MODERN NUCLEAR TECHNOLOGY

A SURVEY FOR INDUSTRY AND BUSINESS

Editors

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 Library of Congress Catalog Card Number 59-15058

42393

Dedication

To those of us who knew him well, the late Dr. Mark M. Mills left within us a feeling of gratitude for having been associated with one who will always be remembered as contributing so materially to the defense of the United States and of the free world.

A mild-mannered man with a keen sense of humor, he possessed the quality of attracting friends wherever he went. His devotion to work was never failing, and perhaps because of this he met an untimely death. On the night of April 7, 1958—while this text was still in preparation—the helicopter in which Dr. Mills was making an inspection trip crashed near one of the islands in the Eniwetok Atoll during a rain squall. Dr. Mills was at that time responsible for an important special mission relating to the HARDTACK nuclear weapons test series at the Eniwetok Proving Ground.

A brief glimpse at his biography reveals his outstanding qualifications. He was born in Estes Park, Colorado, on August 8, 1917, and received his higher education at the California Institute of Technology, where he obtained a B.S. degree in 1940 and a Ph.D. degree in 1948. In 1942 he became a leader in the development of solid rocket propellants at the Jet Propulsion Laboratory, Pasadena; he returned to the Institute in 1945 and continued work on jet propulsion until 1948, when he went to North American Aviation Corporation to become a member of a special research section studying various types of reactors. This section became one of the nuclei for the establishment of the Atomics International Division.

After spending a year at the Forrestal Research Center at Princeton University, he returned to Atomics International in 1952 as a staff specialist. In 1953 he joined the staff of the University of California Lawrence Radiation Laboratory at Livermore and became head of the Theoretical Physics and Mathematics Division. He was made an associate director in 1956 and up to the time of his death was deputy director of the laboratory.

Dr. Mills also served as a member of the Nuclear Fuels Propulsion Panel of the U.S. Air Force Scientific Advisory Board on Evaluation of the Aircraft Nuclear Propulsion Program; was appointed to the U.S.

Atomic Energy Commission's Reactor Safeguards Committee and served as a member of the Advisory Committee; was a member of the Advisory Committee on Atomic Energy of the Secretary of Defense; and both as a consultant and friend, was instrumental in guiding Aerojet-General Nucleonics.

Another accomplishment, outstanding in itself, is that through Dr. Mills's efforts a formal curriculum in nuclear engineering was initiated at the University of California. He devoted much time to this program and, in conjunction with his duties at the University of California Lawrence Radiation Laboratory, was Professor of Nuclear Engineering in the Division of Mechanical Engineering at the university.

Arthur T. Biehl
Robert Mainhardt

Preface

The material contained in this book is, for the most part, a result of a series of lectures given under the Engineering Extension Program of the University of California in Berkeley during the summers of 1956 and 1957. This course, entitled "A Survey of Nuclear Engineering for Management," consisted of lectures presented by approximately twenty guest speakers over a period of one week.

The purpose of the survey course was to present on a management level the fundamentals of nuclear engineering, in order to help management personnel understand some of the problems which are inherent in this new and rapidly growing part of our economy. The level of presentation was directed toward the mature industrialist who, it was assumed, had an engineering degree but had not been actively engaged in engineering for a period of five or ten years or who was part of management and had sufficient experience in the administration of engineering programs. Therefore, no lectures were given on basic engineering principles or methods of approach for the solution of engineering problems. Accordingly, many of the lectures (which are chapters in this book) may readily be considered too unsophisticated for the nuclear expert or the engineer actively engaged in reactor design or operation. Similarly, a person without experience in other fields of engineering may find the lectures too advanced or too "scientific." The editors hope, however, that the balance between being too technical and not technical enough has been satisfactorily attained and that the book will therefore yield information of value to management. It should be noted that, for the most part, each chapter stands by itself, since it corresponds to a lecture given by an individual; in editing this book, little effort was made to cross-correlate data between chapters.

Nuclear engineering as practiced today uses a confusing set of units and introduces some new vocabulary. Historically, the units have been developed by the physicists; therefore the metric system—kilograms, centimeters, ergs, etc.—is commonly used in nuclear engineering, while the older field of heat transfer, also of importance in nuclear engineering, maintains the more familiar English units—Btu per square foot per hour, inches, etc.

Vocabulary is even more important in that one must acquire these new words which have crept into our language to keep abreast of the literature and for informative discussions with persons competent in the field. But this simple study of a glossary of these words is insufficient to acquire a working use. This can be attained only by working in the field, by discussions, and by reading material in which the words are used in their proper context. This book, then, should be of service to those who wish to start acquiring this new nuclear engineering vocabulary.

The editors are indebted to Dean Emeritus Morrough P. O'Brien of the Engineering College of the University of California; to Mr. Kenneth L. Downes, who administered the Nuclear Engineering Extension Program, and to Mr. Donald C. Bryant, Program Coordinator; to Professor Thomas H. Pigford, Chairman, Department of Nuclear Engineering, for his helpful and general comments on the book; and to the late Dr. Roy W. Goranson, Mrs. Louise Crouch, Mrs. Donna Rogers, and Mr. Raphael J. Jaffe for their invaluable aid in preparing the manuscript.

Arthur T. Biehl
Robert Mainhardt

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INTRODUCTION

An Informal History of Atomic Energy

EDWARD TELLER

Half a century ago a chemistry professor at a Canadian university looked at a paper by a faculty member. He was deeply shocked. The paper suggested that elements could be transformed into each other. The old and respectable science of chemistry had been built on the firm axiom of immutable elements. To call this axiom into question was certainly not consistent with the dignity of one of the leading universities of Canada.

The person who wrote and published this disquieting paper was a young man from New Zealand by the name of Ernest Rutherford. About 30 years later another unconventional person, Leo Szilard, went to see the first Baron Rutherford of Nelson. Szilard suggested that one could use atomic energy for some practical purpose, for the production of power. Rutherford (who had, meanwhile, made many wonderful discoveries) threw Szilard right out of his office, in the style of the old chemistry professor.

Szilard, however, wrote down his ideas at that time in the form of a patent. This patent outlines all of the field that we now call "atomic energy." [1]

Szilard said that atomic energy could be produced with the help of three kinds of atoms—beryllium, thorium, or uranium. He said that the way atomic energy could be made to work in a practical sense was that the nuclei of these atoms could somehow be made to split, that during the

[1] British patent no. 630726, application first filed June 28, 1934, specification accepted Mar. 30, 1936, finally published Sept. 28, 1949.

splitting of these atoms neutrons might be emitted, and that these neutrons could then enter other atoms and cause them to split.

Szilard pointed out that in this way a chain reaction could be started, and the condition needed for this chain reaction is that one neutron yield more than one other neutron in the splitting process that it causes. In order for this divergent chain reaction to proceed, it is necessary to have a reasonably big amount of material—a critical amount. If one has less than a critical amount, the neutrons (which pass through matter with relative ease) will escape in sufficient numbers so that the chain reaction will not be propagated. Szilard even went so far as to describe the distribution of neutrons in such a critical amount.

All of this is really very simple and straightforward. The one question is: Why did he choose these three atoms—beryllium, thorium, and uranium—at a time when fission was not yet recognized? And why, indeed, did he include beryllium, which cannot be used in chain reactions?

The reason he did all this was Einstein's relation, $E = mc^2$. This relation everyone has seen—on photographs of scientists' blackboards, if not elsewhere. Szilard had noticed that uranium, thorium, and beryllium were a bit heavier than they had any business to be. They contained so much energy that they were ready to blow up. They might need only a little additional tickling to make them disintegrate. It has turned out that this indeed is precisely what happens with uranium and thorium but not with beryllium; in the case of beryllium, the measurement on which Szilard based his conclusions happened to be slightly in error.

Now the amusing fact is that at the time when Szilard took out a patent in London, Fermi in Rome had already produced fission, but Fermi did not know it. When the neutron was discovered (it carries no charge and therefore can approach any nucleus quite easily) it had at once occurred to Fermi that the neutron could be used with the greatest of ease to bombard other nuclei and produce many artificial transmutations. With a number of collaborators, he set out to shoot neutrons on every atom, every nucleus in the periodic table. In so doing, he found a great number of new radioactive species, many of which are now well known and used in various experimental techniques. He studied these and went on to study in very great detail the properties of the neutrons themselves.

Neutron Reactions

The properties of the neutrons are a little bit puzzling. You just do not handle them as you handle ordinary things. You cannot keep them in a bottle, because they go through any piece of material with considerable ease, their range in most materials being several inches. Neutrons can be produced by several nuclear reactions and they move with considerable speed—say one-twentieth the velocity of light. The behavior of these neutrons, however, was puzzling in some cases. For instance, Fermi and company found that experiments made near a window would turn out a little differently from those made in a darker portion of the room. This was puzzling because light could not have any connection with the phenomenon.

Then one day during lunch, Fermi said: Now I've got it! And he said it was exceedingly simple, because the table near the window was wooden, but the other table was metal. Neutrons colliding with hydrogen atoms—with hydrogen nuclei—will with each collision lose half their energy. In 10 collisions the neutron will reduce its energy, on the average, to one-thousandth; in 20 collisions, to one-millionth; and in about 25 collisions the energy of the neutron will come down from its original energy of 1 or 2 million ev to the value of approximately one-fortieth of an ev, which is the energy that all particles have because of thermal agitation. And in this way you can transform fast neutrons into slow neutrons.

It was recognized by the experimenters, and understood with the help of the wave nature of the neutron, that the slow neutrons would interact in an entirely different way from the fast neutrons. The fast neutrons interact with all nuclear species rather indiscriminately, although the result of the reaction is not the same in every case. In some cases the neutron has a chance of adding itself to the nucleus; in some cases, it induces the emission of a particle; in many other cases the neutron simply loses some energy and leaves behind it a nucleus in an excited state.

If, instead of fast neutrons, slow neutrons were used, then in most cases only one reaction could take place. That reaction is the capture of the neutron. The neutron is added to the nucleus and excess energy is carried away by a gamma ray. The reaction is almost always the same, but the possibility of the reaction is vastly different from case to case. Some nuclei react with slow neutrons even if the neutron is at a considerable distance. It is as though the nucleus could suck in the neutron. This is explained by the wave nature of all matter, which becomes particularly important when the particle moves with the lowest speeds.

The result is that most capture processes are observed when slow neutrons are present. These processes predominated on the wooden table, which contained a good deal of hydrogen and moderated the neutrons well. They also became more important when Fermi bent over the apparatus at the right time. No wonder that the experimental results seemed capricious. To verify the moderation theory, Fermi actually dunked both source and sample into the fishpond behind his laboratory. An increase in induced reactions was indeed observed.

Szilard had understood, many years before fission was discovered, the principles of the divergent chain reaction based on neutrons. Fermi, on the other hand, had gone a long way toward the theory of neutron diffusion, neutron slowing down, or what we now call "age theory." The one foresaw the outlines of the future without bothering about details, the other mastered the most important details and avoided the difficult problems of the future.

In the process of bombarding various nuclear species with neutrons, Fermi finally got to the end of the periodic table and bombarded uranium. But in the bombardment of uranium he found something new. He found that instead of just producing one or two or, at most, three different kinds of activities, an exceedingly great number of various activities were generated. Activities of all kinds of lifetimes, activities that chemically behaved in all manner of ways were found. Now, being guided by an analogy formed from his other experiments, Fermi said: These must be transuranic elements. We have at last made elements heavier than uranium. These behave in strange and unexpected ways.

But the only trouble was that, at once, there seemed to have been produced a great many of these transuranic elements. People did start to worry about it, and were puzzled as to why so many activities should occur. A German chemist, Mrs. Noddack, wrote a letter to Fermi which she later published. In it she said that Fermi's observation could very well be the splitting of uranium—uranium being the most strongly charged of particles and one which may, upon being hit by a neutron, split in two. She quite clearly suggested the possibility of fission. Fermi set the suggestion aside. He did so for a very good reason. Because Fermi was really an extremely good physicist he would not be satisfied with the qualitative argument that uranium had much charge, had much excess energy, was a little heavier than other things and was, therefore, ready to split. No, he went back and looked into the question:

This is how heavy uranium is; this is how heavy the two fission fragments (the two particles into which the uranium disintegrates) would be. Because of the difference, I see I can gain energy. But, before I gain energy, I first have to take these two things slightly apart while they are still in their mutually attractive fields. Although in the end I can gain energy, I first have to go through a very big barrier.

Now we all know that this is precisely what happens in fission, that particles do go over such a big barrier. The size of this barrier, however, is a sensitive function of the energy and thus an even more sensitive function of the mass, because very little mass change will correspond to a very great difference in energy. Unfortunately, though the masses of these particles were measured, they had not yet been measured with sufficient accuracy and, what is more trouble, the lack of accuracy was not known.

Therefore Fermi calculated with slightly wrong masses, which gave the result that the potential barrier to be overcome in fission would be much greater than could possibly be overcome. Therefore Fermi threw out Mrs. Noddack's hypothesis. He continued to investigate, however, and devised the following beautiful experiment.

He took a piece of uranium, and said: This piece of uranium is emitting alpha particles. Now, let us bombard it with neutrons. When a neutron attacks a uranium nucleus, there will be some excess energy available. Maybe this excess energy will be taken up by the alpha particle which is about to leave, in which case this alpha particle will have an excess energy. Let's look for extra-long-range alpha particles coming from uranium under neutron bombardment. But, in order to make the experiment correctly, we are going to cover this piece of uranium with a thin foil which will stop all the normal alpha particles, so that only the extra-long-range alpha particles will come through.

This he did. Had he forgotten to put the foil there—had he forgotten only once—then he would have seen something remarkable. Because the neutrons made some fissions in the uranium, and these fissions give a lot of energy to the fission fragments, there would have appeared in the counters in Rome the biggest kicks that anyone had ever seen. However, Fermi was an extremely good physicist and never once forgot to put the foil there. And so, unfortunately, all the fission fragments stuck in the foil and were not seen.

There were other physicists in those days who were playing with experiments of a similar kind. One of them tried to do some experiments with thorium. He did not use any foil and he did see the big kicks, but he shook his head and said: Yesterday this counter was in good working order, and today it sparks.

Discovery of Fission

Fission was right under our noses for several years. Had it been discovered earlier the consequences might have been quite serious. In the 1930s there was little or no scientific war work in our country or in the countries of our allies. There was, however, already at that time a great deal of work of this kind in Germany. Had fission been discovered two or three years earlier I should not be too sure but that the Nazis might have had atomic bombs during the war, and the outcome of the war might have been different.

In the end, in December, 1938, the discovery came from the chemists. Hahn and Strassmann, investigating in very great detail the chemical properties of some of the activities, finally came to the conclusion that some of them absolutely had to be identified with a known element, barium, which has an atomic number much smaller than that of the original uranium. Therefore, the conclusion that fission had occurred was obvious.

Once this conclusion was drawn, it was easy to look for fission products. To perform this experiment, one bombards uranium with neutrons and looks for the big kicks. This was first done by Frisch and Meitner. Within days the experiment was verified in a great many other laboratories, and we all started to worry about the consequences. People like Fermi and Szilard, who at that time worked together at Columbia University, were fully aware of the possibilities of the chain reaction. Its feasibility now depended on the question of whether or not some additional neutrons got loose during the disintegration of uranium.

It was not very easy in those days to find the additional neutrons. In order to produce fission one had to have a respectable stream of neutrons to begin with. Then the question was: Were more neutrons produced? One could, of course, do as the French worker, Frederick Joliot, did. He counted neutrons that came in, and then counted the neutrons that were produced. The experiment is difficult, because the neutrons cannot be contained in any small region. In addition, it is a very tricky experiment because neutrons of various energies behave differently. Before they can be counted they must all be brought down, reliably, to the same condition. This was done in Joliot's laboratory.

Szilard was prepared with a scheme which was more ingenious and direct, although less quantitative. Szilard rigged up a very simple experiment (together with Zinn, who was getting started in this work), in which uranium was bombarded exclusively by slow neutrons. They looked for fast neutrons that might emerge. They correctly assumed that the neutrons emitted in any nuclear reaction are apt to be fast neutrons and, if one had conscientiously slowed down all the neutrons first, and then used a piece of uranium and found fast ones, indeed one could be sure that additional neutrons were being emitted. During the spring of 1939, the existence of additional neutrons was established, within a very few months of the discovery of fission.

I was, at that time, in Washington, and some of my very good friends, including Szilard, Wigner, Fermi, and others, were quite interested in these developments. They tried, at first unsuccessfully, to interest the government in the great potentialities of nuclear fission. Finally Szilard decided that only the President of the United States would be able to initiate such a development. In order to get Roosevelt's attention, Szilard wrote a letter and asked one of his much older and much more famous friends, Einstein, to sign it. Then he devised an ingenious scheme by which the letter was to get the attention of President Roosevelt. The scheme took many weeks, but finally the letter landed on the President's desk. I was told that the response was instantaneous. Disregarding the fact that the Bureau of Standards had been closed for the day, Roosevelt called the Director of the Bureau of Standards, Dr. Briggs, informed him of the letter, and asked him to look into the matter most thoroughly. This happened after Poland had been invaded and overrun by Nazi and Communist armies.

Dr. Briggs called a meeting. Szilard and Wigner were there arguing that nuclear fission was extremely important. That outlook was not unanimous. I participated at this meeting as a representative of Fermi and Tuve, who could not come. I repeated the arguments of the excellent paper by Bohr and Wheeler: Fission in uranium is mostly due to the rare isotope U^{235}. In order to get sufficient reactions, one has to slow down the neutrons so that they will interact more strongly with U^{235}. For this, a material is required which slows down the neutrons but does not absorb them. The best material is graphite of a very high purity. Therefore what we need is sound financial support to get such unusually pure graphite.

I submitted all this information with the statement that it would be reasonable to spend, during the first year, the great sum of $6,000. I still have not lived down the fame which I thereby acquired, although I have in the meantime conscientiously endeavored to better my ways.

The question of pure graphite turned out to be a not unimportant one. We were struggling at that time with really simple and obvious things. Cross sections were not known; neutron energies were not known. All calculations were based on crude estimates. We did not know how much of the neutron loss occurring in the slowing down in graphite was unavoidably due to the carbon itself, and how much was due to impurities. The Bureau of Standards undertook to make sure that the graphite delivered at Columbia University would be pure enough. They did so, and certified the graphite to be quite pure. Thereupon the experiments were made: It was found that a reactor composed of graphite and uranium could not possibly work.

Szilard did not quite like this conclusion and, being a rather stubborn[2] person, checked up on the graphite. He found that, between the time that the Bureau of Standards had made its investigation and the time that the graphite was delivered, the fabrication methods of the graphite were slightly changed. Its purity was not as high as expected. This was corrected, and the measurements started to give more hopeful results.

The actual plan called for a reactor that utilized to the fullest extent the small U^{235} content (0.7 weight per cent) of normal uranium. This will be done if the neutrons are slowed down as rapidly as possible and if the slowing down of the neutrons occurs at a place where no uranium

[2] The word "szilard" is an exceedingly polite translation into Hungarian of the English adjective "stubborn."

is present. During the slowing process the neutrons assume all kinds of different energies. At some of these energy values the common, abundant isotope of uranium, U^{238}, absorbs the neutrons with very great ease. Therefore, *during* the slowing-down process the neutrons should not be in contact with the uranium. But *after* the slowing-down process the neutrons should be able to enter the uranium in order to cause fission in U^{235}. These precautions are not necessary if the valuable U^{235} isotope is separated from the bulk of the uranium. But in those early days we did not have separated isotopes and we did not know whether we could get them in sufficient quantity.

The simple procedure which was adopted was to make a uranium-graphite lattice. Fast neutrons are produced in the uranium. They will make some collisions with uranium in which the uranium nucleus gets excited and the neutrons lose energy. But after the neutrons have been slowed to a couple of hundred thousand electron volts, further slowing down will mostly occur in the graphite. If a neutron has been slowed down into one of the energy ranges in which U^{238} acts as a strong absorber, this neutron is in danger of being lost. If it finds a piece of uranium it will get absorbed. Actually, it will be absorbed within a short depth into the uranium. The interior of the uranium piece is not, in effect, absorbing these neutrons. We may compare this situation with the one which would result if the uranium were uniformly distributed in graphite. In that case all the U^{238} could participate in catching neutrons which have fallen into the dangerous energy range. Thus it is much more advantageous to have a lattice structure in which uranium and graphite are separated.

In an elastic collision of a neutron with a heavy nucleus like uranium, little energy is lost. But in a collision with carbon or oxygen, the energy is decreased. The energy changes to a greater extent. Therefore it is important not to have the uranium mixed with either carbon or oxygen, otherwise a substantial amount of slowing down would occur inside the uranium itself. Thus we needed uranium in the metallic form.

Shortly after Pearl Harbor, reactor work was concentrated at the University of Chicago under the name of "Metallurgical Laboratory." This name was just a cover-up but it happened to pinpoint a most important phase of our work. When we started to acquire the first expensive cubes of real metallic uranium it became possible to execute an experiment in which I had been interested for some time and which needed a large amount of metal. We wanted to be quite sure as to whether the graphite-uranium lattice was necessary to produce a chain reaction. Perhaps a sufficiently big metallic piece of uranium could be made reactive, too. Uranium U^{238} does fission when bombarded with sufficiently fast neutrons. We did not know at the time how fast the neutrons which are emitted in the fission process were. Nor did we know how quickly they lose their energy by exciting uranium nuclei. It was important to find out whether a sufficient number of fissions could be made in normal uranium by fast neutrons to support a chain reaction before the fast neutrons lost more of their punch.

The man who actually carried out the experiment, who did a really wonderful job and after whom the experiment is named, is Mr. Snell. He built up a block of uranium in front of the cyclotron, bombarded the thing, and then we had all kinds of detection equipment to measure the neutron distribution in that block. We were very, very sure that the block was not big enough to explode, yet. However, people were a little careful in putting things together. The block did not explode. Furthermore, it showed conclusive proof that a pure, natural metal block of uranium never would become a nuclear reactor. Therefore the graphite was an actual necessity. The block of uranium, weighing about a half ton, which we had sitting there in front of the cyclotron was a respectable object. To produce it, at that time, cost half a megabuck ($500,000).

The First Reactor

Before the first reactor was built very careful consideration was given to the manner in which a reactor can be controlled. On the basis of these theoretical considerations we expected no trouble. But because of the possible danger involved we continued to be slightly worried. Easy control of a reactor is possible because some of the neutrons produced in fission are delayed. You may imagine the fission process as a tearing apart of a uranium nucleus into two smaller nuclei. In the process neutrons are being emitted without any measurable delay. In fact, approximately 99 per cent of the neutrons are emitted in such a fashion. However, you sometimes get a fragment which suffers a beta decay in course of time, and after the beta decay the nucleus is left with a loosely bound neutron and in an excited state so that the loosely bound neutron is emitted. Such a neutron is called a delayed neutron because it is emitted several seconds after the fission.

Now a reactor will go critical if the neutron chain can be maintained counting *all* neutrons, both prompt and delayed. If, however, you establish the situation where the chain cannot be maintained by the prompt neutrons alone, but only by the prompt and delayed neutrons together, then you have an easily governable situation. The reactivity can increase, the number of neutrons can multiply, but only if one waits for the delayed neutrons. Therefore the multiplication period will be at least several seconds.

A reactor can be started with a very small number of neutrons which are there for some natural reason, such as cosmic radiation or neutrons emitted by the material of the reactor itself. It is safer (and it is generally adopted at present) to start with a greater number of neutrons furnished by some artificial neutron source. In any case one will have to increase the number of neutrons by a very big factor in order to get a sizable energy output.

Thus, starting from a cold reactor and allowing time for the delayed neutrons, it will take many minutes before a sufficient neutron density is built up. During all of this time one can count the neutrons and see how they increase. Even when the reactor is at full power, further changes can be made at leisure because of the period of grace allowed by the delayed neutrons.

All of this was known, but not tested, when the first reactor was put into operation.

Finally, at the end of 1942, the first reactor stood at the University of Chicago. It was a uranium-graphite lattice, although there was not enough metallic uranium to make it a simple metal-graphite lattice. The central region was metal and graphite, but then there were regions farther toward the outside which consisted of uranium oxide and graphite. When the first reactor was made to function, there were, of course, a great number of safety precautions taken. The neutron-absorbing rods were withdrawn exceedingly slowly, measurements were taken to see how the number of neutrons increased; everything was cocked and set, ready to push the absorbing rods back again in case anything should go wrong. And there stood a slightly jittery individual on the top of the pile with a vessel containing some cadmium solution, in case everything should fail. The solution might be spilled and that, of course, would spoil the graphite, but maybe save the University of Chicago. Well, the reactor went critical, it worked beautifully, and not a drop of the solution was spilled.

Enter the Engineers

Atomic energy was invented by physicists, and everything was correctly predicted by physicists. I have just told you that we had completely understood the reactor before it was ever built. The job of constructing the first large reactor was put into the hands of the Du Pont Company. They obtained a piece of desolate land in the State of Washington and started to put up graphite reactors—huge beasts—constructed in a way which later sent some slight shudders down the spines of the members of the Committee on Reactor Safeguards.

During the planning of these reactors there was considerable discussion of how the reactor should be constructed. There was considerable worry