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Personal recollection

Nuclear physics in Heidelberg in the years 1950 to 1980. Personal recollections

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Abstract. After World War II, nuclear physics was a central research theme in the Faculty of Physics and Astronomy at Heidelberg University. That tendency was amplified by the founding of the Max-Planck-Institut für Kernphysik in Heidelberg in 1958. The author witnessed these developments as a student and, later, as a member of the Heidelberg Faculty and of the Max-Planck-Institut.

1 Student years

I grew up in East Germany and obtained my final high-school diploma in Dresden. It was under the communist government there, in 1951. I was interested in history, in literature, in mathematics and in physics. But eventually the decision was clear and simple because in the humanities I felt insecure. I wondered what were the criteria for truth, for valid statements. I found mathematics very fascinating but I was not sure whether it tells us about the working of our brain or whether it reveals to us facts that go beyond that, facts that have absolute validity. I am so to speak a philosophical realist. I believe that the world that we live in is real and not a fiction of our minds. So the combination of mathematics and the investigation of nature gave me the clearest hope that I would be able to find out something which exists and is valid. That determined my choice. I was never interested in technical applications. Therefore from the very beginning, really as a 16 years old boy, I did not aim at becoming an experimental physicist. I was sure I wanted to become a theoretical physicist. It was conceptual structure connected to reality, to data that interested me, not so much the getting of the data.

In 1951 we moved to West Germany and I started to study at Bonn University. I remained there for 5 semesters. I got my "Vordiplom" – the first exam – and then I moved to Heidelberg University. I was looking for a good theoretical physicist as thesis advisor, and I felt certain that Bonn was not the right place. Otto Osberghaus and Helmut Steinwedel in Bonn recommended that I should go to Heidelberg. There Hans Jensen was head of the Institute of Theoretical Physics, famous as the co-discoverer of the nuclear shell model although at the time I was not aware of that fact.

In retrospect the overall situation in German physics was quite bad, although I then lacked the comparison. Most major cities had been destroyed. Many young

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people had been killed in the war, the Jewish part of the population had either been murdered or driven into exile. Many Jewish professors had left for England or the US. The number of highly qualified physics professors was small, there was little money for equipment, and the physics buildings in many Universities were damaged or destroyed.

Heidelberg was an exception. The nucleus for the developments there was Walther Bothe. He had been a student of Max Planck. His focus was on experimental nuclear physics. He developed the coincidence method for which he was awarded the Nobel Prize in Physics in 1954. Originally a Professor of Physics at the University of Heidelberg, he relinquished his chair and moved to the newly founded Kaiser-Wilhelm Institute for Medical Research in Heidelberg in 1934. In that way he was sheltered from the direct and strong influence that the Nazis had at the University.

The Kaiser-Wilhelm-Gesellschaft was founded in the early years of the 20th century upon the instigation of German industry. It was felt that universities as they existed at that time did not have the means to do the large-scale research in basic science that was needed. So there was a move to establish independent research institutes that would fill the gap. In the beginning the emphasis was on basic science. There were also some Institutes in Law. After WW II, the Kaiser-Wilhelm-Gesellschaft became the Max-Planck-Gesellschaft. Nowadays the Max-Planck-Gesellschaft supports also Institutes in the Humanities. The Kaiser-Wilhelm-Gesellschaft was and the Max-Planck-Society is basically much more flexible than any University because the University has to offer complete education in a given set of fields. The Max-Planck-Society is free to open and to close Institutes depending on the promise and perspective offered by a certain field of research. That is the only criterion for supporting a Max-Planck-Institute.

The Kaiser-Wilhelm-Institute for Medical Research was founded in 1930. The founding director, Rudolf Krehl, a medical doctor, held that medical research at that institute should be done from a very fundamental and broad point of view. There were four separate sub-institutes and one was the Institute for Physics that Walter Bothe took over in 1934. The cyclotron built in his Institute in subsequent years became the first functioning such accelerator in Germany. After the war, Bothe took up research in nuclear physics again as soon as that was possible (it was forbidden by the Allies right after WWII). He also returned to the University and, thus, had a double appointment. He had a number of students and collaborators who after the war were instrumental in reviving nuclear physics in Germany: Herbert Daniel, Wolfgang Gentner, Heinz Maier-Leibnitz, Theo Mayer-Kuckuck, Ulrich Schmidt-Rohr. I actually never met Bothe, I only saw him once. I came to Heidelberg in 1954 when he was already quite ill. He died in 1957. But I think he was instrumental in rebuilding the Physics Faculty in Heidelberg.

Hans Jensen, then in Hannover, joined the Heidelberg Faculty in 1949. Otto Haxel, an outstanding experimental nuclear physicist, came from Göttingen in 1950. And Hans Kopfermann, the leading atomic spectroscopist in Germany, came from Kiel in 1953. At that time it was thought that atomic spectroscopy had run its course, and Kopfermann directed part of the effort of his Institute towards nuclear physics. The betatron was put into operation at the Physikalische Institut at Philosophenweg 12 during the time I was a student in Heidelberg. As a consequence of these developments Heidelberg was back on the international scene only a few years after WWII. On the occasion of the festivities in honor of Kopfermann's 60th birthday in 1955, there was a big international conference and quite a few famous people attended, both from Europe and the US.

Perhaps now is the time to say something about the history of nuclear physics. It had been known since the days of Rutherford that atoms consisted of electrons orbiting around a center, the atomic nucleus. The typical size of an atom is 10^{-8} cm, that of a heavy nucleus is 10^{-12} cm. The enormous difference in size is related to the similarly enormous difference in characteristic excitation energy between atoms and nuclei discussed below.

After the discovery of the neutron in the early 1930's it was clear that nuclei were built of neutrons and protons. Atomic physics addresses the structure of the electronic shell, the dynamics of the electrons orbiting the nucleus, while nuclear physics is concerned with the physics of that central piece, the atomic nucleus, that is, of the way neutrons and protons are bound together. Studying that central piece is possible only when you can tickle it, when you can scratch it, when you can do something to it. But for that you need projectiles that seriously do that. An electron Volt (eV) is the characteristic energy of electrons in the atom, and a million electron Volt (MeV) is the characteristic energy of nuclear excitations. You see that a gigantic jump in energy is necessary to go from the study of the electron shell (atomic structure) to the study of nuclear structure. When you want to probe properties of the individual nuclear constituents (proton and neutron) you need much higher energies yet. An important task of nuclear physicists in the 1930's and then the 1950's and in all following years consisted in building accelerators that would produce projectiles of sufficient energy to answer the questions that had arisen. In the 1930's and 1950's a big accelerator would produce particles with energies of a few MeV. And the development of nuclear physics and the later development of elementary particle physics has been possible only because of the big advances in accelerator techniques, and elementary particle physics became a field of its own only at a time when it was possible to build accelerators that would investigate the constituents of the nucleus as separate entities. That involves again another jump in energy and became possible only at the end of the 1950's and the beginning of the 1960's. And then nuclear physics and particle physics sort of separated. Nuclear physics deals with the nuclear many-body problem, with the interaction of the constituents of the nucleus, neutrons and protons, and with the way these manifest themselves in properties of the nucleus. Elementary-particle physics deals with the properties of individual constituents of the nucleus, neutrons, protons and the many other elementary particles that were discovered later. The term "nuclear physics" eventually was used only for nuclear-structure physics (nowadays also referred to as "low-energy" physics), but during the 1950's the term "nuclear physics" was still strongly connected with all what was related to the nuclear realm. At that time CERN, the European Organization for Nuclear Research, was founded, with the word nuclear in its title, even though its mission is elementary-particle physics.

Let me return to the history of nuclear physics in the 1930's. At that time some theorists held that the nucleus could be described in a manner that is similar to that of atoms, namely, in terms of a mean field: The forces that the nucleons (neutrons and/or protons) exert upon each other can largely be accounted for by an average potential. In lowest approximation, the motion of the nucleons is then the motion of independent particles in that mean field. Evidence from the binding energies and the ground-state spins of light nuclei lent support to that view. But in the 1930's Fermi and collaborators in Rome made a very important discovery. They observed that in the scattering of neutrons on nuclei there were many close-lying sharp resonances [Fermi et al. 1934, 1935] [Fermi and Amaldi 1936]. Niels Bohr concluded in 1936 [Bohr 1936] that that discovery contradicts the idea of independent-particle motion in the nucleus. He promoted the idea of the compound nucleus which is the opposite extreme of independent-particle motion. He argued that the sharp and narrowly spaced resonances can occur only because nucleons interact very strongly. In his view, the nucleus is a strongly interacting many-body system, the energy is exchanged all the time between all the constituents. Only when the total available energy is accidentally focused on a single nucleon will that nucleon escape, leading to the decay of the compound nucleus. The long time needed for that to happen accounts for the small widths of the observed resonances. Bohr's view became the prevailing view that dominated nuclear physics till the end of the 1940's. Nuclear fission, discovered in 1938 by Otto Hahn and Fritz Strassmann [Hahn and Strassmann 1939] and explained in 1939 by Lise Meitner and Otto Frisch in terms of Bohr's liquid drop model of the nucleus [Meitner and Frisch 1939] likewise did not lend itself to a straightforward interpretation in terms of single-particle motion.

But there were data in nuclear physics that pointed in another direction. Isotopes with distinct values of proton number Z or neutron number N possess larger binding energies than found on average and are more abundant than others. That is the case for N or Z equal to 2, 8, 20, 28, 50, 82 and $N = 126$. These "magic numbers" and other regularities intrigued many nuclear physicists. Otto Haxel and Hans Süß (a chemist) frequently discussed these findings with Hans Jensen who eventually realized that a shell model could explain these facts [Haxel, Jensen and Suess 1949]. Maria Goeppert-Mayer in Chicago had the same idea [Goeppert-Mayer 1949]. In that model, the nucleons move independently in a central mean field which is supplemented by a strong spin-orbit force. If one assumes that the ground state of a nucleus is described by putting neutrons and protons into the lowest available single-particle states of the shell model (consistent with the Pauli exclusion principle) one finds that the magic numbers correspond to closed shells. These are expected to be particularly stable. Further early support for the model came from the spins and magnetic moments of nuclei with neutron or proton numbers that differ from magic numbers by one unit. Motivated by these results, experimentalists set out to study low-lying nuclear excitations or nuclear reactions involving light nuclei (where the shell model is easiest to handle). There was a real flood of new data that gave an ever increasing support to the model. The shell model was well-established by the mid-1950's and has since become one of the cornerstones of nuclear-structure physics. Still, the idea of Niels Bohr concerning the compound nucleus was apparently valid at high excitation energies (that is, near neutron threshold) but somehow the community lost sight of that fact and attention was focused on the shell model and its implications at low excitation energies. The basics of the model are reviewed by M. Mayer and J.H.D. Jensen in the volume *Elementary Theory of Nuclear Shell Structure* [Jensen and Mayer 1955]. For their discovery, both Jensen and Goeppert-Mayer were awarded the Nobel Prize in Physics in 1964. Bothe's work on the coincidence method and the discovery of the shell model established Heidelberg as an important center of nuclear-physics research worldwide.

I wanted to do my diploma thesis in Theoretical Physics. Jensen refused to accept diploma students because he thought everybody should do an experimental diploma thesis so as to know what experiments are all about and what data mean. And while I basically agree with that point of view, at the time I was eager to do a theoretical diploma thesis and I found a diploma thesis advisor, Heinz Koppe, who worked in condensed-matter physics and held a position at Jensen's Institute. So I did my diploma in condensed-matter theory. Koppe was a master in analytical techniques and I learned a lot from him. After the diploma came the question what to do for the PhD. Heinz Koppe suggested a problem but it did not excite me. At that time a letter from Hans Jensen arrived. He was then visiting the University of Chicago. He had talked to the experimentalists there and he wrote to us that there was a new experiment that was unexplained and that he was very much interested in and if one of us – the PhD students or PhD students to be – would be interested in looking at it that would be a good thing. So I thought this was my chance, and I jumped on it. What was the problem? The group working at the Chicago cyclotron led by Willibald Jentschke used a beam of deuterium nuclei to bombard light nuclei. In that reaction, which later came to be called a direct reaction or, more pictorially, a stripping reaction, the neutron, one of the two constituents of the deuteron, is captured by the target nucleus and the proton escapes. And what was measured was the spin polarization of the outcoming proton. And that spin polarization interested Jensen. His idea was that perhaps this might be a test of the spin-orbit coupling that he had found as a very important ingredient of the nuclear shell model.

I started working on the problem. The difficulty was that at that time the theory of nuclear reactions was in its infancy. Moreover there was little knowledge of whatever existed of the theory because the emphasis in Jensen's Institute had been on nuclear structure. So I was basically on my own. And that is why it took me about a year or a little more before I even had an idea of what I should calculate. And then I had to work out the formulas and to do all the numerical calculations – the evaluation of several dozen radial integrals – by hand, on a little mechanical computing machine, because that was before electronic computers became available. Eventually I could indeed show that the spin-orbit coupling is responsible for the measured polarization of the proton, and that the sign of the coupling is important for getting agreement with the data. I published that work in German [Weidenmüller 1958] and it therefore did, unfortunately, not receive much attention although it was really the forerunner of what later became known as direct-reaction theory. After getting my PhD in the fall of 1957 I stayed on as a postdoc for a year in Jensen's Institute. I had sent my thesis to the few people who had written papers about my thesis problem and one day I got a letter from Warren Cheston in Minneapolis, inviting me to join him as a postdoc. I accepted and left in the fall of 1958.

2 In the United States

Upon my arrival in Minneapolis I learned that Warren Cheston had meanwhile left for London to become scientific attaché at the US embassy there. He had not informed me, which was of course not very good for me and I think not very nice of him. So I continued working on what I had done for my thesis but I did not have a very clear direction at that time. In Minneapolis I met Charles Porter, whom I think of as a very interesting person. Porter was on leave from Brookhaven National Lab and he was looking for a university appointment. In Minneapolis he held the position of visiting associate professor hoping to get tenure. We occasionally met over a cup of coffee and he would tell me what he was interested in. That was random matrices. These had been introduced into physics by Eugene Wigner a few years earlier [Wigner 1955] and Porter was one of few people that worked on the topic. In his most important paper [Porter and Thomas 1956], Porter had (in collaboration with R.G. Thomas) used Wigner's idea to derive the distribution of resonance widths of the compound nucleus, with implications for nuclear reaction cross sections. A few years later he and Norbert Rosenzweig analysed the (then very scarce) evidence for universal level repulsion in atoms as predicted by random-matrix theory [Rosenzweig and Porter 1960].

I was interested in random matrices from the outset because they related to an unfinished aspect of my thesis problem. The stripping reaction that I had worked on was then considered a so-called surface reaction, supposedly taking place when the deuteron grazes the target nucleus without hitting it centrally. In a central collision the formation of a compound nucleus was expected, and it was thought that surface reactions and compound nucleus reactions would be two complementary aspects of nuclear reactions. But Bohr's compound nucleus was kind of a black box. And understanding that black box was the unfinished part of my thesis problem. What I learnt from Charles Porter was that there was a way of looking at a complicated problem like the compound nucleus in statistical terms using random matrices. Random matrices have played a major role in my scientific life later and I will return to that subject below. I now consider it a mistake that I did not take up a collaboration with Charles Porter but continued on my PhD project out of ill-conceived loyalty towards the funding agencies. Unfortunately random matrices were not very popular at that time and Porter did not get tenure in Minneapolis. As for myself, I was not very satisfied with what I worked on in Minneapolis. During that year I met Hans Jensen in the US. He was kind enough to suggest to his friends at Caltech – the California Institute of Technology – to offer me a postdoc position which they did. So I moved to Caltech in Pasadena, California, in September 1959.

At Caltech I was paid in equal parts out of the research contracts of the Kellogg Radiation Laboratory, then perhaps the leading experimental group world-wide in the study of light nuclei, and of Felix Boehm, working on nuclear beta and gamma decays. I shared an office with Bogdan Povh, experimentalist from Yugoslavia (Slovenia), later my colleague and co-director at the Max-Planck-Institute in Heidelberg. Except for Bob Christy, full professor of Physics and increasingly interested in nuclear astrophysics, I was the only theoretical nuclear physicist on Campus. I liked to share my knowledge and soon found myself consulted by numerous graduate students in the Kellogg Lab who asked for information on the theory underlying their individual experiments. That forced me to learn an enormous amount of nuclear theory and the way it relates to data. A major part of my own work was devoted to an experiment done at the Kellogg Lab on nuclei of mass 8 and 12. It was motivated by the then novel conserved-vector current (CVC) theory of Richard Feynman and Murray Gell-Mann, both at Caltech. The theory postulated a relation between electromagnetic properties and beta-decay properties of nuclei [Feynman and Gell-Mann 1958]. The experimental test consisted in comparing the decay rates for electromagnetic and for beta-decay transitions [Nordberg, Morinigo and Barnes 1960, 1962]. But to make the test unique it was necessary to disentangle in the electromagnetic decay rates two contributions, one coming from the spin and the other one from the orbital angular momentum of the constituent nucleons. That could be done only theoretically. The CVC theory applied only to the spin part. That is why I undertook lengthy shell-model calculations. I found that the spin part was significant and so was, therefore, the experimental test [Weidenmüller 1960]. Fortunately my result coincided with the one obtained simultaneously by one of the world experts in the theory of light nuclei, Dieter Kurath in Chicago [Kurath 1960]. That outcome earned me the respect of my senior colleagues Tommy Lauritsen and Willy Fowler and convinced them that I knew what I was doing.

In my work for Felix Boehm I was mainly concerned with nuclear beta decay. I remember a talk given by Bob Christy, the nuclear theorist at the Kellogg Lab, on Rudolf Mössbauer's first publication [Mössbauer 1958]. He predicted how enormous the impact of that experiment was going to be. He saw very clearly that the Mössbauer effect would increase the accuracy of certain experiments by several orders of magnitude, and that it would make it possible to perform entirely novel experiments. His talk was the first step towards inviting Mössbauer as a full professor to Caltech. He joined Felix Boehm's group. We had many discussions and I got to know him quite well. I had first met Mössbauer when he was a graduate student in Heidelberg. Under the supervision of Heinz Maier-Leibnitz (a former student of Walther Bothe and then professor of physics at the TU Munich) he had worked at the Max-Planck-Institute for Medical Research on the problem of recoilless gamma emission. He came a few times to our Institute to talk to Jensen about his work. That is the experiment that Christy discussed and that earned Mössbauer the Nobel Prize a few years later.

I regularly attended the theoretical seminar of Feynman and Gell-Mann. I presented a talk about the calculations mentioned above, and Feynman was very interested. I was on friendly terms also with some of the younger theorists in that group. At the same time I was not considering the idea to switch my research activities to what was emerging at that time as the field of theoretical elementary particle physics. I was not attracted for two reasons. One is the very big role that symmetries played in these early developments of elementary particle physics. Theorists were coping with the experimental discovery of a whole zoo of "elementary" particles, classifying them using group theory, and predicting the existence of particles not yet observed. Group theory had been introduced into quantum theory by Wigner and Giulio Racah already in the 1930s and through the 1940s, but most physicists knew little about it. That topic did not capture my imagination. I was much more interested in complexity as encountered in the compound nucleus and in the question: how does a strongly interacting system behave. The second reason was the extreme ambition among the young people, postdocs and PhD students, in that group. I wanted to do physics because I love doing it and not because I am primarily driven by ambition.

In 1960 Caltech offered me the position of Visiting Assistant Professor and I started to teach. During the year, I was offered a full professorship in Theoretical Physics at the University of Marburg. That fact gives me the chance to say a little bit about the situation there which I think is rather more representative of the situation in German Universities after World War II than the situation in Heidelberg was. I took a leave from Caltech and spent 6 weeks in the summer of 1961 in Marburg, teaching a course in theoretical nuclear physics to the local experimentalists. I found that all three chairs in Theoretical Physics in Marburg were vacant. This was a consequence of the disastrous period 1933 to 1945. It became clear to me that if I was going to accept the offer I would have to do the work of three theorists (teaching and giving exams) plus the administrative work connected with filling the vacant positions. I decided that I could not accept the offer without fatally jeopardizing my research activities. On my way back to the US I passed through Heidelberg. I saw Hans Jensen and I told him my view of the situation. He answered "Perhaps something can be done in Heidelberg". That was at the beginning of the time when the Universities in Germany expanded rapidly. And in fact, two years later a new chair in Theoretical Physics did materialize in Heidelberg.

I returned to Caltech. I had been asked to give a course there in Theoretical Nuclear Physics during the academic year 1961/62. Such a course had never been given before at Caltech, and it existed only in very few other places. I had to build the course from scratch. It was a wonderful challenge. Of course there were Fermi's lectures [Fermi 1950] and there was the book by Blatt and Weisskopf [Blatt and Weisskopf 1952]. But I wanted to address the many-body techniques that were then up-to-date and that were not to be found in these works. Needless to say I spent quite a bit of time preparing the course. Notes were taken by two of the graduate students, and the set of lecture notes was in use at Caltech for the regular course in Nuclear Physics for a decade or more. I was approached by some publishers to make the text into a book but I thought that would require too much more of my time and I never did.

Caltech was a fantastic place. It was really one of the world centers of research in the sciences. There were quite a few Nobel awardees, in physics as well as in chemistry and biology. The campus was small and one would meet these people. The atmosphere was fantastic. Nevertheless I had not much inclination to stay there. Why? Social life outside the campus was very strange. The population in the Los Angeles basin was very mixed. During the war many people had come to Southern California because of the weapons industry, many people had come because of the sunshine, and I found it difficult to establish deeper bonds of friendship. It was like quicksand. So I was not unhappy to return to Heidelberg in 1962, first as Visiting Professor and, since April 1963, as Full Professor of Theoretical Physics in the chair that had meanwhile been created.

3 Professor in Heidelberg

Needless to say, Heidelberg was a much more attractive place than Marburg. There were colleagues with international reputation, there existed a lively international exchange, and teaching and exam-giving were shared by several colleagues.

I have mentioned already that Nuclear physics played a very important role in Heidelberg. Why was that? As far as I can see, it had to do with the evolution of physics after the discovery of quantum mechanics in the 1920's. Atomic physics was the birth place of quantum theory, and much work had been done in the field. The fact that Kopfermann decided to become involved in nuclear physics shows that at least some people in atomic physics thought that the field was more or less exhausted. (The discovery of the laser a few years later has shown, of course, how mistaken that view was.) Today quantum physics plays an important role in many branches of physics but at that time the application of quantum concepts to condensed-matter physics and other areas was still in its infancy. So the field where concepts of quantum physics could most fruitfully be applied was nuclear physics. That is why at that time nuclear physics was considered one of the or the leading field of physics. And there was enormous worldwide financial support, both because of nuclear power and, in some countries, because of the atomic bomb.

However, in terms of experimental equipment Heidelberg was not competitive with leading nuclear-physics laboratories in the US. In changing that situation, the Max-Planck-Gesellschaft played an important role. At the end of WWII the Kaiser-Wilhelm-Gesellschaft practically no longer existed. Being aware of the great tradition, it was the British who in their occupation zone reorganized the former Kaiser-Wilhelm-Institutes. Later the reorganization expanded into the French and into the American zones. The name of the organization had to be changed. Shortly before his death Max Planck agreed that the new society should carry his name. The Max-Planck-Institutes had been very important in bringing research in Germany back on track. Because of political reasons research in nuclear physics was declared illegal by the allied forces at the end of WWII. I do not remember exactly when this ban was lifted. But the ban meant that Germany was lacking behind very seriously compared certainly to the US but also compared to other European countries. So a doubled effort was necessary to catch up. In Heidelberg, it materialized as follows. In 1957 Walter Bothe had died and Wolfgang Gentner, his former assistant who was then professor at Freiburg University, was offered Bothe's position at the Max-Planck-Institute for Medical Research. Gentner, a nuclear physicist, had been involved in the nuclear research effort at CERN, the European Center for Nuclear Research. He was director of the synchrocyclorotron there. And he was fully aware of the fact that dedicated and intense efforts were necessary to reestablish research in nuclear physics. He tried to convince the Max Planck Society – Otto Hahn was then the president, a nuclear chemist himself and the discoverer of nuclear fission – that Bothe's Institute at the Max-Planck-Institute for Medical Research was not an adequate place for doing that, and that it was necessary to found in Heidelberg a new institute specifically devoted to nuclear physics research. Academia, government, and industry all wanted Germany to reestablish itself in that field. In 1958 the Max-Planck-Society decided to found a new Max-Planck-Institute for Nuclear Physics in Heidelberg, with Wolfgang Gentner as its founding director. Many of the buildings were under construction or had been finished by the time I returned to Heidelberg in 1962. The Institute was very well equipped, and it grew very substantially during the first few years. In 1962 it received one of the first Tandem Van-de-Graaff accelerators worldwide. That machine produced a proton beam of up to 10 MeV with an unprecedented energy resolution, making it possible to measure nuclear cross sections much more precisely than before. I held a position at the University but from the very beginning I also spent one or two days a week up on the hill at that new Institute and in contact with the experimentalists.

The main focus in nuclear physics worldwide at that time was on nuclear structure. It became possible to do experiments with ever higher resolution. These yielded massive data of high-quality spectroscopic information. The effort was spurred by important theoretical developments. I have mentioned already that the shell model was extremely successful for the understanding of the spectroscopy of light nuclei and of nuclei with neutron or proton numbers near the magic values. For nuclei with two or more nucleons away from magic numbers, the pure single-particle model (the original form of the shell model) was not sufficient. Residual interactions not accounted for by the mean field had to be taken into account. The theoretical work involved the group-theoretical methods developed by Wigner and Racah. There was also the question whether and how the shell-model could be justified in terms of the basic nucleon-nucleon interaction (properties of which became ever better understood via scattering experiments of protons on protons and deuterons). That interaction is strongly repulsive at short distances, ruling out a straightforward mean-field approximation. Brueckner [Brueckner 1955] and Bethe had developed an approximation scheme that became heavily used for approximate calculations of light and medium-weight nuclei. The origin of the strong spin-orbit interaction remained elusive, however, for many years.

Spectroscopic data were analyzed not only with the help of the shell model. For medium-weight and heavy nuclei with mass numbers far away from closed shells, it was for technical reasons nearly impossible to use the shell model, and the collective models dominated the scene. The first of these was developed by Aage Bohr and Ben Mottelson a few years after the shell model and likewise became hugely successful (the original article in the journal of the Royal Danish Academy of Sciences and Letters [Bohr and Mottelson 1953] is not easy to find; the standard reference is their book *Nuclear Structure* [Bohr and Mottelson 1969, 1975]). The authors were awarded the Nobel Prize in 1975. They started from a quantized version of Niels Bohr's liquid drop model of the nucleus. In that model excitations are described as surface vibrations and as rotations of an intrinsically deformed nucleus. Such so-called collective modes of excitation are conceptually very different from the single-particle modes of the shell model. In addition to the geometric model of Bohr and Mottelson, Arima and Iachello [Arima and Iachello 1975] developed an algebraic model (the interacting boson model) for collective nuclear motion that also was extremely successful. All three models were heavily used for the analysis of spectroscopic data, with typical excitation energies up to several MeV. There also was the challenge to understand the limits of validity of each model and the relation between them. Would it be possible to understand, for instance, collective modes of excitation as superpositions of nearly degenerate shell-model states? Considerable theoretical effort went into understanding the relation between the shell model and the collective models and bridging the gap between them, and also in deriving the shell model from the basic nucleon-nucleon interaction. Curiously, at excitation energies of ten MeV or more, Bohr's compound nucleus idea prevailed, modified by the idea of direct reactions. The relation between the shell model and the compound-nucleus model received little attention.

It should be clear from this narrative how differently nuclear and particle physics had developed since the 1950's. In nuclear physics much emphasis was placed on nuclear structure, i.e., on the multitude of excited states found in medium-weight and heavy nuclei with excitation energies up to several MeV. In particle physics, on the other hand, investigations of the structure of the nucleon and of the big zoo of elementary particles required energies in the 100 MeV or GeV range.

Coming back to my own story, upon my arrival in Heidelberg I had a very pleasant sensation – suddenly I felt totally free to work on problems of my own choice.

Although I loved being at Caltech, I basically felt obliged to offer my services to the experimentalists. I enjoyed doing that but it was not at the center of what I really wanted to do: Understand nuclear reactions and the compound nucleus. For that I felt it was necessary first to work on nuclear reaction theory per se, irrespective of the complexities of the compound nucleus. At the time there were several theories of nuclear reactions on the market, the one due to Eugene Wigner [Wigner and Eisenbud 1947], to Rudolf Peierls [Kapur and Peierls 1938], to Jean Humblet and Léon Rosenfeld [Humblet and Rosenfeld 1961], and to Herman Feshbach [Feshbach 1958, 1962]. Some of these predated the shell model. All were very formal and none attempted to formulate a dynamical theory based upon the shell model. The shell model provided a basis for the formulation of a truly dynamical theory, and I spent the next few years working on that approach, studying both general aspects of reaction theory and the dynamical aspects of the shell model. Much of that work was done in collaboration with Claude Mahaux, a former PhD student of Jean Humblet in Liège. That effort was summarized in a monograph [Mahaux and Weidenmüller 1969]. We aimed at clarifying the relationship between the above-mentioned formal reaction theories in the light of the nuclear shell model. We addressed general questions of analyticity and unitarity of the scattering matrix. Finally, we gave explicit analytical expressions that could be used for nuclear cross-section calculations. The book is very frequently cited even today.

At the same time I also had a close collaboration with one of the experimental groups at the Max-Planck-Institute. In the manner I had done for the PhD students at Caltech, I acted as the group's theoretical mentor. The group was led by Theo Mayer-Kuckuck, later professor in Bonn. He had spent the academic year 1960/1961 at Caltech working at the Kellogg Lab. We had gone on trips together and become friends. He was in his mid-30's at the time. His group was very young, it consisted of diploma students and PhD students. The group used the newly installed Van-de-Graaff accelerator, and they were totally enthusiastic. They knew that they had a tool that was one of the first of its kind in the world. And if you have such a tool you can discover new things. They investigated a phenomenon that had been predicted a few years earlier by Torleif Ericson [Ericson 1960, 1963] and, a little later, also addressed by Brink and Stephen [Brink and Stephen 1963]. The prediction was that the compound-nuclear cross section (that is, the intensity with which particles are scattered into a given direction as a function of the energy of the incident particle) undergoes rapid and random fluctuations with energy. That prediction contradicted earlier wisdom. The effect had not been observed before because the necessary energy resolution had not been available. Investigating these so-called Ericson fluctuations, the group quickly established itself as one of the internationally leading experimental groups in the field. Many of the internationally important players visited and gave talks. In the course of time, Ericson fluctuations came to be understood as a universal phenomenon that occurs when waves are scattered on, or in chaotic or random media. These fluctuations have played an important role in several fields of physics. They mirror the statistical properties of the compound nucleus and were, thus, close to my own research interests. Ericson had had a wonderful insight that led him to his prediction but it lacked a convincing theoretical foundation. That was a strange situation because the phenomenon obviously existed. You have the feeling here is something which is really interesting and perhaps quite general. That stimulated me. But I maintained my direction and first worked out in a very systematic way the shell-model based general nuclear reaction theory before addressing the statistical compound-nucleus problem.

Another group I want to mention is the group founded and led by Günther Hortig and by Rudolf Bock. They started work on a novel kind of nuclear reactions. Traditionally nuclei were bombarded with light projectiles consisting of one or a few nucleons. Hortig and Bock were curious about what happens if you take a heavier projectile, let us say a nucleus of mass 16 or mass 20 or mass 30, and shoot it onto a medium or heavy-weight nucleus. For historical reasons these reactions are called heavy-ion reactions even though they really are heavy-nucleus reactions. It took a dedicated effort to develop beams of heavy ions and to detect the reaction products. The field turned out to be very interesting, showing aspects of nuclear behavior that were quite different from what had been known before. At the Institute for Applied Physics of Heidelberg University, Christoph Schmelzer had been working on particle accelerators and had developed the idea of an accelerator for heavy ions. As a consequence of both these developments, the GSI (Gesellschaft für Scherionenforschung) in Darmstadt was founded in 1969. Hortig died very early but Rudolf Bock became one of the principal scientists at the GSI. In addition to studies of Ericson fluctuations, this is another very important development that influenced nuclear physics in Germany and beyond. Heavy-ion research carried out at GSI was extremely successful, GSI has kept growing, and it is now a multidisciplinary and multinational institution. Heavy-ion reactions play an important role also in elementary-particle physics. CERN sets aside a certain part of its accelerator beam time for that type of work.

Because of the work done by these and other groups, the experimental nuclear-physics effort in Heidelberg became internationally very visible. It was very fortunate that the new Tandem van-de-Graaff accelerator allowed for interesting novel research. It is not only that you have to be good, you also have to be lucky when you decide on such a major investment. The research work at the accelerator was done by groups from within the Institute and groups from the University. Since Bothe's time the collaboration, the contact, between the physicists at the University and at the Max-Planck-Institute has been very close, very intense, and it remains so to this day. This is one of the reasons for our success in Heidelberg. Wolfgang Gentner was a full professor at the University, Hans Jensen was external scientific member of the Max-Planck-Institut. Bogdan Povh, Professor at the University since 1965, had his research group at the MPI. Many scientists at the MPI teach at the University. Postdocs and PhD students follow these examples. That is important because Heidelberg has one of the largest numbers of physics students in Germany. Coping with these numbers would not be possible without concentrated efforts of all the physicists in Heidelberg. In subsequent years, nuclear physics expanded also in other German universities. Many universities established chairs in nuclear physics and the people that were offered these chairs had to be taken from where the biggest activity was and one of or maybe the leading place at that time was Heidelberg. So Heidelberg was also important in providing the human resources for building this very strong effort in nuclear physics in the '60s and '70s.

Tandem-Van-De-Graaff accelerators became rather wide-spread worldwide. For a number of years, these machines dominated the experimental effort in nuclear physics. Compared to the older cyclotrons they had a much better energy resolution, almost the same energy, and they were more flexible regarding the kind of particles one could accelerate. In addition to Ericson fluctuations and heavy-ion reactions, many other phenomena were discovered and investigated. I mention isobaric analogue resonances as an example.

4 Changes

Upon his retirement in 1968, Yale University offered me the chair of Gregory Breit, a very respected quantum theorist. Yale University had also acquired a Tandem Van-de-Graaff accelerator. The head of the Wright Nuclear Structure Laboratory, Alan Bromley, had seen how well the collaboration between the theoretical group and the experimentalists in Heidelberg worked and he simply wanted me to be at Yale rather than in Heidelberg. I was interested. I took a leave for the year 1968/1969, and my family and I went to New Haven. There I came under heavy pressure to accept the position. I was surprised to find that at that time at Yale, professorships in physics were like little kingdoms, one for each full professor of experimental physics. That was not so much to my liking. I really was looking for free exchange and for free collaboration. And Bromley gave me the impression of being more interested in his own career than in physics. I eventually felt that for my work Heidelberg was the better place. In addition my wife and I preferred to be in Europe, and so we returned to Heidelberg after that year.

Concerned about my possible departure, Wolfgang Gentner had meanwhile successfully proposed that I be appointed as director at the MPI. The offer came to me as a total surprise. I had been working with people at that Institute now for 5 or 6 years without having any formal position at the Institute, and I was quite happy with that arrangement. Also I did not need additional funding. I have never had a very big group because I wanted to work myself and needed time to think. I was surprised and not totally happy. I did not wish to relinquish my chair at the University, and I was worried about the additional administrative burden on top of my obligations as full professor at the University. But my colleagues had obviously made a major effort to obtain that position so I accepted the position as director at the Max-Planck-Institute starting January 1968 on top of my professorship at the University.

I mention that because it signaled a major change in the way Max-Planck-Institutes were organized. The Max-Planck-Institute in Heidelberg was founded with Wolfgang Gentner as its one director. That was the standard in the Max Planck Society at that time and in some sense it corresponded to the situation in the universities where you had one chair for a given field. Probably through his CERN connections, Gentner became convinced that modern research in physics needs big Institutes. He had installed a group of directors at the MPI. That was a novelty in the Max-Planck-Society. With Wolfgang Gentner, Ulrich Schmidt-Rohr and Josef Zähringer, I became the fourth director at the Institute. Schmidt-Rohr had worked with Bothe and then held a position at the Nuclear Research Center in Jülich, and Josef Zähringer had come from Freiburg with Gentner. Gentner's interests were not only in nuclear physics but also in the application of nuclear physics methods to the history of the planetary system and to dating and analyzing probes from the solar system and earth. That effort, led by Zähringer, came to be known as cosmo-chemistry even though it was really nuclear physics. In later years the number of directors at the Institute grew even more.

Both at the University and at the Max-Planck-Institute, research in elementary-particle physics played an ever increasing role. That development mirrored the general trend referred to above. I have mentioned CERN as the central European effort. In 1959 the German Electron Synchrotron DESY was founded in Hamburg with Willibald Jentschke as founding director, the person whom I mentioned earlier in conjunction with the experiments done at Chicago that led to my PhD thesis. In theoretical physics, Bertold Stech, also a former student of Bothe's and later a postdoc with Hans Jensen, came back from Caltech in 1958 and built a group in theoretical elementary-particle physics and field theory at the Institute for Theoretical Physics of the University. And after Hans Kopfermann died in 1963, his chair at the Erste Physikalische Institut of the University was split up into three chairs. Two of these were occupied by Joachim Heintze, a former student of Otto Haxel, and by Volker Soergel. These two worked at CERN and at DESY in elementary-particle physics. In leaving the University and joining the Max-Planck-Institute, Bogdan Povh also redirected his research activity towards CERN. CERN was very important for European Physics and beyond. It was the first common European accelerator project. Because of its size it could not be funded by any nation individually. And it brought together physicists from all over Europe into genuine joint research work. Even at the height of the Cold War, CERN brought together scientists from both sides of the iron curtain.

These developments changed the way physicists work. The research work at CERN was done in such a way that the basic infrastructure was provided by CERN but that the detector equipment and the software were at least partly provided by the users so it was a joint effort. Many colleagues at the University spent a substantial fraction of their time in Geneva. It was a tough challenge to combine that research activity in a far-away place with teaching obligations, with faculty meetings and administrative issues and, last but not least, with family life at home. Under such circumstances the entire fabric of human relations at home is in danger of being weakened. Analogous problems arose in nuclear physics some years later. At GSI there is now under construction that new big accelerator FAIR, and people will be compelled to do their experiments there. But it took another 20 years or so in nuclear physics before that happened. Experiments at the home institution have the advantage that the students can be trained in research while still taking classes. Nowadays a PhD student is sent to some far-away research lab but that reduces his chance to take courses and advance his general education in physics.

5 The Student Revolution

The 1960's were a period of great expansion and rapid development for physics in general and at the University and at the Max-Planck-Institute in Heidelberg in particular. But in Germany the decade concluded with a turbulent period, triggered by the May 1968 events in France. The Student Revolution had a strong impact on German universities and in its wake also on German society. I now consider it as an important step in addressing the wrongs of the older generation during the Nazi regime. It had a very positive long-term influence on the development of German society. But at the time it brought big problems as well. And it hit Heidelberg particularly strongly. In its wake the parliament of Baden-Württemberg passed a new law for the universities. All university committees had to have "quarter representation", as it was called: a quarter students, a quarter post-docs or younger assistants, a quarter young faculty, and a quarter full professors, all with equal voting rights. Naturally the young people who volunteered to be elected to these committees belonged to the politically active group, which at that time meant the far left. And that created enormous problems and, above all, cost an enormous amount of time for all concerned. At that time I was elected chair of the committee for curriculum reform in the physics faculty. Basically that is a straightforward assignment: Every ten years or so the curriculum has to be adjusted to new developments in physics and with regard to demands on physicists in industry and teaching. These issues determine the weight given in the curriculum to different fields of physics, to experimental versus theoretical courses, to mathematics, etc. But these were not the issues that we spent most of the time on. The young people in the committee had revolutionary ideas. For instance, they wanted the first-year students in physics to be compelled to attend a course on Marxism-Leninism. Another big item was education. Some professed educators claimed that to teach a subject, you do not have to know the subject. You have to know how to teach it. That idea also had much appeal for the younger members of our committee. They valiantly fought for these ideas, and we spent endless hours in what seemed to the older committee members totally useless discussions. In addition to my teaching duties, I essentially spent two semesters fully engaged in such activities. The effort paid out: Eventually and after lengthy discussions the faculty implemented a new curriculum that was perfectly reasonable.

I have told that story at some length because it affected myself and eventually also theoretical physics at the University. Right after finishing my task as chair of the curriculum committee I was asked to be a candidate for election to the chair of the Faculty of Physics and Astronomy. That was a two-year job and, at the time and because of the sort of discussions I mentioned, a big, time-consuming job as well. In my eyes I had just wasted two semesters, and I was really not keen to accept that new responsibility. On the other hand I considered it not honorable to simply refuse because to chair the faculty is a task that every one of the full professors eventually has to accept. So the only honorable way out of that dilemma that I could see was to quit my chair at the University and switch to full-time employment at the Max-Planck-Institute. I wanted to continue teaching but not have big administrative duties. That is what happened, and in 1972 I became a full-time member of the Max-Planck-Society while continuing as a full professor without pay at the University. I have continued to teach because I believe that a theorist must do that. Otherwise he is in danger to remove himself from the students and to lose sight of the basic aspects of his field.

Shortly thereafter the Filthuth Affair rocked the physics faculty. That was a big scandal related to misappropriation of funds. It might have shattered the faculty. But because of the great integrity and the great engagement of some of my experimental colleagues the faculty was saved from falling apart. Volker Sörgel, Gisbert zu Putlitz, and Joachim Heintze were, I think, the main persons that steered the ship of the faculty through these turbulent waters. Hans Jensen was particularly shocked by these developments. He died in 1973.

These events led to changes which I think proved to be very positive in the long run. Two of the professors in theoretical physics accepted offers elsewhere, I had left my chair, Hans Jensen had died, and a total reorganization of theoretical physics was called for. On the instigation, I think, mainly of Berthold Stech and myself the faculty decided to install a new sub-field of theoretical physics at the Theoretical Institute. In addition to chairs in elementary-particle physics and nuclear physics, two chairs were devoted to statistical and condensed-matter physics and were filled by Franz Wegner and Heinz Horner. That broadening of the spectrum of theoretical activities at the University turned out to be extremely beneficial for all of us. Applying quantum theory, theoretical condensed-matter physics had taken a fantastic development in the 1960's and had expanded very dramatically, both in terms of subject matter and in terms of the number of people involved in research. I personally benefitted very much from discussions with Franz Wegner and Heinz Horner and from their seminar.

6 The Minerva project

Hans Jensen had been a member of what was called the Gentner committee or, later, the Minerva Committee, a committee that oversaw the exchange between Israel and Germany of young scientists and the distribution of research funds that came with that exchange. That committee was chaired by Wolfgang Gentner. After Jensen's death Gentner asked me to step in.

It seems necessary at this point to recall some events in the life of Wolfgang Gentner and the history of the Minerva project. After a PhD in what would now be called Biophysics in Frankfurt and a postdoctoral position at the Institute Curie in Paris in the early 1930's, Wolfgang Gentner had joined Bothe's group in Heidelberg. During the war Gentner was drafted to the German Army. After the armistice with France, Paris was occupied by the Germans. There Frédéric Joliot, incumbent of the chair of Nuclear Chemistry at the Collège de France, had built a cyclotron.

That accelerator was not yet operational. Gentner was ordered by the German Army to inspect the status of the machine. He himself has written about that episode. He saw that the high-frequency part of the accelerator needed improvement. He was asked to act as director-in-charge of Joliot's Institute and to see to it that the cyclotron would be completed. That was morally a difficult assignment, particularly because Gentner knew Joliot well from his postdoctoral stay in Paris. During his first visit and during an unobserved moment he asked Joliot: "Can we meet afterwards down in the café?" And so they met in the café and Gentner asked Joliot: "Do you want me to do that?" And Joliot said, better you than anybody else. So Gentner accepted and became director-in-charge of Joliot's Institute.

Gentner and Joliot agreed that the German physicists would occupy the first three or four rooms of the Institute. Before they would go further into the Institute, Gentner would call Joliot. Gentner's team took on the responsibility for the high-frequency part of the cyclotron. During that time first Paul Langevin and later Joliot himself disappeared. They had been detained by some German authorities. Gentner had a hard time finding out by whom, the Gestapo, the SS, the SA, or the army. Eventually Gentner managed to have them set free. Paul Langevin left Paris and spent the rest of the war in Geneva. Eventually I think somebody told on Gentner. Apparently he was too close to the French. In any case, in 1942 he was replaced by Wolfgang Riezler. Gentner returned to Heidelberg to work with Bothe. After the war Gentner was awarded the rank of officer of the Légion d'Honneur which I think is a wonderful recognition of the way he acted.

After the war and while a professor in Freiburg, Gentner served as director of the synchrocyclotron in Geneva. There he met Armos de Shalit, an Israeli physicist who at that time was chair of the department of nuclear physics of the Weizmann Institute of Science. In that capacity he visited CERN. De Shalit was aware of Gentner's history, of the fact that he was trustworthy and had been anything but a Nazi sympathizer. I later had the good fortune to meet Amos. He was a visionary, warm, and wonderful person. He was convinced that it was necessary that Germany and Israel should somehow establish relations again. At that time there were virtually no contacts. There was no embassy, there were no scientific or economic relations. Parallel to a high-level meeting of Ben Gurion and Adenauer in New York, de Shalit and Gentner discussed ways of starting contacts between scientists of the two countries. As a result and with the blessing of the two governments, a group of the Max-Planck-Society comprising Otto Hahn, the president, and Wolfgang Gentner travelled to Israel in 1959 and visited the Weizmann Institute. Upon his return Gentner wrote a memorandum suggesting the exchange of young scientists. Eventually that program was implemented, and it was first supported by the Volkswagen Foundation as a startup program. Later it was taken over by the German government. The Israeli side refused to accept funds from the German government. So the money was funnelled through a subsidiary from the Max Planck Society called the "Minerva Foundation of Research." And that is why the whole program and the committee that supervised the operation eventually carried the name of Minerva. The Minerva committee was composed in equal parts of German and of Weizmann scientists. The German members were selected by Gentner. Hans Jensen, Heinz Staab, a Heidelberg chemist, later president of the Max-Planck-Society, and Feodor Lynen from Munich, a biochemist, were among the first members.

Before the first young people from Germany could come to the Weizmann Institute, a vote was taken separately in each department. The vote had to be unanimous. The first department to accept German visitors was the Department of Nuclear Physics. Lorenz Krüger, a close friend of my wife and myself, and another theorist were the first visitors. The program was started with theorists from Hans Jensen's Institute. Theorists do not need to be in contact with persons from the workshops, and the danger of a misunderstanding is minimized. That was in 1961. A little later the Minerva Committee was established to oversee the exchange. It took several years before the first Israeli young scientists would come to Germany. For them it was very difficult for obvious reasons. The first physicist that came to Heidelberg was Uzy Smilansky from the Weizmann Institute. He came with the assurance of his Dean that he could return any time without giving any reasons. But he stayed for the full length of his term. The program has been very successful. Part of the reason is that during the first encounters, the scientists could discuss scientific issues that had nothing to do with personal history and Nazi crimes, and could do so on a completely rational and detached basis. And after some trust had been established they could start talking about the serious historical and moral issues. At the beginning the program was confined to the Weizmann Institute. But when it became so successful, other institutions of higher learning in Israel joined in. The Minerva program was extended and has become a major operation, both in terms of the number of people and in terms of the funds that are involved.

Over the years and via my membership in the Minerva committee I became involved in various aspects of the scientific exchange between Israel and Germany. I have been visiting Israel at least once a year for the last 35 years and my wife and I have made friends in Israel. After all that has been done to the Jews in Europe, to me having an Israeli friend is something very special, and I consider it wonderful that I have been given the chance to be involved in this program.

Various encounters had prepared me emotionally and psychologically for serving on the Minerva Committee. When I was a PhD student we had a visitor, a Fulbright fellow, Aaron Temkin, whom I made friends with. He was Jewish and he came, as he said, to Heidelberg not only because of Hans Jensen but also to find out who were the people who killed all the Jews. That was my first encounter with a Jewish person. At Caltech there were quite a few Jewish people, and there was always this distance that they kept to me before we got to know each other better. Conversations following these encounters had made me acutely aware of the Nazi crimes and of the distress they had inflicted upon the Jewish people, much more so than reading newspapers or historical accounts.

I served on the Minerva Committee for 20 years. After that I held for 9 years a scientific-administrative position at the Weizmann Institute that had nothing to do with the Minerva program. I mention this because it shows how the atmosphere has changed over these 35 years. It has become possible for a German to serve in an important position at the Weizmann Institute. Another beautiful aspect of the same story relates to the retirement of Dirk Schwalm, one of my co-directors at the Max-Planck-Institute in the 1990s. At the time of his retirement the Max-Planck-Society was short of funds, and the installment of a successor would have to be delayed by three years. But at that time there was an important and very expensive piece of equipment under construction at the Institute, and a successor of Dirk Schwalm was badly needed to keep the effort going. Dirk Schwalm had been collaborating under the auspices of the Minerva program with Daniel Zeifman, an experimentalist at the Weizmann Institute. He stepped in for three years as director at our Institute. He could not actually serve the full term because in the middle he was elected president of the Weizmann Institute.

The history of the Minerva project is really a beautiful story. It shows that science can help bringing people together. Nowadays Germany is the second most important exchange partner in science for Israel after the United States. Of course every young person from Germany becoming involved must address the past and become aware of what has been done. There exist several historical accounts of the project. The latest is the article by Dieter Hoffmann [Hoffmann 2015] where earlier references may be found.

7 The 1970's

In the 1970's nuclear physics had become an established field. As in every such field, the atmosphere in nuclear-physics research differed from that of the pioneer days. There were what you might call routine questions that aimed at completing our understanding. There was less in the way of opening new windows onto nature. There was still the determined search for novel insights, of course. It spurred the development of new accelerators like that at GSI. In that changed environment it was vital for our Institute to retain the spirit of curiosity and to remain competitive. The success can be measured by the fact that the Institute continued to produce young people who later made a big career and are now senior leaders in the nuclear physics community in Germany and elsewhere. The experimental directors at the Institute discussed ways to widen the spectrum of experimental possibilities. Eventually that led to the construction in the 1980's of the test storage ring (TSR) at the Institute. Charged particles accelerated in the Tandem Van-De-Graaff could be stored and investigated or used further by making them revolve in the ring. The TSR became operational in 1988 and was used to perform accelerator, atomic and molecular physics experiments. That machine turned out to be a big hit, similar devices were installed elsewhere, and its successor serves an important function in the Institute even today.

In the 1970's I was interested in two topics. Reaction theory remained central. An important second line was the perturbative expansion of the effective nucleon-nucleon interaction of the shell model. That expansion – known as the linked-cluster expansion – followed ideas of Bethe, Brueckner, and Goldstone. For reviews, see [Rajaraman and Bethe 1967] and [Brandow 1967]. Calculating the effective interaction in terms of the basic interaction between free nucleons along such lines was very popular among nuclear theorists at the time as it promised to yield a complete understanding of the shell model from first principles. Thomas Schucan and I showed that the linked-cluster expansion diverges. Results obtained in that framework are, therefore, of doubtful use [Schucan and Weidenmüller 1972, 1973]. As a consequence of this and other developments the entire effort was essentially abandoned for about 30 years. New and non-perurbative approaches were needed to revitalize it.

In nuclear reaction theory, my interests turned toward the following aspects: precompound reactions, deeply inelastic heavy-ion reactions, and nuclear fission. In the first case, a light projectile of several 10 MeV energy generates a series of two-body collisions with the nucleons in the target nucleus. That eventually leads to the formation of an equilibrated system (the compound nucleus). But during the precompound phase of the reaction, particles are emitted that carry higher energy and are more forward-peaked than is the case in a compound-nucleus reaction. The challenge was to work out the distribution in energy and direction of these particles, and to base the theory on a statistical approach. The deeply-inelastic heavy-ion reactions are grazing collisions of two massive nuclei in which a substantial amount of the energy and angular momentum of relative motion is transferred into internal excitation energy and spin of either fragment. The process is accompanied by substantial mass transfer between the two reaction partners. Because of the large number of degrees of freedom involved in these reactions, a statistical approach again is called for. In nuclear fission there was growing experimental evidence that friction-like forces play an important role on the way of the fissioning nucleus from the saddle point to the scission point. Fission is a classical concept that again relates to the presence of many degrees of freedom and calls for a statistical approach as well.

I became convinced that a thorough understanding of these processes could be attained only if first the comparably simpler compound-nucleus reaction was thoroughly understood on a statistical microscopic basis. That led me to random-matrix theory. I have mentioned already that the idea goes back to Wigner. In his formal theory of nuclear reactions (the Wigner-Eisenbud theory mentioned above) there appeared these resonances that had led Niels Bohr to his idea of the compound nucleus. Wigner was grasping for a way of dealing with these resonances theoretically. The question was: What can we say about a system about which virtually nothing is known? Wigner hit upon the answer accidentally in a book on mathematical statistics. In physics terms, the answer goes as follows. The dynamics of a quantum system is governed by the Hamiltonian which encompasses the kinetic energy and the interaction between the constituent particles. In Hilbert space you can write the Hamiltonian as a matrix. That is a quadratic arrangement of numbers. Each element of the matrix is obtained by sandwiching the Hamiltonian between a pair of states of Hilbert space. The matrix gives a complete representation of the dynamics of the system. And the idea of random-matrix theory is to say all the elements of that matrix are random numbers drawn from some probability distribution. That certainly sounds wild. There is no dynamical input. And the question is whether this can lead you anywhere. It actually does, and it has become a very big field in physics and also in mathematics.

The idea behind random matrices is not as crazy as it may look at first sight. If instead of an individual physical system described by an individual Hamiltonian, you consider a set of different physical systems, each with its own individual Hamiltonian matrix, and you ask: How does a specific element of the Hamiltonian matrix change as we run through that set? – then it may not be so far-fetched to say that the resulting sequence of matrix elements forms a set of random numbers. That hand-waving argument may make it plausible why random-matrix theory is useful in determining universal properties of quantum systems. In other words, random-matrix theory describes those properties of a physical system that are not system specific and are shared by many systems. And such universality is the reason for the tremendous success of random-matrix theory in several branches of physics. The theory actually does not encompass all physical systems. It uses probability theory with an integration measure. Just as the rational numbers form a set of measure zero on the real axis with regard to the common Riemann-Lebesque integration measure, there are physical systems that have measure zero with regard to the measure used for random matrices. In that sense the theory makes statements not about all but about almost all physical systems.

Wigner, Dyson, Porter and others succeeded in deriving properties of the states that underly the compound-nuclear resonances. But there was still no connection between random-matrix theory and Wigner's theory of nuclear reactions, and it remained a challenging task to write down and work out a statistical theory of nuclear reactions where the Hamiltonian would be a random matrix, and to deduce properties of nuclear cross sections within that approach. The connection between random matrices and nuclear cross sections had become an important topic. Peter Moldauer at Argonne National Lab worked on it for many years [Moldauer 1980]. The MIT group around Arthur Kerman made important contributions, obtaining results that, although based upon intuitive reasoning, were significant [Kaway, Kerman and McVoy 1973]. But a consistent overall theory was still lacking. The problem was not only theoretically interesting. It was also practically important because the community needed precise predictions for nuclear cross sections where these had not been or could not be measured. Our group in Heidelberg worked intensely on that problem during the 1970's. We made use of the insights obtained within the shell-model approach to nuclear reactions. We did make progress but the complete solution of the problem still eluded us. Relevant papers are [Engelbrecht and Weidenmüller 1973] [Hofmann, Richert, Tepel and Weidenmüller 1975] [Agassi, Weidenmüller and Mantzouranis 1975]. "We" here stands for a number of collaborators with whom I worked over the years. I have been very fortunate time and again to work with excellent young people throughout my career in Heidelberg. In fact there are very few papers that I signed as sole author.

A solution to the problem, accompanied by a widening of the scope of compound-nucleus reaction theory, was arrived at only in the 1980's, concurrent with increasing interest in chaotic motion in quantum systems. I mention that only briefly in order to conclude my story and not as part of an ongoing historical narrative. The "Bohigas-Giannoni-Schmit conjecture" postulated a connection between the spectral properties of chaotic quantum systems and those of random matrices [Bohigas, Giannoni and Schmit 1994]. The conjecture (well established by now) widened the scope of random-matrix theory in physics. Compound-nucleus scattering now came to be viewed as a special case of chaotic scattering, a general problem of quantum physics. Coupling a random-matrix Hamiltonian to a number of channels and using Efetov's supersymmetry method, Jac Verbaarschot, myself, and Martin Zirnbauer succeeded in solving the compound-nucleus problem. More precisely we calculated the S-matrix correlation function for chaotic scattering [Verbaarschot, Weidenmüller and Zirnbauer 1985].

There still remained the dichotomy between the success of the shell model at low excitation energies and that of the random-matrix description of nuclei at energies near and above neutron threshold. Light on that problem was cast, among others, by Vladimir Zelevinsky and his group in the 1990's. The residual interaction of the shell model mentioned above is the agent that bridges the gap. That interaction is important at low excitation energies, for instance, in lifting degeneracies amongst shell-model states. With increasing excitation energy it causes an increasing mixing of shell-model configurations. Using large-scale shell-model calculations involving thousands of shell-model configurations, Zelevinsky and collaborators showed that with increasing excitation energy the mixing becomes ever more efficient and eventually causes the spectral properties of the shell-model Hamiltonian to resemble those predicted by random-matrix theory [Zelevinsky, Brown, Frazier and Horoi 1996]. That is a very satisfactory insight. It was later supplemented by further numerical evidence but a general theoretical proof is still lacking.

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