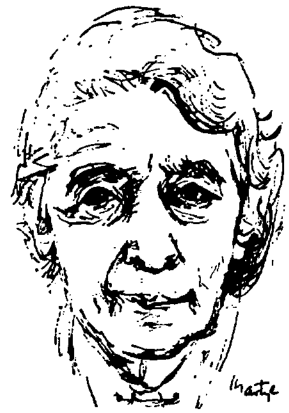

LISE MEITNER

Looking Back

Though I may try to tell you something of the development of physics since the beginning of this century, I naturally cannot give you a connected or comprehensive report. I can only pick out a few things which I especially remember, and which form as it were a magic musical accompaniment to my life.

I believe all young people think about how they would like their lives to develop; when I did so I always arrived at the conclusion that life need not be easy provided only it was not empty. And this wish I have been granted. That life has not always been easy—the first and second world wars and their consequences saw to that—while for the fact that it has indeed been full, I have to thank the wonderful development of physics during my lifetime and the great and lovable personalities with whom my work in physics brought me in contact.

Although I had a very marked bent for mathematics and physics from my early years, I did not begin a life of study immediately. This was partly due to the ideas which were then generally held with regard to the education of women and partly to the special circumstances in my native city, Vienna. In order to catch up the several years I had lost I was coached privately for the leaving certificate (matura) along with two other girl students by Dr. Arthur Szarvasy, who was at that time a lecturer at Vienna University and later became professor of experimental physics at the Technische Hochschule in Brno. We were fourteen girls in all and took a not altogether easy exam (only four of us got through) at a boys' school, the Akademisches Gymnasium in Vienna. Dr. Szarvasy had a real gift for presenting the subject matter of mathematics and physics in an extraordinarily stimulating manner. Sometimes he was able to show us apparatus in the Vienna University Institute, a rarity in private coaching—usually, all one was given were figures and diagrams of the apparatus. I must confess that I did not always get correct ideas from these, and today it amuses me to think of the astonishment with which I saw certain apparatus for the first time.

• *EARLY DAYS IN VIENNA*

From 1901 until the end of 1905, I studied mathematics, physics, and philosophy at Vienna University. No doubt, like many other young students, I began by attending too many lectures. Indeed, at that time it was very unusual for a girl to attend university lectures at all. My first term, I studied differential and integral calculus with Professor Gregenbaur. In my second term, he asked me to detect an error in the work of an Italian mathematician. However, I needed his considerable assistance before I found the error, and when he kindly suggested to me that I might like to publish this work on my own, I felt it would be wrong to do so and so unfortunately annoyed him forever. This incident did make it clear to me, however, that I wanted to become a physicist, not a mathematician.

In 1901, the chair of theoretical physics was vacant, as Ludwig Boltzmann had left Vienna again, this time for Leipzig. He had already left once before to spend three years in Munich, but had then returned; so the Austrian government, in the hope that he would return this time, too—as, in fact, he did in 1902—left the chair vacant. This meant that I was able to hear his lectures from 1902 until his death in 1906. In his opening lecture on the principles of mechanics, Boltzmann mentioned that there was no need for him to pay the usual compliments to his predecessor, since he was his own predecessor. This lecture was really a most stimulating experience—it is found in the series of Boltzmann's popular works.

Boltzmann had no inhibitions whatever about showing his enthusiasm while he spoke, and this naturally carried his listeners along. He was also very fond of introducing remarks of an entirely personal character into his lectures—I particularly remember how, in describing the kinetic theory of gases, he told us how much difficulty and opposition he had encountered because he had been convinced of the real existence of atoms, and how he had been attacked from the philosophical side, without always understanding what the philosophers had against him. I wonder what he would have to say about our huge machines and teamwork when I remember how bitterly he complained, in a popular lecture as early as 1899, about the great extension of the subject matter of physics and the resulting overspecialization. He stated categorically that Helmholtz was, without any doubt, the last physicist who had been able to have an overall view of the whole field. For all that, I am sure he would be very happy about the triumphant progress of the atomic theory, even though mechanical explanations were always much to his liking.

At that time, the Institute for Theoretical Physics was located in a very primitive, converted apartment house in Türkenstrasse 3. The entrance looked like an entrance to a hen house, so that I often thought, "If a fire breaks out here, very few of us will get out alive." However the internal fittings of Boltzmann's lecture room were, relatively speaking, very modern. On the middle of three large blackboards he wrote up the main calculations and the subsidiary calculations on the boards on either side, so that it would almost have been possible to reconstruct the entire lecture. Indeed, Boltzmann was not only a very great scientist, who opened up entirely new fields in thermodynamics and statistics, but was also a man who aroused admiration and affection. On his sixtieth birthday, Paul Ehrenfest, who had come from Göttingen to Vienna, asked the audience to remain standing after the professor had come in as he wished to pay tribute to Boltzmann's great achievements in a short ceremonial address. I am afraid I cannot remember what Boltzmann said in reply. Two years later he was dead, a victim of one of his occasional fits of depression.

I received my first experimental training in the Department for Elementary Practical Work (Anfängerpraktikum), directed by Anton Lampa. Lampa was an excellent experimentalist, but, as an enthusiastic follower of Mach, was rather skeptical of the modern developments of physics. He was perhaps more interested in epistemological and philosophical questions than in pure physics, although he did write a manual on experimental physics which was really good for that time. As an introductory course in practical work, it was certainly very well conducted, but the extremely primitive apparatus available limited the possibility of carrying out experiments. I remember I once asked him where to get ice for an experiment, and he replied in a rather scoffing way that I had only to go down into the yard and fetch some snow. Later he took over the chair in Prague from Mach and helped to bring Einstein to Prague, in spite of the Austrian government's indecision. After the first world war, Lampa came to Vienna and was later made director of the Urania.

My doctoral thesis was done on thermal conductivity in nonhomogeneous bodies, under the direction of Professor Exner and his assistant, Dr. Benndorf. It was published in the Proceedings of the Vienna Academy. Although Exner was also an excellent experimentalist who did very valuable work in the most varied fields, I cannot say I have a very lively recollection of his lectures on experimental physics. These were delivered almost without experiments, between noon and one p.m., when most of the students were already very tired. Sometimes I was really afraid I would slip off my chair. Our mathematics lectures were from eight to nine a.m. in winter and from seven to eight a.m. in summer, so that by midday we were already pretty tired. I rather doubt whether the student of today could be enticed into a lecture so early in the morning.

At the time of which I speak, women's education was just beginning to develop in Vienna, and indeed in Austria, but I knew very little of this development and must confess I cannot say, even today, whether or not my university teachers were in favor of it. I was very unsure as to whether I would be able to become a scientist, so I also took my teaching diploma and did my year's trial at a girls' high school, in order to keep open these possibilities. At the same time I did try to carry my scientific education a stage further by working in Professor Boltzmann's institute. Through Stefan Meyer, who took over temporarily after Boltzmann's death, I became familiar with the new field of radioactivity, although I certainly never had any intention of specializing in it. Initially, my thoughts ran in the direction of more general physics.

• *AND ON TO BERLIN*

Paul Ehrenfest had drawn my attention to Lord Rayleigh's scientific papers, whose article on optics prompted my first independent work. This was published in the *Proceedings* of the Academy of Sciences under the title "Some Conclusions Derived from the Fresnel Reflection Formula." In it, I was able not only to explain an experiment which Lord Rayleigh had carried out finding unexpected results, but also I could predict some other consequences and give them experimental proof. This gave me courage to ask my parents to allow me to go to Berlin for a few terms; I stayed thirty-one years! I must admit that, at that time, I knew nothing at all about German universities. I only knew Planck's name, and that not because I had knowledge of his theory of radiation. After Boltzmann's death, Planck was invited to take the post in Vienna—although he did not accept it—and I had occasion to see him when he came to Vienna to have a look around the Institute of Theoretical Physics.

As is well known, in 1900 Planck developed the theory of thermal radiation—that an atom cannot take up or emit radiation in a continuous manner, but in quite specific, discrete quanta, hence the name quantum theory. I have often wondered why Boltzmann never said a word to us about this. After all, I was still attending his lectures five years after Planck's discovery. It was, however, a very long time before quantum theory won general acceptance. Even so, Planck did not arrive at his theory until he had accepted Boltzmann's atomic theory as well as the use of statistics which Boltzmann had introduced. Yet I never heard anyone so much as mention Planck's theory before I went to Berlin. This is really rather surprising because the photoelectric effect, i.e., the fact that metals exposed to light of suitable wavelengths emit electrons, had been completely explained by Einstein in 1905 on the basis of Planck's quantum theory, which Planck himself initially had been careful always to call his "quantum hypothesis." Though in the same year, 1905, Einstein developed an experimentally demonstrable formula for the Brownian movement which afforded the best possible evidence for Boltzmann's atomic theory, I never heard Boltzmann so much as mention Einstein's name. It was not until I went to Berlin in 1907 that I got to know about all this revolutionary work.

When I registered with Planck at the University in Berlin, so as to attend his lectures, he received me very kindly and soon afterwards invited me to his home. The first time I visited him there, he said to me, "But you are a Doctor already! What more do you want?" When I replied that I would like to gain some real understanding of physics, he just said a few friendly words and did not pursue the matter any further. Naturally, I concluded that he could have no very high opinion of women students, and possibly that was true enough at the time. He did, however, make me his assistant five years later, which not only gave me a springboard to really develop my scientific faculties, but also contributed greatly to my development as a person. I must admit that to begin with, I was a little disappointed in Planck's lectures because despite their really classic clarity, they sometimes gave a rather colorless impression compared with Boltzmann's lectures, which were so strongly marked with feeling. But I soon saw that this was my mistake.

● *PLANCK AND HIS STUDENTS*

In outward behavior Planck was very reserved, for all the affection he inspired. Some people mistakenly regarded this as a sign of conceit, for nothing could have been further from his character. He had a rare honesty of mind and an almost naive straightforwardness, well matched by his simplicity in externals. It was his express desire to enter into closer personal contact at least with his advanced students, and he used to invite not only the research students, but also his own assistants and those of the professor of experimental physics regularly to his home. He enjoyed cheerful company and his house was the center of good companionship. In the summer we ran races in the garden, and Planck joined in with an almost childlike eagerness and pleasure; he had a very good turn of speed and was very happy when he caught one of us, as he often did. Planck once told us that Josef Joachim, with whom he often used to play chamber music, was such a wonderful man that when he went into a room, the air in the room became better. Exactly the same could be said of Planck. This was very strongly felt by the younger generation of physicists in Berlin, among whom I may include myself, and it undoubtedly made a very great impression on us.

Planck was one of the first to recognize and stress the great importance of the special theory of relativity. In all those years of the twenties and thirties, when Einstein was unhappily exposed to so many scientific and personal attacks, Planck always stood beside him, ever ready to assist him. Planck was also one of the three professors who went to Zurich in 1913 to persuade Einstein to accept the chair at the Berlin Academy. Einstein, himself, I met for the first time in 1909 through Professor Lampa at the scientific congress in Salzburg. On that occasion Einstein gave a lecture on the development of our views regarding the nature of radiation. At that time I certainly did not yet realize the full implications of his theory of relativity and the way in which it would contribute to a revolutionary transformation of our concepts of time and space. In the course of this lecture he did, however, take the theory of relativity and from it derive the equation: energy = mass times the square of the velocity of light, and showed that to every radiation must be attributed an inert mass. These two facts were so overwhelmingly new and surprising that, to this day, I remember the lecture very well.

This congress was altogether a very impressive experience. It was attended by theoretical and experimental physicists from the entire world—Planck, Einstein, Laue, Born, Hasenöhrl (who succeeded Boltzmann), Schweidler, Stark, and R. W. Wood, the well known specialist in optics from America. It was really something quite out of the ordinary, a most stimulating meeting. I reported on two minor pieces of work which Otto Hahn and I had carried out, in the course of which we had discovered and properly classified two new groups of beta emitters in the radium series.

I had, after all, finally landed in work on radioactivity and the nuclear physics to which it was giving birth. In addition to attending Planck's lectures on theory, I wanted to do some experimental work and approached Professor Rubens, head of the department of experimental physics in Berlin. He told me the only space he had was in his own laboratory, where I could work under his direction, that is, to a certain extent with him.

Now it was quite clear to me then, as a beginner, how important it would be for me to be able to ask about anything I did not understand, and it was no less clear to me that I should not have the courage to ask Professor Rubens. While I was still considering how I could answer without giving offense, Rubens added that Dr. Otto Hahn had indicated that he would be interested in collaborating with me, and Hahn himself came in a few minutes later. Hahn was of the same age as myself and very informal in manner, and I had the feeling that I would have no hesitation in asking him all I needed to know. Moreover, he had a very good reputation in radioactivity, so I was convinced that he could teach me a great deal. The only difficulty was that Hahn had been given a place in the institute directed by Emil Fischer, and Fischer did not allow any women students into his lectures or into his institute. So Hahn had to ask Fischer whether he would agree to our working together. I went to Fischer to hear his decision, he told me his reluctance to accept women students stemmed from his constant worry with a Russian student lest her rather exotic hairstyle result in its catching fire on the Bunsen burner. He finally agreed to my working with Hahn, if I promised not to go into the chemistry department where the male students worked and where Hahn conducted his chemical experiments. Our work was to be confined to a small room originally planned as a carpenter's workshop; Hahn had fitted it out as a room for measuring radiation. For the first few years, I was naturally restricted to this work and could not learn any radiochemistry. But when women's education was officially regulated in Germany in 1909, Fischer at once gave me permission to enter the chemistry department. In later years, he was most kind in supporting me in every respect, and I have him to thank for the fact that in 1917, I was given the responsibility of setting up a department of radiation physics in the Kaiser Wilhelm Institute of Chemistry. Although it naturally took some time for matters to proceed this far, this is not to say that I was in any sense isolated. Admittedly, the assistants in the Chemistry Institute had no particular love for women students—it sometimes happened that if Hahn and I were walking together on the street and one of the assistants met us, he would say somewhat obviously, "Good day, Herr Hahn."

• *A FAMOUS GROUP*

From the very beginning, I found the physicists to be very friendly and understanding. Laue, whom I met as a result of Planck's lectures, was a very good friend until his premature death. The other young physicists I met mainly at the Wednesday colloquia, which later became so famous. These were led by Rubens and later by Laue. Originally, it was a very small group of at most thirty people who came—the professors, of course, such as Planck, Nernst, and later Einstein, but above all many young people including, in particular, Rubens' assistants: Otto von Baeyer, James Franck, Gustav Hertz, Robert Pohl, Peter Pringsheim, Erich Regener, and many others. Later, of course, other people came too, such as Otto Stern, Hans Geiger, and Hans Kopfermann. Even in 1907, these colloquia were already an exceptional intellectual center. All the new results which were then pouring out were presented and discussed there. From the very first years of my stay in Berlin, I remember lectures on astronomy, physics, chemistry—for example, a lecture on the stars of various ages given by Schwarzschild, a theoretical astronomer, another by James Franck on what were then called metastable states of atoms, or one on the connection between ionization energy and quantum theory. It was quite extraordinary what one could acquire there in the way of knowledge and learning.

As soon as Lane discovered his celebrated x-ray interferences, he sent the first picture taken by his colleagues, Friedrich and Knipping, to Pohl, who at once brought it before the colloquium and discussed it. It was obvious what an immensely important piece of information this represented, for it not only showed that atoms or molecules in crystals had a completely regular spatial arrangement, but it also resolved the longstanding question of the nature of x-rays by showing that they are really radiations of very short wavelength.

I met Rutherford (who had developed the model of the atomic nucleus in 1911) for the first time in 1908, when, on the way back from receiving the Nobel Prize for chemistry in Stockholm, he visited Berlin to see his pupil Hahn. When he saw me, he said in great astonishment: "Oh, I thought you were a man!" He had not realized that my first name is a girl's name.

This group of young physicists made up an unusual circle. Not only were they brilliant scientists—five of them later received the Nobel Prize—they were also exceptionally nice people to know. Each was ready to help the other, each welcomed the other's success. You can understand what it meant to me to be received in such a friendly manner into this circle. It was not long before Hahn and I entered into a closer working relationship with one of them, Otto von Baeyer. Hahn and I had chosen as our joint subject of study the behavior of beta-rays on passage through aluminum. We had accepted the assumption, recently formulated by the German experimental physicist H. D. Schmidt, that a given radioelement should emit beta-rays of constant velocity and that these rays should be absorbed in accordance with an exponential law. Now I must admit that, today, I cannot understand how we could assume that to be true—in fact, it was entirely false. Anyhow, in carrying out our experiments, we found several new beta emitters in the three natural radioactive series and were able to confirm this by means of chemical separations and also by the so-called recoil method. Not surprisingly, however, our false assumptions gradually became evident to us and we realized that we were not in a position to say anything whatever about the velocity of the rays in these experiments. If we did occasionally find something approximating an exponential law, the reason was that, by and large, we were measuring the scattering of the rays and not their absorption at all. After a discussion with Otto von Baeyer, we realized that in order to say anything about the velocity of radiation, we would have to use deflection in a transverse magnetic field. As there were no magnets in the Chemistry Institute, we carried out these experiments with Otto von Baeyer in the Physics Institute. Hahn and I attempted to precipitate in as radioactively pure a condition as possible the substances whose beta radiation we wished to investigate in the thinnest possible layers on very short lengths of very thin wire. The precipitation did not always work. We simply had to try, and, if our efforts were successful, we raced out of the Chemistry Institute as if shot from a gun, up the road to the Physics Institute a kilometer away, to examine the specimens in von Baeyer's very simple beta spectrometer. It was a rather primitive method, which today may seem somewhat comical. Even so, these investigations did enable us to discover the so-called line spectra of beta radiation, which, in fact, have no connection with primary beta radiation, although it took us—or rather me—until after the first world war to realize the fact.

• *THE NEW BASIS OF PHYSICS*

In 1913, Hahn and I moved from the carpenter's shop to the Kaiser Wilhelm Institute for Chemistry, which had been founded in 1912 as the first of the Kaiser Wilhelm institutes. Here, Hahn was given a small section; at that time I was Planck's assistant and was at first invited into the Institute as a guest, although later I obtained a post there, too. We were extremely interested in determining the mother substance of actinium, for it was not then known that this represents a special uranium series. It gradually became clear to us that this mother substance must be an alpha-emitting pentavalent substance, and we spent several years looking for it. It was really a lucky chance that our work was not totally interrupted by the first world war, for Hahn was called up at once and I worked from mid-1915 to autumn 1917 as a radiologist in Austrian hospitals at the front. However, Hahn was then assigned to Haber's group, which was working on defensive and offensive measures in gas warfare. In this way, he often came to Dahlem, while I was able to get leave of absence from my voluntary position frequently enough for us to be able, even before the end of the war, to point conclusively to the mother substance of actinium, which we called protactinium, the longest lived isotope of element 91.

I will pass over the depressing consequences of the first world war as they affected our work in the Kaiser Wilhelm Institute, and will merely mention that, for a time, we had the Workers' and Soldiers' Council in the Institute, though to tell the truth the consequences were comical rather than tragic. In 1917, I had been given the task of establishing a department of radioactive physics. This was only possible because the I.G. Farben Industrie very generously undertook to support our Institute financially, since otherwise it would have been impossible to make the necessary conversions, buy the apparatus, and engage the staff. As a result of this division of the Institute in two, Hahn and I no longer worked together from about 1920. In the chemistry department, Hahn and his colleagues did very important work on applied radiochemistry. Hahn also found the first examples of nuclear isomerism, uranium-Z, which he found to be isomeric with uranium-X2.

Our work was naturally directed more toward physics; for example, we investigated the line spectra of beta radiation and were able to establish its relationship to gamma radiation. We also checked the theory of Klein and Nishina in regard to the passage of gamma radiation through different materials and, in this connection, incidentally discovered pair formation—not that we recognized it as pair formation, but we did report the presence of some previously unknown effect of the atomic nucleus. Despite this break in direct collaboration between Hahn and myself, there was still very close indirect collaboration. Indeed, it was only natural that the chemistry assistants should help and advise the physicists on all chemical problems. They also made up any preparations we needed for our experiments, while the physicists, in turn, built auxiliary apparatus such as amplifiers or counters for the chemists.

Meanwhile, during the first world war, physics had been placed on an entirely different basis, both from the experimental and from the theoretical points of view. The main credit for that rests with Niels Bohr and his work on the structure of the atom. Not only has this work had a very decisive effect on physics, it has also had repercussions in astronomy, chemistry, and even biology. I do not think that any scientist has had such a worldwide influence on at least two generations of physicists as Niels Bohr. Not even Rutherford, despite his immense genius—after the war he achieved the first artificial transformation of elements by bombarding them with alpha radiation, as a result of which protons were split off and alpha particles captured. Certainly Rutherford had a great many pupils, but I do not think that, as regards the influence they exerted, he or anyone else can be compared with Niels Bohr. Bohr's work on the atomic nucleus gave an extraordinary impetus to the development of nuclear physics itself, finally leading to the fission of uranium. As a result of his work, the periodic system of the chemical elements was correctly explained for the first time. This understanding ultimately led to the recent work done in England elucidating the nature of such complex compounds as proteins. Bohr's influence was indeed exceptional in all fields, including astronomy and, as mentioned, biology.

• *BOHR WITHOUT BIGWIGS*

I first met Bohr in 1920 when he was lecturing to the Physics Society in Berlin. In his lecture, he stressed the importance of series of spectral lines and, for their interpretation, introduced his correspondence principle for the first time. I must confess that when James Franck, Gustav Hertz, and I came out of the lecture, we were somewhat depressed because we had the feeling that we had understood very little. In this half-depressed and half-playful spirit, we decided to invite Bohr to spend a day at Dahlem, but not to include in the party any physicists who were already professors. That meant that I had to go to Planck and explain to him that we wanted to invite Bohr, who lived with Planck, but not Planck himself. In the same way, Franck had to go to Professor Haber—because, after all, if we were going to have Bohr in Dahlem for the whole day, we wanted to give him something to eat—and ask Haber for the use of his clubhouse for our discussion "without bigwigs" (bonzenfrei), again stressing that we did not want to invite Haber himself, as he was already a professor. Haber was not the least put out. Instead, he invited us all to his villa—this, you must remember, was the very difficult period after Germany had lost the war, and to get something to eat was rather difficult in Dahlem. Haber only asked our permission to invite Einstein to lunch as well. We spent several hours firing questions at Bohr, who was always full of generous good humor, and at lunch Haber tried to explain to Bohr the meaning of the word "Bonze" (bigwig).

I did not really get to know Bohr personally until a year later, in 1921, when I was invited to give a lecture in Copenhagen on beta and gamma radiation and had the good fortune to spend many hours with Bohr and his wife. The great difficulty for Germans then was that they were strictly excluded from all scientific congresses, and Bohr put himself to a great deal of trouble to get them admitted again. He also told me a great deal about the war and his experiences in England. In a word, we spoke about everything under the sun, whether grave or gay. Even today, I can still feel the magic of this our first meeting, a magic which was only enhanced in the subsequent years when it was often my privilege to take part in Bohr's famous conferences. I have often thought how fortunate it is that there are such people and that it is given to one to make closer acquaintance with them. Following these years, there were at almost annual intervals the famous conferences in Copenhagen, where every new advance in physics and neighboring fields was discussed. Naturally, I cannot mention all the work that was done in these years. Almost every month brought its new surprise development, for example, Dirac's relativistic theory of the electron, the so-called "hole" theory, which led naturally to the elucidation of spin or to the question of pair formation, the discovery of the neutron, and similar things. My selection is based on no kind of practical considerations, but is simply determined by my memory—in any case, I must draw the line somewhere.

• *WORLD WAR II AND AFTER*

If I may revert for a moment to Dahlem, I must say that the years up to 1933 were very stimulating. We needed and we developed complicated apparatus in both departments, and we were surrounded by a crowd of young people, students and staff, who not only learned from us, but from whom we too could learn a great deal, as regards human relations and sometimes as regards our work. There was really a very strong feeling of solidarity between us, built on mutual trust, which made it possible for the work to continue quite undisturbed even after 1933, although the staff was not entirely united in its political views. They were, however, all united in the desire not to let our personal and professional solidarity be disrupted. This was a special feature of our circle and I continued to experience it as such right up to the time I left Germany. It was something quite exceptional in the political conditions of that day. In this way, from 1934 to 1938, Hahn and I were able to resume our joint work, the impetus for which had come from Fermi's results in bombarding heavy elements with neutrons. This work finally led Otto Hahn and Fritz Strassmann to the discovery of uranium fission. The first interpretation of this discovery came from O. R. Frisch and myself, and Frisch immediately demonstrated the great release of energy which followed from this radiation. But by then I was already in Stockholm. There, too, I was able to watch many interesting new developments in physics. It was mainly Oskar Klein, professor of theoretical physics in Stockholm, who in his friendly way helped me to understand the many new developments in the field of physics, for instance, the discovery of mesons and hyperons. While for the fact that the inner structure of a reactor has not remained entirely a closed book to me, I have to thank Sigvard Eklund, who has always been a very good and helpful friend to me in and outside of physics. Finally, I ought to mention Professor Borelius, whose work has gained greatly in importance, owing to the attention now devoted to semiconductors—a field in which he did much preliminary work. When he opened his new institute, he placed a few rooms at my disposal for a small department of nuclear physics, of which I remained in charge until my retirement. In this way, I can say that in Sweden too, physics has brought light and fullness into my life. What still gives ground for anxiety, of course, is what mankind will make of this newly won knowledge, which could come to be used for destruction on a tremendous scale.