is sometimes used carelessly when dose-response curves are actually being referred to.

'genetic' injuries to somatic cells and use of the expression 'hereditary changes' is now preferred rather than 'genetic injuries' when it comes to properties that can be inherited.

The construction of the first experimental reactor began in the Forest of Argonne, but the work was delayed. In the meantime, Fermi, who had now come to Chicago, began building an experimental plant in the squash courts under the west stand at Stagg Field at the University of Chicago. This work was still done under the OSRD with Arthur Compton as project manager and the plant was called 'CP-1' (for 'Chicago Pile No. 1').

Nothing was known about the stability of a reactor and whether it could be kept under control. This was where the information on delayed neutrons had now become important. The discovery of neutrons that were not emitted immediately during nuclear fission had been published in 1939 by Richard Roberts' group at the Carnegie Institute's research laboratory (DTM) in Washington DC before all physics reports that could be of any military significance became subject to secrecy.[*]

For a reactor to be able to maintain a chain reaction, the production of neutrons had to be at least as great as the leak of neutrons from the reactor plus the loss of neutrons through the absorption thereof. The multiplication factor (k) refers to the quota of production/(leak + absorption) and the multiplication factor has to be at least equal to 1. If k is equal to 1, the situation is called critical. If k is less than 1, it is called subcritical and if k is greater than l, it is called supercritical.

Of the neutrons produced, approximately 0.75% are delayed.[†] This means that as long as k is less than l.0075, it is relatively easy to keep the chain reaction under control by introducing control rods of a substance that is a strong absorber of neutrons. If k is greater than l.0075, the reactivity[‡] of the device becomes independent of the delayed neutrons and the reactor's energy development peaks extremely quickly. Even if it is not a matter of an actual reactor but simply an accumulation of fissile material in which a chain reaction develops out of control, it is called a *criticality accident*. The chain reaction does stop quickly, however, when the heat development and the high temperature have vaporised material so that the reactivity is reduced. As Frisch had realised (see Chapter 6), fast neutrons and a bomb of highly-enriched uranium-235 (or plutonium) are needed for the chain reaction to develop quickly enough for an actual 'nuclear explosion' to take place before the reactivity becomes negative.

However, without the delayed neutrons, it would not be possible to gain satisfactory control of a reactor. This was a generally known fact following the DTM report in 1939.

Fermi's group began building the experimental plant in November 1942. It consisted of blocks of graphite and cubes of compressed uranium oxide or metallic uranium. To keep neutron-absorbing air at bay, the idea was to encapsulate the whole structure in a cubic rubber balloon from which the air could be pumped out. The Goodyear Tire and Rubber Company which had received the order for the large balloon was greatly surprised, but no-one could quell their curiosity.

While the reactor was being constructed, one side of the balloon was held open so it was like working in a tent (it was later found that the balloon was unnecessary and it was never sealed). There, blocks of pure graphite were alternately stacked with blocks of graphite containing cubes of uranium oxide or uranium metal. The pile was up to seven metres high and eight metres in diameter, and the 57th layer of graphite blocks completed it. The geometric shape of the CP-1 is usually described as a doorknob, i.e., a flattened sphere. The total materials used were approximately 350 tonnes of graphite, 36 tonnes of uranium oxide and 5 tonnes of uranium metal. When the structure was a certain size, you could count on the device being critical. Fermi had calculated exactly how many layers would be needed. He was absolutely sure he was right - and he was.

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[*] Roberts, R.B. et al. in *Physical Review* 55 (1939), p. 664.

[†] The Smyth report stated that 1 % are delayed by at least 0.01 of a second and 0.07 % are delayed by as much as a minute.

[‡] The reactivity of a reactor is defined as (k-1)/k where k is the (effective) multiplication factor of the reactor. When the reactivity is positive, the reactor is supercritical and its effect increases, and when the reactivity is negative, the reactor is subcritical and the effect becomes less. The reactivity is thus zero when the reactor is critical and working with constant effect.

If a reactor is subcritical, i.e., the multiplication factor k is less than r, and neutrons are added from a special neutron source such as a radium-beryllium preparation at the bottom of the pile, this will lead to the creation of a neutron flow which appears to increase until, after a while, it reaches a saturation value. If n_0 is the number of neutrons per unit volume from the start, the saturation value nm will be r/(r-k) times as great. If the multiplication factor is k = 0.5, for example, the saturation value will be twice as much as the start value was with the added neutron source. Measurements of the number of neutrons in the reactor at different times after an external neutron source has been added enable us to establish the value of nm/n_0 and calculate k on that basis. The quota nm/n_0 with subcriticality is usually called the *subcritical multiplication factor*. As criticality is approached, the saturation values become higher and higher, and at criticality, the neutron flow continues to increase until the process is stopped or it stops itself through the development of heat.

In the afternoon of 1 December 1942, measurements approaching criticality were shown. Cadmium control rods were lowered into the reactor. When the number of layers Fermi said were needed had been stacked, it would be time to pull up the control rods. Over the night to 2 December, those present were tempted to do so, but since Fermi was not there, they resisted the temptation. Walter Zinn and Herbert Anderson took measurements overnight and found that the reactor 'was alive' but, thanks to the control rods, subcritical. Herbert Anderson has described the situation (quoted from Rhodes):

> When the 57th layer was completed, I called a halt to the work, in accordance with the agreement we had reached in the meeting with Fermi that afternoon. All the cadmium rods but one were removed and the neutron count taken following the standard procedure which had been followed on the previous days. It was clear from the count that once the only remaining cadmium rod was removed, the pile would go critical. I resisted great temptation to pull the final cadmium strip and be the first to make a [uranium] pile chain react. However, Fermi had foreseen this temptation and extracted a promise from me to make the measurement, record the result, insert all cadmium rods, and lock them all in place.

On the morning of 2 December, we began to lift the control rods in front of a group of interested onlookers at the squash hall gallery, including Arthur Compton. Huddled on the graphite pile sat three young scientists, the 'suicide squad'. Their assignment was to pour a fluid containing cadmium over the reactor should anything go wrong. And then began the lifting of the control rods, one by one, except for the last rod which would be pulled up by a young physicist, *George Weil*. They worked very carefully facing the unknown. There might be an explosion or an eruption of neutrons which could cause those present deadly injuries. Not until 5.30 did Fermi finally call: 'Pull it out another foot, George,' and to Arthur Compton he said: 'This is going to do it.'

And it did. Herbert Anderson describes the event (according to Rhodes):

> At first you could hear the sound of the neutron counter, clickety-clack, clickety-clack. Then the clicks came more and more rapidly, and after a while they began to merge into a roar; the counter couldn't follow anymore. That was the moment to switch to the chart recorder. But when the switch was made, everyone watched in the sudden silence the mounting deflection of the recorder's pen. It was an awesome silence. Everyone realized the significance of that switch; we were in the high intensity regime and the counters were unable to cope with the situation anymore. Again and again, the scale of the recorder had to be changed to accommodate the neutron intensity which was increasing more and more rapidly. Suddenly Fermi raised his hand. 'The pile has gone critical,' he announced. No one present had any doubt about it.

The multiplication factor was 1.0006 but the reactor output was only 0.5 watt. Later, on 12 December, the output was increased to 200 watts. They did not want to go higher because the pile had no cooling device so the temperature could have become dangerously high had they tried to increase the output further.

Everyone drank Chianti from paper cups and Arthur Compton went to report to James Conant by long distance telephone call. They had not pre-arranged any special coded message – it went without saying.

> 'Jim, you'll be interested to know that the Italian navigator has just landed in the new world,' said Compton. And since he had recently told Conant that it would take another week for the reactor to go critical, he added an explanation: 'The Earth was not as large as he had estimated, and he arrived at the new world sooner than he had expected.'
>
> 'Is that so?!' was Conant's excited response. 'Were the natives friendly?'
>
> 'Everyone landed safe and happy.'

Only Leo Szilard was glum. He was the one who had been the most stubborn in warning the American government about the risk of a German atomic bomb. He had made energetic contributions to the accomplishment of the American atomic bomb project. It was now becoming a reality and Szilard realised what this could mean. Although he thought the bomb was necessary, he realised what suffering it could bring. When everyone else had left the squash court, he shook hands with Fermi but said that this day might go down as a black day in the history of mankind.

9. THE PRODUCTION PLANTS

BY NOW IN MID-1942, next to nothing had been done regarding bomb constructions. The army's assignment under General Styer had not included the actual bomb, just the construction of the production plants. This was also the assignment that Styer had given Colonel Marshall on 18 June and that involved the formation of the Manhattan Engineer District. Arthur Compton still had the assignment from Bush and Conant to develop the bomb.

In June 1942, Compton had employed Robert Oppenheimer to take responsibility for the construction of the bomb together with a small group in Berkeley. However, after Groves was made responsible for the whole bomb project in September 1942, one of his first measures was to start a special project that was devoted to the construction of the bomb. This new project was called 'Project Y'.

Neither Groves nor Bush and Conant felt bound by the fact that Oppenheimer was already working on the same task, but searched without inhibition for someone to head the Y project. Ernest Lawrence was a strong candidate but he could not be released from the task of the electromagnetic separation of uranium-235, an important project. Arthur Compton was needed in Chicago. The simplest solution was to let Oppenheimer continue, but many objected to him. However, since no-one was able to come up with a better proposal, Groves decided to settle on Oppenheimer.

After Oppenheimer was appointed as head of Project Y, it was a matter of finding a suitable place for the activities. Groves summarised the requirements that he thought ought to be set:

> We needed good transportation, by air and rail, adequate water, a reasonable availability of labor, a temperate climate, to permit year-round construction and out-of-doors experimental work, and all the other things that make for an efficient operation. As before, we sought an isolated area so that near-by communities would not be adversely affected by any unforeseen results from our activities. Yet this installation would be different, because here we were faced with the necessity of importing a group of highly talented specialists, some of whom would be prima donnas, and of keeping them satisfied with their working and living conditions. In view of our requirements, we concentrated our search on the southwestern part of the United States.

The final choice was Los Alamos in New Mexico. The following was said in the official Smyth report about the bomb project:

> By November 1942 a site had been chosen at Los Alamos, New Mexico. It was located on a *mesa* [table mountain] about 30 miles from Santa Fe. One asset of this site was the availability of considerable area for proving grounds, but initially the only structures on the site consisted of a handful of buildings which once constituted a small boarding school. There was no laboratory, no library, no shop, no adequate power plant. The sole means of approach was a winding mountain road.

Oppenheimer was already at the site in March 1943 but the army's security service, over which Groves did not as yet have full control, was unwilling to approve him. In the end, the impatient Groves grew tired of it. On 20 July 1943, he sent Colonel Marshall the following instruction: 'In accordance with my verbal directions of July 15, it is desired that clearance be issued for the employment of Julius Oppenheimer without delay, irrespective of the information which you have concerning Mr. Oppenheimer. He is absolutely essential to the project'.

As soon as Oppenheimer had arrived in Los Alamos, frantic activities began. Scientists or groups of scientists from well-known universities such as Princeton, the University of Chicago, the University of California, the University of Wisconsin, the University of Minnesota, etc. came to Los Alamos.

Oppenheimer gathered around him an 'extraordinary galaxy of scientific stars' (as it was expressed in the Smyth report). The University of California undertook to run the plant.

The task of leading the department of theoretical physics fell to Hans Bethe who was at Cornell University at the time. *R.R. ('Bob') Wilson* was responsible for experimental nuclear physics, J.W. Kennedy and *C.S. Smith* for chemistry and metallurgy, Kistiakowsky for the explosives department, *R.F. Bacher* for bomb physics, and Fermi for the development department. Samuel Allison assisted Oppenheimer with the coordination of the work. Niels Bohr would spend a lot of time at Los Alamos, as did Chadwick as head of the British cooperation delegation. Many other prominent physicists were there within the different departments or as consultants.

General Groves thought that the technical bomb section needed a military boss and sought advice in Washington DC. Vannevar Bush had a proposal and wondered whether Groves had anything against the idea of a naval officer, which he did not. Once both Groves and Oppenheimer had convinced themselves that the candidate, *Commander William Parsons* (1901-1953), was probably suitable for the job, he was appointed. Parsons was 43 years old and a man who was said to have spent the whole of his life disputing idiotic rules and conservatism within the Navy.

There was a funny side to Parsons' arrival in Los Alamos in spring 1943. Los Alamos was well guarded, particularly at the main entrance a few kilometres further down from the actual plant. The guards were army volunteers who had not had much experience of the Navy's uniforms. They had been instructed to be particularly suspicious of people in uniforms that were different in some way from the normal ones. Parsons was the first naval officer to come to Los Alamos and he arrived dressed in the Navy's summer uniform. He was stopped by the guard who rang his sergeant and urgently reported: 'Sergeant, we've really caught a spy! A guy is down here trying to get in, and his uniform is as phony as a three-dollar bill. He's wearing the eagles of a colonel, and claims that he's a captain!'*

While the search was on in Los Alamos to find the best bomb construction, the frantic work elsewhere was construct production plants for the fissile materials without which there could be no bomb. The experimental facility that had initially been intended for construction in the Forest of Argonne was not completed. The risks with the plutonium were feared to be too great to dare to do the experiments near Chicago. It therefore became important early on to find another place for the experimental facility.

Already in July 1942, before Groves was given the responsibility for the atomic bomb project, he had been asked by Colonel Marshall to look for a suitable place for all production plants, i.e., plutonium-producing reactors and plants to separate uranium-235. The area that was found to be the most suitable lay close to Knoxville in Tennessee, right by a small community called Clinton to be precise. Initially, therefore, the plants that were built were called the Clinton Engineering Works. As the construction operations intensified, a new community also grew up as a living area for those who worked with and within the Clinton Engineering Works. Schools and hospitals were built there and there were some shops of course. This community was named Oak Ridge.

The name Oak Ridge was also used as a postal address for the Clinton Engineering Works to avoid arousing unwanted curiosity. From 1947, when the newly-formed American Atomic Energy Commission became responsible for the plants, the activities were reorganised to become the Oak Ridge National Laboratory, although the shorter name of Oak Ridge had been used prior to this in everyday language rather than the official but much longer Clinton Engineering Works. I will continue to do the same in many places as I continue writing.

Clinton Engineering Works had more than around 22 000 hectares of land (approximately 44 000 acres) at its disposal. The location offered many benefits. The road link with Knoxville, where there was

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* The American rank of "captain" in the Navy corresponds somewhat surprisingly to Lieutenant Commander within other parts of the army and is the equivalent of a commander in the Swedish Navy.

an airport, was excellent and there were acceptable rail links. Tennessee Valley Authority* was willing to deliver the large quantities of electrical energy that were believed to be needed.

Oak Ridge was also the place where they decided to build the experimental facility for the production and purification of plutonium (reprocessing) rather than in the Forest of Argonne by Chicago since the processes for the extraction of plutonium might release the harmful radioactive substances and constitute a hazard to both personnel and surroundings. However, the University of Chicago reluctantly agreed to run the plutonium plant on condition that it received technical support from Du Pont.

The purpose of building an experimental reactor in Oak Ridge early on was primarily to produce sufficient quantities of plutonium for the scientists in Los Alamos to be able to study the properties of the new element before the final production got underway.

The experimental reactor that was built in Oak Ridge came to be called 'X10' since a code-name for the place for the Clinton Engineering Works was site X. The reactor had graphite as a moderator and was cooled with air. It was cube-shaped and approximately seven metres wide. 1 248 channels were drilled into the graphite through which aluminium-encapsulated uranium spheres could be pushed and through which large fans also blew cold air. The channels went right through the two-metre-thick heavy concrete radiation screens surrounding the reactor. They opened out over a sunken pool of water in which the irradiated spheres could be stored until the shortest-lived fission products had disintegrated. The spheres were then transferred to a remotely-controlled reprocessing plant where chemists could separate the plutonium using processes that Glenn Seaborg and his colleagues had worked out on a microscopic scale in Chicago.

In summer 1944, it was plutonium from the X-10 plant which gave the bomb designers in Los Alamos the first possibility of studying the physical properties of the substance more closely.

Du Pont refused to accept any profit whatsoever from its undertakings in either Oak Ridge or later when the final production reactors would come into the question, and nor did the company want any patent rights.†

In autumn 1942, a decision had been made to limit the number of methods for the separation of uranium-235 at Oak Ridge to two processes: electromagnetic separation and gas diffusion. The plant for electromagnetic separation was given the code-name Y-12, the building work started in February 1943 and the plant was ready for use from November 1943. This was the plant which produced the uranium-235 that was used for the Hiroshima bomb. a prototype had been built in Berkeley but there was never time for it provide any form of guidance. Everything had to be built directly at Oak Ridge. Stone & Webster were responsible for the actual construction but Eastman-Kodak were chosen to operate the plant with its subsidiary, Tennessee-Eastman. The plant was erected on a 330-hectare area approximately 8 km south-east of the Oak Ridge community.

The electromagnetic separation was carried out on the basis of the research that had taken place under the leadership of Ernest Lawrence at the University of California. Just three companies were thought to be big enough to cope with the installation. All three were engaged in order to avoid the possibility of overload. General Electric was responsible for the power supply, Allis-Chalmers for the magnets and Westinghouse for the separation chambers. The same principle was used for these as for the mass spectrometers. Each unit consisted of 96 fourteen-tonne steel vacuum chambers standing on their edges and separated by 48 magnets (20 feet x 20 feet x 2 feet). The whole installation formed an oval that the

* Tennessee Valley Authority (TVA) is a federal organisation that was formed in 1933 as part of President Roosevelt's New Deal policy. The intention was to improve the social and economic conditions in the poor area around the Tennessee River. An important part of the project was the construction of dams and power plants. Access to cheap electrical energy attracted energy-intensive industries to the area.

† When Japan capitulated, Du Pont received a symbolic fee of 1 dollar. However, the contract period had not yet expired so the government's auditors demanded that the company pay back 33 cents! General Groves writes: 'Fortunately, the officers of Du Pont had retained their sense of humor throughout their many years of transactions with the government, and were able to derive considerable amusement from this ruling'.

technologists called a race track.* Every such race track was calculated to produce 5 grammes of enriched uranium-235 per day. Since the need for uranium-235 was estimated as 40 kg per bomb, 10 race tracks were built. The first plants were called ‘alpha tracks’. It was still not certain how long these could continue the enrichment for. If this was not enough, additional plants would need to be built. These reserve plants began to be built just in case and were called ‘beta tracks’.

It had already become clear during summer 1942 that the magnetic windings would need more copper than could be obtained. The decision was to use silver instead. Colonel Marshall contacted the Secretary of State in the Ministry of Finance, Daniel Bell, and asked to gain access to tonnes of silver. Bell’s response was: ‘Colonel, in the Treasury we do not speak of *tonnes* of silver; our unit is the Troy Ounce.† However, Marshall was able to borrow 47 000 tonnes (!) of silver with a promise of a further 39 000 tonnes. After the war, all of the silver was returned except for 0.035%, a seemingly negligible percentage but still a considerable quantity – tens of tonnes.

The Clinton plant was colossal, requiring 24 000 people to operate it. Each and every one of the 96 vacuum chambers in one racetrack drew more electric current than one large radio station. In spite of the size of the plant, only eight accidental deaths occurred until December 1946 (five from electric current, one from gas, one from fire and one from a fall). The total cost of the plant amounted to more than 300 million dollars and the operating cost up to and including 1946 was more than 200 million dollars. The electricity bill alone was close to 10 million dollars.

Early on, it appeared that insufficient accuracy when producing the magnets could cause short circuiting. They were forced to allow Allis Chalmers to rewind the magnets, which delayed the operation. Lack of experience with such colossal magnets also created problems. The powerful magnetic fields displaced the fourteen-tonne steel vacuum chambers almost a decimetre before this was prevented by welding the chambers in place.

The alternative separation method of gas diffusion had been studied at Columbia University under what would later be called the SAM laboratory under the management of Harold Urey (the name came from Substitute Alloy Materials or Special Alloy Materials). This was the project in which John Dunning and Eugene Booth attempted to separate microscopic quantities of uranium-235 in November 1941 using a filter membrane. In December 1942, the OSRD’s S-1 committee had placed the gas diffusion method third in order of priority after electromagnetic separation and plutonium production.

However, one single membrane was not enough; thousands were needed in a consecutive series with pumps between them. Since uranium hexafluoride, the only gas that could be used, was so corrosive, the membrane had to be made of nickel. Making the tubes of nickel would require more nickel than was available, so they had to make do with nickel-plated tubes.

Uranium hexafluoride also attacks organic material, so there could not be any oil in the system and the pumps had to be lubricated in another way. A special fluoroethylene plastic was developed for this, which was later marketed by Du Pont under the brand name Teflon.

The major problem was obtaining membranes with small enough pores. In December 1942, M.W. Kellogg Company requested the development of methods for a gas diffusion plant. Kellogg formed a special subsidiary, Kellex, for this and also cooperated with scientists at Columbia University. The responsibility for running the plant, which was called ‘K-25’, was given to Union Carbide and Carbon. The construction of the plant began in Oak Ridge for a cost of a couple of hundred million dollars before any useable membrane became available - a big gamble by Groves.

The first successful separation, the one brought about by the Columbia scientists in January 1942, had been executed using a membrane the size of a silver dollar, but this membrane was not effective enough for the final plant. In autumn 1943, Groves had a choice to make: either continue constructing a factory to produce ineffective nickel membranes as per the age-old idea from the Columbia group, or

* Another name for such a “race track” was calutron, derived from *Cal*ifornia *U*niversity Cyclo*tron* since the principle had originally been stated by Ernest Lawrence.

† The usual English and American weight measurement ounce (oz) is 1/16 of a pound (lb), i.e., 28.35 grammes. The measurement *Troy ounce* is found in an ancient coin-weight system and is also used for jewels and noble metals. This unit is equal to 31.10 grammes.

change the factory to produce a new membrane that Kellex had just experimented and come up with. Urey thought that the latter alternative would delay the gas diffusion option so much that it would not affect the outcome of the war. Better then to put aside the gas diffusion alternative since it would take up resources that could otherwise be used to speed up the other methods. But Groves decided in any case to concentrate on the Kellex membrane. It was obvious he thought that the production of uranium-235 for atomic bombs would be important after the war as well.

In June 1944, Oppenheimer, who had now found out about Abelson's work for the Navy, suggested that thermal diffusion could be used as an initial step before the gas diffusion procedure. Groves grabbed the idea. At the time, the Navy was already building a plant through Abelson at its yard in Philadelphia where the required vapour sources were available. Groves engaged the company H.K. Ferguson to build exact copies of the plant in Philadelphia in Oak Ridge but in greater numbers. Vapour for heating was available from power plants that had been built to produce electricity. The plant in Oak Ridge was given the code-name S-50 and ended up consisting of 2 142 separation columns, each with a vertical, 15-metre-high nickel tube surrounded by a copper tube of equal length, cooled on the outside by water contained within a four-inch galvanised iron tube. Hot water vapour was conveyed at high pressure through the inner nickel tube. The columns were grouped in units of 102 to form large bundles. These bundles were arranged in three groups of seven bundles each. Every column was based on the Clusius-Dickel principle but Abelson had found that the device had to be fed with fluid rather than gas containing uranium. Some more of the lighter molecules rose up to the warm central tube and some more of the heavier ones dropped down to the cold exterior tube. The weakly enriched uranium became important as a basic material for the other separation plants.

So, a number of completely different plants were built at Oak Ridge: X-10 for the experimental plutonium production, Y-12 for the electromagnetic enrichment of uranium-235, K-25 for the enrichment of uranium by means of gas diffusion, and S-50 for weakly-enriched uranium for use in Y-12 and K-25. The coordination of all this work required much reflection and planning and was taken care of by Lieutenant Colonel Nichols.

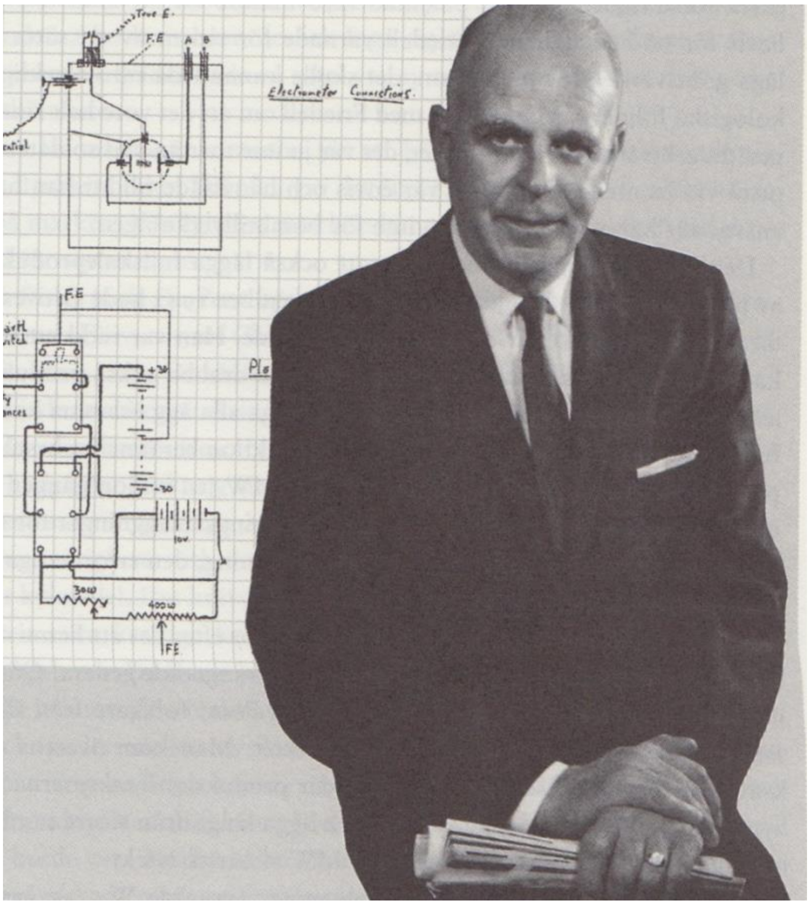
The radiation protection was already assessed as being very important right from the start. Du Pont who, together with the University of Chicago was responsible for running the X-10 plant for plutonium production, sent his own doctors to Chicago early on to be trained by Robert Stone. When the operations in Oak Ridge began in summer 1943, Simeon Cantril was the first of the key people from Chicago to move to Oak Ridge, where he became head of the medical department. He took Herbert Parker with him, who founded the first section for health physics, and Parker's group included the majority of the radiation protection people from Chicago, including *Karl Ziegler Morgan* (1907-1999). Morgan would later be called the father of health physics, but Herbert Parker probably ought to be seen as the actual father. Morgan on the other hand would subsequently realise the formation of both the American Health Physics Society and the International Radiation Protection Association (IRPA).

Parker efficiently organised the radiation protection in Oak Ridge. He was not happy with the name of Health Physics since, with his background as a medical physicist in England, he thought that the actual discipline was radiophysics, i.e., radiation physics. Radiophysics had applications within medical radiophysics and radiation protection but the borders were not particularly important since the medical radiophysicists also had to pursue radiation protection. Radiation protection also covered more than radiation physics. In Parker's view, what was called Health Physics was not a separate discipline and ought therefore to be closely linked to the main discipline of radiophysics. Parker's view fitted in well with what was often stated by Rolf Sievert.

As the army gradually took over the responsibility for the different activities within the Manhattan Project and OSRD-controlled research was replaced by industrial operations, the position of the Met. Lab's health department became less and less clear. In April 1943, Groves, Marshall, Compton and Stone met to clarify the distribution of the responsibility between the army and the University of Chicago for Met. Lab's project. It was agreed that Stone's health department would have the full responsibility for health and safety at Oak Ridge for those who worked there and for the general public.

When Kenneth Nichols succeeded Colonel Marshall as 'District Engineer' in August 1943, he moved the headquarters from Manhattan to Oak Ridge. He also established a medical bureau there for which he appointed the doctor named *Stafford Warren*[*] (1896-1981) as head. Warren had graduated from the medical faculty at the University of California in 1922 and had been Professor of Radiology at the University of Rochester since 1933.

In November 1943, Warren was appointed Colonel of the Medical Corps and assisted by the younger radiologist *Hymer Friedell* (born in 1911). Conflicts soon arose between Warren and Stone. The discord primarily concerned the pursuit of biological research which could not be expected to bring results of immediate significance. Stone thought that Warren was getting involved in things that were nothing to do with him and maintained his right to make his own decisions. The district of Manhattan could provide help and advice but, according to the contract between the army and the University of Chicago, had no right to control the protection inputs for which Stone was responsible.



Herbert M. Parker

Warren thought that normal industrial healthcare was adequate and that extra caution was not called for unless there was clear proof that it was needed. Friedell warned them about the risk of setting the limit values for plutonium too low as it could lead to 'untoward psychological effects'. Many agreed with Friedell that it would be fully reasonable to expose the workers to extra risks since that was no more than was expected of the soldiers at the front line. But Stone was stubborn and referred to the fact that he was responsible for health and safety, not for the production of the bomb.

The original intention was also to locate the full-scale production of plutonium in Oak Ridge, but in November 1942 Groves had already decided to look for a more suitable place. He was afraid that an

[*] Not to be confused with the better-known *Dr. Shields Warren* (1898-1980), who also played an important role, primarily later as American representative in the UN's scientific radiation committee (UNSCEAR). There is great risk of confusion since both are often referred to as "Dr S. Warren".

accident with the production reactors could also affect the production of uranium-235; it was a question of not putting all his eggs in one basket. The full-scale plant for the production of plutonium was estimated to consist of six production reactors, each of 200-250 MW (one million times the output of Fermi's reactor) and three reprocessing plants to separate the plutonium. The need was almost a guess; the required quantity of plutonium was not yet known.

On 14 December 1942, less than two weeks after Fermi demonstrated the possibility of getting a reactor to become critical, General Groves arranged a meeting in Wilmington with representatives of Du Pont, scientists from Chicago and army officers from the Manhattan district. The requirements that must be set for the area in which the production reactors could be built were agreed. The area had to be isolated and be a long way away from the main community, but access was needed to approximately 100 MW electrical power.

The most suitable area, code-named 'area W', was thought to be next to the Columbia River in Washington State around the small community of Hanford. It was named Hanford Engineer Works and lay just north of the point where the river crosses the border to Oregon. The Grand Coulee dam with one of the world's largest power plants lay approximately 250 kilometres upstream.

The area was not used very much for agriculture because the climate was so dry that it had to be artificially irrigated, which was something that only a few farmers could afford. In February 1943, the Manhattan district bought an extensive area of land. Everyone who lived in the central area was moved from there and the area was completely shut off. The outer areas were bought so that the interior area could remain difficult to access. No-one was allowed to live within the nearest zone but land could be leased for agriculture. The farmers were allowed to live in the outlying zone but had to follow specific restrictions. The land that was requisitioned was a total of approximately 200 000 hectares, i.e., 2 000 square kilometres.

Groves let the landowners stay to reap their harvest. This appeared to be an expensive decision. The harvest that year was exceptionally good, which raised the price of the land. Leniency towards the farmers therefore turned into a considerable additional expense for the State.

At Oak Ridge, Du Pont had built the experimental plant for plutonium production but the University of Chicago was responsible for its operation. At Hanford, Du Pont would be responsible for the erection and the operation. When Du Pont's engineers began the construction of the reactors, they were called 'reactors' for the first time rather than 'piles' as they had previously been called.

The initial intention had been to cool the reactors with helium since this substance had absolutely no capacity to capture neutrons, but great practical difficulties were encountered and it was very quickly understood that helium was not a suitable coolant. Enormous steel tanks were required to keep the gas contained, plus powerful compressors to lead it through the reactor at high pressure. Du Pont's designers doubted that the helium-cooled reactors could be built as quickly as General Groves required.

They consulted the nuclear physicists to find out whether there was any other possibility. Eugene Wigner thought the experiment with Fermi's reactor had shown that the multiplication factor had been greater than that calculated. It indicated that it ought to be possible to use clean water as a coolant in a reactor in which the moderator was graphite. The reactors could then be built in a similar way, although much larger, to the experimental reactor at Oak Ridge. The major difference would be that instead of air, substantial quantities of water would be pumped through the channels that pierced the graphite. The purchase of land located by the Columbia River had been visionary. The river water appeared to be so clean that no special treatment plant was needed, although such a plant was actually built erring on the side of caution.

Each production reactor would contain 200 tonnes of uranium in the form of aluminium-encapsulated cylindrical spheres, positioned in the more than one thousand channels that pierced the 1200 tonnes of graphite. Almost 300 cubic metres of water per minute were pumped through the channels to divert the heat generated through a thermal output of 250 MW, around one tenth of the heat output of one of the big reactors at the nuclear power stations in Forsmark or Ringhals.

The production reactors were supplied with triple control systems: control rods that could be pushed into the reactor core from the side, control rods that were suspended above the core and could be released immediately if necessary, and finally a device to drown the core in a fluid with neutron-absorbing substances if necessary. In addition to the plutonium-producing reactors, a material testing reactor was built which had such a low output that cooling it was not a problem.

The spheres of fuel from the production reactors were transferred after a while for use in the reprocessing plants for separation of the plutonium that had been formed. Three such plants were built, two of which would be used and one of which would be held in reserve. The uranium spheres were held under water and transported by specially-constructed railway wagons to a special storage site. They were stored there until the radioactive fission products had disintegrated sufficiently to enable the chemical separation of the plutonium. The two buildings in which this took place were 250 m long, 20 m wide and 25 m tall - such impressive dimensions that the workers called each building Queen Mary after the great ocean liner (which was significantly smaller, mind you). The buildings were massive concrete structures with small cells for the chemical processes. As protection against the intensive gamma radiation, the cells were surrounded by concrete walls that were two metres thick. Each ‘Queen Mary’ had forty cells that were available from above when a 35-tonne concrete cover was lifted by a crane that could be rolled along the building.

When the uranium spheres arrived at the separation plant, the first one was dissolved in hot nitric acid. The separation process that followed was principally the same as the one developed on a microscopic scale by Glenn Seaborg and his colleagues but which had now been industrially escalated to a scale of $1{:}10^{10}$. When the concrete cells had presumably been contaminated with any radioactive material, maintenance personnel could no longer enter them. All maintenance work therefore had to take place through remote control.

The coast of Washington State is the rainiest in the United States and Seattle is known as a windy and rainy place. It is therefore difficult to imagine that the interior of Washington is almost desert-like in places. The midsection of the Columbia River lies in the rain shadow of the 3 000-4 000-metre-high Cascade Range with their snow-clad tops (the highest, Mount Rainier, reaches a height of 4 392 m). Where Hanford is located between Rattlesnake Hills and Saddle Mountains, there is a dry, desolate high plateau, sagebrush country with dry plains and infertile hills.* Just south of the blocked-off area lay the small town of Richland, which was expanded for those who came to the area when the Hanford plant was erected. Slightly further down from the river, just before the big Snake River starts to flow from Idaho, lie the older communities of Kennewick and Pasco. Higher up along Snake River to the border with Idaho is where the potato fields become common and are able to survive well thanks to irrigation from the river.

Many of the workers who came to Hanford were disappointed when they encountered desolate natural surroundings instead of the famous green forests of Washington State. It was no longer easy to find a workforce, the war having put an end to unemployment. The construction workers were also a tough crowd. ‘There was nothing to do after work except fight, with the result that occasionally bodies were found in garbage cans the next morning’ wrote one of the employees. Rhodes says that Du Pont built the bars with windows that were easy to open from outside to throw in tear gas capsules. Du Pont erected more than 200 barracks to house the building workers, of whom there were approximately 40 000 when the work culminated. At the time, the work camp constituted a town of 60 000 inhabitants, the fourth largest town in Washington State. When the building was finished, the camp was almost completely abandoned since the personnel operating the reactors were accommodated in Richland.

One important requirement was not to damage the salmon in the Columbia River. The Columbia River, with its length of almost 2000 km, an important source of clean water; it is a healthy and lively river whose many waterfalls gave the Cascade Range their name. When the construction of the temporary living quarters started in April 1943 (the erection of the actual plant did not start until June), Groves and his colleagues were anxious about the consequences of using the river water to cool the reactors. There would be very big cooling water flow and the consequences of a temperature increase in

* Sagebrush in the west of America is a general name for brushy vegetation but, to be more precise, is a bush from the sage family, *Artemisia tridentata*, which in these parts characterises the bushy steppes. It is an interesting coincidence that another plant of the same family, *Artemisia vulgaris*, appears to be called ‘chernobyl’ in Russian.

the river for the salmon stocks was not known. Nor could the possibility of a radioactive substances leak be precluded; what sort of consequences would such a leak have?

Groves discussed the problem with Stafford Warren, who then discussed it in Chicago with a group consisting of himself and Friedell and a number of Met. Lab. representatives, including Arthur Compton, Robert Stone and Eugene Wigner who now led the group constructing the reactors. It was agreed that the problem was important but they saw no immediate solutions. Warren and Stone then looked for a biologist who could devote himself to the matter. They found a suitable candidate in the 40-year-old *Lauren Donaldson* who had previously been a fish conservation consultant for the Ministry of the Interior with regard to the Grand Coulee Dam.

In August 1943, Donaldson was summoned to Washington D.C. for a meeting at the OSRD's headquarters where he met Warren who was deliberately incorrectly introduced as consultant to the OSRD. Warren asked Donaldson if he was willing to carry out experiments on the impact of radioactive substances on fish. Warren could not disclose why these experiments were very important but said that they concerned the Columbia River. However, this had to remain secret and the experiments therefore had to be carried out in Seattle within the University of Washington. 'Investigation of the use of X-rays in the treatment of fungoid infections in salmonid fishes' was chosen as a code-name that did not reveal the project.

The university signed a contract with the OSRD to establish a special laboratory for Donaldson without either the latter or the university management knowing that research was ongoing in the Manhattan Project. Donaldson himself did not discover the purpose of the experiments until one year later. The new laboratory in Seattle was given the vague enough name of the Applied Fisheries Laboratory, a name which ought really to have aroused curiosity through its very vagueness. Donaldson was initially given three colleagues and the group soon proved to be efficient.

In summer 1944, Herbert Parker came from Oak Ridge to Hanford when Arthur Compton, in consultation with Robert Stone, had proposed to Du Pont that the anticipated plutonium problems required a radiation protection expert with Parker's competence. At Oak Ridge, Parker left behind a well-organised Health Physics section with procedures for developing instruments and taking radiation measurements. Parker was succeeded there by *Karl Morgan*, who had already been part of his group since the Chicago period.

Parker's good friend Simeon Cantril also left Oak Ridge to take up a position with Du Pont as head of company healthcare in Hanford. He was succeeded by John Wirth, who happened to be the very radiologist that Cantril had succeeded in 1938 as head of the cancer clinic at the Swedish Hospital in Seattle. Wirth's medical physicist, John Rose, had then accompanied Wirth from Seattle and had also accompanied him to Oak Ridge where Rose had been in Parker's group. Wirth and Rose had both hoped to be summoned to Hanford. but when they were not, Rose left Oak Ridge to later become head of radiation protection at the Argonne Laboratory in Chicago.

Parker's new employer, the Du Pont group, took an unusual interest in the safety of its personnel, even when it conflicted with costs and risks of losing time. This aroused criticism. According to Rhodes, the critics also included Leo Szilard, who was irritated about Du Pont's 'irrational [and] exaggerated consideration for worker protection'. However, Parker's comment was that Du Pont's motto of 'safety first' was not just empty words. And nor indeed was it for the doctors and physicists who were responsible for the radiation protection under Met. Lab. After the war, Robert Stone was able to ascertain that none of the employees had died as a consequence of external radiation or the radioactive substances while working to produce the first atomic bombs – which was something of a contrast to the purpose of the bomb!

Up to now, quantitative measurements of radiation doses had been based primarily on the capacity of radiation to ionise air and thus make it electrically conductive. The radiation dose unit adopted by the International Unit Commission (which was later called the ICRU) in Stockholm in 1928 was therefore based on the ionisation of air. It was redefined in 1937 to that quantity of x or gamma radiation such that the associated corpuscular emission per 0.001293 gram of air produces, in air, ions carrying one electrostatic unit of quantity of electricity of either sign. It is to be noted that 0.001293 g is the mass of 1 cm3 of dry atmospheric air at 0° C and 760 mm mercury pressure.

The unit was called the ‘röntgen’ and was designated by ‘r’. The quantity it concerned was not precisely described - it was simply called a ‘quantity’ of radiation. Sometimes it was called ‘dose’ and later ‘exposure’, but these names did not describe what was referred to by ‘quantity’. However, it was quite clear that it was intended to be what an irradiated body was exposed to rather than what the radiation caused in the body.* You could talk about a radiation exposure in vacuum and by so doing mean a ‘quantity of radiation’ that it would have caused the corresponding ionisation in a hypothetical volume of air had such been provided.

It was practical to use the quantity of ‘exposure’ to describe the capacity of incident radiation to ionise matter in its path. This was dissatisfactory, however, when someone wanted to describe the way in which a ‘radiation dose’ varied within an irradiated body, as was the case with radiation treatment. And it was completely dissatisfactory when it came to stating the ‘radiation dose’ from the radioactive substances inside a body. Another quantity was needed for these cases.†

In 1931, Rolf Sievert had proposed a unit to state the radiation intensity from a radium preparation and called it the Imc (intensity millicurie). On that basis, an ‘Imc-hour’ could designate the radiation dose from the gamma radiation r cm from a spot-shaped radium preparation with the activity r millicurie (i.e., r milligrammes of radium) after an hour of irradiation. This unit was called sometimes a ‘sievert dose’. Using Eve’s number‡ (4.85 $^{10-9}$, dimension: see footnote), it is possible to calculate that the exposure rate (exposure per time) at a distance of r centimetres from r milligrammes of radium is 8.4 röntgen per hour. A Sievert dose, i.e., r Imc-hour, was thus equal to 8.4 r.

However, stating the exposure, i.e., the ionisation, in air was not sufficient. People also wanted to know what took place physically in the irradiated tissue. This was where the Bragg-Gray principle was found to be very useful. It gave an explanation that could be used quantitatively to show that a ‘wall effect’ was obtained in small ion chambers. Ionisation in a small cavity in an irradiated substance is not determined by properties of the gas, e.g., air, which fills the cavity but by properties of the surrounding substance. An irradiation with x rays or gamma radiation knocks electrons from the atoms (see p. 144f). These electrons in turn cause ionisations. The ionisation that takes place in a small cavity is caused almost solely by electrons that have been knocked out of the surrounding medium and their number thus depends on the properties of the medium and not the gas in the cavity.

The quantitative connection between the radiant energy that is absorbed per unit volume in an irradiated substance and the ionisation that can be measured per unit volume in a cavity in the substance was stated in 1929 the British biophysicist *Louis Harald Gray* (1905-1965) but, since it was later pointed out that the same principle had actually already been mentioned in 1921 by *Sir William Bragg* (1862-1942), both names are usually used for the principle.

The Bragg-Gray principle made it possible to construct small ion chambers, known as ‘thimble chambers’, with such a choice of wall material that it either measured what the ionisation would have become in air, i.e., the exposure stated in röntgen, or the ionisation in a biological tissue.

The development within nuclear physics led to the necessity for dose estimates for types of radiation other than x-ray and gamma radiation, e.g., neutrons and energy-rich heavier particles from accelerators. People then began to depart from the ionisation measurements and thought that the energy that was absorbed per mass unit in different parts of an irradiated body was a better quantity for stating doses. They started calling this quantity the absorbed dose, and that is what it is still known as today.

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* The ICRU report “Radiation Quantities and Units” (report 10a, 1962) gives the first interpretation of the “quantity of radiation” concept, i.e., energy flux density (joule per square metre).

† A description of early dose quantities can be found in Pandora’s Box, Chapters 13 and 14.

‡ The number of ion pairs per second in air of normal temperature and pressure, caused by the electrons produced in air, per cubic centimetre, at a distance of 1 centimetre from a spot-shaped radium preparation of 1 milligramme with the radium in equilibrium with its daughter products, calculated by *A.S. Eve*.

The first step towards an absorbed dose was to state 'röntgen equivalents'. The energy that is given off per gramme in an irradiated tissue at an exposure of 1 röntgen is between 83 and 85 erg* per gramme, depending on which energy (W) is used per ion pair formed (between 32 and 34 electron volts per ion pair). For water and soft tissue, it was estimated that 1 röntgen would correspond to between 93 and 95 erg per gramme. When different authors stated absorbed doses in 'röntgen equivalents', they meant the different values depending on the proposed doses in air or in tissue.

Herbert Parker introduced the unit 'rep' as an acronym for 'roentgen equivalent physical'. He initially attributed it to air and then proposed 83 erg per gramme, but soon changed the value to 93 erg per gramme to apply to soft tissue.

Introducing a quantity of energy was a step in the right direction, but what was actually needed in a radiation protection context was a quantity that could also be seen as a measurement of the likelihood of biological damage. One and the same absorbed dose could be expected to lead to completely different risks of damage, depending on the type of radiation. For example, it was known that alpha radiation was at least one factor of ten times as effective as gamma radiation. What was needed was a dose quantity that reflected the biological risk. Parker therefore introduced a new dose quantity, the biologically effective dose, for which he called the unit 'reb', an acronym of 'roentgen equivalent biological'. The biologically effective dose stated in reb was equal to the 'physical dose' stated in rep, multiplied by a weighting factor which indicated the biological effectiveness of the radiation in relation to a comparison radiation. The weighting factor was what is now called the RBE (relative biological effectiveness).†

The 'reb' was not long-lived. When Parker was to give an account of his new quantities and units at the start of the 1940s, he went down with a cold. His audience could not hear any difference when he said 'rep' and 'reb'. Learning from the experience, Parker decided to rename the latter unit 'rem', which he formed as an acronym of 'roentgen equivalent man' (or 'roentgen equivalent mammal' with reference to mammals). Along with the 'röntgen', the 'rep' and the 'rem' were the ones that were used in the Manhattan Project. Not until after the war, at the 7th international radiology congress in Copenhagen in 1953, did the ICRU decide to formally introduce the quantity absorbed dose and decide that the unit would be called the 'rad' and be equal to 100 erg per gramme, i.e., 0.01 joule per kilogramme. Parker's 'rep' thus almost survived and the erg number was changed from 93 to 100 and 'rep' to 'rad'. The current unit for absorbed dose, named after L.H. Gray, is 1 joule per kilogramme, i.e., 1 gray = 100 rad.

The quantity that was stated in 'rem' was later named 'dose equivalent' and is now called 'equivalent dose'. Its current unit is named after Rolf Sievert. The connection between the sievert (Sv) and the gray (Gy) corresponds to the previous connection between the rem and the rad, so 1 sievert is approximately equal to 100 rem (but not exactly since the weighting factors have changed over the years). The way in which an absorbed dose must be weighted to be transformed into an equivalent dose and the way in which the weighting factors have been changed and named over the years is a story to be told later on.

In Hanford, people were aware that neutron radiation could be dangerous but as yet had no experience of damage. The RBE values they were able to estimate for neutrons were based mainly on Stone's experiences of the use of neutrons for radiation treatment. For röntgen radiation, the tolerance dose was 100 milliröntgen per day from 1934 but to be on the safe side, Du Pont applied a lower value in consultation with Robert Stone: 10 milliröntgen per day for x-ray and gamma radiation (i.e., approximately 25 mSv per year; in 1990, the ICRP lowered its recommended dose limit to 20 mSv per year). Stone must have recalled Henshaw's proposal from 1941 for a heavy lowering of the tolerance dose (see Chapter 6). As a responsible employer, the Du Pont company was unique.

When Stone was asked which quantities of plutonium the workers at Oak Ridge and Hanford would be allowed to take into their bodies, his response was: 'the only safe practice is to try to avoid any intake at all.'

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* The energy measurement erg is hardly used nowadays. 1 erg is 0.1 microjoule, i.e., 10^{-7} joule.

† The weighting factor has changed its name over the years. The first to be used was the RBE value, then a nominal RBE value for radiation protection calculations called the "RBE_P" ("p" for "protection"), then a "quality factor" first designated by "*QF*" and later by "*Q*", and finally a weighting factor for radiation (radiation weighting factor, designated by w_R).

One particular problem at Hanford was the large number of workers who had nothing to do with the running of the plants and who had therefore not undergone personnel checks. These workers had no knowledge of the nature of the plants and therefore also no knowledge of the actual risks. Various rumours abounded and many were scared by the imagined risks of a different type altogether from radiation. Cantril and Parker worked out a circular to calm the employees down. Unfortunately, the circular never reached them – it was classified by the military secret service.

The radioactive substances leaked or were released from the plants in quantities that were not thought to be environmentally hazardous but that were far greater than emissions from other plants. The radioactive substances that reached the Columbia River were mainly short-lived activation products that had arisen or been dissolved in the cooling water as it passed through the reactors. The most radioactive of these were copper-64, sodium-24, neptunium-239 and chrome-51, the latter-mentioned having a half-life of 28 days, the other with half-lives of less than 3 days. The emission of fission products from the reactors was very insignificant, and the majority of the fission products that could be detected in the cooling water had been formed through the fission of natural uranium which was already in the river water.

The reprocessing plants for the separation of plutonium produced more waste. The low-level and intermediate-level parts of this waste were discharged to ground 16 kilometres from the river in the hope that the dry sediment would retain the radioactive substances. These discharges subsequently led to considerable criticism when in the 1960s it was shown that some nuclides, primarily tritium and ruthenium-106, had reached down to the river. Ground discharges of waste containing plutonium in quantities corresponding to a critical mass led to widespread concern when the rumour said that a nuclear explosion could occur, although this is something that could be proven to be physically impossible.

During reprocessing, radioactive noble gases, vapours and small particles are also emitted. Of these in Hanford, only the noble gases reached the atmosphere; other radionuclides were retained in filters.

The liquid high-level waste that is the residual product following the reprocessing had been stored in steel-covered, reinforced concrete vessels lowered into the ground. These waste tanks began to leak after a number of years as a consequence of corrosion, cracks or thermal expansion due to overheating. According to what Parker reported in a general article in the *Encyclopedia Britannica* in 1978, there had been a total of twenty leaks since the start of the waste storage, with volumes varying between 5 and 435 cubic metres. Authorities now require high-level waste to be turned into solid form. The waste tanks that were built at a later stage were also made with double steel walls so that measures could be taken were any leak to take place through the inner wall before the outer wall was also penetrated.

In his general article, Parker wrote that 'the leaks at the Hanford plant and the use of its unique dry sediments for waste retention probably did significant damage to its over all waste-management image'.

Herbert Parker made early estimates of the consequences to which a major accident with the reactors or an enemy attack on the waste tanks would lead. He ascertained that the activity of the radioactive substances in one of the Hanford reactors a couple of hours after an accident would exceed the activity of the radioactive substances from an atomic bomb of the size being prepared for. The reactor would contain approximately 6 million curie* of each of the biologically important nuclides such as strontium-89, ruthenium-103 and iodine-131.

The activity of short-lived radioactive fission products in a reactor is proportional to the neutron flow and thereby to the output of the reactor. After three-four half-lives for a given nuclide, its activity has almost reached a saturation value because of an equal number of atomic nuclei disintegrating and being newly formed per time unit. This activity therefore undergoes only an insignificant change with time.

For long-lived nuclides, the activity never reaches the saturation value. If the half-life of the nuclide is much longer than the time the fuel spends in the reactor, its activity increases with time and is

* 1 *curie* (Ci) is the unit for the activity of a radioactive substance, i.e., the number nuclear disintegrations per time unit. The current unit is 1 disintegration per second, also called 1 *becquerel* (Bq). 1 curie = 37 billion becquerel.

proportional to both the output of the reactor and the time the fuel spends in the reactor, i.e., proportional to 'the burnup' of the fuel.*

In a power-producing reactor, the burnup is high and in such a reactor therefore has significant activities of the particularly important nuclides strontium-90 and caesium-137, both of which have a half-life of approximately 30 years. In the Hanford reactors where plutonium-239 was produced, the burnup (for reasons to be shown later) was much lower and the quantities of the two said nuclides in the *reactor* were therefore relatively small. On the other hand, the quantities accumulated in the waste were independent of the burnup and only in proportion to the total operating time of the reactors. As regards the long-lived nuclides, an accident scattering the contents of the stored waste was therefore more of a threat where long-lived environmental contamination was concerned than an accident scattering the contents of a reactor.

On 26 September 1944, the first production reactor in Hanford was ready to be started but it did not become critical until after midnight. They allowed the output to go up to 100 MW, the highest output that any reactor had reached so far. The reactor worked very well for an hour but the output then began to fall which forced an adjustment of the control rods in order to maintain the output. This was of no help at all – the reactor output simply fell and the reactor 'went out'.

The next day, 28th September, the reactor came back to life but then the same thing happened: after a while, the reactor lost its output.

John Wheeler, who was now in Hanford, knew what the error was: he had at one time already been concerned by the possibility that some of the fission products that were formed while the reactor was operating would have just as great a neutron-absorbing effect as cadmium or borium. They would then 'contaminate' the reactor and act as extra control rods that had been pushed in.

Wheeler interpreted the period of time for what had happened as the initial formation of a non-neutron-absorbing fission product, which had then a bit later disintegrated into a neutron-absorbing daughter product with a longer half-life. It would then take a while for this to increase, but when this had occurred it would absorb neutrons to such an extent that the reactor would become subcritical and the reaction would cease. However, if the neutron-absorbing daughter product was in turn radioactive and disintegrated within a day and its daughter product, the third nuclide in the chain, did not absorb neutrons, the reactor would come back to life; and this is exactly what had happened.

Wheeler searched in nuclide tables for a disintegration chain with half-lives that could correspond, and he found that this could be the case for a radioactive isotope of the noble gas xenon, xenon-135. The disintegration chain is started with the fission product tellurium-135, which constitutes more than 5 % of the total mass of the fission products. Tellurium-135 disintegrates very quickly while emitting beta radiation to iodine-135. This in turn disintegrates with a half-life of 6.7 hours to xenon-135, which in turn disintegrates with a half-life of 9.2 hours to the very long-lived nuclide caesium-135. Of these nuclides, only xenon-135 is heavily neutron-absorbing. The disintegration chain can be written as:

$$\text{tellurium-135} \rightarrow \text{iodine-135} \rightarrow \text{xenon-135} \rightarrow \text{caesium-135} \rightarrow \text{barium-135}$$

Despite Wheeler's premonitions, the xenon contamination came as an unpleasant surprise. *Crawford Greenewalt*, Du Pont's head engineer who had already been part of things at the start of Fermi's CP-1 reactor in Chicago, rang Samuel Allison in Chicago for advice. Allison in turn contacted Walter Zinn, who was now in the laboratory in the Forest of Argonne which had been the initially intended site for the construction of CP-1. He was working on a new experimental reactor there called CP-3. It consisted of a large, radiation-protected tank containing 6.5 tonnes of heavy water in which there were 121 aluminium-encapsulated uranium rods. The reactor had not been used for some time but could be run with an output of 300 kW. Zinn now started the reactor at full output and let it run for twelve hours. He was then able to confirm the xenon contamination.

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* 'Burnup' means the level of use of the fuel in a reactor. The burnup is usually stated in megawatt-days per tonne of fuel (MWd/t).

Thanks to Wheeler's suspicions regarding 'contamination' with fission products, it was already possible for them to save the reactors that had already been finished. If Wigner had had his way, the reactors would have been built for exactly the reactivity that was needed. However, Wheeler had convinced Du Pont to take the extra and extremely substantial cost of 504 channels additional to the 1 500 that Wigner had calculated. These channels were now in place when they were needed and saved the situation.

They were finally able to commission the first two production reactors at the end of December 1944, and plutonium started becoming available in required quantities. Groves reported proudly to Army Commander George Marshall that he counted on having 18 five-kilo plutonium bombs ready during the second six months of 1945.

10. ‘I AM BECOME DEATH, THE DESTROYER OF WORLDS’

IN LOS ALAMOS, furious work was underway to find suitable bomb constructions. The principle for an atomic bomb is actually simple: bring together a critical charge of uranium-235 or plutonium so quickly that a chain reaction splits most of the material in an explosion, although the question of ‘How?’ was not easy to answer. A good hundred of the world’s sharpest brains were thinking about this on the mesa where Los Alamos is situated.

The endeavour was surrounded by deep secrecy and protected by a special security organisation. During the first year of the Manhattan Project, the security service was taken care of by the War Department’s counter-espionage personnel under *Major General G. V. Strong* (1880-1946). Strong was in close cooperation with the security police, the Federal Bureau of Investigation (FBI). In April 1943, an agreement was reached by Strong and the FBI that the Manhattan Project would be monitored solely by the army. Later that same year, the counterespionage was reorganised with reduced central control as a consequence. General Groves then created a separate security organisation within the Manhattan Project and appointed *Major John Landsdale Jr.* to lead it. At the end of the war, Landsdale’s personnel for this purpose, called ‘creeps’, were 485 in number.

The security service was not aimed at spies from any country in particular. Groves did not think that leaking information to Japanese or Italian agents could cause any great amount of damage. He thought that Germany was the only country that could benefit from information on the atomic bomb project. However, it was very soon realised that the only essential espionage to occur was for the Soviet Union.*

After a while, the scientists in Los Alamos began to wonder how much uranium-235 they had started to produce in Oak Ridge. With few exceptions, those who worked there had no understanding of the properties of the substance when it came down to a bomb, and the majority of them did not even know that the purpose was to produce a nuclear explosive. Emilio Segrè in Los Alamos thought that someone from there ought to go to Oak Ridge to see how the measurements of the quantity of uranium-235 were handled. The military in Los Alamos initially did not want any scientists to leave the closely monitored plant but eventually agreed, so off went Segrè to Oak Ridge.

As he wandered through the laboratories, he found some men being a little care-free in their way of handling a large demijohn containing a green fluid which turned out to be a uranium nitrate solution. Segrè asked whether they intended to handle the demijohn in the same care-free way when the uranium had been enriched to uranium-235, but the men did not understand.

> ‘Won’t it explode?’ wondered Segrè.
> ‘Huh! *Explode*??’

The episode is described in Nobel Prize winner Richard Feynman’s book *Surely you're joking, Mr. Feynman!*. The military who were responsible at Oak Ridge appeared to be aware of the risk of explosion, but they had worked out that there would never be enough uranium-235 for a critical mass at any one time. What they did not know on the other hand was that the uranium in aqueous solution would be dangerous because released neutrons would be slowed down in the water and increase the risk of nuclear fission. There would not be an explosion but there could be a criticality accident with a sudden burst of neutrons that would be deadly to people nearby.

Segrè’s report led to Oppenheimer starting an investigation in Los Alamos. *Robert Christy* examined the conditions for criticality accidents with aqueous solutions and *Richard Feynman* (1918-1988)

* The most flagrant case originates from the time after the war when the German-born English citizen and nuclear physicist Klaus Fuchs (1911-1988) in 1950 was revealed as a spy for the Soviet Union.

worked out the likelihood of reactions with dry uranium powder. The aim was that Christy would travel to Oak Ridge and inform them of the risks and investigate where criticality accidents could occur. However, Christy went down with pneumonia and the twenty-five-year-old Feynman was ordered to travel. He was asked by Oppenheimer to say that Los Alamos could not be responsible for the safety at Oak Ridge unless the measures that Feynman found necessary were undertaken.

The military who were responsible at Oak Ridge were the first to receive the young Feynman and they had doubts about letting him proceed. The information he wanted to give would spread secret information about the properties of the bomb. Finally, however, they allowed him to fulfil the assignment and Feynman began to tell them about neutrons, that slow neutrons are more effective, that water slows down neutrons and that cadmium absorbs them - knowledge which, would you believe it, was completely new to the personnel at Oak Ridge. The result was an agreement stating that all construction drawings would be reviewed and that necessary changes would be made. Then, in a couple of months' time, Feynman would return and examine the result.

On his return, Feynman was led into a room in which a couple of engineers were waiting with a tall blueprint of construction drawings. The engineers explained that they had now examined all transport routes for the enriched uranium and eliminated all possibilities of sufficient uranium accumulating anywhere for an accident to occur. Feynman was not used to deciphering construction drawings but did not give the game away, which was a mistake since he ought to have admitted his lack of knowledge from the start.

The critical construction elements were cranes and valves, but Feynman was not sure how they were designated on the drawings. He guessed that a certain symbol meant 'valve' but he was not sure. He therefore took the bull by the horns and put his finger on one of these symbols and said: 'What happens if this valve gets stuck?' He waited anxiously to hear: 'But that's not a valve!'

Instead, the engineers looked at each other, and one of them said: 'Well, if *that* valve gets stuck...' They then looked at the drawings, the one following the lead of the other, and then stared in astonishment at the young Feynman. 'You're absolutely right, sir!' And the officer who had followed him wherever he went at Oak Ridge said: 'You're a genius, Mr Feynman!' Something unfortunate *could* have happened had that particular valve become stuck.

Once the availability of plutonium in Los Alamos began, they were anxious about radiation protection. By then it was known that plutonium, like radium, was a 'bone-seeking' substance. Where radium was concerned there was experience of the risks, from the tragic cases of the female luminescent paint workers and from different cases of misuse of radium.* These experiences had been assessed by Robley Evans in the United States and *Boris Rajewsky* (born in 1893) in Germany. As recently as 1941, a work group under the American Advisory Committee, which would later become the NCRP, had recommended that the tolerance quantity for the body burden of radium-226 ought to be 0.1 microgrammes.

Since both plutonium and radium are accumulated in the skeleton and are alpha-emitting, it was obvious to assume that tolerance level for plutonium-239 could be calculated on the basis of the energy emitted with alpha radiation. It was found that radium-226 emitted fifty times the amount of alpha energy per time than the same weight of plutonium-239. It was therefore first assumed that the tolerance level for plutonium-239 was 5 microgrammes – but this assumption would soon be revised.

Oppenheimer had searched early on for someone who could take responsibility for radiation protection in Los Alamos. His choice fell on young doctor *Louis Hempelmann* who had been educated at Washington University in St. Louis. After graduating, Hempelmann had studied biology in Berkeley for a while with Robert Stone and Ernest Lawrence's brother, *John Lawrence* (1904-1991). Unlike the picture in Chicago, Oak Ridge and Hanford, radiation protection was not an independent position in Los Alamos. Instead, Hempelmann worked directly under Oppenheimer.

In February 1944, Oppenheimer sent Hempelmann to Berkeley to find out about the experiments with the irradiation of rats which were carried out by Joseph Hamilton. Oppenheimer wanted to make sure

* See the story about this in Chapter 16 of Pandora's Box.

that these experiments also included plutonium. Hempelmann found that it was – and Hamilton was the first to gain access to plutonium-239, 1 milligramme, for biological experiments. But the results of Hamilton’s experiments were surprising.

The experiments confirmed that plutonium accumulated in the skeleton up to 99 %, but the biological effect of plutonium-239 was much more powerful than the effect of radium-226, so much more powerful that the Manhattan Project lowered the tolerance level from 5 to 1 microgramme. The main explanation was that plutonium is distributed in the bone tissues in a different way from radium. Radium is distributed over the whole of the tissue while plutonium is accumulated close to the surface and therefore gives the most sensitive cells a greater radiation dose.

This was taken into account in the dose calculations, but instead of directly calculating the radiation dose absorbed in the outer layer of the bone tissue, they continued to calculate the average dose in the whole of the bone tissue. When they then came to calculate the ‘röntgen equivalent’, they introduced an extra weighting factor which was designated as ‘N’, or ‘non-uniform distribution factor’. This factor survived until the 1990s and was included in the definition of the dose equivalent (H) for a long time as it was calculated from the absorbed dose (D). In addition to the quality factor (Q) which took into account the different biological efficacies of different types of radiation, the definition also contained the factor N:

$$H = Q \text{ x } N \text{ x } D$$

This was actually a clumsy way of correcting the fact that the radiation dose was calculated for incorrect cells.

However, it soon appeared that plutonium in the skeleton was not as great a problem as had first been feared. Plutonium, unlike radium, is absorbed in only insignificant quantities by the body if it is ingested orally. It is therefore a display of ignorance on the part of journalists (and politicians) when they sometimes call plutonium ‘the most toxic substance in the world’. The essential risk with plutonium is associated with the inhalation of dust containing plutonium and high doses of radiation in the lungs. This is where Herbert Parker did dose calculations and was the first to introduce the concept of the ‘Maximum Permissible Concentration, MPC) and gave an MPC value for plutonium-239 in breathing air.

Initially, no biological radiation research was carried out in Los Alamos. The situation changed following an incident in August 1944 when 10 milligrammes of plutonium squirted up into the face of a chemist, causing the latter to involuntarily swallow an indeterminate quantity. Joseph Kennedy, who was head of the work with chemistry and metallography in Los Alamos, was dismayed when he discovered that the radiation protection people could neither measure the body burden nor predict what the radiation dose in the skeleton would be. Hempelmann was equally disturbed by it and asked Oppenheimer to approve a programme for medical biological research to obtain answers to a number of acute questions. Oppenheimer consented, and the work with finding methods to measure plutonium in urine and faeces and correlate the measurement results with the body burden of plutonium was entrusted to biochemist *Wright Langham* (born in 1911). Langham cooperated with scientists in Berkeley, Chicago and Rochester.

In Chicago, biological research into uranium and plutonium was first carried out at the Michael Reese Hospital and later also at ‘Site B’, an old brewery that was equipped for laboratory work. In Rochester, the Manhattan Project used parts of the university’s medical institute (the University of Rochester Medical School). In principle, Langham had solved the problem with measuring the skeletal content of plutonium in January 1945, but the methods were time-consuming and long-winded and were not put to routine use until the end of 1945. However, no practically viable method had been found to measure plutonium in the lungs.

The work in Los Alamos was not without friction; cooperation problems arose in several places despite Oppenheimer’s efforts to avoid such occurrences. One of the conflicts concerned Hans Bethe and Edward Teller. Although Teller had been there in Los Alamos from the very start, Oppenheimer appointed Bethe as head of the theoretical department. According to Rhodes, Bethe later wrote:

> That I was named to head the [Theoretical] division was a severe blow to Teller, who had worked on the bomb project almost from the day of its inception and considered

himself, quite rightly, as having seniority over everyone then at Los Alamos, including Oppenheimer.

But Teller did not want to criticise Oppenheimer. Instead, he expressed his great esteem for Oppenheimer as a person (quoted from Rhodes):

> Throughout the war years, Oppie knew in detail what was going on in every part of the Laboratory. He was incredibly quick and perceptive in analyzing human as well as technical problems. Of the more than ten thousand people who eventually came to work at Los Alamos, Oppie knew several hundred intimately, by which I mean that he knew what their relationships with one another were and what made them tick. He knew how to organize, cajole, humor, soothe feelings - how to lead powerfully without seeming to do so. He was an exemplar of dedication, a hero who never lost his humanness. […] Los Alamos' amazing success grew out of the brilliance, enthusiasm and charisma with which Oppenheimer led it.

Robert Oppenheimer and General Groves

However, Teller did not like Bethe’s way of leading his section of the operation and thought that it had become too military in style. According to Rhodes, Bethe’s comment was:

> I believe maybe [Teller] resented my being placed on top of him. He resented even more that there would be an end to free and general discussion. […] He resented even more that he was removed [by lack of administrative contact] from Oppenheimer.

Bethe wanted Teller to perform calculations for explosions but Teller preferred to devote himself to something different: '[Bethe] wanted me to work on calculational details at which I am not particularly good while I wanted to continue not only on the hydrogen bomb but on other novel subjects'.

In spring 1944, Teller withdrew from the cooperation. Bethe has said (according to Rhodes):

> With the pressure of work and lack of staff, the Theoretical Division could ill afford to dispense with the services of any of its members, let alone one of such brilliance and high standing as Teller. Only after two failures to accomplish the expected and necessary work, and only on Teller's own request, was he, together with his group, relieved of further responsibility for work on the wartime development of the atomic bomb.

Teller was replaced by Rudolf Peierls. Oppenheimer left Teller to his novel ideas and thoughts about a hydrogen bomb, the one which people were now starting to refer to as the Superbomb. The work with the ordinary atomic bomb continued apace as the fissile material started to become available for studies and experiments. Two possible principles for bomb constructions: the cannon method and the implosion method.

It was a matter of rapidly getting the fissile material to form a critical mass. Speed was of the essence since neutrons from spontaneous nuclear fission could otherwise be too early in starting chain reactions that did not utilise all of the material. The result would then be a weaker explosion and the fissile material would melt and be dispersed early.

The easiest way of building up a critical mass in a short time is to shove together two subcritical masses in a 'cannon' using normal explosives. This was the basis of the cannon method, which was the one that Commander William Parsons, the head of military weapon development, primarily wanted to concentrate on.

There were also other methods, however. *Richard Talman*, a Professor of Physics at the California Institute of Technology (CalTech) and Deputy Chairman of the Defence Research Council (NDRC), thought of the possibility of arranging the fissile material in the form of an empty subcritical sphere and surrounding the sphere with an exterior shell consisting of a normal explosive. When this exploded, the fissile material would be compressed into a compact sphere and thereby become supercritical.

Owing to the practical difficulties of such an arrangement, not that much notice was taken of Tolman's idea but a Professor of Physics from the University of California, *Robert Serber*, mentioned it as one of several alternative ways of firing the pieces together when he held classes in Los Alamos in April 1943. One of his audience, another CalTech Professor of Physics by the name of *Seth Neddermeyer*, took to the idea with great enthusiasm. He understood that the implosion method described would bring together the fissile material at a considerably greater speed than the cannon method. He therefore suggested that the method ought to be investigated and Oppenheimer agreed to a small study.

However, getting an implosion bomb to work is a difficult task. The explosive power is intensified if an extra shell with a tamper is inserted in between the explosive charge and the hollow sphere of plutonium or uranium-235. The outer explosive charge must then first accelerate the tamper shell and then the shell of fissile material in towards the centre of the bomb sphere. It was possible to mathematically show that a heavy material could be accelerated towards a lighter one without the interface between the two collapsing. However, if you accelerated a light material in the direction of a heavier one, the interface was destroyed in turbulence and the two materials were mixed in an unpredictable manner.[*] In the bomb, the outer explosive was lighter than the tamper material, and this in turn was lighter than plutonium.

Neddermeyer's experiment and mathematical calculations by John von Neumann, who was summoned as a consult, also showed that an even distribution of the usual explosive in the outer shell would lead to pressure waves which would create irregularities through interference and not lead to the symmetrical compression they were looking for. This could be countered by arranging the explosive so that it led to a shaped charge; these configurations were called 'explosive lenses'. von Neumann also pointed out that the implosion method had an extra advantage that had been overlooked until now: it would achieve a higher pressure than the cannon method and therefore not need as much of the fissile material. It would also be possible to use a homogenous sphere instead of the hollow one. The high pressure would compress it to criticality.

An essential detail in a nuclear weapons is a device to start the chain reaction at the right time by suddenly supplying neutrons, known as a neutron initiator. To allow good control over the power of the nuclear weapon, the initiator must work with great precision within a few millionths of a second. The construction of the neutron initiator has been kept very secret.

von Neumann's result led to greater interest in the implosion method. Teller thought the fact that the benefits of the method had previously not been fully understood was an annoying omission. General Groves blamed Captain Parsons for not having concentrated adequately on the implosion method, having instead devoted himself almost exclusively to the cannon method.

Since Parsons had no confidence in the implosion method, he and Neddermeyer had difficulty getting on together. Oppenheimer therefore convinced Conant to in turn persuade George Kistiakowsky to come to Los Alamos to make headway with Neddermeyer's work. Kistiakowsky quickly discovered that the cooperation difficulties outweighed the physical problems. He has said (according to Rhodes):

> After a few weeks […] I found that my position was untenable because I was essentially in the middle trying to make sense of the efforts of two men who were at each other's throats. One was Captain (Deke) Parsons who tried to run his division the way it is done in military establishments - very conservative. The other was, of course, Seth Neddermeyer, who was the exact opposite of Parsons, working away in a little corner. The two never agreed about anything and they certainly didn't want me interfering.

In June 1944, Kistiakowsky had had enough of the cooperation difficulties and wrote to Oppenheimer, saying that it would probably be best if he were released from his mediation duties. But Oppenheimer did not want to lose Kistiakowsky and made an organisational change which placed Kistiakowsky in the position of Deputy Head of Department under Parsons, with total responsibility for developing the implosion method. Neddermeyer, along with Luis Alvarez who had recently come to Los Alamos from Chicago, would be technical adviser only. Neddermeyer accepted but was bitter – the implosion method was his baby.

Kistiakowsky had had his new assignment for no more than a month before the work in Los Alamos was affected by a staggering blow that made Oppenheimer consider resigning. It concerned a problem that Seaborg had warned of very early on: the presence of the isotope plutonium-240. Plutonium-240 is formed in reactors as a consequence of neutrons irradiating the plutonium-239 that is formed to start with. It was known that the plutonium produced by the reactor contained both plutonium-239 and plutonium-240, but the consequences of this had not yet been understood. It was Emilio Segrè who discovered them.

Some spontaneous fission of atomic nuclei takes place in both uranium and plutonium. Segrè studied this spontaneous fission because it could determine how pure the bomb material needed to be. In autumn

[*] The phenomenon is called the Raleigh-Taylor instability.

1943, he had been given a new laboratory for his studies, about which he has written (according to Rhodes):

> At this time I acquired a special small laboratory for measuring spontaneous fission, the like of which I have never seen before or since. It was a log cabin that had been occupied by a ranger and it was located in a secluded valley a few miles from Los Alamos. It could be reached only by a jeep trail that passed through fields of purple and yellow asters and a canyon whose walls were marked with Indian carvings. On this trail we once found a large rattlesnake. The cabin-laboratory, in a grove shaded by huge broadleaf trees, occupied one of the most picturesque settings one could dream of.

Segrè found that normal uranium displayed the same level of spontaneous fission that had been found in Berkeley but that the spontaneous fission in uranium-235 was significantly greater. He realised the explanation must be that the cosmic radiation had greater intensity at Los Alamos at more than two thousand metres above sea level. The active particles had to be slow neutrons that did not have the capacity to split uranium-238 but did to split uranium-235.

Where plutonium was concerned, Segrè began by measuring plutonium that was produced in one of Berkeley's cyclotrons and found no unexpected level of spontaneous fission. However, when he then measured plutonium produced in the X-10 reactor at Oak Ridge, he found significantly more. The presence of plutonium-240 was the obvious explanation. It was shown that plutonium-240 undergoes spontaneous fission to a greater degree than plutonium-239. The presence of plutonium-240 therefore adds extra neutrons that can start a chain reaction too early in a plutonium bomb and therefore make the effectiveness of the bomb uncertain.

The Hanford reactors had a much stronger neutron flow than X-10. Once produced, the right bomb material could therefore be expected to have a share of plutonium-240 that was insufficient to allow the combining speed permitted by the cannon method time to achieve an intended explosion. And the implosion method was still only at the development stage. It was only thanks to Seth Neddermeyer's stubborn belief in it, despite the surrounding - and primarily Parsons' - doubts, that the studies of the method had continued and might now offer a navigable path.

The level of plutonium-240 in reactor-grade plutonium is initially very low; plutonium-239 must be formed first before the isotope 240 can occur. However, the longer the fuel is in the reactor, the greater the share of plutonium-240 is formed. If you change to new fuel at an early stage and separate the plutonium that has been formed, it is possible to obtain a level of 90-95 % plutonium-239. This is usually called 'weapon-quality plutonium'. If on the other hand the fuel remains in the reactor for three to four years, as is the case with power-producing light water reactors of the same type as the Swedish ones, the plutonium has a much lower level of plutonium-239, approximately 60-70 %, while the level of plutonium-240 can exceed 20 % (the rest is other isotopes of plutonium, primarily plutonium-241 but also plutonium-238 and plutonium-242). These are the conditions that make plutonium from power reactors unsuitable for nuclear weapons and mean that nuclear weapon production requires regular replacements of the fuel in a production reactor.[*]

Faced by the threat of the plutonium from Hanford not being useable in a 'cannon-type' bomb, a disconsolate Oppenheimer contacted Conant on 11 July 1944. Six days later, Compton, Conant, Fermi, Groves, Nichols and Oppenheimer met in Chicago to discuss possible ways out. They precluded the option of separating plutonium-240 from plutonium-239, the difference between the masses of the two plutonium isotopes being too insignificant. The only solution seemed to be to concentrate wholeheartedly on the implosion method. Oppenheimer discussed the problem on long walks with the manager of Los Alamos' Department of Experimental Physics, Robert Bacher, and said he was thinking of stepping down as head. Bacher convinced him to stay and, after consulting Bacher and Kistiakowsky, Oppenheimer decided on a radical re-organisation. He took personnel and resources from Parsons'

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[*] The level of plutonium-240 in relation to plutonium-239 does not change within the foreseeable future since both of the nuclides have half-lives of several thousand years (6 600 years and 24 000 years respectively).

weapons department to create two new, parallel departments under Bacher and Kistiakowsky to develop different aspects of the implosion method. The new departments were called ‘G’ (for ‘gadget’) and ‘X’ (for ‘explosives’). Parsons, who had only the cannon method left, complained vociferously. Kistiakowsky’s memory is as follows (according to Rhodes):

> [Oppenheimer] called a big meeting of all the group heads, and there he sprang on Parsons the fact that I had plans for completely re-designing the explosives establishment. Parsons was furious - he felt that I had by-passed him and that was outrageous. I can understand perfectly how he felt but I was a civilian, so was Oppie, and I didn't have to go through him.[…] From then on Parsons and I were not on good terms. He was extremely suspicious of me.

But from then on, the development of the implosion method was given the highest priority and a strong increase in personnel numbers, while Parsons got to concentrate on the cannon method for uranium-235. The workforce increased from 1 100 to 2 500 within the space of less than a year. Some said that ‘implosion meant an explosion at the Laboratory population’.

In autumn 1944, uranium-235 started coming to Los Alamos from Oak Ridge’s electromagnetic separation plant Y-12. Otto Frisch led the group in Parsons’ department who experimented with critical assemblies. They used uranium trihydride, i.e., a compound of uranium and hydrogen, in spheres that could be stacked to increase the reactivity, whereby the subcritical multiplication factor was determined and the assembly studied to see how it approached criticality.

These experiments constituted the greatest risk in Los Alamos and would (after the war) kill a few people. A criticality experiment could have killed Frisch. He writes himself (Frisch, 1979):

> There was one occasion when I myself very nearly became victim of a similar accident. We were building an unusual assembly, with no reflecting material around it; just the reacting compound of uranium-235, because this was a good way to test the reliability of our calculations. For obvious reasons we called it the Lady Godiva assembly. I did follow all the rules. I had a student helping me. He was standing by the neutron-counting equipment, and we both watched the little red signal lamps blinking faster and faster and the meter clattering with increasing speed. Suddenly to my surprise the meter stopped; I looked up and saw that the student had unplugged it. Immediately I leant forward and called out ‘Do put the meter back, I am just about to go critical’, and at that moment, out of the corner of my eye I saw that the little red lamps had stopped flickering. They appeared to be glowing continuously. The flicker had speeded up so much that it could no longer be perceived.
>
> Hastily I took off some of the blocks of the uranium compound which I had just added, and the lamps slowed down again to a visible flicker. It was clear to me what had happened: by leaning forward I had reflected some neutrons back into Lady Godiva and thus caused her to become critical. I hadn’t felt anything, but after we had completed the experiment with appropriate care, I took a few of the blocks and checked their radioactivity with a counter. Sure enough, the activity was many times larger than what should have accumulated if that little incident hadn’t occurred.

Frisch had been seconds away from losing his life. If he been slower to remove some spheres, he would have received a deadly neutron dose. Such a fate affected two of his colleagues after the war, *Harry Daghlian* and *Louis Slotin*. On 21 August 1945, due to his interest in the work but in breach of the safety rules, Daghlian had stayed behind on his own when his colleagues had gone home. When he built up an almost critical assembly, he lost a block of reflecting material in it whereby the assembly became supercritical and he saw a blue shimmer of ionised air around it. He felt nothing immediately but felt sick when the ambulance he himself had called arrived. He was admitted to hospital, his blood values deteriorated and he died of a trivial infection after less than one month to which he had no resistance.

Louis Slotin had not breached the safety rules in the same obvious way, but in May 1946 he had slipped with a screwdriver while trying to separate two spheres of fissile material in the assembly, which thereby became supercritical. Slotin saved the other seven who were in the room by quickly removing a sphere, but he received a high neutron dose and died nine days later. His fate was the subject a novel by Dexter Masters (*The Accident*, Forum, 1961).

Despite his unpleasant experience, Frisch continued to think of different criticality experiments for the purpose of confirming the theoretical proposals on the quantity of uranium-235 needed in the bomb. The laboratory had now obtained sufficient uranium-235 to form what was surmised to be a critical mass, but was this supposition correct? It could not be tested in practical terms with less than a full bomb explosion. Or could it? Frisch had had an idea for a drastic experiment, which he himself describes (Frisch, 1979):

> The idea was that the compound of uranium-235, which by then had arrived on the site, enough to make an explosive device, should indeed be assembled to make one, but leaving a big hole so that the central portion was missing; that would allow enough neutrons to escape so that no chain reaction could develop. But the missing portion was to be made, ready to be dropped through that hole so that for a split second there was the condition for an atomic explosion, although only barely so.

Frisch’s proposal to experiment came up for discussion within the group of prominent physicists who were responsible for planning the work. Despite his young age, the group included Richard Feynman. When the experiment was described to him, he burst out laughing and said, ‘That’s like tickling the tail of a sleeping dragon.’ And so the experiment became known as the ‘dragon experiment’.

The experiment was carried out close to Los Alamos in a deserted area in which a three-metre-high iron stand called the ‘guillotine’ was built. Vertical aluminium guide rails were fixed next to the iron stand. Around these, spheres of hydride of uranium-235 were stacked to table height. A core of uranium hydride approximately 5 cm thick and around 15 cm in diameter was placed at the top. The intention was that it would fall down through the assembly of uranium spheres and the guide rails would ensure that it did not go astray. While it was falling it would be accelerated by gravity at 9.8 metres per second per second (9.8 m/s^2) so its speed through the passage could be calculated. For a short, calculable moment, the uranium would form a critical mass. However, since the uranium was combined with neutron-slowing hydrogen, the chain reaction would be slower than in the bomb’s pure uranium-235. In his memoirs, Frisch writes:

> It was as near as we could possibly go towards starting an atomic explosion without actually being blown up, and the results were most satisfactory. Everything happened exactly as it should. When the core was dropped through the hole we got a large burst of neutrons and a temperature rise of several degrees in that very short split second during which the chain reaction proceeded as a sort of stifled explosion. We worked under great pressure because the material had to be returned by a certain date to be made into metal [...].

The dragon experiment clearly showed that an explosive nuclear reaction was possible. The energy was produced at a speed of up to twenty million watts and a temperature increase of 2 °C per thousandth of a second, but because the passage time was just a few thousandths of a second, the total energy development remained moderate.

On 12 April 1945, President Roosevelt died of a brain haemorrhage in Warm Springs, Georgia, and was succeeded by Harry S. Truman. On 30 April 1945, Adolf Hitler committed suicide and the war in Europe had ended just after that. The allies, i.e., the United States, the Soviet Union, the United Kingdom, France and China, then prepared for a summit meeting in Potsdam, 20 km south-west of Berlin, to discuss peace terms but also the requirements regarding Japan since the war was continuing in the Pacific Ocean area. Truman who, despite having been Vice President, had not known about the Manhattan Project and was now anxious to test an atomic bomb before the Potsdam Conference so that he had a trump card to play during the negotiations. He therefore tried to get the conference arranged as late as possible. General Groves hurried with the work on the bomb for the same reason.

In spring 1945, the situation was that there was still not enough uranium-235 for one single bomb until the summer so it was therefore not possible to do a test explosion of a uranium bomb. On the other hand, the production of plutonium was faster and there was enough for a test. There was also a greater need to test this since the implosion method involved many uncertain elements. The test that would take place in the Alamogordo desert (in the test that was called 'Trinity') as far as the technologists were concerned was therefore not primarily the bomb, but the implosion method. The fact that uranium-235 and plutonium could explode was taken for granted. On the other hand, the American politicians had no concept of the technical conditions; they just wanted to be able to indicate a successful test, an explosion that would impress the allies and give them a stronger position from which to negotiate. This difference in attitude is not difficult to understand but it has still led to some speculations. Samuel Walker writes, for example (Walker, 1996:1):

> If Truman and his advisers realized during the summer of 1945 that the uranium bomb was almost ready and almost certain to work, it is curious that they reacted with so much surprise and elation to the news of the Trinity shot. If they did not understand that they would soon have an atomic bomb no matter what happened at Alamogordo, it suggests that they grasped or remembered little of what they were told about the details of the bomb project. Part of the explanation is that policymakers did not want to rely on the bomb until it definitely had proven to be successful, and they were unwilling to believe that it would make a major difference to them until they were shown what it could do. But they seemed to have little awareness that two types of bombs were being built. Even Stimson, the best-informed and most reflective senior official on matters regarding the bomb, appeared to think in terms of a single weapon that had to be tested at Alamogordo. The issue is not one of transcending importance, but it could help to clarify the significance that Truman and his advisers attached to the bomb and its role in their planning. It might in that way resolve some of the contradictions and apparent confusion in Truman's diary.

When the bombs had finally been completed, they were different shapes. The implosion bomb with plutonium was almost spherical and would, owing to its volume, be called Fat Man. The cannon bomb with uranium-235 was instead long and narrow and was therefore initially called Thin Man. The Fat Man - Thin Man pair appears to have hinted at Churchill and Roosevelt, but following the death of the latter, the uranium bomb instead became known as Little Boy.

The test that was prepared for the plutonium bomb was given the code-name 'Trinity'. It appears to have been proposed by Oppenheimer, inspired by the sonnets written by the English poet *John Donne* (1573-1631) at the start of 1600s. Donne's sonnet number fourteen which begins 'Batter my heart, three person'd God ...' seems primarily to have impressed Oppenheimer, although another source of inspiration is said to have been the Hindu mythology and Sanskrit epic *Mahabharata*, where the sixth book *Bhagavadgita* contains a conversation with the god Vishnu about the duty of warriors to fight. Hindu mythology describes a trinity: the creator, the maintainer and the destroyer, represented by the gods Brahma, Vishnu and Shiva.

The search for a suitable area for the bomb test had already begun in spring 1944. It would be within a reasonable distance of Los Alamos but not close enough to create any obvious link. As few people as possible ought to be living nearby to facilitate a total evacuation of the area. It was reckoned that a successful explosion would scatter the radioactive substances and if the explosion failed, the usual explosion would scatter the plutonium in any case. The final choice was between two areas: the military firing ranges and bomb-dropping areas at Alamogordo in New Mexico and the Mojave Desert in California. In September 1943, General Groves decided on an area approximately 30 times 40 kilometres in the north-western part of the Alamogordo field. The explosion site lay almost 20 kilometres from the nearest farm and 43 kilometres from the nearest sizeable community. Groves had set a condition that the area should not include residences for Indians since he feared it would lead to bureaucracy problems. In his book on the Manhattan Project, he writes:

> I added one special prohibition: that it should have no Indian population at all, for I wanted to avoid the impossible problems that would have been created by Secretary of the Interior Harold L. Ickes, who had jurisdiction over the Bureau of Indian Affairs. His curiosity and insatiable desire to have his own way in every detail would have caused difficulties and we already had too many.

The scattering of plutonium if the nuclear charge did not explode was a point of concern not just from the health point of view but also due to the value of the plutonium. The initial plan was therefore to let the bomb explode in a special steel container called Jumbo. It was very difficult to transport Jumbo from the producer on the east coast to the Alamogordo desert. Jumbo was colossal. It's interior diameter was more than 3 metres and it was almost 8 metres long. The steel inner wall was 15 centimetres but it was surrounded by a number of layers of steel bands that were 1 centimetre thick so the total thickness of the wall was around 35 centimetres. Jumbo was produced on the east coast and transported to New Mexico on fortified railway wagons. From the last railway station it was carried on a truck trailer with 36 large wheels. On top of everything, the idea of Jumbo was abandoned when confidence in the bomb increased. In his book, Groves says: 'It is interesting to speculate about what would have happened, with the actual explosion of almost twenty thousand tons [TNT], if we had used Jumbo. That the heat would have completely evaporated the entire steel casing is doubtful. If it did not, pieces of jagged steel would probably have been hurled for great distances'.

The exact time of the bomb test was uncertain and depended on when the bomb would be fully assembled in Los Alamos. Bush, Conant and Groves therefore prepared themselves by making a series of visits to the Manhattan Project's plants in Hanford, San Francisco and Pasadena (the headquarters of California Institute of Technology).

Groves and company arrived at the test site on 15 July 1945, aware that the Potsdam Conference would begin two days later. Groves was very anxious for the test to have taken place prior to this. He thought that a successful demonstration of the bomb would make it possible for President Truman to push through a strong ultimatum to Japan from the allies in Potsdam. Groves did not count on the war being over until a couple of atomic bombs had been dropped on Japan, but the bombs could not be used until the Japanese had had a chance to consider the ultimatum of the allies. The sooner this was done, the sooner the bombs could be dropped and the sooner the war would be over. Every extra day of war would cost American lives, feared Groves. At the same time as the preparations for the Fat Man test, Little Boy was shipped to the airbase in the Pacific Ocean from where the bomber would leave for Japan.

The thick plutonium bomb was fully assembled and placed on the top of a 30-metre-high steel tower but the atmosphere was nervous. When Groves consulted Oppenheimer and his representative, General Farrell, the weather appeared to be unsuitable.

The weather was important for several reasons. Six months previously, one of the scientists in Los Alamos, *Joseph Hirschfelder*, had drawn attention to the anticipated radioactive fallout, which he thought would constitute a bigger problem than had been thought up to now. It was therefore very important for the wind not to be blowing in the direction of any big town which would be difficult to evacuate if this were necessary. Nor should it rain so that the radioactive dust would fall to the ground too soon and leave a dangerous ground covering rather than being dispersed in the atmosphere. Heavy rain could also short-circuit electrical wires to measurement instruments and maybe also to the bomb trigger. Finally, General Groves wanted to have a clear enough view for an observation plane to be able to fly over the surroundings.

The first proposal was to allow the bomb to explode at 4 o'clock in the morning on 16 July, a time that was suitable if you wanted to avoid arousing attention from the surroundings. At 1 o'clock that same morning, Groves and Oppenheimer left the base camp, which lay 16 kilometres from the explosion site, for a protection room around 8 kilometres from the tower where the bomb was.

A few hours later, it was obvious that the test would have to be postponed. Groves and Oppenheimer, who left the protection room time after time to see what the weather was like, faced a difficult decision. The longer the test was postponed, the damper all wires and instruments would become. The technicians who were at the bomb tower for the final preparations were also becoming more and more tired and would soon be obliged to rest which would mean losing a full day. To top it all, Captain Parsons announced from the airfield in Albuquerque that the person in charge of the site refused to allow the observation plane to start up due to the bad weather.

At the base camp, Fermi was trying to lighten the atmosphere by inviting bets as to whether the explosion would transmit to the atmosphere and, if so, whether only New Mexico or the whole world would be destroyed. It was not meant seriously but was a sign of the nervous tension that prevailed.

However, there had previously been serious discussions regarding the possibility that the explosion would cause a chain reaction in the atmosphere and maybe in all of the oceans.

Bethe had gone through all conceivable physical possibilities and found it impossible. Teller had also done the same thing. He has said (Teller, 1962):

> Before Alamogordo, I asked for and obtained a most important assignment, one that many considered superfluous. There had been some suggestions that we might have miscalculated, that the explosion could be much larger than we had anticipated. Could the enormity of the atomic bomb be even more enormous? Might we set off a chain reaction that would encircle the globe in a set of fire? It was my job to make a last check and review.

When Teller had satisfied himself that the fears were unfounded, he thought that no-one needed to worry. The question is whether such self-confidence is comical or frightening.

In the end, General Groves decided that the test would go ahead at 05.30. With this decision made, everything went according to the prepared plans. The five men who had been guarding the bomb left the bomb tower half an hour before the explosion. One of them was Kistiakowsky and they were taken by jeep to the protection room, which they fortunately reached in time. Groves left Oppenheimer in the protection room and returned to the base camp.

Everyone was ordered to lay flat on their stomachs on the ground with their feet towards the bomb tower. They should close their eyes and cover their eyes with their hands. As soon as they were aware of a flash of light, they would get up and watch the explosion through dark glass. Groves describes the situation:

> As we approached the final minute, the quiet grew more intense. I, myself, was on the ground between Bush and Conant. As I lay there, in the final seconds, I thought only of what I would do if, when the countdown got to zero, nothing happened.
>
> I was spared this embarrassment, for the blast came promptly with the zero count, at 5:30 a.m., on July 16, 1945.
>
> My first impression was one of tremendous light, and then as I turned, I saw the now familiar fireball. As Bush, Conant and I sat on the ground looking at this phenomenon, the first reactions of the three of us were expressed in a silent exchange of handclasps. We all arose so that by the time the shock wave arrived we were all standing.
>
> I was surprised by its comparative gentleness when it reached us almost fifty seconds later. As I look back on it now, I realize that the shock was very impressive, but the light had been so much greater than any human had previously experienced or even than we had anticipated that we did not shake off the experience quickly.

Oppenheimer’s memory is quoted by Rhodes:

> We waited until the blast had passed, walked out of the shelter and then it was extremely solemn. We knew the world would not be the same. A few people laughed, a few people cried. Most people were silent. I remembered the line from the Hindu scripture, the Bhagavad-Gita: Vishnu is trying to persuade the Prince that he should do his duty and to impress him he takes on his multi-armed form and says, ‘Now I am become Death, the destroyer of worlds.’ I suppose we all thought that, one way or another.
>
> Teller was in the company of Richard Feynman and journalist William Laurence, whom General Groves summoned to prepare future releases. Teller describes the situation (Teller, 1962):

> Early on the morning of July 16, 1945, I was one of a group watching the explosion of the world’s first atomic bomb. Our observation post was about twenty miles from the Alamogordo test site. We were told to lie down on the sand, turn our faces away from the blast, and bury our heads in our arms. No one complied. We were determined to look the beast in the eye.
>
> But, having practiced to expect the impossible, I was cautious. Beneath the welder’s glasses provided us, I wore an extra pair of dark glasses. I smeared my face with sun-tan lotion and offered some to the others. I wore a heavy pair of gloves. Holding the welder’s glasses securely to my face with both gloved hands, I converted the glasses into goggles [Teller means that he used his gloves to shield all light from the sides as well].
>
> The countdown began: ‘It now is minus twenty minutes, nineteen minutes, eighteen minutes, seventeen minutes … It now is minus thirty seconds, twenty-five, twenty, fifteen.’ At ten, the count-down was second by second: ‘Nine … eight … seven … six … five.’ Then there was silence.
>
> The five seconds of quiet stretched out until I thought the explosion had failed. I was almost ready to take off my protective glasses. But then, through the glasses, I saw a tiny pin point of light. I was disappointed: ‘Is this all? Is this what we have worked so hard to develop?’

But then Teller remembered that he was wearing heavy welder’s glasses. He removed one hand so the light came in from the side. ‘It was like opening the heavy curtains of a darkened room to a flood of sunlight. Then I was impressed.’

The shock wave and the roar from the explosion came one and a half minutes later. A scared Laurence asked: ‘What was that?’ Richard Feynman, who stood next to Laurence, was surprised at the latter’s lack of basic knowledge of physics. Feynman’s philosophy was to apply his knowledge. He assumed that normal light could scarcely be dangerous to the eyes.[*] Dark glasses, thought Feynman, were really silly. What you needed protection against was ultraviolet light. Feynman therefore stood behind the windscreen of a truck, thinking it would give sufficient protection. He says (Feynman, 1985):

> Time comes, and this tremendous flash out there is so bright that I duck, and I see this purple splotch on the floor of the truck. I said, ‘That’s not it. That’s an after-image.’ So I look back up, and I see this white light changing into yellow and then into orange. Clouds form and disappear again – from the compression and expansion of the shock wave.
>
> Finally, a big ball of orange, the center that was so bright, becomes a ball of orange that starts to rise and billow a little bit and get a little black around the edges, and then you see it’s a big ball of smoke with flashes on the inside of the fire going out, the heat.
>
> All this took about one minute. It was a series from bright to dark, and I had seen it. I am about the only guy who actually looked at the damned thing – the first Trinity test. Everybody else had dark glasses, and the people at six miles couldn’t see it because they were all told to lie on the floor. I’m probably the only guy who saw it with the human eye.

Oppenheimer and Farrell left the protection room and returned to the base camp by jeep. Farrell’s first words to Groves were: ‘The war is over.’ Grove’s response was: ‘Yes, after we drop two bombs on Japan.’ Groves then sat down with Oppenheimer to draw up a report to Secretary of War Stimson in Potsdam, while Rabi opened a bottle whisky and passed it around. Rabi won the bet on the strength of

[*] Feynman became a Nobel Prize winner and was a brilliant physicist but his perception of what was hazardous to the eyes was completely wrong. The retina of the eye can only be damaged by the radiation that can penetrate the eye and the visible light that is too intense can cause heat damage. Ultraviolet radiation is already absorbed by the cornea and can cause wounds there. If Feynman’s retina was not damaged, it was solely thanks to the long distance of 20 kilometres to the bomb.

the bomb. He had guessed in advance at 18 kilotonnes and accurate measurements indicated 18.6 kilotonnes.*

The shock wave had been so powerful that the heavy Jumbo, which stood on its edge five hundred metres from the bomb tower, capsized so it was then standing as a leaning monument to Trinity. General Groves' immediate plans were also, according to what he says in his book, overturned:

> I had planned to remain at Alamogordo a number of hours after the explosion to make certain that there was no fallout problem. In order to make full use of the time, I planned to discuss and settle a number of matters involved in our operations against Japan with the members of the Los Alamo group, some of whom were due to leave almost immediately for Tinian*†. I had also counted on having a discussion with Oppenheimer on some other important points. These plans proved utterly impracticable, for no one who had witnessed the test was in a frame of mind to discuss anything. The reaction to success was simply too great. It was not only that we had achieved success with the bomb; but that everyone – scientists, military officers and engineers – realized that we had been personal participants in, and eyewitnesses to, a major milestone in the world's history and had a sobering appreciation of what the result of our work would be. While the phenomenon that we had just witnessed had been seriously discussed for years, it had always been thought of as a remote possibility – not as an actuality.

Because the test detonation had not been able to take place at 4 o'clock in the morning as was originally intended, more people were awake and up and about than General Groves would have liked. There were therefore more questions from the nearby community than expected. The unpredictable behaviour of the shock wave caused a few surprises, including windowpanes disintegrating in Silver City almost 200 kilometres to the south-west, and the explosion was heard and caused anxiety in El Paso in Texas, the same distance to the south.

General Groves was anxious to get a vague press release out but he was still unsure as to whether the radioactive fallout would require any evacuation. In the end, the following release was issued:

> *Alamogordo*, N.M., 16 July
> The Commanding Officer of the Alamogordo Army Air Base has made the following statement today:
> 'Several inquiries have been received concerning a heavy explosion which occurred on the Alamogordo Air Base reservation this morning.
> A remotely located ammunition magazine containing a considerable amount of high explosives and pyrotechnics exploded.
> There was no loss of life or injury to anyone, and the property damage outside of the explosives magazine itself was negligible.
> Weather conditions affecting the content of gas shells exploded by the blast may make it desirable for the Army to evacuate temporarily a few civilians from their homes'.

The release was examined by many people. when a few days later one of Du Pont's scientists visited Groves, he said: 'Oh, by the way, General, everyone at Du Pont sends you their congratulations.' When Groves appeared not to understand, he continued: 'It's the first time we ever heard of the Army storing high explosives, pyrotechnics and chemical in one magazine.' My only response was: 'That was a strange thing, wasn't it?'

The damage to the surrounding area was not extensive beyond the bomb crater because there was not much to damage. Dead wild rabbits were found almost one kilometre away from explosion site, however.

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* Right from the start it has been customary to state the blast effect of nuclear weapons with the quantity TNT which causes the same blast effect. An atomic bomb with an effect of 18 kilotonnes is therefore considered to have the same blast effect as 18 thousand tonnes of TNT (trinitrotoluene). The "blast effect" has actually looked at the developed energy rather than the level of impact on the surroundings.

† The island in the Pacific Ocean from which bomber would leave for Japan.

Wooden panels and crates of wood wool which were positioned at various distances were charred at a distance of one kilometre and slightly burned at a distance of two kilometres.

The radiation dose at the actual explosion crater appeared to be higher than expected; radiation from induced activity in the ground had been overlooked. Hempelmann’s radiation protection group guarded the site in the initial weeks along with the military police, who noted the visitors to it. In October, Hempelmann discovered that some military police regularly guided visitors to the crater and that they were thus subject to repeated exposures so that a couple of them were estimated to have had a total of around 30 x rays (approximately 300 millisieverts).

The possibility of significant radiation doses from fallout from the radioactive substances from the cloud that rose from the explosion site was discussed early on but was initially not thought to be a great problem. Hempelmann’s radiation protection boss, *Joseph Hoffman*, had the task of organising and taking measurements. General Groves detached a body of 144 men from the army under the leadership of an officer by the name of *T.O. Palmer* to search for people who may have lived in a danger zone and help them to move to safety. Hoffman had the task of deciding whether an evacuation would be necessary. Stafford Warren and Hempelmann had agreed on a limit of 75 röntgen (approximately 700 millisieverts) for the radiation dose over two weeks if it was a matter of the whole body being irradiated. For the dose rate, i.e., the dose per time, they thought that the value when at its highest ought not to exceed 15 röntgen (approximately 150 millisieverts) per hour.

After the explosion, the wind appeared to drive the radioactive cloud to the north. The community that was immediately affected was Bingham, ‘little more than a country crossroads store’, by motorway 380. In the morning after the test, Joseph Hirschfelder and his colleague *John Magee* drove to Bingham to take radiation measurements. They visited the country store and Hirschfelder recounts (according to Hack, 1967):

> John and I rang the doorbell and an old man came out. He looked quizzically at us (John and I were wearing white coveralls with gas masks hanging from our necks). Then he laughed and said, ‘You boys must have been up to something this morning. The sun came up in the west and went on down again.’ There was some fallout there, but the level of radioactivity was not dangerous. The soldiers bought almost everything in the store and left his shelves bare.

Hirschfelder and Magee continued on a small gravel road north of Bingham to a searchlight outpost that had been set up to illuminate the cloud in the light of the dawn. There, Magee was able to measure 2 röntgen (20 millisieverts) per hour at half past eight in the morning. The radioactive cloud had just arrived there and fallout came like snowflakes. Hoffman was radio-messaged the measurement values and came to Bingham, where he now measured 3.3 röntgen per hour. At around 12, Hempelmann also arrived. When the cloud had passed, the measurement values began to fall but the measurement values were still high along the small gravel road that wound its way up towards Chupadera Mesa north-east of Bingham. The highest values were measured where the road went down into a canyon which was immediately christened Hot Canyon. However, the maps showed no settlement in the area, so in the evening they concluded that the relatively high radiation levels had not harmed anyone.

However, the next morning, Hempelmann and Friedell travelled to chart the fallout a bit closer to hand. To their surprise they found, several kilometres to the side of the small gravel road, a farm that was not mentioned on the map but which was located where the fallout was strongest. An older couple lived there with their grandchild, a ten-year-old boy. The family was called Raitliff and owned livestock and several dogs. Later, another farm was found nearby, occupied by a family named Wilson. Hempelmann later estimated that the Raitliffs had received a radiation dose of approximately 50 röntgen (500 millisieverts) over twelve weeks. Neither of the doctors thought it was necessary to evacuate the two farmers.

Hempelmann returned to the Raitliffs in August and October. He found that the skin of the livestock had been damaged and they had suffered hair loss here and there where radioactive material had worked its way down to the skin. The dogs also had damage to their paws where radioactive material had become wedged between the toes.

The grass-covered plains in the area were also pastures for livestock other than those belonging to the Raitliffs and the Wilsons. In October 1945, a farmer demanded compensation for injuries to his animals and when the army inspected the area, it was estimated that maybe six hundred cattle has received some form of skin damage. The army offered to buy as many animals as the owners wanted, which appeared to be seventy-five. Some were sent to Los Alamos for examination while the rest were sent to Oak Ridge. After having examined the injuries, Robert Stone estimated that the radiation dose in the skin of the animals could have been biologically equivalent to 20 000 röntgen, i.e., approximately 200 sieverts.

The people on the farms showed no injuries. Clothes, staying indoors and washing kept the radiation doses below the risky values. However, Stafford Warren concluded that they had been dangerously close to serious injuries and that the Alamogordo field was therefore not a suitable area for any future test detonations. Oppenheimer kept all reports on fallout and its effects secret for a while; he was afraid of claims for compensation that would have been difficult to handle.

The Trinity test was kept secret until the bombs were used in Japan. The atomic bombs over Hiroshima and Nagasaki therefore came as a total surprise to everyone except the few who were responsible for the Manhattan Project and the use of the bombs. Many do still appear to have a memory which confuses the secret explosion in the Alamogordo desert in summer 1945 with the first, very well-publicised bomb test which was carried out in the Pacific Ocean after the war. One of those who certainly does misremember is *Maja Ekelöf* (1918-1989) in her *Rapport från en skurhink* [Report from a cleaning bucket] (1970), when she writes:

> I heard Palmstierna's[*] 'Urgent' on the radio yesterday. After having listened to him you can better understand how important it is for the youth of the world to protest. We do not have many years left on our Planet Earth unless we change something. The air will run out because so much poison is put out to sea: the poison will destroy the green algae which in turn supplies our oxygen. The fresh water will also not last long. The thin layer of soil (fifteen cm) will be destroyed in many places through all the deforestation and jungle devastation. Then only mountains and layers of stone will remain. The scientists have known this ever since the Hiroshima bomb in 1945. I already suspected it in 1944 when the United States did its first atomic bomb test. I remember that day well. I had read about the test in the newspaper. Some scientists were saying that the Earth would split when the test took place - that there would be large cracks in the Earth's crust. On that very evening when the test was to take place, my mother and I were invited to the home of a lady in a new HSB house. We wanted to see what her place was like. Just before the test was to take place I said that I felt sick. I took a taxi home. Wanted to be with my children when the Earth split. That was the evening my anxiety began.[†]

[*] *Hans Palmstierna* (1926-1975), chemist and environmental conservation debater.

[†] The first test detonations at Bikini Atoll in the Pacific Ocean took place in July 1946 in a series of experiments called Crossroads. The first detonation was called Able, but it was the second, called Baker, which attracted the most attention. Baker was the first test to be carried out underwater and it was given huge publicity, particularly when photographs showed how the 26 000-tonne battleship *Arkansas* was lifted up in the 10 million-tonne water column which shot up a couple of kilometres above the lagoon.

11. HEISENBERG'S BOMB

IN GERMANY, the development had proceeded rapidly up until summer 1940 but then appeared to come to a stop. Military research under Diebner's command was run by physicists von Weizsäcker, Heisenberg's student *Karl Wirtz* (1910-1994) and a usually absent Heisenberg in the 'Virus House' in Berlin-Dahlem, and also directly by Diebner in Gottow. Heisenberg also experimented on an independent basis in Leipzig together with *Robert Döpel*, and Harteck did research in Hamburg. However, 1941 started sombrely because there had been no access to any useable moderator material.

In this position, Harteck began experimenting with dry ice as a moderator. The dry ice contained only pure carbon and oxygen. Heisenberg had also said that the chain reaction would be slowed down by increasing temperatures. In that case, ought it not also be easier to get it going at low temperatures?

Harteck contacted The Leuna Works in Merseburg[*] and was promised some ten tonnes of dry ice. Diebner promised that the War Department would make available a special railway wagon to rapidly transport the ice from Merseburg to Hamburg. All that remained was to procure an adequate quantity of uranium.

This is where the cooperation problems were exposed. The various experimental physicists had been allocated small quantities of uranium for experiments. No single large quantity had been accumulated. Every researcher wanted to study the properties of uranium in the hope of being the first to come up with a 'uranium burner'. Everyone wanted more uranium, and no-one wanted to step back for the benefit of the competitors. The Auer factories would certainly supply more uranium oxide for the summer but the Leuna Works would then no longer have any dry ice to spare since it would be needed to cool foods for the military.

However, under pressure from the military, the different scientists temporarily gave up their small quantities of uranium but under protest and with caustic remarks. However, Heisenberg refused to give up any of his uranium - he wanted significantly larger quantities himself.

Harteck started the experiment on 3 June 1941 with 15 tonnes of dry ice but just 185 kg of uranium, which he knew was insufficient. As expected, he was also unable to observe any neutron multiplication. Had he obtained all the uranium he needed from the start, he would probably have been able to demonstrate the practical possibility of extracting the atomic energy in early summer 1941. The experiment would then probably also have led to reconsideration of the suitability of graphite as a moderator and the Germans would have had a two-year lead over the Americans. Harteck was now forced to discontinue his experiments; the dry ice was needed in summer 1941 to store food during the campaign in Russia.

Heavy water from the Vemork factory at Rjukan did not become available until autumn 1941. Heisenberg then did an experiment in September 1941 in Leipzig using 142 kg of uranium oxide in two plates surrounded by 164 kg of heavy water, but produced no results there either. However, he remained pretty optimistic in the hope of obtaining more heavy water.

Heisenberg has said that he was thinking about a moral problem at this point in time – what on earth was he doing? To get an answer to the question, he travelled to Copenhagen to meet the man whom he particularly looked up to and hoped to obtain advice from: Niels Bohr. He was with von Weizsäcker, and their visit to Copenhagen has been the object of innumerable interpretation attempts because Bohr and Heisenberg have given completely different accounts of what really happened.

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[*] The Leuna Works was the popular name for the *Ammoniakwerk Merseburg GmbH* in Merseburg just west of Leipzig. The factory was established in1916 for the production of nitrogen during the First World War, later producing synthetic fuels and nitrogen fertiliser.

Irrespective of what Heisenberg's real intention behind the visit to Bohr may have been, it is indisputable that he went in wholeheartedly with a total lack of understanding that, as a German in occupied Denmark, he should be careful of what he said. Heisenberg defended the German invasion of Poland, and when Bohr said that the atrocities against Poland were indefensible, Heisenberg's response was that Germany had not sought to destroy France in the same way. Heisenberg had also said that Bohr ought to seek help from the German Embassy if his situation became worse. By all accounts, Heisenberg was completely unaware that his statements irritated Bohr to boiling point.

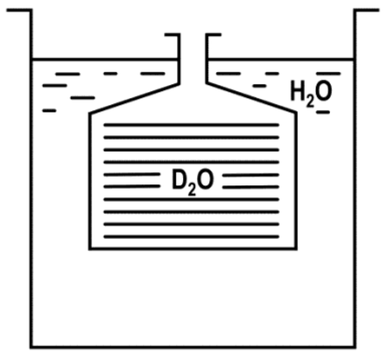
According to Heisenberg himself, he asked Bohr whether he thought it was right for physicists to busy themselves with uranium research during wartime. Bohr is meant to have answered: 'Do you really think that uranium fission could be utilised for the construction of weapons?' When Heisenberg's response was positive, Bohr became very indignant. In his book, *Physics and Beyond*, Heisenberg writes:

> Bohr was so horrified that he failed to take in the most important part of my report, namely, that an enormous technical effort was needed. Now this, to me, was so important precisely because it gave physicists the possibility of deciding whether or not the construction of atom bombs should be attempted. They could either advise their governments that atom bombs would come too late for use in the present war, and that work on them therefore detracted from the war effort, or else contend that, with the utmost exertions, it might just be possible to bring them into the conflict. Both views could be put forward with equal conviction.

Victor Weisskopf has given the following account of what Bohr told him about Heisenberg's visit (according to Powers):

> Heisenberg wanted to know if Bohr knew anything about the nuclear program of the Allies. He wanted to propose a scientists' decision not to work on the bomb, and he wanted to invite Bohr to come to Germany to establish better relations. The idea of a common policy of the world's scientists not to work on the bomb – Bohr did not accept this at all. He said to himself, 'Either Heisenberg is not being honest, or he is being used by the Nazi government.' He thought perhaps the government tried to use Heisenberg to prevent the Allies from building a bomb. But Bohr always said he was never quite sure what Heisenberg wanted.

When Bohr came to the United States and Los Alamos two years later, he showed Oppenheimer and his colleagues a diagram that he said originated from Heisenberg's visit in autumn 1941. This is what the diagram looked like:



Jeremy Bernstein (Bern, 1995) has pointed out the fact that Bohr can scarcely have had this diagram or the idea for it from anyone other than Heisenberg because it shows the reactor arrangement that was typical of the latter - uranium plates lowered into heavy water. Bohr appears to have believed that the diagram concerned a bomb. Bernstein writes:

> […] there seems to be little doubt that Heisenberg attempted to describe a nuclear device to Bohr. It seems that this device was his version of a reactor. He may, or may not, have given Bohr a drawing, but Bohr clearly retained a visual memory of the design.

> Bohr, however, did not understand the difference between a reactor and a bomb at the time and assumed that Heisenberg was describing a bomb.
>
> […] None of the individuals I have contacted are sure that the drawing they saw was in Heisenberg's hand – only that it was a drawing of Heisenberg's reactor. This I think solves the puzzle, but it does not solve the mystery. What was the purpose of Heisenberg's visit in the first place? Those who admire Heisenberg have argued that it was to show Bohr that the Germans were working only on a 'peaceful' reactor.
>
> It also must be noted that when Heisenberg visited Bohr, he clearly knew that reactors could be used to manufacture plutonium and that plutonium could fuel a nuclear weapon. Why, then, did he visit Bohr? What message was he trying to convey? What was he trying to persuade Bohr to do, or not to do? What was he trying to learn? That is the real mystery, one we may never solve.

In the winter months between 1941 and 1942, the German economy began to crumble as a consequence of the Russian campaign. The army could not afford to continue research on a full scale and it was partially transferred to the Ministry of Education, so Professor Esau came back into the picture. Great confusion was afoot in February 1942 when the National Research Council and the Army Supply Administration jointly convened two different conferences which almost coincided. The meeting that was organised by the National Research Council was intended to give the scientists who were summoned to the Army Supply Administration's subsequent meeting an opportunity to first hold a series of popular talks in front of an auditory which would include some very prominent guests, including Himmler. The National Research Council, represented by Esau, hoped that this would convince the Nazi elite, who were as yet ignorant of the existence of nuclear research, of the great importance of continuing research within the area.

However, it was now time for Fate to intervene. An error in sending the invitations meant that the prominent guests received only the agenda for Army Supply Administration's conference, the scientific titles of whose talks were such a deterrent that they bewildered Himmler and other rulers and they declined to attend. The National Research Council's information meeting was therefore something of a fiasco.

However, Minister for Education Bernhard Rust was also present at this meeting, which was held on 26 February 1942. In his talk, Heisenberg mentioned the possibility of using plutonium as bomb material. He said (according to Bernstein, 1996:2):

> As soon as such a pile begins to operate, the question of producing the explosive receives a new twist: through the transmutation of uranium inside the pile, a new element is created (atomic number 94), which is in all probability as explosive as pure uranium-235, with the same colossal force.

Heisenberg never made a careful estimate of the critical mass for a bomb until after the war had ended. He gave different answers on different occasions, varying from a few kilogrammes to a couple of tonnes. On the contrary, Diebner and his colleagues wrote a report in February 1942 which stated the critical mass of uranium-235 as being between 10 and 100 kg.

Minister for Education Rust now gave scientists his support but the military leadership was not as convinced as their experts in Gottow. The situation may have been different had the bigwigs not been too intimidated to attend the National Research Council's information meeting where a more comprehensible language had been spoken.

The whole time was spent waiting for heavy water to resume its flow – they now had the uranium. Early on, the military had thought it unsuitable to work directly with the uranium oxide that the Auer factories could produce and wanted to use pure uranium metal instead. However, Auer had no resources to reduce the oxide to metal. Its laboratory manager, Dr. Nikolaus Riehl, had therefore approached

Degussa[*], which had previously produced thorium for Auer. Degussa could essentially also use its thorium factory in Frankfurt to produce uranium. Therefore, by the end of 1940, Germany already had a factory production of pure uranium metal on the go, two years before any significant quantity of uranium was available in the United States.

The end of 1941 and the start of 1942 saw the beginning of an end to the German war successes, and both the economy and material supplies were facing problems in the long term. In February 1942, the military leadership was forced to make a decision on the continuing investment in uranium research. Professor Schumann, the immediate supervisor, was of the opinion that there was no hope of a German atomic bomb being finished in time to affect the outcome of the war, and that it was therefore not reasonable to continue to tie up resources for that purpose. Almost simultaneously (in June 1942), Vannevar Bush drew the opposite conclusion in the United States. By this time, roughly the same knowledge existed in both countries, yet the development now went in different directions. Schumann's decision involved the military giving back the Kaiser Wilhelm Institute for Physics to the Kaiser Wilhelm Society and cutting back on the research efforts in Gottow.

The military's cutbacks meant that in March 1942, the National Research Council resumed control of the nuclear physics research. At the same time, the Kaiser Wilhelm Society's influence was reduced and the power returned to Minister for Education Rust and Professor Esau. Esau took over 'Arbeitsgemeinschaft Kernphysik' from the military but Diebner continued in Gottow in spite of the fall in support. However, he had to leave the Virus House in Dahlem, and Heisenberg was appointed as head of the Kaiser Wilhelm Institute for Physics in April as successor to Debye.

On 1 May, Degussa had supplied Heisenberg, Diebner and Riehl with 3.5 tonnes of uranium powder. Heisenberg had thereby obtained more uranium than he had had available at the time of his first experiment in Leipzig in September 1941, but he still had insufficient heavy water. He still wanted to perform a new experiment with his colleague Professor Döpel. The larger quantity of uranium may have been of crucial significance.

Heisenberg and Döpel placed 750 kg of finely-ground uranium in two spherical shells, with heavy water between the two uranium shells and inside the inner shell. The whole device was surrounded by an exterior housing consisting of two half-spheres of aluminium, screwed together tightly. In spring 1942, this device showed for the first time that a greater number of neutrons penetrated the aluminium housing than the number fed into the centre of the apparatus using a radium-beryllium preparation, but still no chain reaction. It was hoped that one could be achieved simply by enlarging the reactor, however. Heisenberg's calculation estimated that five tonnes of heavy water and ten tonnes of uranium metal were needed.

The fact that the military no longer thought it justifiable to invest in nuclear physics research did not mean that the political policy makers, i.e., the Nazi regime or Hitler himself, had lost interest. In practice, the decision lay with the Minister for Education, *Albert Speer* (1905-1981), in whom Hitler had full confidence and who had total control of the German industrial resources. Several industrialists and military bigwigs had told Speer that Germany's only hope of winning the war now lay in superior new weapons and that Speer ought to investigate what the physicists had in mind when they talked about an atomic bomb.

Speer now began to intervene in the development. Owing to complaints about the National Research Council's thrift with grants and materials, he convinced Hitler to let the Council come directly under Göring, a touch that simultaneously flattered the latter and gave the Council greater power. Speer also convened a meeting in Harnack Haus in Dahlem, the house where Szilard had once lived. The meeting was advertised as being on 4 June 1942 and its purpose was to bring together the military and scientists. Hosts of the meeting were President of the Kaiser Wilhelm Society *Albert Vögler* and Secretary of the same, *Ernst Telschow*. Those invited included technologists such as *Ferdinand Porsche* (1875-1951) and military bigwigs, including Generals *Friedrich Fromm* (1888-1945), *Emil Leeb* and *Erich*

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[*] According to Riehl's memoirs, Degussa (Deutsche Gold und Silber Scheideanstalt) had acquired Auer after Hitler had come to power. Auer's Jewish owner, Leopold Koppel, had been forced by the Nazis to sell the company to the "Aryan" owners of Degussa.

Schumann, Field Marshall *Erhard Milch* (1892-1972) plus a few admirals. The scientists included Werner Heisenberg, Otto Hahn, Fritz Strassmann, Carl Friedrich von Weizsäcker, Erich Bagge, Walther Bothe, Klaus Clusius, Manfred von Ardenne, Arnold Sommerfeld, Kurt Diebner, Paul Harteck, Georg Joos and Wilhelm Groth.

At the meeting, which discussed a number of military physics matters, Speer asked the direct question: 'How can nuclear physics be applied to the manufacture of atom bombs?' Heisenberg's response was that the principle for the bomb and the scientific bases were known, so there was nothing to prevent an atomic bomb being built. But it would take several years and be extremely costly.

Field Marshall Milch asked: 'How big must a bomb be in order to reduce a large city like London to ruins?' Heisenberg held up his hands and cupped them around an imaginary bomb. 'About as big as a pineapple,' he responded.

Milch then asked how long it would take for the Americans to produce a reactor and an atomic bomb. Heisenberg's response was that if they gathered all resources, it would take at least one year to get a reactor working and another couple of years to produce an atomic bomb. Not until 1945, he believed.

Speer had heard there was a risk that something could go wrong, enabling a chain reaction to spread throughout the whole of the globe and turn it into a glowing star. Could Heisenberg guarantee that no such thing could happen? Heisenberg was unable to give any guarantees.

Speer finally asked how his Ministry could help scientists in their work. Heisenberg said that they needed money, new buildings and access to some material of which there was a shortage. Speer asked the scientists to specify a sum. von Weizsäcker said 40 000 Reichsmark, a drop in the ocean in this contest. Milch and Speer looked at one another and shook their heads with pity 'at the unworldliness and naivety of these people'.

This would have been the right time for Heisenberg and the other scientists to propose a massive investment in the atomic bomb had they really wanted to, but they evidently did not want to or did not dare to get involved in a project that was so large that failure would have led to serious consequences for those responsible. But the physicists got what they asked for and more into the bargain. On 23 June, Speer gave a report of the meeting to Hitler as point 16 of a long agenda. In his notes, he wrote: 'Reported to the *Führer* about the meeting regarding nuclear fission and the support we have given'.

In his memoirs, Speer writes:

> Hitler had sometimes spoken to me about the possibility of an atom bomb, but the idea quite obviously strained his intellectual capacity. He was also unable to grasp the revolutionary nature of nuclear physics. In the twenty-two hundred recorded points of my conferences with Hitler, nuclear fission comes up only once, and then is mentioned with extreme brevity. Hitler did sometimes comment on its prospects, but what I told him of my conference with the physicists confirmed his view that there was not much profit in the matter. Actually, Professor Heisenberg had not given any final answer to my question whether a successful nuclear fission could be kept under control with absolute certainty or might continue as a chain reaction. Hitler was plainly not delighted with the possibility that the earth under his rule might be transformed into a glowing star. Occasionally, however, he joked that the scientists in their unworldly urge to lay bare all the secrets under heaven might some day set the globe on fire. But undoubtedly a good deal of time would pass before that came about, Hitler said; he would certainly not live to see it.

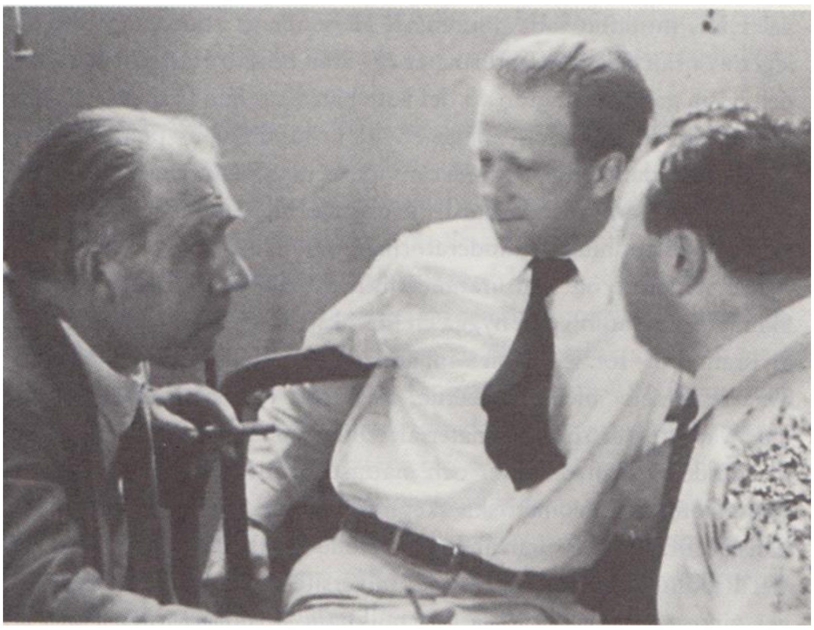
When Heisenberg came back to Leipzig after the meeting in Harnack Haus, an uncomfortable surprise awaited him. The experimental reactor, which was entirely submerged in a water tank, began to give off bubbles. Professor Döpel found that the content of the bubbles was hydrogen gas, which indicated that water could have penetrated the uranium powder through a leak and reacted with it.

Döpel decided that the reactor tank should be lifted out of the water tank so the seal could be opened to see how much water had penetrated. But when the seal was released, it was shown that underpressure had occurred inside the reactor sphere and air had been sucked in. The air ignited the uranium powder and a long flame shot out through the aperture. Döpel then poured water over the sphere and succeeded in putting out the flame. He then pumped out the expensive heavy water and an assistant screwed the half-spheres tightly back together. At that time, Heisenberg arrived at the laboratory, summoned in haste from a seminar. The reactor sphere was now quite hot and Heisenberg decided that it should be lowered back down into the water tank to cool off.

This was a mistake. Uranium powder splits water into hydrogen and oxygen, i.e., detonating gas, while releasing heat. The water in the large tank was heated to boiling point and the submerged reactor sphere shook ominously and appeared to swell. Heisenberg and Döpel rushed out of the room at the last second.

A violent detonation gas explosion tore open the reactor sphere and slung uranium powder up to the ceiling where it caught fire in contact with the air. Döpel called the fire brigade, who arrived after eight minutes but who needed two days to completely extinguish the furious uranium fire. All of the uranium and the vast majority of the heavy water were wasted in the world's first 'reactor accident'. To crown it all, Heisenberg and Döpel had to be content with being congratulated by the fire station officer for 'a fine display of explosive 'atomic fission'.

Döpel later wrote a bitter letter to Auer and reproached them for not having warned him of the insidious properties of uranium. The company's very brief response through Dr. Riehl was that the tendency of uranium to react with air and water was well-known to every chemist, and that if in spite of everything the physicists were ignorant of this, they ought to have looked at a circular sent out by Degussa a year ago, warning of these very consequences. Döpel's mood did not improve by having had his fingers rapped, and it took until 1945 for him to reconcile with Riehl. They were then both prisoners of war in Moscow and were ordered to assist with the Russian atomic energy programme.



Niels Bohr, Werner Heisenberg and Wolfgang Pauli

Diebner being forced to give up control of the research in Berlin-Dahlem did not mean that he abandoned uranium research; he was able to continue by himself in Gottow. There, he made some of his most significant achievements when he no longer needed to take into account the academic scientists.

His intuition told him that Heisenberg's device with stratified layers of uranium and moderator material – Harteck's idea from the start – did not have the best geometry and it would be better to allow the uranium to enter as cubes rather than plates. Heisenberg's stubborn use of plates was justified by the fact that such a geometry facilitated calculations. It was the same justification that had irritated Szilard to beyond measure when Fermi wanted to work with a homogenous mixture of moderator material and uranium.

Diebner had insufficient uranium metal but had good access to uranium oxide. In summer 1942, he had an experimental reactor built consisting of 2 802 cubes of uranium oxide – a total of 25 tonnes - embedded in 4.4 tonnes of paraffin with a distance of 2 centimetres between the cubes. The reactor was lowered into a concrete trough containing water as a reflector. Different channels led into the reactor for the insertion of neutron sources and measurement instruments. Diebner's reactor was the first of its geometrical shape to look like the reactor that the American scientists would get to function six months later.

Unlike the physicists at the university institutes, Diebner strove to implement all conceivable protection measures. His technologists were attired with special protection clothing and repeated blood samples were taken to ensure that no harmful radiation doses had occurred.[*] The level of radiation was also carefully checked through physical measurements. In this connection, Diebner can be said to have run the first health physics activity at a nuclear reactor in history at the same time as Dr. Stone in the United States was given the task of organising the protection operations for Arthur Compton's Metallurgical Project. The ambition to maintain good radiation protection ought not to be detracted from by the circumstance that the reactor never gave rise to any radiation.

While Diebner was experimenting in Gottow, Heisenberg was performing experiments with uranium powder and paraffin in the Virus House in Berlin-Dahlem. 1942 was drawing to a close. In December the American physicists had made up for the head start that the Germans had. On 2 December, Fermi succeeded in achieving the world's first controlled chain reaction of nuclear fissions in the squash courts in Chicago.

In Hamburg, Paul Harteck had followed the development with mixed feelings. In his opinion, since the rapid start in 1939, the physicists in Leipzig and Berlin had not been particularly efficient. As for himself, Harteck had not just been responsible for the original initiative; he had also come up with practical advice on how the uranium and the moderator ought to be distributed in a reactor. He had almost got a reactor to function using his unconventional idea of dry ice. He had succeeded in getting *Norsk Hydro* to provide a tenfold increase in its heavy water production. He had made great efforts to get the Clusius-Dickel method for separating uranium to work. He had trained a colleague who had good ideas on the way in which uranium-235 could be separated using ultracentrifuges. Harteck was behind near enough every idea of practical value. He had all the qualifications to successfully produce a German atomic bomb but he was not given any wholehearted support due to general rivalry and academic complacency. And maybe it was just as well.

Harteck's colleague, Wilhelm Groth, was the person who had drawn attention to an American description of an ultracentrifuge. Groth had contacted *Anschütz & Co.*, a company in Kiel specialising in gyroscopes. The firm had already received drawings of a centrifuge on 10 October 1940. However, it had encountered difficulties in finding material suppliers – the rotor of the centrifuge had to be able to tolerate very substantial centrifugal forces and the work had thereby been delayed. They did manage to complete the first prototype in April 1942, but it exploded when it was turned up to a high speed. A new attempt one month later with a smaller rotor led to the same discouraging result. However, in June 1942, enough progress had been made for them to be prepared to try uranium hexafluoride for the first time. The intention was for the centrifugal forces to separate molecules with different masses to thereby enable the enrichment of the uranium in uranium-235. Harteck thought that this project was more important than that of building a bunker for a reactor that could not yet be completed owing to a lack of heavy water.

Harteck thought that if the gas centrifuge fulfilled its potential, they would be able to build a reactor with uranium that was slightly enriched with regard to uranium-235, and ordinary water would then be sufficient as a moderator. Heisenberg had calculated that enrichment to 11 % uranium-235 (as opposed

[*] Blood tests as routine checks for the same purpose as dose measurements became the fashion for a couple of decades before it became clear that they simply said nothing about the individual person's radiation exposure apart from the fact that the radiation doses had been very high. The first critical views of the method were put forward by Robert Stone, including at an ICRP meeting in Stockholm in 1952, and at the start of the 1960s, the general view became that the routine blood tests could sooner lull the personnel into a false sense of security.

to 0.7% in natural uranium) would be enough to allow the use of ordinary water rather than heavy water. We now know that a significantly lower level of enrichment is adequate: in light water reactors, it is between 2.5 % and 3.5 %.

Following an initial attempt to centrifuge uranium hexafluoride in August 1942 (with enrichment to 3.9%), Harteck suggested a number of improvements to the centrifuge. He then appealed to Esau for substantial support for the centrifuge project and Esau brought the case to Göring. As a result, *Anschütz & Co*. was asked to build a new prototype as per Harteck's instructions. By this time, Göring's interest would no doubt have increased considerably and the inputs have been proportionally increased had Esau forwarded Harteck's points of view on the possibility of achieving an atomic bomb. But Esau was cautious and confided in his colleagues that it would be dangerous to give Göring such ideas. He would then probably issue brusque orders for the physicists to produce a bomb within a prescribed time - and what would happen if they failed to succeed?

Degussa's uranium production continued according to plan from 281 kg of pure uranium metal in 1940 to 5 602 kg in 1942, but in summer 1942 there was still insufficient access to heavy water. The Allies were aware of the German interest in the heavy water, but they did not know what the crucial significance of the access to heavy water was. An attack on Rjukan on 19 November 1942 failed and revealed to the Germans that their interest in heavy water was no secret. The security measures at Vemork were fortified.

When the 'Arbeitsgemeinschaft Kernphysik' (established by the military) was transferred to the National Research Council and placed under Esau's management in December 1942, the latter was asked by Göring to devote himself to four assignments:

1. Nuclear physics research for the purpose of extracting nuclear energy from uranium.
2. Production of luminescent paint without using radium.
3. Production of sources for fast neutrons.
4. Safety measures for when working with neutrons.

The atomic bomb was thus not mentioned and Göring's opinion, if he ever gave any thought to the problem, was probably that it was a utopia in which it was not worth investing resources. For the scientists on the other hand, it was both an attractive and a disturbing possibility, disturbing bearing in mind that the initiative in question could result in the Nazi leaders making someone responsible were the results to fail.

Esau did not have an easy time of it. The Kaiser Wilhelm Society refused to allow its institutions to submit to his directions. The President of the Society, Dr. Albert Vögler, did not want to allow Esau to determine the distribution of the available funds. At the same time, the Vemork factory at Rjukan was expected to produce the necessary quantities of heavy water.

However, the Allies had no intention of allowing the production in Vemork to continue eastwards. On the night between the 27th and 28th February 1943, a group of saboteurs exploded the electrolysis cells in the Vemork factory, which meant that 350 kg of heavy water was flushed away. The production was unable to resume until 17 April, and the plant was not ready to resume the production of heavy water until June.

Faced with these problems, the Germans began to seriously look at other possible ways of producing heavy water rather than relying solely on the vulnerable Vemork. There were two hydrogen electrolysis plants in Italy, near Merano and Cotrone. Maybe they could be used for initial heavy water enrichment, whereupon a German factory could continue the enrichment. Harteck and Esau had inspected the plant in Merano but Esau was doubtful as to whether he would dare put his money on this alternative.

At the end of March 1943, the War Department made a definitive withdrawal from research into uranium and refused to invest more money. However, Diebner was promised that he could still use the Gottow laboratory, but the whole of the uranium research now had to be paid for from Esau's budget which was established by Göring as 2 million *Reichsmark* for the 1943-1944 budget year. The main entries in the budget concerned the production of 10 ultracentrifuges (600 000 RM) and an experimental plant for the production of heavy water in Germany (560 000 RM).

Diebner was not disheartened by the lack of military support. He was anxious to continue his experiments with uranium in cubic form, but this time with heavy water as a moderator. Bearing in mind Heisenberg's and Döpel's detonating gas explosion, he realised that the uranium had to be encapsulated so as not to come into contact with the water. The encapsulation problem was not solved, however, and it was also conceivable that the housing material, e.g., aluminium, would have a negative effect on the properties of the reactor. Inspired by Harteck's attempt with dry ice, Diebner decided to use heavy water in solid form, i.e., heavy ice.

This experiment was arranged at the National Institute for Technical Chemistry in spring 1943. According to Diebner, the sides of the uranium cubes ought to have been 6.5 cm but had to be made to 5 cm for practical reasons during production. A total of 232 kg of cube-shaped uranium was distributed in 210 kg of heavy ice, all embedded in a paraffin sphere that was 75 centimetres in diameter. The result was a surprisingly good one. A better neutron multiplication was achieved than the one achieved by Heisenberg and Döpel in their potentially explosive Leipzig experiment. Diebner thought that an increase in the quantity of uranium and heavy ice would doubtless have made the reactor critical. He now thought he had full proof that the cube-shaped uranium blocks were more appropriate than the flat or spherical plates that Heisenberg was still working with.

There was a general, widespread idea among the German technicians and politicians that for as long as they themselves had not solved all the problems, nor could this be true of the Allies. On 8 July 1943, Rudolph Mentzel wrote the following letter to a Dr. Görnnert in Hermann Göring's Secretariat:

> Dear party brother Görnnert:
>
> I hereby enclose a report from State Counsel, Professor Dr. Esau, authorised representative for nuclear physics, on the current stage of the work with a request that the *Reichsmarschall* be informed of this. As you can see from the report, the work has advanced quite considerably in just a few months. Although the work will not lead to the production of practically useable machines or explosives in the near future, it does provide assurance that the enemy powers can have no surprises in store for us within this area.
>
> With best wishes
> Heil Hitler!
> Yours, MENTZEL

When this was written, those in the United States were no further on than the process of building the production reactors in Hanford.

One major problem for Germany's 'enemy powers' was the effectiveness of the German submarines. It had been a relief for the United Kingdom when at the end of 1941 the United States, although the country had not yet entered the war, provided assistance through lend-lease agreements that made it possible for the United Kingdom to import crucial necessities from the United States without having to overspend on the dollar front. The American Navy then began to protect British convoys over the Atlantic. In October 1941, a German submarine sank *USS Kearny*. In reality, the United States was at war on the Atlantic long before the Japanese attacked Pearl Harbour. When Hitler attacked the Soviet Union in June 1941, a new convoy route was opened from Iceland to Murmansk. In December 1941, Hitler declared war on the United States when the Japanese attacked Pearl Harbour and a full-scale fight began on the Atlantic. The Americans had no particular experience of submarine attacks and were not equipped to face them. German submarines sank 360 merchant ships in the American coastal waters during the first six months of 1942.

The German dominance of the Atlantic was broken thanks to a British invention - the centimetric radar. Radar (an acronym from *ra*dio *d*etection *a*nd *r*anging) or 'radio echoes' was not a new invention. Following the disaster of the Titanic in 1912, many ideas had come up for how to detect icebergs in good time. Several of these ideas were based on the registration of the echo that radio waves would create. The first experiment with radio echoes had been done in 1903 by German *Christian Hulsmeyer*. Radio echoes were used in the United States by *Gregory Breit* (1899-1981) in 1925 and the previously-mentioned Merle Tuve to measure the height of the ionosphere. In the United Kingdom, a chain of radar transmitters called Chain Home had been built to detect aircraft. The chain became very important during the war. Each transmitter had a power of 250-300 kilowatts, a high output for a short-wave transmitter at the time. The wavelength was 10-15 metres, i.e., the frequency was 20-30 megahertz. The Germans had corresponding transmitters along the coast but with a lower output and thereby a shorter range; they used a wavelength of 2 metres.

The British Navy was also equipped with radar equipment on several warships but the problem was that they were so bulky. However, Admiral *Andrew Cunningham* (1883-1963), the head of the British Mediterranean Fleet had room for the radar on his battleships and found a use for it when he conquered the Italian Navy outside Cape Matapan in a famous naval battle on 29 March 1941. However, the equipment was too large for it to be used in aircraft.

The situation changed when a group of British scientists developed a special electron tube for the generation of microwaves, the magnetron. These microwaves had wavelengths of around 1 centimetre and the equipment could be used in aircraft. This made it possible to use the radar to detect the German submarines when they surfaced at night to charge their batteries using diesel engines or to prepare for an attack.

The centimetric radar was probably the one single invention that contributed the most to the outcome of the war in Europe (which is something that the atomic bomb never ended up doing). In May 1943, Admiral *Karl Dönitz* (1891-1980) put in an appearance to see Hitler in Berchtesgaden and reported that the fight on the Atlantic was going the way of the enemy. According to Arnold-Forster, Dönitz said:

> What is now decisive is that enemy aircraft have been equipped with a new location apparatus [...] which enables them to detect submarines and to attack them unexpectedly in low cloud, bad visibility or at night. If enemy aircraft did not have such a location apparatus, it would not be able to detect submarines in rough sea or at night, for example. The largest number of submarines now being sunk is being sunk by aircraft.

Dönitz had lost the advantage. The Allies did not lose one single vessel due to German submarines in the Atlantic during the summer of 1943. But the activities of the German submarines had previously been devastating. They sank 2 828 allied merchant ships and 45 warships.

It is remarkable that a number of the British scientists like John Cockcroft and Mark Oliphant who made substantial achievements in the development of nuclear weapons also had time to play leading roles in radar research.

In summer 1943, a report was written by two Heidelberg scientists, Professors *H. Pose* and *E. Rexer*, on the most suitable configuration of uranium and moderator. They concluded that the mass of uranium and moderator ought to be approximately the same and that the best distance between l cm-thick uranium plates in heavy water would be 20 cm. However, they did support Diebner’s view that cubes of uranium were superior to plates of uranium. However, since it would be difficult to arrange the diversion of heat from the uranium cubes, they suggested that a compromise ought to be made by using uranium rods (as is the case now).

Despite this advice, Heisenberg insisted that the large reactor that would eventually be tested in the Dahlem bunker would have uranium in the form of plates, still mainly justifying this with the fact that it was much easier to carry out the theoretical calculations for plates.

However, the plates were not easy to produce. Auer also had trouble finding a suitable form of housing. Auer’s technicians did not succeed in finding a method using enamel phosphate until November 1943, and the company began producing a series of uranium plates. Diebner then succeeded in getting Esau to convince Auer to also produce cubes that were coated with a protection layer of polystyrene at the same time as producing the plates.

Diebner rapidly built up an experimental assembly in Gottow with the new cubes. The cubes hung in wires in rows that formed a three-dimensional pattern. There should have been a total of 240 cubes but Auer had been unable to deliver more than 80. Each cube weighed 2.4 kg. Diebner had to make up the shortfall by adding 60 older cubes made of plates, each weighing 2.2 kg, so the 240 cubes therefore weighed 564 kg altogether. The whole assembly, hanging in their wires, could be lowered into a cylindrical aluminium tank which was lined with 4.3 tonnes of paraffin as a reflector and had room for the uranium and the heavy water. When Diebner replenished the weight of the uranium in heavy water, the device was shown to have had a 6 % increase in the added number of neutrons (i.e., a subcritical multiplication factor of l.06; cf. p. 194). This was beyond expectations and Diebner now waited impatiently for more heavy water and more uranium cubes, preferably with sides of at least 6 cm, rather than the 5 cm that Auer had already supplied, in order to really be able to initiate a chain reaction.

The rumour of German successes with uranium research spread to England and the United States, and in addition to the potential threat of an impending atomic bomb, there was concern about the radioactive substances that could be produced if the Germans were to get a reactor to work. On 1 July 1943, James Conant wrote:

'It now seems extremely probable that it will be possible to produce by means of a self-sustaining pile large quantities of radioactive materials with varying half-lives of the order of magnitude of twenty days'. In 1944, Conant thought it was definitely possible that the Germans would initiate a heavy water reactor that could produce the radioactive substances in activities corresponding to one tonne of radium every week. With such radioactive substances they could contaminate a city the size of London to the extent that it would have to be evacuated.

And nor did this statement do much for the mood of the Englishmen. *Sir John Anderson* (born in 1882), Churchill's Minister of Finance and also the government member who was responsible for the British nuclear energy programme, discussed the gloomy prospects with Conant at the Cosmic Club in Washington DC in August 1943.

The possibility did not involve anything new. The committee established by the American Academy of Sciences in spring 1941 under Arthur Compton to look at the uranium matter had already touched on the possibility of using 'radioactive toxins' in its first report in May 1941. A special report on the matter had then been submitted on 10 December 1941 by Eugene Wigner and *Henry Smyth* (1898-1986), pointing out that the fission products that are formed during one day's operation of a 100 MW (thermal output) nuclear reactor could be used to make the large areas uninhabitable. Wigner and Smyth did not suggest any such use, however.

During the discussion of this problem in England, the British Medical Research Council was particularly concerned about the genetic impacts of ionising radiation. These were mainly known through H.J. Muller's research with fruit flies. The Germans also showed considerable interest in these possibilities, and the head of the Kaiser Wilhelm Institute for Biophysics, Russian-born Professor Boris Rajewsky, received special research funds to investigate 'the biological impacts of particle radiation, especially neutrons, with regard to the possibility of using them as weapons', so even the Germans were afraid that the enemy would come up with the idea of spreading radioactive substances. So, both camps were worried about a weapon that no-one had seriously considered using.

However, the concern lasted so long that on 23 March 1944, General Groves warned General Marshall that the invasion forces in Normandy could find beaches that were contaminated with radioactivity. This resulted in the Allies' medical director giving orders for all blackening of photographic film to be reported immediately. The fear turned out to be unfounded, however.

In Norway, the Managing Director of *Norsk Hydro*, *Bjarne Eriksen*, made the bold decision to propose that his board refuse to supply Germany with heavy water, his justification for this being that the heavy water seemed to be of such great interest to the military that it increased the risk of allied bomb attacks on *Norsk Hydro's* plants. However, before there was time to make any decision, Eriksen was arrested and sent to a prisoner of war camp in Germany. The unwillingness on the part of the Norwegians did mean, though, that the Germans promised not to demand supplies of heavy water that were greater than supplies corresponding to secondary products in connection with the normal production of ammonia.

In autumn 1943, the Nazis made a definitive decision to root out the Danish Jews. At the start of October, it was established that 6 000 Danes would be arrested for deportation to German concentration camps. The majority were pre-warned and succeeded in fleeing over Öresund to Sweden at night, among them Niels Bohr. On 6 October, Bohr continued his journey by flying to London. He was picked up by a British Mosquito bomber which was completely unarmed to be able to land in neutral Sweden. Bohr was put into the area where the bombs would be stored, supplied with a parachute, and was told that he would be released like a bomb if the plane were attacked. Because the plane had to fly at a very high altitude for safety reasons, Bohr had been given oxygen equipment which he would use as instructed through the headphones that he was also given. However, Bohr's skull was so large that the headphones did not reach his ears. He therefore missed the request to use the oxygen and soon lost consciousness. However, when the pilots failed to gain any contact with him, they lowered the altitude of the plane and Bohr regained consciousness. Later in London, he was able to tell the English that the Germans probably intended to produce an atomic bomb, and he told them of his talk with Heisenberg. In December 1943, after two months in England, Bohr continued with his son *Aage* (1922-2009) to the United States. He was sent to Los Alamos where he was able to inform Oppenheimer and the most prominent physicists of his talk with Heisenberg in 1941 and show the latter's diagram of a German reactor.

The Allies were becoming more and more concerned about the deliveries of heavy water from Vemork. They had initially thought that the sabotage in February 1943 had deprived the Germans of around two years of heavy water production, but towards the autumn, it was gradually realised that the interruption to the operation had lasted just a few months. General Groves called for drastic measures so as not to fall short in the race for the bomb. In November 1943, the joint Chiefs of Staff group decided that the American 8th Air Force stationed in England for precision bombing should be engaged. On 16 November 1943, no fewer than 140 'Flying Fortresses' (Boeing B-17) from the 3rd flying division's 100th bomb group engaged in a 33-minute attack on Vemork, whereupon an additional 15 bombers attacked Rjukan itself. More than seven hundred 500-pound bombs were dropped over the Vemork factory and one hundred 250-pound bombs over Rjukan. Three bombs hit the hydroelectric power station's pipelines and four bombs hit the power station itself. Two hit the electrolysis factory. Twenty-two Norwegians were killed. And there was still no damage to the heavy water plant in the basement of the Vemork factory.

However, with the power station destroyed, the undamaged enrichment facility for the heavy water could not be run, so the operation had been successful from this point of view. However, the Norwegian government-in-exile had not been consulted and it protested strongly. It did not understand the reason for the attack and suspected the Americans of being influenced by commercial interests bearing in mind the competition situation after the war. The government maintained that 'the action against the heavy water could have taken place with far greater care and also with greater effect'.

It now seemed as though there was no alternative for the Germans other than to build a heavy water plant in Germany. An experimental plant code named 'The Stalin Organ' had already been built by *I.G. Farben* at Leuna outside Merseburg (west of Leipzig) according to a method instructed by Harteck and Suess. Based on their experiences of the experimental plant, the Leuna plant's technicians were able to inform them that a full-scale plant would cost 25 million *Reichsmark* and consume 50 tonnes of lignite per hour.

Professor Esau realised that the plant was necessary but still had doubts as to the size of the project. Malicious tongues were already calling the uranium research a waste of time, something with which the physicists duped the politicians in order to be able to sit undisturbed in their laboratories while all others were risking their life in the war. Every big step forward in the form of important and expensive decisions involved the decision-maker taking on more responsibility and possibly coming to grief on the day when accounts were demanded. If any part of the project concerning the enrichment of uranium-235 succeeded, Esau would have difficulty justifying an unnecessary expense of 25 million Reichsmark to facilitate a heavy water reactor.

In Gottow, Diebner's impatience grew as he waited for the heavy water. It was no great consolation in the current situation that Esau had already taken up 800 000 Reichsmark of the budget for an additional smaller heavy water plant in Germany, and that he was also prepared to propose a contract with I.G. Farben for an enrichment plant for 1.5 tonnes of heavy water per year based on slightly enriched water from Italy.

While Diebner waited for sufficient quantities of heavy water, Heisenberg had been able to take over the water that Diebner had used in his previous experiments. But Heisenberg had not received all of the uranium plates he needed. Degussa was not producing uranium at the anticipated rate for various reasons.

These setbacks and Esau's difficulties with making decisions meant that in October 1943 it was hinted that he ought to step down from his post as research administrator. When Esau had protested to no avail, on 2 December 1943 Göring appointed Professor *Walther Gerlach* (1889-1979) from Munich (who was five years younger) as his successor. Gerlach had no experience whatsoever of uranium research and had last been involved with research concerning the magnetic triggering of torpedoes. After the war, he claimed that his objective as administrator of the uranium research had primarily been to prevent German physicists from being called up for frontline service. But Gerlach was an ambitious man and no doubt also wanted to succeed, whatever success could now mean in that context.

In order for the research to be able to continue without major delays, it was necessary to save the 614 litres of heavy water that remained in Vemork after the bombing for Germany. With that quantity, either Heisenberg or Diebner would have been able to perform a successful criticality experiment.

The heavy water was moved by rail from Vemork down to Tinnsjön where the goods wagon would continue to be transported on a ferry along the lake to roll down towards a harbour from the southern end of the lake to be shipped to Germany, but the heavy water never came arrived. Three Norwegian saboteurs were assigned to place an explosive charge with a delayed-action trigger on the ferry. At 10.45 in the morning of Sunday 20 February 1944, 45 minutes after the ferry had started its journey, the explosion took place. The ferry capsized, the goods wagons rolled overboard and the barrels containing the heavy water sank in the very deep mountain lake. Four Germans and fourteen Norwegians lost their lives. Twenty-five civilians and four German soldiers were saved by Norwegians who were winter fishing on the lake.

The rumour that subsequently circulated said that the barrels had contained only ordinary water and that the heavy water had been secretly conveyed to Germany in another way. But in 1993, a couple of the barrels appeared to have been recovered from a depth of 430 metres and really were shown to have contained heavy water. After the war, Kurt Diebner said in an interview (according to Rhodes):

> When one considers that right up to the end of the war, in 1945, there was virtually no increase in our heavy-water stocks in Germany […] it will be seen that it was the elimination of German heavy-water production in Norway that was the main factor in our failure to achieve a self-sustaining atomic reactor before the war ended.

However, Diebner underestimated the quantity of heavy water that could have been used had the cooperation between the German scientists been better organised. From 1939-1944, a total of 2 840 kg of heavy water was delivered from Vemork, 185 kg of which Professor Joliot received in 1939 while the Germans took the remainder. Some had been lost when Heisenberg and Döpel saw their reactor explode in the summer of 1942, but approximately 2.5 tonnes of heavy water must have been found to be available in Germany in spring 1944. But not one of the German scientists had had the whole of this quantity available for a coordinated experiment.

Gerlach had now taken over Esau's problem. Would he invest enormous sums in a large heavy water plant? Or would he prioritise one of the methods for the separation of uranium-235? Would he divert all available uranium and heavy water to the reactor bunker in Dahlem for an experiment under Heisenberg's management? Or would he rely on Diebner instead?

Like Harteck, Diebner was an exuberant and inventive man with a practical but also unconventional streak. In spring 1944, given the lack of the heavy water that he needed, he was encouraged by Gerlach to start playing with completely new thoughts – those of the hydrogen bomb, using the fusion process rather than fission, nuclear fission. The discovery of fusion, in principle just as important as fission, had already taken place in 1934 in Ernest Rutherford's laboratory when Rutherford, along with Mark Oliphant and the guest researcher Paul Harteck, had observed an unexpectedly high energy development when heavy hydrogen nuclei were accelerated towards a target of heavy hydrogen. However, this discovery did not lead to a commotion anything like the one caused when Hahn discovered fission.

In order to bring about the fusion of atomic nuclei, the participating nuclei both need to have very high speeds to be able to penetrate one another. This involves temperatures that are so high that they occur only in stars such as our sun. Such a thermonuclear reaction on the Earth could conceivably only be triggered by the explosion of a nuclear charge based on nuclear fission.

With no atomic bomb to act as a detonator, Diebner made a few desperate attempts with shaped charges, which were in fashion in the Los Alamos scientists' attempt to construct an implosion bomb and among the Swedish physicists in Rolf Sievert's military physics organisation. In Gottow, Diebner experimented with trinitrotoluene rods that were supplied with a conical cavity that was filled with paraffin containing heavy hydrogen. The paraffin also contained silver foil that would become radioactive if it were hit by any neutrons from fusion processes upon detonation. The intention was to measure the activity of the silver following the explosion. If activity was evident, a thermonuclear reaction would have been achieved. In practice, it was not possible to measure the silver – it disappeared at the time of the explosion.

On the next attempt, a spherical, silver shell 5 cm in diameter was used. The sphere was filled with heavy hydrogen. Trinitrotoluene was placed around the sphere. The pressure wave in towards the sphere was incredible at the time of the explosion; the silver melted and the melted silver compressed the hydrogen so that the kinetic energy was briefly concentrated in a small sphere of glowing hydrogen exposed to extremely high pressure and temperature. The procedure corresponded to the one that the Americans used for the implosion bombs using plutonium, but the silver remained inactive. Making a hydrogen bomb was a considerably more difficult task.

However, the possibility that a specially-arranged explosion of conventional explosives could trigger a thermonuclear reaction remained in the minds of the scientists for a long time. When the Berlin bombings continued to intensify, a rumour arose that the largest bomb craters could have been created by bombs that had been fortified with heavy hydrogen. The rumour was further fuelled by the interest demonstrated by the Allies through the extensive bombing of Vemork. Could heavy hydrogen be even more important than they had initially thought?

At Gerlach's request, a number of technicians equipped with Geiger counters examined the bomb craters in Dahlem but the result was negative (or maybe we should substitute that word for positive): Berlin had not been hit by any hydrogen bombs. All it would have taken was for people to have seen how things would have looked in their mind's eye if Berlin had been hit - there would have been no need to take any measurements.

Like Esau, making decisions was not one of Gerlach's strengths. He dared not have the resolve to rely wholeheartedly on either Heisenberg or Diebner. Diebner was Gerlach's favourite but was underrated by the other physicists, partly because he was not a lecturer. However, Gerlach found Diebner's calculations of the best configuration of the uranium in a reactor to be so advanced that he proposed Diebner for a post as an assistant professor at the Technical University of Berlin. However, this came to nothing due to strong protests from the established academics and particularly from the group surrounding Heisenberg.

Gerlach would have preferred to have relied on Diebner but dared not look past Heisenberg. Whoever he chose, he risked criticism and he therefore did not dare to make a choice. The result was that neither Diebner nor Heisenberg received adequate resources for a successful experiment which both of them had the conditions to carry out.

Gerlach also soon found that his assignment differed in many other respects from what he had been expecting thus far. On one occasion he received a midnight visit (or telephone call - the versions vary) from an SS General who asked him whether he knew where Niels Bohr was. Bohr had to be liquidated immediately. Gerlach, alarmed, was relieved to respond that Bohr was probably safe in London, whereupon the SS General burst out with: 'Excellent!' In London, he said, the Nazis had particularly capable agents and a murder there would arouse less attention than it would in a neutral country.

A dismayed Gerlach objected that an attack on Bohr would simply damage Germany's reputation. The SS General retorted that if Gerlach thought that one human life was worth all that much, it would not be long before he started to think otherwise. Luckily, by this time Bohr was already in Los Alamos under the name of Mr. Nicholas Blake, and Gerlach succeeded through his own SS contacts in the Ministry for Foreign Affairs in preventing any retribution from affecting Bohr's other colleagues who were still in Copenhagen.

In Berlin-Dahlem, the Kaiser Wilhelm Institute's personnel were under pressure from major difficulties. As a consequence of the bombing of Berlin, a large section of the Institute had been evacuated to Hechingen, south of Stuttgart. At the end of May 1944, Gerlach was still able to report that they were preparing themselves for the major, definitive reactor experiment in the Dahlem bunker. Auer had finally found a suitable encapsulation method. The uranium was dipped in an alkaline cyanide solution, thereby giving it a satisfactory surface layer to protect the metal against the water.

The assembly for the reactor in the Dahlem bunker was impressive. In the middle of the bunker there was a large circular pool. Above it was a telpher device on a track in the ceiling. The room was equipped with extra ventilation to rapidly suck out radioactive gases. Enamelled steel tanks lay in wait for the heavy water. The laboratory was equipped with devices to remotely control the uranium and there were inspection holes through which the reactor could be observed from behind shields. The reactor room was joined to adjacent rooms through double, airtight steel doors. Four years after Schumann had first attempted to collect the German uranium research under a common roof in Dahlem, it now really did look as though one common physical effort were possible.

The reactor had been built up under the leadership of Karl Wirtz, but Heisenberg's influence was such that uranium plates were still being used. The definitive experiment would take place in May 1944. Following a number of preparatory attempts, a distance of 8 cm between the 1-cm thick uranium plates was used. These were fitted horizontally in a cylindrical tank, 124 cm tall and 124 cm in diameter. The tank had been supplied by *Bamag-Meguin* in Berlin and was made of a magnesium alloy. The experiments took place using 1.5 tonnes of heavy water in the tank and quantities of uranium that could vary between l and 2 tonnes.

The experiment failed due to the unsuitable geometry. Despite the expensive equipment in Dahlem, the development went no further. The attempts to build a functioning reactor had no direct link to an atomic bomb programme – the primary motivation for the scientists in Dahlem was the same desire that had previously driven Fermi in New York: to be the first to produce a controlled chain reaction. Indeed, the interest of the policy makers was kept above water with vague words about nuclear explosives and the fact that the use of plutonium was known to be a possibility, but no serious interest was devoted to the bomb.

Others did though, primarily Diebner's group, in spite of the fact that the military leaders were no longer pinning their hopes on a nuclear weapon. Diebner's first colleague, Erich Bagge, had already stated a principle for the enrichment of uranium-235 in 1940. Bagge's apparatus, which ended up being called an 'isotope sluice', consisted of a device in which a high-pressure jet of uranium hexafluoride gas was aimed at a system of rotating plates. Since different heavy molecules could be expected to move at different speeds and if the rapidly rotating plates were supplied with suitably-positioned vents, the hope was to be able to differentiate molecules with different speeds and therefore different masses.

In 1941, the firm called Bamag-Meguin had agreed to build an initial prototype, but this and a subsequent one had been destroyed when the plant was bombed. However, on 10 July 1944, it was possible to start experiments with a third prototype in Bamag's plants in Butzbach near Frankfurt. The apparatus was kept running for six days and produced 2.5 grammes of highly enriched uranium hexafluoride during this time. However, on 6 June 1944, the Allies had invaded Normandy and entered Germany. The bombings were intensified and it was necessary to disassemble Bagge's isotope sluice and take it to Hechingen. Germany had come by a technically-speaking fully useable method for enriching uranium-235 for the first time, but too late.

The continued experiments with Harteck's ultracentrifuge were less promising. They took place in Kandern close to the Swiss border in a building called 'Vollmer's Furniture Factory' and in a former textiles factory called 'The Angora Farm'.

In July 1944, Gerlach's home in Munich was destroyed during the fierce bombings. He then moved to the reactor bunker in Berlin but soon realised that research could not continue there for long because the intensity of the bombings was increasing. He therefore began making plans to move the uranium and the heavy water, although this would not take place until six months later.

Gerlach, who was a big lover of flowers, had become attached to the small Swabian village of Haigerloch in the hills just west of Hechingen. He had visited it once when the lilacs were in flower. One steep, craggy cliff sported a castle, a prison and a church. Beneath the cliff was a wine cellar, which Gerlach now recalled. He decided to use Haigerloch as his headquarters. The cellar beneath the rock was enlarged and equipped to be able to accommodate a reactor with the uranium and the heavy water from Berlin. The new laboratory was code-named the 'Speleological Research Unit' with reference to the cave in the hill.

In the summer and autumn of 1944, it was Germans' turn to be on the run. With this ongoing, Gerlach, supported by Schumann, endeavoured to help the German physicists to survive. Many people supported them. Professor *Carl Ramsauer*, chairman of the German Physical Society and head of research at A.E.G., emphasised that three thousand soldiers fewer or more would make little difference to the front but that access to three thousand physicists could determine the outcome of the war. Albert Speer was far from convinced but Göring, Bormann and Himmler were influenced by the warnings. In July 1944, Himmler averted a mass call-up of German scientists with the following letter to the immediate superior SS General:

> I have heard that the intention in the ongoing call-up notice (call-up operation SE-IV) is to include 14 600 of the personnel who are reserved for German military research.
>
> I order you to put an immediate end to these call-ups within the military research sector since I consider it insane to close down our scientific research.

On 11 August 1944, the electrolysis cells were moved from Vemork to Germany. Nine were installed in the Dahlem bunker and nine were forwarded to Haigerloch. The Leuna plant had recently been destroyed in a bomb raid.

The German scientists became increasingly anxious that Hitler would realise the possibilities offered by an atomic bomb and give a desperate order for a bomb to be produced in a short time. The rumours that an American atomic bomb was close to completion spread to Germany through various channels but there was not much to back them up and they were not taken seriously. In the summer, Göring's aide, Major *Bernd von Brauchitsch*, had visited Heisenberg in Berlin and asked whether such rumours could be true, hut Heisenberg's response was that he did not believe them. The German scientists had trouble imagining that anyone else could succeed with the thing that was causing themselves such major difficulties.

On 9 August 1944, the 'Alsos' experimental group arrived in France with great powers at their disposal. The group was set up by the American War Department at the request of General Groves with the task of following the invasion troops in Europe to find out how far the Germans had got with their attempts to produce an atomic bomb.* Heisenberg's old friend Samuel Goudsmit was appointed as the scientific leader of Alsos. The military leadership was entrusted to *Colonel Boris Pash*. The secretary of the group, *Mary Bohan*, would later end up working for UNSCEAR (the United Nations Scientific Committee on the Effects of Atomic Radiation) in New York.

During December 1943 to March 1944, an initial Alsos assignment in Italy had also been under the leadership of Pash, but the assignment in Germany in 1944 with Goudsmit as scientific leader is the one that is usually associated with the concept of 'Alsos'.

The Alsos group arrived in Paris together with the Free French liberation troops on 25 August 1944 and immediately visited Professor Joliot, who told them how the Germans were showing an interest in his laboratory. He had had visits from Schumann, Esau, Diebner, Bagge and Bothe but he did not think that the Germans had made any significant progress within nuclear research.

Meanwhile, Dr. Lauriston Taylor, the physicist from the U.S. National Bureau of Standards, was in France. In spite of being seen as the doyen of international radiation protection, Taylor had never assisted with the Manhattan Project. He had been deeply involved in the development of remote sensing detonators for grenades and had temporarily moved away from the radiation problems. In a letter to me in September 1998, he writes about why he never became part of the Manhattan Project:

> I could actually have [got involved] but I was already heavily involved in war-related projects when I was rung twice by Arthur Compton. I was not willing to give up a programme that was clearly expected to succeed if it were completed. Owing to the secrecy, Compton was not able to give me any leads as to why he wanted me except that it concerned radiation. I was unable to give him any indication of my project for the

* The name "Alsos", which the Army's secret service had given the project, was the Greek name for "grove". This wordplay alarmed Groves when it was pointed out to him, but it was then too late to change the name.

> same reason. After the war, he told me what it was about. I suspect that I could have ended up in Parker's or Morgan's group. Imagine how the world would have looked!

At the end of the war, Taylor was Colonel and deputy head of the American 9th Bomb Squadron and was temporarily stationed in Rheims. There, he found out that Goudsmit wanted to meet him in Paris at a given address on the Champs-Elysées.

Goudsmit wanted to know whether any of Taylor's people had been at any of the plants for nuclear physics work or had heard any talk of such work. Goudsmit asked Taylor to keep his eyes open but Taylor thought the whole thing was silly and did not want to get involved when he found out how secret the assignment was. However, in the end, Goudsmit convinced him to give two of his men *carte blanche* to visit laboratories and workplaces that could come under suspicion. However, Taylor demanded that they report directly to Alsos and not involve him. 'I don't like secrets,' explained Taylor.

Later on, Taylor looked for old friends in Germany. He found Rajewsky's institute in ruins but found Rajewsky in a basement that was fitted out as a laboratory. Taylor and Rajewsky had been good friends before the war and had even discussed having one another's children to stay for a year. They were now on different sides and Taylor was also accompanied by an armed soldier. The friend belonged to those who had been conquered and Taylor suddenly realised that he had no idea of Rajewsky's opinion on the Nazis. The conversation became formal. However, Taylor found out that another friend, physicist *Robert Jaeger* at the PTR, had survived. Following his visit to Rajewsky, Taylor decided not to do any further investigations into old friends and acquaintances. The meetings would be too painful and heart-breaking.

On 9 September, Goudsmit came to the liberated Brussels and quickly examined Union Minière's delivery records, which revealed that more than 1 000 tonnes of the Belgian uranium had been transported to Germany. What worrying information.

Some members of the Alsos group had been given the task of taking water samples from the Rhine to be examined for radioactivity in the hope that this could reveal whether an already active nuclear reactor was using the river water as cooling water. A prankster in the group sent along a bottle of Rhine wine for activity analysis. The prank led to a great deal of unnecessary work: the wine was actually radioactive and it took a great deal of effort to clarify that the radioactivity was of natural origin.

Another clue was to chart the consumption of electrical energy in Germany to see whether there were any signs of energy-intensive isotope separation. Enriching natural uranium from 0.7 % to 3 % uranium-235, i.e., to an extent that it can be used as nuclear fuel with ordinary water as a moderator, already requires quantities of energy amounting to a few per cent of the energy that can later be extracted from the uranium. Enriching uranium-235 to such purity that it can be used in nuclear weapons therefore requires very substantial quantities of energy.

Substantial interest was thus aroused when aerial photographs revealed that a number of strange plants were being built south of Stuttgart in an area that had oil shale which, as with Swedish shale, could very probably contain high levels of uranium. Reports to Alsos about the Swedish progress with finding uranium in the shale did nothing to reduce Goudsmit's concern. The plants were therefore bombed to rubble and the discovery that they had purely and simply been intended to extract the oil from the shale was not made until later.[*]

Another mystery was Auer's substantial interest in thorium. However, the explanation was very simple. Since thorium could potentially be used for nuclear fission experiments, the forward-thinking Dr. Riehl had decided to buy up all available thorium for Auer for no specific purpose, purely as an investment that could lead to a payoff. In the worst-case scenario, uses could still be found for the thorium. Auer had already sold toothpaste containing thorium hydroxide in the 1930s. It was called Doramad and the advertising text said: 'I am the radioactive substance. My rays massage your gums. Healthy gums – healthy teeth'. If the thorium would not do for anything else, thought Riehl with support from the company management, you could no doubt do good business on the post-war market by giving Doramad a new lease of life.

[*] Construction of the Swedish shale oil plant at Kvarntorp in the Närke region began in 1941 and at the relevant time, 1944, 70 000 cubic metres of oil were produced there per year.

In spite of all attempts to throw them off track, Alsos came closer and closer to the German uranium scientists. In fact, a big step forward was actually taken thanks to a red herring like that. One suspicious minor character who fell into the hands of Alsos appeared to have had contact with Auer's plants in Oranienburg, which were suspicious in themselves, but he also had a hotel bill from Hechingen on him. This was particularly interesting since the mysterious plants had been discovered in the shale area close to Hechingen. Was Hechingen a centre for uranium research?

The suspicions increased when it was found that the captured German had with him a letter which showed that Hechingen was a 'Sperrgebiet', which the Americans translated as 'restricted area', which was a mistake. Hechingen was an evacuation area but had already received the full number of people whom it had been thought possible to accommodate there. 'Sperrgebiet' therefore meant that it was an area that could not take on the continued transportation of fleeing people. To the Alsos members, 'restricted area' indicated military secrets, and a decision was made to scrutinise Hechingen very thoroughly. So, the Alsos group's attention was on Hechingen, the place where the remainder of the Kaiser Wilhelm Institute for Physics was actually located, for completely the wrong reasons.

In the final weeks of 1944, the final reactor experiments were performed in the Dahlem bunker in Berlin, this time with graphite as the reflector, which had now got as far as achieving a subcritical neutron multiplication of 3.37. It must have been by virtue of the graphite, but it still did not dawn on Heisenberg that this could actually mean that Bothe's determination of the absorption of neutrons in graphite was not relevant to really pure graphite.

On 29 January 1945, a new experimental assembly had been completed in Dahlem, this time finally using cubes of uranium. 1.5 tonnes of heavy water had been used as a moderator. Success could now have been achieved with the same thing that Fermi had succeeded with in Chicago around two years previously, but the experiment never came to fruition. The German eastern front was broken. Marshall Shukov was on his way to Berlin. There were victorious hollers of 'Hitler kaputt!' from the Russian soldiers in the advancing Soviet armoured units.

On 30 January, twelve years to the day after Hindenburg had appointed Adolf Hitler as Chancellor of Germany, Gerlach decided to close down the operations in Berlin. They began to disassemble the reactor, and on 31 January the uranium and the heavy water were transported to Stadtilm 22 km south of Erfurt.

Gerlach had been thinking that it was now Diebner's turn to do an experiment with the uranium and the heavy water which were moved from Berlin. He discussed this with Heisenberg by telephone on 2 February, but Heisenberg was not willing to allow an experiment to take place with Diebner in charge. It was therefore agreed that the transport would be allowed to continue to Haigerloch, which is where the material arrived at the end of February.

Heisenberg now had the opportunity of doing one final experiment. He had access to 1.5 tonnes of uranium cubes, 1.5 tonnes of heavy water and 10 tonnes of graphite blocks and cadmium which could be used to stop the reactor were they to lose control over it.

The graphite blocks were stacked around the magnesium alloy cylinder. Inside this hung 78 chains with a total of 664 uranium cubes surrounded by the heavy water. The last experiment began on one of the last days of February 1945. There appeared to be a shortfall of 750 kg of heavy water for the reactor to function. This water was still in Stadtilm, although that place was being evacuated by SS troops who indeed knew that the heavy water was of military importance but did not know to whom. Such chaos now prevailed that it was inconceivable to continue experimenting.

In April 1945, Gerlach's archive fell into the hands of Alsos and thereby a full account of the experiment in the Dahlem bunker. On 23 April, the Americans found the majority of the German uranium ore stocks next to Stassfurt, approximately 1 200 tonnes. On the same day, Alsos reached Haigerloch through Hechingen. An American force under the command of Alsos' Colonel Pash occupied the town and found the cavern containing the laboratory and the remains of the Germans' last reactor assembly. The American experts were surprised at the complete lack of radiation protection devices. If the reactor really had got going, those present would have received very high radiation doses.

Alsos disassembled the reactor and blasted the cavern so as not to reveal the secret of the uranium research. Russian and French troops were advancing. From General Groves' point of view, it would have made no difference as to who got hold of the secret. He assumed that the Frenchmen would have consulted Professor Joliot, of whom Groves had said: 'My recent experiences with Joliot had convinced me that nothing that might be of interest to the Russians should ever be allowed to fall into French hands.'

Heisenberg had left Hechingen a few days before the Alsos group arrived there. He had cycled off to his family in Bavaria in an area that was as yet unoccupied by the Allies: Urfeld, a small village south of Munich by Lake Walchen at the foot of the Alps. Colonel Pash hurried there before the American troops. It soon became known that an American Colonel was present in the area and before Pash had found Heisenberg, he was visited by an SS General who wanted to surrender with a full division – his assumption was that where there was an American Colonel there had to be an American regiment. Bearing in mind that Pash had only six men at his disposal, he had to ask the General to wait until the next day for the surrender when the American troops arrived *en masse*. Just after that, the Allies occupied Munich and Alsos were also able to get their hands on Gerlach and Diebner.

On closer investigation in Haigerloch, Alsos found the heavy water hidden in a mill and the uranium buried in a nearby field. The German uranium research was now completely eliminated, although this took place in the shadow of greater events. President Roosevelt had suddenly passed away on 12 April, the Russians had reached the outskirts of Berlin on 21 April, and on 30 April it was reported that Hitler was dead.

The majority of the German knowledge was taken over by the Americans through Alsos, as were the heavy water and most of the uranium. The Americans bombed Auer's plants for the production of pure uranium metal in Oranienburg to smithereens in order to prevent them from being taken over by the Russians.

The Russian share of the loot was lean as regards materials, consisting of approximately 20 tonnes of uranium ore and 5 tonnes of uranium metal. On the other hand, the Russians took care of a relatively large number of knowledgeable German scientists, primarily Dr. Riehl with his in-depth knowledge of Auer's plant secrets, Baron von Ardenne with his plans for an electromagnetic isotope separator and a number of physicists, including Professors Döpel and Hertz.

And that is where the German atomic bomb endeavours came to a lamentable end. The German physicists had been extremely proficient in the theoretical sense, but in spite of efforts from Harteck and others, the research had never been adequately coordinated with the industrial resources. Some of the scientists, such as Harteck, Diebner and Bagge, had been concentrating directly on an atomic bomb. Others like Heisenberg, von Weizsäcker and Gerlach had attempted to produce a functioning reactor in the first instance. The literature is full of discussions about their capacity and willingness to produce an atomic bomb. Some authors have maintained that Heisenberg and his colleagues refrained from seriously attempting to produce a bomb for moral reasons. Others have said that they were willing but lacked knowledge and capacity. None of these suppositions appears to ring true. It appears most likely that the physicists benefitted from their unique position by virtue of the fact that their research could be considered to be of military interest while most people did not believe that a bomb could be possible before the end of the war. Heisenberg was so disinterested in the bomb itself that he made no serious attempt to estimate the critical mass of uranium-235 before he was interned in England after the war.

A few important facts were unknown to the Germans, including the toxicity of xenon, the spontaneous fission of plutonium-240 and the significance of delayed neutrons. This meant that the Germans were a long way behind where practical reactor technology was concerned and had not examined the control mechanism well enough.

Of the German scientists who have been mentioned in the book, Bagge, Diebner, Gerlach, Hahn, Harteck, Heisenberg, von Laue, von Weizsäcker and Wirtz were interned together with physicist Horst Korsching[*] in England through Alsos at a manor called Farm Hall. This was in the village of Godmanchester near Cambridge and had previously been used as a safe house for the European resistance and agents from MI6 who trained them to be sent behind the enemy lines. The whole operation of getting the German scientists there and listening into and making notes on their conversations went

[*] Horst Korsching (1912-) worked with methods for isotope separation at the Kaiser Wilhelm Institute for Physics under Diebner and Heisenberg.

by the name of Operation Epsilon. The primary purpose was to prevent them from ending up in the wrong occupation zone and perhaps, finally, in the Soviet Union in this connection. The corralling of the scientists in Germany began on 2 May 1945 and they arrived at Farm Hall on 3 July. Of those who were interned, Max von Laue was the only one who refused to follow the Nazis in all respects. The reason why he was taken along was because General Groves wanted to have him as someone to rebuild German science. However, no-one told von Laue about this, who therefore felt deeply wronged. Weizsäcker complained that some ‘underlings’ were included among those who had been selected, referring to Diebner and Bagge. Some of the German scientists also disliked being monitored by dark-skinned soldiers.

Germany surrendered on 7 May 1945. The American atomic bombs that were produced with enormous efforts to face the threat of Germany were never needed for the war in Europe. They might now instead be needed against Japan.

12. FROM OTHER HORIZONS

Although the main players in the unequal race for an atomic bomb were the USA and Germany, significant achievements were also made in other countries. This chapter tells what happened with 'atomic research' in the United Kingdom, Canada, France, the Soviet Union, Japan, Norway and Argentina. The stories concern time periods that last for a number of years after 1945 and thereby disrupt the time flow of the other chapters. However, this forward jump in time is needed for the reader to be able to put the course of events into context. The next chapter continues the story from 1945.

THE UNITED KINGDOM

In England, the report from Frisch and Peierls in spring 1940 (saying that the critical mass of uranium-235 was kilogrammes rather than tonnes) had started off the bomb research, primarily research into the possibility of separating uranium-235. However, since they were foreign nationals, neither Frisch nor Peierls was part of the Maud Committee which dealt with their report. They were therefore in suspended animation for a long time as regards finding out what had happened to the report.

In the end, Peierls wrote a formal letter to the Committee through Professor Oliphant, simply addressing it as 'Dear Sir' since not even the name of its chairman had been revealed to him. The exclusion of himself and Frisch from the Committee would quite simply lead to unnecessary delays in the work that was of common interest; they would of course be able to directly answer a great deal of questions. And what was the point of keeping their own secret from them?

Oliphant showed the letter to the chairman of the Committee, Professor Thomson, who contacted Frisch and Peierls and agreed that their assistance was important and promised that they would be used as advisers. Peierls then pointed out that there were several important immigrants who ought to be involved in the work. He was thinking primarily of *Professor Franz Simon*, a proficient German chemist whom F.A. Lindemann had helped from Germany in 1933 to work at the Clarendon Laboratory in Oxford.

Frisch believed that thermal diffusion with help of the Clusius tube could be a practicable way of enriching uranium-235. Peierls (with Oliphant's consent) asked the advice of Simon, who recognised the difficulties of large-scale enrichment should that be the case. He said: 'It was like getting a doctor who had after great labour made a minute quantity of a new drug and then saying to him: 'Now we want enough to pave the streets'.' At Peierls' proposal, the Maud Committee discussed the desire to engage Simon on a serious basis but several of the members were dubious about accepting help from a German immigrant. However, he was finally included in the work in August 1940 and later, unlike Frisch and Peierls, became a member of the Maud Committee. Simon began intensive research into gas diffusion on behalf of the Committee.

Frisch wanted to do practical thermal diffusion experiments instead, but he found that this was difficult at Oliphant's premises in Birmingham where he and Peierls did their research. Oliphant prioritised radar research which was of more immediate importance to the warfare. However, James Chadwick in Liverpool was willing to make room for Frisch at his institute, a unique offer to an immigrant since Liverpool was a protection area to which foreigners did not normally have access. Frisch therefore left Birmingham and Peierls in autumn 1940.

Despite Thomson's promise, Frisch and Peierls did not think they had been adequately consulted. In August 1940, Peierls wrote to Thomson: 'You can rest assured that in the interests of the work I shall continue to carry on as best I can whatever the conditions and to put at your disposal all our results without waiting to be asked for them'. However, there were people who were even more impatient and displeased than Peierls. In July 1940, Joliot's colleagues von Halban and Kowarski had come to England

and in August they had started work in Cambridge with tasks from the Maud Committee; von Halban protested loudly at not being allowed to be part of the Committee.

A solution to these objections was found in September 1940 when the Maud Committee was supplemented with a technical sub-committee which accommodated all of the eager experts. The original Maud Committee became a small policy committee with just one representative for each and every one of the universities participating in the work. Its new directions read:

(i) To supervise on behalf of the Director of Scientific Research, M.A.P. [Ministry of Aircraft Production] an investigation into the possibilities of uranium as contributing to the war effort;
(ii) To consider the recommendations of the Technical Committee and to advise the Director of Scientific Research accordingly.

So, in the summer of 1940, the Maud Committee was a committee under the Ministry of Aircraft Production. Tizard's Committee for the Scientific Survey of Air Warfare had been dissolved in June 1940.

The directions for the technical sub-committee read:

(i) To consider the problems arising in the Uranium investigation;
(ii) To recommend to the Maud Policy Committee the experimental work necessary to establish the technical possibilities;
(iii) To ensure co-operation between the various groups of investigators.

In Oxford, Professor Simon was now throwing himself into his research into gas diffusion through porous membranes. At the end of 1940, his assessment was that this separation method was possible on an industrial basis. On the other hand, Frisch's experiment with thermal diffusion in Liverpool soon showed that this method was not usable for uranium in gaseous form, the same conclusion that would be drawn by Germans and Americans.

Oxford scientists found, as did their American colleagues later on, that it is difficult to produce barely permeable membranes with sufficiently small holes. An aperture diameter of between one hundred thousandth and one thousandth of a millimetre was required. However, in December 1940, Simon thought he had solved the problem and submitted a report called 'Estimate of the Size of an Actual Separation Plant'. Simon's plant could produce uranium-235 to a purity of 99 per cent in a procedure that would take twelve days.

The plant could produce one kilogramme of uranium-235 per day. It would consist of 18 000 units, each with 20 separation stages. It would need a land area of around 40 acres, the equipment would weigh 70 000 tonnes and it would consume approximately 60 megawatts of electric power. The cost of the plant was calculated at 4 million pounds Sterling and the operating costs at £1.5 million per year. It would need 1 200 people to run it. In terms of cost, the project would correspond to that of a battleship or a large ammunition factory and was thus very expensive but not impossible.

The hypothesis on which the activity of the Maud Committee largely rested was the validity of the estimated critical mass of uranium-235 given by Frisch and Peierls: a few kilogrammes. The estimate in turn depended on Peierl's estimate of the fission cross section for uranium-235 for fast neutrons. This estimate needed to be verified, and this task was undertaken by Chadwick in Liverpool and Merle Tuve in Washington DC. The first information from Tuve was alarming: he had found much lower values for the fission cross section, which would mean that Frisch and Peierls had heavily underestimated the critical mass. On the other hand, Chadwick's result indicated that their estimate could be correct. In March 1941, Tuve's result did finally tally with Chadwick's. Frisch and Peierls had underestimated the critical mass but not by much. The new results indicated 8 kg without tamper and 4-5 kg with tamper.

In spite of his formal position under the Ministry for Aircraft Production, the Maud Committee was principally an academic organisation that was heavily dependent on voluntary inputs from the university. The Professors such as Chadwick, Peierls and Simon did work which was paid for mainly by the university and the Committee had a very loose association with the Ministry; it lived a largely separate life. The Ministry provided assistance when it came to maintaining priority for the procurement of things that the scientists needed for their experiments. The uranium research was kept secret despite so many university scientists beings involved. Not even Lord Hankey knew that the Maud Committee existed.

In September 1940, the scientists in Birmingham, Liverpool and Oxford needed a total of 3 kg of uranium hexafluoride. The majority of the chemical research was carried out by Professor of Chemistry *Sir Norman Haworth* in Birmingham

Haworth had already previously obtained assistance from Imperial Chemical Industries but the company was dubious about the new order and wanted a guarantee that larger orders would come in the future. This was difficult for the scientists to give – it was not possible to say whether large quantities of uranium hexafluoride (‘hex’) would be needed until experiments had been conducted with small quantities. Professor Simon then wrote to F.A. Lindemann and asked him to convince the Director of I.C.I., *Lord Melchett* (1898-1949), to involve himself personally in the matter. Lord Melchett did so and the cooperation with I.C.I. went very efficiently after that.

Bearing in mind exactly what had been accomplished within a few months, in autumn 1940 the research in England was way ahead of the research that was being run on an all fingers and thumbs basis in the United States and Germany. The United States had not yet entered the war and the research managers there did not have the same interest in an atomic bomb as the Britons, nor the same conviction that it was possible. In the autumn, a British research delegation led by Sir Henry Tizard travelled to Washington DC to exchange experiences between the Maud Committee and the American Defence Research Council under Vannevar Bush. The delegation included Professor Cockcroft.

Cockcroft was accompanied by *Professor Ralph Fowler* who had recently come to Canada as a contact person with the Canadian Research Council. The pair visited a number of American scientists and also participated in a meeting of Briggs’ Uranium Committee. Cockcroft got the impression that the Americans were several months behind the British in the uranium research and that the Americans needed to be shaken up. This view was shared by the others in Tizard’s delegation, and they searched for possibilities to improve the contacts. One result of Tizard’s and Cockcroft’s visit was that *Dr. Charles Darwin* was posted as the British contact person in Washington DC. In Canada, Fowler was succeeded in spring 1941 by *Sir Lawrence Bragg* (1890-1971) who had won the Nobel Prize in Physics for work with x-ray crystallography along with his father Sir William Bragg in 1915.

In February 1941, an American delegation led by Conant came to England and there were discussions concerning cooperation matters between the Defence Research Council (NDRC) and the British government. On this occasion, Conant ate lunch with Churchill and was taunted by F.A. Lindemann for his ignorance of the atomic bomb.

In April 1941, Simon’s group at the Clarendon Laboratory in Oxford had started a separation stage on a 50 % scale. The task to be solved was to get an initial separation stage to function, and then to ‘just’ build a plant with numbers of similar stages in a series. The biggest problem was producing a functioning membrane. When gaseous uranium hexafluoride is pressed through a membrane at a temperature of approximately 60 °C with a pressure difference of approximately 100 kilopascals (a pressure of one atmosphere), the lighter molecules pass through more easily. On the rear side of the membrane, the gas that has come through initially is enriched with uranium-235. This gas therefore has to be pumped out before more of the uranium-238 has managed to come through. Each separation stage leads to an extremely small enrichment and a very large number of stages are therefore necessary. Since the enrichment is so insignificant, the remaining quantity of uranium hexafluoride can be recirculated through the stages at the rear to increase the enrichment.

While experiments were ongoing with an individual separation stage, Simon was pressing for an experimental plant with 20 stages. A contract for such a plant was signed with Metropolitan-Vickers in May 1941. I June 1941, an extended contract was signed with I.C.I. regarding help with chemical problems during the construction of the Metropolitan-Vickers 20-stage plant.

The British had agreed that Hans von Halban and Lew Kowarski could do research in Cambridge in exchange for their making their nuclear fission patent available. They began with reactor research using the heavy water that they had brought with them when fleeing from France. Their colleagues *Egon Bretscher* and *Norman Feather* had alerted the Maud Committee to the possibility that the element 94 that was expected to be formed following the capture of neutrons in uranium-238 might be just as fissile as uranium-235. However, the Committee concentrated on uranium-235. No theoretical obstacles were seen - the problems with producing a uranium bomb were solely technical and financial. A bomb with element 94 (it was not yet called plutonium) required a functioning reactor, but this was still only a utopia in 1941 and in any case not something that could be counted on to completed before the end of the war. The Maud Committee wrote the following in its final report:

> We know that Germany has taken a great deal of trouble to secure supplies of the substance known as heavy water. In the earlier stages we thought that this substance might be of great importance for our work. It appears in fact that its usefulness in the release of atomic energy is limited to processes which are not likely to be of immediate war value, but the Germans may now have realized this, and it may be mentioned that the lines on which we are now working are such as would be likely to suggest themselves to any capable physicist.

The research that Kowarski and von Halban carried out was therefore not seen as part of the bomb research but rather as being of interest to a future peaceful nuclear energy. As in Germany, reactors were called ‘atomic machines’ but sometimes also ‘boilers’. Kowarski and von Halban eagerly studied different moderators and also control systems. It would be important to keep the ‘boiler’ under control but it was understood that a reactor could not explode like an atomic bomb. The following was written about the reactor process in the Maud Report:

> Unless it is controlled, the reaction will multiply so rapidly that an explosion will occur. (The explosion would be violent but only comparable with that of an equal weight of high explosive. It will not be similar to the ‘uranium bomb’ for in the present case the reaction is propagated by thermal neutrons, therefore relatively slowly, and the system will fly apart and the process will stop before an appreciable fraction of the available energy has been released.)

Kowarski and von Halban were frustrated. They thought that functioning nuclear reactors had an enormous future, irrespective of whether or not there was any intention of using the plutonium. They therefore thought that their research was far more worthy of attention and considerably greater resources; they mainly needed more heavy water (like Bothe in Germany, they had not yet found that graphite could be used as an alternative moderator). However, the British thought they had to prioritise the atomic bomb and the efforts that would lead the fastest to the bomb, i.e., the enrichment of uranium-235. However, the Maud Committee realised that the situation would be unsustainable in the long term. Something had to be done about von Halban and Kowarski, but what? And, when it came to thinking about the future, would the enrichment of uranium-235 really be possible in England? Should they take the risk of building Professor Simon’s big plant in a war-torn country that was the victim of repeated bomb attacks?

Regarding the heavy water, the British contacted the Americans, and Professor Urey, the first of them to separate heavy water, began searching for the possibility for a heavy water plant in the United States.

On 15 July 1941, the Maud Committee approved its final report (which has already been mentioned in Chapter 7). The report concentrated fully on a bomb with uranium-235 and isotope separation using the gas diffusion method. The Maud Report was enough of a catalyst on the Americans to get the Manhattan Project going. The report also got Churchill to change his mind following Lindemann’s presentation. Chapter 7 has also recounted Professor Oliphant’s trip to the United States which, although it actually concerned radar matters, would have a major influence on the attitude of the Americans.

When Churchill had definitely changed his mind about the atomic bomb project, it became obvious that a new organisation would be needed. Up until now, the work had been run by a number of scientists who had worked more or less under their own steam in an organisation that was no more than vaguely linked to the Ministry for Aircraft Production. Firmer control was now needed. The General Staff also declared that the intended weapon had to be shrouded in great secrecy. This secrecy requirement would prevail until the American atomic bombs fell on Japan. The secrecy requirement was so strict that the atomic bomb, despite its strategic and political importance, was not even discussed within the British War Cabinet. *Clement Attlee* (1883-1967), who was Deputy Prime Minister, said afterwards that he had only heard that a ‘bigger and better bomb’ was being developed.

The responsibility was thus beyond the remit of the War Cabinet and was shouldered by Sir John Anderson, whose background was unusual for a cabinet member in that he had been a chemist and had actually worked with uranium. When Anderson had read the Maud Report, he was so impressed that he told Hankey and Cherwell that he wanted to work with the bomb project, and Churchill had therefore given him the assignment.

Anderson was already responsible for a number of scientific institutes within the government, including The Department of Scientific and Industrial Research (DSIR), whose secretary was Sir Edward Appleton, the man whom Lord Hankey had previously relied on to investigate the atomic energy problem. Anderson suggested that the Department would also be suitable to outwardly administer the continued work, and Appleton agreed.

However, the actual organisation needed a special manager who would preferably be determined and possess sufficient technical knowledge to be able to take stock of the problem area. The choice was *W.A. Akers*, who was head of research at Imperial Chemical Industries.

The new organisation needed also a cover name. Anderson and Akers agreed to call it the ‘Directorate of Tube Alloys’, which was vague enough not to arouse curiosity. Sir John Anderson became chairman of an advisory committee, which also included among others Lord Cherwell, Lord Hankey, Sir Edward Appleton and the Minister for Aircraft Production. They also decided to appoint a technical committee which would consist of Akers as chairman and Professors Chadwick, Peierls and Simon, Dr. von Halban and a representative of I.C.I.

At the end of 1941, American overtures were made for closer British-American cooperation. President Roosevelt wrote to Churchill (according to Gowing):

> It appears desirable that we should soon correspond or converse concerning the subject which is under study by your M.A.U.D. Committee and by Dr Bush’s organization in this country in order that any extended efforts may be coordinated or even jointly conducted. I suggest for identification that we refer to the subject as MAYSON.

However, the British felt self-assured and that they were way ahead of the Americans. They declined the closer cooperation, their justification for so doing including the fact that it was important to keep the British atomic bomb project secret and that it would be difficult for things to remain secret in the United States, which is in retrospect a somewhat comical statement bearing in mind the secrecy that the Americans appeared to be able to maintain and the breaches of secrecy on the part of the British which paved the way for spies such as *Klaus Fuchs* (1911-1988).

In mid-1942, there was a radical change to the situation. The Americans were in full swing with the Manhattan Project. The British on the other hand saw major difficulties with building a separation plant and would have preferred to have been in cooperation, but the Americans had now lost interest.

In August 1942, Sir John Anderson contacted the Canadian government’s representative in London and expressed hopes of being able to transfer the group that was doing reactor research in Cambridge to Canada. He feared that von Halban and the latter’s group would become too isolated if they stayed in England - their work did not fit in properly with the British nuclear weapons programme. The Canadian government’s response was very enthusiastic in welcoming the French-British group.

The decision was to move the French-British-Canadian cooperation to the Canadian National Research Council’s laboratories at the new university in Montreal since they were finding it more difficult to locate laboratory space and accommodation in Ottawa. In addition to the group expected from Cambridge, it was possible to link a number of very prominent scientists to the project. The well-known French physicist *Pierre Auger*[*] (1899-1993) agreed to lead the department of experimental

[*] Pierre Auger is known for the *Auger effect*, which he discovered in 1925. When a substance is irradiated and its atoms are supplied with energy, electrons can be “knocked out of an interior electron shell” (to use a simplified atomic model). The vacancy that has arisen can be filled by an electron from an outside shell, whereupon energy is released. The released energy can be emitted in the form of characteristic x rays. However, in light atoms it is more usual for the energy to release an electron and give it kinetic energy. This is what is called the Auger effect. The released electron is called an Auger electron.

physics. *Professor Friedrich (Fritz) Paneth* (1887-1958), a clever Austrian chemist who worked in England, accepted an invitation to lead the chemical department in Montreal. Another European chemist, Frenchman *Bertrand Goldschmidt*, was already in Canada. The brilliant Czech physicist *George Placzek* was willing to lead the department of theoretical physics. I.C.I. provided clever engineers, technicians and metallographers.

von Halban was given the task of leading the whole operation when the Cambridge group moved to Canada (he was later succeeded by John Cockcroft). At first, this led to problems when it appeared that his colleague Lew Kowarski would just be given a subordinate position. The other scientists in the Cambridge group refused out of solidarity with Kowarski to travel at first. In the end, a solution was found in that Kowarski remained in England for the foreseeable future as a colleague of Chadwick. However, he did join the group in Montreal in July 1944.

In September 1942, General Groves took over the American atomic bomb programme. He discussed the cooperation with Bush and Conant early on and says the following in his account of the Manhattan Project:

> Soon after I came into the project, Bush and Conant and I reviewed the situation and reached the conclusion that in the future the British effort would probably be limited to the work of a very small number of scientists without any significant support from either the British Government or industry; and that inevitably a large amount of information would pass from the United States to the united Kingdom, while practically none beyond preliminary laboratory data would pass from the British to us.

General Groves thus had a very negative view of continuing the exchange of information. At the end of 1942, W.A. Akers came to the United States. He was head of Tube Alloys at the time and had discussions with Bush, Conant and Groves during which he pleaded for a greater flow of information regarding the production processes. However, the Americans were suspicious and believed that Akers was influenced by a commercial interest in the post-war situation. They therefore became very restrictive and only gave out information that could be of direct use to the British war efforts. This policy followed a decision from the military policy committee in December 1942, a decision that had been approved by President Roosevelt. The Britons were most displeased with this. Groves has the following to say about it:

> If the British, particularly Akers, had not displayed such an interest in, and had not insisted on obtaining, material of value solely for postwar industrial possibilities, the existing interchange of information might not have been affected. Negotiations broke down not because of American policy, but because the British refused to accept our view that collaboration should be for the purpose of winning the war – not for post-war purposes.

An important matter for the British was how to assure themselves of sufficient uranium. They knew that uranium existed in many places throughout the world but that the most important deposits lay in the Belgian Congo and Canada. Practically all the uranium ore mines in Canada were run by Eldorado Gold Mines Ltd. at Great Bear Lake just below the Arctic Circle, with the ore being transported from there to the company’s refinery in Port Hope, Ontario. The mine was closed at this time owing to surplus production.

The British authorities realised the significance of Eldorado’s operations and began discussing the possibility of taking control of the company. Efforts to do so failed, however. In September 1942, Eldorado discontinued the contract with the British and worked totally for the Americans who had increased their orders for uranium ore and also allowed Eldorado’s refinery to process the 1 200 tonnes of Belgian ore which had been stored on Staten Island. At that time, the Canadian government had gained control over Eldorado on behalf of the United Kingdom but in reality was using Eldorado for the United States.

In summer 1943, W.A. Akers attempted to get the Americans to allow Eldorado to deliver the hundred tonnes of uranium oxide that von Halban thought they needed. However, General Groves was not willing to relinquish his orders from Eldorado; the Americans had actually ordered more uranium oxide than Eldorado could deliver. Groves explained to the British that those in the United States had now come so far that they were on the way to producing an atomic bomb while the British and Canadians were doing research that could not result in a weapon for many years. It was therefore reasonable for the United States to have all the uranium oxide that Eldorado was able to produce.

So, in many respects the British-American cooperation had broken down by the start of 1943. However, Churchill was not satisfied with this. On 25 May 1943, his adviser, Lord Cherwell (F.A. Lindemann), had a meeting with Roosevelt's adviser *Harry Hopkins* (1890-1946) in the White House together with Vannevar Bush. Cherwell said firmly that the British interest certainly did concern the post-war period but only in military terms, not commercial. If the Americans did not give the British the information they needed, they would be forced to redirect some of their own war efforts into obtaining the information for themselves.

This brought results. On 20 July, Roosevelt wrote to Bush: 'While I am mindful of the vital necessity for security in regard to this, I feel that our understanding with the British encompasses the complete exchange of all information'. However, Bush was in England and the telegram he received there about Roosevelt's letter was not clearly worded. On 22 July Bush met Churchill, Sir John Anderson, and Lord Cherwell and succeeded during the discussion in getting Churchill to draw up a cooperation agreement that was somewhat more restrictive than that described in Roosevelt's letter. This agreement was signed by Churchill and Roosevelt in Quebec on 17 August 1943.

The Quebec agreement, which promised 'full and effective collaboration', enabled British scientists to participate in the Manhattan Project. A Combined Policy Committee was also created and arrangements were made for the joint procurement of uranium, and the parties were forbidden to give information to third parties without the other party's consent. Almost all British scientists who were involved with uranium-235 affiliated themselves to the Manhattan Project, most of them in Los Alamos. This was the whereabouts of people like the explosives expert *William Penney* (1909-1991) who would subsequently lead the British nuclear energy programme, *Greg Marley* who ended up being a well-known British radiation protection expert, and the brilliant theoretical physicist Klaus Fuchs who would become a spy for the Russians. No British or Canadians were allowed in Hanford, however. This may have been due to Groves' mistrust of the French and the interest of the French emigrants in the production of plutonium. However, following the Quebec agreement, the Americans began to support the British-French-Canadian plant in Canada. Subsequently, reactors were developed there that were both cooled and moderated with heavy water and which became prototypes for the Canadians' CANDU nuclear power reactors.[*]

In Washington DC, Chadwick became the leader of the British atomic energy contact and was on surprisingly good terms with General Groves, so this considerably improved the British-American cooperation. Chadwick helped British experts to come over to the United States to work in the common interest. However, the consequence was that the British atomic energy development practically ceased for the rest of the war. The emigration of British scientists led to a number of administrative problems, although these were quickly resolved. Scientists who had emigrated to England were quickly made British citizens and were equally quickly exempted from military service which would otherwise have been unavoidable. The security test also took place quickly, sometimes too quickly, as when the green light was given for the spy Klaus Fuchs.

In February 1944, Bohr returned from Los Alamos to Washington DC, where Bohr was perceived as an unofficial ambassador from England with backup from Sir John Anderson, although President Roosevelt, who had not met Bohr, said that he wanted to be cautious and not go behind Churchill's back. Roosevelt said that he welcomed new ideas but did not want to discuss them until he knew that Churchill himself was behind them. The new ideas that Bohr discussed with the President's adviser concerned the attitude towards the Soviet Union. Bohr predicted and was worried about a race towards the new weapon.

[*] CANDU stands for Canadian Deuterium Uranium Reactor, a reactor with natural uranium in horizontal fuel rods containing uranium dioxide, cooled by heavy water which also functions as a moderator.

He wanted the British and Americans to forewarn the Russians that they were on the way to producing an atomic bomb. This, thought Bohr, could create trust and facilitate agreements regarding future nuclear weapon control.

The result of Bohr's discussions in Washington DC was that he, rightly or wrongly, thought his assignment from Roosevelt was to contact Churchill and discuss his views with him. *Lord Halifax* (*Edward Wood*, 1881-1959), former British Foreign Secretary and now British ambassador in Washington DC, thought the assignment was so important that Bohr ought to travel to London immediately. And so Niels Bohr, along with his son Aage, flew to England by military aircraft at the start of April 1944.

Sir John Anderson supported Bohr. He had already sent Churchill a memorandum in which he suggested that the 'tube alloys' project should be surrounded by less secrecy and also be discussed in the War Cabinet. He referred to the risk of nuclear weapons spreading after the war and that international agreements were therefore necessary. He suggested that they ought to inform the Russians that the British and Americans would have a terrible weapon by a particular day. They ought to invite them to cooperate in drawing up a plan for international control.

Churchill had circled the word 'cooperation' in Anderson's memorandum and had written in the margin 'on no account'. When After Bohr's arrival, Anderson wrote to Churchill again, suggesting that the latter send Roosevelt a telegram proposing a discussion, and Churchill's brief response was: 'I do not think any such telegram is necessary nor do I wish to widen the circle who are informed'.

Bohr and Cherwell were finally invited to see a negative Churchill at no. 10 Downing Street on 16 May 1944. Aage Bohr has said (according to Rhodes):

> We came to London full of hopes and expectations. It was, of course, a rather novel situation that a scientist should thus try to intervene in world politics, but it was hoped that Churchill, who possessed such imagination and who had often shown such great vision, would be inspired by the new prospects..

The meeting turned into a disaster. Churchill was unwilling to waste time on other things when he was fully occupied with the preparations for the invasion of Normandy. He could not understand why his colleagues showed such reverence and respect for the long-haired, incomprehensible man whom Cherwell had forced on him. He could see nothing remarkable about the atomic bomb: 'After all this new bomb is just going to be bigger than our present bombs.' Nor could he see any post-war problems: 'There are none that cannot be amicably settled between me and my friend, President Roosevelt.'

After the meeting, Bohr said (according to Rhodes):

> We did not speak the same language. […] It was terrible that no one over there (England and America both) had worked on the solution of the problems that would arise when it became possible to release nuclear energy; they were completely unprepared. […] It was perfectly absurd to believe that the Russians cannot do what others can ... There never was any secret about nuclear energy.

In September 1944, Churchill and Roosevelt confirmed and expanded the Quebec agreement at a meeting at Roosevelt's Hyde Park estate, with an agreement on full British-American cooperation for both peaceful and military atomic energy after the war. However, after Roosevelt's death in spring 1945, the agreement became worthless. A new agreement on full and effective collaboration was entered into by Attlee and Truman in Washington DC in November 1945, but it was just as illusory.

In October 1945, the new labour government under Attlee established an atomic research station for a national atomic energy project to cover all uses of atomic energy. The operations were moved to a military airfield by Harwell, twenty kilometres south of Oxford. Cockcroft was summoned back from Canada to be its director. At the same time, it was announced that the responsibility for the development of atomic energy had moved from the Department of Scientific and Industrial Research to the Ministry of Supply where 'the Directorate of Tube Alloys' had also moved. Harwell would become a major and important research plant for nuclear energy matters, although initially subject to great secrecy.

In January 1946, an organisation was created for extensive planning of the plants that would be needed for the production of fissile material for both peaceful and military use. The man who was chosen to lead this organisation was an engineer by the name of Christopher Hinton. His task was to plan and build factories for the production of uranium, reactors for irradiating the uranium with neutrons, reprocessing plants to separate plutonium and gas diffusion plants to enrich uranium-235, not necessarily for bomb material but to be able to utilise the uranium that has been depleted from this nuclide in the reactors. Hinton's organisation was called the Division of Atomic Energy Production (under the Ministry of Supply), and the headquarters was placed in Risley in Lancashire. On 4 February 1946, the whole personnel, twelve people, moved into Risley. Only one of them had any experience of atomic energy. He had been in Canada and had to start by quickly training up the others. The organisation took on the name of the place, i.e., people talked about 'Risley thinks ...', 'Risley has decided ...', etc.

The first plant that had to be built was a uranium production factory. The construction of reactors had to wait until uranium was available. Within a month, a site had been selected for the factory at the Springfield plant, a chemical factory for military purposes, at Salwick near Preston in Lancashire.

The coordination between Harwell and Risley was unclear, primarily with regard to Harwell's responsibility for research concerning the military applications.

The two organisations both came under the Ministry of Supply. In 1946, a Chief Superintendent of Armament Research (CSAR) was appointed there, i.e., a head of weapons research, to create a number of secret atomic bomb plants. William Penney was appointed CSAR and became the youngest of the three atomic bosses (in addition to Penney there was also Cockcroft in Harwell and Hinton in Risley). Penney later became head of the British nuclear weapons laboratory in Aldermaston in 1953.

In April 1946, the Joint Policy Committee in the United States, set up in 1943, ceased to function and in August 1946, the American Atomic Energy Act, known as the McMahon Act, was adopted. This signalled the kiss of death to the British-American cooperation. The Act forbade the distribution of classified information, even to the British, with a threat of harsh penalties, including the death penalty. This meant that the agreements from Quebec, Hyde Park and Washington DC from 1943, 1944 and 1945 were worthless.

In January 1947, a formal government decision was made regarding the production of British atomic bombs and the decision was revealed to Parliament in May 1948. The British decision on its own atomic weapons was partly a matter of prestige – the United Kingdom was the third big power after the United States and the Soviet Union and wanted to be able to assert itself and be accepted as a cooperation partner with the United States. But the national defence need was also important; after the war, the United States had not assumed any liability to defend either the United Kingdom or any of the other war-torn European countries in the face of a threat from the Soviet Union when the cold war began to escalate in 1948. A communist leadership was imposed on Czechoslovakia in February 1948 and the Soviet blockade of Berlin began in June. National defence cooperation was not initiated until NATO (the North Atlantic Treaty Organisation) came into being in 1949.

The British scientists had never had access to Hanford but they knew that the plutonium-producing reactors there were graphite-moderated and water-cooled and used natural, i.e., non-enriched, uranium. The British scientists who participated in the cooperation with French and Canadian scientists in Canada had constructed a reactor there called GLEEP (graphite low energy experimental pile), which was erected in Harwell and commissioned in 1947. It was followed in the same year by a bigger reactor called BEPO (British Experimental Pile) with an output of 6 MW. However, these reactors, both of which were air-cooled, had too small an output to be used for the production of plutonium for nuclear weapons.

The first idea was to cool a more powerful reactor with water, like at Hanford. But each and every one of the Hanford reactors had used several hundred cubic metres of water per minute. This was fine when there was access to a big river, but where were there such rivers in the United Kingdom? Water-cooling a graphite reactor also led to other problems. A shortfall in the flow of water would increase the temperature in the reactor but, even worse, the reactivity would also increase since the water would have absorbed neutrons and the moderator, graphite, would remain. A shortfall in the flow of water could lead to the fuel element being vaporised and the radioactive substances being spread over large areas. General

Groves had said that he was in constant fear of being notified that the Hanford reactors had exploded. It was this risk that had led to the choice of such a remote site for the American reactors.

The British scientists therefore abandoned the thought of water cooling in April 1947 when there was success in finding technical solutions for adequate air cooling. However, this did not exclude the risk of the reactor overheating, but there would be less of a dramatic occurrence. Graphite as a moderator did lead to particular risks, however. Physicist Greg Marley, who had become head of radiation protection at Harwell, observed early on the possibility of fire in the graphite if the energy stored through the Wigner effect was suddenly released. The Wigner effect means that irradiation with neutrons displaces some of the carbon atoms from their positions in the graphite, which leads to the storage of energy. When Teller and medical physicist Gioacchino Failla from the Columbia University Medical Centre visited England in 1948 and discussed the environmental problems from a graphite reactor, Teller also warned of the consequences of a graphite fire caused by released Wigner energy.*

Bearing in mind the risk of an accident, a decision was made to locate the plutonium-producing reactors a good distance from large settlements. The place chosen was Sellafield, previously the site of an ammunition factory. Sellafield is on the Cumbrian coast of the Irish Sea, right opposite the Isle of Man. The plant was called Windscale after a nearby steep cliff and its construction began in September 1947. Two plutonium production reactors were erected at Windscale. Each consisted of 2 000 tonnes of precision-produced graphite blocks as a moderator in an octagonal structure with a diameter of 15 metres. The blocks had thin spaces between them to take into account changes to length and breadth as a consequence of irradiation with neutrons. The whole of these reactor cores was contained within a 'biological' shield made of 2-metre-thick reinforced concrete (it was given the slightly far-fetched name of 'biological' since it was meant to avoid the risk of biologically harmful radiation effects). There were 3 440 fuel channels through the graphite, each containing 21 fuel elements so that the total number of fuel elements in each of the reactors amounted to more than 70 000. The output of the reactors was regulated in both the reactors by 24 horizontal boron steel control rods and there were 16 vertical rods for an emergency stop which were held up by electromagnets and which could fall down into the reactor core if the process had to be stopped quickly.

Outside the concrete shell (the biological shield) were two fan housings for each reactor, each containing four fans. These blew cooling air through the reactor and up through 125-metre chimneys.

Windscale reactor no. 1 was started in October 1950 and no. 2 in June 1951. The first delivery of irradiated fuel rods for the chemical reprocessing plant took place in January 1952 and the first batch of plutonium for a bomb, or rather a test charge, was ready in October 1952. It was exploded off the west coast of Australia at the Montebello Islands on 3 October 1952 and had a blast capacity corresponding to 20 kilotonnes of TNT.

The planned gas diffusion plant for the enrichment of uranium-235 did not start to be built until March 1950, but Risley's engineers had already two years previously begun to examine the drawings that were produced during the Tube Alloys contact with the I.C.I. between 1943 and 1945. Thereafter, the interest had moved from uranium-235 to plutonium production in reactors, but the I.C.I. and Harwell were still cooperating with regard to the development of membranes. The gas diffusion plant was located next to the small village of Capenhurst on the Wirral peninsula opposite Liverpool and was consequently named Capenhurst.

The United Kingdom's 'Los Alamos' was finally created in 1953 in Aldermaston approximately 80 km west of London between Newbury and Reading. The British nuclear weapons were produced there at the Atomic Weapons Establishment. It perhaps makes sense that the name was abbreviated to 'AWE', which means respect mixed with fear in English. As already mentioned, William Penney became the first head of the AWE.

CANADA

* It was such a graphite fire that caused the Windscale accident in October 1957 when small quantities of radioactive iodine were spread throughout Europe.

The Canadian government's decision in December 1942 to approve a contract between the United States' government and the Canadian mining company Eldorado had come as a surprise to the British. Churchill complained that the Canadian government had 'sold the British Empire down the river'. It was of little comfort that the Canadian minister who had signed the contract said he had not understood the consequences. The hundred-strong group of scientists in Montreal under the leadership of von Halban was now without access to uranium and the atmosphere within the group became pretty exasperated.

The materialisation of the Quebec agreement between the United States and the United Kingdom in August 1943 did improve the situation, however. Canada had certainly not participated in the agreement but did get to benefit from its consequences. However, the restriction that was made was particularly vexatious for the Montreal group – it could not visit Hanford. This served to block the very information that was so important to the group. At a meeting between representatives of the group and General Groves in Chicago in January 1944, Groves justified his reasons for excluding Hanford from the cooperation: the plutonium producing reactors were already fully underway and could not be improved by efforts by the British and Canadians. Thus the latter were excluded from information regarding some technology that would be of vital importance to the post-war period's peaceful use of atomic energy, including the reprocessing of used fuel for the separation of plutonium. It was perfectly obvious to the scientists in Montreal that it was not the secrecy issue with reference to the atomic bombs which prevented them from visiting Hanford – there was no corresponding obstacle to their visiting Los Alamos, the most secret of the American plants. It was obvious that the competitive situation in the anticipated peaceful post-war use of atomic energy was the deciding factor.

In April 1944, the British-American Joint Policy Committee made a decision which gave the Montreal scientists a feeling that, in spite of everything, they were dealing with something meaningful: large heavy water reactors were to be built in Canada, and firstly an experimental reactor of maybe 5 MW. However, the Americans required the project to be led by a 'real' Brit, not an immigrant like von Halban, so von Halban was therefore replaced by John Cockcroft.

Before the planned large reactor was built, a much smaller prototype was erected under the leadership of Lew Kowarski who came to Canada in summer 1944. The site of the reactor would be by the Ottawa River next to the small community of Chalk River, 180 kilometres west-north-west of Ottawa. The site was sufficiently isolated to satisfy secrecy requirements and requirements regarding distance from larger communities in the event of an accident. Its proximity to the Ottawa River was important, bearing in mind the need for cold water. The small community of Indian Point seven to eight kilometres north of Chalk River was chosen as the area where the personnel would live. The name Indian Point was eventually changed to Deep River. Kowarski's small reactor, which was eventually called ZEEP (for ZEro Energy Pile, i.e., zero energy reactor[*]), was started up at 15.45 on 5 September 1945 and thereby became the first reactor to operate outside the United States.

At the end of 1944, the Montreal scientists formed a 'Graphite Group'. The expectation was that a research station for nuclear energy matters (this would be Harwell) would be created in England immediately after the war and that it would need an experimental reactor with graphite as a moderator. The preparations for this reactor (BEPO) and its prototype (GLEEP) were made in Canada.

The British-Canadian scientists in Montreal complained about the lack of information from the United States on plutonium and about the technology for reprocessing the used fuel to no avail. The only thing they received was a few used fuel rods from Oak Ridge with milligrammes of plutonium to experiment with. Bertrand Goldschmidt has recounted how the choice was made between two reprocessing methods. One was that which the Americans had used and which had been worked out by Seaborg. Goldschmidt knew of the method as he had worked with Seaborg in Chicago before the restrictions for the British-Canadian scientists. However, Seaborg had thought that there was another, better method but lack of time meant that the first method was the one that was used.

All reprocessing methods start with storing the used nuclear fuel for a period while waiting for the short-lived radioactive substances to disintegrate and then dissolving the fuel element in nitric acid. In

[*] An inappropriate name - it ought to have been called the zero output reactor.

nuclear weapons production, this intermediate storage period can be short – three weeks to three months – since the fuel has only been in the reactor for a short time and the more long-lived radioactive substances have therefore not had time to achieve such high activity. In the civilian handling of fuel from power reactors, the storage period is considerably longer, maybe ten years. In the Manhattan Project, the Americans then added chemicals that precipitated the plutonium together with a few other substances from which the plutonium was then purified. This process was the best documented and involved the least risk of failure but it could not be run continuously.

The method that Seaborg actually preferred was based on extraction using a suitable organic solvent. This involved finding a solvent that absorbed the plutonium but not the fission products. When the acid solution is combined with the solvent, the uranium and the plutonium are transferred to this. They can each be washed out of the organic solution at a later stage. In the year that followed the arrival of the irradiated fuel rods from Oak Ridge in July 1944, the scientists in Montreal examined two hundred and fifty different solvents before they found one or two that seemed to be useable. In October 1944, they were then able to give instructions on the way in which a reprocessing plant could be built. However, there was still a great deal of work to do before even an experimental plant could be erected, but the research results in Montreal provided a stable basis which also offered the conditions for the British success with a reprocessing plant in Windscale after the war. The method described here was further developed in the United States in the 1950s and has become the most used under the name of the Purex process (from *P*lutonium *U*ranium *R*edox *EX*traction).

In autumn 1945, the chemists moved from Montreal to new laboratories in Chalk River where there were better possibilities for the remote handling of highly active material. The planned large experimental reactor called NRX (National Research X-metal, 'X' designating the still secret uranium) was built at Chalk River in 1945-1947 for an output of 10 MW, with material that was received from the United States: approximately ten tonnes of heavy water and equal amounts of metallic uranium. The shortage of heavy water meant that the NRX reactor could not use the water as both a moderator and a coolant. The consequence was that the reactor had to be cooled using ordinary water, although this absorbed more neutrons. So, it was even more important that the encapsulation material around the uranium rods did not also steal an unnecessary number of neutrons. Aluminium was therefore chosen not just as the encapsulation material but also for the cooling water tubes and for the reactor tank in which the encapsulated uranium rods were lowered into heavy water. The reactor tank was surrounded by a graphite shield that was intended to reflect back neutrons.

The construction of the NRX reactor took longer than expected. This was due to a lack of material and qualified manpower. The delay worried both Chadwick in Washington DC and Sir John Anderson in London since the NRX reactor was thought to be very important to the post-war cooperation with the Americans and their perception of the competence among the British. Keeping in mind the limited resources, it was still something of a feat to get the reactor ready for operation on 22 July 1947. The NRX reactor had an appreciably high neutron flux and was considered to be one of the best research reactors in the world for many years. It was also one of the most important producers of induced radioactive substances such as cobalt-60 for medical radiation treatment.

In 1952, the operations in Chalk River were transferred to a newly-formed state company, Atomic Energy of Canada Limited (AECL). Two years later, this company started cooperation with the power companies Ontario Hydra and Canadian General Electric (the company's name at the time) to build nuclear power plants. Surprisingly enough, the isotope production in the NRX reactor was used by Eldorado Mining & Refining Ltd., which began selling medical radiation devices with reactor-produced cobalt-60 as the radiation source.

On 12 December 1952, a sensational accident occurred in the NRX reactor. One of the employees accidentally opened some valves which meant that the control rods in the reactor changed position. The result was that a warning light was lit. The operating engineer then left the control panel and went down to the reactor to see what had happened. He found the opened valves and closed them.

In the control room, the warning light had gone out, supposedly indicating that the control rods had returned to the right position. This was not the case, however. They had jammed in an extended position which made it even more dangerous to run the reactor.

When the operating engineer called the control room, he was told that everything was OK. He then asked his colleagues in the control room to press a couple of buttons which would restore the normal operating conditions. Unfortunately, he gave the wrong button numbers. The employee in the control room therefore pressed a button which withdrew additional control rods. This would not have been dangerous had the first rods not already jammed, but the reactor output now increased.

The increasing reactor output was observed in the control room and the button that would push in all the control rods into the reactor and stop it was pressed. But for some reason, all rods were now stuck apart from one and the output continued to rise. Drastic measures were now needed and the heavy water was released into a pool beneath the reactor. At the same time, the measurement instruments sent out an alarm to indicate radioactive contamination of the air in the building, which was evacuated immediately with the exception of the control room where the personnel donned gas masks.

The temperature had now risen so far that the fuel element had started to melt. The uranium then came into contact with water vapour and reacted, forming hydrogen. The hydrogen caused detonating gas explosions and the reactor broke down completely.

As luck would have it, no human being received dangerously high doses of radiation but it took time before the tidying up process after the accident could be started. 4 000 cubic metres of water* containing approximately 400 terabecquerels (four hundred thousand billion becquerels) of radioactive substances, of which approximately 40 terabecquerels were of strontium-90, had to be pumped up from the basement beneath the reactor. The reactor had to be taken apart, decontaminated and reconstructed. The clearing up work involved a radiation burden for the workers, so as many people as possible were engaged to do the work in order to share the radiation exposure among many people and prevent anyone from receiving too high a dose of radiation. The highest individual radiation dose was 160 millisieverts. In 1963, the collective radiation dose (the product of the number of people and their average dose) was estimated at 2 000 man·rad, i.e., 20 man·gray or, in this case, also 20 man·sieverts. At these radiation doses, it was unlikely that any human would come to any harm.

As an emergency measure, the spent radioactive cooling water was pumped out into the trenches that had been dug into the sand in the area where radioactive waste was usually stored. Subsequent measurements showed that the ground had great capacity to bind the radioactive substances, but it was said in 1956 that an activity corresponding to approximately 40 million becquerels of strontium-90 per day reached the Ottawa River. By comparison, it can be mentioned that when the radioactive fallout from the nuclear weapons testing in the Pacific Ocean and the Soviet Union was at its greatest in 1963, approximately 4 million becquerels of strontium-90 fell on the Ottawa River every day and ten thousand times as much over the river's catchment area.

The operation of the NRX reactor was resumed as early as 1953.

FRANCE

As well as the pioneering von Halban and Kowarski, three other Frenchmen were involved in the British-Canadian cooperation in Canada: chemists Bertrand Goldschmidt and *Jules Guéron* and physicist Pierre Auger. However, the latter returned to France as early as summer 1944 following the liberation of the country. The French group made important achievements but it was too disjointed to be able to act on behalf of France. The situation might perhaps have been quite different if Joliot had left Paris and moved to London.

On 11 July 1944, General *Charles de Gaulle* (1890-1970) visited Ottawa. He, like other representatives of the Free French, was completely unaware of the American nuclear weapons programme at the time. Auger, Guéron and Goldschmidt therefore decided to contact him. Goldschmidt writes about this in the English update of his book *Le Complexe Atomique* ('The Atomic Complex'):

> The matter was delicate, for we could not tell the British authorities of our intentions, and we did not wish to inform anyone other than the general himself. As a first step,

* There was a large quantity of water but the NRX reactor used heavy water only as a moderator; the coolant was ordinary water.

> while not divulging the reason, we had to persuade the Free French delegate in Canada, Gabriel Bonneau, to seek an interview for us with the general on a top secret matter of the highest importance, during the 15 minutes or so that he was expected to spend with the French delegation in Ottawa. Bonneau, trusting in our good faith, agreed to ask the leader of Fighting France to receive just one of us in private. Thus it was Guéron, whom de Gaulle already knew, who gave him our information – in a secluded little room at the end of a corridor where the general, forewarned, allowed us three precious minutes of his time.
>
> We believed very sincerely, in these still difficult days for our country, that it was imperative to inform the general of the importance of the project that, in a reference in his memoirs to our disclosures, he called an 'apocalyptic undertaking.' We wanted him to be aware of the very considerable advantage that possession of the new weapon would represent for the United States, to be ready to relaunch atomic research in France as soon as practically possible, and finally to know of the existence of uranium resources in Madagascar. At that time we believed these resources to be very much larger than they turned out to be.
>
> A few minutes after his meeting with Guéron, all three of us were officially presented to the general, with other Free French representatives in Canada. When it was my turn he said simply – giving me for the first time in my life the title 'Monsieur le Professeur': 'Thank you; I have understood very well.'

The French presence in the British-Canadian nuclear energy project created new problems in 1944, the reason being that the early patents that Joliot, von Halban and Kowarski had allowed the French Research Council (*Caisse Nationale de la Recherche Scientifique*) to register since their research were paid for by the CNRS. von Halban maintained that he had a proxy from Joliot to negotiate the patent rights, including later patents that von Halban and Kowarski registered in England during their research at Cambridge. Since this research had been paid for by the British government in the hope of being able to participate in the patents, von Halban entered into an agreement which gave the British government the full patent rights within the British Empire and the French the same right within the French areas. The British hoped that the patent rights would give them an extensive monopoly, but the Americans frowned upon the agreement.

In November 1944, von Halban asked to be able to travel to the recently-liberated France by way of London to give Joliot the patent agreement. Chadwick and Cherwell, who happened to be in Washington DC, found this unsuitable since it could signify the start of a flow of secret information to Joliot and maybe then on to the Russians. They warned Sir John Anderson in London but, after having consulted Churchill, he allowed von Halban to begin his journey.

In London, von Halban explained that it would be impossible for him to discuss the patent issues with Joliot unless he was also able to inform him of the development of the research since 1940, and primarily about the properties of plutonium as fissile material. Anderson consulted the American ambassador in London, *John Winant* (1889-1947). The latter sent an enquiry to Washington DC where General Groves definitely wanted to prevent von Halban's visit to Paris. However, Winant did not receive any information from Washington DC in time and, following renewed prompts from Sir John, he consented to the visit. Groves was furious when he found out and thought that the matter ought to have been discussed by the Joint Policy Committee. To top it all, von Halban did not succeed in obtaining Joliot's approval for the distribution of the patent rights. This event did nothing to improve French-American relations.

Sir John continued to plead for French participation in the British-American cooperation. He warned Churchill that Joliot might start cooperating with the Russians if he were not treated well. Churchill made reference to the Quebec agreement which did not allow any flow of information to unauthorised countries, and Sir John had to give way. von Halban's action made it impossible for him to be involved in the continued cooperation between British and Americans and he could not, and nor did he want to, return to Montreal. In April 1945, he left his assignment under the Department of Scientific and Industrial Research and ended up settling in Oxford in 1946 after being invited by Lord Cherwell and Professor Simon.

The other Frenchmen's contract in Canada expired at the end of 1945, and Guéron and Kowarski then wanted to return to France. Goldschmidt would have chosen to stay a while longer to finish research into plutonium extraction, but General Groves demanded that he also leave Canada. There was no longer room for any French in the British-American cooperation.

In spring 1945, Raoul Dautry, the minister who had supported Joliot's research at the start of the war, had once again obtained a ministerial post, now in General de Gaulle's provisional government. He reminded de Gaulle how important it was to resume the nuclear energy research without delay and pointed out the role that Norway could be expected to take on bearing in mind the production of heavy water. A couple of months later, Joliot and Pierre Auger proposed the creation of a special organisation to deal with such matters.

On 18 October 1945, the French State took over the production and distribution of gas and electricity. At the same time, the responsibility for all nuclear energy matters, not just concerning research and industry but also the military defence, was given to a new organisation, the French Atomic Energy Commissariat (*Commissariat à l'Énergie Atomique*, or CEA).

Formally speaking, the CEA would answer to the Prime Minister but the organisation enjoyed a high level of administrative and economic independence along the lines of the company called Renault. The responsibility for administration and economics fell to a Managing Director who was a government representative. The responsibility for the scientific and technical work fell to an 'haut commissaire' or high commissioner, who was Frédéric Joliot from the start. The administrative director was Raoul Dautry. The CEA thus had two leaders, each within his own particular area. They were supported by a committee which included a representative of the Defence; this committee could be seen as the CEA's board.

With France being excluded from the British-American cooperation, there was no access to any great quantities of uranium. The uranium that was available was a total of 10 tonnes, some of which had been hidden in Morocco and some of which had accidentally been found in a Belgian goods wagon in Le Havre in the form of sodium uranate. The available quantity of uranium made it impossible to build a graphite-moderated reactor, so the only option that remained in practice was to use heavy water. As luck would have it, it was possible to use the knowledge and experience that Pierre Auger, Bertrand Goldschmidt, Jules Guéron and Lew Kowarski had from Canada in such a case. The Norwegians were also standing by their previous agreement on the supply of heavy water and promised to give it priority.

The first experimental reactor was called ZOÉ (*Z*éro, i.e., zero [power], *O*xyde, i.e., oxide [fuel], *É*au lourde, i.e., heavy water). It was built in 1946 under the leadership of Kowarski in an old castle, Fort de Chatillon, near Fontenay-aux-Roses east of Versailles, one of the many fortresses forming a ring to surround Paris. The reactor was commissioned in 1948 and it gave the French a lot of confidence. The appearance of ZOE was a major event in France. The name ZOÉ was used in various commercial ways; such as the sale of a soft carbonated drink called 'ZOÉ, le soda atomique' (ZOÉ, the atomic soda), a drink 'which provides endless energy like the atomic pile'.

The space in Chatillon soon proved to be too limited. Therefore, on l May 1947, the CEA took over 271 hectares of land at Saclay in Essonne, 8 km south of Versailles. The first French atomic research centre, *Centre d'Études Nucléaires de Saclay*, was inaugurated there in 1949, for research within metallurgy, nuclear chemistry, nuclear physics and radiation biology.

The matter of nuclear weapons was kept open throughout this early development of nuclear energy research in France. No decision was made until much later. In June 1946, the French UN delegate Alexandre Parodi said in an address to the UN's newly-formed Atomic Energy Commission:

> I am authorised to state that the objectives assigned by the French government to the research carried out by her scientists are entirely peaceful. It is our hope that all the nations of the world will as soon as possible take the same course; and with this end in view France will readily submit to those rules which shall be considered the best for ensuring worldwide control of atomic energy.

In 1950, the French Atomic Energy Commissariat was shaken by a severe crisis. The cold war had increased during 1948-1949 with the Berlin blockade and the formation of NATO. Joliot, who had sympathised strongly with the Communists since his contacts with the resistance movement during the war, participated in protest meetings which condemned NATO. He explained that as a scientist he would never participate in the development of nuclear weapons or participate in any preparations for war against the Soviet Union. In 1949, Joliot was chairman of a peace congress in Paris. The protests of the French Communists against the increasing American nuclear weapons armaments increased the pressure on Joliot to make strong public statements. This also increased the pressure on the French government to consider the way in which it would handle an awkward '*Haut Commissaire*' [High Commissioner]. The result was that Joliot was dismissed on 28 April 1950 with the following justification (quoted from Goldschmidt):

> The Prime Minister has made it known to the government that he has, regretfully, been obliged to end M. Joliot's term of office. M. Georges Bidault* has pointed out that, whatever the scientific merits of this scientist, his public statements and his unreserved acceptance of the resolutions voted by the Communist party's recent congress make it impossible for him to remain as *Haut Commissaire*.

Joliot was succeeded by Francis Perrin, one of the pioneers of French nuclear physics. Dautry, who died the following year (1951), was succeeded by *Pierre Guillaumat*, a mining engineer and oil expert. A third person who ended up playing an important role in the development of French nuclear energy was Secretary of State *Felix Gaillard* (1919-1970), who worked for the Prime Minister. Within the government, Gaillard was responsible for the nuclear energy programme from 1951-1953.

The circumstances determined the development. The shortfall of uranium meant that a uranium-235 enrichment plant could not be considered, so the only possibility that remained was the production of plutonium. Perrin wanted to keep this production low as he was afraid that access to a great deal of plutonium would arouse the interest of the military and increase its influence over the CEA. The government, on the other hand, was not against the idea of a French nuclear weapon.

In this situation, Gaillard pulled off a coup. He explained on the radio that France intended to produce 50 kg of plutonium per year. Perrin saw this as a political bluff. He thought that it was not possible to rapidly build sufficient numbers of plutonium-producing reactors. However, Gaillard succeeded in gaining the support of Guillaumat and was thereby able to control the CEA's endeavours in practice. Together, Gaillard and Guillaumat drew up a five-year plan for the French nuclear energy work, a plan that would cost 40 billion francs which at that time corresponded to 100 million dollars. The plan involved the construction of plutonium-producing, graphite-moderated reactors and a reprocessing plant to extract the plutonium. It was decided to locate the French plutonium production in Marcoule, 30 km north of Avignon.

This five-year plan was adopted by Parliament in July 1952. No statements were made regarding the possibility of nuclear weapons, but the government's comments on the plan ended with the following words: 'It is our duty, today, to ensure that in 10 years' time France will still be an important country in the modern world'. The Communists were suspicious and realised there was a possibility of a nuclear weapon. They suggested an assurance that the plutonium to be produced would never be used for nuclear weapons. This proposal was voted down by a large majority in Parliament.

The adoption of the five-year plan involved a major change to the CEA's operations and position. From having been principally a scientific research organisation, the CEA now became industrially-orientated with military undertones. However, there was still a long road to French nuclear weapons. In December 1954, as Prime Minister, *Pierre Mendès-France* (1907-1982) made the decision to start research into nuclear weapons, but the decision was not initiated before his government fell. Following long European discussions on defence cooperation, the French Senate decided to set up a military department within the CEA in June 1956. The then Prime Minister, *Guy Mollet* (1905-1975), officially admitted that nuclear weapons research was taking place and declared that France would not assist with any agreement that could limit the country's freedom to arm itself with nuclear weapons.

* *Georges Bidault* (1899-1983) was the French Prime Minister from 1949-1950 and had helped to create the North Atlantic Treaty.

The next international development to affect the French nuclear weapons decision was the Suez crisis. When Egypt's President *Gamal Abdul Nasser* (1918-1970) had nationalised the Suez Canal in July 1956, Israel's lightning attack on Egypt followed on 29 October, in turn followed by the landing of French and British troops in Port Said on 5 November, the day after the Soviet troops had stifled the uprising in Hungary.

After the Soviet Union had condemned the French-British intervention, President *Dwight Eisenhower* (1890-1969) convinced the British to withdraw in order not to risk Soviet action where the use of nuclear weapons was not precluded. The French were thereby also forced to discontinue their action and found that they were abandoned by all of their Allies while having substantial difficulties with war in Algeria. The idea of having their own nuclear weapons now seemed a greater temptation than ever.

The final step was taken in November 1956. Mollet's government instructed the CEA not just to do research into nuclear weapons but also, following the government's statement, to develop weapon prototypes and organise test explosions. The CEA were also requested to prepare the construction of a uranium-235 separation plant. France would become a nuclear weapon state.

THE SOVIET UNION

Following the big Soviet nuclear physics congress in Moscow on 20-26 November 1940, opinions were divided as to how to continue with nuclear energy research. Kurchatov had made it quite clear that extraordinary efforts would be required to start a chain reaction. He had prepared a letter to the government with a request for greater resources. Khlopin on the other hand had made a statement that advised against such an initiative while the war was ongoing in Europe and there was also a greater need for resources within other areas.

The Uranium Commission met on 30 November. Fersman and Khlopin gave an account of the stocks of uranium. They thought that mining uranium ore would require significant investments and that the options were not that promising. Other alternatives such as obtaining uranium from existing stocks within industries that used the metal for different purposes were therefore discussed.

At the start of 1941, the Russian scientists had noticed that articles which discussed nuclear fission no longer seemed to appear in the periodicals from England, Germany and the United States. On 3 December 1940, Joffe had written to Niels Bohr and complained about the lack of news about the progress of research abroad, but Bohr had given nothing more than a vague response.

The Soviet Union was now most interested in the possibility of enriching uranium-235. Scientists prioritised two methods: thermal diffusion and centrifuging. Electromagnetic separation was thought to require too much energy. Thermal diffusion was studied at the Radium Institute, among other places, while the advocates of the centrifugal method were at the Ukrainian Physical-Technical Institute (UFTI) in Charkov, an institute set up by Joffe in 1928 with the support of the Ukrainian authorities. The Biogeochemistry Laboratory in Moscow (set up by Vernadsky in 1930) undertook to produce uranium hexafluoride.

The Uranium Commission's work was made difficult for several reasons. One was what Vernadsky called 'the routine and ignorance of the Soviet bureaucrats'. Another was the already-mentioned conflict between the two dominant groups - the physicists and Vernadsky's group. There were strong confrontations but none of the scientists lowered themselves to using political methods such as accusations of Anti-Marxism.

The ongoing World War limited the Soviet Union's research resources. The country had not yet been directly dragged into the war but had already been hit hard by Stalin's cleansing in 1937-1938 and by the embarrassing winter war against Finland in 1939-1940. The military successes of the Germans in France were worrying, but Stalin did not believe that Hitler would start a war against the Soviet Union for as long as the UK remained undefeated. However, for safety's sake, he annexed the Baltic States and Bessarabia and Bukovina, i.e., the areas bordering on the Ukraine.

The German attack of 22 June 1941 came without any forewarning or declaration of war and the Red Army, caught off guard, initially suffered major losses. A special Defence Committee was set up with Stalin as Chairman and the Soviet Union's second in command, *Vyacheslav Molotov* (1890-1986), as Deputy Chairman. Other members were the head of the Soviet Security Service, the People's

Commissariat for Internal Affairs (NKVD, a forerunner to the KGB), *Lavrenti Beria* (1899-1953), Marshall *Kliment Voroshilov* (1881-1969) and the Secretary of the Central Committee, *Georgy Malenkov* (1902-1988).

The day after the German attack, the Soviet Academy of Sciences' presiding committee met to discuss the war efforts that could be made by the scientists. After the meeting, some of the leading chemists wrote to Stalin and suggested setting up a special organisation to steer the scientists' efforts, an initiative corresponding to the one taken by Sievert in Sweden two years previously. The chemists were called to a meeting with Molotov who made a note of their proposal. A special Council for Science and Technology was established. The Council included prominent Academy members such as Joffe, Kapitsa, and Semjyonov. Its Chairman was *Sergej Kaftanov*, a man who was responsible for higher education within the Soviet Union.*

The Council for Science and Technology was given the responsibility of organising military research within the scientific institutes and evaluating proposals for research programmes. After a while, 90-95 per cent of the research within the physics institutes was aimed at military information.

Kurchatov's institute was abandoned and Kurchatov moved first of all to Sevastopol where, instead of working with nuclear research, he worked with developing methods to protect ships against magnetic mines, and then to Kazan in the Republic of Tatarstan, which was were some of Joffe's institute had moved to. Many other scientists came to Kazan, including Khariton and Zeldovich, and whole of the Radium Institute was moved there during the war.

Vernadsky and a number of older Academy members were moved to a health resort in Kazakstan. Before Vernadsky left Moscow on 15 July 1941, he wrote to his son that he was extremely happy that the Soviet Union was now firmly linked with the Anglo-Saxon democrats: 'It is precisely here that our historic place is'. He hoped that a victory in the war would lead to democracy and freedom of thinking.

The war became increasingly arduous. In October 1941, the German troops were on the outskirts of Moscow but their continued advance was prevented by the Russian winter and the Germans were forced to retreat. The government moved to Kuybyshev (Samara), by the easternmost curve of the Volga 800 km east-southeast of Moscow but Stalin remained in Moscow. In spring 1942, a German offensive was started which led to conquest of Crimea. The purpose of the continued German offensive during the summer was to conquer the Caucasian oil fields and Stalingrad where the Russian armaments industry was concentrated.

It is surprising that the Soviet leaders had time to give any thought to nuclear weapons research under this pressure, but they did actually do so. The British Maud Committee's report in the summer of 1941 had been noted by the NKVD's representative in London. The information probably originated from Lord Hankey's secretary who was a Soviet agent. The information that reached Moscow was detailed and revealed much of the contents of the Maud Report. The information did not lead to any immediate reaction since it came during the most hectic days of October 1941 when it was uncertain as to whether the Germans would capture Moscow.

However, it was not long before Beria issued a report to Stalin and the Defence Committee with a recommendation that the information from England be evaluated. The NKVD asked the Academy of Sciences what the likelihood was that other countries were in the process of developing nuclear weapons. Khlopin, who evidently did not get to see the agent's information, answered that the only sign of such an operation was the obvious secrecy which meant that scientific publications within the field were no longer issued. However, Kaftanov and Joffe recommended that the Defence Committee create a nuclear research institute.

The consultations with the scientists continued. In 1942, Joffe, Kapitsa, Khlopin, Kurchatov and Vernadsky were summoned to Moscow for discussions with people such as Kaftanov regarding the resumption of nuclear energy research. One important question was who would be responsible for acting

* Kaftanov is notorious for his intervention in 1948 to support the charlatan *Trofim Lysenko* (1898-1976) and the latter's heresies regarding heredity doctrine. As Minister of the universities, Kaftanov ordered the dismissal of all "supporters of the chromosomal theory", the destruction of all educational books on genetics and even that all of the fruit flies, geneticists' usual research objects, be killed.

as leader. The 63-year-old Joffe declined with reference to his age, but he recommended Kurchatov, who was given that assignment in the end.

At this time, the 39-year-old Kurchatov was not a particularly well-known physicist but he had made a good impression on Kaftanov. Kurchatov had just started cultivating a beard that made him look like an orthodox priest. He would not shave his beard until the Germans had been defeated, he said. His nickname then became ‘The Beard’ (*Boroda*).

However, a good while passed before Kurchatov was formally given the assignment. The Defence Committee had not yet given the scientists the agent’s information from England but in autumn 1942, Molotov showed it to *Michail Pervushin* (1904-1978) who was Deputy Prime Minister and the People’s Commissar for the chemical industry. Molotov said that Stalin wanted to hear Pervushin’s view of the espionage material. Pervushin’s response was that the physicists ought to be able to see the material and express their opinions on it. Molotov did not want to go along with this. Instead, he asked Pervushin to ask the forerunning physicists what they knew about what was happening abroad and what was going on in the Soviet Union.

On 9 January 1943, Pervushin met Kurchatov, who said that to all appearances it was possible to achieve an immediate chain reaction in uranium-235 with the development of enormous quantities of energy. Pervushin asked Kurchatov and a couple of colleagues to write a memorandum on Molotov’s behalf. This took no more than a few days and Pervushin then handed the memorandum to Molotov together with a recommendation to take the physicists’ proposal seriously. Molotov then asked Pervushin and Kurchatov to jointly write a report on the possibilities of a bomb and to estimate the time it would take to produce an atomic bomb. Molotov recalls (according to Holloway):

> I was entrusted with responsibility for this research, with finding the kind of person who would be able to realise the creation of an atomic bomb. The Chekists [i.e., the NKVD] gave me a list of reliable physicists who could be depended on, and I made the choice. I summoned Kapitsa, the Academician. He said we were not ready for that, and the atomic bomb was not a weapon for this war, but a matter for the future. Ioffe was asked – his attitude to this was also unclear. In short, I was left with the youngest, Kurchatov, who was not known to anyone… I summoned him, we had a talk, he made a good impression on me.

Finally, Kurchatov was formally given the task of being the scientific leader of the atomic bomb project on 10 March 1943. The whole the procedure of creating this project took place at the same time as the ongoing fight for Stalingrad, and the Soviet Union was transformed from suffering a loss into being a great power. Stalin knew at the start of 1943 that the German atomic bomb efforts were not realistic; he had received information to that effect from Klaus Fuchs before the latter left England for the United States. Fuchs had found out about the information on the situation in Germany which was sent to England by Paul Rosbaud. Stalin’s interest in the atomic bomb was therefore not conditional upon its unavailability during the ongoing war - but he probably also had no realistic perception of what significance the atomic bombs would have after the war.

The Leningrad physicists were now in charge of the resumption of nuclear physics research. This irritated the Vernadsky group but neither Vernadsky nor Khlopin knew what had really happened. Their written complaints show that they primarily felt like outsiders and were offended at not having been asked.

Not until Kurchatov was given his assignment in March 1943 did Molotov decide to show him the agent’s reports. He was very impressed. According to Holloway, he wrote:

> On one hand, the material has shown the seriousness and intensity of research in Britain on the uranium problem; on the other, it has made it possible to obtain very important guidelines for our research, to bypass many very labour-intensive phases of working out the problem and to learn about new scientific and technical ways of solving it.

This was available to read in a detailed report that Kurchatov gave to Pervushin on 7 March. Professor Simon's proposal for a gas diffusion plant for the enrichment of uranium-235 was the main thing that lodged in his mind. The secret information from England also confirmed that which Zeldovich had recently ascertained, i.e., that thermal diffusion offered no viable way forward.

Kurchatov was also surprised to see that von Halban and Kowarski had found it possible to use heavy water as a moderator to achieve a chain reaction in natural uranium - the Russian scientists had thought this was impossible.

Other important information was that it could be possible to use fissile transuraniums (i.e., plutonium) instead of uranium-235 in a bomb, which would render enrichment plants unnecessary.

Kurchatov was pleased to find that Otto Frisch had confirmed the occurrence of spontaneous nuclear fission, the discovery that had already been made in 1940 by Flerov and the latter's young colleague *Konstantin Petrzjak* in an experiment proposed by Kurchatov. In fissile nuclides such as uranium-235 and plutonium-239, some atomic nuclei are split spontaneously without the effect of external neutrons. This spontaneous fission made it impossible to bring together all the fissile material into one clump to then be detonated through the addition of neutrons - it would detonate by itself. The Russians were therefore sure at an early stage that an atomic bomb had to be produced so that the nuclear explosive was distributed in a way that no criticality could be achieved until the parts were rapidly brought together using a conventional explosive.

On 22 March 1943, Pervushin received a second report from Kurchatov. The latter had now carried out more in-depth studies of the usefulness of the new elements that he called 'eca-rhenium' and 'eca-osmium', i.e., the elements with the atomic numbers 93 and 94 (neptunium and plutonium). He wrote that completely new possibilities could be seen where the solving of the 'uranium problem' was concerned, and added that 'the prospects of this direction are unusually attractive'. If eca-osmium had the same properties as uranium-235, it would be possible to produce an eca-osmium bomb (i.e., a plutonium bomb) from material produced in a 'uranium boiler'.

Following a decision by the high Defence Committee to invest in the nuclear research, on 12 April 1943 the Soviet Academy of Sciences issued a secret instruction to set up a new laboratory called Laboratory no. 2. It was first housed in the premises of the Seismological Institute in central Moscow but within a year, Kurchatov had found an area near the Moscow River north of the city where he saw development prospects. He took over a building that was originally intended for the Soviet Institute for Experimental Medicine but was soon able to erect additional buildings. On 25 April 1944 there were 74 personnel, 25 of whom were scientists.

Kurchatov prioritised the construction of a reactor and initially intended to use heavy water as a moderator. But by July 1943 he had already changed his mind and instead decided to use graphite which ought to be possible to produce with sufficient purity within his country. However, the planned reactor would then require more than 50 tonnes of uranium. Kurchatov gave *Abram Alichanov* (1904-1970) the responsibility for having the reactor built, something which the latter agreed to with some trepidation. He was jealous of Kurchatov and had hoped to be given the assignment that Molotov had in the end given to Kurchatov.

Kurchatov realised that it would take several years to build the reactor but he was eager to be able to study the properties of eca-osmium, i.e., plutonium, so as early as March 1943 he asked *Leonid Nemenov*, a man who had previously worked with Joffe's cyclotron in Leningrad, to get a cyclotron going in Moscow within sixteen months.

Nemenov moved from Moscow to Leningrad to obtain important parts of the Leningrad cyclotron that had been hidden when the personnel left the Institute prior to the German advance. The siege of Leningrad had been stressful and its inhabitants endured a lack of food and other necessities. In January a Russian counteroffensive had opened a communication path to what the Leningraders called the 'mainland'. Nemenov and his attendants had an introduction letter from Pervushin for the party secretary in Leningrad. They also had with them hundreds of packages from colleagues for their relatives and friends in the besieged city. They found the components they were looking for, including the 75-tonne electromagnet that was set up just three kilometres from the front line and, with the help of Russian soldiers, succeeded in loading everything onto two rail wagons for transportation to Moscow.

Many important parts of the cyclotron were missing and had to be produced in Moscow. However, Nemenov succeeded in completing the task two months before the deadline established by Kurchatov. After having made sure that the cyclotron worked, Kurchatov invited the work group home to celebrate the event with champagne. Then they started irradiating uranyl nitrate to produce neptunium which might then create plutonium. However, they did not succeed in separating any plutonium from irradiated uranium until 1946. In order to improve his relations with the dissatisfied Khlopin, Kurchatov asked the Radium Institute to try plutonium separation.

Kurchatov now had considerable power. He had the right to exempt scientists from military service and he was in charge of 100 passports that allowed their holders to live in Moscow, a privilege that was very limited. When it became known that Alichanov would be voted into a vacancy at the Academy of Sciences, Joffe and Kaftanov asked the government to introduce one further place for Kurchatov, which they did despite protests from older physicists who did not think he had adequate scientific qualifications. Kurchatov became a member of the Academy in September 1943.

Kurchatov organised research on a broad front. Experimental physicist *Isaak Kikoin* (1908-1984) was asked to work out the method of enriching uranium-235 using gas diffusion. In 1944, *Lev Artsimovich* (1909-1973), a physicist from Joffe's Institute for Technical Physics in Leningrad, was asked to examine the possibility of electromagnetic separation. Attempts were also made with centrifuging but the centrifuge broke down at resonant frequency.

Major problems arose when it came to producing graphite and uranium for the reactor. Kurchatov's physicists had to help the technicians at Moscow's electrode factory to purify the graphite to an extent that those at the factory initially claimed was impossible. Sufficiently pure graphite was not available until summer 1945.

As early as January 1943, the Soviet government had asked the American authorities in Washington DC for assistance with uranium. General Groves did not dare to refuse since he was afraid it would arouse suspicions and reveal the American atomic bomb plans. So, in summer 1943, the Soviet Union had received an approximate total of 800 kg uranium in the form of oxide and nitrate. In autumn 1943, an advisory body was set up for uranium prospecting with Vernadsky and Khlopin as members but there was little support from the government.

The difficulties made Kurchatov impatient. He had detailed information on what the British were doing but he had no corresponding information from the United States and feared that the Americans were way out in front. It is uncertain as to whether in the summer of 1943 Kurchatov even knew of Fermi's successful reactor experiment in December 1942.

In December 1943, the Russians obtained new information from Klaus Fuchs who had left England to assist with the Manhattan Project in Los Alamos. However, Fuchs had no knowledge whatsoever of the plutonium part of the project. Kurchatov found out that the Americans were working with gas diffusion on a large scale, information which led Kikoin to concentrate on gas diffusion rather than the centrifugal method. The Russians did not have a full overview of the American activities until 1945, but by then they would soon gain access to the Americans' own report, the Smyth Report.

The work with finding a suitable construction for the bomb itself took place in parallel with the more scientific endeavours. This work was led by the 39-year-old Khariton. It started with exploring the cannon method and measuring the speeds at which parts of a nuclear charge could be brought together. However, the presence of plutonium-240 rendered the cannon method useless for a plutonium bomb. At that point, information from Klaus Fuchs contributed to an essential change to Khariton's plans – the American solution to the problem for the plutonium bomb was to replace the cannon method with the implosion method whereby a subcritical sphere of plutonium suddenly reached criticality through compression caused by an explosive charge.

This was something on which Fuchs had first-hand information. Precisely, his task at Los Alamos was to perform calculations for the implosion method. Fuchs gave his contacts detailed information on the implosion method, on the critical mass of plutonium and on how much plutonium was expected to be used in the bomb. This information was supplemented with data on the fission cross section for fast neutrons for uranium-235 and plutonium.

In June 1945, Fuchs gave detailed dimensioned drawings of the Trinity bomb and revealed that the initiator used 50 curies of polonium. He also revealed how an optimally-shaped charge had been arranged with the help of charge ‘lenses’ and that the bomb was expected to have an explosive effect corresponding to 10 000 tonnes of TNT. He also gave the time and place of the bomb test and said that atomic bombs would be used against Japan were the test to succeed.

The Russians had a good number of information providers in the United States, but none as important as Klaus Fuchs. Fuchs did not cooperate with the NKVD but with the GRU, the Soviet General Staff’s intelligence service. After the war, he returned to England where he became head of the Department for Theoretical Physics within the British Atomic Energy Programme.

When Fuchs was exposed, he was sentenced to fourteen years in prison following a trial in which he himself was responsible for producing most of the evidence for his guilt. He was released after serving two thirds of the sentence and moved to (the then) East Germany where he became deputy head of the East German Institute for Nuclear Physics. The punishment meted out by the English for the most important ‘atomic spy’ is very different from the death penalty that was enforced on the Rosenberg couple in the United States in 1953 during McCarthy’s time.*

In March 1945 when the German troops had been driven out of Czechoslovakia, the Czech government’s exiled President *Edvard Beneš* (1884-1948) returned from London to Prague. On the way there he visited Moscow where he signed an agreement which meant that the Soviet Union would be given the exclusive right to mine uranium ore in the Czech mines, including Jáchymov (Joachimstal). This was something of a coup on the part of the Soviet leadership since Beneš did not know the actual value of the uranium.

When the Red Army entered Germany, it was followed by the equivalent of the American Alsos, a group of approximately thirty scientists whose task was to study the status of German nuclear research and examine which assets could be utilised in the Soviet research. The group was led by *Avraami Zaveniagin* (1901-1956), a metallurgist who advanced to Colonel General (the highest Soviet rank of general) reporting directly to Beria within the NKVD. The group included Artsimovich, Flerov, Khariton, Kikoin and Nemenov but not Kurchatov. The most prominent scientists wore uniforms bearing the NKVD rank of Lieutenant Colonel.

Like Alsos, the Soviet group found that the German scientists had not made much progress. They had no functioning reactor and no equipment to separate uranium-235 and they had no actual knowledge of how to construct an atomic bomb. The most prominent German physicists had also been snapped up by Americans and Britons and taken beyond the reach of the Russians. Some important scientists could still be enveloped: Manfred von Ardenne, Robert Döpel, Gustav Hertz, Nikolaus Riehl and *Peter-Adolf Thiessen*, Nazi party loyalist from the Kaiser Wilhelm Institute for Physical Chemistry in Berlin. The German scientists were conveyed to the Soviet Union in May-June 1945 and kept isolated in relative comfort in a *dacha* outside Moscow.

However, not all scientists were treated equally. In Germany, there were Russian citizens at the universities who were participating in research and education. One of the better-known was Professor of Genetics *N. V. Timofeev-Ressovsky* (1900-1981). He was arrested and sentenced to 10 years in prison. He was treated like a convict and his health was completely ruined; he even incurred permanent visual impairments. When Beria’s people discovered that he could be useful in the atomic bomb project by virtue of his knowledge of radiation biology, he was moved from his team to a research institute in Sungul between Sverdlovsk and Chelyabinsk. He was treated well there and was finally given redress in 1964 following the downfall of Lysenko.

Khariton and Kikoin succeeded in their search for considerable quantities (more than one hundred tonnes) of hidden uranium oxide, which Kurchatov explained would shorten the time for the production of a reactor by at least one year.

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* *Julius Rosenberg* (1918-1953) and his wife *Ethel* (1915-1953) were the only people in the USA who were assassinated for espionage in times of peace. The legal process, which took place during the McCarthy period, is thought to have been a legal scandal, not because the couple were innocent but because they were convicted on the basis of evidence from Ethel’s brother who gave evidence against his sister to protect his wife. The Senator (from Wisconsin), *Joseph McCarthy* (1908-1957), was chairman of the Senate’s investigation committee into Communist infiltration. His heavy-handed methods and groundless accusations against many civil servants was a symptom of the fear of the Communists that prevailed in the USA at the start of the cold war. The Senate condemned his methods in December 1954 and removed him from his position as chairman.

After the war, the Russian scientists made great efforts to renew contact with the outside world. A powerful advocate for cooperation with the United States after the war was Vladimir Vernadsky, but he did not get to see the end of the war - he died in January 1945 at the age of 82. The Soviet authorities did not counteract the attempts to make contact to start with. In June 1945, the Soviet Academy of Sciences held a congress to celebrate its 220th anniversary and a hundred or so foreign scientists were invited. Very few accepted the invitation, however, one exception being the Joliot-Curie couple – chaos still reigned in Europe. Molotov was in favour of closer contacts between Soviet research and the research in other countries. Kapitsa said that there was no such thing as ‘Soviet science’, ‘British science’, etc., just one science that ought to aim to improve people’s welfare.

Despite improved conditions, the Soviet nuclear physics programme was slow to advance. Pervushin and Kurchatov certainly did have substantial authorisations but Russian bureaucracy meant that very little really happened unless someone pulled the strings of the top government leaders, which meant Stalin, Molotov and Beria. Pervushin has written (according to Holloway):

> Matters connected with the solution of the atomic problem, for several reasons caused by the war, were going slowly. Therefore in May 1945 Igor Vasil’evitch Kurchatov and I wrote a memorandum for the Politburo of the Central Committee and Comrade I.V. Stalin in which we briefly elucidated the position with the atomic problem and expressed alarm at the slow development of the work.

It is said that one reason why the controlling politicians were not keener to invest in nuclear energy research was Beria’s almost pathological mistrust. He did not believe the reports he received. According to Holloway, a subsequent KGB officer by the name of Anatoli Yakovlev (who had been the Soviet contact person in New York during the flow of information from Klaus Fuchs) wrote:

> From the very beginning Beria suspected disinformation in these reports, thinking that the enemy was trying to draw us in this way into huge expenditures of resources and effort on work which had no future. […] Beria retained his suspicious attitude to the intelligence even when work on the atomic bomb was in full swing in the Soviet Union. L.R. Kvasnikov has recounted that once when he was reporting to Beria on the latest intelligence data, Beria threatened him: ‘If this is disinformation, I’ll put you all in the cellar.’

Beria’s mistrust may have rubbed off on Stalin and Molotov; in any case, it appears as though none of them yet understood the strategic and political importance that the atomic bomb would have after the war. But they all knew what an atomic bomb was and they were very well aware of what Pervushin and Kurchatov had in mind. The information that Klaus Fuchs provided regarding the American plans to use atomic bombs on Japan was probably also known by Stalin.

It was therefore no surprise to Stalin when President Truman told him on 24 July 1945 in Potsdam that the United States now ‘had a new weapon of unusual destructive force’. According to the British Foreign Secretary *Anthony Eden* (1897-1977), Stalin had nodded and expressed thanks for the information. According to Stalin’s interpreter, Stalin had expressed nothing, but simply bent his head as confirmation that he understood what Truman had said. Marshal *Georgy Zhukov* (1896-1974) has told in his memoirs the story of what happened when the Russian delegation returned from the meeting (quoted from Holloway):

> In my presence, Stalin told Molotov about his conversation with Truman.
> ‘They’re trying to bid up,’ said Molotov.
> Stalin laughed. ‘Let them. We’ll have a talk with Kurchatov today about speeding up our work.’
> I realised they were talking about the development of the atomic bomb.

Molotov and Zhukov let it be understood that Stalin had immediately realised that Truman had been speaking about the atomic bomb and that his silence and visible lack of reaction were because he did not want to confess to the backward status of the Soviet Union, but it has also been said that Stalin was actually not yet aware of the importance of the atomic bomb. In any case, nothing mentionable happened with the Russian bomb programme until after the bombing of Hiroshima.

At the Yalta meeting in February 1945, Stalin had repeated a previous promise that the Soviet Union would enter the war against Japan when Germany had been conquered. He had a number of territorial requirements that the western powers accepted without much hesitation; they were evidently keen on the Soviet Union's help against Japan. Stalin gave no indication as to when Russia would declare war since he said that China, under *Chiang Kai-shek* (1887-1975), must first accept the Yalta agreement.

In summer 1945, the western powers gradually changed their positive view of a Russian attack on Japan, largely due to the Soviet Union's policy in Europe - a hotbed for the cold war. Stalin began to start worrying about what would happen if Japan made peace before the Soviet Union managed to attack. According to Holloway, *Nikita Khrushchev* (1894-1971) has recalled:

> Stalin was leaning on our officers to start military actions as soon as possible. Stalin had his doubts about whether the Americans would keep their word. […] What if Japan capitulated before we entered the war? The Americans might say, we don't owe you anything.

On this basis, it is not surprising that the Americans encouraged Chiang Kai-shek to delay acceptance of the Yalta agreement and that the Russians were indifferent to Japanese appeals for peace negotiations.

At the Potsdam Conference, Truman, Churchill and the absent Chiang Kai-shek had agreed on a statement which threatened the immediate and total destruction of Japan unless the Japanese government agreed to an unconditional surrender. They had done this without informing Stalin. Molotov, who was afraid that the statement could result in a Japanese surrender before the Soviet Union entered the war, tried to postpone the publication in vain. It was now just a matter of days.

When Stalin had returned to Moscow, he received a report from Marshal *Alexandr Vasilevsky* (1895-1977) saying that the preparations for the war against Japan were now almost complete and that Stalin ought to start an attack no later than 9th or 10th August to take advantage of the beneficial weather. On 6 August, the United States dropped the atomic bomb on Hiroshima. On 8 August, the Japanese ambassador was summoned to the Kremlin in Moscow and was told by Molotov that the Soviet Union considered itself to be in a state of war against Japan. The Red Army attacked the Japanese forces in Manchuria on the same day at 18.10 Moscow time with 1 ½ million men, 5 500 tanks and 3 900 aircraft. The Soviet Union had joined the war at the last minute and Japan had still not surrendered.

The atomic bombing of Japan finally made Stalin realise the importance of the bomb and led him to take measures to speed up the work with a Russian equivalent. After discussions with Kurchatov and The People's Commissar for Weapons and Ammunition *Boris Vannikov* (1897-1962), on 20 August 1945 he asked the Defence Committee to set up a special committee to control 'all work for the use of intra-atomic energy from uranium'.

Beria was appointed as chairman of the new committee. Its members included Secretary of the Central Committee Georgy Malenkov, Boris Vannikov, Michail Pervushin and two scientists, Kapitsa and Kurchatov. The fact that Beria was now directly responsible for the production of nuclear weapons was not as unreasonable as it might seem. Molotov has called Beria 'a talented organiser, but cruel, ruthless'. Stalin's daughter complained about Beria's evil influence. Beria had great power; people trembled in the face of his decisions. He was cruel, but also very efficient, particularly as he could make full use of the secret police. The fact that Beria was given the responsibility of creating an atomic bomb could be seen as a sign from Stalin of how important he now considered the project to be. Beria himself feared no-one but Stalin, the man who guaranteed his power. Without Stalin, Beria was a dead man, something that would go on to be confirmed following Stalin's death.

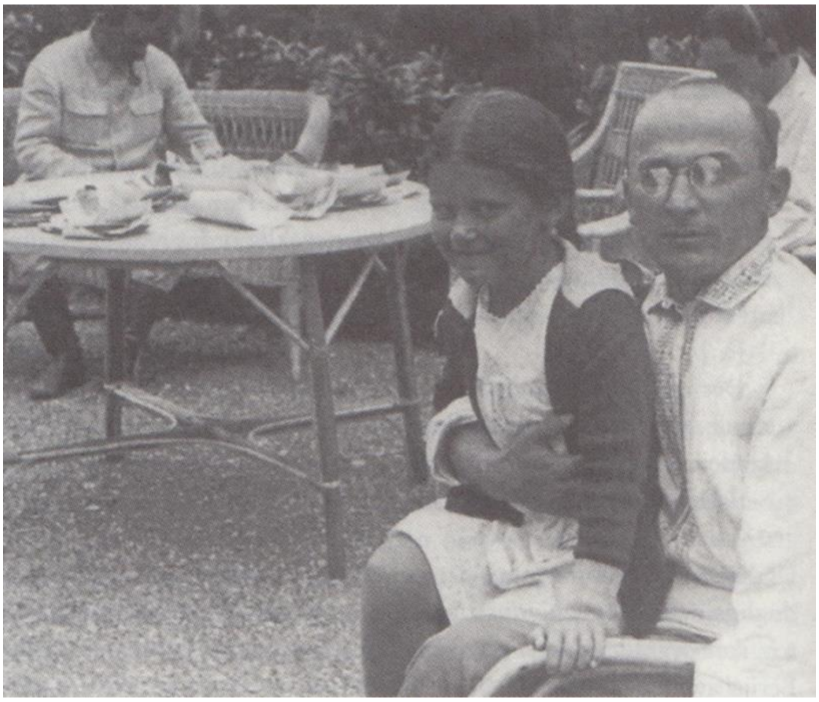
However, Beria could also show more pleasant attributes. The captured Nikolaus Riehl says in his memoirs about his first meeting with him:

> I had two encounters with Beria, the infamous organizer of the NKVD work prisons. The first relatively simple meeting occurred soon after we were taken to the Soviet

> Union. Beria had invited Hertz, Volmer*, von Ardenne, and me to visit him in order to become acquainted with us. Each was separately invited into his office where perhaps 20 other individuals, mainly scientists and a minister, were seated.
>
> Beria greeted us very cordially. His approach was exceedingly pleasant. It is well-known that people regarded his manner in personal matters to be very nice. I am told that even Himmler could be a charming companion. At the very beginning of our chat he said that our people should forget that we have been in a terrible war with one another. He had the opinion that the Germans were very proper and would display respect for orders when given. He stated, by way of example, that he had learned that if, at the end of a battle, no explicit orders were given to stop shooting the German soldiers would continue firing.
>
> [...]
>
> Outstanding was the curiosity with which Beria was observed by all present. Particularly noteworthy to me was a man with a dark beard and glowing black eyes who watched me with an unbroken friendly look. Later I learned that he was Kurchatov.

Under Beria's leadership there were now not just scientists in the project but also competent and efficient industrialists such as Vannikov and Pervushin. Kurchatov, who was responsible for the scientific leadership, decided under pressure from Beria that the fastest route to success was to use the American bomb construction as Fuchs had described it.

Beria's hard-handed leadership, with high officers from the NKVD as supervisors of the different sub-projects and the plants, irritated the Russian scientists just as much as General Groves had irritated their American colleagues. One who became tired of this early on was Kapitsa, who complained that Beria did not show the scientists enough respect. On 3 October 1945, Kapitsa wrote to Stalin and asked to be released from his membership of Beria's special committee. He did not agree with Kurchatov that the fastest route to an atomic bomb was to copy what the Americans had done. Their programme had cost two billion dollars, something which the Soviet Union would not be able to afford. There must be less expensive ways, thought Kapitsa, now that it was known that the bomb was actually possible.



Lavrenti Beria with Stalin's daughter and Stalin in the background

* *Max Volmer*, German chemist and colleague of Riehl.

When Stalin did not respond, Kapitsa wrote again on 25 November (quoted from Holloway):

> Although it will be difficult, we must in any event try to make the A.B. [atomic bomb] quickly and cheaply. But not by the path we are now following, which is completely unmethodical and without a plan. […] We want to retry everything the Americans did rather than try to follow our own path. We are forgetting that to follow the American path is beyond our means, and will take a long time.
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> [At the end of the letter Kapitsa added:] I wish Comrade Beria to be acquainted with this letter, for it is not a denunciation, but useful criticism. I would have told him all this myself, but it's a great deal of trouble to get to see him.

Beria reacted immediately. He rang Kapitsa and asked him to come but Kapitsa, who did not want to be anyone's errand boy answered that Beria had to come to his institute if they were to meet. Beria actually did so, and even had a present with him, a double-barrelled shot gun. However, Beria and Kapitsa never managed to agree and Kapitsa left the Soviet atomic bomb programme on 19 December 1945, not because of scruples but because he disliked the methods and wanted more power for the scientists. He also thought that Beria ought to learn a bit of physics.

Still, Kapitsa's criticism did seem to have made an impression on Beria. Khariton thought that Beria behaved correctly towards the scientists and helped them to obtain what they needed. Kurchatov was successful with walking the tightrope of keeping in Beria's good books and retaining the trust and loyalty of his scientific colleagues.

In December 1945, Kapitsa had completed the manuscript for an article in which he pleaded for civilian nuclear power. The atomic bombs were not that remarkable, he wrote. In Hiroshima and Nagasaki, most of the energy had radiated away as heat energy and had the Japanese not been living in 'cardboard boxes', the disaster would not have been as bad. The bomb was therefore not of unique importance; the future of nuclear energy lay in its peaceful use. Kapitsa asked Molotov for permission to publish the article but Beria opposed the publication. Kapitsa began to fall from grace. Beria wanted to arrest him but Stalin said no; not so much for Kapitsa's sake but probably to show Beria who made the decisions.

Meanwhile, Niels Bohr and Kapitsa began to communicate. Bohr reiterated his hope that Soviet and western scientists could openly discuss the consequences of a world with nuclear weapons. In November 1945, Beria had sent one of the scientific advisers to the NKVD, *Yakov Terletski*, to Copenhagen to ask Bohr about the atomic bombs that had recently been used.

Contact with Bohr was arranged for 2 November by a Communist member of the Parliament, who asked Bohr to undertake a secret assignation with Terletski who brought with him greetings from Kapitsa. An agitated Bohr explained that the contact was 'a regrettable mistake' and insisted that any meeting must take place quite openly and that he could discuss only information that was generally available. Bohr reported the attempt at contact to the British ambassador that same day and General Groves found out about it early on.

The fear was that the desired contact was the start of an attempt to kidnap Bohr, and when the meeting did finally take place on 14 November, it took place under authority protection. Aage Bohr, who was 23 at the time, was present throughout the discussion with Terletski, and the 21-year-old son Ernest was in the room outside, armed with a pistol.

Bohr has been accused of having given the Russians atomic secrets, and reference has then been made to Terletski's visit. However, the account that has been given by Aage Bohr tallies with what Terletski himself wrote in his memoirs. Terletski asked 22 questions which Bohr answered very vaguely, saying that he did not know all details. Bohr also handed over a copy of the Smyth Report, 100 000 copies of which had already been sold at that point and was being translated into Russian.

Terletski's report, with an accurate account of the questions and Bohr's responses, is now available. It was disappointing to Beria since Bohr had not said anything that was not already generally known. However, when Beria sent the report to Stalin, he gave him to understand that Terletski's assignment had been successful - the assignment was Beria's own idea after all.

On 25 January 1946, Stalin summoned Kurchatov for a talk. They had met before, but this time Stalin took his time. The talk was so interesting that Kurchatov made notes about it. Molotov and Beria were present.

Stalin made it clear that he did not want to go along with Kapitsa's proposal for a cheaper path to the atomic bomb. He said that the project must now take place rapidly and with no regard for the costs, but that he expected practical results and political loyalty from the scientists. He also wanted to improve the living conditions of the scientists. According to Kurchatov (quoted from Holloway), he thought that:

> […] our scientists were very modest and they sometimes did not notice that they live poorly. […] our state has suffered very much, yet it is surely possible to ensure that several thousand people can live very well, and several thousand people better than very well, with their own *dachas*, so that they can relax, and with their own cars.

Creating a Soviet atomic bomb required an extensive industry but the Soviet centrally-planned economy with state-owned companies was particularly suited to building this up. The production of uranium was the most acute problem. Initially, it was possible to rely on the mines in East Germany and Czechoslovakia but they also relied on mines in Central Asia, although those involved a tiresome transportation problem.

Nikolaus Riehl had been in charge of Auer's purification of uranium in Germany and was given the same task in the Soviet Union. His assignment was facilitated by the fact that he was born in St. Petersburg and spoke fluent Russian. His father was German and had been the director of Siemens' operations in St. Petersburg but his mother was Russian. Riehl was welcomed by several of the Russian physicists, bearing in mind his previous research within physics. Riehl had been a doctoral student under Lise Meitner in the 1920s but had not appreciated having a woman as a boss. He became head of research at Auer and there studied the substances that were used in sock mantles to fortify the light from gas lamps. He then invented the fluorescent lamp in the 1930s, i.e., the fluorescent (materials) tube in cooperation with the Osram lamp factory. The fluorescent tube was developed at the same time by General Electric in the United States in connection with research into suitable fluorescence for TV screens. His work with fluorescence was what made Riehl known among the Russian physicists rather than his uranium work.

The man who searched for a suitable place for uranium production in consultation with Riehl was none other than the former leader of the Soviet 'Alsos' mission, General Zaveniagin. He looked for a place with beautiful natural surroundings while Riehl wanted the uranium plant to be near Moscow or Leningrad. In the end, a suitable premises was found in an ammunition factory that had closed down just 50 km east of Moscow. It lay in an industrial complex called Electrostal, near the small town of Noginsk. It had the buildings they needed but also workshops and good workers. The uranium factory in Electrostal became the second important plant within the Soviet atomic bomb industry and its existence was kept very secret.

Riehl began to use his previous cleaning method to remove impurities from the uranium oxide that was sent to Electrostal, but the method was slow. When he came to read the American Smyth Report, he found that the American method, extraction using ether, was faster. After the metallurgical part of the process had also been improved, Electrostal was able to deliver 3 tonnes of metallic uranium per week by autumn 1946.

The work with the planned first reactor was ongoing in the first plant in the atomic bomb project, Kurchatov's Laboratory no. 2. The work was delayed primarily due to a shortage of sufficiently pure graphite. Adequate quantities of this did not become available until the end of 1945. Kurchatov took over the leadership of the work and the reactor group grew in 1946 from 11 people in January to 76 in December. In 1946, 500 tonnes of graphite were delivered. Uranium in the form of cylindrical pellets for experiments with different configurations became available at the start of the year. However, in the summer it was found that some of the uranium supplied by Electrostal was contaminated with small quantities of boron. Owing to the high neutron absorption in this substance, they were forced to take extra precautionary measures and work out methods to detect small contaminants at an early stage.

In the summer of 1946, a special building for the first reactor was erected at Laboratory no. 2. It measured 5 metres x 40 metres and inside the building was a 7-metre-deep cavity that was intended for the reactor. The reactor was also surrounded by biological protection consisting of soil and sand and, in the critical places, also lead and blocks of paraffin and boric acid. There were measurement instruments in different places in the building, connected up to alarm devices in case the level of radiation outside the reactor became too high.

It was difficult to calculate theoretically the size of the reactor since there were still small quantities of impurities in both the uranium and the graphite. Experimentation told them that 50 tonnes of uranium and 500 tonnes of graphite would be needed and that the reactor would have a diameter of 6 metres. The reactivity would be kept under control with the help of three cadmium control rods.

At 14:00 on 25 December 1946, the final layer of graphite was added and they realised that the reactor was now close to criticality. All those who were not directly involved with taking measurements were asked to leave the building and Kurchatov manoeuvred the control rods. At 18:00, the device became critical and the Soviet Union had achieved its first functioning nuclear reactor. Before turning it off late at night, Kurchatov had briefly increased the output to 100 watts. Later, the reactor was run for longer periods with several tens of watts and briefly up to almost 4 kilowatts, doing so by using a special control panel that was set up at a distance of more than one kilometre from the reactor.

The finished reactor contained 45 tonnes of uranium and 400 tonnes of graphite, i.e., was of approximately the same size as Fermi's reactor four years previously. It was called the F-1 (for *Fizicheski-1*, i.e., 'Physical-1') and was supplied with a horizontal tunnel with a cross section of 40 cm x 60 cm to facilitate experiments with different materials and different lattices of uranium pellets. The F-1 reactor turned out to be very useful as an experimental reactor. The shielding capacity of different materials was tested and laboratory animals were irradiated to gain an idea of the biological effects of neutron radiation.

A few days after the reactor had first become critical, Beria visited to see a demonstration. He stood next to Kurchatov at the control panel but was disappointed when he thought that nothing had happened. Pointers moving on measurement instruments and clicking sounds in amplifiers were not something that impressed him. He could not let go of from his suspicion that the physicists were palming him off.

It was then time to build the first plutonium production reactor, a significantly more difficult task due to the high output required which called for the reactor core to be cooled. To this end, the reactor had to be supplied with cold water tubes as was the case with the Hanford reactors. The first question was where the plant, 'Soviet's Hanford', would be located.

Coincidence led to an obvious answer to the question. The person immediately responsible for the decision regarding location was still General Zaveniagin who, at the start of his career in the Supreme Soviet, had represented an area in the Urals and was very fond of its natural surroundings with mountains, forests and lakes. Strange though it sounds, he liked the idea of the first production reactor being located in these beautiful natural surroundings and he had found a suitable place, an area of land which had belonged to Baron *Mellor Zakomelski* before the October revolution. Strangely enough, before the First World War, the estate had been taken care of by an American company, the director of which was none other than the man who later became the President, *Herbert Hoover* (1874-1964). Its name was Kyshtym, and Hoover had assisted with developing mining and copper production in the area. However, this operation had closed down after the revolution.

So, General Zaveniagin chose the area next to Kyshtym (which was now a small village) for the new plant. It was given the name of Chelyabinsk-40 as per the Soviet tradition of calling secret plants after their nearest town and a postcode. The nearest large town, 80 km away, is Chelyabinsk, which became a town in the 1700s but saw an upturn in the 1890s through the Trans-Siberian Railway. In 1947, Chelyabinsk had 273 000 inhabitants but is now a city of one million. During the 1900s it grew into an important industrial city which included large tractor factories, and was very important as a place for an extensive weapons industry during the Second World War, including tank production.

The work with Chelyabinsk-40 began in early 1946 with road building and ground preparation. The building work was led by Major General *Jakov Rappoport* who used 70 000 prisoners for the work. Rappoport was notorious for slavery at the time of previous major building projects when tens of thousands of prisoners had died of the privations. The foundations for the reactor building were laid during autumn 1946 and the building was finished at the end of 1947.

In December 1947, all material for the reactor, together with the group that would build it, was moved by rail from Moscow to Chelyabinsk. At the start of 1948, Kurchatov and Vannikov arrived there and construction of the reactor began in March. Kurchatov gave a solemn speech where he quoted Pushkin's epic poem *The Bronze Horseman* (1833) in which Peter the Great gives a talk on the shore of the Neva about how the new city of St. Petersburg would defy Russia's arrogant neighbour (Sweden). We are unfortunate enough to have plenty of arrogant neighbours, said Kurchatov, and continued (quoted from Holloway):

To spite them [a town] will be founded. In time your town and mine will have everything – kindergartens, fine shops, a theatre and, if you like, a symphony orchestra! And then in thirty years' time your children, born here, will take into their own hands everything that we have made. And our successes will pale before their successes. The scope of our work will pale before the scope of theirs. And if in that time not one uranium bomb explodes over the heads of people, you and I can be happy! And our town can then become a monument of peace. Isn't that worth living for?

The plant was ready in May 1948. The reactor was built in a pit below ground with 3-metre-thick concrete walls surrounded by water tanks as radiation shields. The reactor core had a diameter of 9.4 metres and contained 1 168 fuel channels. Kurchatov started up the reactor on the evening of 7 June and allowed it to reach an output of 10 kilowatts into the small hours of 8 June. More uranium was then added and the output was gradually increased. On 22 June, the reactor could be run with the planned output 100 megawatt. The production of plutonium began in earnest in July.

The reactor was called 'Installation A' and commonly referred to as 'Anoushka' (little Anna). The next plant, 'Installation B', was the reprocessing plant for the separation of plutonium from the irradiated fuel. Kurchatov had engaged the Radium Institute and Khlopin for the construction of the plant. In summer 1947, Kurchatov himself had succeeded in separating microgrammes of plutonium in Laboratory no. 2, and in December 1947 almost 1 milligramme had been separated in a special experimental laboratory for the purpose (called N11-9).

Installation B followed the instructions in the American Smyth Report which, as we saw in the account of the Canadian research, involved neither the best nor the fastest method. The Smyth Report wrote: 'The separation equipment is housed in a series of adjacent cells having heavy concrete walls. These cells form a continuous structure (canyon) which is about 100 feet long and is two-thirds buried in the ground'. The Russians also spoke of a 'canyon'. When the aluminium cladding had been removed, the fuel element was dissolved in nitric acid and fed, as at Hanford, into one end of this canyon and then conveyed from cell to cell to separate the radioactive fission products. Owing to the strong radiation, the whole process had to be remotely controlled. Radioactive noble gases and volatile substances such as iodine were ventilated out through a chimney.*

There were evidently initial difficulties with Installation B, but the reprocessing began to function at the start of 1949. However, metallurgical treatment was then required to produce pure plutonium metal for an atomic bomb. This took place in the third plant within Chelyabinsk-40, 'Installation V' ('V' is the third letter of the Russian alphabet). It was not finished until August 1949 and in the meantime, the plutonium from Installation B was treated in a temporary plant called Workshop no. 9. This was the 'workshop' that produced the plutonium for the first Soviet atomic bomb.

All of this work was monitored by Beria and the officers in his security service. Beria's mistrust was shared by several high politicians and military personnel. Lysenko had maintained that biologists and geneticists were conspiring against him and giving a false picture of the science. Maybe the physicists were also trying to hoodwink the policy makers. In 1949 a campaign was also introduced against 'cosmopolitans', i.e., in reality Jews. The suspicions could sometimes be expressed in comical ways.

* Radioactive noble gases released from the reprocessing plants of the nuclear weapon powers were spread globally in the atmosphere. The quantity of the long-lived nuclides (such as krypton-85) is proportional to the burnup of the fuel and thereby also to the quantity of plutonium produced. In the 1950s and 60s, military scientists all over the world attempted to use measurements of the levels of long-lived radioactive noble gases in the atmosphere to conclude which quantities of military plutonium were produced.

The head of the reprocessing plant in Chelyabinsk-40, *Anatoli Alexandrov* (1903-1994), has told how a group of generals came to Chelyabinsk-40 in 1949. Alexandrov just happened to be holding in his hand a nickel-plated half-sphere of the plutonium that would be used in the first test explosion. They asked what he was doing (quoted from Holloway):

> I explained, and then they asked a strange question: ‘Why do you think it is plutonium?’ I said that I knew the whole technical process for obtaining it and was therefore sure that it was plutonium and could not be anything else! ‘But why are you sure that some piece of iron hasn’t been substituted for it?’ I held up a piece to the alpha-counter, and it began to crackle at once. ‘Look,’ I said, ‘it’s alpha-active.’ ‘But perhaps it has just been rubbed with plutonium on the outside and that is why it crackles,’ said someone. I grew angry, took that piece and held it out to them: ‘Feel it, it’s hot!’ One of them said that it did not take long to heat a piece of iron. Then I responded that he could sit and look till morning and check whether the plutonium remained hot. But I would go to bed. This apparently convinced them, and they went away.

Kurchatov had not placed all his eggs in one basket. In parallel with the production of plutonium, work was ongoing to enrich uranium-235. Once the Smyth Report had become available, a decision had been made in 1945 to follow two paths: gas diffusion and electromagnetic separation. The construction of the two plants that were needed for these procedures started in 1946s.

Kikoin was made responsible for the gas diffusion project. The plant was called Sverdlovsk-44 and was built in the Urals north of Chelyabinsk-40 near the small town of Nevyansk approximately 50 km north of Yekaterinburg, which was called Sverdlovsk between 1924 and 1990.

The responsibility for the electromagnetic separation was given to Artsimovich. The plant was called Sverdlovsk-45 and was built further north, close to the town of Tura.

Kikoin initially made great progress and succeeded in producing a useable membrane of sintered nickel powder. A large diffusion plant – although smaller than the one at Oak Ridge – was built at Sverdlovsk-44 with impressive speed. It contained 6 000 compressors in groups of 128. However, Kikoin had difficulties getting it to function; in 1949, the enrichment was no greater than 40% instead of the in excess of 90% which was required for a bomb. With the situation as it was, some of the German scientists who had been working elsewhere were summoned, including Hertz and Thiessen, but they also failed to succeed in finding the error. Beria visited the plant and gave Kikoin an ultimatum: he must solve the problem within three months. He succeeded in doing so – the reason for the error was corrosion caused by the uranium hexafluoride. In 1951, the plant began to produce uranium that was enriched to more than 90%.

The experiments with electromagnetic separation also encountered major difficulties initially and had to give way to the gas diffusion which did eventually succeed.

With the exception of Riehl and his assistance with the gas diffusion project, the captured German scientists worked separately from the actual Soviet projects. They formed two groups led by Gustav Hertz and Manfred von Ardenne. These groups were allowed to set up research institutes in Georgia close to Sukhumi, a renowned seaside resort and spa on the Black Sea. They had no exact instructions on what was expected of them other than that they were to do research into isotope separation.

An additional research institute was set up in Sungul between Sverdlovsk and Chelyabinsk. K.G. Zimmer was one of the people there and Timofeev-Ressovsky who had been treated as a convict had also been sent there in 1947 when it was discovered that he could be useful. Although his status of convict did not change, Ressovsky became head of the research institute’s biological department. He was given his own house and his family were able to join him from Germany.

The Russian doctor *Leonid Ilyin*, who later became Soviet representative on the UN’s scientific radiation committee UNSCEAR and a member of the ICRP, visited Sungul at the end of the 1950s and was invited to Timofeev-Ressovsky’s home for a ‘general chat’. In his book on the Chernobyl accident, he tells us about this visit:

> [...] I had noticed that Timofeev-Ressovsky scarcely looked at the laboratory notes that I showed during my story. I now realised that his sight was poor. At the end of our unforgettable social evening he told me the reason why.
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> 'Until very recently, my friend, I was taking care of wagons in the mine and was ruthlessly beaten several times by guards who aimed their punches at my head. Then there's the pellagra I had in prison.' Later, Timofeev-Ressovsky could not write his articles himself but dictated them to his wife. It was surprising that the dictated and then printed out text never needed any editing.
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> At this social evening in Ressovsky's home, which continued into the small hours, we discussed a number of questions that concerned radiation biology. It should be mentioned that Elena Timofeeva-Ressovskaya was a prominent expert in radiation ecology. Her research into the dispersion of radionuclides in water systems and the ground and also into decontamination methods is now classical within the area. These fundamental studies, the first to be done in this country, gave rise to many research specialisations that are still successfully followed up today.* Her work served as one of the components of the scientific evaluation of the consequences of the Chernobyl accident.
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> The 'general chat' at Timofeev-Ressovsky's provided food for thought. I realised for the first time the enormous role of a scientist's impartial effort in the analysis of scientific data and, what is particularly important, to the objective interpretation thereof. Timofeev-Ressovsky referred repeatedly to Mendel and the latter's classical studies, and he emphasised the astounding and ingenious simplicity of the idea behind the big scientist's experiment and his perfect methodology. In his description (in his inimitable sour and sarcastic way) of Lysenko's 'work', he overturned the latter's conclusions. The main attack concerned Lysenko's major, elementary methodological mistake, his total ignorance of the laws of statistics and the method of research and the absence of adequate control.
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> It was then that Timofeev-Ressovsky made his famous statement: 'Lysenko's dominance in our country's biological science is more fateful in its consequences than a natural disaster.' The truth of his words was confirmed by the Chernobyl accident. After this meeting with Timofeev-Ressovsky, I concluded that genuine science has to rise above politics, commercial interests and private attitudes or else the scientist would unavoidably become an imitator.

The work in Chelyabinsk-40 was taking place under severe time pressure, so no particular thought had been given to the risks of radiation. The situation in the uranium mines was particularly difficult and the mine workers were usually convicts who were pushed hard. Nor did it bother Beria as to what happened to the scientists and their assistants after material for bombs became available. The convicts were never properly released; after serving their punishment they were transported to remote areas and isolated as hazardous to the State. There are therefore few accounts of their work situation.

However, in 1948, the Ministry of Health drew up protection provisions for the work within Installation A (the production reactor) and Installation B (the reprocessing plant). The radiation dose would be limited to 100 millirem (1 mSv) per six-hour day, corresponding to approximately 30 rem (300 mSv) per year, i.e., the dose limit was fifteen times that which is now recommended by the ICRP. Even more significant was the fact that there was no recommendation to lower the dose further; the belief was that there was no risk provided the radiation dose did not exceed the stated limit.

The protection provisions could not be complied with. In 1949, the *average* dose for those who worked at the reactor was 94 rem (940 mSv). At the reprocessing plant, the average annual dose was 113 rem (1.13 sievert) when it was commissioned in 1951. They were aware that these doses were not without danger but the priority was the fast production of the bomb. The situation at Chelyabinsk-40 did not improve until after 1953.

* Mrs. Ressovskaya had extraordinary possibilities of studying contaminated watercourses and areas of land in the Urals since the discharge of the radioactive substances from the atomic bomb factories was extremely extensive. The big accident of 1957 at a waste repository in one of the plants at Kyshtym also gave rise to another very substantial instance of environmental contamination.

What were even worse than the high radiation doses to the personnel were the environmental problems, since they led to equally high doses of radiation among the public. During 1948-1951, i.e., when the material for the first atomic bomb was produced, 76 million cubic metres of radioactive waste were discharged into the Techa-Iset-Tobol river system. The rivers and riverbeds became heavy with radioactive contamination, which led to significant doses of radiation to more than one hundred thousand people. The extent of the contamination was ascertained at a survey in 1951 and ten thousand people were forced to leave their homes.

When sufficient plutonium for an atomic bomb had been produced by 1949, there was also the question of knowing how to use it. Early on, Khariton had been commissioned by Kurchatov to study the construction of an atomic bomb early on but Laboratory no. 2 was not suitable for bomb experiments. Kurchatov then decided to form a special ‘Design Bureau 11’ (‘KB-11’) with Khariton as head designer. Beria appointed *General P.M. Zernov* as administrative manager, who had been the Deputy People’s Commissar for the tanks industry. In April 1946, Zernov and Khariton agreed on a suitable place for KB-11. It was a small town, Sarov, by the Tiosha River 60 kilometres south of the larger town of Arzamas and 400 kilometres east of Moscow.

Similarly to what happened regarding the other plants in the atomic bomb industry, KB-11’s plant in Sarov was named after the nearest large town, so was named Arzamas-16. It became the Soviet Union’s equivalent of Los Alamos and was sometimes called ‘Los Arzamas’ among the initiated. Laboratories and houses for the personnel were built by prisoners from a nearby work camp. Khariton gathered together a very competent group of physicists and chemists for the assignment. His former colleague Zeldovich was given the task of leading KB-11’s Department for Theoretical Physics.

One major practical problem was to get all explosive charges which surrounded the plutonium sphere that would be compressed to criticality to explode simultaneously in less than a microsecond. The problem was solved by physicist *V.S. Komelkov* in 1948, who succeeded in constructing detonators that could be triggered synchronously. A second problem was getting a neutron initiator to start the chain reaction at the right moment.

In summer 1946, Khariton and Zernov sent the government a report asking for the plans to be approved. Had problems with the gas diffusion plant not arisen, a uranium-235 bomb would have been finished first, but now plutonium was the first to become available.

In July 1948, *General N. L. Dukhov* (1904-1964) was summoned to Arzamas-16. Dukhov was an experienced mechanic who had been the head designer at the Kirov works for the production of tanks. He was given the post of Khariton’s deputy. Two half-spheres of plutonium, approximately 8 cm in diameter, were produced in Chelyabinsk-40 in June 1949. It was one of these that Alexandrov was holding in his hand when the mistrustful generals doubted that it really was plutonium; he had been asked to nickel-plate them so that they were easier to handle.

When everything was prepared, Kurchatov asked Khariton and his colleagues if they were prepared to do a test explosion with the bomb. When the answer confirmed that they were, Kurchatov asked Stalin’s permission to test the bomb. Stalin summoned the leading scientists and engineers to ask them all. Kurchatov was the first to gain entry. He has since said that every time he met Stalin, he got the impression that the latter viewed him as an irritating fly that he wanted to get rid of as soon as possible. Khariton was the next man to be called in to Stalin. Kurchatov was still there, as was Beria. This was the first and only time that Khariton met Stalin.

Stalin asked if it was possible to split up the plutonium so that there was enough for two smaller bombs - there would then be one in reserve. Khariton’s response was that it was not possible and Stalin accepted the answer. According to Holloway, Stalin asked no technical questions and nor did he get to see the plutonium charge as has sometimes been said. One does wonder how he would have reacted to looking at a sphere no bigger than a large orange. Would he have thought it nonsensical that this could destroy a whole town?

In 1949, the atmosphere in the Soviet Union became increasingly opposed to foreigners. Lysenko’s victory over the biologists in 1948 had given the impression of a big conspiracy among the established scientists against Lysenko and his ‘true biology’. The politicians began to wonder whether there were equivalents within other sciences such as physics and chemistry. Opportunists, some at the University of Moscow, came into conflict with the better-known scientists within the Academy of Sciences.

'Cosmopolitism' became a term of abuse and, as Stark and Lenard had once explained that there was a special 'Aryan physics', many now maintained that there was a special Soviet physics and that the science could not be liberated from the political representation. Nationalistic pride meant that they wanted to be free from all foreign influence, and there was an almost comical attempt to show how important discoveries were first made in Russia. Newspapers all over the world bantered about the Russian claims to have invented everything from the wheel to television.

Preparations were made over a period of six months for a major physicists' conference which would be devoted to handling the cosmopolitan physics and banning those who did not give physics a measure of the right ideology. Physicists who were engaged in the atomic bomb project warned Beria of the consequences of this for the work with the atomic bomb. The last meeting of the conference's organisation committee took place on 16 March 1949. The conference would take place on 21 March. In the meantime, Stalin intervened and banned the conference. He reputedly said the following to Beria about the physicists in the atomic bomb project: 'Leave them alone. We can always shoot them later.' The atomic bomb had saved the Russian physicists.

When the bomb was to be tested in summer 1949, the test site chosen was an area of grassland in Kazachstan by the River Irtysh next to the border between north-eastern Kazachstan and Russian Siberia, around 140 kilometres north-west of the city of Semipalatinsk. The plant was therefore called Semipalatinsk-21 and was later given the name Kurchatov. The bomb would be detonated 70 kilometres south of there. One of the participants has described the site (according to Holloway):

> Early every morning we went out in trucks to the working buildings near the test site. Along the way there were neither houses nor trees. Around was the stony, sandy steppe, covered in feather-grass and wormwood. Even birds here were fairly rare. A small flock of black starlings, and sometimes a hawk in the sky. Already in the morning the intense heat could be felt. In the middle of the day and later there lay over the roads a haze, and mirages of mysterious mountains and lakes.
>
> The road led to the test site, which was situated in a valley between two small hills.

The Russians had made fruitless attempts to obtain information on the effect of the American bombs (the first official American report, *The Effects of Atomic Weapons*, was not published until 1950; the first edition of the standard work, *The Effects of Nuclear Weapons*, came in 1957). There would now instead be an opportunity to study the effect of their own bomb. Single-storey timber houses were built and four-storey brick houses, bridges, tunnels, water towers and other structures around the tower in which the bomb would be positioned. Different types of measurement instruments were put in position, ion chambers and high-speeds cameras. Test animals were also positioned around the tower, outdoors and indoors.

Pervushin was responsible for the test site. Kurchatov was the first in command, even over the army unit that was in place; he was present from May. In August, a special State supervisory commission led by Beria arrived, accompanied by Zaveniagin to observe the test. Beria's presence reminded the scientists of their potential fate were the test to fail.

The final assembly of the bomb was monitored by Beria, Kurchatov, Zavenyagin, Khariton, and Zernov. It was the middle of the night going into 29th August. It was not actually a bomb but purely the bursting set that could be used for a bomb. After being assembled, the device was rolled out of the assembly hall and to the tower where it was placed in a lift and hoisted up to the top of the tower.

Up in the tower the detonators were positioned and connected to the ignition system. A cable ran from the tower to the control station 10 kilometres away. Kurchatov, Khariton, Pervushin, Flerov and Zavenyagin and Beria and his commission were there. Two observation posts had been set up 15 kilometres north and south of the tower.

When Kurchatov gave the order to fire at dawn on 29 August 1949, an automatic countdown process was started. Komelkov, who was at the northern observation post, has given a graphic description of what happened next:

> The night was cold and windy, and the sky was covered by clouds. Gradually the day broke. A sharp north wind was blowing. In the small room about twenty people

> were gathered, all huddled up. Gaps appeared in the low scudding clouds, and from time to time the field was lit by the sun.
>
> Signals came from the central point. A voice from the control panel was carried over the communications network: ‘Minus thirty minutes.’ That meant the instruments had been turned on. ‘Minus ten minutes.’ Everything is in order. Without prearrangement, everyone went out of the building and began to watch. The signals could be heard out here too. In front of us, through the gaps in the low-lying clouds could be seen the toy tower and assembly shop, lit up by the sun. In spite of the multi-layered cloud and the wind, there was no dust. Light rain had fallen during the night. Waves of fluttering feather-grass rolled away from us across the field. ‘Minus five’ minutes, ‘minus three’ minutes, ‘one,’ ‘thirty seconds,’ ‘ten,’ ‘two,’ ‘zero.’
>
> On top of the tower an unbearably bright light blazed up. For a moment or so it dimmed and then with new force began to grow quickly. The white fireball engulfed the tower and the shop and, expanding rapidly, changing colour, it rushed upwards. The blast wave at the base, sweeping in its path structures, stone houses, machines, rolled like a billow from the center, mixing up stones, logs of wood, pieces of metal, and dust into one chaotic mass. The fireball, rising and revolving, turned orange, red. Then dark streaks appeared. Streams of dust, fragments of brick and board were drawn in after it, as into a funnel. Overtaking the firestorm, the shock wave, hitting the upper layers of the atmosphere, passed through several levels of inversion, and there, as in a cloud chamber, the condensation of water vapour began.
>
> … A strong wind muffled the sound, and it reached us like the roar of an avalanche. Above the testing ground there grew a grey column of sand, dust, and fog with a cupola-shaped, curling top, intersected by two tiers of cloud and layers of inversion. The upper part of this étagère, reaching a height of 6 – 8 kilometers, recalled a cupola of cumulus storm-clouds. The atomic mushroom was blown away to the south, losing its outlines, and turning into the formless torn heap of clouds one might see after a gigantic fire.

There was great joy at the control station. Beria embraced Kurchatov and Khariton and kissed them on the forehead. Khariton said that he was happy because they had now eliminated the possibility that anyone would dare to use an atomic bomb against the Soviet Union. Cameramen filmed Kurchatov for posterity.

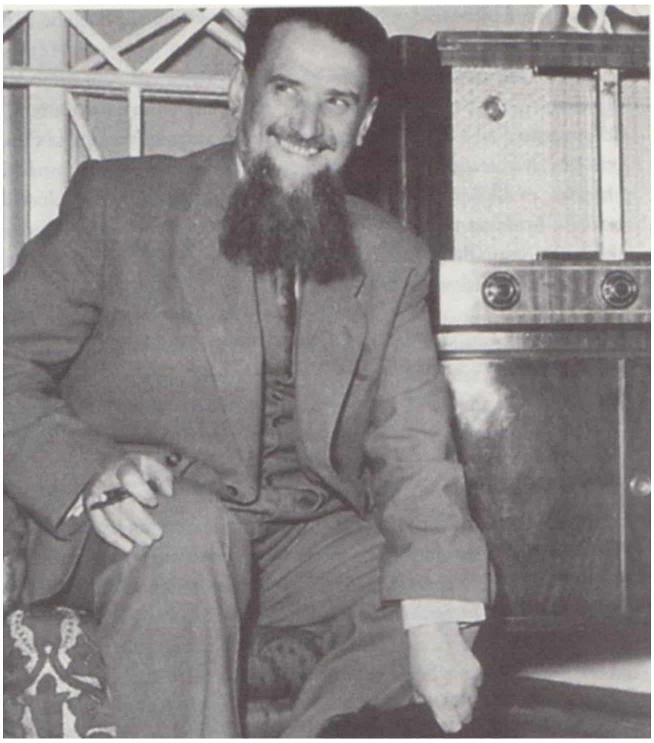
The strength of the detonation was estimated to correspond to 20 000 tonnes of TNT, i.e., it was approximately as powerful as the bomb that the Americans had tested in the Alamogordo Desert and as the bombs they then dropped over Hiroshima and Nagasaki. Aircraft traced the radioactive cloud and vehicles were sent out to map the radioactive fallout and take samples for laboratory analysis.

The contributing scientists were handsomely rewarded according to a decree issued by Beria. Some received the greatest accolade: the title of Hero of the Soviet Union, which was accompanied by a bust in their hometown and a bronze plaque bearing their name in Kreml. With the title also came a cash amount, a car and a *dacha*. Kurchatov and Khariton received the most exclusive cars, ZIS-100s while others made do with a Pobeda. Those who were remunerated received free trips for themselves and their family within the Soviet Union and free education for their children in educational establishments of their choice. Those who were honoured besides Kurchatov and Khariton included Flerov, Khlopin (who was dying), Pervushin, Vannikov, Zavenyagin, Zelodvich and Zernov. Of the German prisoners of war, Nikolaus Riehl was the only one who received the same reward.

One story which could be true claims that Beria determined the size of the rewards according to the punishment he would have prepared in case of the bomb not having detonated: those who were meant to have been executed were now the Soviet Union’s heroes; those who would have received long prison sentences now received the Order of Lenin instead.

The test explosions were followed by an increasing number of tests during the 1950s and 60s but the big task that now lay before them was to produce much more powerful thermonuclear weapons, ‘hydrogen bombs’. The person who was already engaged for this task in 1948 was nuclear physicist Andrej Sacharov. He would spend 1950-1968 in Arzamas-16 but that is a story for later on.

When Beria lost his power after Stalin’s death in 1953, he was taken to court and sentenced to death. In this connection, he was removed not just from life but also from the great Soviet encyclopaedia in which the pages about Beria were replaced by loose-leaf pages about the Bering Sea. Kurchatov remained as scientific leader of the atomic bomb programme until his death in 1960.



‘The Beard’, Igor Kurchatov

JAPAN

Japan also had an atomic bomb project. It was started in April 1940 with the director of the imperial army’s Aviation Technology Research Institute, Lieutenant General *Takeo Yasuda*, who followed the international development, giving his aide, Lieutenant Colonel *Tatsusaburo Suzuki*, the task of looking at the technical conditions to produce an atomic bomb.

In October 1940, Suzuki reported to General Yasuda that Japan, bearing in mind that Burma and Korea had been occupied, now had access to sufficient amounts of uranium to make a bomb. Yasuda approached the head of the Japanese Institute for Physical and Chemical Research for advice. The latter transferred the matter to Bohr’s and Oskar Klein’s former colleague Yoshio Nishina, who had a research laboratory (Riken) in Tokyo which was equipped with a cyclotron. Nishina then began to study neutron reactions.

In April 1941, the Army’s Air Force ordered research into the atomic bomb. Nishina then studied various possibilities for the separation of uranium-235: gas diffusion, thermal diffusion, electromagnetic separation and centrifugal methods.

In May 1941 physicist *Tokutaro Hagiwara* lectured at the Faculty of Natural Science at the University of Kyoto on ‘Super-explosive U-235’. He had read available literature and had understood that chain reactions in uranium took place in uranium-235.[*] He appealed for methods to enrich uranium-235 on a large scale but the new subject in his lecture was that he discussed the matter of thermonuclear fusion. He said (according to Rhodes): ‘If by any chance U-235 could be manufactured in a large quantity and

[*] Hagiwara was no beginner. In a paper submitted to the Japanese magazine *The Review of Physical Chemistry* in October 1939, he had reported measurement results which showed that the number of neutrons per nuclear fission in uranium was 2.6.

of proper concentration, U-235 has a great possibility of becoming useful as the initiating matter for a quantity of hydrogen. We have great expectations for this.' In making this statement, Hagiwara appears to have been the first to think of a hydrogen bomb, before either Fermi or Teller.

In spring 1942, the Japanese Navy also came into the picture by starting research into naval reactors. The following statement was made (quoted from Rhodes):

> The study of nuclear physics is a national project. Research in this field is continuing on a broad scale in the United States, which has recently obtained the services of a number of Jewish scientists, and considerable progress has been made. The objective is the creation of tremendous amounts of energy through nuclear fission. Should this research prove successful, it would provide a stupendous and dependable source of power which could be used to activate ships and other large pieces of machinery. Although it is not expected that nuclear energy will be realized in the near future, the possibility of it must not be ignored. The Imperial Navy, accordingly, hereby affirms its determination to foster and assist studies in this field.

This sounded peaceful enough but at the same time, the Naval Technological Research Institute secretly set up a committee of leading Japanese scientists whose task it was to meet every month to follow the research until an opinion could be given for or against a Japanese atomic bomb. The committee was led by Nishina who was given two clients in this connection. The committee also included Professor *Hantaro Nagaoka* (1865-1950), who had once proffered a simple atomic model.

Nishina was thus in a position between the army and the Navy since they were rivals. The Navy's secret committee assumed that the United States was working on an atomic bomb. The matter was investigated but in March 1943, the conclusion was that even though an atomic bomb were possible, neither Germany nor the United States had sufficient industrial resources to manage to produce a bomb in time to affect the outcome of the war. This conclusion led the Navy to dissolve the Committee and recommend that the scientists devote themselves to something more useful such as radar research.

However, Nishina did continue the research for the army and planned uranium enrichment using the diffusion method as well as the development of weapons. In July 1943, he reported good prospects of success. He estimated that he needed 10-50 kg of 50 per cent enriched uranium-235 for a bomb.

Nishina concentrated on thermal diffusion but he did not know that Frisch in Birmingham had found the method difficult to use (Abelson used liquid rather than gas). Nishina had difficulties producing uranium hexafluoride. In summer 1944 he had just 170 grammes and did not get the separation to succeed. It was now clear to his colleagues that he was starting to despair. There has been speculation as to whether he continued the work purely out of loyalty or whether he saw a possibility of gaining continued support for his laboratory and thereby being able to save some of his younger colleagues from military service. The Major General who was his contact person with the military was of little help. He was so ignorant that at one stage he asked Nishina why they could not just use 10 kg trinitrotoluene instead of 10 kg uranium-235.

In April 1945, Tokyo was bombed and thereby also the Riken laboratory. Nishina's separation plant was destroyed, which put a definite end to the Japanese atomic bomb project. One of the Japanese physicists has said (Kuro, 1989): 'The Japanese nuclear program was like the cherry blossoms. It was in full bloom, but it was all over in 1945.'

At the end of the war there were five cyclotrons in Japan: two in Tokyo, two in Osaka and one in Kyoto. On 5 September 1945, the American General Staff issued instructions stating that all useable military equipment that the enemy had would be destroyed provided that it was not so new or unique that it ought to be examined more closely. This was followed on 30 October by a General Staff order to commanders in the Pacific Ocean area that all equipment for nuclear physics research should be confiscated and that all people who were involved with such research should be captured. On 7 November, General *Douglas MacArthur* (1880-1964) received a contradictory order from the Secretary of War that all five Japanese cyclotrons should be destroyed. The order had been written by General Groves' colleague but, according to Groves, was the result of a misunderstanding. The cyclotrons were confiscated and destroyed.

The destruction of the Japanese cyclotrons led to great publicity and criticism in American newspapers. The War Department admitted it was a regrettable mistake and General Groves devotes a full chapter of his memoirs to examining how it all could have happened.

NORWAY

It may come as a surprise to many that our neighbouring country was ready with the practical application of nuclear energy at a very early stage. The man who took the initiative in this respect was Major *Gunnar Randers*, an exuberant astronomer and military physicist who had got the Norwegian government to invest money in a reactor project with reference to Norway's heavy water production.

Randers was one of those to show initiative regarding the formation of the Norwegian Defence Research Establishment, and when it was set up in April 1946 he became head of its physics department. On a study trip in the United States just afterwards, he made a personal decision. He writes in his book, *Lysår* [Light Years]:

> The American trip has convinced me that we can build a uranium reactor in Norway. In my own mind it will be the right thing to do. However, it is difficult to see how to tackle it. The Defence Research Establishment with its fresh new forces and decent appropriations obviously has to be the starting point. On the other hand, it's also clear that the Establishment's budget alone is insufficient. There is also the question of whether the Defence Department can agree to the project having such an open and civilian slant that we do not have a repetition of the fight between civilian and military that is ongoing in the United States.

A special research station, Kjeller, north of Lilleström 15 kilometres east of Oslo, would be the place for the reactor. However, the problem faced by Randers in 1950 when the reactor was almost completed was that he did indeed have access to heavy water but not to uranium.

Randers tried to get the British government to trade uranium for heavy water but the British did not dare, bearing in mind its dependence on the United States. Randers then turned to France, a country that was outside the British-American coalition. He was able to refer to the help with the heavy water that Norway had given France during the war. It was also heavy water from

Norway which facilitated the first French experimental reactor, ZOÉ. Randers therefore contacted Joliot following some trepidation. The Norwegian government was not enthusiastic about cooperating with a communist scientist.

Joliot thought all the trump cards were in his hand in being the only one who could help Randers. He demanded that the Norwegian reactor be seen as a joint French-Norwegian project and offered to give the Norwegians uranium, but no instruction as to how to transform it into purified metallic form.

Randers refused to accept the terms. Instead, he turned to the Netherlands, who happened to have ten tonnes of uranium oxide that a forward-thinking university professor had purchased in 1939. The French regretted having been so exacting and now suggested that the project be a cooperation between the three countries, whereby the French promised to provide all necessary technical information.

This worried the Americans, who were afraid that France would gain too dominant a position. If the Norwegians limited themselves to cooperation with the Dutch, the United States was willing to isolate metallic uranium from the uranium oxide.

The Norwegian-Dutch reactor in Kjeller was completed in 1951 and was the first reactor plant that was opened to scientists from other countries. It was called JEEP (Joint Establishment Experimental Pile) and the planned output was 250 kilowatts. This was to ensure an adequate neutron flow to be able to produce radioactive nuclides for medical use and (wrote Randers) 'for neutron research with a view to subsequent power reactors'.

The Norwegian-Dutch cooperation continued until 1960. The original hope that it would lead to vital nuclear power industries in both of the countries never needed to be fulfilled. Hydropower, natural gas and North Sea oil made nuclear power unnecessary. However, another Norwegian reactor, located in Halden and with a heat output of 25 MW, was built during the 1950s in a research project under the OECD for testing nuclear fuel, among other things.

ARGENTINA

Argentina is now a competent nuclear power nation but was the scene of a bizarre nuclear power fraud in the 1950s.

The story begins in London in 1946 when a German flight engineer by the name of *Kurt Tank* met Ronald Richter, a German physicist with a very good power of persuasion. Richter spoke enthusiastically about nuclear physics and said that one possibility that had been disregarded was the achievement of controllable fusion, i.e., the fusion of light atomic nuclei. Rather than making bombs, a source of energy could be developed which could be used to run boats and even aircraft.

Tank was infected by Richter's enthusiasm. When he was invited by the Argentinian government to work at the Aviation Engineering Institute in Córdoba in 1947, he recommended that someone contact Richter for further discussions. Richter had just been refused a visa to the United States and accepted an invitation to Argentina, where he had the opportunity to meet *Juan Perón* (1895-1974) on 24 August 1948, who had become President in the previous year.

Richter's enthusiasm also rubbed off on Perón. Without looking into Richter's background or qualifications (Richter had never published any scientific paper) and without taking advice from any experts within the area, Perón took note of Richter's idea and gave him near enough *carte blanche* to realise it.

With Perón's support, Richter searched for a suitable place for a research laboratory. Perón was interested in populating Patagonia and asked Richter to examine the conditions there. The beautiful landscape attracted Richter and he chose to locate the laboratory on the small island of Huemul in the 700 km^2 Nahuel Huapi lake near the small town of San Carlos de Bariloche at the foot of the Andes close to the border with Chile. The area around Nahuel Huapi is breathtakingly beautiful and has since also been exploited by the tourist industry. In the winter, Bariloche is reminiscent of the skiing resorts in Switzerland and Austria.

The building work began on 21 July 1949, and Richter became an Argentinian citizen in March 1950 and moved with his wife to Bariloche. In April, Perón visited the plant together with his wife '*Evita*' (1919-1952). On 31 May 1950, the Argentinian Atomic Energy Commission CNEA (*Comisión Nacional de energía Atómica*) was formed to create an administrative background for the Huemul project. Until 1952, the CNEA had just four members. Perón himself was chairman and his old friend Colonel *Enrique González* secretary; the other two were the Minister for Technical Matters *Raúl Mendé* and Ronald Richter.

The device, let us call it a reactor, constructed by Richter consisted of a cylinder that was 2 metres in diameter and 3 metres tall. Inside it were electrodes to provide an electric arc. The temperature in one of these can be very high, up to 6 000 °C, and Richter must have hoped that this would be enough to fuse light atoms.[*] The reactor also contained a spectrograph and Richter reckoned that the fusion would affect the spectral lines so that the spectrograph could be used as a thermometer.

Richter had just two colleagues in the laboratory. One was a German physicist, *Wolfgang Ehrenberg*, who worked alongside Richter for Baron Manfred von Ardenne in Berlin in 1942-1943. The other, *Heinz Jaffke,* was also an old friend.

On 16 February 1951, Jaffke performed an experiment in which he exposed hydrogen and lithium to the arc of light. When the film from the spectrograph was developed, the spectral lines seemed to be a little displaced. Jaffke believed that this was because the film had moved in its container but Richter was convinced that he now had evidence that the fusion had succeeded. Jaffke suggested that the experiment be repeated but Richter did not want to listen to him.

Instead, Richter contacted Colonel González and told him he had had success with fusion. Gonzalez told Perón who, on 24 March, proudly made a public statement that a controlled fusion reaction had been achieved in Argentina. The statement aroused international attention and physicists the world over wondered what sort of secret apparatus it was that President Perón had at his disposal. Many speculated

[*] Tens of millions of °C are required for fusion.

about the possibility that Perón had procured an atomic bomb. On 28 March, Richter received a distinguished medal from Perón and was awarded an honorary doctorate at the University of Buenos Aires.

In October 1951, Richter announced that he had performed successful new experiments and promised that both domestic and foreign scientists would be able to participate in a demonstration. However, Colonel González grew suspicious and set up a Commission to examine what really was happening on the island of Huemul. However, Richter succeeded in convincing Perón that an investigation was not necessary. Perón then replaced González with an Air Force officer by the name of *Pedro Iraolagoitía* as Secretary General for the CNEA.

On 20 April 1952, Iraolagoitía visited Huemul and soon found that he shared González' view that an Investigation Commission was needed. Perón then set up one. The chairman thereof was *Father Bussolini* who was scientific adviser to Perón. Bussolini was not a physicist but had studied astronomy. The secretary of the new Commission was Captain *Emanuel Beninson* (1886-1979), whose son *Dan* (1931-2003) would go on to become one of the most colourful and influential players within international radiation protection. The Commission now also included, for the first time, three nuclear physicists.

The Commission visited Richter's plant on the island of Huemul on 5 September 1952 and inspected the 'reactor'. Richter's explanations of the theory behind his experiments failed to convince the nuclear physicists, and they were even less impressed by the account of the experiments that had been performed. After the Commission had issued a negative statement, the army took over the plant on 22 November 1952 under order from Iraolagoitía. In February 1953, Richter left Bariloche in secret to move to Buenos Aires where he was taken to a villa 60 kilometres from the city. The following year he returned the medal he had received from Perón after having been disgraced in Parliament.

Richter's adventure did not only lead to negative consequences. The Argentinian Atomic Energy Commission, which was set up on false premises, survived and was given important tasks in a real atomic energy programme. In Bariloche, research and educational institutes were set up in nuclear physics. Bariloche would come to be completely dominated by the CNEA's activities and is now an internationally respected research centre.

13. POTSDAM AND THE ATOMIC BOMB DECISION

IN THE SUMMER of 1943, General Douglas MacArthur, the commander of all of the Allied Armed Forces in the Pacific Ocean area, began his offensive against Japan. He forced the Japanese from island after island with bloody conflicts. In the battle of the Mariana Islands in the summer of 1944, the Japanese troops fought down to the last man, which also involved big losses on the part of the Americans. When the Japanese had also been driven away from the Philippines, two important islands remained, Okinawa and Iwo Jima, between the Allied powers and Japan itself. The capture of these islands in spring 1945 was particularly bloody in nature, with the landing on Okinawa in June claiming 13 000 American lives. And still the war was not over: a couple of million soldiers were waiting on the Japanese home islands to defend their country.

In order to break the Japanese fighting spirit, Japanese cities were subjected to extensive bomb attacks with the Allies being completely dominant in the air. On 9 March 1945, Tokyo suffered a terror bombing in an attack that perhaps claimed more than 100 000 lives and annihilated the physical surroundings. But, in the same way that the English defence spirit suffered little reduction as a consequence of Hitler's bomb attack on London, these attacks had no crucial influence on the fighting spirit of the Japanese. 'We will fight until we eat stones' was an old phrase meant there was a great likelihood that an allied invasion of Japan could claim hundreds of thousands of lives.

In discussions on which strategy would be preferable, a direct invasion or the isolation and exhaustion of Japan, both MacArthur and Admiral *Chester Nimitz* (1885-1966), Supreme Commander of the Armed Forces for the USA's Pacific Ocean Fleet, recommended the invasion. So, in April 1945 the air defence control recommended that an invasion ought to take place as soon as possible. Such an invasion was estimated to require 36 divisions with a total of more than one and a half million men. The order for the invasion of Japan's important southern island, Kyushu, was issued on 25 May 1945 to the three responsible military bosses: General MacArthur, Admiral Nimitz and General *Henry Arnold* (1886-1950), the head of the American Air Force. The time of the invasion was set as 1 November 1945.

However, In Japan, the fighting spirit was not as great in all areas as the Allies had feared. On 7 April 1945, Admiral Baron *Kantaro Suzuki* (1868-1948) formed a new government and invited diplomat *Shigenori Tojo* (1883-1950) to be Foreign Secretary. Tojo had already previously spent a year as Foreign Secretary in General *Hideki Tojo's* (1884-1948) aggressive military government during the very period when Japan attacked Pearl Harbour. He subsequently disagreed with Tojo and left the government as early as September 1942.

Suzuki was 77 years old when he succeeded as head of the government, and it is said that he accepted the assignment purely out of duty. Tojo says in his memoirs[*] that he asked Suzuki to express his outlook on the future and the ongoing war. Suzuki's response was that he believed that the war could continue for two or three years. Tojo retorted that perseverance must be dependent on continued production and access to war materials and that he could not believe that Japan could hold out for more than a year. He therefore did not think he could accept the invitation to become Foreign Secretary when he and the Prime Minister had such different views of the future.

The following day, Tojo asked the advice of an older Admiral whose judgement he trusted. The response was that Suzuki could no doubt be persuaded to change his mind and that Tojo was needed in the government to get him to do so. A cabinet secretary also pleaded with him and said he must realise

[*] After the war, both Tojo and Suzuki were served with long prison sentences, Tojo mainly due to his participation in Tojo's government during the attack on Pearl Harbour. Owing to his poor health, Tojo spent a large part of his prison sentence in the American military hospital in Tokyo. Tojo's memoirs obviously constitute an apology but are still a good source of knowledge of what happened within the Japanese government before the end of the war. More critical literature was published at a later date, however.

that the Prime Minister could not exactly *say* that he did not believe Japan could hold out, but that Tojo ought to read between the lines as regards what Suzuki was actually expecting. Then when another well-known civil servant in the Government Secretariat said he did not believe that Suzuki's views were signed and sealed and that it was Tojo's duty to enter the government to change them, Tojo accepted the assignment.

The civil servant gave Tojo a word of solace to go on: 'Anyway, I believe that you needn't worry too much, because it seems that the Emperor is considering the ending of the war.' The charismatic Emperor's actual attitude is still disputed, however.

The appearance of Suzuki's government was seen by the now resigned German Nazis as proof of Japan's downfall. On 9 April 1945, Goebbels wrote in his diary three weeks before his suicide:

> Suzuki's new Japanese government is composed of fairly unknown people. Suzuki himself is temporarily taking over the Foreign Ministry but it is thought that in a few days he will entrust foreign policy to Togo, the former Japanese Ambassador in Berlin. Togo is among the more pliable characters and there is nothing to be expected from him as far as we are concerned. […] As far as Japanese war policy is concerned, therefore, one thing may be taken as certain: it would be the bloodiest irony in the history of this war if in the end Japan too was lost to us and we were left quite alone in the field.

From when he started as Prime Minister in April 1945, Suzuki (according to Tojo) led a stubborn fight to put an end to the war. It was not easy for several reasons. One reason that we in the west may not be able to understand all that readily was the solemn requirements. His ally Tojo was a diplomat and was not expected to say exactly what he thought. But the overriding reason was the discord within the government and primarily within its inner circle, a presiding committee that was sometimes called the 'inner cabinet' but was usually referred to as the Supreme War Council. This presiding committee, which consisted of six people, was the group that controlled the country's war policy in reality.

The six members of the Supreme War Council apart from Suzuki and Tojo were the Secretary of the Navy *Mitsumasa Yonai* (1880-1948), who also shared Suzuki's opinion, and 'the hawks' Secretary of War *Korechika Anami* (1887-1945), the Chief of Army Staff General *Yoshijirō Umezu* (1882-1949) and the Chief of Naval Staff Admiral *Teijiro Toyoda* (born in 1885). There was nothing to choose between them when it came to willingness for war. The fact that the Emperor wanted to put an end to the war was still of no crucial significance since, formally speaking, he had no political power and was unable to give the Council of War any directions. It would later be shown that the Emperor had already made the decision in any case.

This was the position in East Asia on 12 April 1945 when President Roosevelt suddenly suffered a brain haemorrhage in Warm Springs and passed away. The modest man who was his Deputy President and was suddenly landed in the political limelight unprepared would end up being counted as one of the USA's most competent Presidents. However, when Harry S. Truman took up his presidency, he still knew nothing about the new weapon, which a few people who were in the know saw as a means of avoiding the big losses to which the invasion of Japan, which had already been decided on, was expected to lead.

In Berlin, Goebbels rang from his work room to Hitler whom he would not meet until ten days later in the underground bunker. He had ordered the very best champagne to celebrate the day. 'Mein Führer', he said, 'I congratulate you! Roosevelt is dead. It was written in the stars that an important event would come in the second half of April. Today is Friday 13th April!' However, Goebbels' interpretation of the stars was wrong. The second half of April did indeed involve a turning point, but not the one he was thinking of. On 1 May, both Hitler and Goebbels were dead.

On the date of Roosevelt's death, Harry Truman had, as was his habit, written a letter to his mother and sister. 'Dear Mama and Mary ...' he began, and continued to write: 'Turn on your radio tomorrow night at 9:30 your time and you'll hear Harry make a Jefferson Day address to the nation. I think I'll be on all the networks, so it ought not to be hard to get me. It will be followed by the President, whom I'll introduce'.

And it did not happen. Truman had written his letter while he was chairman of the Senate during a particularly uninspiring address by a Senator from Wisconsin. The Senate's meeting finished just before

17.00. Truman went to get a drink from the speaker of the House of Representatives *Sam Rayburn* (1882-1961). When Rayburn passed him a glass of bourbon and water, he mentioned that someone from the White House had been looking for Truman. Truman rang the White House and was asked to come over immediately. When he came to the White House just before 18.30, he was shown up to *Eleanor Roosevelt's* (1884-1962) work room one floor up. 'Harry,' said Mrs Roosevelt, 'the President is dead.'

At 19.08 on 12 April 1945, Truman became President of the United States. Roosevelt's ministers stayed behind for a while to hear that Truman wanted to continue Roosevelt's policy at home and abroad and that he wanted the ministers to remain in their posts. After the short meeting, Secretary of War Stimson stayed behind and explained that he had an important message. In his memoirs, Truman recollects that Stimson

> […] wanted me to know about an immense project that was under way - a project looking to the development of a new explosive of almost unbelievable destructive power. That was all he felt free to say at the time, and his statement left me puzzled. It was the first bit of information that had come to me about the atomic bomb, but he gave me no details.

Truman received the next information about the bomb the following day, 13th April. 'Jimmy Byrnes came to see me', wrote Truman in his memoirs, 'and even he told me few details, though with great solemnity he said that we were perfecting an explosive great enough to destroy the whole world.'

Jimmy Byrnes was *James Francis Byrnes* (1879-1972), private citizen for a few weeks now but prior to that the powerful head of the United States' war mobilisation office since 1943 with crucial authorisation to control the country's economy during the war years and thus not unaware of the Manhattan Project.

In 1944, Byrnes had been a candidate for the post of Vice President, encouraged by Roosevelt. It was then obvious that this would be Roosevelt's last term of office as President and it was thereby probable that his Vice President would be Presidential Candidate for the Democrats in 1948. However, Roosevelt had changed his mind at the last minute and preferred Harry Truman from Missouri to the Conservative Byrnes from the south. This was something that Byrnes had difficulty digesting, and when he looked up Truman on 13 April, he could not help but feel that he was the one who ought to be in the White House.

However, Truman, who was not unaware of Byrne's feelings, needed his help and tried to offer him as high a position as he could for his pride to allow him to accept. Truman therefore asked Byrnes to be his Foreign Secretary, the most important post in the American Cabinet. Roosevelt's Foreign Secretary for the past few years had been *Edward Stettinius* (1900-1949). Stettinius was heavily involved with the preparations for the creation of the United Nations at the conference for this purpose which would be held in San Francisco from 25 April to 26 June 1945. This was an important assignment and Stettinius stepped down as Foreign Secretary in June to become the United States' representative on the Security Council and American delegate at the UN's general meeting. That was when Byrnes became Foreign Secretary.

Harry Truman did not receive a detailed description of the atomic bomb and its anticipated effects until a few days after Byrnes' visit when Vannevar Bush came to the White House to give a detailed report. Admiral *William Leahy* (1875-1959), the President's Chief of Staff, also listened to Bush but openly declared that the atomic bomb was a folly that would never function.

On 25 April, Secretary of War Stimson and General Groves had a long talk with Truman about the bomb and submitted a 24-page memorandum. Stimson predicted that the atomic bomb would enable the Allies to completely dictate their own terms to the Japanese. The bomb will be available within four months, thought Stimson, a bomb whereby one alone could destroy a whole city. It was not simply conjecture on the part of Stimson – Groves' planning was aimed at having a bomb ready for use on 1 August, and everything had gone to plan up until now.

Stimson expressed concern for the post-war period where the new destructive weapon could possibly be spread to other nations, primarily the Soviet Union. It would be necessary to have an agreements system. This was something that Vannevar Bush and Conant had been trying to convince Stimson of for six months, and their efforts now appeared to be bringing results. Prior to this, Stimson had wavered between different views, including the reasonableness of Niels Bohr's proposal that the Russians should be informed of the atomic bomb early on to safeguard future peace through 'an open world'.

One person who strongly mistrusted the Russians was the United States' ambassador to Moscow *Averell Harriman* (1891-1987). He had rushed to Washington DC to ensure that the new President understood to the same degree as Roosevelt that Stalin was capable of breaking all promises and agreements. A barbaric invasion of Europe was to be expected, Harriman had said, whereby the Soviet Union would install its system of secret police and state control in its neighbouring countries. Truman's response was that he 'intended to be firm in his dealings with the Soviet government'.

At the time of the meeting with Groves and Stimson, Truman was overburdened with emergency tasks and had difficulty finding time to read through their 24-page report on the atomic bomb, but both of the advisers insisted. Truman thought the details were irrelevant; Byrnes had already told him that the new weapon could obliterate whole cities. In the end, Stimson put forward a proposal that had originally come from Bush and Conant, namely that the President ought to set up a special committee to advise him on the 'atomic policy'. Truman accepted the proposal. The committee that was set up a few days later would be called the Interim Committee.

Before this committee got around to meeting, Einstein's second letter to the President was found among Roosevelt's papers that he left behind. Truman handed the letter to Byrnes with a recommendation that the latter ought to meet Szilard. Szilard, who would rather have met the President, travelled to Byrnes' home in Spartanburg, South Carolina, together with colleagues *Walter Bartky* (1901-1958) and Harold Urey. Byrnes agreed with Bartky and Urey but was irritated by Szilard, whom he found was aggressive and had pretensions of sharing the responsibility for political decisions with the government. Szilard thought that Byrnes did not understand the significance of atomic energy.

In addition to the Interim Committee that Stimson created with the President's approval, at the end of April another important group was formed, set up by General Groves: The Target Committee. To his surprise, Groves had been given the responsibility for the operative planning of the bombing. He had assumed that this would be taken care of by the General Staff's OPD (Operations Planning Division) but General Marshall had told him: 'I don't like to involve too many people in this. Is there any reason why you couldn't deal with this yourself?' Groves' response was: 'No Sir, I will.' Groves said later: 'This constituted the only directive that I ever received [about the bombing] or needed.'

The chairman of the Target Committee was Brigadier General of the Air Force Thomas Farrell, whom Groves had appointed as his deputy and representative of the Manhattan District in the Pacific Ocean area. From the military side, the Committee included two Air Force Officers and from the scientific side five scientists of whom John von Neumann and Briton William Penney were the best known. Groves set great store by the advice that was given by von Neumann - he was a mathematical genius, creator of the game theory for decision-making and the first to develop the theoretical principles for the function of current computers, the von Neumann architecture (the structure of a computer processor and work memory and the way in which these communicate with each other).

The Target Committee met at the Pentagon, the military headquarters in Washington DC, on 2 May. The meeting was opened by Groves, who emphasised the importance of secrecy and said that the first thing that was needed was the name of four bomb targets. General *Lauris Norstad* (1907-1988), Chief of Staff at the Army's Strategic Air Force, assured him that the Army's Air Force* would assist with data, analyses and maps if necessary. Groves and Norstad then left the Committee and let Farrell continue with the proceedings.

A summoned meteorologist said that the best weather that could be expected in Japan later in the year would be in August. Farrell emphasised that they were depending on the weather; it was necessary to drop the bombs visually to be sure of hitting the right target and they also did not want cloud which

* The name "Army's Air Force" may make you wonder. However, there was no independent Air Force in the USA until July 1947. Before June 1941, the Air Force was called the "US Army Air Corps" and between 1941 and 1947 the "US Army Air Forces" (USAAF) and belonged to the Army. In July 1947 the Air Force was separated from the Army and became an independent force with the name of "United States Air Force" (USAF).

would make it difficult to immediately document the damage. Groves recalled the conditions he had given Farrell:

> I had set as the governing factor that the targets chosen should be places the bombing of which would most adversely affect the will of the Japanese people to continue the war. Beyond that, they should be military in nature, consisting either of important headquarters or troop concentrations, or centers of production of military equipment and supplies. To enable us to assess accurately the effects of the bomb, the targets should not have been previously damaged by air raids. It was also desirable that the first target be of such size that the damage would be confined within it, so that we could more definitely determine the power of the bomb.
>
> There were now not many cities left in Japan that had not been bombed. According to the Target Committee's notes, Tokyo, for example, was completely bombed out with fire bombs and was nothing more than rubble and ash except for the Emperor's Palace, which had been spared. However, after a number of meetings, the Committee was ready to propose four bomb targets:
>
> (1) Kokura, with one of the largest weapon factories in Japan.
> (2) Hiroshima, with an important military harbour, a number of industries and a local Army headquarters.
> (3) Niigata, also with an important harbour and major industries.
> (4) Kyoto, Japan's former capital city, with a million inhabitants and where many industries moved when other cities were destroyed during air attacks.

When General Groves received the report with these proposals, he intended to show it to General Marshall for approval. Prior to this, he had another matter that he wanted to discuss with Secretary of War Stimson. Stimson then asked him whether the selection of bomb targets was ready. Groves' response was that he was on the way to meet General Marshall and show the latter a proposal. Stimson was not satisfied with this and demanded to see the report. Groves objected that it was a military matter and that he ought first to show Marshall the proposal. Stimson's response was that this was a decision that he must make, not Marshall. Then he asked again to see the report. Groves said that it would take time to get it over from his office. Stimson's response was that he had all morning available and asked Groves to telephone for the report. He then asked Groves which the four proposed targets were.

When Stimson heard that Kyoto was one of these targets, he said that this was something he could never approve. You also had to remember the United States' unique position after the war, he thought. Anything that could then damage the nation's reputation must be avoided. Kyoto was the old capital city and was of great religious importance to the Japanese. Stimson had visited the city himself and had been impressed by its old culture.

As far as Groves was concerned, Kyoto was the only bomb target that was large enough to allow an assessment of the power of the bomb. It is true to say that it did not have many large factories, but every Japanese city with a population of a million had to be expected to be involved in work that was important to the war. Groves maintained that all small industries and workshops worked to manufacture products that were important to the war.

The conflict between Groves and Stimson lasted a couple of months before Groves had to give way after his futile attempt to gain the support of General Marshall, and finally encountered a decision favouring Stimson from President Truman. Kyoto was saved. Groves went on to write the following about this conflict:

> Nothing is more illustrative of the relationship between Secretary Stimson and me than this episode. Never once did he express the slightest displeasure or annoyance over my repeated recommendations that Kyoto be returned to the list of targets. Nor did I ever feel that he wanted me to remain silent, once I had learned his views, on a matter of such great importance. I believe the affair was also typical of his attitude toward other senior officers.
>
> Events have certainly borne out the wisdom of Mr. Stimson's decision. I think, however, he did not foresee that much of the criticism he so scrupulously sought to

> avoid would come from American citizens; certainly he never mentioned this possibility to me. After the sudden ending of the war I was very glad that I had been overruled and that, through Mr. Stimson's wisdom, the number of Japanese casualties had been greatly reduced.

During the Target Committee's work, Groves had informed the Committee that he and General Arnold had agreed that the control over the use of the bombs would be in Washington DC. Groves had been concerned by the fact that the Air Force representatives in the Committee appeared not to understand what the bomb involved. They simply saw it as a new weapon that could be handled like all other new weapons, i.e., handed over to local commanders to be used at their discretion. The same applied to the proposed bomb targets. Groves and Arnold worked on the basis of the fact that the use of the bomb was such a serious matter that it must be controlled directly from Washington DC with the approval of Stimson and Marshall and maybe also the President himself.

The fact that Washington DC would control the use of the bombs was not a new idea – it was part of the long-term planning. Already in spring 1944, Groves and General Arnold had discussed the conditions for the bombing at a time when it was not obvious where the atomic bombs would be used. The largest American bomber in the Second World War was Boeing's Superfortress B-29, built especially for the war in the Pacific Ocean. It was driven by four Wright R-3350-23 18-cylinder twin-row radial engines and could carry 10 tonnes of bombs for a distance of 1600 km at a speed of approximately 500 km/h. It was 30 metres long and had a wingspan of 42 metres.

Groves assumed that a B-29 could be adapted to the atomic bombs that were prepared but there was always a risk of it failing. If this was the case, they would be forced to use a British Lancaster plane which Churchill would no doubt be willing to provide. But General Arnold did not want hear talk of that possibility - the American bomb would be dropped from an American plane.

In August 1944, the concrete plans had been made and Groves needed access to aircraft and suitable crew; it was predicted that there would be a need for training and experiments for a long time. The Air Force was unsympathetic at first and unwilling to release aircraft and personnel. There was a shortage of aircraft and also little time to train the crew. The Air Force's contact person, Colonel *Roscoe Wilson*, told Groves that he was obliged to ask General Arnold again if any plane could be provided, and if even Arnold did agree, said Wilson, Groves could still not count on gaining access to a plane until right before the bombing.

Groves approached Arnold who, unlike his subordinates, fully understood the importance of the atomic bomb project. He said that bearing in mind the enormous efforts that lay behind the Manhattan Project, he did not intend to allow the Air Force to cause any problem. He rang Wilson and ordered him to ensure that Groves got what he needed. It was then clear to his staff that requirements from the MED (Manhattan Engineer District) would be met with no argument.

Arnold was equally accommodating in spring 1945 when it was realised that the B-29s that had been provided were inadequate and ought to be replaced with new ones. Colonel Wilson explained once again that this was impossible due to the enormous need for B-29s in the Pacific Ocean war. Arnold, on the other hand, asked Groves how many new planes he needed. 'As a minimum I need one to carry the bomb,' answered Groves. Arnold then responded that he would give Groves fourteen new planes and ensure that another fourteen were kept in reserve. No-one would ever be able to say that the Air Force had not done its utmost to support the Manhattan Project, said Arnold.

Groves and Arnold had agreed early on that a special Air Force unit ought to be set up for the purpose. Already in September 1944, a division had separated from the 504th Bombardment Group to form the core of the new unit. The latter was formally established on 17 December 1944 under the name of the 509th Composite Group. It included a heavy bomb division, a troop transport division and maintenance units which made it fully independent. Initially it was located in Wendower in Utah before starting to move to the Pacific Ocean area in April 1945.

The 509th Composite Group consisted of 225 officers and 1 542 recruits. Colonel *Paul Tibbets*, especially selected by General Arnold, became their boss. Tibbets had previously been in the 97th Bombardment Group in North Africa and Europe. According to Groves, he was an excellent pilot and was just as familiar with the B-29 as anyone else within the Air Force.

In Tokyo, Foreign Secretary Tojo had trouble interpreting the Soviet Union's attitude to Japan following the surrender of Germany. He could now expect the Allies to concentrate their military endeavours on the fall of Japan. According to Tojo, once Okinawa had fallen, Japan's situation would be hopeless. He must now also count on the Soviet Union starting to move its troops eastwards. At the start of April, the Russians had announced that the Soviet Union would not be renewing the Russian-Japanese neutrality pact, justifying this by saying that Japan had helped Germany, the Soviet Union's enemy, and fought the United States and the United Kingdom, its friends.

Tojo instructed Japan's ambassador in Moscow, *Naotake Sato*, to seek an assurance from the Soviet Union regarding its peaceful intentions. Foreign Secretary Molotov had given the ambiguous response that the Soviet Union's 'attitude in connection with maintenance of neutrality had not altered'. However, the Japanese General Staff were worried by the Red Army's troop concentrations in Siberia and appealed to Tojo to do what he could to keep the Soviet Union out of the war.

8 May 1945 was V-day and General Eisenhower addressed the American public with complacency over the radio, but he emphasised that the victory in Europe meant that only some of the work was done. The war in the Pacific Ocean was still ongoing.

On 9 May, the Interim Committee met about the nuclear energy policy for the first time at Stimson's office. Stimson was its chairman and *George L. Harrison* (1887-1958) was deputy chairman. Harrison was the director of New York Life Insurance Company. James Byrne was the personal representative of President Truman. Other members were *Ralph Bard* (1884-1975), Secretary of State for the Department of the Navy *William Clayton* (1880-1966), the Deputy Foreign Secretary Vannevar Bush, Karl Compton and James Conant.

The Interim Committee found it needed an advisory scientific committee and so established one, consisting of Arthur Compton, Enrico Fermi, Ernest Lawrence and Robert Oppenheimer. The leading scientist, Albert Einstein, who had started the whole atomic bomb project with his letter to Roosevelt, was excluded. The main reason for this was security. Vannevar Bush had explained this with the following words:

> I am not at all sure that if I place Einstein in entire contact with his subject he would not discuss it in a way that it should not be discussed. […] I wish very much that I could place the whole thing before him […] but this is utterly impossible in view of the attitude of people here in Washington DC who have studied into his whole history.

Einstein was therefore excluded as a consequence of his outspokenness and his previous political standpoints.

In Japan, the meetings of the Supreme War Council were becoming increasingly difficult. Members could not speak freely because others were also present, including secretaries. This meant that they were not only forced to choose their words carefully for security reasons, but primarily because the meetings had become very formal, particularly if the Emperor was present as an observer. Therefore, at the suggestion of Foreign Secretary Tojo, as of 10 May it was arranged that the informal meetings would be for members only and without the Emperor. In mid-May, it was decided that what was said in the 'members' group' would be kept secret since the rumour of what was being debated could damage the morality of the military power. They managed to maintain this strict secrecy until the middle of July when the negotiations with the Soviet Union made it impossible to prevent information leaks.

On 11-14 May, the main point of discussion for the members' group was the matter of Russia. Chief of the Navy Yonai wanted Japan to negotiate the purchase of oil and weapons. Tojo thought it was too late to expect any help from the Soviet Union, which was now probably bound by agreements in Teheran and Yalta. The only thing they could hope for, thought Tojo, was to keep the Soviet Union out of the war. Secretary of War Anami said that it ought to be in the interests of the Soviet Union for Japan not to be weakened too far. After the war, he conjectured, the Soviet Union and the United States would come into conflict with one another. The Russians might therefore be willing to mediate between Japan and the Allies.

Prime Minister Suzuki believed that it was in Stalin's nature to act fairly and that they ought to appeal to the Soviet Union for help with negotiating peace, but Foreign Secretary Tojo was doubtful. He warned against acting as though others thought in the same way as the Japanese, but agreed that the suggested way out had to be tried, even if his fear was that there would be a high price to pay. You could not be optimistic about the Russians, said Tojo - they always acted realistically and ruthlessly.

On 18 May 1945, the first units of the special 509th Bomb Wing came to Tinian, a small island in the mountainous, volcanic group of the Mariana Islands approximately 2500 km south of Japan and 2000 km east of the Philippines. The group consists of 17 islands which form a ‘new moon’ in a north-south direction. The largest island is Guam, which is also the southernmost.

The Mariana Islands were annexed by Spain in the 1500s and were colonised in the 1600s. The group of islands is named after Filip IV’s second consort, Maria Anna of Austria. After the Spanish-American war, Guam became American and the other islands were sold to Germany. They were conquered in 1914 by Japan but were administered by the League of Nations during the interwar period. The Japanese occupied Guam in 1941 but were driven from the Mariana Islands by the Americans in 1944.

Tinian had been chosen as the base for the 509th Composite Group because of three characteristics: it was closer to Japan than the better-equipped Guam, it was more isolated which made it easier to monitor the secrecy of the operations and, primarily, it had the largest military airfield in the Pacific Ocean area. The island is approximately the same size and shape as Manhattan, which made the American soldiers call roads ‘Broadway’, ‘42nd Street’, etc. Despite its rocky nature, people used to say that Tinian had a flat surface like an ‘anchored aircraft carrier’. The 509th Composite Group was placed in an area near the airfield and the final preparation for the use of the atomic bombs was started, an operation for which the War Department had approved the code name ‘Centerboard’ on 5 April.

The 509th Composite Group with Colonel Tibbets as head was formally placed under the operative leadership of Major General *Curtis LeMay* (1906-1990), the head of the 21st Air Force. It was LeMay’s bomber that then hit Japan with B-29s on 1 June 1944, first from China but then from the Mariana Islands and Iwo Jima. Over the night between the 9th and 10th March 1945, LeMay had dropped 2 000 tonnes of bombs over Tokyo, which killed approximately 100 000 people and injured 40 000.

General Curtis LeMay was a very controversial person. Rhodes describes him as ‘a wild man, hard-driving and tough, a bomber pilot, a big-game hunter, a chewer of cigars, dark, fleshy, smart’. During the Vietnam War, LeMay talked about ‘bombing North Vietnam back into the Stone Age’. He was later accused of trying to provoke a third world war against the Soviet Union. He also appears to have been the role model for the rabid bomber in Stanley Kubrick’s film *Dr. Strangelove*.

While the 509th Composite Group prepared itself, General Groves had taken steps to ensure that the Manhattan Project would be documented for posterity. For that purpose, he had engaged two key people, Professor Henry Smyth from Princeton and science journalist William L. Laurence from the *New York Times*.

In April 1944, Henry Smyth had already been given the task of writing a report on what had occurred within the Manhattan Engineer District, the scientific and administrative problems, etc. General Groves predicted that there would be a deluge of enquiries once the bombs had been used and the Manhattan Project had become public knowledge. The technicians and scientists involved would then need to have a guideline regarding what they could say without breaking any promise to keep quiet. The intention was for them to be able to tell everything that was in Professor Smyth’s report but not another word.

Laurence had been recruited in spring 1945 when Groves had reached an agreement with his chief editor at the *New York Times*, according to which the newspaper would continue to employ him and pay his salary but he would be secretly loaned to the Manhattan Project. The intention, of which the chief editor was not informed, was that Laurence would help to write the press announcements, and primarily the statements that would be made from the White House after the bombs had been used. Laurence was not unknown to Groves; his article in the Saturday Evening Post in 1940 on the possibilities of atomic energy had been so well-informed that it aroused concern from the security point of view.

In order to be able to carry out the assignment, Laurence needed to familiarise himself with what was going on. He was therefore sent to Oak Ridge, Hanford and Los Alamos on a study visit and gained access to the areas that were otherwise completely out of bounds to visits from outsiders. His visit to Los Alamos caused a stir when some scientists recognised him as a science journalist and thought that a serious security breach had occurred. Laurence later participated in the test explosion in Alamogordo

Desert as an observer and his participation as an observer was prepared in an aircraft for when the first atomic bomb would be dropped.

On 31 May, the Interim Committee was to meet again to discuss the atomic energy policy. Before this, the frustrated Szilard tried to convince Oppenheimer, who was to participate in the meeting, to argue against the bombing of Japanese cities, but Oppenheimer disagreed. He did not think that the atomic bomb was of any military value - its value was political and it was to scare the Russians. According to Szilard, Oppenheimer said:

> ‘The atomic bomb is shit.’ ‘What do you mean by that?’ I asked.
> He said: ‘Well, this is a weapon which has no military significance. It will make a big bang - a very big bang - but it is not a weapon that is useful in war.’

The Interim Committee met its scientific sub-committee on 31 May. Oppenheimer was troubled. He had not believed that anyone in Washington DC would give any thought to its future following Roosevelt’s death. He was positively surprised by the Secretary of War’s welcome speech. Stimson said that the bomb was not just a new weapon but a revolutionary discovery of man’s relationship with the universe, an historic milestone in the same class as gravitation and Copernicus’ picture of the world. ‘It may *destroy* or *perfect* International Civilisation; it may be *Frankenstein*[*] *or* means for World Peace’ is what he had written in his notes to support what he would say.

Arthur Compton gave a technical overview of the status of nuclear technology. He estimated that it would take approximately six years for a competing nation to catch up with the United States’ lead. Upon enquiry from Conant as to how long it would take to develop a thermonuclear weapon (a ‘hydrogen bomb’), Oppenheimer’s response was that he thought it would take at least three years. Oppenheimer also said that the usual atomic bomb could be improved to a blast capacity corresponding to 100 000 tonnes of TNT (‘trinitrotoluene’) compared with the bombs corresponding to a maximum of 20 000 tonnes of TNT that would be dropped on Japan. But hydrogen bombs could be made with a blast capacity of up to 100 million tonnes of TNT, an incomprehensible amount of energy. Oppenheimer believed the bombs that were prepared for use in Japan could kill 20 000 people in a situation where a shelter was used.

Ernest Lawrence from the University of California did not appear to share Oppenheimer’s view of the atomic bomb’s military insignificance. He recommended that the United States build up a large store of atomic bombs and he was supported by the brothers Arthur and Karl Compton. The Committee then discussed the international situation after the war. Oppenheimer had been influenced by Niels Bohr’s view that it was important for the world’s heads of state to pull together and find a solution to the problem of living under the threat of atomic bombs. As Alfred Nobel once did, Bohr hoped that the supreme capacity of the new weapon would be sufficient to incite fear to deter the powers that be from using it. Oppenheimer was clear about this but did not succeed in getting the message through. Instead, he recommended that the United States share its knowledge as far as possible, thereby strengthening its moral position.

He was supported by General Marshall, who went as far as to suggest that the United States ought to invite a couple of prominent Russian scientists to observe the Trinity test, i.e., the test explosion that would be carried out in the Alamogordo Desert one and a half months later. Groves was dismayed bearing in mind the strict secrecy under which everyone had worked until now. Byrnes, who had not yet become Foreign Secretary but who had a strong position as the President’s representative on the Committee, found that it was now time to intervene. Not even the English had all the information, he said, and if the Russians were invited, Stalin would soon demand more. The best thing would be to continue with the bomb production so that the United States would retain its lead while attempting to improve political relations with the Soviet Union.

Over lunch, Stimson complained that the war entailed lack of conscience and compassion and said he was disturbed by the indifference with which the mass bombings in Europe and Japan were being

[*] From an account of what Stimson actually said, he appears to have confused Frankenstein’s monster with its originator.

acknowledged. He was unable to applaud the bombings of Hamburg, Dresden and Tokyo. Arthur Compton wondered whether maybe they ought to simply be content with demonstrating the bomb to foreign observers. This proposal led to long discussions. What would happen if the bomb did not explode? What would happen if the Japanese still refused to give up – would they have been warned so they could defend themselves more easily against later bombers?

Some wondered whether they ought to warn the Japanese anyway before dropping the atomic bomb on a military target. Others feared that if this were the case, they would send Allied prisoners of war to the target areas. Someone said that the unique thing about the bomb was the fact that it was a single bomb rather than the anticipated destruction or mortality figures it implied. They baulked at the idea of causing substantial losses among the civilian population in Tokyo without warning, so shouldn't they now look at the matter differently?

Stimson took exception to the bombing of large communities. He hoped it would be possible to precision bomb industries and military plants. By contrast, the military experts thought that in Japan, every home was a small industry for the manufacture of parts for aircraft, weapons and ammunition. They thought the civilian population participated in the war.

The Interim Committee's meeting continued the next day, now with four industrialists present. The director of Du Pont, *Walter Carpenter* (1888-1976), thought it would take at least four years for the Soviet Union to build a plutonium factory the size of Hanford. The director of Tennessee Eastman doubted that the Russians could grasp the precision technology sufficiently to build an electromagnetic separation plant for uranium-235. On the other hand, the director of Westinghouse, *George Bucher* (born in 1888) believed that it would also be possible, maybe in three years' time, if the Russians were assisted by German scientists and technicians. The Deputy Director of Union Carbide gave his opinion on gas diffusion and thought it would take the Russians ten years to erect such a plant from scratch unless they could obtain essential information through espionage. If they could, only three years would be needed.

In the end, since Stimson had left the meeting, Byrnes formulated a recommendation that everyone supported. The minutes read as follows (quoted from Rhodes):

> *Mr. Byrnes recommended*, and the Committee *agreed*, that the Secretary of War should be advised that, while recognising that the final selection of the target was essentially a military decision, the present view of the Committee was that the bomb should be used against Japan as soon as possible; that it be used on a war plant surrounded by workers' homes; and that it be used without prior warning.

Byrnes went directly from the meeting to the White House and informed the President of the advice that the Committee had formulated. Truman said he had thought about the problem himself and drawn the same conclusion. However much he regretted it, he must give the order to use the bomb.

Seven scientists met at the Met. Lab. in Chicago on 4 June, convened by James Franck in an attempt to stop the use of the atomic bomb on Japan. The group included Glenn Seaborg and Leo Szilard. They continued their deliberations during the week and finished a report that was given to George Harrison to submit to Secretary of War Stimson. The seven scientists feared that, in spite of all secrecy, other countries would also soon have atomic bombs. If there were a nuclear war, the United States would be particularly vulnerable due to the large concentrations of population and industry. The only way of preventing a war was international control of the development, thought Franck's group. They also said (quoted from Knebel & Bailey):

> We believe that these considerations make the use of nuclear bombs for an early unannounced attack against Japan inadvisable. If the United States were to be the first to release this new means of indiscriminate destruction upon mankind, she would sacrifice public support throughout the world, precipitate the race for armaments and prejudice the possibility of reaching an international agreement on the future control of such weapons.
>
> Much more favourable conditions for the eventual achievement of such an agreement could be created if nuclear bombs were first revealed to the world by a demonstration in an appropriately selected uninhabited area.

While Franck's group had formulated this statement, the Interim Committee's scientific group (Compton as chairman, Fermi, Lawrence and Oppenheimer) met on 9-10 June in Los Alamos to discuss whether a demonstration of the bomb was viable. The most eager advocate of this was Ernest Lawrence. Conceivable places were discussed: uninhabited islands, New Mexico and even an area in Japan evacuated for the purpose. But the group found none of these possibilities realistic and finally agreed to write a report recommending military use. The report reached George Harrison on 16 June, five days after the report from Franck's group. It said (quoted from Knebel & Bailey):

> Those who advocate a purely technical demonstration would wish to outlaw the use of atomic weapons, and have feared that if we use the weapons now our position in future negotiations will be prejudiced. Others emphasize the opportunity of saving American lives by immediate military use, and believe that such use will improve the international prospects, in that they are more concerned with the prevention of war than with the elimination of this specific weapon.
>
> We find ourselves closer to these latter views; we can propose no technical demonstration likely to bring an end to the war; we see no acceptable alternative to direct military use.

On 18 June 1945, President Truman called his top military bosses to the White House to be able to make a decision on the strategy against Japan. The General Staff had spent a few days working out definitive plans for two invasion operations. The one, called Olympic, proposed an invasion with approximately 800 000 men of the southernmost of the large Japanese islands, Kyushu, on 1 November. The second, called Coronet, concerned an invasion of the main Japanese island, Honshu, near Tokyo five months later.

General Marshall did not believe that the losses* through an invasion of Kyushu would be greater than those during the capture of the Philippines' island of Luzon when the Americans had lost 31 000 men (the Japanese lost 100 000). The losses ought in any case to be fewer than during the capture of Okinawa which, in its last few days, had seen 13 000 American soldiers killed and 26 000 wounded (100 000 Japanese soldiers had been killed). The potential cost of operation Coronet in terms of human life was not discussed as far as we know. Truman approved the plans and so did Secretary of War Stimson, although the latter said later on that he '***HOPED*** for some fruitful accomplishment ***THROUGH OTHER MEANS***' and was obviously referring to the atomic bomb.

In Japan, Foreign Secretary Tojo thought that the military ought to realise that the war could no longer continue. He was supported by the head of the Japanese army's intelligence service, who thought the country's situation was desperate. The lack of materials was now making things difficult and the workers for the industries, who were now often children, lacked sufficient experience.

On 22 June, the Supreme War Council met with the Emperor present. The latter thought that efforts ought to be made to terminate the war and reprimanded Chief of Army Staff Umezu. Ambassador Sato in Moscow also advised the government to terminate the war.

On 12 July, on behalf of the Emperor, Foreign Minister Togo telegraphed to Sato: 'It is his Majesty's heart's desire ...' to restore peace but not without condition, ' ... as long as America and England insist on unconditional surrender, our country has no alternative but to see it through in an all-out effort for the sake of survival and the honour of the homeland'. In Moscow, Sato undertook a fruitless search for Molotov, who could not afford him any time; Molotov was going to the Allies summit meeting in Potsdam. On 12 July, Prince *Fumimaro Konoe* (1891-1945) travelled to Moscow to negotiate. Prince Konoe was one of the greatest peace advocates among Japanese politicians; he was head of government from 1940-1941 and had attempted for a long time to quieten down the most violent of activists to no avail; Konoe committed suicide after the war.

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* The military terminology is often misleading for a civilian reader. A soldier is "lost" if he is no longer capable of fighting. It does not necessarily mean that he has been killed. The word "loss" can sometimes mean dead and wounded but in other contexts simply killed.

On 1 July 1945, Professor Smyth finished his official report on the Manhattan Project and wrote the preface to it. Before the report was sent for printing one month later, a final meeting was held on how it should be handled. At the suggestion of Conant and Groves, it was agreed that it should be published immediately after the use of the first atomic bomb. Conant emphasised that the report would very helpful when it came to handling the unavoidable deluge of questions that could be anticipated. Chadwick, who was also present, became very concerned when he saw the quantity of information in the report which had been kept secret up until now. When he read it carefully, he calmed down and, after a few days, he said (quoted from Groves):

> I am now convinced that the very special circumstances arising from the nature of the project, and of its organisation, demand special treatment, and that a report of this kind may well be necessary in order to maintain security of the really essential facts of the project.

Chadwick continued by saying that the content of the report could be of use to other countries that were doing research into nuclear power. However, he thought, ‘I would agree that such assistance to possible competitors is not as much as one might think at first sight; it is indeed more apparent than real’. Chadwick believed that other countries that tried to make atomic bombs would gain three months at the very most by reading the report.

On 3 July, Jimmy Byrnes was installed as Foreign Secretary and prepared himself to accompany Truman to Potsdam. The Secretary of War on the other hand, whom Byrnes saw as a rival, had not been asked to accompany them. Stimson had diplomatically asked the President whether the reason why he was not being included in the American delegation was because of his age (Stimson was 78). It was an unnecessary opinion, said Stimson knowingly; he had nothing against travelling. The day after Byrnes had been installed, Truman decided to invite Stimson to come along, but by then Stimson had already gone away with the military transport ship *Brazil* by means of Marseilles. Stimson was treated unfairly in Potsdam and did not live in the same place as Truman and Byrnes, and nor did he participate in their discussions. Byrnes appeared to see Stimson as an intruder.

Truman and Byrnes began their trip on 6 July by the cruiser *Augusta* to Antwerp. They had with them a memorandum that Stimson had given Truman on 2 July. It warned of an invasion of Japan and suggested that the Japanese be given a strong warning instead. He wrote (quoted from Rhodes):

> […because] the Japanese are highly patriotic and certainly susceptible to calls for fanatical resistance to repel an invasion, [America would probably] have to go through with an even more bitter finish fight than in Germany [if it attempted to invade].
>
> […] I believe Japan is susceptible to reason in such a crisis to a much greater extent than is indicated by our current press and other current comment. Japan is not a nation composed wholly of mad fanatics of an entirely different mentality from ours. On the contrary, she has within the past century shown herself to possess extremely intelligent people, capable in an unprecedentedly short time of adopting not only the complicated technique of Occidental civilization but to a substantial extent their culture and their political and social ideas. Her advance in these respects […] has been one of the most astounding feats of national progress in history.
>
> It is therefore my conclusion that a carefully timed warning be given to Japan [...].
>
> I personally think that if in [giving such a warning] we should add that we do not exclude a constitutional monarchy under her present dynasty, it would substantially add to the chances of acceptance.

The question of the monarchy and the Emperor’s position was absolutely crucial. The American public could be expected to be have no comprehension whatsoever of any decision to allow the small, short-sighted Emperor to remain in his position - he was Japanese militarism personified and according to many ought to be punished as a war criminal. On the other hand, the Japanese public would have absolutely no comprehension of any negotiation regarding the Emperor’s situation; it was not for people to decide – the Emperor’s power was divine.

Truman and Byrnes began to realise that it could be beneficial if all parties were to allow the Emperor to remain; with his help, it would be considerably easier to obtain a total surrender.

On 10 July, General Groves approved a press release written by Laurence and intended for publication after the bomb had been dropped. The release was written before the test had taken place in the Alamogordo Desert and therefore had to be changed slightly after the test.

In Chicago, chemist *Farrington Daniels* (1889-1972), the new head of the Met. Lab., had been given the task by Arthur Compton of asking the personnel for their views on the use of the bombs. On 12 July, Daniels himself contacted 150 scientists and let them read a sheet of typed paper describing five numbered conceivable uses of the bombs. The contacted scientists were given a sheet of paper to write down the number that they thought best matched their view, and Daniels collected the sheets and counted them. The result was as follows (quoted from Knebel & Bailey):

1. (23 votes) Use the bombs in the manner that, from a military point of view, is most effective in bringing about prompt Japanese surrender at minimum human cost to our armed forces.

2. (69 votes) Give a military demonstration in Japan to be followed by renewed opportunity for surrender before full use of the weapon is employed.

3. (39 votes) Give an experimental demonstration in this country, with representatives of Japan present, followed by a new opportunity to surrender before full use of the weapon is employed.

4. (16 votes) Withhold military use of the weapon, but do a public experimental demonstration of its effectiveness.

5. (3 votes) Maintain as secret as possible all developments of our new weapons and refrain from using them in this war.

So, there were many views among the scientists but the machinery was now already in motion. On 14 July, Major *Robert Furman* signed for the load of uranium-235 in Los Alamos for transportation to Tinian. It was still the only load there was and it was therefore extremely expensive. If something were to happen to it, the use of an atomic bomb in Japan would be delayed by several weeks. On 16 July the uranium was very secretively put on board the heavy cruiser *USS Indianapolis* in San Francisco's harbour. The Captain understood that it was something unusual and believed that such a cherished load was a bomb for biological warfare.

On Tinian, while awaiting the uranium, you could listen to *Tokyo Rose* who spoke in English for radio broadcasts from Tokyo. It was a concern that she knew of the 509th Composite Group and even more worrying when she said that the Japanese anti-aircraft defence would easily shoot down their bomber since it was easy to recognise by the large 'R' that was painted on the tail fin. This 'R' had actually just been painted there on the left side of some planes to confuse the enemy.

Those participating in the summit meeting began to arrive in Potsdam. The meeting was delayed by one day because Stalin had had a slight heart attack, but also because the 'man of steel' was afraid of flying and undertook the journey by a special train whose most luxurious carriages had been obtained from a rail museum. *Augusta* arrived in Antwerp on Sunday 15th July. Truman was driven by car to Brussels and flew from there to Berlin by the presidential plane called *The Sacred Cow*. He and Byrnes were driven by car from there to Potsdam where he ate dinner and retired to bed early. Churchill had also flown to Berlin and travelled by car to his accommodation in Potsdam. He spent the warm evening on a balcony where he sat drinking whisky with his hat on.

Potsdam was in the Russian occupation zone and the Russians were therefore the ones who made the practical arrangements. The meeting would begin on Tuesday 17 July in the Cecilienhof Palace; Monday was a public holiday. In the morning, Churchill came to visit Truman and they appeared to take an instant liking to one another. Churchill called Truman 'a man of immense determination. He takes no notice of delicate ground; he just plants his foot firmly on it'.

That same day, the Trinity explosion had taken place in the Alamogordo Desert and the coded messages from Harrison began to reach Stimson. Groves preferred this method of contact to direct messages to the President since telegrams from Groves to Truman could have aroused undesirable curiosity. The first message to Stimson came on the Monday evening and read (quoted from Rhodes):

> OPERATED ON THIS MORNING. DIAGNOSIS NOT YET COMPLETE BUT RESULTS SEEM SATISFACTORY AND ALREADY EXCEED EXPECTATIONS. LOCAL PRESS RELEASE NECESSARY AS INTEREST EXTENDS GREAT DISTANCE. DR. GROVES PLEASED. HE RETURNS TOMORROW. I WILL KEEP YOU POSTED.

Stimson was also pleased and relieved. He said to a colleague: 'I have been responsible for spending two billion dollars on this atomic venture. Now that it is successful I will not be sent to prison in Fort Leavenworth.'

On the Tuesday morning, Stimson hurried to show the telegram to Byrnes and Churchill. Stimson appealed to Byrnes to warn the Japanese before the bomb was used, but Byrnes did not want to listen. Stimson had just as little success with Churchill when he suggested that Stalin ought to hear about the bomb.

Since Stalin had not yet arrived, Churchill decided to take a trip to Sanssouci, Fredrik II's summer residence, and Truman was therefore on his own to meet Stalin when the latter arrived at 12.00 and apologised for being late. Stalin confirmed that the Soviet Union would attack Japan in mid-August, something which Truman no longer liked the idea of as much as he had before. It is important to keep your word, thought Stalin.

The summit meeting began at 17.00. Stalin suggested that Truman could chair the meeting. Churchill was restless and probably worried about the outcome of the impending election in England. His British colleagues were anxious. *Sir Alexander Cadogan* (born in 1884) wrote to his wife (quoted from Mee):

> The P.M., since he left London, has refused to do any work or read anything. That is probably quite right, but then he can't have it both ways: if he knows nothing about the subject under discussion, he should keep quiet, or ask that his Foreign Secretary be heard. Instead of that, he butts in on every occasion and talks the most irrelevant rubbish, and risks giving away our case at every point. Truman is most quick and businesslike. He was only trying, at this first meeting, to establish a list of the questions we must deal with. Every mention of a topic started Winston off on a wild rampage from which the combined efforts of Truman and Anthony [Eden] with difficulty restrained him.
>
> Another member of the British delegation was impressed by Stalin (also quoted from Mee):
>
> He spoke quietly, shortly, in little staccato sentences which Pavlov, his young interpreter, translated immediately into forceful English. In the discussions Stalin was often humorous, never offensive; direct and uncompromising. His hair was greyer than I expected, and was thinning. His eyes looked to me humorous, and often showed as mere slits.

A slight dizziness overcomes you when you think of the irrational and arbitrary way in which the future of the world can be determined by a few people whose actions are often controlled by momentary moods and frames of mind. One example is the discussion that concluded the first day's negotiations. It concerned the German fleet (quoted from Mee):

TRUMAN:	'We must specify the concrete questions for discussion at tomorrow's sitting.'
CHURCHILL:	'The Secretaries should give us three or four points, enough to keep us busy.'
TRUMAN:	'I don't just want to discuss, I want to decide.'
CHURCHILL:	'You want something in the bag each day.'
TRUMAN:	'I also propose that we should start our sittings at four o'clock instead of five.'

STALIN: 'Four? Well, all right.'
CHURCHILL: 'I will obey your orders.'

Stalin could not resist the opening that Churchill gave him.

STALIN: 'If you are in such an obedient mood today, Mr. Prime Minister, I should like to know whether you will share with us the German fleet.'
CHURCHILL: '…this navy should be either sunk or divided.'
STALIN: 'Do you want it sunk or divided?'
CHURCHILL: 'All means of war are terrible things.'
STALIN: 'The fleet should be divided. If Mr. Churchill prefers to sink the fleet, he is free to sink his share of it; I have no intention of sinking mine.'

Anthony Eden, the British Foreign Secretary, was furious at Churchill for having so casually gambled away an important card. The whole of the German Navy was in the hands of the British but Churchill had been spellbound by Stalin's personality and let the Russians take over half.

That Tuesday evening of 17 July, Stalin held a banquet for those who had participated in the meeting, with champagne and Russian caviar. The host had temporarily swapped his pipe for a cigar, which Churchill took to be a good sign. When Stimson returned to his overnight accommodation, the next coded telegram from Harrison awaited him (quoted from Mee):

> DOCTOR HAS JUST RETURNED MOST ENTHUSIASTIC AND CONFIDENT THAT THE LITTLE BOY IS AS HUSKY AS HIS BIG BROTHER. THE LIGHT IN HIS EYES IS DISCERNIBLE FROM HERE TO HIGHHOLD AND I COULD HAVE HEARD HIS SCREAMS FROM HERE TO MY FARM.

The officers who deciphered the telegram thought that Stimson had become a father and wondered whether the event ought to be celebrated - he was seventy-eight years old after all. 'The doctor' was of course General Groves, the *Little Boy* was the name of the uranium bomb and the 'big brother' the plutonium bomb that had been tested in the Alamogordo Desert. The stated distances referred to how far away the light and the sound from the plutonium bomb could be perceived. 'From here' meant from Washington DC. 'Highhold' was Stimson's Long Island farm, 400 km from Washington DC. 'My farm' was Harrison's farm 80 km outside Washington DC.

However, the new and crucial message in the telegram was that a charge had now been produced for the uranium bomb and they could now bomb Japan at any time (the plutonium had all been used for the Trinity test and a new charge was not yet ready).

Deciphered telegram in hand, Stimson wandered over to Truman's house for a late dinner. As well as Stimson, the guests were Generals Marshall and Arnold and Admiral *Ernest King* (1878-1956), head of all of the United States' Naval Forces. All guests rejected Truman's plans to use the atomic bomb on Japan without warning. Truman warded off a discussion by saying that he would not make the final decision until he had been able to read General Groves' full report on the Alamogordo test.

What Stimson objected to was the decision to drop the bomb without warning; he, like most of the Generals, thought the use of the bomb was necessary. However, he fell out with General Eisenhower on that matter since he had a different view. Eisenhower was also irritated about the, as he thought, childish wording of the latest telegram. Eisenhower's account of the quarrel reads (quoted from Rhodes):

> The cable was in code, you know the way they do it. 'The lamb is born' or some damn thing like that. So then he told me they were going to drop it on the Japanese. Well, I listened, and I didn't volunteer anything because, after all, my war was over in Europe and it wasn't up to me. But I was getting more and more depressed just thinking about it. Then he asked for my opinion, so I told him I was against it on two counts. First, the Japanese were ready to surrender and it wasn't necessary to hit them with that awful thing. Second, I hated to see our country be the first to use such a weapon. Well...

> the old gentleman got furious. And I can see how he would. After all, it had been his responsibility to push for all the huge expenditure to develop the bomb, which of course he had a right to do, and was right to do. Still, it was an awful problem.
>
> Note that Eisenhower talks about 'his' war, as Stalin talked about 'his' share of the German Navy.

In Moscow, Ambassador Sato heard from the Russian Ministry for Foreign Affairs that Prince Konoe's assignment seemed far too vague - what did the Japanese actually want? On 19 July, Sato telegraphed Foreign Secretary Tojo. Sato did not think that the conditions were right for peace negotiations and recommended that Japan surrender unconditionally. Tojo responded on 21 July with an instruction to Sato to inform the Russians that Prince Konoe was there to negotiate the Russian-Japanese relations and that the Japanese government hoped for assistance by means of the Russian's rapid termination of the war.

On 19 July, a power struggle between Generals Groves and LeMay also ended. The latter could not understand why Washington DC retained the responsibility for the new weapon. When it became available, thought LeMay, it ought to be 'my baby'. But LeMay had to give way, and when he was visited by Tibbets on Guam on 19 July, he accepted Tibbets' demand that the 509th Composite Group must work independently and that Tibbets would carry out the first bombing. LeMay set just one condition. He wanted one of his own officers, Colonel *William Blanchard*, to be able to participate in a test flight on the same day to make sure that Tibbets and his crew 'knew what they were doing' despite the fact that they had not jointly participated in any war operation. When the test flight had taken place and the plane had landed, Blanchard is said to have been white-faced after Tibbets' advanced flying and to have simply said: 'OK, you've proved your point.'

Tibbets had selected fifteen crew to fly B-29s and lie in wait for the bombing assignment. They had to do test flights over Japan, partly to get used to the risks and partly to get the Japanese used to seeing individual bombers. On 20 July 1945, ten of this crew started for the first time. Their test assignment consisted in dropping a 5-tonne blockbuster over different stated targets. If the view deteriorated too much, they would choose their own targets but must not drop any bombs on the potential atomic bomb targets of Hiroshima, Kyoto, Kokura and Niigata (Stimson's dispute with Groves over Kyoto had still not led to a result on Tinian).

One of the planes, called *Straight Flush*[*], was flown by Major *Claude Eatherly* (1919-1978). He had given his crew to understand that he was an experienced fighter pilot, but the assignment was actually the first in which he would fly with the risk of being shot down. Eatherly found that his primary target was not visible due to bad weather. He then decided to choose a secondary target which he hoped would afford him a place in history – the Emperor's Palace in Tokyo.

It never occurred to Eatherly that this would contravene the United States' policy, that it could compromise the Potsdam negotiations and that the consequence of bombing the Emperor's Palace could be to increase the will of the Japanese to continue the war. Luckily, the weather was also bad in Tokyo and Eatherly had to drop the bombs at random. The only effect was that *Tokyo Rose* said on the radio in the evening (quoted from Thomas and Morgan-Witts):

> The tactics of the raiding enemy planes have become so complicated that they cannot be anticipated from experience or common sense. The single B-29 which passed over the capital this morning was apparently using a sneak tactic aimed at confusing the minds of the people.

At 11.35 local time on Saturday 21 July, Stimson in Potsdam received General Groves' detailed report on the Trinity test. He asked for time to meet President Truman and in the meantime read through the report himself and showed it to General Marshall. At 15.30 he was able to show the report to the President and Foreign Secretary Byrnes before the day's meeting began at 16.00. Stimson told Churchill

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[*] The name referred to nothing as innocent as a poker hand – the painting between the words on the aircraft showed a toilet bowl in which a Japanese person was being flushed down.

about the report in the evening, and the latter was able to read it the following day. Churchill's reaction is shown by Stimson's diary note from Sunday 22 July (quoted from Groves):

> Churchill read Groves' report in full. He told me that he had noticed at the meeting of the Three yesterday that Truman was much fortified by something that had happened, that he had stood up to the Russians in a most emphatic and decisive manner, telling them as to certain demands that they could not have and that the United States was entirely against them. He said, 'Now I know what happened to Truman yesterday. I couldn't understand it. When he got to the meeting after having read this report, he was a changed man. He told the Russians just where they got on and off and generally bossed the whole meeting.' Churchill said he now understood how this pepping up had taken place and he felt the same way.

When Stimson asked when the bombs would be ready for use, Harrison telegraphed the following answer on Monday 23 July (quoted from Rhodes):

> Operation may be possible any time from August 1 depending on state of preparation of patient and condition of atmosphere. From point of view of patient only, some chance August 1 to 3, good chance August 4 to 5 and barring unexpected relapse almost certain before August 10.

Stimson had also asked for a list of the bomb targets 'always excluding the particular place against which I have decided. My decision has been confirmed by the highest authority'. Stimson was anxious for Kyoto not to be included again. Harrison's response was: Hiroshima, Kokura and Niigata, in said order of choice. Nagasaki was not yet included.

On the morning of Tuesday 24 August, Stimson was able to show President Truman yet another telegram, this time showing information on the plutonium bomb, *Fat Man*. The first bomb of this type was estimated to be ready for use on 6 August and a second bomb on 24 August. They reckoned on producing three plutonium bombs per month after that.

Truman was very pleased with this information but for how long could Stalin be kept in the dark? And if he was informed, would he accelerate a Russian attack on Japan? This worried Truman since it would make it easier for Stalin to make demands. However, if Stalin found that Churchill and Truman had gone behind his back by not telling him about the atomic bomb, he might be less willing to cooperate. Truman discussed the problem with Byrnes over Tuesday lunch and decided to hedge his bets with a half-truth.

After Tuesday's meeting, Truman walked slowly around the big, cloth-covered meeting table so he happened to pass Stalin and had an opportunity to exchange a few pleasantries with apparent innocence. During the informal conversation, he took the opportunity to say a few words about a new weapon. Truman himself has said (quoted from Mee):

> I casually mentioned to Stalin that we had a new weapon of unusual destructive force. The Russian Premier showed no special interest. All he said was that he was glad to hear it and hoped we would make 'good use of it against the Japanese'.

After this conversation, Truman was able to maintain that he had behaved as an honest and reliable cooperation partner, and he and Churchill could breathe a sigh of relief. Stalin appeared not to have understood the significance of the information. Churchill has given the following account (quoted from Mee):

> I was sure that [Stalin] had no idea of the significance of what he was being told [...] If he had the slightest idea of the revolution in world affairs which was in progress his reactions would have been obvious. Nothing would have been easier than for him to say, 'Thank you so much for telling me about your new bomb. I of course have no technical knowledge. May I send my expert in these nuclear sciences to see your expert tomorrow morning?' But his face remained gay and genial and the talk between these

> two potentates soon came to an end. As we were waiting for our cars I found myself near Truman. 'How did it go?' I asked. 'He never asked a question,' he replied.

In actual fact, Stalin knew perfectly well about the atomic bomb, even if it was not clear whether he understood the significance thereof before the bombs were dropped on Japan. And Truman had now decided to use the bombs. In his diary he wrote (quoted from Rhodes):

> [...] This weapon is to be used against Japan between now and August 10th. I have told the Sec. of War, Mr. Stimson, to use it so that military objectives and soldiers and sailors are the target and not women and children. Even if the Japs are savages, ruthless, merciless and fanatic, we as the leader of the world for the common welfare cannot drop this terrible bomb on the old capital or the new.
>
> He & I are in accord. The target will be a purely military one and we will issue a warning statement asking the Japs to surrender and save lives. I'm sure they will not do that, but we will have given them the chance. It is certainly a good thing for the world that Hitler's crowd or Stalin's did not discover this atomic bomb. It seems to be the most terrible thing ever discovered, but it can be made the most useful.

On Wednesday 25 July, the meeting began at 11.00 in the morning. Truman read out a statement which said that the American Constitution did not give him the jurisdiction to enforce the agreements reached in Potsdam without the approval of the American Senate. Strangely enough, this reservation led to no great discussion.

Churchill then left the meeting to return to London and await the election results. If he lost the election, the opposition leader Clement Attlee would succeed him even during the final days of the Potsdam meeting (Attlee was in the British delegation). Churchill took his leave and said that he hoped to return. Stalin said that by the look on Mr Attlee's face, he concluded that the latter was none too eager to take over Churchill's responsibility. The conference then took a two-day break since not only Churchill but the majority of the British delegation left Potsdam to be in London for the election. Before Churchill travelled, on that same day he had signed an ultimatum to the Japanese government along with Truman. They had also obtained Chiang Kai-shek's approval of the text by radio but Stalin had been excluded. When Molotov was told by Byrnes, the Proclamation had already been sent. When Molotov asked why Stalin's was not among the signatures, Byrnes responded that the Soviet Union was not at war with Japan.

The ultimatum, called the Potsdam Proclamation (as opposed to the minutes from the whole conference which are usually called the Potsdam Declaration), was worded as follows (quoted from Mee):*

> (1) We - the President of the United States, the President of the National Government of the Republic of China, and the Prime Minister of Great Britain, representing the hundreds of millions of our countrymen, have conferred and agree that Japan shall be given an opportunity to end this war.
>
> (2) The prodigious land, sea and air forces of the United States, the British Empire and of China, many times reinforced by their armies and air fleets from the west, are poised to strike the final blows upon Japan. This military power is sustained and inspired by the determination of all the Allied Nations to prosecute the war against Japan until she ceases to resist.
>
> (3) The result of the futile and senseless German resistance to the might of the aroused free peoples of the world stands forth in awful clarity as an example to the people of Japan. The might that now converges on Japan is immeasurably greater than that which, when applied to the resisting Nazis,

* The use of language varies. The Potsdam Conference mainly concerned the problems in Europe, and Japan is not mentioned in the minutes (the Potsdam Declaration). Rhodes also calls the separate ultimatum to Japan the Potsdam Declaration and it was also a declaration from Potsdam of course, but without Stalin's input. To avoid misunderstandings, I preferred to use Mee's name, the Potsdam Proclamation, for the ultimatum.

necessarily laid waste to the lands, the industry and the method of life of the whole German people. The full application of our military power, backed by our resolve, will mean the inevitable and complete destruction of the Japanese armed forces and just as inevitably the utter devastation of the Japanese homeland.

(4) The time has come for Japan to decide whether she will continue to be controlled by those self-willed militaristic advisers whose unintelligent calculations have brought the Empire of Japan to the threshold of annihilation, or whether she will follow the path of reason.

(5) Following are our terms. We will not deviate from them. There are no alternatives. We shall brook no delay.

(6) There must be eliminated for all time the authority and influence of those who have deceived and misled the people of Japan into embarking on world conquest, for we insist that a new order of peace, security and justice will be impossible until irresponsible militarism is driven from the world.

(7) Until such a new order is established and until there is convincing proof that Japan's war-making power is destroyed, points in Japanese territory to be designated by the Allies shall be occupied to secure the achievement of the basic objectives we are here setting forth.

(8) The terms of the Cairo Declaration shall be carried out and Japanese sovereignty shall be limited to the islands of Honshu, Hokkaido, Kyushu, Shikoku and such minor islands as we determine.

(9) The Japanese military forces, after being completely disarmed, shall be permitted to return to their homes with the opportunity to lead peaceful and productive lives.

(10) We do not intend that the Japanese shall be enslaved as a race or destroyed as a nation, but stern justice shall be meted out to all war criminals, including those who have visited cruelties upon our prisoners. The Japanese Government shall remove all obstacles to the revival and strengthening of democratic tendencies among the Japanese people. Freedom of speech, of religion, and of thought, as well as respect for the fundamental human rights shall be established.

(11) Japan shall be permitted to maintain such industries as will sustain her economy and permit the exaction of just reparations in kind, but not those which would enable her to re-arm for war. To this end, access to, as distinguished from control of, raw materials shall be permitted. Eventual Japanese participation in world trade relations shall be permitted.

(12) The occupying forces of the Allies shall be withdrawn from Japan as soon as these objectives have been accomplished and there has been established in accordance with the freely expressed will of the Japanese people a peacefully inclined and responsible government.

(13) We call upon the government of Japan to proclaim now the unconditional surrender of all Japanese armed forces, and to provide proper and adequate assurances of their good faith in such action. The alternative for Japan is prompt and utter destruction.

The Proclamation did not mention the role of the Emperor and thus gave no guarantee of his position following a surrender. Nor did it mention the atomic bomb and therefore did not give the warning that Secretary of War Stimson would have wanted to see.

In Washington DC, the final order was issued to use the atomic bombs. It was aimed at General *Carl Spaatz* (1891-1974) who was head of the Pacific Ocean area's bombers and signed by General *Thomas Handy* (1892-1982) who, while General Marshall was staying in Potsdam, stood for the latter as General Chief of Staff. But the order was written by Groves. The two first points of the order read (quoted from Knebel & Bailey):

1. The 509th Composite Group, the 20th Air Force, will deliver its first special bomb as soon as weather will permit visual bombing after 3 August 1945 on one of the

targets: Hiroshima, Kokura, Niigata and Nagasaki. To carry military and civilian scientific personnel from the War Department to observe and record the effects of the explosion of the bomb, additional aircraft will accompany the airplane carrying the bomb. The observing planes will stay several miles distant from the point of impact of the bomb.

2. Additional bombs will be delivered on the above targets as soon as made ready by the project staff. Further instructions will be issued concerning targets other than those listed above.

Nagasaki was now also stated among the bomb targets for the first time.

On Thursday 26 July, Churchill lost the British election and was succeeded by Attlee. On Saturday 28 July, Attlee returned to Potsdam as the British representative, accompanied by his Foreign Secretary *Ernest Bevin* (1881-1951), who succeeded Anthony Eden. The statement that summarised the decision made at the meeting, i.e., the Potsdam Declaration, was signed by Attlee, Stalin and Truman. Since the document concerned only European matters, Chiang Kai-shek was never asked.

14. HIROSHIMA AND NAGASAKI

ON 26 JULY 1945, the cruiser *Indianapolis* came to Tinian with the uranium for the first atomic bomb. It was the cruiser's last assignment as three days later she was sunk by a torpedo from a Japanese submarine. The Potsdam Proclamation's text was sent by radio from San Francisco early in the morning Japanese time on Friday 27 July. The Japanese leaders found the text enigmatic but observed that it did not have Stalin's signature, which could indicate that the Russians still considered themselves to be neutral. Both the head of government Baron Suzuki and Foreign Secretary Tojo noted that the Russians were not at the bottom of the Proclamation, and they thought that they ought to continue to try and get them to assist with peace negotiations. When the government met in the afternoon, Tojo suggested that they might like to wait until they were sure about the attitude of the Russians before making their mind up about the Proclamation.

A shortened version of the Proclamation was published in the Japanese morning newspapers of 28 July. It had omitted the promise that the Japanese soldiers would be able to return to their homes in peace and that the Allies did not intend to enslave the Japanese people. But what outraged Tojo was the fact that the newspapers wrote that the government had decided to *ignore* the Proclamation. It seems that this was the word that Suzuki, when pressed by the military leadership, actually used in a conversation after the government's meeting.

Rarely has the importance of a Japanese word been the subject of such detailed discussions as that word, *mokusatsu*, which Suzuki had used to deal with the Proclamation text. One translation is 'to pass by in silence', another is 'discard' or 'reject' (*moku* = be silent, *satsu* = to kill). These interpretation nuances probably made no impression on President Truman. The fact that the Japanese had not said outright that they accepted the Allies' terms and that Stalin said that Japan's peace overtures in Russia had been too vague made Truman stand by the decision to bomb.

On 28 July, the security of the Smyth Report was reviewed and the report was now ready to be published immediately after the bombing. General Spaatz arrived on Guam to participate in a meeting with a number of key people along with his Chief of Staff LeMay: Tibbets, Parsons, Blanchard and a meteorologist. Spaatz read out the order to bomb and Parsons read out a memo from Oppenheimer (quoted from Thomas and Morgan-Witts).

> It is not expected that radioactive contamination will reach the ground. The Ball of Fire should have a brilliance which should persist longer than at Trinity since no dust should be mixed with it. In general, the visible light emitted by the unit should be even more spectacular. Lethal radiation will, of course, reach the ground from the bomb itself.

Of course, Oppenheimer cannot have meant every word of what he had written. When the nuclear fission ceased, every atom released into the atmosphere, with the exception of the radioactive gases, had to reach the ground sooner or later. However, the radioactive contamination moved away from Hiroshima with the wind and some of the atoms were no longer radioactive when they reached the ground.

On 1 August, General Handy in Washington DC was asked by Spaatz whether the known presence of prison camps in Nagasaki changed the plans. Spaatz himself would normally have made a decision on this but he was aware that whole bomb project was controlled from Washington DC. Handy contacted Groves and said that he thought that such a matter ought to be decided by Secretary of War Stimson, who had now returned to Washington DC by plane. However, Groves convinced Handy to say no to Spaatz but to let Stimson see the response first so that he had the chance to protest. Groves did not want anyone to ask Stimson to take any responsibility. The Secretary of War did not protest; he simply said thank you for allowing him to see the response to Spaatz.

Another question came from Spaatz at the same time. It had been discovered that there were no prison camps in Hiroshima. Did this affect the decision? Groves and Handy's response was that if this was correct, Hiroshima ought to be the first bomb target (Groves clearly did not know that there were 23 American prisoners of war in Hiroshima, 13 of whom had parachute-jumped from a B-24 called *Lonesome Lady* on the bomb assignment from Okinawa on 28 July).

On 2 August, President Truman left Potsdam to return to the United States on the cruiser *Augusta*. Since the ship was in Plymouth Harbour, Truman and Byrnes had to fly to England first. Anchored next to *Augusta* was the British *H.M.S. Renown*, aboard which Truman and Byrnes were invited to lunch by King George VI before their departure. Admiral Leahy, who was still sceptical about the atomic bomb, told the King that he doubted that the atomic bomb would function. But George VI, who had been fully informed about the bomb, answered that he was prepared to wager on the matter.

On Tinian, Tibbets began to prepare for the flight to Hiroshima which had now definitely been designated by Spaatz as the primary target owing to the response from Handy. When choosing the aircraft, Tibbets decided on the B-29 which was normally flown by 26-year-old Captain *Robert Lewis*. Unlike many of the other pilots, Lewis had not given his plane a name other than its number '82'. Tibbets, feeling that he was facing an historic assignment, decided to christen the plane *Enola Gay*, after his mother's given names. There were normally nine crew on the B-29s but *Enola Gay* would carry an extra three, including Captain William Parsons who was responsible for the bomb. Six B29s would participate in the attack on Hiroshima, and nearly all of them had original names. The most important plane apart from *Enola Gay* was *The Great Artiste*, which would not be flown by its ordinary crew but had Major *Charles Sweeney* as its pilot. Sweeney's assignment was to take measurements of the explosion and he had Luis Alvarez with him as responsible scientist, who would much later go on to win the Nobel Prize in Physics (in 1968).

Captain *George Marquadt* would also not fly his usual plane, but a plane called *Necessary Evil*. He had the task of photographing the explosion using a high-speed camera. Two planes, *Straight Flush* and *Jabit III*, were flown by their ordinary pilots, Major Claude Eatherly and Captain *John Wilson*, and had to report on the weather situation.* A seventh plane, *Big Stink* with Captain *Charles McKnight* as its pilot, would wait on the small island of Iwo Jima north of the Mariana Islands to take over the bomb if something went wrong with *Enola Gay*.

At 15.00 on 4 August, the crew gathered for a briefing. Tibbets showed maps of the target. Parsons was to play a film of the Trinity test but the projector did not work. He therefore had to improvise and recounted the great distance over which the sound was heard and the light was seen from the explosion. None of the crew had heard anything said about the atomic bomb until now and nor was the word 'atom' mentioned now. Tibbets said that they would probably fly in the morning of Monday 6 August. He said everything that they had previously been involved in was peanuts compared with the bomb they would be dropping on Hiroshima and that the bomb might shorten the war by six months.

One last briefing took place at midnight before the Monday. Tibbets emphasised the importance of following the plans and obey orders. He reminded everyone about the protection glasses that had been handed out. Finally, he handed over to a field priest who said a short prayer (quoted from Thomas & Morgan-Witts):

> Almighty father, Who wilt hear the prayer of them that love Thee, we pray Thee to be with those who brave the heights of Thy heaven and who carry the battle to our enemies. Guard and protect them, we pray Thee, as they fly their appointed rounds. May they, as well as we, know Thy strength and power, and armed with Thy might may they bring this war to a rapid end. We pray Thee that the end of the war may come soon, and that once more we may know peace on earth. May the men who fly this night be kept

* Eatherly was the same man who earlier wanted to drop a bomb on the Emperor's Palace. He left the Air Force in 1947 with a reputation of being a gambler with a weakness for strong drinks. He was later arrested for robbery. At the trial, a psychiatrist said that Eatherly's feelings of guilt about Hiroshima had contributed to his mental problems. A German author wrote a book in which Eatherly was portrayed as the pilot on the plane that dropped the bomb and Eatherly was exploited by the press. Not much of what was written corresponded with reality.

> safe in Thy care, and may they be returned safely to us. We shall go forward trusting in Thee, knowing that we are in Thy care now and forever. In the Name of Jesus Christ. Amen.

In Hiroshima, the Japanese had started destruction to protect the city against fires. They had demolished 70 000 homes during the summer to make way for three wide firebreaks in an east-west direction. In the north-south direction it was assumed that the River Ota's seven channels would provide corresponding protection. The work had been started by a local construction company, but when it was going too slowly the army had intervened with work battalions, possibly using Korean prisoners of war. At the start of August, 30 000 adults and 11 000 students were occupied with this work. The demolition of homes had necessitated a comprehensive evacuation. Up to 100 000 of the city's original approximately 380 000 inhabitants had already been relocated.

There were many industries in the city, and most were involved in the war efforts in one way or another. Hiroshima had Japan's largest shipyard for the repair of smaller ships. A large food factory produced canned goods for the military and the Kirin Brewery was there. Mitsubishi's factories produced aircraft engines and other equipment for military use. The large rail depots were intensively used for the transportation of substantial quantities of war material for defence against an anticipated invasion. However, there were fewer soldiers than when the harbour had been open. Hiroshima was now isolated from the sea since the American plane had dropped so many mines over the inland waters that ships no longer dared to enter the city. The information on the number of remaining soldiers in the city at the start of August varies from 24 000 to 45 000.

On 6 August at 02.45 Tinian time, *Enola Gay* set off for Hiroshima accompanied by *The Great Artiste* with Alvarez' group of scientists and *Necessary Evil* with camera equipment. They had not dared to fit the atomic bomb before starting out since they were afraid that it could have been released on Tinian by a start-up mishap. Parsons therefore had to insert the detonator into the bomb after the start by the light of a torch held by the crew's electronics engineer. The bomb was still not unsecured, however.

The *New York Times'* science journalist William Laurence, who had been asked to document the events, had counted on being able to be in one of the aircraft to Hiroshima but was disappointed. When he found that he had been left out, he asked Captain Lewis to take notes for him.

At 04.55 Japanese time (05.55 Tinian time; Tinian lies to the east of Japan but west of the date line and is therefore ahead in terms of time) the day began to dawn and *Enola Gay* passed Iwo Jima in light. Tibbets took the plane up to an altitude of 9 300 feet to join *The Great Artiste* and *Necessary Evil*. The three planes continued in V formation. They had previously conversed over the radio but from 05.00 (Japanese time) there was no radio. When *Enola Gay* approached the Japanese coast, they started to record everything that was said in the aircraft.

Tibbets explained to the crew that the recording was taking place 'for history, so watch your language!' And so he gave the final message: 'We are carrying the world's first atomic bomb.'

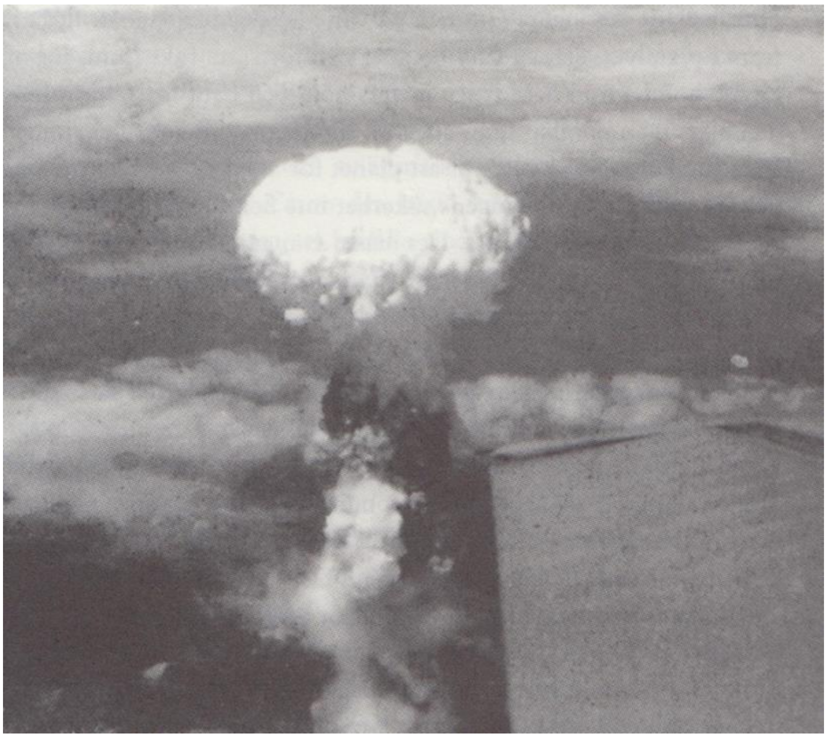
At 06.30 the electronics engineer climbed down to the bomb and exchanged the three green fuse plugs for red plugs. The bomb was now unsecured.

Just after seven in the morning, Hiroshima's inhabitants could see a single American bomber circling above the city at a great altitude. It was *Straight Flush* reporting on the weather. Observation planes simultaneously flew over Kokura and Nagasaki. All potential bomb targets had good weather. When *Straight Flush* left Hiroshima, Tibbets in *Enola Gay* received a radio message from Eatherly advising him to bomb 'the primary' - Hiroshima was the target.

At 07.40 (Japanese time), *Enola Gay* climbed from 9 300 to 31 000 feet, the bomb altitude, and the pressurised cabin was put to work. Tibbets told everyone to don the special protection glasses, first over their foreheads and then, on a special signal, over their eyes.

No fighters opposed them and nor were they troubled by anti-aircraft. At 08.13.30, Tibbets handed the control of the plane to bombardier Major *Tom Ferebee* with the words 'It's all yours.'

At 08.15.00, i.e., 15 seconds before the bomb was to be dropped, Ferebee turned on a warning signal and everyone except for himself pulled their protection glasses down over their eyes. The warning signal was heard over the radio a long distance away. It was heard in the three returning weather observation planes and by Captain McKnight in the reserve plane *Big Stink* on Iwo Jima. Everyone understood that the bomb was about to be dropped on Hiroshima. The bomb left the plane at 08.15.17, which jerked upwards as it lost 4.4 tones. Simultaneously, *The Great Artiste* dropped three packages containing instruments suspended from parachutes. Seconds before everyone was engulfed in a conflagration, the Japanese hurrahed at Hiroshima's gates - they thought that their anti-aircraft had forced the pilots to jump.



The atomic bomb explosion over Hiroshima photographed from *The Great Artiste*

Tibbets has since given an account of the bombing (quoted from Arnold-Forster):

> Up to this point, it was common practice in any theatre of war to fly straight ahead on a level, drop your bombs and keep right on going because you could bomb several thousands of feet in the air and you could cross the top of the place that you had bombed with no concern whatsoever. But it was determined by the scientists that, in order to escape and maintain the integrity of the aircraft and the crew, that this aeroplane could not fly forward after it had dropped the bomb. It had to turn around and get away from the bomb as fast as it could. If you placed this aeroplane in a very steep angle of bank to make this turn, if you turned 158 degrees from the direction that you were going [in], you would then begin to place distance between yourself and that point of explosion as quickly as possible. You had to get away from the shock wave that would be coming back from the ground in the form of an ever-expanding ring as it came upwards. It's necessary to make this turn to get yourself as far as possible from an expanding ring and 158 degrees happened to be the turn for that particular circle. It was difficult. It was something that was not done with a big bomber aeroplane. You didn't make this kind of a steep turn – you might almost call it an acrobatic manoeuvre - and the big aircraft didn't do these things. However, we refined it, we learned how to do it. [...]
>
> As soon as the weight had left the aeroplane I immediately went into this steep turn and we tried then to place distance between ourselves and the point of impact. […] We had just made the turn and rolled out on level flight when it seemed like somebody had grabbed and hold of my aeroplane and gave it a real hard shaking because this was the shock wave that had come up. Now after we had been hit by a second shock wave not quite so strong as the first one I decided we'll turn around and go back and take a look.

> The day was clear when we dropped that bomb, it was a clear sunshiny day and the visibility was unrestricted.
>
> As we came back around again facing the direction of Hiroshima we saw this cloud coming up. The cloud by this time, now two minutes old, was up at our altitude. We were 33,000 feet at this time and the cloud was up there and continuing to go right on up in a boiling fashion, as if it was rolling and boiling. The surface was nothing but a black boiling, like a barrel of tar. Where before there had been a city with distinctive houses, buildings and everything that you could see from our altitude, now you couldn't see anything except a black boiling debris down below.

While *Enola Gay* turned and distanced herself while the bomb fell, tail gunner *George Caron* had the best view and was asked to report on what he saw but to keep his safety glasses on. The bomb was intended to explode after 43 seconds and exploded at almost exactly 08.16 on Monday 6 August 1945 at an altitude of approximately 580 metres. When the bomb exploded, *Enola Gay* and *The Great Artiste* were 13 kilometres away and *Necessary Evil* with the film camera at 27 kilometres away. Caron has given the following description of the explosion (quoted from Rhodes):

> I kept shooting pictures and trying to get the mess down over the city. All the while I was describing this on the intercom [….] The mushroom itself was a spectacular sight, a bubbling mass of purple-grey smoke and you could see it had a red core in it and everything was burning inside. As we got further away, we could see what looked like a few-hundred-foot layer of debris and smoke and what have you.
>
> I was trying to describe the mushroom, this turbulent mass. I saw fires springing up in different places, like flames shooting up on a bed of coals. I was asked to count them. I said, 'Count them?' Hell, I gave that up when there were about fifteen, they were coming too fast to count. I can still see it - that mushroom and that turbulent mass - it looked like lava or molasses covering the whole city, and it seemed to flow outward up into the foothills where the little valleys would come into the plain, with fires starting up all over, so pretty soon it was hard to see anything because of the smoke.

In *The Great Artiste*, Luis Alvarez saw the boiling cloud but could not see any signs of a city. He first thought that Ferebee had missed the target and dropped the bomb over a desert landscape. In *Enola Gay*, Caron warned Colonel Tibbets because the radioactive cloud appeared to be approaching them. The three planes, *Enola Gay*, *The Great Artiste* and *Necessary Evil*, turned and began their return to Tinian. Tibbets sent a couple of radio messages. The first stated openly that *Enola Gay* had bombed its target with a clear view and with 'good results', no fighters and no anti-aircraft.

On the return journey when *The Great Artiste* was 5 km off the Japanese coast, Luis Alvarez had already begun to write to his son (quoted from Knebel and Bailey):

> Dear Walter, This is the first grown-up letter I have ever written to you, and it is really for you to read when you are older. […] Today the lead plane of our little formation dropped a single bomb which probably exploded with the force of 15,000 tons of high explosive. That means that the days of large bombing raids, with several hundred planes, are finished. A single plane disguised as a friendly transport can now wipe out a city. That means to me that nations will have to get along together in a friendly fashion, or suffer the consequences of sudden sneak attacks which can cripple them overnight.
>
> What regrets I have about being a party to killing and maiming thousands of Japanese civilians this morning are tempered with the hope that this terrible weapon we have created may bring the countries of the world together and prevent further wars. Alfred Nobel thought that his invention of high explosives would have this effect by making wars too terrible, but unfortunately it had just the opposite reaction. Our new destructive force is so many thousand's times worse that it may realise Nobel's dream. [...]

Enola Gay landed at the airfield on Tinian at 14.58 local time. By then, the plane had been in the air for twelve hours and thirteen minutes. A couple of hundred officers and other key people surrounded the aircraft when Tibbets and his crew emerged, and several thousand had gathered at the airport and along the path of the plane over the ground. General Spaatz cleared a path to the plane and met Tibbets, on who's overall he pinned the Distinguished Service Cross decoration. Later, all military personnel who had participated in the operation received some form of decoration but none of the four civilian physicists who accompanied them (i.e., Alvarez and his group) – the decorations were only for the military.

The atomic bomb had three different adverse effects: injuries from the gamma radiation that is emitted while most of the fission products were radioactive, burns from heat radiation and mechanical injuries from material that was flung around by the shock wave. Around 2/3 of those injured were affected by burns, 2/3 by mechanical injuries and 1/3 by acute radiation injury - many thus had several types of injury simultaneously.

Between 30 and 40 per cent of the energy given off by the bomb was conveyed by heat radiation. The bomb's ball of fire reached temperatures of tens of millions of degrees. It is therefore not surprising that heat radiation was the dominant cause of injury for Hiroshima's inhabitants. The greatest intensity of this heat radiation was during the first few seconds and it had ceased to be emitted within one minute of the explosion. The faster you went behind some sort of cover, the greater your chance of avoiding burns.

The burns were not your usual burns. Since heat radiation was emitted for a short time, it was of high intensity. This meant that the heat energy emitted when radiation encountered a body did not have time to be diverted before irradiation had finished. The temperature was therefore very high close to the surface of the body. Immediately beneath the bomb over Hiroshima where it exploded at an altitude of 580 metres, the surface temperature of irradiated objects could rise to between 3 000 and 4 000 ˚C. Paper, thin wooden structures and garments could reach sufficiently high temperatures to catch fire at a distance of two to three kilometres. Thicker material charred on the surface. Paler surfaces reflected some heat radiation while objects with dark surfaces became hotter.

Photographs of the victims of the heat radiation were what made the strongest impression when reports on the bombing spread around the world. A picture of a wall with a charred surface apart from one light, spared stain. A stain that took the shape of a silhouette of a human body, a body that had not been spared and that was missing from the picture.

Immediately after the explosion, thousands of fires broke out, partly due to the first few seconds of intense heat radiation but also due to the many household hearths on the morning of the destroyed houses.

An approximate total of 60 000 buildings were completely destroyed. As a consequence of the heat from the fires, the air was drawn upwards, creating a suction which would cause a ground wind which had reached strengths of 15-20 metres per second within a couple of hours. Since this hard wind was blowing in towards the centre, it did limit the scope of the fire to some extent but simultaneously intensified the fire within the area ravaged by fire.

Enola Gay lands on Tinian after having dropped the atomic bomb on Hiroshima

Rescue and first aid were made difficult by the rapid spreading of fires and because there was no organised civilian defence. The survivors rushed towards the river tributaries, the parks or the outskirts of the city. The main first aid consisted in preparing refuges for them.

Very little could be done to restrict the raging fire. Fire stations and rescue material were destroyed and the personnel were injured. The streets were blocked by landslides and were inaccessible to vehicles. The water pipes had been ripped open so they had no pressure. The fire pumps could not be used due to lack of electricity.

The city was particularly vulnerable in that air raid alarms with no consequences had become so usual that the inhabitants no longer bothered to take cover. Those who were outside and not in the shadow of buildings were immediately burned to death within a distance of a few kilometres. Those who were indoors in the central parts of the city were injured by falling ceilings or by shrapnel being flung around, primarily in the form of glass from windows. Those who suffered severe injuries or broken bones died in the flames from which they were unable to escape.

The difficulties dealing with the injured were greatly increased because doctors, nurses, fire personnel and police suffered major losses themselves. It was also difficult to spread information about the scope of the disaster and to give the inhabitants instructions and advice. Gradually the messages reached spared areas in attempts to establish contact between relatives.

It was important to remove the dead since the weather was warm. Most of them were burned where they were found. 90 % of the city's doctors had been killed or injured but, despite their injuries, a number of doctors tried to help to deal with the bomb victims. Just three of the forty-five hospitals in the area were in a serviceable condition. The number of patients meant that only a few could be given satisfactory help. Available stocks of medicine were completely inadequate and there were still no effective antibiotics to be had.

Because all windowpanes had been crushed, it was impossible to keep the treatment places free from dust, dirt and insects. The treatment that it was possible to give to the survivors probably had no essential impact on the mortality figures. The scope of the disaster made the population panic and eventually resulted in a fear of radiation injuries, with thousands more people going to the already overfull hospitals.

Since the drinking water pipes had been destroyed, the water was contaminated in places where it was actually available. There was also a lack of disinfectants and flies were attracted by refuse, excrements and suchlike. It is surprising that no major epidemics broke out; instead, several normally-occurring epidemic diseases such as paratyphus were reduced, probably because the inhabitants were forced to leave what were densely populated areas.

The ionising radiation[*], i.e., primarily gamma radiation, from the bomb, was initially of secondary importance but was not inessential. Gamma radiation has a great penetration capacity and is absorbed to only an insignificant extent in air, so it can (like heat radiation) also reach people who are some distance away. It can then cause deterministic and stochastic injuries (see Chapter 8 for an explanation). The deterministic injury in this case is called acute radiation sickness since it causes symptoms as early as hours or days later.

The typical picture of acute radiation injury arises from radiation doses (exposures) of 100-500 röntgen (corresponding to approximately 1-5 gray).[†] Nausea and vomiting can then arise within a few

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[*] Ionising radiation is the correct name for x rays and radiation from radioactive substances (alpha, beta and gamma radiation and neutron radiation, despite the fact that gamma and neutron radiation ionise only through secondary processes). Unfortunately, the linguistic tautology "radioactive radiation" is often used for radiation from radioactive substances. It is not radiation that is radioactive. This misuse leads to the misunderstanding that part of the radioactive substance is radiating out and that the object encountered by the radiation therefore becomes radioactive in itself; this is not the case.

[†] In the following discussion about radiation injuries, I will use the unit of dose "röntgen" since this was the unit that was usually used in 1945. As I have mentioned in Chapter 9, Herbert Parker introduced the unit "rep", whose numerical value was approximately equal. Actually, the number of röntgen stated the *exposure*, i.e., the capacity of the incident radiation to ionise air, while the number of reps stated the *absorbed dose*, i.e., the energy per unit mass absorbed in the irradiated body. The absorbed dose is now stated in *gray* (Gy). The connection between the numerical values for the units is 1 Gy = 100/0.93 reps, which corresponds to just over 100 röntgen (see Chapter 9).

hours but disappear after a few days. At 100 röntgen, there is a very small risk of death but the outcome of more than 500 röntgen will probably be fatal if no treatment is given.

The reason why deaths occur at these doses is that the bone marrow that forms blood has been injured so that the number of granulocytes (a type of white blood cell that has the capacity to decompose foreign particles and kill bacteria) is substantially reduced. The quantity of granulocytes following irradiation that carries a risk of death falls to a minimum after approximately 30 days. That is when its resistance to infections is heavily reduced and the risk of death is at its greatest. The treatment therefore consists primarily in protecting the patients from infections, something which would have been very difficult in the ravaged Hiroshima even if the risk had been known.

At higher radiation doses, injuries occur to the mucous membranes in the gastrointestinal tract, which impairs the uptake of nutrients and can lead to death after one or two weeks. In this case, the treatment consists of intravenous nutrition. However, at radiation doses of more than 1 000 röntgen, there is a very small possibility of survival. Radiation doses of thousands of röntgen also injure the central nervous system and cause convulsions, difficulty with coordinating movements, loss of consciousness and death within a few days. At 10 000 röntgen (100 gray), death is as good as instantaneous.

If the radiation dose is not fatal, the number of granulocytes will return to normal and the risk of infection will cease. Anyone who has survived for a couple of months therefore has a good chance of making a full recovery. The remaining risk mainly concerns stochastic injuries, i.e., cancer and hereditary injuries. However, the risk of cancer does not occur until after a minimum latency period of several years, maybe not until several decades have passed, and has not been as extensive in Hiroshima as the acute injuries and deaths. In the mid-1990s, a total of approximately 1 000 people more than usual among the Hiroshima and Nagasaki survivors had died of cancer, and it is estimated that another few thousand may die of cancer caused by radiation from the bombs, i.e., an approximate total of 1 % of the survivors. If you have survived the acute radiation sickness, there is also a good chance that you will not be affected by cancer as a consequence of radiation.

It has not been possible to detect any increase in the occurrence of hereditary injuries because inadequate numbers of children have been born to the survivors. However, it is thought that ionising radiation can increase the occurrence of hereditary injuries that also occur under different circumstances. On the other hand, radiation leads to no new types of hereditary injury.

The types of ionising radiation that can cause injuries great distances away from an atomic bomb explosion are gamma radiation and neutron radiation. Gamma radiation from the bomb arises in several different ways. The actual fission process leads to instantaneous gamma radiation but this is of lesser importance. It arises before the material in the bomb has been dispersed by the explosion and is therefore partly absorbed by the bomb material.

Neutrons that have been released during nuclear fission but that have not contributed to the continued chain reaction radiate out from the bomb. They can cause nuclear reactions by being captured by atoms in the bomb material and also by the nitrogen in the air. The reactions lead to atomic nuclei with a surplus energy that is partly emitted in the form of gamma radiation. In practical terms, some of this gamma radiation is also instantaneous.

The neutrons can also convey energy to these by colliding with atomic nuclei, with said partially emitted immediately as gamma radiation. The emission of neutrons and initial, instantaneous gamma radiation takes place within the space of one second.

The splitting products (fission products) that are formed at the time of the explosion are radioactive and some give rise to radioactive daughter products and, in some instances, decay chains of the radioactive substances. When these radioactive substances decay, gamma radiation is often emitted. Some of this radiation is emitted very early on while some of the fission products have long half-lives. Some of this delayed gamma radiation can be included in the instantaneous initial radiation which is emitted over the course of a few seconds, a lesser share may come from the radioactive mushroom cloud several minutes after the explosion, and an even smaller share can come hours, days, weeks, months or years later from the radioactive cloud driven by the wind or from radioactive dust that has fallen to the ground. A small share of this delayed radiation may also come from activation of atomic nuclei that have captured neutrons. In Hiroshima, most of the radiation dose to the survivors came from gamma radiation. The contribution from neutron radiation is thought to have been insignificant.

The estimated number of deaths as a consequence of the atomic bombing of Hiroshima is fairly uncertain due to the chaos that prevailed. One piece of information on the size of the population is that there were approximately 320 000 people, approximately 45 000 of whom were military.

The military was hit the hardest. The fortress was completely destroyed and there was 90 % mortality among those who were in it. Several thousand soldiers on the parade ground were killed directly, as were several thousand who were working to create firebreaks. Between 25 000 and 30 000 soldiers are said to have been killed instantaneously. The victims in the fortress also included a number of schoolgirls who worked at the communication centre, and some of the American prisoners of war.

The fate of the 23 American prisoners of war in Hiroshima is not fully known. The pilot of *Lonesome Lady*, Thomas Cartwright, survived. He had avoided the bomb by having been taken to Tokyo for a trial. His tail gunner, *William Abel*, who successfully avoided capture, had remained hidden for ten days, but gave up on 7 August. Although he was mistreated, he did actually survive. One of the crew members had been seen dying beneath a bridge. A number of the prisoners had been kept locked in a cellar at the fortress and had survived the bombing but then appear to have been killed by outraged Japanese.

It is estimated that the number of near-instant deaths was approximately 50 000, and an additional 30 000 people may have died within one month. In addition to these 80 000 victims, a number of the up to 100 000 injured persons also died some time later. Here, the estimate is very unsafe. Many of the survivors have also suffered severe burns and other challenges which may have affected their general health for life.

When the bomb fell over Hiroshima at 08.16 on the morning of 6 August 1945, the time in Washington DC was 19.16 on the Sunday evening of 5 August, the time difference counted from Hiroshima being 13 hours behind (or, if you will, 11 hours in front minus one day).* General Groves was waiting impatiently for news from Tinian. He did not receive the message that *Enola Gay* had begun her journey to Hiroshima until 18.45, i.e., 07.45 Japanese time. Thereafter, Groves waited in his office for the message that the bomb had been dropped. The anticipated message, sent by Parsons, did not arrive until 23.30. It read (quoted from Groves): ‘Results clearcut, successful in all respects. Visible effects greater than in the New Mexico test. Conditions normal in airplane following delivery’.

It was almost midnight in Washington DC yet General Marshall still received a message that the bombing had been successful. Groves made a first draft of a report to Marshall and then slept for a few hours in a folding bed at the office. He was aroused at 04.30 by a duty officer who handed over a longer telegram written by General Farrell after *Enola Gay* had returned to Tinian.

At 07.00 on the Monday morning, Groves gave the report to Marshall at the latter’s office where General Arnold and George Harrison had also arrived. Marshall rang Secretary of War Stimson and said that the bombing of Hiroshima had been successful. Everyone congratulated one another but Marshall said that they oughtn’t to go overboard with their celebrations since the successful bombing had claimed many thousands of human lives after all. Groves retorted that he was thinking less of the dead Japanese than of the men who had been forced to do the death march from Bataan, and General Arnold agreed.

The death march from Bataan on the Philippines is almost unknown to us Swedes but was very significant in the United States. Bataan is a peninsula where, from the north, the bay ends almost outside Manila in the west. In the Japanese expansion to capture colonies, on 22 December 1941 the whole of the Japanese 14th Army landed on northern Luzon, the largest island in the Philippines on which Manila lies. The American and Philippino troops who were on the island (General MacArthur was training the Philippino Army) were forced back to the Bataan peninsula where they were besieged at the same time as preventing the Japanese from using Manila’s harbour. However, their situation was hopeless. MacArthur was ordered by Roosevelt to do other tasks and his successor on Bataan, General *Jonathan Wainwright* was compelled to surrender to the Japanese General *Masaharu Homma* on 8 April 1942. The exhausted and emaciated American and Philippino soldiers were forced by Homma to march 105 kilometres north to the O'Donnell army camp where they were interned. They were treated with extreme

* The times stated in the reference books are very confusing since Tinian time and Japanese time are interchanged, often with no indication of which is used. The time difference between Tinian and Washington DC is 14 hours behind, counted from Tinian.

brutality and more than 20 000 soldiers are said to have died on the march. Homma's action is considered to have been the worst example of Japanese brutality and disregard of international law for the treatment of prisoners of war. Another good 20 000 of the soldiers died in the prisoner of war camp. 'The Death March from Bataan' aroused wrath and hate in the United States and obviously also affected the attitude of the American Generals as to which measures people had the moral right to take against the Japanese. In his memoirs, Tojo says that the Japanese government was completely ignorant of Homma's crimes. 'I naturally could not imagine the occurrence of such atrocities', he writes.

General Groves rang Oppenheimer and congratulated him. Oppenheimer said that he felt 'tolerably well'. Groves said that he was proud of Oppenheimer and all his colleagues and that he had made one of his best decisions when he chose the manager of Los Alamos.

In Chicago, Szilard wrote a letter to Gertrud Weiss (quoted from Rhodes):

> I suppose you have seen today's newspapers. Using atomic bombs against Japan is one of the greatest blunders of history. Both from a practical point of view on a 10-year scale and from the point of view of our moral position. I went out of my way and very much so in order to prevent it but as today's papers show without success. It is very difficult to see what wise course of action is possible from here on.

In Germany, Samuel Goudsmit was examining the ruins of Himmler's headquarters in Berlin on behalf of Alsos when he suddenly received an urgent telephone call from Frankfurt and was ordered to immediately board a ready-waiting aircraft with Alsos Secretary Mary Bohan to fly to Frankfurt. Upon arrival he was given no explanation as to the rush, but when he escorted Mary Bohan to her hotel following a joint dinner in the evening, they both heard the news of the atomic bomb over Hiroshima on the radio in the lobby. The reason for the rush was now clear. General Groves had decided to ensure that neither Goudsmit nor any other Ally with knowledge of the bomb would fall into the hands of the Russians. Berlin was in the danger zone. Mary Bohan had had no real idea of the purpose of the Alsos operations but she now suddenly realised the connection and had innumerable questions for Goudsmit.

President Truman received the message about Hiroshima while on the cruiser *Augusta* on his way home. The President was eating lunch with the crew when he received the first deciphered telegram, which read (quoted from Mee):

> HIROSHIMA BOMBED VISUALLY WITH ONLY ONE TENTH [cloud] COVER AT 052315A*. THERE WAS NO FIGHTER OPPOSITION AND NO FLAK. PARSONS REPORTS 15 MINUTES AFTER DROP AS FOLLOWS:
>
> 'RESULTS CLEAR-CUT SUCCESSFUL IN ALL RESPECTS. VISIBLE EFFECTS GREATER THAN IN ANY TEST. CONDITIONS NORMAL IN AIRCRAFT FOLLOWING DELIVERY'.

Another telegram followed from Stimson:

> BIG BOMB DROPPED ON HIROSHIMA AUGUST 5 AT 7:15 P.M. WASHINGTON TIME.
>
> FIRST REPORTS INDICATE COMPLETE SUCCESS WHICH WAS EVEN MORE CONSPICUOUS THAN EARLIER TEST.

The President hastily left the table and went to Foreign Secretary Byrnes who was sitting at another table and showed him the telegram. 'It's time for us to get home,' he said. His behaviour puzzled the crew but he asked for their attention and explained that he had just heard that a terrifically effective weapon had been used on Japan, a bomb whose power corresponded to that of 20 000 tonnes of trinitrotoluene.

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* = 5th August at 23.15 Greenwich Mean Time = 19.15 Washington DC time = 08.15 Japanese time.

Truman then hurried to the officers' mess where he repeated the message. 'We won the gamble!' he said, and the officers hurrahed and applauded. Just then the news was also heard on the ship's radio and Truman found out that he had made a statement which was reads out. It was the previously prepared text which was worded (quoted from Knebel & Bailey):

> Sixteen hours ago an American airplane dropped one bomb on Hiroshima, an important Japanese army base. The bomb had more power than 20 000 tonnes of TNT. It had more than two thousand times the blast power of the British 'Grand Slam' which is the largest bomb yet used in the history of warfare.
>
> The Japanese began the war from the air at Pearl Harbour. They have been repaid many-fold. And the end is not yet...
>
> It is an atomic bomb. It is a harnessing of the basic powers of the universe. The force from which the sun draws its power has been loosened against those who brought the war to the Far East [...]
>
> It was to spare the Japanese people from total annihilation that the ultimatum of 26 July was issued in Potsdam. Their leader immediately rejected this ultimatum. If they do not now accept our terms, they may expect a rain of ruin from the air, the like of which has never been seen on this earth.

Tokyo's radio was the first to broadcast information about the bombing, but it was scant and misleading. It was *Tokyo Rose* who said in her smooth voice that Hiroshima had been attacked by three bombs. A few hours later, a radio message came stating that the train connections with Hiroshima had been temporarily interrupted.

On the morning of 6 August, it was found to be impossible to gain contact with Hiroshima. The railway's telegraph connection was broken just north of the city, and from the stations that it was possible to contact it was said that there had been powerful explosions in Hiroshima. Tokyo's morning paper *Asahi* ('Morning Sun') received through different channels the message that Hiroshima had been almost completely destroyed. The military headquarters in Tokyo attempted to contact the communication centre in the fortress but did not succeed. Finally, at 13.00, a short but drastic message came from a naval station outside Hiroshima: 'Hiroshima has been annihilated by a single bomb and the fire is spreading.'

In Tokyo, the government's information and censoring body called representatives of the five biggest daily newspapers and the biggest news bureau to an information meeting. One press officer said (according to Knebel & Bailey):

> We believe that the bomb that was dropped on Hiroshima is different from an ordinary one. However, we have inadequate information now, and we intend to make some announcement when proper information has been obtained. Until we issue such an announcement, run the news in an obscure place in your papers and as one no different from one reporting on an ordinary air raid on a city.

The radio message on Hiroshima in the evening was therefore brief:

> A few B-29s hit Hiroshima city at 8:20 A.M. August 6, and fled after dropping incendiaries and bombs, the extent of the damage is now under survey.

And on 7 August, the morning paper *Asahi* had just one small announcement which read (quoted from Knebel & Bailey):

> HIROSHIMA ATTACKED BY INCENDIARY BOMBS
> Hiroshima was attacked August 6th by two B-29 planes,
> which dropped incendiary bombs.
> The planes invaded the city around 7:50 a.m. It seems
> that some damage was caused to the city and its vicinity.

On the morning of 7 August, the news media heard the American radio broadcasts containing President Truman's acknowledgement of the atomic bomb. Not that many people knew what an atomic bomb was and the Generals who were in a crisis meeting with the government fully rejected a proposal from Foreign Minister Tojo to review the terms of the Potsdam Proclamation. Truman's statement about an atomic bomb ought to be kept from the Japanese people until there had been time to examine the case in more detail.

An initial step in such an examination, nuclear physicist Yoshio Nishina was asked to accompany an army officer in a small aircraft to Hiroshima. Once he had studied Truman's statement, Nishina was sure that it really was a matter of an atomic bomb. The statement that the power corresponded to 20 000 tonnes of TNT tallied with his own calculations. The only thing that surprised him was that the Americans had succeeded in producing an atomic bomb in such a short time.

At 15.30 on 7 August, the military leadership reluctantly issued a communication, although this still did not use the words 'atomic bomb'. It said that 'significant injuries' had been caused by a 'new type of bomb'.

In the English farm, called Farm Hall, the captured German scientists heard the news. They did not believe it at first. They were convinced that they themselves knew more than anyone else and that an atomic bomb would be impossible to produce in the time the Americans had had available. Heisenberg thought that Goudsmit had deceived him by not talking about the bomb when Heisenberg had been interviewed. Gerlach saw himself as a defeated General and was very upset. But the person who was hurt the most was Otto Hahn who felt responsible for the fact that so many people had died as a consequence of the discovery he had made. His colleagues gave him gin to calm his nerves and watched over him until two o'clock in the morning.

In Japan on 7 August, Foreign Secretary Tojo spent two and a half hours talking to Secretary of War Anami. Anami admitted for the first time that Japan could not avoid losing the war. When the government met, the military Chiefs of Staff refused to believe that it was an atomic bomb that had been used on Hiroshima.

On Guam and Tinian, preparations began for the use of the plutonium bomb, *Fat Man*, the only additional atomic bomb that was available. LeMay asked Colonel Tibbets if he also wanted to lead this attack. Tibbets declined. He had had enough publicity. The others had worked long and hard and could do the job just as well, he said.

On 7 August, the plutonium bomb was in the process of being assembled in a purpose-built, air-conditioned assembly hall. There was already a similar bomb there – except for the plutonium – that had been assembled on 5 August for practice purposes. According to the plans, the right bomb would be dropped on 11 August but Tibbets convinced Parsons to bring the attack forward to 9 August since the weather was expected to become unsuitable later on. The decision brought with it practical problems - the bomb was not yet fully assembled.

The person who had to deal with the problems was a young Second Lieutenant by the name of *Bernard O'Keefe* who toiled to finish coupling the final cables before the outer jacket of the bombs was assembled. To his horror, he found that it was not possible to connect an important cable to the intended contact. This was because there were female contacts at both points - someone had assembled the cable incorrectly. Replacing the cable would mean disassembling the implosion sphere, which would mean delay by one day so there might not be time to use the bomb before the weather deteriorated.

O'Keefe took the risk of taking the matter into his own hands without reporting the problem. There were no electrical sockets in the assembly hall, so he searched for a couple of long leads that could be connected outside so he could work on the bomb with a soldering iron, against all the rules. So, he unsoldered the cable's female contact and replaced it with a male contact and the situation was saved - just one example of the many unforeseen practical problems that could crop up and jeopardise the plans.

On the morning of 8 August, Tojo deliberated with Prime Minister Suzuki and then asked to meet the Emperor. In a shelter beneath the Emperor's Palace he described the atomic bomb to the Emperor and said that it offered an excellent excuse to terminate the war. The Emperor agreed with Tojo and said that it was important not to lose this opportunity by trying to negotiate better surrender terms. He asked Tojo to inform Suzuki of his decision. Tojo also asked Suzuki to call the Supreme War Council to a meeting on 9 August. However, Molotov had already notified Sato in Moscow on 8 August that the Soviet Union would consider itself to be at war with Japan on 9 August. And Russian troops entered Manchuria on 9 August.

The fact that Russia had declared war was not known in Tokyo until the morning of 9 August. Tojo visited Prime Minister Suzuki in the morning and told him about the Russian attack and stressed that the war must end immediately. Suzuki agreed with this. The Supreme War Council met at 11.00 on the morning of 9 August. This was the time when the plutonium bomb fell on Nagasaki.

President Truman had returned to Washington DC on the previous day. He adhered to his view that another bomb needed to be used and this view was shared by General Groves and Admiral Purnell in the President's policy group. And so the preparations continued according to their plans.

The intended bomb target was actually Kokura, a city of military importance with what was perhaps Japan's largest war industry at the Shimonoseki Straits between the islands of Kyushu (on which Nagasaki lies) and Honshu, Japan's main island. Next to Kokura were the cities of Tobata, Wakamatsu, Yawata and Moji, which in 1963 would end up being merged Kokura so that the five cities would now form the new industrial city of Kitakyushu.

Nagasaki lies on the other side of Kyushu, approximately 150 km to the south-south-west. This was the secondary target if the weather would not permit the bombing of Kokura. Nagasaki had big naval dockyards and industries for the production of weapons and ammunition.

On Tinian, the task of leading the attack on Kokura had fallen to Major Charles Sweeney, who had piloted *The Great Artiste* in the attack on Hiroshima. However, Sweeney now chose another plane, called *Bock's Car** as it was normally flown by Captain *Frederick Bock*. This time, Bock himself flew *The Great Artiste*, which now also had the task of making observations and taking measurements. A third plane, *Big Stink*, flown by Captain *James Hopkins*, brought camera equipment but also a few observers, including two Englishmen, one of whom was none other than William Penney, and the authorised journalist William Laurence who was allowed to go with them this time.

The bomb was loaded into *Bock's Car* on 8 August. As had been the case with *Little Boy*, it was not possible to finish the assembly in the plane after starting up. The bomb was therefore adapted from the start. Groves thought that there was a very small risk of a start-up accident but the ground personnel on Tinian were worried by the possibility. They demanded a written assurance that the start would be completely safe in spite of the loaded bomb. Captain Parsons signed such an assurance and silenced his conscience with the fact that he would scarcely be alive to be held responsible if something were to happen after all.

At 03.47 on Thursday 9 August, *Bock's Car* left Tinian just before the accompanying plane to fly to Kokura. The person responsible for the bomb on this occasion was not Parsons but Commander *Frederick Ashworth*, who has recounted the start (quoted from Rhodes):

> The night of our takeoff was one of tropical rain squalls, and flashes of lightning stabbed into the darkness with disconcerting regularity. The weather forecast told us of storms all the way from the Marianas to the Empire. Our rendezvous was to be off the southeast coast of Kyushu, some fifteen hundred miles away. There we were to join with our two companion observation B-29s that took off a few minutes behind us.

So, the weather did not provide the best conditions for the assignment. To top it all, before the start, a fault had just been found on the fuel pump that was there to provide an extra 3 000 litres of fuel from a reserve tank if necessary. Therefore, in order to save fuel, Sweeney did not fly over Iwo Jima as had first been planned but straight to the island of Yokushima off the Japanese coast where he would meet the other two planes.

Sweeney found only one of the expected planes, *The Great Artiste,* on Yokushima and circled in vain for half an hour in the hope of also finding *Big Stink*. This meant that costly time and fuel were lost here. The weather plane that had flown in over Kokura in advance reported that the weather would permit visual bombing but when *Bock's Car* arrived, the weather had deteriorated. Sweeney made three flights over the city and lost another 45 minutes without being able to drop the bomb. He consulted Ashworth

* The name, as it was painted on the aircraft, was actually BOCKSCAR, word play on the American word "boxcar", which means goods waggon.

and decided to continue to the secondary target, Nagasaki. They no longer thought they could return to Tinian; they only just had enough fuel to be able to fly over Nagasaki once and then continue to Okinawa which was closer than Tinian. If it was necessary to loiter over Nagasaki, they would be forced to do an emergency landing on the sea and hope that a submarine would save them.

It was as cloudy over Nagasaki as it was over Kokura. They had an order to drop the bomb visually but, bearing in mind the shortage of fuel, Sweeney and Ashworth agreed to contravene the order and fly in to drop the bomb with the help of the radar. A gap in the cloud enabled them to glimpse a racetrack north of the city for orientation and the bomb was dropped.

It exploded at an altitude of 500 metres above the northern outskirts of Nagasaki at 11.02 on the morning of Thursday 9 August 1945, right over two Mitsubishi weapon factories a few kilometres from the planned point. This and the hilly terrain made that the damage was considerably less than in Hiroshima, despite the fact that *Fat Man* was considerably more powerful than *Little Boy*. The power of the bomb was estimated to correspond to 22 000 tonnes of TNT. 20 000 tonnes had been the estimate for the Hiroshima bomb, but later estimates have given a considerably lower value, approximately 13 000 tonnes.

The information on the number killed in Nagasaki varies, as does the information for Hiroshima. The first official American estimate of 35 000-40 000 dead and 60 000 injured is probably an underestimate, Rhodes states that there were 70 000 deaths in 1945. The suffering of the population was no doubt just as appalling as in Hiroshima but, since it was not first in line, Nagasaki has since usually been dismissed with the addition '... and Nagasaki'.

Some of the dreadful circumstances were communicated in a letter written by an American marine officer to his wife one month after the bombing (quoted from Rhodes):

> A smell of death and corruption pervades the place, ranging from the ordinary carrion smell to somewhat subtler stenches with strong overtones of ammonia (decomposing nitrogenous matter, I suppose). The general impression, which transcends those derived from the evidence of our physical senses, is one of deadness, the absolute essence of death in the sense of finality without hope of resurrection. And all this is not localized. It's everywhere, and nothing has escaped its touch. In most ruined cities you can bury the dead, clean up the rubble, rebuild the houses and have a living city again. One feels that is not so here. Like the ancient Sodom and Gomorrah, its site has been sown with salt and ichabod[*] is written over its gates.

Bock's Car continued directly to Okinawa where the practically empty tanks were filled. It then continued to Tinian where the plane landed five minutes before midnight. The reception was less solemn than when *Enola Gay* had landed. Tibbets officially commended Sweeney and his crew for the assignment carried out but said privately that if a third bomb were to come into the question, he himself wanted to lead the attack. The bombing of Nagasaki had not gone completely according to plan.

Big Stink, which had missed the rendezvous over Yokushima, was 150 kilometres away at the time of the explosion but then flew over Nagasaki but without being able to see anything as it was still too cloudy. The extent of the damage could not be seen until one week later - 44 % of the city had been destroyed.

In Tokyo, the Supreme War Council had just begun its crisis meeting when the bomb fell on Nagasaki. The military members were still unwilling to find a peaceful solution. Secretary of War Anami, who had previously admitted that the war was lost, thought that they ought to continue fighting anyway. The Council of War closed the meeting at 13.00 without having reached any agreement. Prime Minister Suzuki then summoned the government to a meeting. According to Toga, this began at 14.00.

Suzuki told the government that it ought to accept the Potsdam Proclamation but he was unable to accomplish an agreement. The discord was not about whether they ought to surrender but about which terms they ought to demand in order to do so. There was one term that everyone agreed on - the

[*] Ichabod means "The glory is departed" (1 Sam. 4:21).

Emperor's position must not be compromised since it was of divine origin. However, several ministers, primarily Anami, wanted to add several terms regarding occupation, disarmament and legal proceedings against war criminals. Foreign Secretary Tojo and Marine Minister Yonai replied that they were not exactly in a position to negotiate and that it was highly unlikely that the Americans would accept any terms from the Japanese. Anami said that it was possible to stand their ground for a while on Japanese soil and that they could then 'find life out of death'. They failed to agree and it was now late in the evening. The Prime Minister then finally said that he wanted to give the Emperor a report along with Tojo. Suzuki and Tojo then left the cabinet meeting where the discussion continued.

At the Emperor's Palace, Suzuki asked Tojo to tell the Emperor what had been said at the cabinet meeting. He then asked the Emperor to call the Supreme War Council to a meeting in the Emperor's presence. The Emperor agreed to this and the Supreme War Council was called to the palace for a meeting at midnight on the night between the 9th and 10th August.

At this meeting, the Prime Minister asked the Emperor to listen to the two proposals being put forward, one of which was to surrender unconditionally with the exception of the Emperor's position and the other being to set additional terms for surrendering. The proposals were then described by Tojo and Anami. Privy Council Baron *Kiichirō Hiranuma* suggested a wording of the interpretation of the Potsdam Proclamation, i.e., the observation that this 'comprised no demand which would prejudice the prerogatives of the Emperor as a sovereign ruler', and this proposal was accepted.

Suzuki then said that he was unfortunately compelled with the greatest of subservience to ask for the Emperor's decision. The Emperor's calm response was that he supported Foreign Minister Tojo's proposal. The Emperor's conference then ended at 02.30 in the morning of 10 August. The government was then called to a meeting at 03.00 and a unanimous decision was made in accordance with the Emperor's wish. Tojo quickly wrote a telegram to the Allies based on the decision. The telegram was sent to the Japanese Minister in Berne at 07.00 to be forwarded to the United States and China. An equivalent telegram was simultaneously sent to the Embassy in Stockholm to be sent by the Swedish government to the United Kingdom and the Soviet Union.

Tojo's telegram came to Washington DC on the Friday morning of 10 August. The Americans found that the Japanese accepted the Potsdam Proclamation but with the important exemption that it 'does not comprise any demand which prejudices the prerogatives of His Majesty as a sovereign ruler'. President Truman convened his advisers, including Byrnes and Stimson. The latter thought that the President ought to accept the peace offer since 'taking a good plain-horse sense position that the question of the Emperor was a minor matter compared with delaying a victory in the war which was now in our hands'. Byrnes protested and thought that there was no reason to back out of the agreement in Potsdam which was written before the United States had the atomic bomb and before the Soviet Union had entered the war.

In the end, a compromise was agreed and a response which could interpreted in several different ways. The response was formulated by Byrnes (quoted from Tojo):

> With regard to the Japanese Government's message accepting the terms of the Potsdam proclamation but containing the statement, 'with the understanding that the said declaration does not comprise any demand which prejudices the prerogatives of His Majesty as a sovereign ruler,' our position is as follows:
>
> From the moment of surrender, the authority of the Emperor and the Japanese Government to rule the state shall be subject to the Supreme Commander of the Allied powers, who will take such steps as he deems proper to effectuate the surrender terms.
>
> The Emperor will be required to authorize and ensure the signature by the Government of Japan and the Japanese Imperial General Headquarters of the surrender terms necessary to carry out the provisions of the Potsdam Proclamation, and shall issue his commands to all the Japanese military, naval and air authorities and to all the forces under their control wherever located to cease active operations and to surrender their arms, and to issue such other orders as the Supreme Commander may require to give effect to the surrender terms.
>
> Immediately upon the surrender the Japanese Government shall transport prisoners of war and civilian internees to places of safety, as directed, where they can quickly be placed aboard Allied transports.

> The ultimate form of the Government of Japan shall, in accordance with the Potsdam Proclamation, be established by the freely expressed will of the Japanese people.
>
> The armed forces of the Allied Powers will remain in Japan until the purposes set forth in the Potsdam Declaration are achieved.

Byrnes waited until the next day, 11 August, to send the response to Japan via Switzerland. During this time there were divided opinions as to the use of a third atomic bomb. In the morning of 10 August, General Groves had informed Marshall that another plutonium bomb could be sent to Tinian on 12 or 13 August. That same day, General Spaatz suggested that they ought to drop an atomic bomb on Tokyo for ‘the psychological effect on government officials’.

However, President Truman had now decided to use no more atomic bombs. He said that the thought of destroying another 100 000 people was too terrible.

Byrnes’ response to the Japanese reached Tokyo just after midnight on Sunday 12 August. Foreign Secretary Tojo informed the Prime Minister and then went to the Emperor’s Palace to give the Emperor a report. The Emperor said that he thought the Allies’ response was satisfactory and that they ought to accept it as it stood and asked Tojo to tell the Prime Minister.

Suzuki called the government to a meeting at 5.00 to discuss the American response. Tojo went through the text point by point. He thought that everything was acceptable but admitted that the paragraph which said that the future government should be set up according to the people’s wishes could lead to problems. However, he thought that it would be futile to try to negotiate a change. Several government members were still unwilling to place Japan’s government in the hands of the people - this was verging on a lese-majesty since it would restrict the Emperor’s divine authority.

The discussion continued and the cabinet meeting was adjourned until the following day, Monday 13 August, when the government reconvened at 6.00. The discord continued, as did the argumentation between Tojo and Anami. There was simultaneously a rumour of rebellion among the military in favour of continuing the war.

In the meantime, those who were in Washington DC were becoming impatient. On 13 August, General Groves received an enquiry concerning ‘the availability of your patients together with the time estimate that they could be moved and placed’. Stimson recommended that a third bomb be shipped to Tinian.

However, atomic bombs were not needed to cause extensive damage. Truman ordered General Arnold to carry out an attack using ordinary bombs. Arnold placed all available bombs in the Pacific Ocean area. On 13 August, more than one thousand aircraft attacked Japan with five thousand tonnes of anti-personnel bombs and incendiary bombs.

The cabinet meeting in Tokyo continued. The Secretary of War appeared to be growing tired of the discussion but Minister of the Interior and several others besides continued that claim that a number changes ought to the terms set by the Allies to be requested. Tojo describes the situation:

> I answered that, judging from the Allied Powers’ situation, further approaches to them by us not only would be futile but would lead them to doubt the genuineness of our intention to make peace. Byrnes’ reply made to us unquestionably represented the least common denominator of the terms of the several Allies, and it was imperative that we accept them as they now stood, if we were to bring about peace for the sake of reconstruction of Japan and the welfare of the human race. Navy Minister Yonai, as usual, spoke in agreement with me, but there were still a few dissenting. The premier then polled the cabinet.

Two of the Ministers abstained, ten voted for Tojo’s proposal to accept the Potsdam Proclamation as it stood but three voted against it: the Minister of War, the Minister of Justice and Minister of the Interior. Tojo did not believe that Secretary of War Anami would attempt a coup d’état but there was a risk that military officers could cause riots. Tojo therefore appealed to the Prime Minister to make a decision and Suzuki’s response was that he would ask the Emperor to decide the matter the following day. Tojo continues his account:

> I pondered, in the car returning home, that even if we offered the sacrifice of twenty million Japanese lives, they would but fall easy prey to machines and gunfire. We could bear anything, if it promised a return; the arrows and bamboo spears of which the military men were prating promised none. The soldiers' ignorance of the nature of modern warfare was beyond my understanding. In any event, no further delay could be tolerated, and it was, I thought, absolutely necessary that a final decision be reached on the morrow, as planned by the Premier.

On the morning of 14 August, Tojo visited the Prime Minister. An extraordinary cabinet meeting had been decided and would take place in the Emperor's presence. Suzuki drew Tojo aside and said that he had also summoned the highest military leadership. His intention was to ask the few who now had differing opinions to defend this before the Emperor. He therefore asked the chiefs of the Army and Navy and Secretary of War Anami to put forward their views. The Emperor listened and then spoke (quoted from Tojo):

> It was not lightly, but upon mature consideration of conditions within and without the land, and especially of the development taken by the war, that I previously determined to accept the Potsdam Declaration. My determination is unaltered. I have heard the disputation over the recent reply given by the Allied Powers, but I consider that in general they have confirmed our understanding. As to paragraph 5[*] of the declaration, I agree with the Foreign Minister that it is not intended to subvert the national polity of Japan; but, unless the war be brought to an end at this moment, I fear that the national policy will be destroyed, and the nation annihilated. It is therefore my wish that we bear the unbearable and accept the Allied reply, thus to preserve the state as a state and spare my subjects further suffering. I wish you all to act in that intention. The War and Navy Ministers have told me that there is opposition within Army and Navy; I desire that the services also be made to comprehend my wishes.

Tojo writes in his memoirs:

> All the attendants wept at these reasoned and gracious words, and at conceiving the Emperor's emotions. It was an inexpressibly solemn and moving scene; as we retired down the long corridor, while returning in our cars, and at the resumed cabinet meeting, each of us in his thoughts wept again.

The government's work with drawing up the imperial rescript took all day and it was handed to the Emperor in the evening. At 23.00 on Tuesday 14 August 1945, the rescript was announced and sent with the help of the Swiss and Swedish governments to the United States, the United Kingdom, the Soviet Union and China. Overnight, a group of the empirical palace guard tried to get hold of and destroy the phonogram made of the Emperor's rescript and that was intended to be broadcast over the radio the following day. The undertaking failed following a bloody altercation. In the morning, it was announced that Secretary of War Anami had taken his life and that many had followed his example.

The text that was sent to the Allies and that the Emperor read out over the radio on 15 August made no demands for changes to the Allies' terms. On the contrary, 'hopes' and 'desires' were expressed and the suggestion was that the most effective way of putting an end to the conflicts would be an order from the Emperor. Such an imperial order was issued at 12 on 16 August. The Allies were informed that, because communications had been ruined, it might take two days for the order to reach all troops in Japan, six days for Manchuria and China, and twelve days for New Guinea and The Philippines.

Immediately following the announcement of the Emperor's decision Suzuki's government resigned. The Allies sentenced Baron Suzuki to life imprisonment in 1948 and Foreign Secretary Tojo to twenty years' imprisonment. Both died early while serving their sentences. Emperor Hirohito renounced his

[*] The Emperor referred to the fifth paragraph of Byrnes' ultimatum. The corresponding paragraph in the Potsdam Proclamation is the twelfth.

divine status but continued as Emperor, although with pretty restricted authorisations. He did not die until 1989.

On 2 September 1945, Japan's surrender was signed on the American battleship *Missouri* which was anchored in Tokyo Bay. The surrender document was signed for Japan by its new Foreign Secretary *Mamoru Shigemitsu* (1887-1957) and Chief of Army Staff *Yoshijirō Umezu* and for the Allies by the Supreme Commander of the Armed Forces for their forces, General Douglas MacArthur, and representatives of the four States: Admiral *Chester Nimitz* (1885-1966) for the United States, Admiral *Bruce Fraser* (1888-1981) for the United Kingdom, and two lesser-known Generals for China and the Soviet Union. The Second World War was definitely over.

The use of the atomic bombs on Hiroshima and Nagasaki has led to many discussions. Why were the bombs used? Was it necessary? Was it morally defensible?

The word 'moral' can be seen as the level of concordance between the actual action and the ethic that is advocated. We sometimes judge historical documents as immoral when they deviate from the ethic we consider reasonable today. However, we should be careful when using the 'immoral' label for actions that were carried out a long time ago in situations that are difficult to assess, where the ethic may have been different or in any case not as clear-cut as when we view the event with hindsight under completely different circumstances.

The long-lasting discussions on 'right' answers to the three questions listed were summarised in 1996 in a paper sprinkled with references, *The Decision to Use the Bomb* by Samuel Walker, who made a critical assessment of the views put forward. The flora of views and opinions is too wide-ranging for me to be able to show them here, but it is important to reproduce some opinions.

In 1945, the atomic bomb was often considered by politicians and the military to be one weapon among many, certainly of substantial interest due to its immense power but still one in an arsenal of tactical weapons. Its primary benefit was the one pointed out by Kistiakowsky: the effectiveness of the bomb. You could achieve the same level of destruction with only one bomber compared with before using a thousand planes. Many military personnel did not see the atomic bomb as a means of *greater* destruction but as a more efficient means of the same destruction.

The destruction that the atomic bomb could cause was very substantial (and thousands of ordinary bombs would have been needed to cause); this change in the concept of speed was something that people were not able to react quickly to in 1945. There was a noticeable change to the bombing strategy during the Second World War. Military plants were initially precision-bombed (as was said would be done later during the Gulf War). But at the end of the war people had started terror bombing the civilian population in large cities. Hitler started terror bombing as early as 1940-1941 with the indiscriminate bombing of London and Coventry in the bomb offensive known as the Blitz, which killed approximately 40 000 people. Hamburg was bombed to pieces by the Allies in July-August 1943 with 45 000 fatalities, most of them during the firestorm that broke out after the bombing of 27 July. Although Dresden had been declared an 'open city', the city was turned to ruins during a British-American bomb attack on 13-15 February 1945 and maybe 100 000 inhabitants were killed. Tokyo was bombed by the Americans on the night between 9 and 10 March 1945, when one million of the inhabitants were injured and more than 100 000 killed. It was said that never before had so many people lost their lives in such a short time. More may have died in Dresden but not as quickly.

Terror bombing had thus become a habit and had led to a certain level of desensitisation.[*] However, when President Truman talked about the use of the atomic bomb, it was not terror bombing he wanted to recommend, and even Stimson spoke about the bombing of 'military plants'. Practically speaking, Truman had also come into the atomic bomb plans too late to be able to control them. The main responsibility lay with the Generals; in reality, the only thing that Truman could have done was to say no and he could certainly see no real reason to do so.

[*] Peter Englund describes in his book *Brev från nollpunkten* [Letters from Ground Zero] how the head of the British bombers, *Arthur Harris* (1892-1984), was stopped by a member of the traffic police when he was driving his black Bentley too fast. The police constable reproached him and said that he was putting other lives at risk. Harris' irritated response was: 'Young man, I kill thousands of people every night!'

Some think that General Groves was affected by the fact that he had been responsible for the development of the bomb and may therefore have liked to have seen it put to use - so much hard work and so much money had gone into the project. The decision would therefore have been bureaucratic rather than political.

The official reason for the bombing of Hiroshima and Nagasaki, the reason that was primarily given by Secretary of War Stimson, was that the bombing would shorten the war and render an invasion unnecessary, an invasion which, according to Stimson, would have claimed the lives of 500 000 American soldiers. Stimson was supported by Conant, who according to one critic was thought to have had ‘an intense personal sensitivity over how history would judge his role [in the atomic bomb project]’. However, many think that the Japanese would soon have surrendered anyway and that an invasion never would have been needed. Well-founded estimates of the number of human lives that an invasion really would have cost indicates 50 000 rather than 500 000.

A number of critics maintain that Truman’s main motive was political rather than military. According to them, he would have used the bomb to end the war before the Soviet Union had got around to attacking Japan and thereby been able to claim power in the Pacific Ocean area. But Truman’s diary notes contradict this somewhat. After Stalin had promised in Potsdam to attack Japan, Truman wrote: ‘Fini Japs when it happens’. And to his wife he wrote: ‘I’ve gotten what I came for - Stalin goes to war August 15th with no strings on it [...] I’ll say that we'll end the war a year sooner now, and think of the kids who won’t be killed!’ But in his diary he wrote: ‘Believe Japs will fold up before Russia comes in. I am sure they will when Manhattan appears over their homeland’.

Several critics point out that maybe the simplest possibility of ending the war without using an atomic bomb would have been to tone down the demands for unconditional surrender. Had Byrnes given an assurance regarding the Emperor’s position, the Japanese would not have protracted their surrender. According to one critic, not doing so was ‘the most tragic blunder in the American surrender policy’.

Had the bomb not been needed to save half a million American lives or to put a stop to the war, would it have been morally defensible? As regards the previous terror bombings, you might conclude that it was not the atomic bomb but the overall *war* that was morally indefensible. Stimson wrote: ‘In this last great action of the Second World War we were given final proof that war is death ...’. The war as a conciliation between gentlemen was no longer. Admiral Leahy, who never tired of the bomb, wrote in anger (quoted from Knebel & Bailey):

> [...] The use of this barbaric weapon at Hiroshima and Nagasaki was of no material assistance in our war against Japan. The Japanese were already defeated and ready to surrender because of the effective sea blockade and the successful bombing with conventional weapons. [...]
>
> My own feeling was that in being the first to use it, we had adopted an ethical standard common to the barbarians of the Dark Ages. I was not taught to make war in that fashion, and wars cannot be won by destroying women and children.

Apart from morals and ethics and in addition to the tragedy and suffering caused by the bombs on Hiroshima and Nagasaki, they have had two essential side effects. The publicity of the abominable effects that the bomb has had throughout the world has led it to be countered with such abhorrence that no government has dared to use it since. Military personnel who have taken a more materialistic view of the case and have been pleased by the effectiveness of the weapon have come close to using atomic bombs on a number of occasions. Luckily, the governments of countries with nuclear weapons have realised the possible consequences and said no in time, doubtless also influenced by the insight into the destruction power of the thermonuclear weapons, ‘the hydrogen bombs’, corresponding to thousands of Hiroshima bombs.

The other side effect is the impact of the atomic bombs on people’s attitude to inflicted radiation sources. The bombs in Hiroshima and Nagasaki killed and maimed primarily through heat radiation and shock waves. But the factors that the people recall are associated with the relative few who were affected by late radiation injuries.

We forget the tens of thousands of people who died on the day the bombs were dropped, those who were turned to charcoal or crushed by falling blocks and bricks, those who fruitlessly braved the firestorm in the attempt to drag themselves away from the raging fires, all the contorted bodies, disfigured by blood clots and singed flesh; all those who died of their severe injuries for days and weeks afterwards with no opportunity for care or treatment. Instead, we appear to remember some of the hundred or so children who were affected by leukaemia a few years later.

15. THE INFANCY OF ATOMIC ENERGY

WHEN THE NEWS OF the atomic bomb on Hiroshima reached Sweden, it came as a total surprise. The other surprise was the interest that foreign news agencies showed in the flight of Lise Meitner from Austria. The world press drew attention to her with big headlines: ‘Mother of the atomic bomb’, ‘The fleeing Jewess’, etc. Only in Sweden (as is still the case today) did people appear to be unaware of just what a great physicist they had in their country. From abroad came enquiries, telegrams, telephone calls....

At the start of August 1945, the subject of this attention was enjoying the Swedish summer in the Lake Siljan area. As in Christmas 1938, when the big discovery was made, Meitner had booked into a small guest house in Kungälv to spend time with a Swedish friend, she now lived in a small hotel in Leksand to be able to meet her friends Magnhild and Gudmund Borelius who were in the habit of spending their summers in Hjortnäs, not far away.

However, Lise Meitner did not get to enjoy the summer in peace and quiet. The first to get in touch, on the morning of 7 August, was a reporter from *Expressen*. Her conversation with him led to the headline ‘FLEEING JEW’ that same day. Later in the day it was *Falukuriren’s* turn, which on 8 August resulted in an article with the headline ‘Exiled female physicist one of the atomic bomb’s pioneers’.

‘A lot of nonsense will certainly be printed – everyone I talked to understood nothing about it’, she confided in her diary. And that was the exact situation. The reports in the Swedish press on the atomic bomb did not succeed in explaining what had really happened. The exception was the well-informed article in *Falukuriren*; but Lise Meitner had contributed to it herself.

Increasing numbers of reporters flocked to her door, and when she assured them that she knew nothing about the bomb they lost no time in making up dramatic stories for themselves. Many of these were based on what William Laurence had written in the *Saturday Evening Post* in 1940. Unlike his first article in the *New York Times*, the article was more dramatic and gave a picture of Lise Meitner fleeing from Berlin with the atomic bomb secret.

In the evening of 9 August, the same day that the bomb fell on Nagasaki, an unwilling Lise Meitner was interviewed in Leksand over the radio by Eleanor Roosevelt in New York. A couple of days later, an interview was arranged for her at another radio station in New York to defend Otto Hahn against accusations that he was a Nazi. As for Otto Hahn himself, he could neither be interviewed nor defend himself against accusations; he was hidden away with his colleagues at Farm Hall in England.

Lise Meitner never broke off her friendship with Otto Hahn. She wrote him lengthy and often critical letters in which she accused him of being too sympathetic towards the passivity of the German scientists. Hahn actually seemed to gradually forget or repress Lise Meitner’s input. He rarely mentioned her, and when he did so he gave the impression that Meitner had almost been his assistant and not the independent colleague who had been trusted to create and lead the Institute for Physics within the Kaiser Wilhelm Institute for Chemistry and who had explained what Hahn had not dared to believe about nuclear fission. It went as far as Lise Meitner’s workbench with instruments for taking physical measurements being displayed at *Deutsches Museum* in Munich under the label ‘Otto Hahn’s workbench’.

It may have been Hahn’s unavailability at Farm Hall – where no journalist knew he was staying – which focused all the attention on Lise Meitner and aroused unwarranted suspicions that Hahn was a Nazi and had invented nuclear fission on behalf of Hitler. Nor could it be said that there was any other physicist in Sweden who could answer the journalists’ questions since the atomic bomb had come as a complete surprise. It is possible that all the attention directed at Lise Meitner further increased the irritation that Manne Siegbahn may have felt about the alien bird that had flown to his institute. It is also possible that it affected his attitude towards Meitner when, in December 1945, the Academy of Sciences awarded the 1944 Nobel Prize in Chemistry to Otto Hahn on his own with no equivalent Prize in physics to Lise Meitner. Hans Pettersson wrote an indignant letter to Meitner, stating that Siegbahn had opposed her (or, as he termed it, 'put a spoke in her wheel'). Even Klein and Borelius thought it was Siegbahn who had refused Meitner the Nobel Prize in Physics due to 'Swedish royal jealousy'. In 1944 and 1945, the Prize in Physics instead went to Isidor Isaac Rabi and Wolfgang Pauli, who were justifiable choices.

At Farm Hall in England, the news of the atomic bomb on Hiroshima was met with even greater surprise than in Sweden. The interned German scientists refused to believe the information at first. They had naively believed that they were way ahead of the Americans when it came to nuclear physics knowledge. As they were gradually convinced that the atomic bomb was real, they had difficulty understanding how the Americans had been able to produce it so quickly. Heisenberg, who considered himself, and was also considered by the others, to be the leading expert, was besieged with questions that he had difficulty answering. He felt humiliated at having made a mistake in front of his colleagues by stating a completely incorrect critical mass for uranium-235 (he eventually calculated the right one but the damage had been done by then). He was angry that Goudsmit had not told the truth during the Alsos trial when he answered Heisenberg's question by saying that the Americans had other things to do rather than research nuclear physics. If only Goudsmit had told him, he would have avoided embarrassment...

The account of the Germans' discussions to which the Englishmen had listened using hidden microphones makes for an interesting read. When Hahn and Bagge discussed neptunium and plutonium, which they called '93' and '94', Hahn wondered how it was possible to find out the properties of plutonium. Bagge answered that one simply needs to wait until the neptunium has disintegrated, and you then have pure plutonium. Hahn, who was used to thinking in activity units rather than mass units, said that there will be too little plutonium. 'You will get nothing of an element with a period of decay of 10,000 years through the disintegration of a 2.3-day element.' bagge, on the other hand, thought that an equal number of atoms of the two substances would be enough.

The discussions gave birth to the version ('Lesart') according to which the German scientists tried to keep the bomb from Hitler for ethical reasons. This was maintained primarily by von Weizsäcker, who said that: 'History will record that the Americans and the English made a bomb, and that at the same time the Germans, under the Hitler regime, produced a workable engine [reactor]. In other words, the peaceful development of the uranium engine was made in Germany under the Hitler regime, whereas the Americans and the English developed this ghastly weapon of war.' von Laue reacted strongly to this. Nothing indicated that the German scientists, including von Weizsäcker, had had any qualms during the first few years when they had a lead - unless there were any like those that led Heisenberg to make his controversial visit to Bohr.

The Swedish physicists showed little interest in nuclear physics; the interest in the atomic bomb was therefore primarily a military one. When the Smyth Report became available one month after the bombings of Hiroshima and Nagasaki, it was immediately noted by the newly-formed Swedish National Defence Research Institute (FOA) which had originated from Sievert's Military Physics Institute ('MFI') merged with the National Defence's chemical establishment.[*] As head of department 2 at the FOA, the head of the MFI Torsten Magnusson became responsible for the physical defence research in the field of nuclear energy.

Supreme Commander of the Armed Forces *Helge Jung* (1886-1978) immediately asked the FOA to obtain information on the atomic bomb. A record of a meeting of the FOA's board chaired by Director General of the Royal Telegraph Administration *Håkan Sterky* (1900-1992) on 17 August 1945 shows that the Supreme Commander's representative, Lieutenant Colonel Torsten Schmidt, requested 'that the

[*] The Smyth Report was translated into Swedish and published by Bonniers in 1946.

FOA prepare and submit to the Supreme Commander an account of what was currently known about the atomic bomb'.*

As early as September 1945, the then Ecclesiastical Minister Tage Erlander, who had studied science at university, received a letter from Bohr's colleague and friend Torsten Gustafson containing advice from Bohr that the Swedish State ought to safeguard the country's uranium assets. Up until that time, uranium had not been sufficiently valuable for a mining concession to be taken out, but belonged to the landowner. The Mining Act was then changed in favour of the State with regard to uranium.

Tage Erlander and *Ernst Wigforss* (1881-1977) suggested that an international research institute for peaceful nuclear energy be created on the Swedish coast opposite Copenhagen. The leader would be an internationally renowned physicist, and scientists from all countries would have free access. Niels Bohr was proposed as the chairman of the board but he declined for political reasons. Sweden was willing to invest 100 million Swedish kronor in the project. However, the cold war made the plans impossible. The United States did not want the free exchange of information which could accelerate development of a Russian atomic bomb. Sweden was reduced to focusing on national nuclear activities.

On 23 November 1945, a committee was set up and named The Atomic Committee at the proposal of its chairman, to find methods for the 'utilisation of atomic power'. The Directive had been formulated by the then Assistant Professor *Lamek Hulthén* (1909-1995) and did not mention the purposes for which atomic power would be used. The Atomic Committee included Torsten Gustafson, Manne Siegbahn, Hannes Alfven and Asea's former technical manager *Ragnar Liljeblad* (1885-1967). The Svedberg came later. The chairman of the Committee was County Governor *Malte Jacobsson* (1885-1966). The Atomic Committee was initially to be an investigative and advisory committee, but it was soon given administrative tasks concerning the organisation of the research and had research funds at its disposal.

The Committee allocated substantial amounts to Karolinska Institutet for the analysis of radiation from atomic bombs. It was primarily Sievert's institute that received this support, and it was with the help of these funds that Sievert was able to start a number of projects such as the high-voltage hall for research into lightening-speed radiation, stations to measure radiation in different parts of the country and to measure natural background radiation, and for biologist Arne Forssberg's research into protection substances.

The Atomic Committee had no formal responsibility for research into nuclear weapons, but half of its members were also on the board of the FOA where the Director General of the Telegraph Administration, Håkan Sterky, was chairman. This personal link to the FOA meant that it was natural to give some research assignments to the FOA, ones that were also in the FOA's interests, such as research into uranium production and reactor construction. However, The Atomic Committee had sufficient power to avoid being controlled by the FOA; for example, the Committee refused to accept demands from the Supreme Commander of the Armed Forces regarding the secrecy of many of the operations.

The Svedberg thought that keeping things secret 'would have disastrous impacts on our scientific connections with the USA in atomic energy matters'.

In the doctoral thesis called *Hela nations tacksamhet* [The Entire Nation's Gratitude] (1991), Stefan Lindström writes:

> However, one must be careful about drawing any simple and too far-reaching conclusions regarding the unclear relation between civilian and military operations. It could be easy to think of the scientists as being linked to the national defence and as a part of a more or less secret operation. The ambiguity could also be taken as income for the militarisation of research. This may be right in some respects. However, the question is whether or not it is more reasonable to interpret the situation as being that it was a limited group of leading scientists who during this period succeeded in obtaining a

* Schmidt had been head of secretariat of the research committee set up by the Defence in 1943, of which Siegbahn was chairman. The committee's task was to investigate the future organisation of defence research and it proposed the formation of the FOA from the MFI and the National Defence's chemical establishment. The committee had also proposed a research department for the Defence Staff but this proposal was reduced in the government bill to a field officer's position as research officer with the post of head of department. This was the job that was given to Schmidt.

> considerable influence over both the civilian and the military research and that they used this influence to strengthen their own activities. The measures proposed by The Atomic Committee indicate this in any case.

Initially, it was a question of looking at the conditions as regards material and knowledge for producing Swedish nuclear weapons, but also to examine protection against the effects of the bomb. In December 1945, Sievert and Benner issued a report on shelters and the capacity of tanks to protect against atomic bomb radiation. Sievert gave an account of which types of radiation injury could be anticipated. He maintained that gamma and neutron radiation from an atomic bomb were emitted for a very short time at high intensity and that it ought to be investigated as to whether this made bomb radiation particularly hazardous.

In March 1946, The Atomic Committee issued its first report, written by its secretary, lecturer *Gösta Funke* (1906-1991). The report was typewritten on 60 pages in folio format and was not published in any other way, possibly due to concern within the Defence Staff. In a secret memo from 27 March 1946, Chief of the Defence Staff *Carl August Ehrensvärd* (1892-1974) and Torsten Schmidt warned about the risk of outsiders getting to know too much about the Swedish capacity to produce atomic weapons and about who was involved in the work. However, the government bill that took up The Atomic Committee's proposal contained a fairly detailed account of the contents and proposals of the report.

The Committee itself also did not hesitate to mention the atomic bomb, and the report started with the following words:

> The construction of the atomic bomb has suddenly made a wider public concretely aware of the notions long harboured by physicists and chemists of the enormous quantities of energy stored in atomic nuclei based on laboratory experiments and theory.

In the same month as The Atomic Committee brought out its report, the National Committee for Physics organised a two-day conference with forty or so physicists to discuss research within nuclear physics. Lecturer Funke gave an account of The Atomic Committee's work, and Professor Sievert provided information on the risks of working with radioactive substances.

In an interview in *Stockholms-Tidningen* owing to its 50th anniversary in May 1946, Sievert told of his plans: 'We have plans for further development and this year's Parliament will decide whether we can build a high-voltage hall and an independent laboratory for working with radium emanations.'

Sievert also told of the 1941 Radiation Protection Act and of the supervision of radiological work that was carried out in accordance with the law. He said that the charges that the plant owners had to pay for the supervisors' inspections were not looked on with approval by everyone, which was a hindrance to the work. And new work assignments arrived:

> A new important work area for the supervisory activities will be opened through the use of atomic energy and it is fortunate that we in this country concentrate on the radiation protection problems, since with the use of atomic energy, these will play an extremely important role - the same would apply even if atomic energy were used for military purposes. In many cases, the irradiation risks seem to constitute an obstacle to the practical use of the new source of energy.

The Atomic Committee's report also brought welcome support for Manne Siegbahn's operations. His subvention for the running of the Nobel Institute was dependent on his also receiving monetary support from private entities, which he had so far received from the Wallenberg Foundation and the Rockefeller Foundation. In the long term, the support was in danger but the new situation opened up unexpected possibilities. Among other things, Siegbahn wanted support for the construction of a second, larger cyclotron. Tage Erlander now wrote in his capacity of Ecclesiastical Minister that it was obvious that the government could not fail to 'actively support and encourage continued research within nuclear physics and its area of application'.

On 22 May 1946, *Stockholms-Tidningen* carried an article on the test explosions that the Americans had planned at Bikini Atoll in the Pacific Ocean. The article said that both mould spores and larger organisms would be placed on ships around the bomb:

> 4 000 white mice, 200 pigs and 200 goats will be distributed over 22 ships to determine the effects of the bomb on personnel. The goats will be clipped and shaved so that parts of their bodies show similarities to human skin. Special clothes will also be tested on the goats to see how the clothes react. It is thought that lack of knowledge concerning these points may put a whole ship's crew out of operation.

Operation Crossroads was planned at Bikini Atoll, which was inhabited by 162 people in 1946. Bikini Atoll had been discovered in 1823 by Russian *Otto von Kotzehne*. It consists of around thirty islands (of which Bikini is the largest) in an oval-shaped wreath approximately 20 km x 40 km around a lagoon. The Americans made an agreement with *Juda*, the head of the community (Iroiji), who consulted the Council, which consisted of chief of the families. One account[*] says that 'Bikinians, convinced that the Tests would be a contribution to world peace, indicated their willingness to evacuate [the atoll]'.

Crossroads was the first bomb test after the war and comprised the blasts known as 'Able' and 'Baker' on l and 25 July 1946 respectively. At the time of the test, the United States had access to only nine atomic bombs, all of the plutonium type (*Fat Man*). Another year later there were still no more than 13 bombs.[†] The primary reason for this was problems with the production reactors at the Hanford Site, which were experiencing high neutron fluxes that degraded plutonium.

The Baker explosive yield resembled the strength of the Hiroshima device. but the bomb was detonated underwater. It raised a water column consisting of 10 million tonnes of water and the 26 000-tonne *Arkansas* battleship up was also lifted in the column. Photographs of the test impressed the whole world and made a great impact.[‡] The radioactive substances in the lagoon demonstrated very powerful activity following the blast; the assessment group thought it was equivalent to 'many hundreds of tonnes of radium'. Four days afterwards, the radiation level was still too intense for measurement patrols to go inland onto the atolls or to board the ships that were still afloat from the small fleet that had been stationed for experimental purposes.

Crossroads was executed under the command of one of the Navy's explosives experts, Admiral *W.H.P. Blandy* (1890-1954), assisted by William Parsons, who had now become Admiral. The experts included Stafford Warren, John von Neumann, and marine biologist Lauren Donaldson from the Applied Fisheries Laboratory in Seattle.

Extensive marine biology studies were carried out on the area in 1947. The responsibility for these was formally given to the Chief of the Pacific Ocean Navy but the biological investigations were led by Donaldson, whose laboratory now came under the newly-established US Atomic Energy Commission (AEC) which had taken over the responsibility for nuclear weapons from the Manhattan Engineer District. The studies led almost 700 people to Bikini in July 1947 and were led from *USS Chilton*, a ship that was especially equipped for the purpose.

The iron hand with which General Earls had controlled the Manhattan Project had been accepted by the scientists as a necessity, but they were displeased with the 'compartmentalisation' which had made the 'unnecessary' spreading of knowledge impossible. Scientists thought that they were sometimes, owing to lack of information, repeating experiments that had already been done.

After the war, this control led to an open conflict between the military and scientists. The first government bill on the atomic energy policy after the war was drawn up within the War Department and

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[*] Bombs at Bikini: The official report of Operation Crossroads. U.S. Joint Task Force One, 1947.

[†] On l April (!) 1946, *Svenska Dagbladet* still carried an announcement with the header 'USA has 500 atomic bombs'. The announcement said that 'The democratic congress member Hugh De Lacy declared on Sunday that the United States had 1 500 atomic bombs in store and were continuing their production'.

[‡] On both the fashion world (the two-piece bathing suit) and the pop world ('An itsy-bitsy teenie-weenie yellow polka-dot bikini ...').

was put forward as ‘the May-Johnson Bill’ in October 1945, and proposed that the Manhattan District be replaced by a new organisation, the Atomic Energy Commission. However, this proposal was met with strong opposition owing to the fact that the military, at the proposal of Groves, would gain a strong influence. Samuel Allison in Chicago declared that the nuclear physicists would rather study the colours on the wings of butterflies than continue under the secrecy requirements set by the military.

At the end of 1945, the national newspapers carried articles about how the American army had destroyed the five Japanese cyclotrons, and sharp criticism was directed at General Groves. The May-Johnson bill had been adopted by the House of Representatives but not by the Senate. To Groves’ annoyance, the young Senator *Brien McMahon* (born in 1903) convinced the Senate to set up an investigative commission to further study the future legislation in the atomic energy field. The commission worked during the period of November 1945 to April 1946 and organised both public and secret hearings at which those who had been involved in the Manhattan Project were questioned.

The result was a new proposal for an atomic energy law. The scientists had succeeded in convincing the Senate that there were no longer any important ‘atomic secrets’ that needed to be protected through military control. The new law, called ‘the McMahon Act’, was adopted by the Congress at the end of July 1946. The new organisation, United States Atomic Energy Commission (AEC), would be led by a commission consisting of five civilian members. The AEC took over all responsibility for nuclear technical operations, from uranium mining to the construction of reactors and the production of nuclear weapons. The latter-mentioned task would be handled by a special department under the leadership of a top officer.

AEC’s policy would be to permit the exchange of information between the scientists to safeguard the dissemination of ideas and criticism that are essential to operations. But, at the same time, the operation was isolated from the outside world in that for the moment there would be no exchange of information with other nations concerning the use of nuclear energy.

The new Act led to the establishment of not only the AEC but also of (1) an Advisory Committee for the President, (2) a Military Communications Committee set up by the Secretary of War and the Secretary of the Navy, and (3) a Joint Congressional Atomic Committee (‘Joint’ because it was common to the Senate and the House of Representatives). The chairman of the Advisory Committee was Robert Oppenheimer who, after the war, became director of the Institute for Advanced Study at Princeton University. The Committee included Conant, Fermi, Rabi and Seaborg. The first chairman of the Joint Committee was Senator Brien McMahon.

After the war, the desire for international control of nuclear energy and nuclear weapons had increased. Following pressure from Attlee, Truman had gone to a British-American-Canadian meeting in Washington DC in November 1945. Foreign Secretary Byrnes had given Vannevar Bush the task of coming up with proposals for the meeting. Bush suggested that the United Kingdom and the United States send representatives to Moscow to discuss the exchange of nuclear information. The proposal was also accepted in Moscow and they agreed to form an International Atomic Energy Commission (UNAEC) under the auspices of the United Nations.

Meanwhile, in 1946 the United States had established its Atomic Energy Act, which reflected the government’s prioritisation of nuclear weapons production and saw nuclear power production as something quite removed. Byrnes was looking for practical proposals and two committees worked some out: one under the future Secretary of State *Dean Acheson* (1893-1971), the other under *David Lilienthal* (1899-1981) who became the first chairman of the AEC from October 1946 to January 1950.

The joint report was called the Acheson-Lilienthal report. The proposal was put forward at the UNAEC by the delegate Bernard Baruch as the Baruch Plan, the main aim of which was to create an international atomic energy organisation (like the current IAEA). The majority within the UNAEC accepted the proposal, but the Soviet Union did not. In 1946-1947, the Soviet delegate *Andrej Gromyko* (1909-1989) put forward a counterproposal at the UNAEC that all nuclear weapons be declared illegal and be destroyed. A future inspection would then be more easily accepted. Gromyko’s proposal was rejected by the UNAEC, although the latter was no longer able to function after July 1948 and formally ceased operating in 1952.

When Tage Erlander 1946 succeeded *Per Albin Hansson* (1885-1946) as head of government, he asked The Atomic Committee’s experts if the Swedish atomic energy programme could lead to any international complications. The Svedberg's response was that this was quite possible since a uranium pile produced plutonium that could be used for atomic bombs. It was now obvious that The Atomic Committee - which was originally a governmental investigation committee but which had later had its responsibility extended to include the allocation of research funds - was not a suitable organisation to be dealing with the expansion of the plants that were needed within an atomic energy programme. A meeting with some cabinet ministers in January 1947 discussed the formation of a new company, *AB Atomenergi*, with the State as principal owner. On 26 April 1947, The Atomic Committee issued a second interim report, now with proposal for new the company.

Advice had been taken from the industry, which at this early stage preferred to see the first costly development being covered by the State. The State covered 57 % of the share capital (2 million of a total of 3.5 million Swedish kronor), although not through *Vattenfall* which, with *Waldemar Borgquist* (1882-1970) as Director General, was disinterested and thought that hydropower would be enough for a long time.

The responsibility of the government was now shared. While the Ecclesiastical Department with Erlander as Cabinet Minister had taken the initiative, it was now the Department of Commerce under *Axel Gjöres* (1889-1979) which proposed the bill regarding *AB Atomenergi*, popularly referred to as 'Atombolaget' (the Atomic Company).

The company was given the task of 'researching and extracting necessary basic materials for atomic energy use, to build experimental piles for atomic energy use, to subsequently build piles for atomic energy use in research and trade and industry on a larger scale, and to continue research in connection with said operations as well as industrial and commercial activity'. From trade and industry, 24 companies were part owners of Atombolaget, and these agreed to exchange experiences and research results.

There now appeared to be two parallel developments: a civilian one connected with trade and industry and a military one. However, there was still a strong personnel union between The Atomic Committee and the boards of the FOA and Atombolaget.

On 5 May 1948, the Defence Treatment Administration circulated a (secret) proposal as an instruction for medical treatment in atomic bomb attacks and 'in the event of attacks with radioactive weapons'. Luckily, the proposed treatment methods for those injured by radiation (blood transfusion, plasma transfusion, liver sample, toluidine blue) were never used; antibiotics were still things of the future. On 19 November 1948, Sievert was asked by the Defence Treatment Administration to give his opinion on a memorandum drawn up by the 2nd locum doctor *H. Hinricsson* containing a proposal for an expanded cooperation between the Treatment Administration, the FOA and the Institute of Radiophysics. Sievert responded on 5 February 1949 (he had been in contact with Hinricsson in the meantime) that he had already proposed a special committee for the purpose but that he needed money with which to realise it.

In 1949, the FOA and Atombolaget entered into a secret agreement which was approved by the government the following year. This meant that Atombolaget took over the reactor development and a number of nuclear physicists and some equipment were transferred from the FOA to Atombolaget. In 1950, research into uranium extraction and the production of plutonium were also transferred to Atombolaget. It was understood that Atombolaget's results also could benefit military research. The State still had full control of the development, something which worried some of the trade and industry sector, although not Asea, where Ragnar Liljeblad pointed out that operations only incurred charges and would not receive any income in the long term.

In the United States on 1 July 1947, the AEC had stated its intention to establish a test area for experiments and testing atomic weapons in the Pacific Ocean. The choice was now not Bikini but Enewetak Atoll, 300 km west of Bikini. Enewetak is on the outskirts of the Marshall Islands and was therefore considered to be safer for tests. It already had an aircraft landing strip.

Enewetak was discovered in 1794 by Briton Thomas Butler and consists of around forty islands in an almost circular wreath around a lagoon with a diameter of approximately 15 km. The series of tests that was prepared on Enewetak was called Sandstone and was implemented by means of three detonations (15 April and 1 and 15 May 1948), the largest of which was approximately 50 kilotonnes. A special radiation protection group called RadSafe monitored safety for the first time.

Not until the blasts on Enewetak had been completed in May 1948 did the AEC approve a proposal from the Applied Fisheries Laboratory to carry out a more comprehensive study of the residual radioactive contamination on Bikini and to extend the study to Enewetak. Neal Hines writes that it was like ‘hunting invisible clues in a game without rules’. The investigations were carried out in 1948 and 1949. The investigations had practical consequences for Bikini. Enewetak had been seized for an indeterminate period but Bikini Atoll was lent by the population (that moved to Rongerik) for only a short time. Unfortunately, the situation at Rongerik was bad - those who moved there were starving and the American Navy wanted them to be moved. But Bikini Atoll was still too radioactive. Head of the community Juda looked at different possibilities. One option was the island of Ujelang but the population from Enewetak had already been moved there.

In March 1948, Juda and his people were moved to Kwajalein and in autumn of that year on to Kili, a single coral island without a lagoon and with no possibility of fishing but with good farming options. But those who were moved there were not farmers and longed to return to Bikini, where the level of radiation was still too high according to the new surveys. It was a social tragedy.

In 1948, the AEC formed a division for biology and medicine with Shields Warren as director. When the Commission reported about this to the Congress, the following was written (quoted from Hines):

> Over the many decades during which physicians have used X-rays and radium for the treatment of disease, they have become familiar with the harmful effects of overdoses of radiation. Biologists have assisted by studying how radiations affect plants and animals. […] By the time that atomic energy was developed, therefore, science was already familiar with the biological effects of most types of radiations.
>
> What was new to the biologist and the physician in the development of atomic energy was the massive quantity of radioactive materials created and the greater potentialities of these materials for both good and ill. The Atomic Energy Commission has the obligation to investigate these potentialities and to encourage and assist others to do so. It must explore the many benefits in prospect […] and it must learn how to forestall the dangers to human, plant, and animal life.

In 1949, the radiation studies on Bikini were extended, this time with better measurement instruments. Thus far, measurements had been taken only of the total beta radiation from ashes, but now it was also possible to use a proportional counter* to measure alpha radiation and perform some chemical analyses. The scientists were astonished, however. Neal Hines writes:

> But what did this mean? And did it matter? The answers were elusive, difficult to phrase, their significance discernible as concepts rather than as tabulations of absolutes. What good was it to know that algae seemed radiation-resistant, or that microplankton appeared to be rather more important than macroplankton in the transport of radioactivity about a lagoon? The little surgeonfish darting over the iridescent coral shallows of the reef was a quick and lovely little creature living a life attuned only to an endless search for food. Capture him, and in his tissues would be found traces of the radioactivity created in Test Baker, long before he was born. To whom, or to what, was this important? To the fish? To a Marshallese child? To the Navy? To the Atomic Energy Commission? To mankind?

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* Many ionising radiation measurement instruments use measurement chambers in which the radiation ionises molecules of a suitable gas. The electric voltage over the chamber must be great enough to allow the formed ions to be captured by an electrode and create a measurable electric current. The chamber is called an *ion chamber* and the ionisation is used as a measure of the radiation dose since it corresponds to the quantum or particle energy of the incident radiation. If the voltage is further increased, the formed ions can receive an energy that enables them to ionise the chamber gas. It is then possible to achieve a thousand-fold amplification of the ion flow. If the voltage is such that all current pulses are amplified in the same proportion, the chamber is called a *proportional chamber*. If the voltage is even higher, this proportionality disappears and all current pulses become equal in strength, irrespective of the energy of the radiation. The chamber can then no longer be used to measure radiation doses but becomes an extremely sensitive radiation detector and is called a Geiger-Müller chamber (GM tube). It can be incorporated into an instrument where the frequency of current pulses can be read on an instrument or heard as clicks in an amplifier.

In June 1949, the AEC signed a contract with Holmes & Narver to support one of the test stations on Enewetak. The project meant that the atoll was completely set up for land-based operations. Both Crossroads (on Bikini) and Sandstone (on Enewetak) had been largely waterborne expeditions. Crossroads had indeed involved 42 000 people, but mainly on ships. Equipment was now planned on in much larger scale on the atoll itself. Hundreds of measurement stations were erected and homes on Parry Island, one of the islands on the atoll. Thousands of men would come to work on the atoll, but not one woman.

In Sweden, *Sigvard Eklund* (1911-2000), head of research at Atombolaget from 1950-1961, received information that illustrated the close connection between civilian and military research targets during the forthcoming 1950s. He wrote a report on behalf of the FOA regarding the conditions for the production of Swedish atomic bombs, and under the auspices of Atombolaget he constructed the first Swedish research reactor called R1, which ended up being located in a rock shelter next to the IVA's test station on Drottning Kristinas väg near the Royal University of Technology. The reactor would use natural uranium with heavy water from Rjukan as a moderator and was commissioned on 3 July 1954. Eklund had worked at the Nobel Institute together with Hannes Alfvén and was a very vigorous, single-minded man.

The choice of heavy water rather than graphite as a moderator was made for two reasons: partly because it was thought to be difficult to produce sufficiently pure graphite, and partly because a graphite reactor would require considerably more uranium, and uranium was still in short supply. However, in the long run, it was hoped that large quantities of uranium could be obtained from the shales that contained uranium in central Sweden. The uranium content there was indeed low, 200-300 grammes of uranium per tonne of shale, but it was thought that there were very large quantities overall. The shale also contained the carbonaceous rock called kolm, which could contain up to 0.5 % uranium. The kolm was therefore initially what they were interested in.

The FOA's chemical department was asked to research methods for the isolation of uranium. Contact was therefore made with scientists at the Swedish Shale Oil Company and KTH. At KTH there was a young licentiate, *Erik Svenke* (1918), who was employed by *Bolidens Gruv AB*. Svenke was employed at Atombolaget as one of its first scientists as early as 1947. His development of methods to leach uranium from kolm took place at Nitro Nobel's old factory in Vinterviken in southern Stockholm.

Eklund showed that a Swedish atomic bomb programme would not be economically unviable - five bombs per year could be produced for a reasonable cost. However, both The Atomic Committee and Atombolaget were negative in their outlook to such a programme which could prevent a normal development of the peaceful programme. And when President Truman's successor, Dwight Eisenhower, offered other countries technical information and access to uranium fuel in an address at the UN's annual general meeting in 1953, the conditions changed drastically as regards the interest on the part of trade and industry. Both Asea and Vattenfall now took a different view of the development while Erlander began to doubt whether it was reasonable for Sweden to have nuclear weapons.

However, in the 40s, the idea of a Swedish atomic bomb was not unreasonable and the FOA worked toward that objective. If Atombolaget succeeded in building reactors, the FOA's main problem was extracting plutonium from the irradiated reactor fuel, i.e., finding a reprocessing method. Young chemist *Jan Rydberg* (1923-2015), who went on to become Professor of Nuclear Chemistry at Chalmers, was employed to perform the task in 1947. Rydberg worked out a method for reprocessing the fuel using liquid extraction, as previously proposed by Seaborg but which was not used until 1954 when there was access to (Norwegian) irradiated uranium. Rydberg gave a graphic description of his process in a talk in 1993 (quoted from FOA, 1995):

> [The task] was thus to separate plutonium from fission products and uranium. In 1947, the situation regarding information was that we actually had no information at all. We only had the Smyth Report which contained practically no chemistry. So we had to

start from scratch but, with the help of LarsGunnar Sillén*, we managed to get things going fairly well. Of the different chemical separation technologies that we had available to choose from, we immediately rejected the precipitation method; we thought it be difficult to control at a distance and on a continuous basis. So, we began with ion exchange but were caught up with some research problems. Sillén then suggested that we try liquid extraction.

I see that not many chemists here, and maybe also not others, know what liquid extraction is. Simply imagine that you have an aqueous solution of some metal ions - say uranium and plutonium - and pour on paraffin oil. The paraffin oil then lies on top of the water. Then you add a reagent, which we can call X, and stir. Reagent X reacts with plutonium; you obtain a Pu-X compound which is soluble in paraffin oil. You then have paraffin oil containing plutonium and the uranium is left in the water. You then simply pour off the paraffin oil solution and are left with plutonium that is separated from uranium. It's as easy at that!

However, the method was not particularly well-known when we started. We therefore had to start extensive research operations to find suitable X compounds, alternatives to paraffin oil, etc. The first system that we studied more closely was my licentiate work (thorium-acetylacetone in 1950). That same year, we were able to present a thoroughly considered research programme. We obtained a grant from The Atomic Committee. We had 10 publications by the end of 1952. We were actually international pioneers in so far as the English and the French had not published anything in this field, and there were only a few hints from the Americans that they were using liquid extraction. (We later found out that the Americans started their separation of plutonium at the Hanford Site using a precipitation method but changed over to liquid extraction at the start of the 1950s; this was confirmed to us at the Geneva Conference in 1955. When we later compared our system with the American one, it was shown that they had chosen the same solvent as us, hexon, but another salting agent, aluminium nitrate, whereas we had chosen calcium nitrate.) Our work led to us have extraordinarily good relations with the Americans: they viewed us largely as colleagues. [...]

* Before 1952, *Lars-Gunnar Sillén* (1916-1970), who was consultant for the FOA, was Professor of Inorganic Chemistry at Chalmers University of Technology and then Professor at KTH in Stockholm.

16. RENEWED CONTACTS

THE AMERICAN Advisory Committee (US Advisory Committee on X-Ray and Radium Protection) was revived immediately after the war and, upon reforming in 1946, was named the National Committee on Radiation Protection (NCRP), a name which subsequently changed a few times while the acronym, NCRP, remains unchanged.

Initially, the cooperation across large regions and between countries was inhibited by the communication difficulties in the initial post-war years. The contacts were therefore largely limited to discussions between people who had already known each another well before the war. Within the radiation protection field it concerned primarily Evans, Failla, Gray, Mayneord*, Parker, Sievert and Taylor. Of these, only Parker had been fully employed in the atomic bomb project.

The new NCRP had many more members than its predecessor. Before the war, the Advisory Committee's members had come from organisations that were active in protection against radiation within healthcare. After the war, many new organisations were added such as the U.S. Public Health Service, the Atomic Energy Commission (AEC), the Army, the Air Force and the Navy and the Civil Defence. Each and every one of these had the right to have two representatives in the NCRP.

The first NCRP meeting took place in December 1946. By then, the Committee had established seven sub-committees. The two most important were sub-committee 1 under the chairmanship of Professor Failla and sub-committee 2 with Karl Morgan as chairman. Failla's sub-committee was formally responsible for recommendations for protection against external radiation, but it was in reality the group that formulated the NCRP's fundamental radiation protection policy. Morgan's committee was responsible for recommendations on protection against the ingestion of radioactive substances, i.e., internal radiation.

In December 1947, the NCRP's executive committee accepted a proposal from Failla's sub-committee to sharpen the recommendations for limiting doses. A maximum permissible radiation dose of 0.3 röntgen (corresponding to approximately 3 mSv) per week was now recommended instead of the previous tolerance dose of 0.1 röntgen per day, i.e., a reduction of the corresponding maximum annual radiation dose from around 250 mSv to 150 mSv, a significantly smaller reduction than the one stated by Henshaw's now forgotten recommendation from 1941.

The new limit value was included in the NCRP's first report after the war, a report from sub-committee 3 on medical x rays protection for acceleration voltages of up to two million volts.

The American radiation protectionists cooperated with their colleagues in Canada and the United Kingdom soon after the end of the war, primarily within the Atomic Energy of Canada, Ltd. and the British Medical Research Council. The contacts between the Briton Mayneord and the Americans Failla and Taylor were particularly important. They began to plan for a cooperation conference that would be held in Chalk River in Canada in autumn 1949, the first in a series of what would later be called Tripartite Conferences. In preparation for this conference, an AEC meeting was held in Washington DC on 29-30 March 1948. Those participating in it included Doctors *John Loutit* and *Joseph Mitchell* (born in 1909) from England, *A. J. Cipriani* from Canada and Failla, Taylor and Shields Warren from the United States. Minutes from this meeting were written by John Bowers, Assistant Director at the AEC's unit for biology and medicine.

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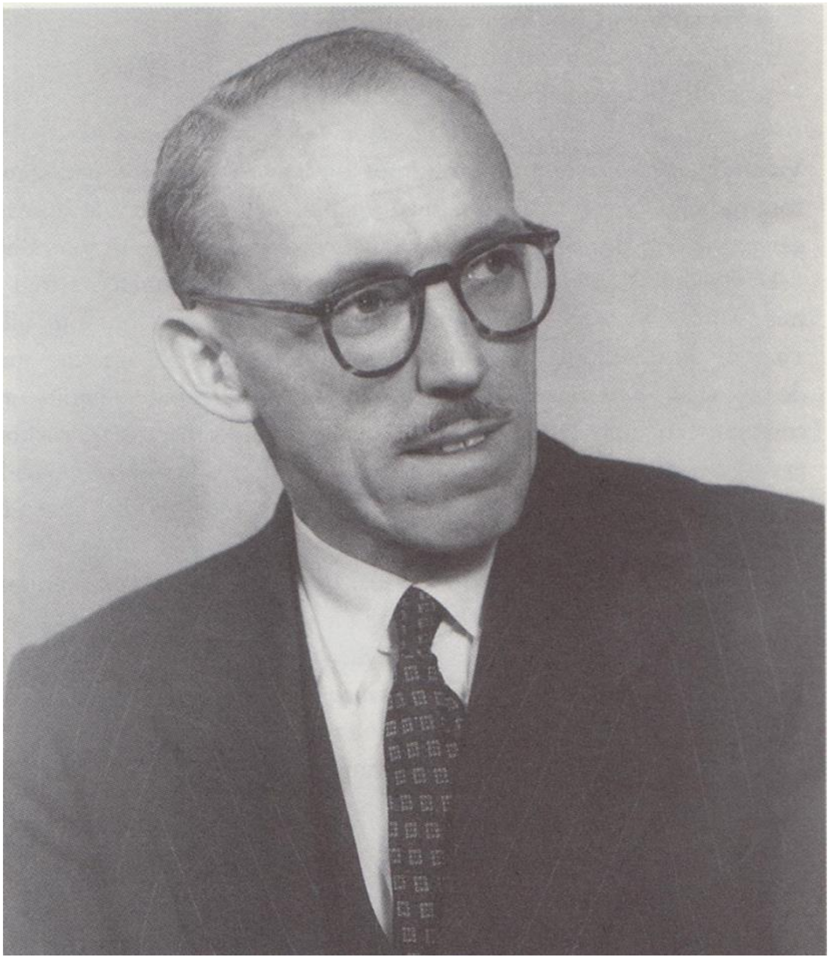
* Professor *W.V. "Val" Mayneord* (born in 1902) was the medical physicist at the Royal Cancer Hospital in London and had substantial influence on the international radiation protection policy. He was an intelligent and original man and was a well-qualified adversary of Rolf Sievert, whose proposals he usually took delight in criticising.

According to the minutes, Dr. Loutit gave an account of English experiments with dogs which were irradiated with 0.1 röntgen per day for a couple of years, which had resulted in a fall in sperm production. It was now agreed that such a high daily dose could be harmful and that half as much, i.e., 0.3 röntgen per week, would be a more suitable limit, just as the NCRP had recommended three months previously.

It was also agreed that the most important injuries one ought to look out for were:

1. Destruction of bone marrow with consequences for blood formation.
2. Reduction in fertility.
3. Hereditary injuries.



Walter Binks

It was also agreed that radiation doses caused by protons which gained kinetic energy by colliding with fast neutrons ought to be considered to be five times as effective as doses caused by x-ray and gamma radiation.

Dr. Shields Warren reported on the first experiences from the Atomic Bomb Casualty Commission (ABCC), the Commission that was set up in 1947 by the American authorities in Japan to do long-term studies of the biological effects of the atomic bombs.* He was able to say that those who had barely survived the crisis that followed three weeks later (due to a minimum of granulocytes, vital white blood cells) had received radiation doses of between 500 and 600 röntgen (approximately 5-6 gray) and nobody had survived higher radiation doses.

An additional preparation for a more comprehensive international cooperation took place on 17 September 1948 at 38, Old Queen Street in London, which is where a work group under the British Medical Research Council met. The group was way down the hierarchy but was important nonetheless. Its task was to discuss tolerance doses at the request of a radiation protection subcommittee under a committee for medical and biological applications of nuclear physics, and Professor Mayneord was chairman. This is where we meet *Walter Binks* for the first time, who was secretary of the work group.

Walter Binks had been employed by the National Physical Laboratory in 1926 to deal with dosimetry matters; in 1952, he would be given the task of acting as the first head of a new radiation protection organisation, the Radiological Protection Service (RPS), which the British Department of Health and the Medical Research Council established on 1 January 1953. In the 1950s, Binks became one of the most influential people within the international radiation protection work. He was a small, serious man of a nervous disposition and was very conscientious with his assignments.

Two Americans had been invited to the meeting at Old Queen Street, Professor Failla and radiation pathologist *Herman Lisco*. Lisco appears to have been the role model for the pathologist, who was called Dr. Beale and who in Dexter Masters' novel *The Accident* expects Louis Slotin, in the novel called Louis Saxl, to die following the criticality accident in Los Alamos.

The other participants in the meeting included Professor Mitchell, Doctors Gray and Loutit, Sir John Cockcroft and *Sir Ernest Rock Carling* (1877-1960). Sir Ernest, a well-known radiologist, would go on to become chairman of the ICRP.

Binks gave an account of previous exchanges of letters between those responsible for radiation protection in the United States and England. In United States, the NCRP had already recommended that the maximum permissible radiation dose be 0.3 röntgen per week. The British work group would have preferred the limit to have been 0.5 röntgen per week. Since the two cases required different ways of measuring the dose (in the American case in free air and in the British case primarily the irradiated body in order to also include backscattered radiation), the British thought that in practice there was no difference between the two cases when it came to the radiation dose in the body.

There then followed a discussion on what they really wanted to limit. Failla thought that it would be best from both the scientific and the legal point of view to set a limit for the radiation dose in the different organs of the body, and that 0.3 röntgen per week or, even better, 0.3 rep. per week, would be a suitable limit. The British maintained that for practical reasons, the limit ought to refer to that which was measured, i.e., ionisation in air rather than the body. They were still able to agree that, whichever was chosen, it would be more suitable to give one weekly dose instead of one dose per day as before.

* Rightly or wrongly, many Japanese believed for a long time that the ABCC was an organisation that had been established by the occupational forces solely to observe the survivors in Hiroshima and Nagasaki, those referred to as the "hibakusha", like trial animals. The fact is that no actual help was offered to the bomb victims by the Japanese government until the occupation ceased in 1952. Initially, the ABCC was a "field organisation" under the American Academy of Sciences, paid for by the Atomic Energy Commission (AEC). The project later became a cooperation project between the American Academy of Sciences and the Japanese National People's Institute of Health.

Gioacchino Failla

They also discussed a maximum lifetime dose and Failla suggested 300 röntgen, bearing in mind the risk of hereditary injuries, but it was agreed that it was unnecessary to make any recommendation regarding this. Binks said that a formal limit for the lifetime dose would be difficult to administer in practice.

The highest body content of radium recommended by the NCRP, 0.1 microgramme, was criticised by Gray and Mitchell. They pointed out that Robley Evans found that everyone who had more than 1 microgramme of radium in their body had suffered from bone sarcoma. If one assumed proportionality between the likelihood of cancer and the radiation dose, the likelihood of a sarcoma at 0.1 microgramme could be 1-2 %, too great a risk. The response to this was that *Dr. A. Krebs* of the Kaiser Wilhelm Institute for Biophysics in Frankfurt had measured average body burden of 0.015 microgramme of radium in people who had not worked with radium, but that an abnormal occurrence of bone sarcoma had been observed. Binks suggested that this ought to be investigated more closely - if the occurrence of bone sarcoma were found in Frankfurt, it would be possible to calculate the risk from 0.1 microgramme of radium.

Owing to the uncertainty of the risk estimate, they agreed to observe caution. For example, they could allow the 0.1 microgramme to apply to people at the age of 70 and demand proportionally lower body burden at younger ages. In the end, it was agreed that the RBE value[*] of 10 for alpha radiation would be accepted. Dr. Failla suggested the value of 5 for slow neutrons.

[*] RBE stands for Relative Biological Effectiveness (see Chapter 9).

On 29-30 September 1949, the first ‘Tripartite Conference’ was held in Chalk River in Canada, with 24 participants from Canada, the United Kingdom and the United States. The best-known participants were A.J. Cipriani from Canada, Professor Mitchell from the United Kingdom, and from the United States Shields Warren (acting as chairman), biologist *Austin Brues* (born in 1906), Failla, Hempelmann, Wright Langham, Karl Morgan, Herbert Parker and Lauriston Taylor. There are two sets of minutes of the meeting, one signed by the English biophysicist *G.J. Neary* and the other by Canadian *G.E. McMurtrie*. The minutes are practically speaking identical.

The most important matter discussed was permissible radiation doses. We should remember that they had not yet given any thought to the different limit values for the public and people who worked with radiation sources, e.g., at hospitals. The concept ‘tolerance dose’ remained in their minds - if one of them tolerated a dose, so would the other. At the proposal of Failla, it was agreed that the maximum permissible radiation dose for exposure to external radiation would be 0.3 rep. per week in the critical body tissue for total body irradiation. ‘Critical’ tissue or organ referred to the body tissue or the organ which, as a consequence of a dose of radiation and radiation sensitivity, would lead to the greatest risk for the irradiated individual. It was agreed that the critical tissue could be seen to be the blood-forming organs (which were assumed to be bone marrow and the spleen) with the expectation of ‘the principal hazard probably being leukaemia’).

It was minuted that 0.3 röntgen measured in free air (the previous recommendation from the NCRP) and 0.5 röntgen measured next to the body (the previous British proposal) were equal and for penetrating radiation could be seen to correspond to 0.3 rep. in the critical body tissue.

The discussion then turned to how large a total body dose could be accepted for one single, powerful instance of irradiation. Dr. Failla suggested 25 rep. (i.e., 250 mSv) for people under the age of 45 and 50 rep. for ages above that. Professor Mitchell said that the British Medical Research Council’s group working tolerance doses thought that radiation exposure in an emergency situation ought not to exceed 10 röntgen (i.e., approximately 100 mSv). This limit applied also to emergency situations within the Chalk River plant. Dr. Parker questioned whether they really would need to deviate from the normal dose limit in atomic energy plants. Dr. Shields Warren summarised everything by saying that everyone had agreed that individual high doses of radiation could occur only in really serious emergency situations but that conference did not need to arrive at any numerical value.

On the other hand, it was interesting to know at which total body dose a serious hazard could occur. Shields Warren stated the radiation exposure that was expected to lead to death for 50% of the irradiated (‘LD-50’ as an abbreviation of ‘50% lethal dose’) to 400 röntgen (corresponding to approximately 4 gray). Professor Mitchell thought that the radiation exposure above which serious injury could occur was approximately 25 röntgen, i.e., the value proposed by Failla as a limit for a one-off dose in an emergency situation. In the end, they agreed that, as far as they knew up until now, no permanent injury could be expected following one-off exposures below 25 röntgen, with the possible exception of pregnant women.

And so they came to internal radiation from the radioactive substances that entered

the body and began with radium-226. There was experience of injuries from radium-226 but not of the other radioactive substances with similar properties. Of these substances, uranium, plutonium-239, thorium and polonium-210 were the most relevant. The easiest method of setting the limit values for these substances was to work on the basis of radium and the relative danger presented by the substances compared with radium.

It was pointed out that the chemical toxicity of the uranium was probably dominant over its radiation risks. For the growing atomic energy operations, a limit value for plutonium-239 was the most urgently needed; Chapter 10 depicted the difficulty in comparing radium and plutonium from the radiation protection point of view. Dr. Wright Langham suggested that the comparison could be made on the basis of the energy absorbed per unit of mass. To this came the objection that animal experiment data for radium-226 and strontium-90 indicated that such a comparison did not always hold.

However, the first thing to do was to establish the limit value for radium-226. They began by estimating the minimum quantity of radium that was known to cause injuries which, according to Robley Evans’ studies, was 1 microgramme. On the basis of the same reasoning portrayed in Chapter 10, they concluded that the corresponding quantity for plutonium-239 was also 1 microgramme. The values for uranium-233, polonium-210 and thorium-234 were determined on similar grounds.

Implicit in this reasoning was the existence of a threshold value below which the risk was zero. In that case, it was simply a matter of selecting a suitable safety margin for the minimum hazardous quantities that had been agreed. They chose a factor ten, i.e., divided the agreed values by ten and obtained the following *maximum permissible body burden*:

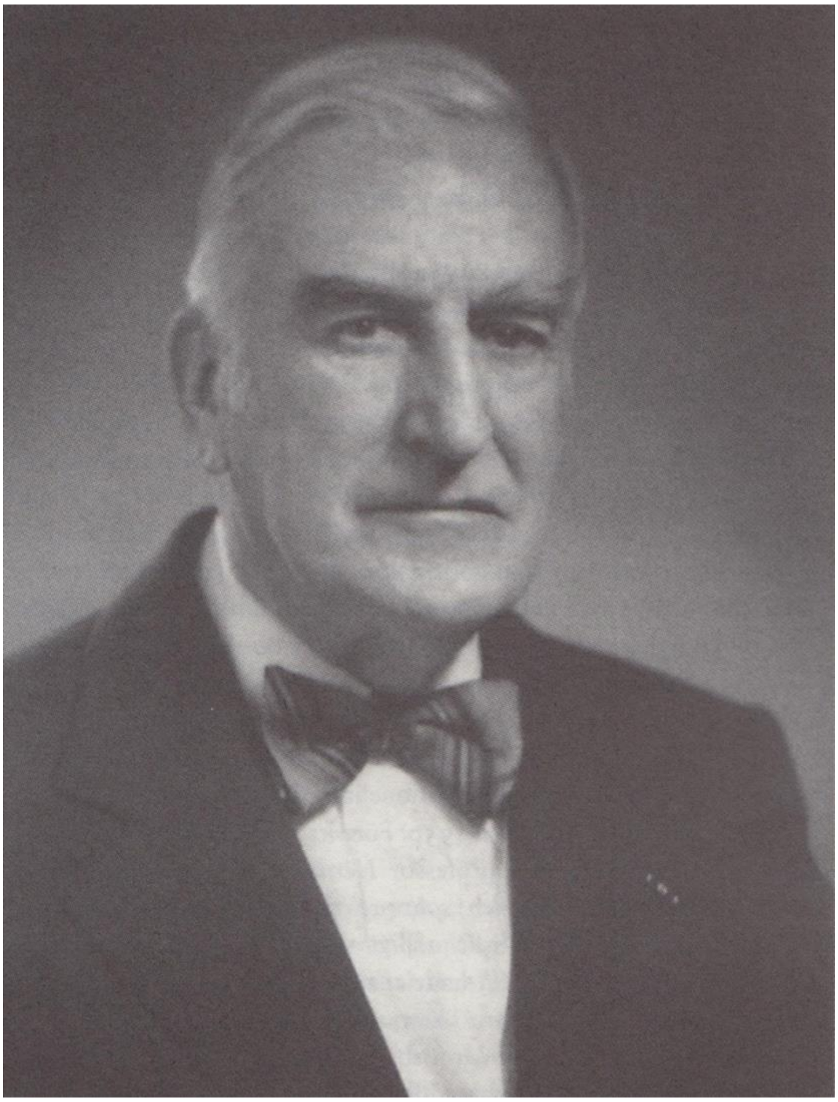
radium-226	0.1 microgramme
plutonium-239	0.1 ″
uranium-233	0.6 ″
polonium-210	0.005 microcurie
thorium-234	0.8 ″

However, they now began to realise that contaminations of things such as drinking water with these substances could not be limited on the basis of calculations based on such high values. You could not rely on the actual existence of a dose threshold. If many people were exposed to contaminated drinking water and air which could lead to such body burdens, the harmful effects could perhaps be noted in disease statistics. Dr. Brues thought that the requirement for protection for the public ought to be one where no harmful effects were statistically discernible. The minutes of the meeting say:

> Consequently, since it could not be ruled out that such effects were linearly related to the dose, he would propose a safety factor of 10 for populations of the order of 105 and 100 for populations of the order of 107 on the figures accepted for the [atomic energy] plant and other special workers. He considered that it would be unsafe to draw conclusions from the existence of natural water with very high radioactive content, since the populations exposed to such waters were very limited. He felt it was undesirable to double the natural radium content of the skeleton. Professor Mitchell referred to the recent work of Dr. Bale and Dr. Hursh indicating a radium content of the skeleton of the order of 2 x 10-4 µg[*]. He felt that an increase of 10 on that figure was the absolute limit of what was justifiable.

They finally agreed a safety factor of 100 against the values that had recently been accepted as limits for the body burden for radiation workers. This meant, for example, 0.001 microgrammes for radium-226, i.e., five times the Rochester scientists' measurement value but just 1/15 of Krebs' value. This was the first time a multinational group recommended a limit to apply to the public. It is also interesting to note that people were already counting on the possibility of a 'linear, no-threshold model' fifty years ago. The discrepancy between Krebs' and the Rochester scientists' measurement results for the natural radium content in the human body involved an annoying uncertainty, and several laboratories, including Rolf Sievert's, started new radium measurements.

[*] *William Bale* and *John Hursh* (1907-) were scientists at the University of Rochester. Their estimate of a body content of 0.0002 microgrammes of radium-226 can be compared with the value of 0.015 microgrammes mentioned at the meeting in London on 17 September 1948 as a result of Krebs' measurements.



Lauriston Sale Taylor

In the same month that the Chalk River conference was held, i.e., September 1949, Failla wrote a draft of an NCRP report on the permissible dose. Failla maintained that they could no longer be certain that 0.1 röntgen per day was a safe dose. He continued writing (according to Taylor):

> […] It is obviously true that the less exposure, the better. This should always be borne in mind by those working with radiation, since in most cases it is within their power to minimize exposure by meticulous adherence to the principles of protection. Thus a worker should never allow his exposure to reach the prescribed limit, if he can avoid it. On the other hand, the limit cannot be set so low as to interfere seriously with important work. There is an element of danger in every human activity and it is unreasonable to expect complete immunity from harm in working with radiation. Be that as it may, the fact remains that in the present state of the art, any limit of exposure that is considerably higher than the background level of radiation must be considered to involve some element of danger – whether it is actually so or not. […]

It was now time for an extended cooperation beyond national committees and the tripartite meetings. The first International Congress of Radiology after the war was planned to be held in London in 1950. Before the war, the intention had been to hold the sixth ICR in Hamburg with *Professor Hermann Holthusen* (1886-1971), the well-known German radiologist, as President but now, a few years after the war, it was no longer practically possible to hold the Congress there. The equally well-known British radiologist Ralston Paterson was selected to be President of the London Congress.

While preparing for the Congress, it was natural to look at the possibility of revising the previous Commissions, the ICRP and the ICRU (as they are currently known). When the troubles in Europe had begun at the end of the 1930s, the ICRP's then Honorary Secretary *G.W.C. Kaye* (1880-1941) had asked Lauriston Taylor to be responsible for the ICRP's and the ICRU's affairs during the war until it would be possible to resume the work. Already in 1947, Taylor had then received a telephone call from *Arthur Christie* (1879-1956), the President of the fifth ICR in Chicago in 1937. Christie asked Taylor for a status report regarding the ICRP and the ICRU.

Before the war, the ICRP had had 8 members and the ICRU 22. Of the ICRP's members, only Taylor and Sievert had survived the war. Taylor and Sievert were also the only survivors of the ICRU's work committee. When Christie heard this, he suggested to Ralston Paterson that Taylor be asked to reorganise the two Commissions. Paterson took up the proposal and asked Professor Mayneord to help Taylor with the assignment.

Following an exchange of letters, Mayneord and Taylor met in Taylor's work room at the National Bureau of Standards where Taylor was now Head of the Department of Radiation Physics. At their proposal and following a further exchange of letters with Christie and Paterson, a proposal was established to ask people whether they wanted to be members of the revived ICRP. Those who were to be asked were:

Sir Ernest Rock Carling,	England (Chairman)
Lauriston Taylor,	USA (Secretary)
Walter Binks,	England
E.L. Chérigié,	France
A.J. Cipriani,	Canada
Robert Jaeger,	Germany
W.V. Mayneord	England
R.R. Newell,	USA
Rolf Sievert,	Sweden

Chérigié's name never appears later; Newell was a radiologist at Stanford University Hospital. What happened next in London belongs to the 50s and falls outside the framework of this volume. However, it should be mentioned that another Tripartite Conference was held in Harwell in connection with the London Congress in 1950.

A great deal would happen within radiology in the 1950s, including the fact that radiation treatment with energy-rich photons, 'million-volt therapy', would get its definitive breakthrough. The pioneering work had taken place in the 1930s. This included van de Graaff's work at the Massachusetts Institute of Technology; the early ideas of Norwegian *Rolf Wideröe* (born in 1902) on different types of accelerator; Ernest Lawrence's accelerator research with things such as the cyclotron; *Donald Kerst's* (1911-1993) development of the betatron at the University of Illinois; *Albert Soiland's* (1873-1946) high-voltage treatments in Los Angeles; Failla's and *Edith Quimby's* (born in 1891) studies of high-voltage therapy at the Memorial Hospital in New York; Robert Stone's work in San Francisco, particularly with *David Sloan's* high frequency system; and the Norwegian million-volt plant at Haukeland Hospital in Bergen. It is still too early to talk about all of this here – it constitutes a background that belongs with the story of the progress made by the million-volt therapy in the 1950s.

17. THE DREAM OF NAUTILUS

IN SPRING 1946, the captains of American warships on the west coast had learned to fear a man standing astride on the quay when the ships were to berth. The man with the awkward-looking stance was the Naval Inspector, Captain *Hyman George Rickover* (1900-1986). Rickover was known for his stubbornness and the almost fanatical energy he put into his tasks. It was said that his critical reports were so hot that you needed asbestos gloves to handle them. Rickover was popular among his friends, who called him Rick, but not always well-regarded among older Naval officers who were irritated by his lack of respect for old-fashioned rules and his unwillingness to participate in cocktail parties and other formal or less formal social rituals.

In April 1946, a secret letter was sent to the Army Air Force, the Navy and a number of large industrial companies in the United States. The sender was Monsanto Chemical Company, the company that took care of some of the operations for the Manhattan District at Oak Ridge. The recipients of the letter were invited to send representatives to the Clinton plant to participate in the erection of what was planned to be the world's first 'nuclear-powered' electrical generating station. Monsanto thought that reactor-powered aircraft and ships could become a reality in the not too distant future, so the military recipients of the letter ought to be interested. The civilian recipients represented industries that had the necessary capital and an interest in producing electrical energy. The initiative was in line with President Truman's desires. After the use of the bombs on Japan, it was desirable from the political point of view to be able to talk about the peaceful use of atomic energy. The President had appealed to the Manhattan District* to start a power production project, but the status there was no longer efficient enough after the war to be able to cope with starting up a new big project.

People in the Navy also had doubts about Monsanto's invitation. Some Admirals did not believe that reactor-powered ships could become a reality until way into the future and therefore did not consider Monsanto's project to be a very high priority. The matter finally ended up at the Naval Bureau of Ships in Washington DC where they decided to send eight people to Oak Ridge, three civilian employees and five naval officers. One of the officers was to be experienced at a sufficiently high level to authoritatively represent the Navy's interests.

It was easy to find the younger representatives, and the four young officers could be selected from among the most talented and well-trained at the Naval Bureau of Ships. On the other hand, it was initially impossible to find a higher officer who was interested in the assignment.

Meanwhile, on the west coast, Naval Inspector Captain Rickover had grown tired of his assignment which he considered to be routine with little to fire the imagination. On a work visit in Washington DC, he told his superiors at the Naval Bureau of Ships that he had heard about the possibilities of atomic energy and that it could be possible to build reactor powered ships. He suggested that they allow him to have special training in nuclear physics to be able to take on this new challenge.

His superiors were not unwilling. The found it appropriate to send Rickover to MIT, the Massachusetts Institute of Technology, for a three-year course. Meanwhile, Admiral *Paul F. Lee* recommended that one of his friends, Captain *Harry Burris*, be the Navy's main representative in the Monsanto Project. This was the situation when Rickover's boss during the war years, Admiral *Earle Mills* (1896-1968), who deputised for the head of the Naval Bureau of Ships, came onto the scene. When Mills heard that Rickover would be sent to MIT and Burris to Oak Ridge, he intervened and opposed

* For the sake of clarity, we should differentiate between the Manhattan Engineer District, i.e., the organisation that was set up under this code name to design the atomic bomb, and the Manhattan Project, i.e., the assignment to produce the bomb. As an organisation, the Manhattan District survived for a time after the original assignment was completed and was then replaced as an organisation by the Atomic Energy Commission, or AEC.

Admiral Lee's recommendation. Mills listened to those who believed in reactor-powered ships. He thought that what was needed in the Monsanto Project was an exuberant man and that Rickover was just the person who was needed.

As soon as Rickover had been given the assignment he moved back to Washington DC to prepare himself, in the first instance by looking at the secret documents that were stored at the Naval Bureau of Ships, among which he found the following recommendation from December 1944 from a group within the Manhattan District called the *Tolman Committee*:

> The government should initiate and push, as an urgent project, research and development studies to provide power from nuclear sources for the propulsion of naval vessels. It might be advisable to authorize the initiation of these studies, without waiting for the postwar period, in order to utilize scientific personnel already familiar with the pile theory and operation. […]

However, during the final hectic stage prior to the completion of the atomic bombs, the nuclear weapon project was given top priority and there was no space for any other undertakings. Rickover also found that the Navy had also had its own plans to develop a reactor for its purposes but that nothing had happened since it was not considered to be appropriate to have a competing activity outside the Manhattan Project.

However, what Rickover found the most interesting was a document appendix written by Phil Abelson with a detailed proposal as to how the Navy could build a reactor-powered submarine. This proposal would have a decisive effect on Rickover's future. Thus far, there had been talk of reactor-powered ships in general terms, but here was a proposal for a submarine. Rickover, who had served on a submarine and attended the Navy's Submarine School in New London, Connecticut, immediately realised the possibilities that Abelson's proposal opened up. For the first time it would be possible to have a *real* submarine, a submarine that would be independent of any air intake and that would not need to surface. It would be able to cross the Atlantic under water. It realised Jules Verne's dreams of Nautilus, Captain Nemo's extraordinary underwater craft in the novel *Twenty Thousand Leagues under the Sea*. Rickover was now convinced that the construction of a reactor-powered submarine ought to be the Navy's primary aim over the next few years, and with this conviction he set off for Oak Ridge.

Before the journey, he participated in an important meeting. The Naval Bureau of Ships had been visited by some prominent representatives of the General Electric Company. The company wanted to start reactor research and had recently received a grant from Manhattan District to build an atomic energy laboratory near Schenectady in New York State, a plant which would eventually be called Knolls Atomic Power Laboratory. General Electric was not a novice in the nuclear arena after having managed the scientific laboratory at the Hanford Site. It now wanted to hear about the Navy's interest in naval reactors.

What General Electric's designers brought to Washington DC was construction drawings to a naval reactor for a nuclear-powered destroyer. The talk impressed the naval officers and the visit led to the shipyard asking the Manhattan Engineer District to enter into a contract with General Electric for the construction of a naval reactor.

Before his trip to Oak Ridge, Rickover met General Leslie Groves at Manhattan District's Washington office. Rickover complained that the secrecy regulations prevented him from seeing everything he needed for his task. General Groves' response was that they would quickly arrange for Rickover to be able to see what he wanted to see. At the meeting with Groves, the latter's deputy, Lieutenant Colonel Nichols, was present and the following day Rickover was allowed to accompany Nichols in the latter's aircraft to Oak Ridge. This high-flown arrival gave Rickover a flying start to his assignment in more ways than one.

Before the four younger officers left Washington DC, they were told that they were being sent to Oak Ridge to study and not to work under Rickover. They should *not* take orders from Rickover. Rickover rapidly changed these conditions; he wanted a coherent group from the Navy. When the younger officers initially referred to their instructions from Washington DC, Rickover pointed out that he, as the highest ranked, was the one who would be filling in the competence reports regarding each and every person, reports that would affect their promotion prospects. This remark was sufficient for Rickover to get the obedient ‘Naval Group’ he had been looking for.

The leader of the Monsanto project was Monsanto’s *C.R. McCullough* (1900-1970) but its scientific leader and the person who was behind the idea and the initiative was Professor of Chemistry Farrington Daniels from the University of Wisconsin, who succeeded Arthur Compton as head of the Manhattan Project’s Metallurgical Laboratory in Chicago during the war. The power reactor they were trying to build at Oak Ridge was therefore soon named the ‘Daniels Pile’.

Rickover’s Naval Group soon gained respect. Its efficiency and work capacity were appreciated. The group wrote reports to Rickover on various aspects of the work every week and he forwarded them to Admiral Mills in Washington DC as coming from ‘the Oak Ridge Naval Group’. The young officers in the group soon started to appreciate Rickover and gradually almost idolise him. For his part, he soon convinced the group that the Navy’s main objective was a reactor-powered submarine, a Nautilus.

However, as the Naval Group in Oak Ridge became increasingly united, it began to lose confidence in the Monsanto project. According to Rickover, the project leadership lacked understanding of the technical problems, which required engineering knowledge rather than expertise in nuclear physics. Farrington Daniels’ reactor concept involved many construction difficulties (the reactor should be moderated with beryllium oxide and cooled with helium). Manhattan District was also falling apart due to lack of leadership and support from the Congress, which was unable to make decisions on the nation’s atomic energy policy.

However, at the end of 1946, as a consequence of the planning in the face of the transformation of Manhattan District into the new Atomic Energy Commission, a request came from Kenneth Nichols, who was now a General and would later become the Managing Director of AEC, for a statement from the Navy on their hopes and expectations regarding reactor-powered ships. Rickover, who was in the Bethesda Naval Hospital outside Washington DC for an operation at the time, seized the opportunity. He postponed the operation, arranged an office at the hospital and obtained secretarial assistance from Admiral Mills. Captain *Lou Roddis* (1918-1991), one of Rickover’s four talented young assistants, was called in from Oak Ridge and Rickover and Roddis jointly drew up a report that favoured reactor-powered submarines. Within five to eight years, the United States would have the first nuclear submarine, they wrote.

The report was sent to the Naval Bureau of Ships where it was read by Admiral Mills and one of his closest colleagues, Commander *Albert Mumma*, who were dismayed at the audacious promises. The shipyard forwarded the report to the Secretary of the Navy but Mills exercised caution and wrote a disclaimer: ‘It should be pointed out that the opinions quoted herein do not reflect the policy of the Navy, but that of two individual officers’.

The work with the Daniels Pile deteriorated further during the latter part of 1946. When Rickover no longer saw any future for the Monsanto project, he gathered his group for a trip to General Electric’s headquarters in Schenectady, the same city where Gösta Forssell and Rolf Sievert had discussed the future of radiation physics a quarter of a century earlier.

The Naval Group from Oak Ridge was surprised at the progress made by General Electric’s scientists at the Knolls Atomic Power Laboratory in consultation with Captain Burris: not only had they come a long way with their study of the conditions for a power reactor for destroyers, they had also ventured into a completely new project, a breeder reactor.

In a breeder reactor, more fissile material is created through the nuclear reactions than that which is consumed, and this occurs by means of the capacity of fast neutrons to transform uranium-238 into fissile plutonium-239. Instead of making bombs from this plutonium, you can either split it for energy extraction in the reactor or take it from the reactor to use as fuel in another reactor. There is therefore no need to replace reactor fuel just because the commonly-used source of energy, uranium-235, runs out – you gain a new source of energy with interest from the conversion of uranium-238 to plutonium. In this way, not only do you use the small share of uranium-235 in the fuel but also a large share of the uranium-238 which constitutes most of the natural uranium. However, the breeder reactor does lead to significantly greater construction problems than the ‘usual’ thermal reactor.

Rickover was impressed but critical at the same time. He thought that the reactor that was intended for destroyers was too large, and Burris supported him in this. Rickover suggested a smaller reactor, suitable for destroyer escorts, or ‘DE’ for short). The smaller reactor could also be suitable for a submarine of course. After a while, Rickover succeeded in getting General Electric to focus on a submarine reactor, although the project continued under the name of DE.

Rickover and his group wrote a final report to Admiral Milis and suggested an immediate focus on the production of a naval reactor with water as a coolant. The work ought to be carried out by a special work group, tentatively consisting of the people who had experience of the work at Oak Ridge. The group ought to work within the recently formed Atomic Energy Commission.

But Admiral Milis, who was now head of the Naval Bureau of Ships, was no longer listening. He ordered that the group be dissolved and assigned the members elsewhere. Rickover’s own fate was discussed in detail. There were powerful forces within the Naval Bureau of Ships who did not want him to remain in Washington DC, but Mills finally decided to keep him as a ‘special adviser’ on nuclear energy matters.

However, Rickover did not give up. He realised that the nuclear submarine never would come to fruition unless he could convince a few people at the very highest level. However, he had a poor basis for this. He had no power within the Naval Bureau of Ships, only the ear of Admiral Mills. The power when it came to reactors lay with Captain Mumma.

However, the exuberant Rickover was not inactive. He received reports on how the work with the Daniels Pile was continuing to deteriorate and that most of those who previously used the project for training in reactor physics had left Oak Ridge, as had Daniels himself. Just a few enthusiasts remained, and Rickover estimated that the grant and resources would run out within six months. But, he thought, these six months ought to be used in full.

Rickover returned to Oak Ridge and gathered the remaining scientists and engineers who were still studying the Daniels Pile. He went straight to the point and asked them whether, since there was no future for the Daniels Pile, they would instead be willing to use the remaining time and resources to work on naval nuclear propulsion. The response from the surprised remaining few was yes.

Since according to Rickover the Navy needed a submarine reactor, the reactor ought to be compact, use thermal neutrons and be water-cooled. Admiral Mills was informed that Rickover had ‘stolen’ the original Oak Ridge project for his purposes but several weeks passed before the Atomic Energy Commission found out what had happened. The AEC decided to leave things alone since the original project had failed and if Rickover could keep the enthusiasts going it was better than that nothing was being done – it was not exactly a question of substantial costs.

But this was just one small step along the path; a major breakthrough was now needed. Since the Navy brass involved so far had been anything but positive towards it, adverse in fact, it was up to Rickover to convince a higher level of power. He saw two possibilities: the Chief of the Navy and the highest level of the AEC. If he could get the Chief of the Navy to sign a letter to the Secretary of the Navy, maybe something would happen.

Writing a letter for the Chief of the Navy to sign was not something that a single Captain could do. Such a letter had to have the blessing of an inordinate number of admirals in different departments, and these admirals had specific points of view as to how Rickover’s draft of the letter ought to be re-written. But Rickover gained support from a few colleagues with submarine experience who succeeded in getting what had been a continuously re-written letter past the Chief of the Navy’s reluctant shield of critical admirals. And so the letter reached Admiral Chester Nimitz, the most renowned officer of the United States Navy.

Nimitz was the one who, along with General MacArthur, had received Japan’s surrender on the *USS Missouri* flagship in Tokyo Bay on 2 September 1945. Nimitz was the Chief of the United States Navy from 1945-1947 when he had been Supreme Commander of the United States Pacific Fleet throughout the Second World War. After having heard a brief talk on the importance of reactor-powered submarines, Nimitz signed the letter to the Secretary of the Navy on 5 December 1947. To the surprise of many, he had immediately shown a positive reaction and appointed the Naval Bureau of Ships as the Navy Unit that would develop a nuclear submarine.

And the opposition to Rickover’s ideas within the Navy thereby ceased, with the previously unwilling admirals now sailing in the new direction of the wind. But not all obstacles were circumvented as yet. It was still Captain Mumma rather than Rickover who was responsible for the Naval Bureau of Ships’ action with regard to reactor matters. And as if that were not enough, the Congress had placed all responsibility for nuclear energy matters with the Atomic Energy Commission. Without the help of the AEC, there would be no nuclear submarine. Rickover's next attack thus concerned the Commission.

After numerous hardships, he succeeded in getting the AEC to prepare to formalise the reactor study that Rickover had independently arranged with the remaining people at the Daniels Pile in Oak Ridge and to transfer this work to a special reactor laboratory, i.e., to show that it was prepared to invest in a naval rector. At the start of May 1948, Rickover had finally set everything in motion but was still not able to control what happened.

Throughout the spring, Rickover had attempted to get different industries interested in the reactor project. With the AEC reluctantly having included the naval reactor in its programme, Westinghouse decided to enter the scene. They had indeed sent a number of people to Oak Ridge for training during the Monsanto project but at the time, no-one had shown any interest in the reactor issue. They were now willing to sign a contract with the Naval Bureau of Ships and set up a special reactor laboratory. General Electric worked with liquid sodium as a coolant since they were trying to develop a breeder reactor. Westinghouse planned to use ordinary water as a coolant. The reactor they were thinking of was based on a proposal by Captain Roddis. The coolant they would use would be water under high pressure (what we now call a pressurised water reactor, or PWR) which required a heat exchanger, a *steam generator*, to create the steam in a secondary circulation system that would power turbines. Roddis had had the idea from *Alvin Weinberg* who later became head of the Oak Ridge National Laboratory. Weinberg has in turn referred to discussions that he had had with Enrico Fermi and has said he remembers the lively discussions he had with Rickover and the latter's Naval Group at Oak Ridge in 1946.

But the contract with Westinghouse was not enough for Rickover who still had no formal power. He now demanded that Admiral Mills choose between him and Mumma. Mumma was a good administrator but lacked Rickover's unparalleled capacity to achieve the impossible. Mumma was removed as head of the nuclear physical operations at the Naval Bureau of Ships and Rickover took over the operations and immediately created a special nuclear physics department. He had the original Naval Group from Oak Ridge restored in this department. Rickover now had the required position of power within the Navy but he needed to fortify his relations with the AEC.

The AEC was still not focusing whole-heartedly on naval reactors although it was now officially accepted that this was a necessary task. However, when Rickover became head of the Navy's reactor development, there was greater pressure on the AEC to give the task greater priority. Because of this, in September 1948 the AEC declared that it intended to set up a special reactor development department which was fully comparable with the Commission's previous two departments for weapon production and military applications. The new organisation did not become effective until February 1949 because there were difficulties with finding personnel, and primarily a head of department. At the end of 1948, the AEC had moved its then not particularly extensive reactor research to the *Argonne National Laboratory*, established in 1946 near Chicago, a plant that was established by the AEC for fundamental nuclear research under the management of the University of Chicago. The move of the reactor research to Chicago led to the loss of personnel who did not want to move, and at first there was no adequately competent person who was willing to lead the expanded operations. In the end, a very capable man was found in the form of *Dr. Lawrence R. Hafstad*, who had been Secretary of the Defence Department's research and development committee. As soon as Hafstad was given the assignment, Rickover called on him since he wanted to convince him that the AEC's new reactor department ought to have a special unit for naval reactors, with Rickover as its head.

It took Hafstad several months to organise the new reactor department, months during which Rickover had difficulty keeping his impatience in check. However, everything was ready in February 1949. The AEC then also described the direction of the department's work. There would be four projects. One would concern a water-cooled submarine reactor as planned by Rickover. The second project concerned General Electric's breeder reactor. The third project concerned a small material testing reactor. Finally, the fourth project concerned an experimental breeder reactor was meant to be commissioned long before General Electric's larger breeder reactor, which was a more long-term project.

In passing as it were, the AEC mentioned that the head of the unit which would develop a submarine reactor was Captain Hyman George Rickover. Finally, Rickover now had exactly what he had wanted:

he led the nuclear submarine operations within the Navy and within the AEC. This reduced the bureaucracy to a minimum. If within the AEC project Rickover needed a decision from the Navy, he simply wrote himself a letter and immediately answered the letter on behalf of the Navy.

The thing that still remained for Rickover to do was to arouse adequate interest on the part of Westinghouse. For a long time, the company had seen naval reactors as something for the distant future. Rickover travelled urgently between Washington DC and Westinghouse's headquarters in Pittsburgh to get the company's directors to understand the worthwhile future that lay in the development of a reactor. When the AEC then announced its decision to establish a special reactor department, those in Pittsburgh began to understand that something really was underway. Representatives of Westinghouse travelled to Washington DC to speak to Rickover and said they were willing that expand their undertakings to cover the whole reactor system. Rickover consulted Hafstad and on 10 December 1948, i.e., already two months before AEC's new organisation became official, the AEC signed a contract with Westinghouse. AEC undertook to pay for the construction of a nuclear power laboratory on a disused airfield, Bettis Airport outside Pittsburgh. Westinghouse undertook to get hold of physicists and engineers to build a water-cooled submarine reactor in cooperation with the AEC's Argonne research laboratory.

After all the administrative difficulties and prolonged persuasion attempts, Rickover and his Naval Group could finally devote themselves to the technical problems. Great attention was paid to the material problems early on. The objects sought included metals with a high melting point that were incapable of absorbing neutrons; for instance, such a metal was needed to house the reactor fuel, i.e., for cladding. The element zirconium was found to have the desired properties. Zirconium is not an uncommon metal but does not occur in large concentrations in the Earth's crust. The most common mineral is zirconium silicate, $ZrSiO_4$, which has been used as a gem that is reminiscent of diamonds. Since its similarity to diamonds led to cases of fraud, zirconium silicate was called *jargon* in French, meaning forgery, which appears to have been the origin of the name *zircon* for the mineral.

From Rickover's point of view, the gem characteristic was a concern since it made the mineral expensive. The mineralogists informed the Naval Group that the price of zirconium was more than one thousand dollars *per gramme*. Rickover asked why and was told that the total quantity of zirconium in the United States was no more than could be fitted into a shoebox and that the production procedure was very time-consuming. Rickover replied that it must be possible to change this. 'From now on, you call me Mr. Zirconium,' he said, 'because I am going to get this stuff produced by the ton!' He succeeded as always. When a few years later Secretary of the Navy *Dan Kimball* (1896-1970) asked those responsible at Westinghouse how in the world they had succeeded in obtaining equipment to produce such quantities of cheap zirconium, the answer he got was: 'Rickover made us get it.'

Initially, the work within Westinghouse proceeded slowly and it was difficult to get the physicists involved to see the project as anything other than long-term, whereas Rickover wanted an immediate payoff. He tried talking with both physicists and engineers. He asked the latter not to be scared by the difficulties seen by the physicists. 'The atomic-powered submarine is 95 percent engineering and only five percent physics', he said. The efforts eventually brought results, progress was made in the Bettis Atomic Power Lab. and, after a while, the company became so ardent that there were complaints about the cooperation partner Argonne National Laboratory not keeping up with the pace.

In the Navy's shipyard, drawings and models of the ship's hull emerged. One of the great benefits of the submarine is that it travels faster under water at the same engine power than it does on the surface. Beneath the water it is only friction against the water that steals energy, whereas on the surface energy is also lost in the wave formation.

The scientists from Argonne had plans to incorporate reactors, heat exchangers, pumps and all other equipment in one single lead-protected packet which only needed to be lifted into the submarine and screwed firmly in place. Rickover was perplexed and asked how they had intended to take care of the maintenance. If, for example, a pump needed to be replaced, you would be forced to lift out the whole installation, with reactor and everything. Westinghouse found a solution that was more maintenance-friendly.

It was now time to start building a land-based prototype. In a meeting between Rickover's Naval Group within the AEC and Westinghouse's engineers, they later described how they had intended to have a large building in which the different construction elements could be spread out. No, said Rickover.

'The land-based prototype will not be spread out in a building. It will be built inside a submarine hull on dry land.'

The Westinghouse engineers protested. You could not incorporate the *prototype* into a submarine hull. There was a problem building the final reactor there later. But Rickover did not want to acknowledge the concept of 'later'. If the problems were not solved at the prototype stage, he argued, it would take a particularly long time to solve the installation problem. Rickover wanted to build the two reactors, the prototype and the final reactor, almost simultaneously with only a short time difference in the development so that the experiences of the construction of the prototype could be transferred directly to the real submarine reactor. Rickover was hoping to gain five years in this way.

The remaining task was to now find a suitable place for the construction. The nuclear physicists certainly assured them that the reactor could never explode like a bomb but even a small explosion could spread the radioactive substances to the surroundings. An isolated workplace must therefore be found. The AEC suggested that they keep to the new 400 000-acre testing station built in between Arco and Idaho Falls in Idaho, later called the Idaho National Reactor Testing Station. Westinghouse protested and thought that the transportation problems would cause the cost to escalate, but finally agreed to use the Idaho plant which was first called the Arco Desert Test Station.

When it was time to start the work, Westinghouse thought that they had to have help from those who would be building the actual submarine and the builder had not yet been appointed. On the east coast of America there were only two yards at which submarines could be built. One was the highly esteemed Electric Boat Company, founded in 1899, which was later (1952) developed into the major defence industry complex called General Dynamics Corporation which produced not only submarines but also fighter aircraft, tanks and cruise missiles. The Electric Boat Company's yard was in Groton, Connecticut.

The other yard was the Navy's own submarine yard in Portsmouth, New Hampshire but the yard could not spare any personnel. Rickover was therefore forced to approach the Electric Boat Company where the director's answer was yes without hesitation. Electric Boat was willing to sign a contract with Westinghouse to help the company to build a land-based submarine prototype in Idaho.

With the risk of lagging too far behind its competitor Westinghouse, on 12 April 1950 General Electric decided to reorganise the operations at its Knolls Atomic Power Lab and change its priorities where the breeder reactor plans had previously been the main priority. The management of the Hanford Site was still the highest priority but the submarine reactor was one below that. However, the reactor now planned by General Electric was not a water-cooled reactor but a reactor with liquid metal as a coolant. It would be an *intermediate* reactor, i.e., a reactor in which neutrons were not completely slowed down as in thermal reactors (Westinghouse's project) yet not unrestrained as in fast reactors (General Electric's breeder reactor project).

Rickover was now facing significant administrative problems. He had his own Naval Group, Westinghouse and General Electric, Electric Boat, personnel at the Argonne National Laboratory, the AEC's bureaucracy and his superiors within the Navy to deal with all at once. He rejected schedule after schedule and thought that the work could go much faster than others believed. He dictated letters to two secretaries at a time and was constantly on the telephone. The number of construction elements requested for the prototype in Idaho exceeded one hundred; specifications had to be worked out and suppliers found, sometimes for 'impossible' parts.

The work proceeded under constant pressure from Rickover. The prototype for the submarine reactor developed in Idaho and construction of the 'right' reactor would soon begin at Westinghouse. Rickover fought against ambitious designers who wanted to fill the submarine with new inventions and refinements while he wanted to keep it simple and primarily sustainable.

When the work with the nuclear submarine was at its most intensive in summer 1951, Rickover, and with him the whole project, was affected by an unexpected blow which originated from a promotion system established by Congress in 1916 to select which captains would be promoted to admirals. In the 1800s, promotion within the Navy had been more or less automatic. At a certain age you could expect to be promoted one step, but the promotion process was slow and when you had finally become an admiral, which the majority of healthy and fairly competent officers became, you were nearing retirement age. At the start of the 1900s, alternative promotion systems were discussed and in 1916, Congress introduced the system that still applies today.

The current promotion system engages a promotion committee called the Selection Board consisting of nine admirals. This Selection Board submits an advancement proposal to the President, who can approve or reject the proposal and forward it to the Senate, again for approval or rejection. If the President does not approve the proposal, the Senate cannot override or change it, but must send it back to the Selection Board which may try again until there is success in finding a proposal that can be approved.

Rear Admiral Hyman George Rickover

On 2 July 1951, a Selection Board met to view a list of candidates for promotion. Among the names on the list was H. G. Rickover. However, Rickover's name was not in the proposal that was sent to the President for approval. The promotion system includes the rule that anyone who has been bypassed by the Selection Board twice is forced to retire. The Selection Board's decision of 2 July had thus placed Rickover in the risk zone.

This did not really worry the vigorous Captain. He was fully occupied by the submarine project. In August 1951, the Navy announced that a contract had been signed with the Electric Boat Company for the construction of a nuclear-powered submarine. This notification led to big headlines in newspapers and the mass media started paying attention to Rickover. *Life Magazine* predicted that Rickover would go down in history as a man who meant as much to shipbuilding as the inventor of the steamer, Robert Fulton.

Just after the Navy had announced its contract with Electric Boat, the notification came from AEC that they had signed a contract with Westinghouse to build the actual submarine reactor. Then the Navy announced that the first nuclear submarine would be called *Nautilus* and that the keel was expected to be placed in spring 1952.

The first thing to be discussed was whether General Electric's prototype would also be built in Idaho, but in the end the company decided to build the prototype outside Schenectady, near West Milton in New York. When the company had consulted the AEC's reactor safety committee, it became clear that this would not encounter any objections – provided that the reactor was built within a 70-metre steel globe whose steel was approximately 3 cm thick. Such steel plate could be produced in only three places in the United States and the material available corresponded to total production over the space of three months. All such steel plate being produced at the time was also intended for the construction of the huge aircraft carrier *USS Forrestal*. Nevertheless, the AEC succeeded in obtaining the steel it needed.

In spring 1952, Rickover's work assignment was expanded. The AEC had secretly approved the Navy's plans to build a reactor-powered aircraft carrier, and Rickover was asked to formally sign a contract with Westinghouse for such an assignment.

On 14 June 1952, the keel of *Nautilus* was laid at the yard in Groton, Connecticut. It was a solemn ceremony with the Army, Air Force and Navy joint chiefs of respective branches of the Defence present, as well as the chairman of the AEC. More than 10 000 people were present and applauded President Harry Truman when he arrived to give his address. He said, among other things:

> All this has been accomplished in an amazingly short period of time. When it was started four years ago, most people thought it would take at least ten years, if it could be done at all. But one tough problem after another has been conquered in a fashion that seems almost miraculous, and the work has forged ahead.

After the President's address, he handed over to the chairman of the AEC, Gordon Dean, who said:

> There are many people who have played a role in the events which have led to this ceremony, but if one were to be singled out for special notice, such an honor should go to Captain H.G. Rickover, whose talents we share with the Bureau of Ships and whose energy, drive, and technical competence have played such a large part in making this possible.

Rickover was sitting impatiently in the audience. He thought that the ceremony was a waste of time. He thought that all the high dignitaries ought to have more important things to do in Washington DC – that was the case for him in any case.

On 8 July 1952, the Navy's Selection Board met again and once again, Rickover was one of the candidates whose suitability for promotion would be reviewed. Once again the admirals sided against him. Despite the honourable mention and medal from the Secretary of the Navy, in spite of a letter from Rickover's superiors in the AEC, in spite of a letter from Senator Brien McMahon when the latter was chairman of the Congress' Atomic Energy Committee (Joint Atomic Committee), in spite of a letter from Rickover's superiors in the Bureau of Ships, in spite of Admiral *R. S. Edwards*' previous recommendation that Rickover ought already to have been promoted owing to his efforts as Naval Inspector, in spite of many other documents putting forward the point of view that if anyone should be promoted to admiral it ought to be Captain Rickover… in spite of all this, the admirals on the Selection Board would not be moved.

The decisive factor could have been Rickover's outspokenness and lack of respect, his contempt for rules and conventions, his way of directly negotiating with the most appropriate person with no regard for insignia or rank. However, we will never know what it was since the Selection Board did not take minutes and members were subject to secrecy.

According to the rules, Rickover, at the age of just 52 and in the middle of a very important project for the United States, would now be forced to retire after 30 years' service, which would be 30 June 1953. But it was not what the future held that worried him the most; the acute problem was the lack of confidence expressed by the Admirals and the immediate consequence that this would have. Would

Rickover be able to maintain even temporarily the respect with which he had previously been treated? Would all work stop in the advent of Rickover being replaced by a less exuberant person? And would he now be asked, each time he pushed through decisions and work inputs, whether his superiors really did back him?

Rickover's case aroused great attention in the press, on radio and on TV while Rickover's name began to disappear from the statements from the Navy. In newspaper articles from the Navy and TV appearances, Rear Admiral *Homer N. Wallin*, the new head of the Bureau of Ships, was instead quoted as the man behind *Nautilus*, a man who at the time had never once visited the test site in Idaho. This story aroused the interest of the journalists and they began to publish accounts of the 'conflict' between Rickover and the Navy's admirals.

When Rickover himself was interviewed, he did not want to talk about any conflict - he only wanted to talk about the nuclear submarine project. But when *Time* wanted to write about the problems that had now arisen within Rickover's Naval Group and send the article for editorial review in accordance with the secrecy regulations, the Navy retained it for 67 days while discussions were ongoing as to what could be done to counteract the harmful consequences that could be anticipated when the article was published. When the article was finally printed, the Navy held a press conference and maintained that the information about problems within Rickover's Naval Group was incorrect. The AEC, which knew better, made a statement contradicting the Navy. The AEC wrote the following:

> […] if Rickover is forced to resign from the Navy on retirement schedules, these things will probably happen: (a) morale of the AEC's Naval Reactors Branch will be affected, (b) some of Rickover's staff will leave, and (c) there will be a time lag that usually occurs when there is a change in a key post.

In the Congress, Rickover's case was taken up by the Senate and the House of Representatives. The questions brought up were echoed in the mass media, which in turn made many industrialists get in touch with both the Navy and their congressional representatives. In the meantime, in the late summer of 1952 Congress had authorised funds for another nuclear-powered submarine once General Electric had been successful with the prototype in West Milton. This second nuclear submarine would be called *Sea Wolf* after a submarine of the same name that had been lost during the Second World War, tragically enough through mistakenly having been annihilated by friendly-fire shots from United States destroyers.

The Selection Board's proposal - the one that had omitted Rickover - had meanwhile been passed on in accordance with valid rules. It had been approved by the Secretary of the Navy, who said that he would rather have seen Rickover promoted but did not think he could get involved in the decision of the Admirals. It had continued to President Truman who had also approved the proposal, and was finally on its way to the Senate's Defence Committee. And there it had remained since the Committee was being reformed due to recent elections.

According to the rules, the Defence Committee could refuse to approve the promotion proposal, but even if this concerned just one single man, the whole list (of 39 Captains put forward to become Admirals) had to be sent back to the Navy for a new nomination procedure. Never before had the Defence Committee opposed a proposed promotion. However, suddenly, in February 1953, the Defence Committee decided to examine the case of Rickover more closely. The head of the Bureau of Ships, Rear Admiral Homer N. Wallin, was asked to provide an account of what had happened regarding the nuclear submarine project and what Rickover's role had been. Wallin appeared before the Defence Committee on 3 March 1953. He gave a long and detailed account of what had happened ever since the Navy discussed the possibility of nuclear reactions with Professor Fermi and other scientists on 17 March 1939. He continuously emphasised the role of the Bureau of Ships and the importance of the project and that it had all been teamwork rather than important inputs by a few.

The Naval Group heard about Wallin's evidence and it infuriated Rickover's colleagues. The Admiral had given a point-by-point account of contributions that were made by Rickover as though they were made by the Bureau of Ships. He had constantly downplayed Rickover's accomplishments. One of the Congress members who had taken up Rickover's case, Senator *Henry Jackson*, who had been a member of a reactor development committee under the Congress' Atomic Energy Committee and was therefore able to read between the lines, contacted Rickover and asked to meet him. Rickover refused with reference to the fact that he did not think it appropriate. Jackson then instead contacted some of Rickover's employees in the Naval Group and got them to come to Washington DC where they were questioned by a number of Senators, including Jackson.

On 5 March 1953, the Senate's Defence Committee met again and then listened to an account from Senator Jackson, who was not a member of the Committee. Jackson convinced the Committee that Admiral Wallin's evidence had been misleading. He also proposed questions that the Committee ought to put to the Navy and asked the Committee to continue with an in-depth review of the case.

At the time, Dwight D. Eisenhower had taken over as President and was warned by his political advisers that a scandal was brewing and could damage the President if he did not arrange promotion for Rickover. The White House told the Pentagon what the President wanted.

The Pentagon now had a new Secretary of the Navy, *Robert B. Anderson*, who quickly got to grips with the case and concluded that an injustice had occurred. After having consulted the chairman of the Defence Committee, Anderson wrote a letter to the Committee suggesting that the Navy could establish a special Selection Board to see which Captains with engineering tasks could be retained in active service for a year and to ensure that one of these was qualified within the field of reactor-powered ships. If Rickover were to be selected for this extended service, he would automatically be a candidate for promotion when the next ordinary Selection Committee met in July 1953. Anderson also said that he would instruct this Selection Board to ensure that a captain with experience of reactor units for powering ships was promoted to admiral.

On 9 March 1953, the Defence Committee published the Secretary of the Navy's letter. His proposal would enable the Defence Committee to approve the promotion of the 39 Captains since it now saw a way to save Rickover. Anderson set up the special committee, which recommended a year's extension of Rickover's service. In July, the ordinary Selection Board then met for the year's proposed promotions. Everyone now expected Rickover to finally get his promotion. But even now, a number of admirals were obstinate and the discussion about Rickover appeared to have taken five hours. However, in the end, the Committee made a majority decision and proposed Rickover's promotion to the rank of Admiral.

When this decision was announced, this justice also affected Admiral Wallin, whose account before the Defence Committee had been misleading. Although he had one and a half years left as head of the Bureau of Ships, he was removed from his position by the Secretary of War and sent to take charge of the Bremerton shipyard in Washington State.

18. THE HIGH-VOLTAGE HALL

WHILE I was doing my national service I had applied to the Royal University of Technology (KTH) in Stockholm; in the first instance for technical physics, out of a mixture of obstinacy and curiosity - that was the department that was the most difficult to get into, and in the second instance for architecture. My marks were not good enough for technical physics but I got onto the architecture course. Military service made it impossible to start there that same year, so I deferred my studies for a year.

My school-leaving certificate marks had almost been good enough for technical physics. When I was in military preparedness at the Norwegian border in 1943, I therefore wondered how I could supplement them to get onto the physics course that autumn. The easiest way seemed to be drawing mark, which carried a lot of weight. I had previously had A for drawing but in the last year of high school we had done ‘linear drawing’ to show how things such as cylinders, cones and level surfaces can intersect one another. It was boring and I did not put as much effort into it, and my drawing teacher, *Gustaf Nordlander*, consequently lowered my drawing mark to a small ‘a’. It oughtn’t to be that difficult to restore it to a capital ‘A’, I thought.

I therefore prepared myself for linear drawing and got permission to travel to Stockholm and meet Nordlander, an unusual man with theatrical gestures. But he was no longer interested in linear drawing. Instead, he fetched a coffee pot off a shelf and said: ‘Draw that!’

I didn’t need to finish the drawing. After a short while, Nordlander interrupted with: ‘I knew you were good at drawing.’ So he wrote confirmation of the higher mark and I was able to return to my preparedness situation and submit a new application to KTH. Thanks to the higher mark for drawing, I got onto the technical physics course in autumn 1943. Had my drawing not been as good I would have become an architect.

I eventually also became assistant to Professor Gudmund Borelius at the Physics Institute. Borelius was a man of honour and the fact that he was already a Professor at KTH in the year I was born, 1922, made an impression on me. Borelius was the one who had taken the initiative to create a special department for technical physics 1932. He was the real experimental physicist and proclaimed once to me that a real experimenter should never have the need to draw any curves in a diagram. If enough points corresponded to the measurement results, Borelius thought that no curves were needed.

Borelius’ lectures were not that compelling, however. His lecturing voice was rather monotonous with Dalecarlian overtones. He spent his recreational time in Siljansbygden, the Dalecarlian heartland, and he said that what he liked the most was lying on his back in a rowing boat and looking up at the clouds while the boat drifted over Lake Siljan with the wind.

The atomic bombing of Hiroshima on 6 August 1945 came as a total surprise to Swedish physicists. It was the summer holidays and I was doing an apprenticeship in a small workshop on Döbelnsgatan and was standing by a machine pressing the pinion of a pencil sharpener when the news came. It was incomprehensible.

In the mid-1940s, the Swedish Association of Graduate Engineers was very active and its premises were at Brunkebergstorg 20. One of the Association’s specialist departments was the Technical Physicists’ Association (TFF). Physicists who had yet to graduate, i.e., technology students, could also participate in the meetings which were often held by the TFF. The number of technical physicists, including the technologists, of whom there were only around twelve on the course per year at the time, was so small that everyone knew one another. The Technical Physicists’ Association’s premises were something of a club premises that you could also visit when no meeting was being held.

The meetings that the TFF held in the building on Brunkebergstorg were well-attended and often combined talks with a communal dinner with lively discussions. The bigger meetings were sometimes held by the Technical Physicists’ Association with other specialist departments present. A big meeting was held soon after the atomic bombs had been dropped on Japan. Talks were then held about the bomb on the basis of what the Smyth Report had said. Lise Meitner was present and was asked whether she believed that it would be technically possible for Sweden to accomplish an atomic bomb. I was there myself and still remember Meitner’s expression of ridicule and contempt as she rejected the possibility without hesitation.

At a later date, probably following a talk by Sigvard Eklund, I met Rolf Sievert for the first time. After dinner he was sitting alone on a sofa with a coffee and cognac in front of him and a large cigar in his hand. He waved me over to him and asked if I wanted to join him. He then began to tell me about the problems that the dentists were causing him with their lack of care when using x-ray apparatuses. The rays are disintegrating their fingers, he said, and they have no concept of the risks. And when we try to inform them of the risks at regular inspections, they complain about the fees!

This episode made me aware that there was a ‘radiophysics’ institute with Sievert as head, and that this was where research was done into radiation. Soon after that I had in my hand a compendium from a course organised by the Swedish Academy of Sciences where there was a lecture on nuclear physics and the radioactive substances, written by Sievert’s colleague Sven Benner. It aroused my curiosity about Sievert’s institute. Nuclear physics and radiation were something new and attractive to me.

When one day Gudmund Borelius showed me a slip of paper with the following text, I did not hesitate to call Sievert’s secretary to arrange an appointment:

> TECHNICAL PHYSICIST OR ELECTRONIC ENGINEER
> for job at research laboratory. Salary 10 000 – 15 000 Swedish kronor
> depending on qualifications and performance capacity.
> Please respond by calling 30 34 55.

The salary was obviously an annual salary. When the day arrived, I cycled up to the Karolinska Sjukhuset area where the Institute of Radiophysics sat on the hill diagonally down from Radiumhemmet, the famous cancer clinic. Sievert received me in his work room one floor up.

Everything there was dedicated to arousing awe and respect. Sievert sat at his big desk, designed by Bruno Mathsson. He seemed to me to be almost gargantuan and scary but soon proved to be friendly and interested. However, he was too curious about my personal circumstances and my hopes sank considerably when I appeared to have no relation to a number of Lindells he had hoped to meet. He then continued to intimidate me.

> ‘Working for me may be interesting but it will also involve a lot of crappy jobs. I expect my employees to be willing to fall in with what I require of them. That’s a requirement. Is the technologist willing to undertake any assignment I request?’
>
> I realised that I needed to display an ounce of courage.
>
> ‘No!’ I said, quite frankly.
>
> Sievert raised his eyebrows. I could not decide whether he was surprised or annoyed.
>
> ‘What do you mean by that?’ he rumbled.
>
> ‘I draw a line at murder,’ I said, and waited for him to explode. Maybe Professor Sievert was not one of those people who would stand for having his leg pulled. But it turned out he was. He started to laugh and his laugh made his big stomach shake like a landscape experiencing an earthquake.
>
> ‘Lindell...,’ he finally said thoughtfully. ‘With one ‘l’ or two?’
>
> I was not at all sure how far I dared to push it; I was not used to bantering with professors but thought I needed to assert my independence.
>
> ‘With three ‘l’s,’ I answered. Sievert nodded appreciatively.
>
> ‘The technologist’s got the job!’ he declared.

It was no small task that awaited me. On the first day, Sievert followed me to the high-voltage hall, a special building connected to the Institute of Radiophysics through a long, free-standing corridor with windows on both sides. From the corridor there was a door to an upper floor of the big hall. It led into a room that mostly resembled the captain’s bridge on a big ship. From that you looked out over the high-voltage hall through large windows. In the middle of the hall stood a Cockcroft and Walton cascade generator. At the side of that stood on plinths gigantic cylinder batteries which Sievert explained were condensers. The intention was to use the cascade generator to charge up the condensers to a voltage of 1.2 million volts and then discharge the condensers through a large x-ray tube that had not yet been built.

Sievert introduced me to three engineers, *Gunnar Eklund* (1925-1998) and *Bengt Håkansson* (b. 1925) who were roughly my age, and the workshop manager, *Axel Berggren* (1901-1998). We went down into the high-voltage hall and one floor further down to a lower room that ended up being called 'Gropen' or 'The Hollow'. In it stood something that resembled a large oven into which a stretcher-like bed could be pushed. The device consisted of a number of long pressurised gas cylinders, arranged to form a larger cylinder that surrounded the bed in all directions. Sievert explained that the cylinders were pressure ionisation chambers and that the purpose of the apparatus was to measure the gamma radiation emitted by the naturally-occurring radioactive substances in our body.

'International discussions are ongoing as to the size of radiation doses people working with x rays and radium should be allowed to be exposed to,' said Sievert. 'The problem is that we also receive radiation doses from cosmic radiation and from gamma radiation from the radioactive substances in the ground and in our own bodies. We don't yet know how large the radiation doses are but it's important to know,' he said, 'or else we'll have difficulty assessing how much extra radiation we can tolerate.'

'Radium in the body is what we are primarily interested in,' continued Sievert. 'But scientists disagree as to how much radium we normally have in our bodies. Early measurements taken by a German by the name of Krebs at the Kaiser Wilhelm Institute for Biophysics in 1939 and 1942 indicated that we have an average of 14 nanogrammes of radium-226 in our skeletons, but it is now said that two Americans in Rochester, John Hursh and a colleague, have found no more than one hundredth as much as Krebs! I now hope to use this apparatus to see who is right.'

Sievert's institute was called the Institute of Radiophysics, where 'radio' was synonymous with radiation and not radios, which sometimes led to misunderstandings when people wanted help with their wireless devices. It was an organisational hybrid. Since Sievert was Professor at Karolinska Institutet (the medical university), he had an institute but the funds he received for such were insignificant. However, Sievert's institute was also one of the two research institutes that belonged to King Gustaf V's Jubilee Clinic (the radiopathological institute was the second). The research institutes were owned by the Jubilee Fund, but Sievert's research went way beyond the Jubilee Clinic's area of interest and was paid for with a number of grants that Sievert was a master at obtaining, particularly from The Atomic Committee. Finally, the Institute of Radiophysics was the executive body for the Medical Board when it came to supervision and the issuing of permits for radiological work.

The regulatory section of Sievert's institute took on an essential share of the total work as regards both personnel and space, although in terms of staffing this was modest, with maybe twenty people when I began working for Sievert. I found out that the radiation protection supervision was divided into three departments. Department A, which dealt with x-ray diagnostics, including those of dentists, was led by Ph. Lic. Matts Helde, who was the first person to introduce me to the radiation protection inspectors. Helde also took me to the doctors' dining room, which was one floor up in the main building, right above the entrance where x-ray diagnostics subsequently had its premises.

Sievert's colleagues appeared to share a table with Radiumhemmet's doctors. Sievert never went there himself - he ate lunch at his home, but the main ones there were Benner, Helde and Thoraeus. From Radiumhemmet there was Professor of Gynaecology *James Heyman* (1882-1956), an ever-present lunch guest, together with some of the assistant doctors, primarily *Lars-Gunnar Larsson* (1919-2009) who in the 1960s would become Vice-Chancellor of Umeå University. On the first occasion I could also see a tall man with bushy white hair choosing food from the long table in the middle. It was the father of Swedish radiology, Professor Gösta Forssell, but I never had the chance to speak to him.

Department B, under Robert Thoraeus, dealt with x-ray therapy and the outpatient measurement operations initiated by Sievert in the 1920s to ensure a nationwide uniform radiation dose measurement. Department C consisted of Sven Benner, who supervised the radium sources and, later, produced artificial radioactive substances. Benner temporarily had the assistance of 'fil. mag.' (MSc) *Agnar Egmark* (born in 1914), an extremely unusual man from Värmland who was gushing with ideas and who had specific views on the way in which things should be done.

At the workshop, Axel Berggren was the boss of three incredibly talented instrument makers. The workshop was Sievert's baby and was very well-equipped with heavy workshop machines such as lathes and mills. You could trace Sievert around the institute by virtue of his cigar smoke, but if you wanted to be certain of bumping into him, all you needed to do was remain in the workshop for a while. This was where he took his numerous drawings of various measurement apparatuses and where he spent many hours expectantly following the work. His drawings were sometimes good enough to get the instrument makers to immediately understand what he wanted, but Axel Berggren usually cleverly transformed Sievert's ideas into technical drawings. Hans Weinberger wrote, in his book about Sievert:

Rolf Sievert and H. Moxnes in Sievert's work room

> As a designer, Sievert was impatient and impulsive. The ideas for a design usually came to him while playing patience at home. Every design was changed numerous times until he was satisfied with the result. Workshop manager Axel Berggren, who lived in a small job-related apartment at the Institute of Radiophysics, was usually called by Sievert late in the evening to give an opinion of the latest idea. After Sievert had convinced Berggren of the design, Berggren worked out a solution that would function in practice. Berggren says that the procedure was then reversed where Sievert acted as the 'prosecutor' and Berggren as the 'defendant' of the whole construction. If Sievert thought Berggren's answer was sufficiently convincing, the work continued until the product was finished. The shape of the instruments had to be aesthetically pleasing and the impression top class. What counted was the feeling created in the user. Sievert thought that if the appearance of the instrument inspired the confidence of the users, they would also have confidence in the function and treat the instrument with care. Many instruments were made black in colour, although chromium plating and frosted enamelling were also important elements.

The instruments and the furniture in Sievert's library and many other things were black and grey in colour because Sievert was totally colour-blind.

Sievert was assisted by another research engineer, *Rune Walstam* (1923-2002), who would eventually succeed him as Professor of Radiophysics. Walstam spent his time on the top floor in one of the rooms that did not come under Sievert's work accommodation. He helped Sievert with designs of small ion chambers carried by hospital personnel and was the person who eventually became the most active in devoting himself to medical physics at Radiumhemmet. The top floor also accommodated *Vera von*

Cronsteen (born in 1895), whose task it was to use an electrometer to read off the remaining electric charge on the ion chamber that was used to measure radiation doses on patients treated with radiation.

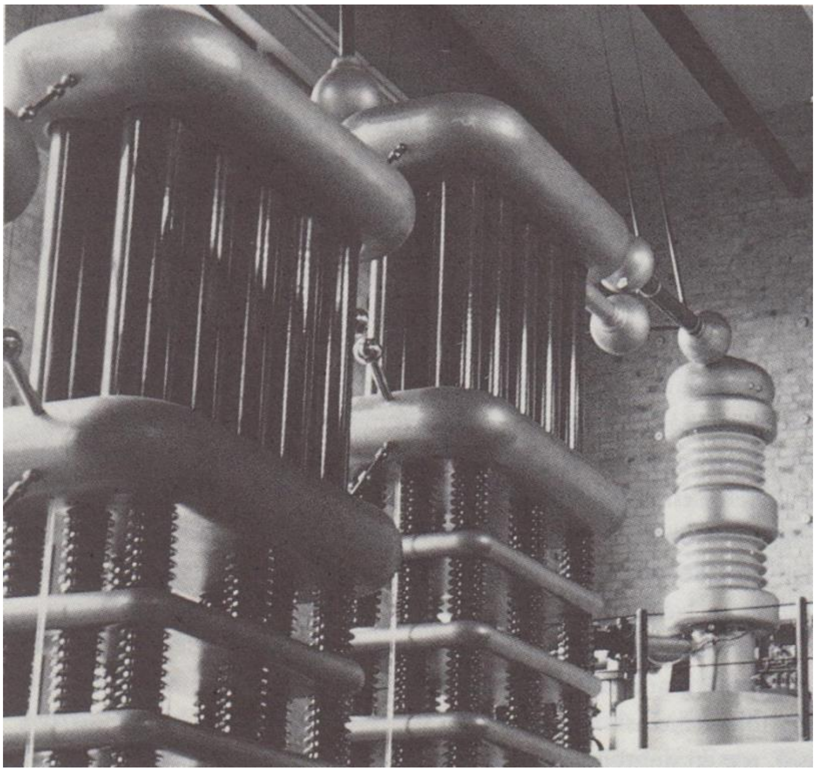
Sievert's work room was in the rooms one floor up where the x-ray department was housed. Next to this were rooms for two key people, Sievert's secretary *Torborg Hammarberg* (born 1913), and Sievert's handyman when it came to finances, administration, and personnel matters, *Svea Forss* (1919-1997). At the opposite end of the same floor was Arne Forssberg, Ph.D., and his radiation biology laboratory with two or three assistants.

At the end of the 1940s, Europe was not particularly accessible for tourist trips. Hitler's crushed Third Reich was occupied by the Allies and trials were taking place against the war criminals in Nuremberg. Glimpses of world events reached us through people visiting from there. One welcome guest was George de Hevesy, who often visited Forssberg and always had a friendly word to say whenever you bumped into him. In 1943, the same year as he moved from Copenhagen to flee from the Nazis, he had won the Nobel Prize in Chemistry for his introduction of the trace elements methodology.

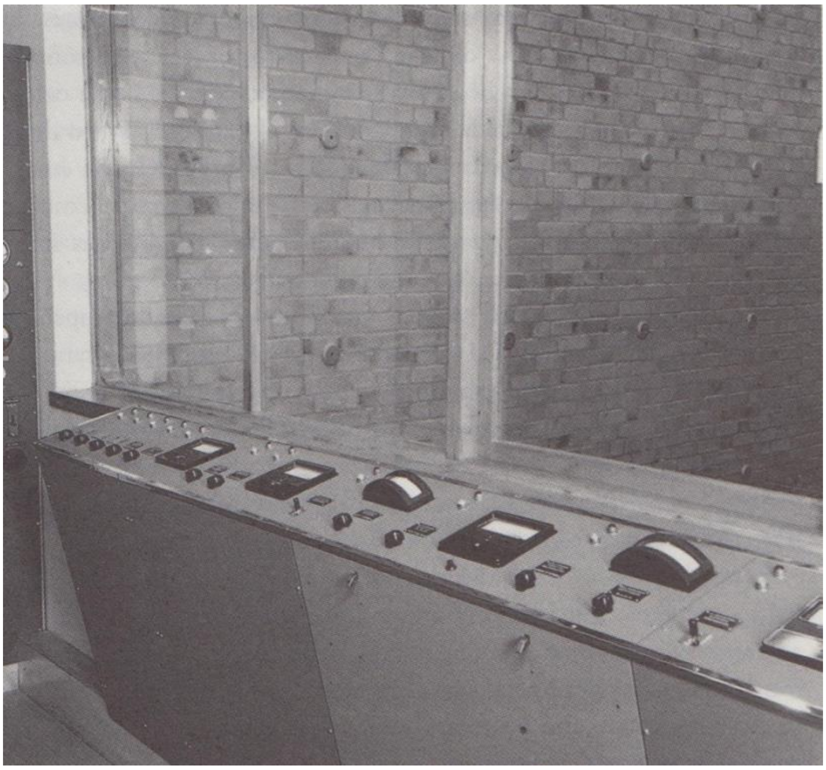
Sievert's Nordic colleagues often visited him after the end of the war. One of the first among them was the head of Norwegian radiation protection *H. Moxnes* and early on also his successor *Kristian Koren* (1911-1990) from the National Physical Laboratory in Oslo. Their equivalent from Copenhagen was *P. Rønne-Nielsen*. Robert Jaeger came to Sievert from Braunschweig (where it was no longer possible to talk about the *Physikalisch-Technische Reichsanstalt*, or Physical and Technical Institute of the German Reich; one had to make do with calling it the Physikalisch-Technisches Institut), pretty worn out after the end of the war. He and Sievert made music together at Sievert's country house in Tvartorp; Jaeger played the violin and Sievert the organ.

The war had obviously made Nordic contacts difficult and the population in the neighbouring countries were in great need. The Secretary General of the Nordic Society for Medical Radiology, the Swedish radiologist Carl Sandström (1914-1991), took a number of initiatives to make assistance collections and Christmas packages with food for Norwegian and Finnish colleagues. The first meeting of the Nordic Society took place in Copenhagen in June 1946 with major practical difficulties. In 1947, the Society met in Helsingfors, in 1948 in Oslo and in 1949 in Stockholm, with Elis Berven as President. There is nothing to indicate that Sievert actively participated in these meeting, nor to suggest that physical radiation problems were discussed.

Sievert gradually allowed me to work more independently. When Gunnar Eklund and I had connected and finished soldering the various instrument racks, it was time to place them in the greater context, in the control board on the 'captain's bridge' in the high-voltage hall where it felt like standing on a captain's bridge of a large ship



The high-voltage hall. The condenser batteries are in the foreground and the x-ray tube is in the background.



The high-voltage hall's control room

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Below the control room stood the tall Cockcroft-Walton generator which would provide a voltage of 1.2 million volts. The generator was a cascade generator and consisted of two towers of series-connected condensers. The towers were linked by as many rectifier tubes as condensers. The device was fed at the bottom by one of the condenser towers with a current from a transformer which emitted a high AC voltage and therefore raised and lowered the potentials of the condensers. It could be compared with the moving stairs that you find at some amusement parks, stairs that are divided into a left-hand and right-hand section that constantly move one step up and down. Correspondingly, the current was 'raised' to a higher potential and prevented from 'flowing' back since the rectifier tubes allowed only one direction of flow.

Using a boom that was vertically adjustable from the ceiling it was possible to connect the top of the cascade generator with the condensers. These formed four batteries, each containing ten condensers – tall, vertical cylinders, really gigantic ones. The four batteries stood on foundations at different heights so that the last battery was at the same height as the cascade generator, 6-7 metres above the floor. The intention was for the condensers, which were series-connected, to charge up to a high voltage and then instantaneously be discharged through an x-ray tube.

The x-ray tube was also colossal. 'The tube' was a huge, water-cooled iron cylinder with an internal volume measured in cubic metres. Inside the cylinder was an anode in the form of a large truncated cone surrounded by 144 tungsten filaments. The cone was suspended in a long metal rod which hung in a cylinder of isolators that stood on the iron cylinder and were several metres tall. Another boom, hanging horizontally from the ceiling, could be lowered so that its one end came close to the top of the isolator tower and its other end was close to the top of the highest condenser battery. When the distance was short enough, a spark flew over to the x-ray tube and a pulse of electrons from the filaments accelerated towards the conical anode. The x rays that arose when the electrons encountered the large anode, which was made of tantalum, could be released through the bottom of the iron cylinder in the form of a conical beam of radiation that was focused on the tip of the cone a little beneath the cylinder.

Sievert was trying to kill two birds with one stone. First of all, he wanted to investigate whether a very short pulse of x rays had any unusual biological impact. Here, the radiation from the x-ray tube would simulate gamma radiation from an atomic bomb explosion. Secondly, he sought to create a beam of radiation of a geometrical shape that would make it particularly suitable for the treatment of deeply-embedded tumours with radiation. For this he had got hold of a treatment table onto which a patient could be placed in the right position beneath the x-ray tube.

Everything was grandiose. The filament current through the 144 wolfram filaments was 3 000 amperes and came from a 16-volt submarine battery which stood in a special outbuilding outside the high-voltage hall in which whole the plant was erected. The current was led from the battery through thick copper bus bars. Even the switch and the remotely-controlled resistor that regulated the current were powerful tools in their own right.

Sievert had obtained 215 000 Swedish kronor from the Atomic Committee, a large sum at that time, for the construction of the high-voltage plant among other things. The big high-voltage hall was erected by building contractor *Anders Dunder* who made a contribution himself to the irradiation room by means of thick concrete walls that surrounded the lower section of the x-ray tube. The Cockcroft-Walton generator had been built by *Sieverts Kabelverk* in cooperation with the Nobel Institute, which would get a similar generator (a third one went to the 'thunderstorm professor' Harald Norinder in Uppsala). The condenser battery had been donated by *Sieverts Kabelverk*. Sievert's ability to get hold of whatever items he needed was unparalleled.

This gigantic plant was largely completed when I came to the institute in 1948 and completely finished for experiments in spring 1949. But Sievert had been too optimistic and had forgotten the law of nature which dictates that you can never count on something actually working. Axel Bergren and I anticipated turning the key and using the plant but we were not experienced in many of the problems that arose.

According to Sievert's drawings, the tower above the metal cylinder should consist of four large isolators similar to those one might see on high-voltage cables, ceramic cylinders with glazed rims on the outside to reduce the risk of leakage currents. Sievert assumed that everything would be available to order from the appropriate suppliers, but isolators as large as those he was thinking of were not to be found in the real world. So, I ended up having to travel to Iföverken in Bromölla to explain what we wanted. That was where I found out that no such large isolators had ever been made since the belief was that they would not have time to cool off without cracking after sintering at high temperature. However, convincing arguments led them to try an experiment, which luckily succeeded.

The use of the x-ray tube required high vacuum in the big volume within walls of iron and ceramics. This involved the use of heavy-duty vacuum pumps and long-term degassing, partly with the help of heating from the filaments. It also involved a considerable time delay each time the tube had to be opened.

The biggest problem was disruptive discharges due to evaporation from the surface of the anode which led to a considerable drop in voltage. We could get the x-ray tube to function as intended up to 400 kilovolts, but the radiation dose outside the tube was still too low for cancer treatment since intensity of the radiation was strongly dependent on the voltage. For biological experiments, we therefore replaced the conical anode with a cylindrical one that sat on a tube into which the object to be irradiated could be lowered. Inside the tube within the anode cylinder, the radiation dose at 400 kilovolts was also high enough for biological experiments. Here, FOA biologist *Arne Nelson* (1910-1993) could also irradiate mice and geneticist *K.G. Lüning* (1924-2004) could irradiate fruit flies. The experiment did not provide support for any appreciable difference in biological impact between the intense x-ray pulse and x rays from normal x-ray tubes.

Sievert was uneasy due to the setbacks. However, he continued to spend all conceivable resources with no regard to reasonableness or costs. I said at one stage that we ought to fit a shield ring next to the filaments to improve the field conditions in the tube. Sievert said that it had to be ready the next day and I claimed that that was absolutely impossible. We had a bet with five Swedish kronor. The next morning, Sievert triumphantly showed a shield ring that had been made at *Hemlins Mekaniska Verkstad* [mechanical workshop] overnight. He had afforded ample overtime payment and the workshop had remained open for half the night. Unfortunately, it also turned out to be a rush job in the sense that the shield ring did not fit into its intended spot. Both Sievert and I maintained that we had won the bet and kept our five kronor.

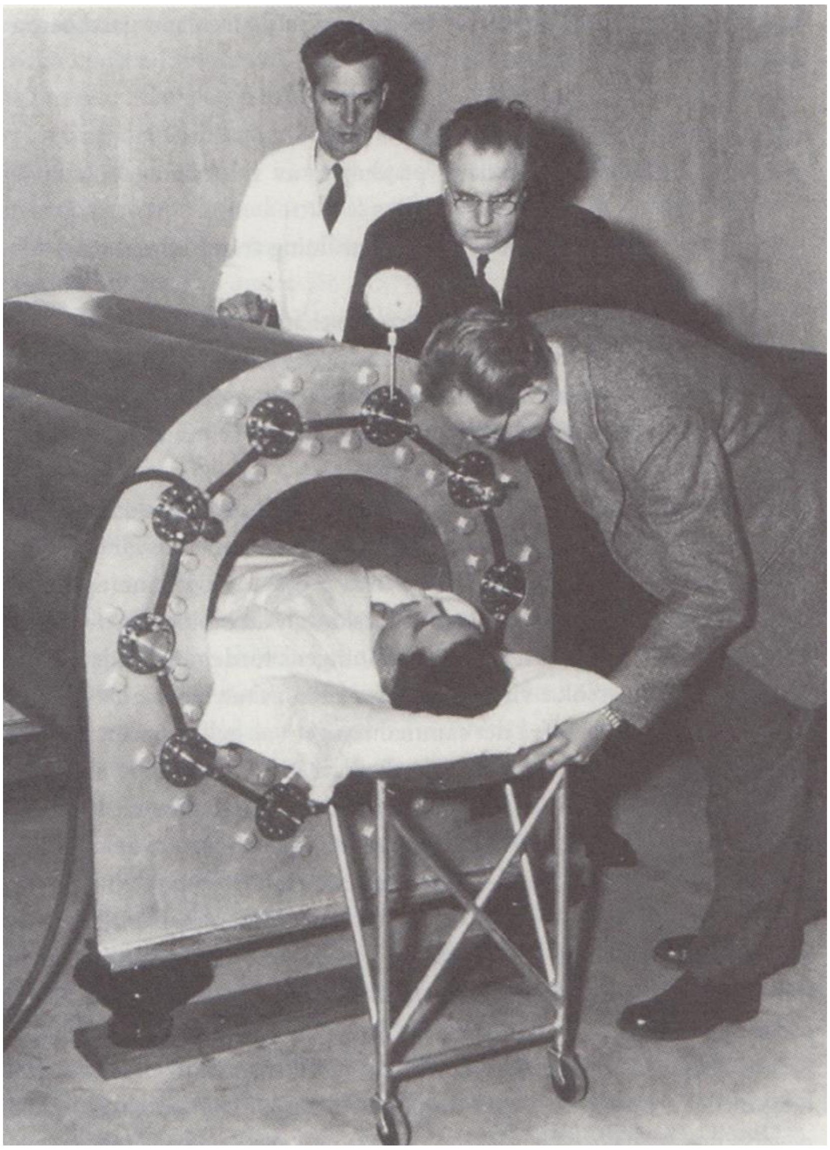
All the components in the high-voltage plant – the cascade generator, condenser batteries and x-ray tubes – were crowned by rounded aluminium casings to prevent corona formation. At the end of each day, Axel Berggren and I went around with earthed cables fixed to long bamboo rods and removed residual charges on the housings, a bizarre type of fishing.

In The Pit (the cellar), Bengt Håkansson experimented with the whole-body counter. At one stage, Sievert asked me to lie on the stretcher that could be pushed into the apparatus so that your body was completely surrounded by tubes containing high-pressure gas in which radiation from the radioactive substances in the body caused measurable ionisations. When I had been pushed in between the gas tubes, Sievert said:

> ‘What are 134 times 236?’
> ‘*Qué*??’
> ‘I want you to calculate the product of 134 times 236 in your head.’
> ‘How come?;
> ‘I want to see whether any radiation comes from your brain when you’re thinking.’
> ‘It can’t, surely. That’s inconceivable!’
> ‘The inconceivable is what wins you Nobel Prizes. Now calculate!’

No-one other than Sievert would have dared to put his prestige on the line by saying something so insane. Unfortunately, nothing radiated from my brain.

When military research had been moved to the recently-formed Defence Research Establishment in 1945, Sievert had been relieved of a significant responsibility and so, having extra time on his hands, had plunged into new projects. The high-voltage hall was just one of many. He also wanted to do research into the biological effects of small radiation doses and the impact of the distribution of radiation over time. He primarily wanted to look at which radiation doses humans usually received from natural radiation sources and, in this context, the whole-body counter was an important tool in The Pit. He had had the idea of using radon instead of radium for the local treatment of tumours from Failla in New York. For that purpose, he erected an ‘emanation laboratory’ (radon is also often called radium ‘emanation’). For this task he engaged Agnar Egmark and the laboratory expanded in a special extension on the other side of the long corridor past the high-voltage hall in the place where the building that would house the Radiation Protection Institute’s dining room was later erected.



A demonstration of Sievert’s first whole-body counter:
Bengt Håkansson acting as trial person and Bo Lindell bending over him
Rolf Sievert and workshop manager Axel Berggren in the background

Sievert was particularly interested in measurement instruments. He wanted to produce tools that would be able to detect radioactive environmental contaminants from nuclear weapon blasts early on, and he played day and night with ideas for the construction of measurement devices that could detect smaller and larger radiation doses under different circumstances, measurement devices that were all built around ionisation chambers.

Sievert was a master of handling ionisation chambers, and he had good colleagues in the shape of engineers who translated his outline diagrams into practical constructions and in the shape of the instrument makers who fulfilled the designs. Instruments of the type constructed at Sievert’s institute over a twenty-year period after the war have not been built anywhere else. The successful combination of Sievert’s reference knowledge and intuition, the skill of the engineers and instrument makers and the dry Swedish climate that did not disrupt the isolators of the instruments facilitated constructions that could not be realised in other countries.

The threat of a nuclear war affected many of Sievert's ideas. He made an early start with the development of measurement devices that could be positioned in different places in Sweden to detect and give off an alarm if there were any fallout from the radioactive substances. The first instruments were taken care of in great secrecy by Gunnar Eklund. This was the forerunner of the measurement stations (of a different design) which are now positioned around the country for the same purpose.

The thing that sometimes irritated those closest to him was the fact that Sievert saw the measurement problem as the main factor and designed his instruments on the basis of this. Whether anyone was interested in taking the measurements and whether they fulfilled any objective sometimes took second place. Some of the ingenious instruments therefore sometimes ended up on a shelf, forgotten. When the measurement problem was solved, the impatient Sievert lost interest.

The military were among those who Sievert thought ought to use his instruments, and he occasionally arranged demonstrations for this purpose. One important demonstration, when many very high officers, possibly even Supreme Commanders, had been persuaded to attend, took place in the high-voltage hall not long after I had been employed there. Sievert needed a radiation source to be able to show how the instruments worked. He therefore borrowed rather a large quantity of radium from Radiumhemmet, large enough for him not to want to let any of his personnel carry the radium down from Radiumhemmet to the high-voltage hall (a distance of maybe one hundred metres) despite the fact that the preparation had been stored in a lead container. Instead, he positioned his employees along the route at even intervals. As when a chain of people passes buckets of water to one another by a fire, those who were in position had to keep the radium container moving so that no-one carried it for more than a short while.

In the high-voltage hall, Sievert had positioned chairs for the visitors and the instruments stood in front of the chairs. 'Aren't you concerned for the military?' I asked. 'They have to sit close to the radium for quite a long time.' 'Ah,' responded Sievert, 'they'll tolerate it; they are soldiers after all and have to take risks!'

On another occasion, Sievert had borrowed a smaller radium preparation from Radiumhemmet, a flat preparation of ten milligrammes or thereabouts. He was going to use it to calibrate a measurement instrument up in his home on the top floor of the Institute. He had placed the preparation by an open window and the measurement instruments some distance further away. It was not that indiscreet since no-one apart from him had access to the room in question. When the instrument had been irradiated for a while, Sievert looked at everything and then discovered to his dismay that the small preparation had disappeared. It is still unclear to his day whether the thief was a squirrel or a magpie. We searched deep into the hospital park - Sievert's institute lay in Karolinska Sjukhuset's (the hospital) area as the Radiation Protection Institute does now but – but the radium was not to be found. I carefully examined my shoes for weeks afterwards to ensure that I had not trodden on the preparation for it to become stuck to the bottom of my shoe. It was not life-threatening but having such a flat preparation too close to your skin for many hours could cause a harmful skin injury.

Sievert's interest in Radiumhemmet and the medical physics that he himself had introduced and developed was after Second World War starting to decline and he began to see Radiumhemmet purely as a supplier of radiation sources for his experiments. He had just as little interest in education. As Professor at Karolinska Institutet, he had a small educational obligation which was mainly met through lectures for Radiumhemmet's doctors. He now convinced Benner and Thoraeus to hold lectures, and when I came onto the scene, the task fell to me. It was of interest to me since it forced me to familiarise myself with an area that was new to me. The interest shown by the doctors was also encouraging. This in turn meant that I began to write a 'radiophysics' compendium, of which Sievert took two hundred copies for interested radiologists. It was well received when it was completed in 1951, as there was still not much educational literature in the field.

Every Wednesday, Sievert invited the personnel to coffee and buns in the long corridor to the high-voltage hall. The buns were home-baked by the family of Sievert's housekeeper and there were still friendly relations – it was like a large family. During the 1950s, the atmosphere would change due to displeasure on the part of the radiation protection inspectors, but this was to come in the future.

Coffee on a Wednesday was not the only thing hosted by Sievert. Each year when the crayfish season came, he arranged a party with crayfish from his own lake and every year also a luxurious St. Lucia coffee with saffron buns, biscuits, Lucia and star boys, singing and ring dances around the premises with Sievert closely followed by the employees and their children of all ages. The radiation protection inspectors viewed the spectacle magnanimously and usually chanted: 'This is our solid ground, twenty ion pairs per second,' to show what they had to live up to.* Most of it was in the spirit of the ion chambers.

But storm clouds began to gather. The first one had appeared immediately after the war when the consequences of the 1941 Radiation Protection Act became tangible. To get the Act through Parliament, Sievert had had the idea that the supervisors of Sweden's x-ray apparatuses would not be a financial burden on the State if a charge were levied for each apparatus. The charge was not that noticeable in the budgets of the hospitals but it annoyed the dentists, who were a big group of several thousand professionals.

There was no possibility for the small number of supervisors at 'department A' to visit all dentists at regular enough intervals for the inspections to guarantee their safety against the risks of radiation. In the first few years, the inspections set out to evaluate x-ray equipment, gain experience of the possible weaknesses of different makes and study generally-used work techniques to find out what the greatest risk element could be. It soon became clear that the biggest risk was caused by the fact that the dentists had the bad habit of either holding the film in the patient's mouth themselves while taking the x-ray or getting a nurse to hold it. This meant that the same fingers were exposed to the primary beam of photons from the x-ray apparatus time after time. This led to a fair number of radiation injuries to the fingers of older dentists.

The greatest benefit from the inspections was to keep the radiation protection inspectors up to date with technical progress, which was something that did not need particularly comprehensive inspections (which would also not have been practically possible). Dentists who therefore had to wait years for a visit felt cheated as far as what they got for the annual charge was concerned. This dissatisfaction culminated in autumn 1946 when Sievert felt compelled to invite representatives of the dentists' organisations to a meeting at the Institute of Radiophysics. The meeting was reputedly quite tempestuous. The dentists claimed that the funds for 'a Professorship in Radiotechnology' were obtained by means of the fees paid by the dentists. Sievert replied that his Professorship was not in 'radiotechnology' and that it was paid for by the State since the latter received in return the required amount from King Gustaf V's Jubilee Fund. Sievert then added with all authority that the institute's officials had better bases than the dentists to assess the way in which the supervisory operations ought to be run.

The 1941 Radiation Protection Act prescribed obligatory medical examinations for personnel in radiology work. At such examinations, blood samples were taken which were analysed with regard to the frequency of different types of white blood cell. Around the end of the war, Matts Helde had studied the statistics that were available at the time regarding the examination results. Helde had examined what he considered to be the risks in various types of radiology work and thought it possible to divide the work into four risk groups on the basis of the radiation doses that the employees 'probably received' (there were no reliable dose measurements as yet). He then reviewed the examination records and calculated the frequency of major deviations from the normal values within each risk group. He then found a connection between the division into risk groups and the occurrence of blood changes.

While Helde was thinking this over, a public report was ongoing for the need for extended holidays for certain professional groups whose work was hazardous to their health. Sweden's first general legislation on holidays had been in the 1938 Holidays Act. However, this involved no distinction between different categories of workers. However, motions to the 1944 Parliament included a claim for an extension to the length of the holiday for certain groups of workers, including for mine workers in

* Air ionisation was referred to as being measured as the number ion pairs per cubic centimetre and second. An exposure of 1 röntgen gives $2.08{\cdot}10^{9}$ ion pairs per cubic centimetre of air. "20 ion pairs per second" therefore corresponds to $(20/2.08){\cdot}10^{-9} = 9.62{\cdot}10^{-9}$ röntgen per second, i.e., 0.3 röntgen per year. The reason why this particular exposure was considered to be a reference value has escaped me.

Norrbotten and Västerbotten and for underground mine workers in other parts of Sweden. The claim concerned an extension of the holiday to three weeks.

In 1942, a holiday committee had been formed. The committee was asked by the Second Legal Committee, which dealt with the motions, to examine the possibilities of generally differentiating between lengths of holiday. However, the Holiday Committee found it difficult to find objective grounds for assessing the danger of different types of work to the health. Letters were therefore written to medical officers, staff medical doctors and hospital doctors and asked them a number of questions. They were mainly looking to find out whether certain professional categories had higher morbidity, whether the work was more pressurised than other work, and whether the doctor thought that this justified extending their holiday. The result of this questionnaire is shown for a selection of jobs in the following table:

		Positive response per cent regarding		
PROFESSIONAL GROUP	NO. OF RESPONDENTS	HIGHER MORBIDITY	PRESSURISED WORK	EXTENDED HOLIDAY
1. Stokers on ships	19	63	90	90
2. Work with no daylight	23	52	78	100
3. Night workers	85	51	85	92
4. Darkroom workers	27	41	85	100
5. Radiology work	5	40	80	100
9. Underground mining	46	35	83	85
11. Producer gas workers	182	63	57	79
12. Nurses	529	16	88	91
24. Oven work in iron foundries	22	32	55	50
37. Boiler-making plant	10	10	40	30

Note that where radiology work was concerned, only five doctors had been asked and they were fully agreed that it would be good to have an extended holiday. Their responses clearly carried authority compared with those of the 144 doctors (certainly of a total of 182) who thought that producer gas workers also needed an extended holiday.

The secretary of the survey's confidence in the five doctors was strengthened when he contacted the Institute of Radiophysics and found out Helde's results. This was what he had been looking for for a long time: quantitative data that could be presented in the form of a diagram.

The Holiday Committee submitted its report with the proposal for a new Holiday Act on 15 December 1945 (SOU, or Swedish Government Official Report, 1945:59). The new Act came in 1946 and gave certain mine workers, night workers and darkroom workers and employees under the age of 18 an extended holiday of a total of three weeks. In addition, people in radiology work, i.e., specific doctors and nurses, were entitled to six (!) weeks' extended holiday. The later development meant that the general holiday soon made the extension to three weeks for the said groups meaningless. On the other hand, the 'radiological holiday' was important for a long time, at least as an employment benefit. A couple of decades later, it was realised that the importance of the holiday extension to reduce the risk of radiation injury was zero. However, Sievert's radiation protection inspectors were not backward in coming forward to take advantage of the new Holiday Act. When doing their inspections they could never be sure that they would not find themselves in a field of radiation, they said, and Sievert had no objections. The radiological holiday also later became a contractual matter, justified by the fact that it was beneficial against the risks of radiation. This made it possible to enjoy up to three months' 'radiological' holiday, which some of Sievert's radiation protection inspectors succeeded in obtaining.

Only then did Sievert start to grumble, and on one occasion when one of his employees was away for a whole summer, he complained: ‘Not only have I given them three months’ holiday, they’ve now had the nerve to actually take it as well!’

Arne Forssberg in the Institute of Radiophysics’ radiobiology laboratory

The discussions regarding the extended holiday meant that I started to become interested in radiation biology, which attracted me because I found Sievert’s biologist, Arne Forssberg, to be an extremely interesting sparring partner for most things, but primarily for radiation biology. Forssberg studied the effect of radiation on an algal fungus (*Phycomyces Blakesleeanus*) that was sensitive to radiation and examined whether there were any chemical counteractants, protective substances, against radiation injury. We discussed different theories on the importance of the dose rate and the classical target theory according to Timofeev-Ressovsky and Zimmer.

On 8 December 1950, Sievert called me into his work room and held a long monologue which I found so interesting that I wrote down everything I could remember immediately afterwards:

> As you know, I have had a great deal to do recently and am feeling tired. I have thought of taking longer leave of absence, one year at a time, and am proposing you as my substitute. Do the lectures take up a lot of your time? Would you have time to do your licentiate in the meantime?
>
> You see, I’m starting to grow old – although I’m not ancient as yet and not as arteriosclerotic as they think here. Nor am I slower in the head than before. You, who did not see me in my younger days, might think that I was cleverer then but I wasn’t. I’ve always been daft and was never interested in knowledge, but got on with things that could be done using common sense. Such things will now soon no longer be possible.

> Before, everything you took up within our field was new and there were no tricks involved in finding methods.
>
> I don't think I can stand the strain for more than five years - or maybe ten - God likes fat people and calls them home early. Since I've grown up with the institute, I want it to be left in good hands in the future, and I must look around me to see who can take over from me. [...]
>
> I dread to think what would happen if I conked out now and Thoraeus succeeded me rather than Benner. Benner's my big worry. I ought actually to retire now. I belong to the olden times and new times are coming, ones that require knowledge. I have no reason to stay and sabotage the development. I'd like to return to the country and enjoy a new youth and hunt and potter about doing all sorts. But I daren't when things are as they are.
>
> The fact that I have no knowledge might be because there's always been so much to do - organisation and letters for grants and buildings, and I've had so much to do with my private affairs. I'm now neglecting them completely, I don't care about them at all – relief for the poor has become so pronounced here in Sweden that I don't run any risk.
>
> I've noticed over the years how little money means. I grew up in a wealthy home with nice furniture and pictures and now such things no longer seem important – alright, so I have my summerhouse, but I think the best thing would be a room with sacking on the walls and a bed - a comfortable bed - and an armchair and maybe a desk. Those old monks weren't daft; they knew what they were doing when they withdrew to a monastery with a rope around their stomachs.
>
> If Benner had had a bit more fire in him I'd have dared to retire.
>
> I must now wait for a bit. Then Benner may become Professor for a few years, that is if you don't manage to get yourself qualified and apply at the same time. But then I'd prefer to know that there was someone who could step in, and I'd therefore like you to do a licentiate degree and then defend your doctorate; I think you have unique conditions to do this, helped by your technical qualification and your capacity for theoretical calculations. Then you'll be Professor in fourteen years. You're 28 now, so at the age of 42, which isn't bad. There probably won't be anyone who can compete with you. [...]
>
> If you deputise for me now for a fair time, a couple of years, that will also stand you in good stead. In the meantime, you'll be looking after the high-voltage hall but that doesn't take up much time, and the lectures. Do you think you'll have time to do a licentiate at the same time? I'll talk to Hulthén if you like. I think it's a good offer and you'd be paid my salary for it for a couple of years, which is something like 16 000 kronor, I believe.

I was surprised and embarrassed. I was very hesitant about Sievert's proposal; I realised it would mean a lot of work for me and I was almost unpleasantly concerned about the expectations he had of me. I did not think I had 'unique conditions' for continuing studies; I would have preferred to have had more free time with the possibility of writing about something other than physics and radiation. But Rolf Sievert was a person who implemented his plans and accepted no objections.

Sievert had underestimated his survival capacity; he lived for another sixteen years, not five or ten as he had anticipated. But his estimate of the time it would take for me to succeed him appeared to be just one year out.

And so the 1940s drew to a close in the spirit of the cold war and with a threatening nuclear war hanging like the sword of Damocles over great world leaders. The atomic age had arrived, for better or worse, but many people were optimistic and full of anticipation despite the unpleasant threat.

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In August 1945, the fateful atomic bombs were released over Hiroshima and Nagasaki. For the first time ever, one nation had used nuclear weapons against another nation, and the world would never again be the same.

Nuclear fission was discovered soon before World War II, changing Einstein's thesis that matter can be turned into energy into a most tangible reality. The enormous amounts of energy released by fission enticed scientists and military people into trying to create a doomsday weapon of hitherto untold explosive power. The 1940s were dominated by these efforts – primarily in the United States, the United Kingdom, France, Germany, and the Soviet Union.

In this process, scientists played a unique role by directly influencing world events. The enormous resources spent on atomic bomb research would also promote discoveries and improved methods at a rapid pace in many other sciences. Much of the current philosophy in radiological protection was founded already at that time, and many of the topics discussed today were debated then too.

The Sword of Damocles, the second part of four in this popular science history, is a direct continuation of Bo Lindell's *Pandora's Box*, and covers the time from the beginning of World War II until the end of the 1940s. It is aimed at persons with a general interest in radiation and requires no previous knowledge.

Professor Bo Lindell (1922-2016) had a degree in engineering physics and a PhD in radiation physics. Having worked closely with the radiation-protection pioneer Rolf Sievert, he took over as Director of the Swedish Radiation Protection Institute in 1965. He retired from that position in 1982 but remained an emeritus adviser until 2008. Lindell was Scientific Secretary and then Chairman of the International Commission on Radiological Protection (ICRP) and the Swedish delegate to, and for a time Chairman of, the United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR).

Lindell wrote this book series, his magnum opus, in Swedish. Aided by generous grants, the Nordic Society for Radiation Protection (NSFS) proudly presents this translation into English.