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My Life as a Physicist

How did I find out that I wanted to be a physicist? Certainly, my interest in mathematics is much older than that in physics. I know that my father explained prime numbers to me when I was quite small, and this subject fascinated me for years to come. I also know that I was fascinated by the eddy in a sink outlet after washing dishes for my mother. She told me that I asked her at an early age: *“PLEASE may I explain Ohm’s law to you?”*

My father was a meticulate book-keeper who worked for the Siemens company and gradually rose in administrative rank with the years. It am sure that my interest in collecting and comparing data from different sources, is one of his gifts for me.

I was probably 13 or so when my father read Pascual Jordan’s book „Die Physik des 20. Jahrhunderts“ aloud to my mother, and then my parents were surprised when I made some remarks about the text. The physics teachers in school were not inspiring. But in 1947, when I was in my last grade in Gymnasium, my parents wondered about my future and somehow had the idea that I should become a theoretical physicist (maybe because this field is close to mathematics, and because they felt that one could not make a living being a mathematician). So my father asked a physicist colleague for a physics textbook, and brought home the „Lehrbuch der theoretischen Physik“ by Georg Joos, for me to look at. The preface of this book starts: „Dies Buch soll keine Bergbahn sein, welche den Leser mühelos auf Gipfel der Erkenntnis bringt“, and this was quite true: as I remember, I hardly understood anything.

My career as a teacher had already started much earlier. At the age of 12 or 13, in Berlin, I gave help sessions in mathematics to my fellow pupils. My first job was a summer job: in **1943**, at 13, I taught mathematics to the miller’s younger son at the Brunnermühle in Traunkirchen, Upper Austria, for free meals and lodging (this was also a welcome vacation from the nightly bombing in Berlin - actually, I never went back to Berlin).

In **1947**, I started to study Mathematics and Physics at the University of Vienna. There were so many students that we got seating tickets for either even or odd days. But when I had

„even“, my friend Gert Sabidussi (who is now professor of mathematics in Canada) got „odd“, and we then shared a seat. The perfect lectures by Prof. Radon on calculus and by Prof. Hlawka on analytical geometry were especially memorable. For many years after, when I stood at the blackboard, I imitated Prof Radon’s way of letting the chalk oscillate for a while before starting to write. And in some ways which I don’t understand, Prof. Adam (a colleague at the University of Linz who started to study late in his life) obtained my careful lecture notes on analytical geometry as a study help: he returned them to me some 45 years later.

Prof. Hans Thirring gave his 6-semester course on theoretical physics. He had all his numbered equations in a little booklet, and whenever he gave the course again, there was a slight renumbering. Shockingly, there was no quantum mechanics in this course! (Later, his assistant Robl ventured first to give a course on quantum mechanics).

Prof. Ehrenhaft, returned from the USA after involuntary exile due to his Jewish descent, gave a two-semester introductory course on physics. This was more like a circus than like a course of lectures. He kept repeating that the „accepted body of knowledge“ is wrong in many respects. One of his points was that he had measured the charge of the electron at the same time that Millikan did, but that he had found also fractional values of the elementary charge (it was long after Ehrenhaft’s death that it became acceptable to go through Millikan’s notebooks to look for evidence of fractional quark charges!). He kept insisting that magnetic monopoles exist, long before they became fashionable again. Another point was that photophoresis is a new fundamental phenomenon; only after after his death did his co-workers explain it as a complicated effect due to established physical laws. But the experiments that he and his assistants showed were beautiful and impressive.

In **1949** and **1950** I had my first contacts with nuclear physics: Prof. Berta Karlik’s course on radio-activity, and the corresponding laboratory in the well-renowned old „Institut für Radiumforschung und Kernphysik“ under her direction. In the laboratory course, a fellow student, Rupert Patzelt, and I decided that we wanted to know how radios work, so we built a one-tube radio (He later became a professor of Electrical Engineering). Although Karlik was not an inspiring teacher, my interest in nuclear physics was aroused (it was, of course, a very modern subject at the time). So, when I got the wonderful chance to study at Purdue University in Lafayette, Indiana, USA, in **1950/51**, I took Nuclear Physics again (among other things), especially lectures by Prof. E. Bleuler from Switzerland, a student of Paul Scherrer's (ETH, Zurich). And I managed to get my Master's Degree in this one year, based on a thesis on the quantum efficiency of Geiger counters with brass cathode, measured using coincidences. This was under the direction of Prof. Steffen (also a student of Scherrer's).

My year in the US was made possible by a scholarship of the US State Department (for re-indoctrination of German and Austrian students), and one of the absolute rules of this scholarship was the requirement to return to Austria after the year. So I did return. But Steffen had invited me to get my Ph.D. under his direction, and so I was back at Purdue in time for the Spring Semester, **1952**. The aim of my thesis was to study the attenuation of g-g-angular correlation in the decay of 181Hf, due the time-varying fields in liquid solutions, at different temperatures. To detect the radiation, I did not use Geiger counters any more, but sodium iodide crystals (which I canned myself). It was indeed possible to find an influence of temperature (and hence viscosity) upon the attenuation, but I doubt if we ever really understood what was going on. Anyway, I finished my work in December, 1954, and I got my Ph.D.

At Purdue, I lived on a Half-time-teaching Assistantship. Teaching laboratory courses to American students was another useful step in developing my teaching abilities.

Steffen had written to several fine US laboratories trying to find a post-doctoral position for me, but it seems that I was not really interested in staying in the US. I also went to interviews at a few small colleges, but again, I did not really want a job. I am not sure if I had really understood in my heart that I would now have to make a living, using what I had learnt.

So I returned to Vienna, and I asked Prof. Karlik whether I could work half-time in her Institut. She agreed, and I got 600 Austrian schillings a month (more like an honorary stipend than a salary). I was very kindly accepted by three energetic young assistants, Rupert Patzelt, Hans Warhanek and Peter Weinzierl (who later all became University Professors in Vienna). They were about to modernize research at the Radiuminstitut, by designing and building electronic equipment. Among other things, I used this equipment to investigate the decay scheme of AcX (223Ra, a decay product in the natural actinium series).

When Karlik learnt that I was going to to marry her secretary, Elisabeth Mathis, she very kindly used her personal contacts so that I got a Ford scholarship to work at CERN in Geneva, starting in October, **1957**. Here, the new 600-MeV Synchrocyclotron (SC, now dismantled) was ready to go (but no research had been done as yet), and the first Proton Synchrotron (PS) was just being built. The SC Division was under the direction of W.Gentner and G.Bernardini; I was assigned to work in a group under the direction of A. Merrison (later: Sir Alec) and G. Fidecaro. Tito Fazzini, another group member and a fine physicist, became a lifelong friend.

At that time, the problem of the electron decay of the charged pion excited many people. Using Fermi´s theory of the weak interaction and a few simple arguments, one could show that, in addition to the regular decay into a muon, there should also be a decay into an electron, in about 1 of 104 cases. But Steinberger at Columbia University, an experienced physicist, had shown experimentally that the electron decay happened at a rate smaller than 1 in 106.

So the idea arose that we should check Steinberger's result. The SC was well equipped to produce pion beams. Because of the small mass of the electron, its kinetic energy would be 70 MeV, much larger than that of the muon. So a large telescope was built of alternating layers of scintillation counters and absorbers, to detect the electrons. In order to determine the detection efficiency of this telescope, it was decided to simulate the passage of electrons by means of a Monte Carlo program. Fidecaro made a block diagram of this program, Prof. Ashkin from Philadelphia, a visitor, helped with the description of radiative effects, and a very fine programmer from the CERN computer division wrote the program.

CERN had a new computer built by Ferranti (called „Mercury“), one of the best available at the time. It used vacuum tubes, of course, and consisted of many racks that filled a whole room. The wiring was point-to-point: it looked highly confusing. An English engineer had the sole job of taking care of the computer. The computer must have been able to test itself, for when it was „happy“ (i.e., in good working order), it beeped a simple tune.

My interest in computing was aroused, and so I decided to write another program based on the same block diagram, but using spherical rather than Cartesian coordinates, which should make it a little more accurate. I used numerical machine code (I think that no higher computer language was available at the time). When the programmer and I had both finished, we compared results, and - naturally - we did not agree. Since we did not know who was right we decided to use identical electrons (i.e., identical random numbers), and so we finally found the reason: at one point in the block diagram, one had to check the polar angle to make sure that the electron does not go backward. The condition on the block diagram read: Is q > p/2 ? For me it was clear that q meant ½q½, but the programmer had taken it verbally. So that explained the difference.

In July, **1958**, there was a high energy physics conference at CERN at which Feynman proposed wild ideas how one could reduce the expected rate of 1 in 104 to Steinberger's much smaller value. At that time, we had found 26 events that looked like p-electrons, and Steinberger had photographed 105 pion decays in a bubble chamber and was beginning to scan the pictures. In August, at the time of the conference on the peaceful uses of atomic energy at CERN, we had 40 events, found with an improved telescope. We learned from Salam that Steinberger had now found 4 events, so it was time to publish. Our result was presented at the conference, during an „Informal Session on Fundamental Physics", chaired by Weisskopf, who said that he was pleased not only that the first scientific result from CERN had turned out to be such an important one, but also that the simple theory turned out to be the correct one.

Walter Thirring (Hans Thirring's son), then professor of theoretical physics at the University of Bern, invited me to give a talk on our work at Bern. He said that I should try to get my Habilitation in Vienna, at the University. Later, when I said this to Prof. Karlik, she was not pleased at all. (It took ten more years until I became a Dozent for nuclear physics, and not under Karlik at the University, but under Weinzierl at the Technical University).

Since we had a nice pion beam set up at CERN, and a nice fast oscilloscope on which the pulses from pion decays could be displayed and photographed, Fidecaro had the idea that we should use our equipment to measure the lifetime of the charged pion, with an accuracy of 1%. A run was made during which 10000 decays were photographed in a few hours. There was also Goldschmidt-Clermont's fine measuring projector for analysing bubble chamber pictures, and a team of nice French ladies trained to use this machine. I got the job of supervising the film analysis (which took weeks, I think), and to write the program that would analyse the numerical data produced by the ladies on the machine. So we published a paper on the pion halflife. (When one looks now into the Table of Fundamental Particle Data, one sees that our estimated error was too small. Maybe the frequency of the oscillator used for time calibration was not as good as claimed).

While we are in Geneva, an Austrian Reactor Center is being planned and built at Seibersdorf, 30 km southeast of Vienna, under the direction of Higatsberger.(I remember that even in these years, it was not easy to find a location where a nuclear reactor would be acceptable to the people, but fortunately, the mayor of Seibersdorf had been somehow involved with the German reactor program as a soldier). Weinzierl is going to head the Institute of Physics, and Patzelt the Institute of Electronics. Weinzierl asks me to work with him in his new Institute, and since my position at the Radium Institute (from which I am on leave) is not a particularly good one, I accept. No doubt, the idea of starting work in a new place with new equipment is also attractive. When I tell Karlik of my decision, she is disappointed (she writes „meine Planung ist jetzt zerrüttet“ (!)) and tells me that surely she would have found a better position for me to return to. I think she never forgave me that decision in the years to come.

The Reactor Center is not ready yet, but fortunately, Prof. Steffen asks me to spend a year (**1959/60**) at Purdue as a Visiting Professor, while he spends his Sabbatical Year in Europe. So I gladly accept. My task is to supervise the research of his graduate students. But I am asked also to teach a physics course for liberal arts students, and I accept, although at that time I still cherish the bias of my student days that at the university, first rate people do research, while second rate people teach. This course is based on the book by G. Holton. While a graduate student at Purdue, I had taught recitation sections for this same course, together with L. Grodzins, who later became a professor at Massachussetts Institute of Technology.

During the year, I also measured angular correlations of g rays in the decay of 124Sb. In talking to Prof. Bleuler, I became interested in a different problem: the shapes of beta ray spectra. The shape of an allowed beta ray spectrum should be essentially statistical and hence, not contain much information. But Prof. Langer at Indiana University had been measuring many shapes with his transverse magnetic spectrometer, and he consistently found deviations from the expected statistical shape. Could these be real?

On Oct. 1, **1960**, I started work at the Reactor Center Seibersdorf (which had been officially opened the previous day). A Siegbahn-Slätis intermediate image beta-ray spectrometer had been set up in the Physics Institute, and one of my first jobs was to get it going. When it worked, I started to measure beta-ray shapes. We had a reactor to produce radioactive sources, a large mass separator to make weightless sources of separated isotopes, and I soon had a system built that would measure spectra automatically, over many hours. And: I did not find Langer's deviation from statistical shape!

But how should I prove that my results were right and Langer's were not? After all, I was a newcomer to the field, and I was working essentially on my own (although in contact, by letter, with many insiders). I studied the spectrometer in great detail, discussed all the possible effects that could produce distortions, and used the result to put a realistic error limit on the result of my measured shapes.

At that time, Higatsberger became quite interested in the Dragon project, a high-temperature helium cooled nuclear reactor that was being built in the south of England to demonstrate the feasibility of such a system. Several departments of our reactor center obtained research contracts to study properties of the fuel that was to consist of graphite coated uranium carbide. An important question was: how well are the fission products contained by the graphite coating? We obtained pieces of graphite from irradiated fuel, measured their gamma spectra, and determined the quantities of fission products by analysing the spectra using a least squares' computer program.

In those years, the US Air Force still had the nice habit of supporting basic research in foreign countries. In a discussion between Weinzierl and an Air Force official the idea came up to study the decay of the neutron using our reactor. A proposal was submitted, and we waited.

But I also felt that it would be good for my scientific career to spend again a year in the US. I asked Weinzierl if he would let me go, and he answered, hesitantly: „If our proposal is turned down by the Air Force, you may go!“. Grodzins (now at MIT) suggested to me that I might write to Mrs. Scharff-Goldhaber at Brookhaven National Laboratory (with whom he had worked before), asking if she had a visiting position for me. Indeed she had, and so I spent the academic year 1965/66 with my wife and two children on Long Island.

It was Mrs. Goldhaber's idea that I should, helped by Mike McKeown, search for a parity admixture in a nuclear state of 180mHf, which would manifest itself by a small circular polarization of the emitted low energy gamma radiation. The sources were produced in the local nuclear reactor, and the measurement was done by comparing the absorption in a thin sheet of iron magnetized either up or down. We had a modern solid-state multichannel analyzer which filled a whole man-sized rack. When it went bad, I had to call the expert at the firm in Chicago, tell him my trouble, and he would tell me on the phone how to repair it.

Unfortunately, it turned out that there was no parity admixture.

I used some of my time at Brookhaven to write a paper for Nuclear Data Tables giving all the known measured shapes of beta ray spectra (this did not make Mrs. Goldhaber too happy). Later, H. Daniel wrote a similar paper for Reviews of Modern Physics, for which he copied my table. But I must add that he also wrote a theoretical introduction (which I did not have in my paper).

When I returned to Vienna in the summer of **1966**, it turned out that the research project on neutron decay had been accepted by the US Air Force after all. The aim was to let the neutrons decay in a beam tube tangential to the reactor core and measure the energy spectrum of the decay protons flying along the beam tube through the reactor wall and into an electrostatic spectrometer situated outside the wall. Interpretation of the spectrum shape would then give the same information as the electron- antineutrino angular correlation, i.e., the ratio of the axial vector and vector coupling constants of beta decay.

Clearly, this was a difficult and complex undertaking requiring the cooperation of many people, including Ph.D. students who would get the task of designing and making particular parts of the whole apparatus. The project was under the direction of Weinzierl, and I felt somehow it was my duty to assume the position of a sous-chef, especially later when Weinzierl became Professor in Vienna and stopped coming to Seibersdorf every day. Dobrozemsky with his technical skill and wild enthusiasm was also very essential to the project.

Naturally, from my experience with beta ray spectrometry, my personal interest was centered about the evaluation of the data, and about the influence of the many disturbing effects that could possibly affect the spectrum shape. I wrote two papers on these questions together with a student of theoretical physics, O. Nachtmann, assigned to Seibersdorf during his military service to learn something about nuclear radiation. I was impressed by the mathematical methods he used (which he considered trivial). He is now professor of theoretical physics in Germany.

The neutron decay project took about 10 years, a terribly long time; it outlived many of the coworkers, i.e., students who got their degrees, and in a sense it outlived also me, since I contributed only little after going to Linz.

In the summer of **1970**, Wilhelm Macke called me on the telephone. He had escaped from Dresden some time before, was now Professor of Theoretical Physics at the newly founded Hochschule in Linz, and had started single-handedly to teach physics there in the fall of 1969. He asked whether I would be interested in becoming a Professor of Experimental Physics in Linz (Weinzierl had suggested my name to him). I said: „I think this should be discussed“, and he answered: „I think so too“. So I made my first visit to Linz, and I accepted his proposal. There were many more visits to Linz, for the external structure of the physics building had been erected, and now the question was what the internal structure should be (Technical requirements for the laboratories, for the experimental lecture hall, for the student laboratories etc. etc.). I met with Prof. Macke and with the architects every week or fortnight and they asked me questions about the requirements for the building. Often I did not know the answer, but I promised to tell them the next time, and I did tell them. It was a very interesting period, a good cooperation with the architects who had never planned a laboratory building before but were now quite enthusiastic.

I became Professor of Physics on April 1, **1971**. This was a wonderful challenge: to build up a program of teaching experimental physics, to bring together a group of young scientists and, together with them, to start a research program from nothing and to take care also of the necessary auxiliary requirements like machine shop and electronics (there had been no experimental chair in Linz before mine).

Two former students came with me from Seibersdorf: O. Benka who had built the electrostatic spectrometer, and D. Semrad who had built the proton test source for the neutron decay experiment. A. Kropf (also from Seibersdorf) was to be responsible for our first computer.

An advertisement in Physics Today gave me the idea to buy a 700 keV Van de Graaff accelerator. Clearly, this could be used for atomic collision physics but not for nuclear physics. So (maybe without knowing) I followed the general trend of the time: to use nuclear physics apparatus to return to the field of atomic physics that had been somewhat neglected due to the great advances in nuclear physics in the preceding decades. I had also a trivial reason for preferring atomic to nuclear physics: there are many hundreds of very different nuclei, but only about one hundred elements: this makes it easier to know them! M. Geretschläger, another new collaborator, was to become the head of our accelerator laboratory.

After coming to Linz, I stopped doing experimental work myself (and considered myself „theoretically an experimental physicist“). Inspired by our work on neutron decay, I used a least squares' program to see how the many experimental data on beta decay available can be used to find not only the size of the vector and axial vector coupling constants, but also of the scalar and tensor constants that might be present although small.

My last work in nuclear physics was a collaboration with H. Vonach: to use the excellent very long time-of-flight path at the large tandem accelerator in Garching (near Munich) to obtain precise beta decay end point energies (for superallowed transitions) not by beta ray spectrometry, but by measuring the Q-value of a corresponding nuclear reaction.

In **1975**, our research program in Linz was producing first results: we had started to investigate proton-induced x-rays, both for the purpose of understanding better the physical process, and for improving its main application: non-destructive chemical analysis by PIXE.

After having served as senator, as dean of our faculty, and as rector of the University (the former Hochschule), I obtained a sabbatical semester in **1978/79**. Spending several weeks at North Texas State University, I was introduced into the theory of proton-induced K-x ray emission (together with Werner Brandt of New York University, Basbas had worked on the corrections that have to be applied to the plane wave Born approximation to make it useable in this field). And, naturally, I wrote a computer program on the time-sharing computer of the department, to compare the experimental data for K-x ray cross sections with each other and with theory (Tom Gray very kindly had sent me two boxes of IBM cards containing his published table of experimental cross sections; this was my original data base). Comparing the data after dividing by Brandt's theory made it possible to distinguish reliable from unreliable data.

I also convinced the people at North Texas to invite John Reading from Texas A&M University to give a seminar on his method of calculating x ray cross sections by the method of coupled channels. This started my cooperation with Reading. I soon noticed that Reading's program tended to give too low cross sections, a discrepancy that has never been resolved, I think (even though Reading, much later, wrote a paper on what he called „The Paul Discrepancy").

During my stay in Texas, I had the idea of organizing a „Workshop on Theories of Inner Shell Ionization" in Linz. It should be small and cheap, bring the experts together in a friendly atmosphere, and offer as much time for discussion as for presenting papers. Basbas and Reading were willing to come. I called Brandt in New York; he also thought it was a good idea and promised to come to Linz in May, on his return from a trip to the far east.

So the first of three workshops on inner shell ionization was born, and it ran quite well. The papers of all the three workshops were eventually published in Nuclear Instruments and Methods, and thus produced a nice synopsis of the state of the art. The reviewing system was not very formalized at the time: the editor, Kai Siegbahn, simply entrusted this job to me, so I and my collaborators reviewed all the papers, without the cloak of secrecy. One of the authors still teased me years later about all the things I had asked him to change.

With better and better understanding, my description of experimental K x ray cross sections improved, and I produced many papers on the subject in the course of time. On one of his visits to attend a workshop, George Basbas called me a „one-man data evaluation center". One of these cross section papers („An analytical cross section formula...") was in the hit list of the internationally most quoted Austrian scientific articles supported by the Austrian Funding Agency produced by that agency several years ago. I do not consider it so very good, but an analytical formula is very convenient, of course.

Eventually, my interest shifted from inner shell ionization to stopping power of positive ions, a subject in which D. Semrad and his students had already been active for many years. Here again, the question came up: how do we prove that the proton stopping powers measured in Linz - often rather different from accepted values - were correct? One important step was to start the collaboration with P.Mertens of the Hahn-Meitner-Institut, Berlin, so that the same proton stopping powers could be investigated with different apparatus, different methods, identical or different targets, until the results agreed. And eventually they did! But another important step was that we held a "Workshop on Stopping Power of Low Energy Ions" in Linz in **1984**, and a second one in **1986**. These workshops not only brought international experts together and advanced our knowledge, but these experts could also meet our group personally and see our apparatus live, and this helped acceptance. A third stopping power workshop was finally held in **1993**, with John R. Sabin from Florida as a co-chairman.

To make our stopping power values for protons more acceptable to the scientific community, we made a statistical analysis of all the data (including our own) for five metal targets in **1991**. But it seems we did not impress Jim Ziegler of IBM very much, since he apparently did not include our data in the improved stopping power data base used for his program TRIM 95.

The last few years brought an interesting task: to produce a chapter on stopping powers for a handbook on *"Atomic and Molecular Data needed in Radiotherapy"*, as part of an international team headed by Mitio Inokuti, for the International Atomic Energy Agency (IAEA). Here it was a special challenge for me to work together with Martin Berger and Hans Bichsel, who of course have been active in this field for a much longer time than I have. And it gave me a chance to see to it that some of our newest data were included in the survey.

In July, **1995**, I had the pleasure of chairing the *Sixteenth International Conference on Atomic Collisions in Solids* in Linz, which started with a talk by the great Jens Lindhard. The chairmanship was a pleasure indeed, for all the work was done by my enthusiastic collaborators, especially D. Semrad, P. Bauer and O. Benka. The friendly preface of the Proceedings (“Helmut Paul, a happy physicist”), written by Mitio Inokuti, is now available in the Internet.

At that time, I was invited to be part of another international team whose was to produce a handbook on stopping powers of heavy ions for the International Commission on Radiation

Units and Measurements (ICRU). This was interesting work, and I liked it.

I have ended my active career as a professor in September, **1996**, and I am now a professor emeritus, i.e., a professor who may do what he likes (at least in principle) but need not do anything. I am happy to have a successor, Peter Zeppenfeld on my chair so that our Abteilung für Atom- und Kernphysik continues to exist (though with the emphasis now on surface physics, and with a different name).

I was fortunate: three times in my life I was asked to help starting something quite new: this was at CERN, then at Seibersdorf, and later at the University of Linz. Also, I am quite happy that I never had to apply for any of my positions: they were all offered to me, and I am grateful for these gifts.

And at the end of my formal career, I have come to think: physics is exciting, but it is the people who are really wonderful, and more important than physics!

Linz, 22 Sep 1998

Eight more years have passed, and I am still alive, still a professor emeritus, still doing physics at age 77, and still enjoying life. Isn't that wonderful?

When I had to hand over my room at the University to my successor, I had to reduce my book collection and my papers, of course. It was evident to me that I could not expect to obtain an "emeritus room" for myself, since none is available. So Peter Bauer, a former collaborator, very kindly suggested that I take over an unused desk in his room, and I did. Maybe he had imagined that I would come to the Institute every one or two weeks, but not that I would be there every day. So I noticed gradually that he would prefer to be alone again. Fortunately, a desk with a PC was available in a room where graduate students work, and I moved in with them. They are all very kind to me and help me with software problems at which they are much better than I, of course. There is a good working atmosphere in this room when several people are quietly thinking at their desks, but we have nice chats too, sometimes.

The work for the ICRU handbook on stopping powers for heavier ions has been continuing in an international team headed by Peter Sigmund of Odense. For this purpose, I collected data on stopping powers, continuing the collection originally started by a student for his diploma thesis. And I started to present these data in graphical form in an internet album ***"Stopping Powers for light Ions"*** which is still growing. This collection has become internationally known over the years, many people have found it useful. So much so that they sometimes do not quote the original authors but only my collection when using the data – a tendency that I do not quite like though it may seem flattering.

I have found a new collaborator in Andreas Schinner, a young theoretical physicist and excellent programmer who works in our group, though without regular employment. Once, when Peter Sigmund was visiting Linz, I introduced Schinner to him, and for many years since then, Schinner has been an essential collaborator of both Sigmund and myself.

I thought that my collection of stopping powers would be useful also to produce an empirical program to predict unknown stopping powers for light ions, and together with Schinner, I produced such a program, called MSTAR, that predicts stopping powers for ions from lithium to argon, in many substances. At the same time, Schinner worked with Peter Sigmund on a theoretical program, called PASS, that predicts stopping powers without using experimental stopping powers as input (like MSTAR does). Since the aim of our ICRU team was to produce a reliable table of stopping powers, I tried to convince my colleagues in the team that MSTAR would be a good basis for that purpose. In one of our team meetings, we even decided, by majority vote, that the new ICRU table would be based on the program that would fit the experimental data best.

Andreas Schinner then wrote a program "Judge" that determines, statistically, how well a particular table agrees with the wealth of experimental data available in my collection. The result was that MSTAR agrees better than PASS – and that is not surprising, since MSTAR is based on experimental data. But eventually, our ICRU Report 73 came out **in 2005** with a table based on PASS – and I published a table in another journal, based on my program MSTAR.

But Judge is a fine program that can be used for comparing the data for various ions and various substances with various published tables, and so I have published several articles with such comparisons, together with Schinner. Unfortunately, my collaboration with

Schinner came to an end in **2006**.

In **2004**, I went to the *Conference on the Use of Accelerators in Research and Industry* (CAARI) in Forth Worth, Texas, a conference whose predecessor I had already attended in **1978**, then at North Texas State University in Denton. It was nice to see former colleagues from Texas, but I also met Oliver Jäkel, a medical physicist from the new center for cancer irradiation by ion beams that is being built in Heidelberg. Later, I visited another medical physicist, Pedro Andreo, at the *International Atomic Energy Agency* in Vienna. And the idea gradually evolved to write a paper together on stopping power ratios (The joint paper came out with Jäkel, but without Andreo as coauthors),

If a beam of ions, say carbon ions, is used to irradiate a malignant tumor in a human being, then the dose must be very accurately known. To measure the dose, one uses an air filled ionisation chamber that determines the dose in air, but the human body consists mostly of water, so one needs the stopping power ratio water/air that can be calculated by an elaborate computer program. But I noticed that almost the same result can be obtained by means of a very simple formula. But I also noticed that the value of the mean ionization used for water in our ICRU Report 73 was quite a bit too low; unfortunately. I published this finding in a letter to the editor in ICRU News which eventually led to an Erratum for ICRU Report 73.

So life is exciting as always, and I am very grateful.

Linz, 12 August 2006.

Seven more years have passed, and I am still enjoying life and enjoying physics. The CAARI conference in Texas happens every two years, and I happen to be invited for a talk every time: in **2012** even for two talks in different sessions! In one of them, I presented arguments that a new Japanese direct measurement of the stopping power of liquid water gives too low values. In the same session, two Spanish scientists made the same claim, based on their theoretical calculations. So we decided to present our results in a common paper that came out in **2013**.

After the CAARI conference in **2012**, Yanwen Zhang, now a professor at the University of

Tennessee, invited me to Tennessee for a few days to give a series of lectures on stopping power to the students of the group. This was remarkable for me: I had not lectured to American students for over fifty years!

I have never managed to write a book. But in **2012**, I was invited to write chapters on stopping power for two different books, one for InTech, the other for a book in the series "Advances in Quantum Chemistry".

I still maintain my Internet Page, **"Stopping Power for Light Ions"** (which is no more restricted to light ions) by adding new data that appear in the literature. Who will carry on this work when I can no more do it?

Linz, 16 August 2013.