A Conversation with Dr. Heinrich Rohrer: STM Co-inventor and One of the Founding Fathers of Nanoscience

CONVERSATION

I met with Dr. Heinrich "Heine" Rohrer, the co-inventor of the scanning tunneling microscope (STM) and one of the founders of nanoscience, at the Palace Hotel in Tokyo, where we were both attending a Global Education Summit. We had a chance to talk about the early days of nanoscience, in which we were able to open our eyes and "see" the atomic-scale world, and also to talk about the future.

PSW: What made you decide to move from your earlier work in magnetism and critical phenomena to try to develop the STM?

Heine Rohrer: In critical phenomena, I think I was as good as I could be, as I ever could get. And, I think I was a reasonably recognized scientist. I think at that time I was simply at the top. But, in order to do something better, I would have to learn new techniques and new methods, and I didn't see that this was worthwhile doing.

So, then I thought about what would be interesting, and I already had some contacts in inhomogeneous electric conduction. There was the central problem of the homogeneity of the oxide, and, I mean, that's simply a statistical problem of $1/\sqrt{n}$. And, if you make it small, then it's small, and then, finally, one inhomogeneity dominates the whole thing.

At IBM, nobody was interested, to my knowledge, certainly not in Rüschlikon [Switzerland]. And so I thought that might be a worthwhile new research area, and management was very pleased.

So, they said, "OK, you can't even find somebody to start with you." Then, I started looking around and I found Gert [Binnig, co-inventor of the STM, Nobel Laureate, and co-inventor of the atomic force microscope (AFM)], and I think that was the right thing to do.

PSW: What did you think you would be able to do, and what came as a surprise?

Heine Rohrer: Once we had decided on an instrument like the tunneling microscope, we thought in terms of very sharp tips. The field emission tips were about 10–50 nm at that time. And that would have given a resolution of 2, 3, 4 nm. That was about the same as the best scanning electron microscopes. But with the tunneling you had a direct handle not just on any structural property but on the electronic properties, and that was of interest from the point of view of the homogeneity of the oxide. The homogeneity of the oxide was, in my view, the important thing—homogeneous tunneling through the oxide.

At that time, we had also Karl-Heinz Rieder. He was the surface scientist, and I heard a lecture from him about growth of oxides, an internal one. So, there were many things coming together.

So then, we made the fantastic complicated apparatus for field emission, and Gert had no idea about ultrahigh vacuum and I had no idea about ultrahigh vacuum, but I had an ultrahigh vacuum apparatus of somebody who was in my group and he never did any measurements. We got rid of him, and the equipment was simply standing around. So, we made very elaborate plans—both Gert and I came from superconductivity, originally—we had the crazy idea that we should do spectroscopy at low temperature. We wanted to make an instrument to work at [liquid] helium temperatures [4 K] and ultrahigh vacuum. And that's, of course, a little bit more difficult.

You have many hangups when you start something, and then we thought it would be super if we could do it. Why didn't anybody else do it? You think, the reasons that nobody else did it? Because it must be very difficult. After two years of workshop

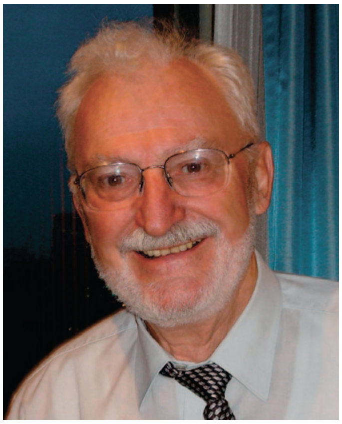

PHOTO COURTESY OF PAUL WEISS.

Dr. Heinrich Rohrer, STM co-inventor and 1986 Nobel Laureate in Physics, in his suite at the Palace Hotel in Tokyo, where we attended the Global Education Summit, hosted by Tohoku University.

To hear Dr. Rohrer's exhortation to young scientists (in *Schweizerdeutsch*), please visit www.acsnano.org.

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construction and then cooling down, then something didn't work, you had to heat it, before cooling down again. And Gert, he sometimes used Scotch tape inside the thing!

Then, he had the crazy idea—he's the most creative guy I know, absolutely, by far. But sometimes he's also too creative. He had the idea to make vibration isolation by superconducting levitation.[1]

We did that. Then, finally, we said to hell with it. We put everything into a small desiccator. And there, it worked. He said, "listen, [there's] no future in this, this careful field emission tip." In principle, any tip, if you don't prepare it very carefully, any tip terminates in an atom. And so, why don't you use this atom? Then you have atomic resolution. That's intrinsic in the tunneling approach.

From the moment where we gave up ultrahigh vacuum, you had to give up the field emission tip; that was clear. The problem is, is the front atom stable or not? But we didn't think too much about that.

And so we did it in the desiccator, and the desiccator has the problem that it doesn't have a good vacuum.

> It took months until we were looking at electronic properties, took months before we realized that if you would scan it, then you can make an image. We had to think about the scanning thing, but this was in our head at the beginning, only to measure here, and measure there, and measure there.

In ultrahigh vacuum, you only have an atom diffusing or one coming down every 50 s, or 20 s, so you might have a little bit of noise. And if you are at ambient [pressure], you have so many [atoms] it doesn't matter. But 10^{-3}–10^{-4} [torr], that's the worse case, that why the signal's very noisy, but it was OK. It's not important. We had one *x*–*y* recorder—they are very slow—that was an intrinsic filter of the data.

It took months until we were looking at electronic properties, took months before we realized that if you would scan it, then you can make an image. We had to think about the scanning thing, but this was in our head at the beginning, only to measure here, and measure there, and measure there. Then, after months we realized it, and then we wrote up the patent disclosure, and the patent attorney asked how many STMs we thought would be sold. So, we said maybe a hundred, and the manager of physics [at Rüschlikon] said *thousands* of them!

Then, after a year, a guy came up from the patent office with the publication of Russell Young.[2] He had this topografiner; he had talked about tunneling and so on. But he talked about increasing the resolution by having a sharper tip. Now, that's not relevant because the resolution depends on the square root of distance and of sharpness. So, we changed the patent application a bit for low temperatures, but I think that was absolutely nonsense because our patent attorneys really were not helpful. And so the patent was on principle, but it really doesn't matter because IBM gives patents away. Everybody can license a patent. They do not sell a patent, and they do not give any exclusive licenses.

PSW: What was the first surface you imaged?

Heine Rohrer: The first thing was simply approaching, looking at the work function. Simply approaching, and then going back again, so you could measure it.[1]

Then, when this worked, that was the first thing we published. We had another guy, Dick Gambino, and he said, "I have a blue crystal that was $CaIrSn_4$ that was very shiny because of the Ir." So we looked, and this had the first nice steps. We could resolve atomic steps, double atomic steps, and so on.[3]

Then, the surface science community always said that that's not real work. You have to have a surface where you know where the steps are, and we said listen, we see steps of the right height and I think these are the steps.

Gert was the first with results. I sent him to the 1982 low-temperature conference in Los Angles, and he gave a post-deadline paper.[4] The chairman came up to him after he talked and said, "Binnig, I congratulate you, this will be a Nobel prize!"

[When we submitted our first paper] the referees said, "we know tunneling goes exponentially and so there is nothing new." They didn't appreciate that we did it completely differently, in a STM configuration. They also said "that's fantastic what these guys did from an instrumental point of view, but it's a matter of [journal] policy whether to publish instrumental achievements", and they didn't publish it, and that was it.

PSW: Do you have a favorite experiment that's been done in the field?

Heine Rohrer: The breakthrough in the scientific community came with the [Si(111)] 7×7. That was the experiment that really got the thing started.[5]

I'm always really happy when I see all these fantastic images. I have nothing to do with them anymore, but you see so many beautiful images of atomic structures. Whether they are now really important or not, it doesn't matter; they are simply beautiful. You see that people can manipulate atoms, that they can do all kinds of things, that they can change molecules, and that they can look at the specific properties of specific molecules. That's good for my heart when I see it.

We could show that you can easily manipulate or position something small in space with an accuracy of 10 pm. I think for me that's really the significance of the STM. When you can do that, you simply have ideas of what you can do.

PSW: You made a comment in your talk at the Summit about the disappearance of disciplines with scale. Can you capture that thought for our readers?

Heine Rohrer: The nanoscale is the bifurcation point where the disciplines develop. That's where materials have their properties and a cluster of 10 atoms does not yet have the same properties as 100 atoms. That is also where the disciplines emerge, and that's why nano has to be completely interdisciplinary.

PSW: If a student came to you and said, "I want to go into nanoscience", what would you tell them?

Heine Rohrer: I would tell them I find this very interesting and if they want to get somewhere they would have to work very hard, there is no way around it.

I think if they really want then to do it, they can make a Ph.D., but after the Ph.D., they have to learn a lot. I think a post-doc should not be allowed to continue in the field where they made their Ph.D. The ones who change their field after they made their Ph.D., these are the really good ones.

Changing fields is very good. I first worked on high magnetic fields and Kondo effect, and then critical phenomena. In each field I was a reasonably established physicist, who went to conferences, shoulder clapping with others, you see. But changing fields, for a while you are a bit lost and lonely—lonely because you are coming into a community nobody knows you and you don't know anybody. It takes personal courage. You cannot be the star from the beginning, but I think what is important is that you might bring in a different way of thinking. You have a certain lightness to approach something that is the expert opinion.

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That's why Nobel work is done at a young age; these young guys are not biased. If older people get the Nobel Prize—take Alex Müller, take me[6]—that's because we changed fields.

PSW: One of the earliest things I learned in meeting you nearly 20 years ago was how encouraging you are to young scientists. What advice would you give to young people wanting to go into nanoscience?

Heine Rohrer: I always give the same message to young people: do what it is you think is interesting. Don't listen to anyone else, not even to your professor, just do what you think is interesting and don't always ask for what could be interesting.

— Paul S. Weiss, Editor-in-Chief

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