"Of those 'principally involved' the Soviet Union must, of course, be one.

"I would be prepared to submit to the Congress of the United States, and with every expectation of approval, any such plan that would, first, encourage world-wide investigation into the most effective peacetime uses of fissionable material, and with the certainty that the investigators had all the material needed for the conducting of all experiments that were appropriate; second, begin to diminish the potential destructive power of the world's atomic stockpiles; third, allow all peoples of all nations to see that, in this enlightened age, the great Powers of the earth, both of the East and of the West, are interested in human aspirations first rather than in building up the armaments of war; fourth, open up a new channel for peaceful discussion and initiative at least a new approach to the many difficult problems that must be solved in both private and public conversations if the world is to shake off the inertia imposed by fear and is to make positive progress towards peace."

LISE MEITNER LOOKS BACK

At the invitation of the Agency, Professor Lise Meitner gave recollections of her career in a lecture in Vienna on 20 September 1963. She spoke as follows:

Though I may try and tell you something of the development of physics since the beginning of our century, I can naturally give you no kind of connected or comprehensive report. I can only pick out a few things which I specially remember, and which form as it were a magic musical accompaniment to my life.

Lise Meitner, 1916
(Photo Radium Institute, University of Vienna)

I believe all young people try to think about how they would like their lives to develop; when I did so during my youth the conclusion I always arrived at was 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 have seen 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 has brought me in contact.

Although I had a very marked bent for mathematics and physics from my early years on, I did not take up a life of study immediately. This was partly due to the ideas which were then generally held with regard to women's education and partly to the special circumstances in my native city, Vienna. So I lost several years, and, in order to catch up, I was coached privately for the leaving certificate (Matura) along with two other girl students and sat the exam at a boys' school, the Akademisches Gymnasium in Vienna, which was not altogether easy. We were 14 girls in all and four of us got through. I should like in this connection to make special mention of our mathematics and physics teacher, Dr. Arthur Szarvasy, who was at that time Lecturer at Vienna University and later became Professor of Experimental Physics at the Technische Hochschule in Brno. He had a real gift for presenting the subject-matter of mathematics and physics in an extraordinarily stimulating manner and was also sometimes able to show us apparatus in the Vienna University Institute, which was a rarity in private coaching; for the most part all one was given was figures and diagrams of the apparatus, and I must confess that I did not always gain from these a correct idea of the shape of the apparatus in question. 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 and, no doubt like many other young students, began by attending too many lectures. For a girl it was indeed at that time something very unusual to be able to attend university lectures at all. I started with differential and integral calculus with Professor Gegenbauer. In my second term he gave me the work of an Italian mathematician, in order that I might detect an error which it contained. However, I needed considerable assistance from him 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 for ever. This incident did make it clear to me however that I wanted to become a physicist, not a mathematician.

Ludwig Boltzmann
(Photo Austrian National Library)

In 1901 the chair of theoretical physics was vacant, as Boltzmann had left Vienna again, this time for Leipzig. He had already gone away once 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, however, that I was able to hear his lectures from 1902 until his death in 1906. They started with a course on the principles of mechanics. In his opening lecture Boltzmann mentioned that there was no need for him to pay the usual compliments to his predecessor, since he was his own predecessor. This opening lecture was really a most stimulating experience - actually you will find it in the series of Boltzmann's popular works. Boltzmann had no inhibitions whatever about showing his own enthusiasm for the branch of science about which he was talking, 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 if we remember how bitterly he complained in a popular lecture given as early as 1899 about the great extension of the subject-matter of physics and the inevitably resulting over-specialization. He stated categorically that Helmholtz was without any doubt the last physicist who had been able to have an over-all view of the whole field. For all that, I am sure he would be very happy about the triumphal progress of the atomic theory, even though mechanical explanations were always very much to his liking.

The Institute for Theoretical Physics was at that time in a very primitive, converted apartment house in Türkenstrasse 3, a lecture room with benches rising steeply behind each other and an entrance which really looked like the entrance to a hen house, so that I often thought, "If a fire breaks out here very few of us will get out alive". The internal fittings of his lecture room, however, were, relatively speaking, very modern. There were three large blackboards and Boltzmann wrote up the main calculations on the middle board and the subsidiary calculations on the boards either side, so that it would almost have been possible to reconstruct the entire lecture from what was written on these three blackboards. 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 us in 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, and 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 sceptical regarding the modern development of physics and was also 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. For an introductory

Türkenstrasse 3, formerly the Institute for Theoretical Physics. The building is at present being converted into the Afro-Asian Institute (Photo All. Bauges. A. Porr, AG.)

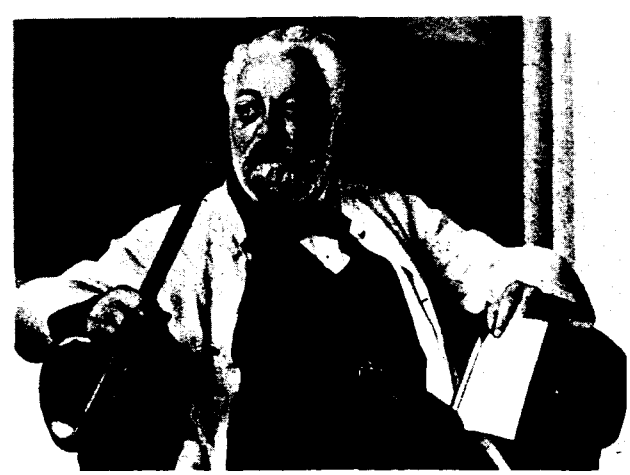
course in practical work our course was certainly very well conducted, but extremely primitive as regards the apparatus available and the possibility of carrying out experiments. I remember I once asked him where I could get ice for an experiment, and he replied in rather a scoffing way that I had only to go down into the yard and fetch some snow, which was not particularly clean. 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 in the matter. After the First World War, Lampa came back to Vienna and was later made director of the Urania.

EDUCATION UNDER DIFFICULTIES

I did my doctoral thesis with Professor Franz Exner on a subject set by him, namely thermal conductivity in non-homogeneous bodies. The work was done under the direction of Professor Exner and his assistant, Dr. Benndorf, and 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 as these were delivered without, or almost without, experiments, in a room where there were no desks but only chairs, and between noon and 1 p.m., when the students were for the most part already very tired. Sometimes I was really afraid I would slip off my chair. Our mathematics lectures were from 8 to 9 a.m. in winter and from 7 to 8 a.m. in summer, so that by mid-day 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 could not say, even today, whether my university teachers were in favour of women's education or not. All I can say is that I was very uneasy in my mind 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 these possibilities open. At the same time I did try to carry my scientific education a stage further. I had a place in Professor Boltzmann's institute, where Stefan Meyer took over temporarily after Boltzmann's death. In this way, through Stefan Meyer, I got to know 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.

Paul Ehrenfest had drawn my attention to Lord Rayleigh's scientific papers, where I had read an article on optics which 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" and in it I was able to give experimental proof of these conclusions, which had been arrived at theoretically. This gave me courage to ask my parents to allow me to go to Berlin for a few terms; I stayed 31 years! I must admit that at that time I knew nothing at all about the German universities. I only knew Planck's name, and that not because I had knowledge of his theory of radiation but because after Boltzmann's death he was invited to take the post in Vienna - although he did not accept it - and I had occasion to see him when he came here to have a look around the Institute of Theoretical Physics. As you all know, in 1900 Planck developed a theory of thermal radiation in which he came to the revolutionary conclusion that an atom cannot take up or emit radiation in a continuous manner, but that this is only possible in quite specific, discrete quanta; hence the

Franz Exner
(Photo Radium Institute, University of Vienna)



name quantum theory. I have often wondered since why Boltzmann never said a word to us about this. After all I was still attending lectures by him 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. Despite that I never heard anyone so much as mention Planck's theory before I went to Berlin. And this is really rather surprising because the photoelectric effect, i.e. the fact that metals exposed to light of suitable wave lengths emit electrons, had been completely explained by Einstein in 1905 on the basis of Planck's quantum theory, which Planck himself had initially been careful always to call his "quantum hypothesis". In the same year, 1905, Einstein developed an experimentally demonstrable formula for the Brownian movement; despite the fact that this afforded the best possible evidence for his 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 be able to hear his lectures, he received me very kindly and very soon afterwards invited me to his home. But the first time I visited him there, he said to me: "But you are a Doctor already! What more do you want?" And 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 or enter into it 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 and this not only gave me so to speak a springboard which made it possible for me 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 colourless impression compared with Boltzmann's lectures, which were so strongly marked with feeling. But, as I soon saw, this was a mistake on my part.

PLANCK AND HIS STUDENTS

In outward behaviour Planck was very reserved, for all that he inspired so much affection. But if some people regarded this as a sign of conceit, that was a great mistake, for nothing could have been further from his character. He had a rare honesty of mind and an almost naïve 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 us regularly to his home, not only the research students but also his own assistants and those of the Professor of Experimental Physics. He enjoyed cheerful company and his house was really the centre 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 very often did. I should just like to add that Planck once told us that he often used to play chamber music with Josef Joachim, whose name is probably still known in Vienna, and said that Joachim had been 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 perhaps be permitted to include myself, and it undoubtedly made a very great impression on us.

Perhaps I may mention here that Planck was, as you know, one of the first to recognize and stress the great importance of the Special Theory of Relativity and in all those years of the '20's and the '30's, 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 in order to persuade Einstein to accept the chair at the Berlin Academy. Einstein himself I got to know 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 derive from it the equation: Energy = Mass times Square of the Velocity of Light, and also showed in this connection that to every radiation must be attributed an inert mass. And these two facts were so overwhelmingly new and surprising that I remember the lecture very well, to this day.

This scientific congress was altogether a very impressive experience. It was attended by theoretical and experimental physicists from the entire world and I may perhaps mention the names of some of those whom I remember. Planck and Einstein were there of course, also Laue, Born, from Vienna Hasenöhrl, who had succeeded Boltzmann, Schweidler, also from Vienna, then Stark, while from America I can remember R. W. Wood, the well-known specialist in optics. It was really something quite out of the ordinary, the most stimulating meeting. I myself reported there on two minor pieces of work which Otto Hahn had carried out with me, in the course of which we had discovered two new groups of beta emitters in the radium series and classified them in

their proper place. So you see that after all I had finally landed in work on radioactivity and the nuclear physics to which it was in process of giving birth, and perhaps I can say a few words on this subject. In addition to attending Planck's lectures on theory I did want to do some experimental work and for this purpose approached Professor Rubens, who was at that time head of the Department of Experimental Physics in Berlin. But he told me the only space he had would be in his own private laboratory, where I could work under his direction, in other words, to a certain extent with him.

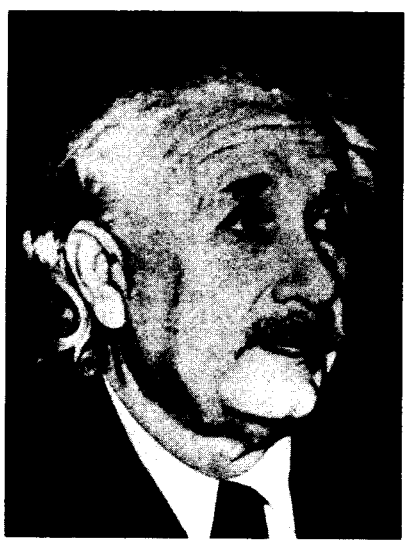
Max Planck
(Photo Austrian National Library)

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 offence, 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 I would have no hesitation in asking him all I needed to know. Moreover, he had at that time a very good reputation in matters of radioactivity, so I was convinced that he could teach me a great deal. The only difficulty was that Hahn told me in the course of our conversation that he had been given a place in the institute directed by Emil Fischer, and that Emil 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 starting work together. And after Hahn had spoken to Fischer, I went to him to hear his decision and he told me his reluctance to accept women students stemmed from an unfortunate experience he had had with a Russian student because he had always been worried lest her rather exotic hairstyle result in her hair catching alight 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 the so-called carpenter's shop. This was a small room originally planned as a carpenter's workshop; Hahn had fitted it out as a room for measurements and he worked there on radiation. For the first few years I was therefore naturally restricted to this work and could not learn any radiochemistry. But as soon as women's education was officially regulated in Germany, i.e. in 1909, Fischer at once gave me permission to enter the chemistry department and I must say that 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 responsibility for 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 and it sometimes happened that if Hahn and I were walking together on the street and one of the assistants met us he said somewhat obviously, "Good day, Herr Hahn".

A FAMOUS GROUP

With the physicists, however, I found from the start a very friendly disposition and also friendly understanding. I got to know Laue as a result of Planck's lectures and he was really a very good friend right up to the time of his premature death. The other young physicists I got to know mainly at the Wednesday colloquia, which later became so famous. These were first led by Rubens and later by Laue. Originally it was a very small group of at most 30 people who came - the professors, of course, such as Planck, Nernst and later Einstein, but above all, to start with, many young people including in particular Ruben's 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, Hans Kopfermann and many others. Even in 1907 these colloquia were already a quite exceptional intellectual centre. 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 which Schwarzschild, a theoretical astronomer, gave on stars of various ages, or another given 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 really quite extraordinary what one could acquire there in the way of knowledge and learning.

As soon as Laue discovered his celebrated X-ray interferences, he sent the first picture taken by his colleagues, Friedrich and Knipping, to the colloquium in Berlin - he himself was at that time in Munich; he sent the picture to Pohl and Pohl at once showed it in 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; it also resolved the long-standing question of the nature of X-rays by showing that they are really radiations of very short wavelength. I cannot tell for certain when I became acquainted with the model of the atomic nucleus developed by Rutherford in 1911, but I met Rutherford 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 and, when he saw me, said in great astonishment: "Oh, I thought you were a man". He had not realized that my first name is a girl's name.



Albert Einstein
(Photo Austrian National Library)

Max von Laue
(Photo Austrian National Library)

If I may go on speaking for a moment about the group of young physicists I was talking about, I would like to stress that they really made up a quite unusual circle. Not only were they brilliant scientists - five of them later got the Nobel Prize -; they were also really exceptionally nice people to know. Each was ready to help the other, each welcomed the other's success, and you can understand what it meant to me to be received in such a friendly manner in this circle. It was not long before Hahn and I entered into closer working relationships with one of them, namely Otto von Baeyer. Hahn and I had chosen as our joint subject of study the behaviour of beta-rays on passage through aluminium, and 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, it gradually became evident to us that our assumptions were false and 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 to an exponential law, the reason for that 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 be able to say anything about the velocity of radiation, we would have to use deflection in a transverse magnetic field; and, as there were naturally no magnets in the Chemistry Institute, we carried out these experiments with Otto von Baeyer in the Physics Institute. For this purpose Hahn and I attempted to precipitate the substances whose beta radiation we wished to investigate, in as radioactively pure a condition as possible, 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 we had been shot from a gun, up the road to the Physics Institute a kilometre away, so as to be able to

examine the specimens in von Baeyer's very simple beta spectrometers. It was, therefore, 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 - till 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 guest, although later I obtained a post there too. We were at that time extremely interested in determining the mother substance of actinium, for it was not

Otto Hahn
(Photo German Federal Ministry for Nuclear Energy & Water Economy)

known at the time that this represents a special uranium series. It had gradually become 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 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, and in this way he often came to Dahlem; while I, for my part, was able to get leave of absence from my voluntary service frequently enough for us to be in a position, even before the end of the War, to point conclusively to the mother substance of actinium, which he called protactinium, the longest lived isotope of element 91.

I will pass over here the rather 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 and 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 necessary apparatus and engage the necessary staff. As a result of this division of the Institute into 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 and Hahn also found the first examples of isomerism, namely uranium-Z, which he found to be isomeric with uranium-X2. In the Physics Department our work was naturally directed more towards physics; for example, we investigated very closely the line spectra of beta radiation and were able to establish its relationship to gamma radiation, and we 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. And perhaps I may stress that, 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, and 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 for astronomy, chemistry and even biology. I do not think that any scientist has had such a world-wide 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. To mention only a few of the consequences of Bohr's work on the atomic nucleus, it gave first of all an extraordinary impetus to the development of nuclear physics itself, finally leading to the fission of uranium; also as a result of Bohr's work the foundation of chemistry, the periodic system of the chemical elements, was for the first time correctly explained, and this understanding led ultimately to the recent work done in England to elucidate the nature of such complex compounds as proteins. Bohr's influence was indeed exceptional in all fields including astronomy and, as already 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 for the first time introduced his correspondence principle. 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 for our permission to invite Einstein to lunch as well. So we spent several hours firing questions at Bohr, who was always full of generous good humour, 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 great good fortune to spend many hours with Bohr and his wife, or with one or other of them, and all I can truly say is that we spoke about everything in the world. The great difficulty for Germans then was that they were so

O. Hahn, G. von Hevesy, Lord Rutherford, H. Geiger, J. Chadwick, Lise Meitner, K. Przibram, S. Meyer, F.A. Paneth (Photo Radium Institute, University of Vienna)

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 really every new advance in physics and neighbouring 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 many similar things. My selection is based on no kind of practical considerations, but is simply determined by my memory - in any case I must naturally draw the line somewhere.

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 did not only learn from us, but from whom we could learn a great deal too, as regards human relations and sometimes also as regards our work. There was really a very strong feeling of solidarity between us, built on mutual trust, and this 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 and I must say this was really a special

feature of our circle and I continued to experience it as such right up to the time I left Germany,

Lise Meitner in Vienna, September 1963

because it was something really quite exceptional in the political conditions of that day. In this way Hahn and I were able from 1934 to 1938 to resume our joint work, the impetus for which had come from the results obtained by Fermi in bombarding heavy elements with neutrons. As you know, this work finally led Otto Hahn and Fritz Strassmann to the discovery of uranium fission. The first interpretation of this discovery came from Otto Robert Frisch and myself, and Frisch immediately demonstrated the great release of energy which followed from this interpretation.

But by then I was already in Stockholm. There too I was able to watch many interesting new developments in the field of physics. It was mainly Oskar Klein, Professor of Theoretical Physics in Stockholm, who in his friendly way helped me understand these things, 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 Dr. Sigvard Eklund, who has always been a very good and helpful friend to me in questions of physics and in other questions as well. 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 in which I was able to set up a small department of nuclear physics, of which I remained in charge till 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 might come to be used for destruction on a tremendous scale. But I believe that here above all is not the place to be pessimistic, seeing that here in Vienna such close attention is directed, and so many efforts are being made, toward the peaceful solution of the very complicated problems with which the world is faced today.