American Mineralogist, Volume 88, pages 938–939, 2003

Acceptance of the Roebling Medal of the Mineralogical Society of America for 2002

WERNER SCHREYER

Professor Emeritus, Institut für Geologie, Mineralogie und Geophysik, Ruhr-Universität, D-44780 Bochum, Germany

Mr. President, members of the Society, and guests:

This is the most important moment in my scientific life. I thank all who were involved in choosing my name for this invaluable honor; and I thank you, Pete, for finding such kind and thoughtful words in your introduction.

Yes, it was a difficult time, which Peter and I—both of 1930 vintage—were born into. But with the humanistic attitude that Peter Wyllie and so many American friends showed, even the defeated former enemies were able to participate in a development of the world which—in its grandeur of progress—has never happened before, and may never happen again. It is my conviction that our generation, at least in Europe, is the most blessed one that ever lived.

The early post-war years in Germany were indeed burdensome. But at least we learned that only hard labor could lead to improvement. As a child in a big city, I was suffering from allergies. The doctors told my mother that I should not spend my life in offices and laboratories but always close to nature: So, I was to become a forester. But after the war, when all the established foresters from the former eastern parts of Germany migrated westward, there was no chance for me. Instead of studying forestry I started geology, with the intention to do much field work. As it turned out, however, I became an experimental petrologist ending up in laboratories and offices after all. And the allergies remained.

The universities of Erlangen and Munich that I attended were slowly recovering from war and Nazi terror, and they were poor. Our education suffered from severe lack of instrumentation. So, I could study these mysterious cordierite-bearing gneisses, which I had collected in the Bavarian Forest for my thesis, only with the petrographic microscope. The general opinion was that all such high-grade metamorphic rocks were of Precambrian age. When I, more intuitively than from hard facts, argued for a younger, Hercynian age of at least some of my gneisses, nobody took me seriously.

An important event in my career occurred just before obtaining my Doctor's degree. My supervisor at Munich, Georg Fischer, asked me to give a seminar reporting on four papers from the Geophysical Laboratory in Washington, D.C., which he had just received. Although I really had to struggle with the text, these articles were an eye-opener to me: This was the way to do research on rocks and minerals! Because of my excitement, Georg Fischer kindly wrote to one of the authors, Felix Chayes, asking about any chances for me at this Laboratory. Three weeks later, I sat trembling in an Amsterdam hotel hall being interviewed by Dr. Abelson, the Director of the Geophysical Laboratory. There was this sudden and harsh question: "Do you work hard?" I could just stammer: "I try to."

So, in 1958, I became the first post-doc from Germany at the Geophysical Laboratory. Dr. Abelson gave me one week to introduce myself to the whole Staff. Then it was up to me to decide with whom I wanted to work. What generosity! I had the good fortune that Hat Yoder was also very interested in the mineral cordierite, and—above all—that he took on the onus of teaching me how to work experimentally, and how to think as a phase petrologist. I was allowed to move into his office where I had the best of tutoring a young greenhorn could wish for.

The research was so fascinating and had so many different facets, that, after four years, people called me Mr. Cordierite. Although I disliked this, it became clear to me that we had set an example—obviously for Hat it was not the first one. We had shown, for an extremely complicated rock-forming mineral, what kind of physical-chemical knowledge is necessary before one can derive any meaningful conclusions concerning the origin of the rock. In all my further work I tried to follow these guidelines.

Back home in Germany, I was again fortunate to be allowed to continue and expand this type of research. But I also had a strange experience concerning my former ideas about the age of metamorphism in the Bavarian Forest. While still at the Geophysical Laboratory, my friends Gordon Davis and George Tilton had been helpful in determining Rb/Sr biotite ages on some of my rocks from this area. To my great satisfaction, they all came out to be Hercynian! So, during the first official geological meeting after my return to Germany in 1962 I reported on these very first absolute mineral ages available for the whole of the Bohemian Massif. The result was that the chairman of the meeting interrupted me, before my alloted time was over, saying that this was now enough of this nonsense, and he ordered me back to my seat. It was slightly comforting for the young frustrated geoscientist that a small revolution took place in the lecture hall afterwards. Moreover, this incident made my name (in whatever sense) known in German geoscientific circles, certainly more than I had anticipated! But I decided from this time on to concentrate fully on my experimental mineralogical-petrological work. Now, everybody accepts that the last metamorphism in the Bavarian Forest is of Hercynian age.

The experimental studies expanded even more after 1966, when I became Professor at the new Ruhr-University at Bochum and received ample financial support for apparatus and personnel. I could also recruit good students and found excellent colleagues, like my first Doctoral student Fritz Seifert, who shared, or even exceeded, my enthusiasm. We became successful and known in the world. One day, at an international conference, I enjoyed listening to a conversation between two of the participants, one asking the other: "Where the hell is this place Bochum?"

An important reason for our success was that—to my great surprise—the experimental efforts in the U.S. had declined considerably. Ironically, this was the second support I received from America. It was the time when thermodynamic calculations alone were considered the most useful tool for understanding mineral assemblages in metamorphic rocks. There was—and is—a lot of truth in this attitude, but by now we know that all this has to be based on the best possible *experimental* data, and I still feel that the final critical test has to come from the experiment.

Starting in the 1970s it was a great pleasure for me to go back to the field as well and select mineralogical and petrological problems worldwide for detailed studies. My sabbatical stays at universities in South Africa and Australia were ideal for gaining the necessary field knowledge and collecting samples. The basic idea was to relate these findings to experimental results, and this worked both ways. We found rocks that duplicated seemingly strange experimental results, and there were these enigmatic findings from nature that guided us to do specific experiments. Let me cite two examples.

Our 1964 experimental results on Mg-cordierite implied that this low-pressure mineral breaks down at high pressures into the assemblage talc + kyanite, but this pair had never been found in nature. Nearly ten years later, my colleague Kulke at Bochum showed me a collection of rocks which he had sampled during his stay in Afghanistan. I could hardly believe it when I picked out a talc schist with spectacular bluish crystals of kyanite. The whiteschists were discovered!

On the other hand, in a sample collected on a Bochum field trip through Yugoslavia, we first discovered the assemblage of talc with potassic white mica, phengite, in a metasedimentary rock. Our experiments designed to study this problem showed clearly that this pair is characteristic for pressures prevailing only below the Continental Moho, where a sediment should never reside. And what a surprise: soon the same pair was found as a widespread constituent in metapelites of the Alps!

Then came the incredible discoveries by our friends of so-called ultrahigh-pressure metamorphic rocks, when Christian Chopin—then working at the Bochum Lab—described coesite and a series of strange minerals from the Dora Maira Massif in the Western Alps, in rocks that had surely formed initially within the continental crust. The absolutely unthinkable became a scientific fact: continental crust of our globe can be subducted at least as deeply as 150 kilometers before returning back to the Earth's surface. Let me emphasize *this* here: without the phase equilibrium work of experimental petrologists and without meticulous studies by those keen rock observers using the "old-fashioned" petrographic microscope, this breakthrough in the geological and geophysical thinking would not have happened. We cannot give up the basic petrologic concept of correlating observation and experiment, but we can now add modelling.

In this exciting world of metamorphism of crustal rocks under mantle pressures, my interest in geochronology was revived, despite previous experiences. George Tilton at Santa Barbara very kindly invited me to spend my last sabbatical in his lab and taught me how to prepare mineral and rock samples from Dora Maira for isotope measurements to be done by him. That the pyrope crystals, previously considered to be products of mantle material exclusively, clearly showed crustal isotope signatures, that the ultradeep subduction must have occurred relatively late in Alpine history, and that exhumation must have been a rather rapid process, all that increased the fascination about the newly discovered terrestrial mechanism even more.

Is it fortuitous that, after last year's recipient, the giant Peter Wyllie, I am the next experimental petrologist to be awarded the Roebling medal? After the man who has conducted experiments on virtually any kind of *igneous* rock, I am really happy, in a very modest way, to represent some part of the *metamorphic* realm, in nature and experiment.

Now, entering mineralogical heaven, I will meet again all the heroes of my youthful days, earlier Roebling medalists like C.E. Tilley, J.Frank Schairer, T.F.W. Barth, Paul Ramdohr, and Fritz Laves. With all of them I had personal contacts and learned from them, from the incredibly keen observer Ramdohr and from Frank Schairer at the Geophysical Lab with whom I could do melting experiments. His Bavarian descent, that was still quite obvious to me, had created a particular attachment to him. In the Lab Frank told me: "Think like a silicate!" And when on one of the annual Lab field trips we were driving through wonderful scenery which we could not identify on our map, Frank made the mysterious remark: "If you don't know where to go, you can't get lost."

This sentence has been haunting me through all these years. It sounds like a terrible way of planning a scientific career. Yet, on second thought: is there not a lot of truth and philosophy in these words? A young scientist cannot foresee what is going to happen to him or her personally, e.g., regarding employment chances, or even what the development of their scientific field will be. They can only register carefully what nature, experiment, and literature tell them, and what they learn from their peers. Then they must ask the right questions, follow logical thinking, and possibly intuition. If they are lucky, they may make the elucidating discovery, without getting lost in the jungle of the imperceptible.

A good deal of this has happened to me over the years and is still happening. I thank all who supported me in my life: my family, my students, my colleagues, and friends at home and abroad. Thank you, Ed Grew for nominating me. Thank you Society, committee, and reviewers, and thank you all for listening.