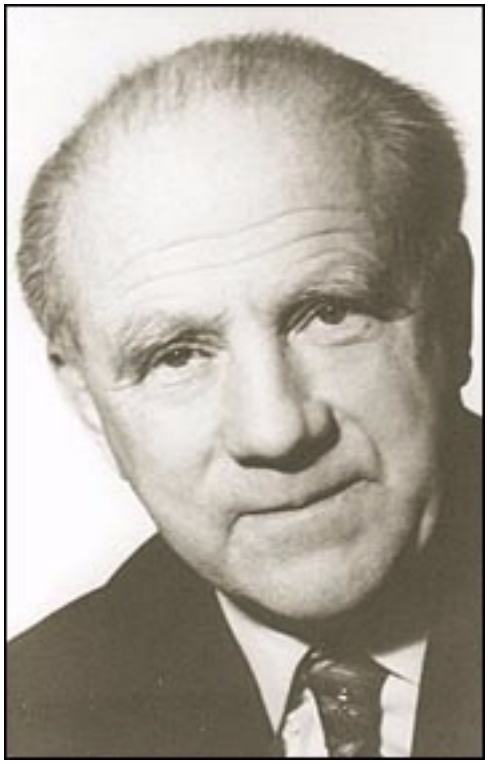


Werner Heisenberg (1901-1976)
Nobel Prize for Physics (1932)

For his work on quantum mechanics, the German physicist Werner Heisenberg received the Nobel prize for physics in 1932. He will probably be best remembered, however, for developing the uncertainty (or indeterminacy) principle, the concept that the behavior of subatomic particles can be predicted only on the basis of probability. Isaac Newton's laws of motion, therefore, cannot be used to predict accurately the behavior of single subatomic particles.

Werner Karl Heisenberg was born on Dec. 5, 1901, in Würzburg. He studied theoretical physics at the University of Munich and received his doctorate in 1923. From there he went to Göttingen to study under Max Born in 1924 and to Copenhagen, Denmark, to work with Niels Bohr. His original quantum theory was published in 1925-26 and his uncertainty principle in 1927. With Bohr he developed the principle of complementarity, a concept of measurement in physics that many physicists, including Albert Einstein, refused to accept.

From 1927 until 1941 Heisenberg was professor of theoretical physics at the University of Leipzig. During World War II he worked with Otto Hahn at the Kaiser Wilhelm Institute for Physics in Berlin on developing a nuclear reactor.
Secretly hostile to the Nazi regime, he worked to keep Germany from developing effective nuclear weapons. After the war he became director of the Max Planck Institute for Physics. He died in Munich on Feb. 1, 1976.

The Atomkeller-Museum at Haigerloch

Haigerloch, the small town at the border of the Schwäbische Alb, around 40 km away from the town of Tübingen, was the side, where the last German experiments on nuclear fission were conducted during World War II by the research group of Werner Heisenberg, Carl-Friedrich von Weizäcker and Karl Wirtz.

The Atomkeller is located directly beneath the Schloßkirche (see white arrow)

The beginning of all that: The discovery of the nuclear fission

In 1938/1939, Otto Hahn had discovered in Berlin together with Fritz Straßmann how to split the atomic nucleus of uranium. Important contributions were also made from Hahn's colleague Lise Meitner since 1907, who wasn't allowed to witness the success of their work in Berlin. But it was she who provided the decisive theoretical calculations from her exile which finally prompted Hahn to publish his discovery. We also want to remember the young female chemist Ida Noddack from the

University of Freiburg, Germany, who expressed in 1934 in the "Zeitschrift für angewandte Chemie" the conjecture of the splitting of the nuclei after the bombardment with neutrons. But she wasn't taken in earnest by Hahn and Fermi.

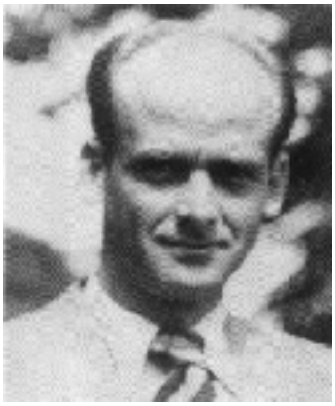
The danger of a bomb explosive

It was soon recognized that it might be possible to bring about a neutron chain reaction of such splitting with a subsequent release of a vast amount of energy. As early as the summer of 1939, Siegfried Flügge had published a newspaper article openly discussing these possibilities. When World War II began, the experiments were declared secret. They were turned over to the Army Weapons Office in Berlin and were looked after by Kurt Diebner, who got the Kaiser Wilhelm Institute for Physics involved in these experiments. Early on, Werner Heisenberg directed their work.

German nuclear research during the War

The total German research work during the War was concentrated on the construction of a nuclear reactor. However, Carl-Friedrich von Weizsäcker recognized early that a bomb could be realized with the Plutonium, that would be produced in the reactor and that could be extracted easily.

Carl-Friedrich von Weizsäcker



Karl Wirtz

Subsequent experimental work was continued by three research teams. One team worked under Heisenberg at Leipzig, a second team at the Kaiser-Wilhelm-Institut

in Berlin (W. Heisenberg, C.F. v. Weizsäcker, Karl Wirtz) and a thrid team was under the military command of Diebner from the Heereswaffenamt in Berlin-Gattow.

The decision

At a meeting at the Heereswaffenamt in February 1942, Heisenberg replied to the question, if there could be built a war-decisive weapon within nine months, with a clearcut "No". Thereupon, the Nazis lost theit interest in the project. The responsibility for the entire enterprise was withdrawn from the Heereswaffenamt and transferred to the Reichsforschungsrat (National Research Council). The control of this project was taken over by Walter Gerlach in 1944.

Danger of life

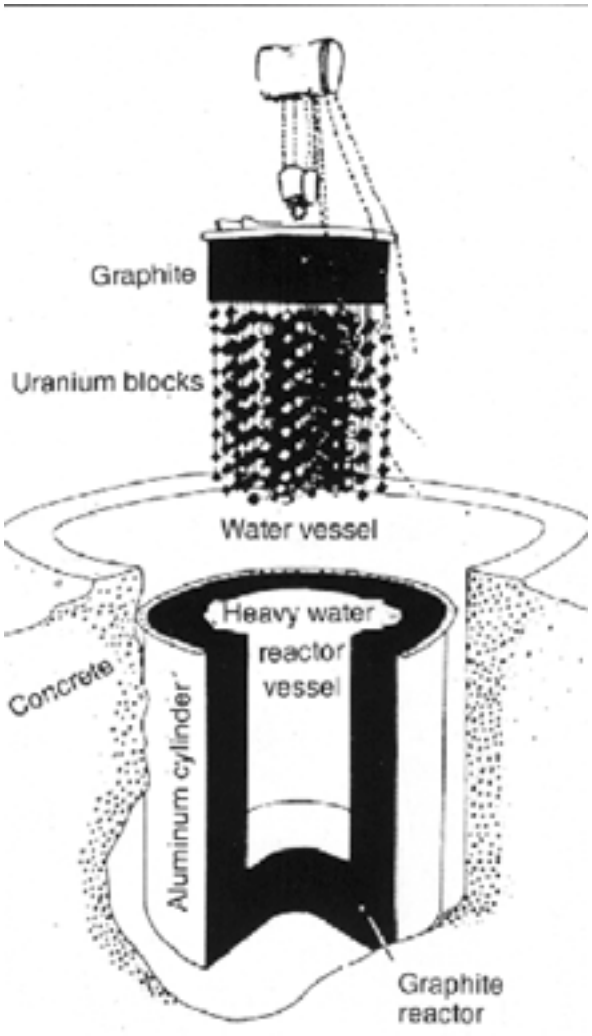
By 1943, the air raids on Berlin were becoming so intense that work became impossible there. An area was sought which was still relatively safe from air attacks. South-West-Germany had largely been spared from such attacks so far. It was also foreseen that in the event of occupation, hardly any Sowjet troops would penetrate into this area. The scientists by no means wanted to fall prey to the Sowjet Union as an occupational power.

The relocation of the lab to Haigerloch

Walter Gerlach, who studied physics in Tübingen and had been professor there, probably recalled the area around Hechingen and Haigerloch. He proposed building a laboratory within a bunker in the narrow limestone valley of the Eyach-River, since it was considered safer from air raids. During their visit to Haigerloch, it was pure coincidence that caused the scientists to find the rockhewn beer cellar of the "Schwanen Inn" and they could rent it for their work.
In an adventuresome trip by truck from Berlin to Haigerloch, the uranium and the heavy water was relocated from Berlin to Haigerloch, where the famous "B8"-experiment was able to be carried out at the end of March and the beginning of April.

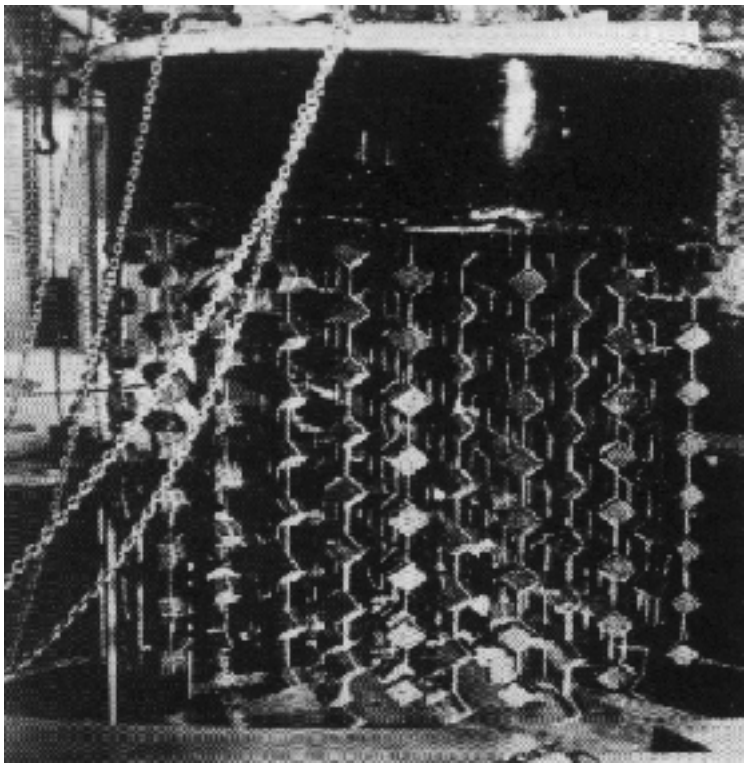
The setup

The reactor was located in a concrete cylinder. Between the outer concrete shell and the inner aluminium shell normal water was filled in for cooling purposes. The aluminium container had a diameter of 210 cm and a height of 210 cm, and contained another vessel made of magnesium. The space between the two vessels was filled with a 40 cm layer of graphitic carbon bricks. These bricks provided an external shield which prevented the escape of neutrons generated during the fission. The 664 uranium cubes (edge lenght of 5 cm) attached to the lid were then lowered into the inner magnesium vessel. It was a spatial grid, and the distance between the closest was 14 cm. Subsequently, the lid was bolted onto the reactor.



The execution

A neutron source was inserted into the center of the installation through the so-called chimney. There were openings in the top for the insertion of the so-called neutron probes to determine an exact measurement of the distribution of neutrons inside the apparatus, in the surrounding graphite bricks, and in the light water in the tank. Lastly, the heavy water was poured in slowly and carefully and the proliferation of the neutrons measured during the filling-up operation. If the reactor had become critical, the experiment would have been terminated.



The result

Empty: The neutron count was taken without uranium and without heavy water, but with the neutron source inserted, in the exterior filled with graphite and light water.

Full: The neutron count was taken in the exterior with uranium inserted and the heavy water poured in.

The ratio of the measurement with uranium, to the measurement without uranium and the heavy water, is called the multiplication factor. It came to about seven. By this, the reactor didn't become critical. Further calculations showed that a func-

tioning nuclear reactor would have had to be about 1.5 times the size of this reactor. However, expanding the reactor was no longer possible in April 1945 due to the lack of both heavy water and additional quantities of uranium blocks.

The end of research work at Haigerloch: The ALSOS-Mission

Shortly after conducting the last experiment, Haigerloch, located within the French zone of occupation, was occupied by a special American Task Force. This was the so-called "ALSOS-Mission".

Colonel Boris Pash (on the left)

Colonel Pash, the commander of this unit, took the scientists prisoner in their offices and private homes at Hechingen. Shortly before this, Heisenberg had fled by bike to his family in Bavaria, but was taken prisoner some time later. The Americans found the Uranium metal and the Heavy Water, which was hided shortly before by the German scientists and was dug up. They also dismantled the facilites in the cellar and took them to the US.

The blowing up

The American Forces had been ordered to blow up the cellar. The then parish priest took Colonel Pash into the baroque Schloßkirche directly above the cellar, and explained that the destruction of the cellar would also mean the destruction of

the church. Knowing this, the Americans confined themselves to limited demolition operations in the cellar.

The detention

The scientists were interned in the "Farmhall" country house near Cambridge by the Allied Powers and kept prisoners there until January 1946 ("as guests of His Majesty, King George VI"). Their conversations during this time were recorded by the English secret service. While prisoners, they heard a BBC news broadcast in August 1945 that the Americans had dropped two atom bombs on Japan. This terrible news and the deaths of so many people resulting from the bombs caused much concern among the scientists. Hahn was suffering very much from these terrible events. In January 1946, the British released the German scientists.

Parts of an Interview with Werner Heisenberg on Nuclear Energy Development in Germany during World War II

Conducted and edited by Joseph J. Ermenc, Professor Emeritus, Dartmouth College, from tapes recorded at Urfeld in southern Bavaria, on August 29, 1967. (cf. references)

HEISENBERG:
Very soon after that event there was a meeting in Berlin at which we had to tell the whole story to the officials. Irving has described this in his book, "The Virus House". You know the Irving book, don't you? I think it has been very carefully done. He studied all kinds of sources: documents and so on. I think he did it pretty well.
ERMENC:
I think the conclusions are rather superficial but it is full of facts.
HEISENBERG:
I would say that ninety-nine percent of all facts are correct. It is a very careful book. I wonder if you have read the interview I had with representatives of `Der Spiegel`? When Irving's book first came out, `Der Spiegel` had a rather extensive report on it. Afterwards they asked me whether I would be willing to give them an interview about the book and say where I approved of it and where I did not. I think that Irving's book is a very good book in the facts. But it has one deficiency. When he

tries to determine motives he does not do very well because he can not really think himself into the atmosphere of a totalitarian country making war. That atmosphere was so totally different from what he had experienced in his life. He could then not quite avoid the error of using cliché thinking. He thought it was obvious that there was competition to make an atomic bomb - there was a race between Germany and America to be the first to make an atomic bomb. This is so obvious to him that he cannot get away from this idea. But this was not the situation. The situation psychologically was different. It was a new situation for us scientists in Germany. Now for the first time we could get money from our government to do something interesting and we intended to exploit this situation. The official slogan of the govern-ment was: We must make use of physics for warfare. We turned it around for our slogan: We must make use of warfare for physics. It was a very natural reaction. Everybody likes to make interesting experiments. We felt already in the beginning that if it were possible at all to actually make explosives it would take such a long time and require such an enormous effort that there was a very good chance the War would be over before that could be accomplished.

When we came to the point of the successful L-4 experiment, when we knew that we could make reactors, and when we knew from Weizsäcker's paper that you could in this way make plutonium or something like it, we knew that in principle we could make atomic bombs. But still we did not make a serious effort in this direction. Now let's talk seriously about this. If we wanted to make the necessary heavy water, it would take one to three years to get a sufficient amount. To make enough pluto-nium would also take another three years. So with the best conscience in the world we could tell our government, "It will not be possible to make a bomb within five years or so". We knew already that they would forbid any new developments which couldn't be used within the next year or so. It was then quite clear that they would say, "No, no! No effort for the atomic bomb".

That was what happened. That made us very happy. I would say this was a very fa-vourable situation for us. First of all we could get money for doing experiments. Then we could probably make reactors because that should now be possible by simply enlarging L-IV.

ERMENC:

I have noted that as early as December 6, 1939, in your report to the War Office, you practically outlined the whole program of uranium utilization.

HEISENBERG:

That was the first theoretical paper. You simply had to use the figures that were available. Most of the figures were nearly correct and you could see how things

would go. But then we had this drawback: two figures had to be checked; one was the neutron absorption coefficient of heavy water and the other was the absorption coefficient of pure carbon. There was perhaps a bit of competition to do this. Döpel fought for doing the heavy water experiment and Bothe fought for doing the carbon experiment. Bothe's Heidelberg people thought it was nicer to do experiments with a larger quantity of material; if you only have a few liters of substance, as was the case with the heavy water, you will have a very small absorption and the chance that you will make a mistake is rather big. We got the heavy water; there was no more than roughly eight liters of heavy water available in the whole of Germany at that time. The Heidelberg people got about a ton of carbon. Then we thought very hard in Leipzig about the kind of measuring equipment we would need to get a pretty accurate determination of the absorption coefficient even with a very small amount of heavy water. It took quite some effort to think out how things should be arranged to obtain reliable values. It was an unusual experiment insofar as we put the sources of neutrons in the middle of a sphere of heavy water; then we measured the absorption of the neutrons into the middle of a sphere. The experiment, which was carried out by Döpel very carefully was a success. The results which we got with this small amount of heavy water were practically the same as the values which your people obtained later on with very large amounts.

ERMENC:

How closely could you duplicate the results of this experimental work? One percent?

HEISENBERG:

I think it was better than that. We gained an accuracy of the order of one per mil. That was necessary in order to get a useful result. It was a very refined measurement. We had two spheres - two bottles of aluminum. One bottle was covered with cadmium and the other was not. Otherwise they were completely equal in size. We had the source of neutrons in the center. It was the diffusion of neutrons from center to the outside which we measured. That was a very sensitive measurement. We got quite good values as I told you.

ERMENC:

How were these reports received by your colleagues in other institutions?

HEISENBERG:

Of course they were interested. Everybody said, "This is all very nice. Now we can try to build reactors." So the next steps were to improve the geometry and to improve the purity of the material.

ERMENC:

Were more funds made available to you? Was there any pressure put on you to go faster?

HEISENBERG:

At this meeting held during the summer of '42, we hesitated very much to ask for too much money. We felt that if we did it would create too great excitement. If we had said that we could make atomic bombs then it is possible that we would have received orders to build factories to make atomic bombs. This would have been the last thing we wanted. We knew this could not be done within the next two or three years and the War would be lost by that time anyway. So we were very careful in speaking about atomic bombs. But we did say that they could be made. This we had to say, to be safe, in case the Americans would actually drop atomic bombs; this was a possibility. Actually I think it was Mr. Milch, during the meeting, who asked, "How long will it take the Americans to make this thing if they go for it?" After some discussions among our group, we mentioned the following figures to Milch: The Americans could not possibly have a reactor going before the end of '42 - which was a very good guess - before the summer of '44. This was on the optimistic side. We wanted it definitely to be on the short side, because if they actually would drop an atomic bomb it would be better if we had given warning. Among ourselves we believed that they would not be able to make atomic bombs before three or four years.

ERMENC:

Why wasn't there more interest in graphite knowing that heavy water was very scarce ?

HEISENBERG:

It was because of the experiment of Bothe's on graphite which was not correct. Bothe had made a measurement of the neutron absorption coefficient of pure carbon and an error had slipped into his experiment. His values were too high but we assumed they were correct and so we did not think carbon could be used. Afterwards we knew what his error probably was. He had built a pile of graphite pieces but in between the graphite pieces there was always some air and the nitrogen of the air has high neutron absorption. Somehow he must have forgotten this. I don't know why but it's understandable.

ERMENC:

Didn't others run similar experiments on carbon to check Bothe?

HEISENBERG:

There were so few groups that we never did an experiment twice. Every group had some job to do. We in Leipzig had done a measurement of heavy water and after that time everybody believed our figure for heavy water; nobody checked it. Nobody checked the measurement of Bothe. We intended in the later development of the thing to use carbon for shielding around the reactors.

ERMENC:

This is what you did at Leipzig?

HEISENBERG:

No, we did this at Haigerloch in South Germany. There we wanted to get carbon as shieldinq for the outside because we realized that carbon was much better than light water. But then there was not enough carbon and it was difficult to get. So not much was done with it. The carbon line was really ruled out by the experiment of Bothe. But still if we would have gotten a carbon reactor going what would have happened next? Would we then have aimed for the atomic bomb? There the answer was also clearly, no. We would not have had this carbon reactor going before say the end of 1943 or the beginning of 1944. If we could have had the reactor going one could say, "You could have made plutonium". But to build a reactor which actually produces enough material for bombs would have been an effort again of two or three years at least. At the beginning of '44 nobody would have thought of doing such a thing. Already in the summer of '42 the leading people in Germany knew that they had to win the war within the next half year if they could win it at all. Their most serious mistake was that they didn't give in after that time.

ERMENC:

After '42 some of you in Germany began to think of the development of the uranium reactor as being very important in postwar development as a source of energy. Was this a driving force at that time or was it still in the curiosity stage?

HEISENBERG:

We knew such a chain reaction could be made. Therefore we could plan to use this reaction for power plants for submarines and other applications. We felt that this was now a very important development in technics, engineering, economics and so on. Whatever the outcome of the War, we felt that we should be in this development after the War. We felt this was really a nice task which we could do during the War. We didn't know what you people would do but we felt it was quite possible that during the War the Americans also would say that atomic bombs are not interesting because they can't be ready before the end of the War, and that working on energy production would be a very interesting development for peaceful use. The ex-

plosive side of the problem could be done after the War when one would have more time.

ERMENC:

I suppose that at this time talking about post-war development and nuclear power wasn't a very effective argument for supporting wartime science?

HEISENBERG:

Yes, but why not ? Apparently, in your country, one didn't think of a quick end of the War. I must say I always felt that the War would end earlier than it actually did. I also was convinced that Germany would lose the War and so the problem of the War didn't interest me too much any more. I was interested in what came afterwards. But in your country, apparently one had the impression that one still could use the bombs during the War. This did not work out for the War between your country and Germany, but it did work out with Japan. This is a point which I also made in the interview with `Der Spiegel`. The decision of our government not to make bombs was a very sensible decision. It would have been sensible even for your government because you would have won the War against Germany earlier if you had made no atomic bombs. There 's no doubt about that because then you would have put this whole effort into airplanes and tanks and whatever else, and the War would have been ended earlier. This may not be true for Japan since the war against Japan was a different matter. But speaking only for the War against Germany I think this is a fact. I can also understand that during war one argues in different ways. One wants to be as strong as possible at the end of the war. One can very well argue for trying such a thing. But we felt there was a fifty-fifty chance that perhaps the Americans would also argue in the same way as we did when we said that during the War these things could not be developed any way and we would leave it for a later time.

ERMENC:

I think Irving mentioned that at the meeting, when you and Weizsäcker were asked how much you would need to continue your work, you mentioned a sum which the officials thought was ridiculously low. In consequence they said that this work could not be important because it doesn't cost very much. Is that correct?

HEISENBERG:

This is perfectly correct. Yes. It was mainly Speer who reacted this way. This was of course a very clear intention of ours. We had to avoid being committed to make a big effort making atomic bombs. What we wanted was to get just enough money to go on with our reactor project, but no more than that. We were very much afraid that otherwise someone would say, "Now let's go for the atomic bomb". Also I read in

`Der Spiegel` - you really should read these editions of `Der Spiegel`- an interview they had with Speer. You know he has been released from Spandau. He is now an old man. He was asked by `Der Spiegel` people:

"You took part in this meeting when the physicists told you about the probabilities of atomic bombs . What was your reaction? "

He said, "We listened when they told us that in principle atomic bombs could be made, but they also emphasized that it would take a number of years; certainly not before five years or so. So I felt"- I think he expressed it in a funny way - "there was not much music in the thing. Therefore, I didn't report the whole thing to the Führer until two weeks later or so and then in a very casual way because I did not want the Führer to get so interested that he would order great efforts immediately to make the atomic bomb".

Speer felt it was better that the whole thing should be dropped, and the Führer also reacted that way. This side of the problem clearly worked out as we had hoped it would. We definitely did not want to get into this bomb business. I wouldn't like to idealize this; we did this also for our personal safety. We thought that the probability that this would lead to atomic bombs during the War was nearly zero. If we had done otherwise, and if many thousand people had been put to work on it and then if nothing had been developed, this could have had extremely disagreeable consequences for us.

ERMENC:

Considering this development beyond the demonstrations of a sustainable chain reaction, did you consider that after this it probably would have been taken out of your hands very quickly?

HEISENBERG:

After the War?

ERMENC:

No, even during the War if you had gone ahead. In our country I know there was some resentment among the scientists in Chicago because they didn't control the project any more after they had proved the principle.

HEISENBERG:

This was also one of the main points we discussed especially among a small group including Weizsäcker, Wirtz and myself. We felt it as very important then these things must remain in our hands, then we could always keep control of what goes on. That we did achieve. Of course, we could only achieve this by not making bombs. If we would have said, "Now let's make a big effort for the atomic bomb", it certainly would have been taken out of our hands.