Autobiography

Two years after the second world war, I was born as a son of a milliner and an engineer. Migration was the starting point. During the war, my mother migrated with her parents from northern Germany to Austria to get away from the bombs having experienced the destruction of their home. Years later she migrated back to Germany. My father, a young soldier, found himself in captivity in Poland. A year after the war he, however, could escape and migrated in a very complex and frightening tour to Germany. He and my mother moved to Frankfurt, where they got married and where I was born. When I was older, I admired my father for being able –unlike many others- to talk in an honest, self-critical way about the Nazi-regime. My parents' relationship did not go that well, but I was lucky to live in a neighborhood with many other children of my age. We had all kinds of exciting activities but also dangerous ones like playing in the ruins the war had left behind.
Going to school was pleasant to me at that time until I got seriously sick for many weeks just before the planned transition to secondary school. I was sent to a private secondary school as my mother thought this would be easier for me in my special state and my grandfather, an important person to me, was willing to pay for it. Three years later I transitioned to a public secondary school in Offenbach, a city very close to Frankfurt. My mother moved with me and my brother to Offenbach because her relationship with my father had come to an end.

The public school had considerably higher standards than the private one. I experienced how it feels to be the worst scholar in the class room– at least in English and Latin. Finally I got caught up and just barely avoided repeating the class. My excitement about education was mixed, but physics in particular fascinated me. Before my first lesson in physics – I did not understand why – I was nervous like before a date with a wonderful girl. Besides the regulated education I enjoyed the

discussions among the scholars and among friends. Today I believe the voluntary lessons I took in general philosophy and on the philosophy from Schopenhauer were important for my development. Music also started to play an essential role in my life. At the age of fifteen I started to play my brother's violin, which he disliked more and more. I did not receive lessons, but nevertheless played on average about three hours a day. Today I regret that I could not start earlier when I was much younger. After two years I started lessons and had to eliminate all the wrong habits I got accustomed to, and soon joined the school orchestra. In parallel I founded a beat band together with three friends after I found out that with one grip, one could play practically all chords on the guitar.
In school I concentrated on the lessons and tried to actively contribute. At home I did less school work, but met with friends, read books and played the guitar and the violin. Doing sports was also important to me. I enjoyed playing soccer tremendously and also tennis and bicycling. Finally I received the university-entrance diploma in 1966. After this I had to join the army, but I found a way to leave again after ten months. It did not really represent the ideal environment for me. Then I started to study physics in Frankfurt.
Around that time I got married to my first wife Lore, who later got a diploma in psychology. She supported me very much in my work and we could have open discussions about everything. This was also important for my personal development. Several years later we had two lovely children. After twenty seven years, however, we separated.

My first year at the university was somewhat disappointing. I expected more philosophical considerations, like why this approach is successful and another one not and how that fits into a bigger picture. My motivation was very low, but improved considerably when I started with my diploma thesis. Doing science fascinated me more than getting told about it. My experimental work was on the low temperature superconductor $SrTiO_3$, an oxide that surprisingly is a superconductor. I succeeded to prepare tunneling junctions for doing spectroscopy on the superconducting state of the material and cooled them down very close to absolute zero temperature – to about 0.015 Kelvin (=15mK). The instrument for cooling the samples was a dilution refrigerator – a very complex system, difficult and very time consuming to operate. After my diploma I continued to work with this instrument, but soon invented and built the first top-loading dilution refrigerator, where samples could be cooled down to the mK regime within minutes instead of several days. I had no experience at all with protecting intellectual property, IP, and universities in Germany had not much more at that time. I filed a patent, but gave up after two years. Soon companies started to create business with this approach and they are still doing it. This bad experience in IP generation helped me to get motivated to learn how to write a patent, and today I think I can formulate patent claims even in "lawyer language" with reasonable quality. The top-loading dilution refrigerator helped me to create interesting results difficult to achieve otherwise. For doing tunneling spectroscopy on the exotic material $(SN)_x$, one had to prepare a contact (a tunneling contact) between this material and a piece of metal. Nobody succeeded at that time to create stable contacts. I could prepare the samples in seconds, cool them down to minus 190 degrees centigrade extremely fast and to 50 mK in minutes. This way the contact did not have time to deteriorate at room temperature and I could do spectroscopy on the superconducting state of this material. For my thesis I very much valued the advice and guidance from my supervisor, Prof. Eckhardt Hoenig and from the director of the Physics department, Prof. Martienssen.

Directly after my PhD I started at the European IBM research center in Rüschlikon close to Zurich. The person who hired me, Heinrich Rohrer, "Heini", gave me the task to study inhomogeneities on surfaces on very small scales. I studied all existing methods, but could not find one with a high enough spatial resolution – they were not fine and local enough. To fulfill the task we had to invent something. Heini and I submitted a patent and around that time I presented to the employees of the laboratory the plan to build a novel microscope with unprecedented resolution – 3 months after my employment. We called the instrument Scanning Tunneling Microscope, STM. With the help of Christoph Gerber and Edmund Weibel we built this tool, made it work and another year later we achieved the first atomic resolution on the famous silicon surface, the so-called 7x7. We had several "firsts" after this breakthrough like spectroscopy on single atom, which means to "see" the color of individual atoms. There are also ways to go beyond imaging. By changing the parameters involved, one can enhance forces so much that surfaces get modified. The scientific community got very excited about the STM and many scientists started working with the instrument achieving great results. Obviously "seeing" and manipulating atoms triggered the discussion on nanotechnology. Can one build artificial structures bottom up – from the atoms to functional units?

There was, however, one problem with the STM. The sample needed to be electrically conducting. This very much limits the possibilities to create functional units. There are many promising structures that can be adapted from biology like DNA strands that are difficult to image and manipulate by STM. Furthermore, the functional units in microelectronics are usually embedded in an isolating matrix. In nanotechnology you want to have this important option as well. I tortured my brain for about two years with the goal to invent an instrument that has the power of the STM, but does not require electrical currents flowing to the sample. All the attempts failed after some critical considerations – despite the fact that there were many hints in my environment that pointed already into the right direction. We noticed there are forces involved in the STM operation between the front atom of the tip and the atoms on the surface of the sample.

Heinrich Rohrer and Nicolas Garcia also argued this, and John Pethica from the Cambridge University, UK, pointed that out first. Although this was obvious in a way, nobody, including myself, thought about making use of those forces. As I was not capable of solving the problem consciously, my subconscious mind did. One day lying on the couch, I could interpret all kind of objects on the structures of the rough ceiling. Suddenly the solution jumped into my eyes: I clearly could see a tip that was mounted on a spring. Yes, that was it. A tip is in close proximity to a surface and the spring enables one to measure the force between tip and surface. From STM we knew that the forces between front atom of the tip and a single atom on the surface is noticeable. From STM we also knew that the instrument is more stable against vibrations the smaller it is. Therefore the spring should be as small as possible, being soft but not sensitive to noise from the environment.

The couch was located in Cupertino, CA. It was my first year as a visiting professor in Stanford. I worked in the group of Prof. Calvin Quate. He created in his group an unbelievable atmosphere of openness, team spirit and creativity. Christoph Gerber worked mainly in the IBM lab in California. We went together from Switzerland to California and stayed with our wives for a while in one house. I discussed the idea with Calvin Quate and Christoph Gerber. Is this realistic? How should we design the instrument? Can we actually measure forces between single atoms? We believed in the latter and therefore called the instrument, that still needed to be proven achievable, the Atomic Force Microscope (AFM). Christoph built it in the IBM lab and we tested it at Stanford. The spring, a cantilever, was not optimal yet and we did not quite achieve atomic resolution in the first experiments. Still we published the results and this paper became the most quoted experimental paper in the history of Physical Review Letters, the most important journal in physics. Later Calvin prepared tiny springs in his lab, the first micro-machined cantilevers, which enabled atomic resolution, and Tom Albrecht, one of his students, got the first atomic resolution on an insulator. Soon companies formed, which built and sold AFM instruments. It spread fast, as it was not too difficult to operate and the resolution was great and one had several operational modes.

In 1985 I was named an IBM fellow. In 1986, the same year we published the AFM paper, I received the Nobel Prize for Physics together with Heini Rohrer for the invention of the STM, just 5 years after the first breakthrough experiment. Before that I already had gotten several prizes, two of them, the King Faisal Prize and the Hewlett Packard Prize, together with Heini.

Many groups demonstrated atomic resolution by AFM, but I started to get worried. The images with the atomic structures mostly looked very ordered and we knew from STM experiments that surfaces have many defects. Why did we not see them? The answer was simple: the first images with atomic resolution were to some extent fake. In reality, they were an overlay of many atomic resolution images. The tip gets pressed against the surface and tip and sample get very slightly deformed, but enough that several mini-tips get in contact - all imaging in parallel. Defects were averaged out. Seven years later while I was working with a small IBM group at the University of Munich as an honorary professor, we achieved true atomic resolution. A PhD student of mine, Frank Ohnesorge, and I made these measurements even with the sample under water. The front atom of the tip needed to pull on the atoms of the surface to avoid this deformation. This is very different from how a record player or a stylus profilometer works. At that time we also imaged living cells – also under water, in their natural environment while we infected them with viruses. Franz Gießibl, also a PhD student in this group, built the first low temperature AFM and got the first images by AFM at 4K. He later continued this kind of work with excellent results.

My stay at the university came to an end after seven years and I moved back to the IBM lab close to Zurich. Here we started a new project. The cantilevers of the AFM are so small that they are not visible with the naked eye. Therefore we tried to find out whether we can build a chip with thousand cantilevers to possibly create 1000 images in parallel. We mainly wanted to evaluate whether the capability of the AFM for modifying surfaces could be used for high density storage. We made great progress, but underestimated the progress of flash memory devices, where you can get today many gigabytes on a small memory stick. We needed to stop the project after a while.

While I was working at the University of Munich I wrote a book about the mechanisms of creativity. Therein I developed the theory of Fractal Darwinism to describe at a very high level how the world develops in a creative process. For a scientific book it became a bestseller with 50,000 books sold just in Germany. A journalist had read this book and had the idea to transform this theory into software. He looked for funding and in 1995 he succeeded with my help and we started a research organization with the goal to turn it into a company after some years. Shortly after the foundation of the company Definiens he left, but I stayed and took care of the development of the technology. The theory could be applied to many different themes, which we did. A small company, however, needs to focus. In the end we concentrated on image analysis in medicine – viewing the cognition process as a stepwise creative cognitive process. We developed a new computer language, the Cognition Network Language, CNL, and expanded this to data mining. There is very valuable information in medical images and a machine can search for correlations of patterns in the images with clinical outcome. Are there patterns that can predict survival or the response to certain therapies? Particularly in oncology there are more and more treatment options and predicting what is the right treatment for a specific patient is essential. We found out that this type of information is indeed contained in medical images beyond what an expert can discover therein.

In 1995 when we started the precursor of Definiens, Delphi II, I got engaged with Renate and married her in 2003. We enjoy doing all kind of activities together and we live in a house close to Munich, where she loves to take care of the garden. Without her help Definiens would not exist. I was working for IBM in Switzerland when Delphi II got founded in Munich. This was a most difficult time for me. My presence in Munich was very limited and we discussed every evening the problems and the potential solutions for hours on the phone. She was responsible for human resources for many years and was essential in getting Definiens over the most critical barriers. Today she is a professional painter with exhibitions in many different countries.

I worked in many different domains, from superconductivity over surface science, cell biology, storage technology, creativity, informatics related to cognition and now have moved to oncology. I always felt most comfortable when I acted as a layman, where there are still new grounds to be discovered. If you do not know everything, you dare to think differently. You might not even notice this. Thinking differently and having ideas is, however, not enough. Those thoughts need to be challenged in a self-critical and sometimes painful process.