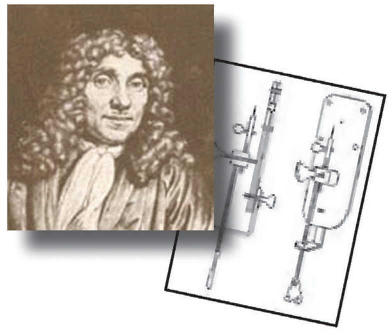
MicroscopyPioneers

The Adventures and Achievements of Ondrej Krivanek

by Laura Wilson and Dara Laczniak, MSA Student Council



Editor's Note: This month, *Microscopy Today*, in collaboration with the Microscopy Society of America Student Council, continues their series of interviews with Microscopy & Microanalysis 2021 plenary speakers and sits down with Dr. Ondrej Krivanek, winner of the 2020 Kavli Prize in Nanoscience, to discuss his incredible work and the paths that led him to findings that have impacted electron microscopy research around the world.

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Anyone working with transmission electron microscopes knows that aberration correction is essential for acquiring clear images; this wouldn't be possible without the work of Dr. Ondrej Krivanek. He has been working in the microscopy field for decades (Figure 1). He pioneered aberration correction, is a leading expert in electron energy loss spectroscopy, and is a highly acclaimed electron microscopist (Figure 2). In 2020, he was awarded the Kavli Prize for Nanoscience—a recognition akin to the Nobel Prize, which honors outstanding scientific achievement. Dr. Krivanek was one of the plenary speakers for this year's Microscopy and Microanalysis (M&M 2021) Conference, and we were lucky enough to get the chance to speak with him. In the following interview, we discuss the trials and triumphs of Dr. Krivanek's career path in electron microscopy. In addition to having some *amazing* stories, he also had some valuable advice for early-career professionals in the field.

You have quite the accomplished research and instrument design repertoire. How did you handle research failures?

Some ideas may seem a little reckless at first, but if your hunch is based on knowledge that suggests *your approach* to the problem is going to work, it's worth pursuing. I have a hard time thinking of outright failures where I went into a project and had to give it up with absolutely nothing to show for it. There's been plenty of learning experiences, though. When you go into a new field, you don't know everything that's important. So, when you build the first version of an instrument, typically what happens is that you realize, "Oh, this is something I should have paid more attention to, and that other thing did not really work out well either." Typically, it's the second instrument that's a *really* good one. But I don't think you would call that a failure. You would just call it a learning experience. There's no way you can actually figure out everything before you start on the project because the literature about the subject has not been written yet—it's brand new. That's basically progress in science, right? At every stage you say, "This was good, but these things need improving," and you move on to the next stage.

Don't rely on popular knowledge. Just because a crowd of scientists has managed to persuade itself that things are a certain way, don't get discouraged. There are two things to consider. One is, is it going to work? This is something you have to decide for yourself. Supposedly there was a famous NSF report that aberration correction was not going to work back in the 1980s. I never read it, but if I had, I would have disagreed. The other one is funding, and that can be a bigger problem. If you want to do something really new and/or something that, in the past, has not worked, agencies may be discouraged from funding you. This is where you have to get inventive. Every project, even if it's really worthwhile, will meet some skepticism in the beginning; if there weren't skepticism, it probably would have been done already. For any really adventurous thing, there will hopefully be some moment of brilliance where you say, "This is the strategy I have to employ to get funded because the conventional routes are not going to work." Good scientists recognize that there are high-risk projects that might not work out but are really worthwhile and should be tried. If you persist long enough, you should be able to have a referee who is in that category, and you should get funded.

What's important is to do really good, fun science and explore new frontiers. Everybody at the start of their careers needs to ask themselves, "Which field is likely to be an interesting one to go into?" You want to know about the

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Figure 1: (left to right) Niklas Dellby (co-founder of Nion), Chris Meyer (an old friend who joined Nion in 2012 and heads the software effort), and Ondrej Krivanek in front of the very first corrector they built at Cambridge UK, circa 1995.

brand-new areas where there's going to be a lot of interesting research to be done, where you young folks can really make a contribution and stake out a career. Start with a [research] hunch, just an expectation, and follow that. If it seems to be turning out well, keep following it!

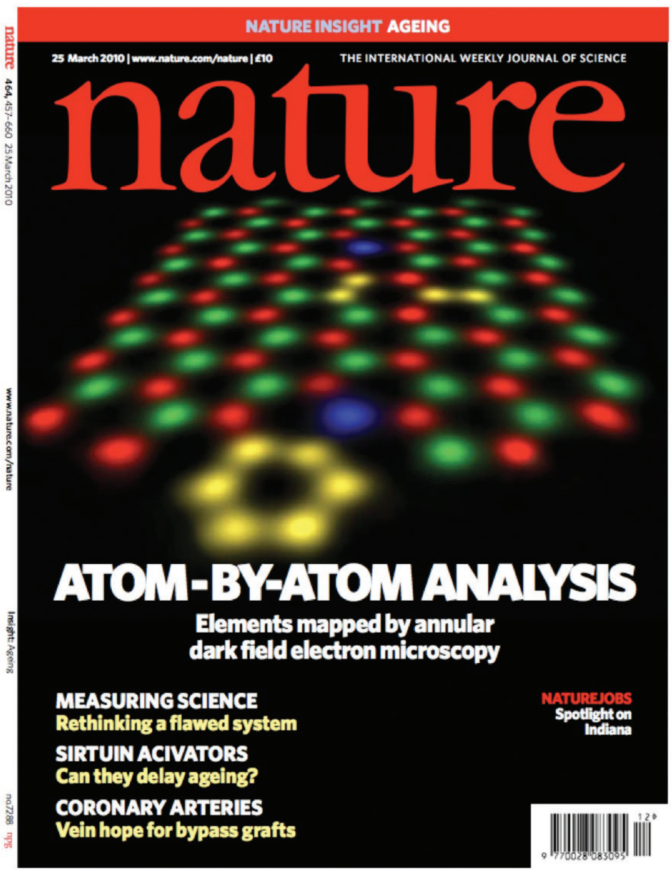

Figure 2: Some of Dr. Krivanek's work, featured on the March 25, 2010 cover of *Nature*.

You made the switch from academia to industry research. How difficult was that switch?

For me, it wasn't that much of a switch. I always found that doing good research wasn't about *where* you were doing it; it was about *what* you were doing. The benefit of working in industry is that finding funding can actually be easier. We had a magical 10 years at Gatan where Peter Swann, the president of the company, was very open-minded. While he would watch the bottom line, I could come to him and say, "This idea should be looked into, and it probably will make money," and he would say "OK, go for it." The benefit of working in academia is that the research payoff—the output—doesn't have to be achieved within the next few years, which is important for riskier projects, like aberration correction. Aberration correction would not have made any sense for me to do while I was in industry, at Gatan; it was too speculative. So, the smart thing to do was to first take a leave of absence and go explore the concept in academia, which was what I did! But then, in academia, it would have been *very* difficult to get funding for round 2 of the project: improving the corrector so it would work better and be easier to operate. If I had written that proposal in academia, it would have been rejected. What's the point? We had already done it and were just trying to improve it. However, in industry, at a small company like Nion, we were able to get the funding to continue corrector development and improve the instrument.

What was it like transitioning from research scientist to entrepreneur and really starting your own company?

I think that every university professor is actually an entrepreneur! He/she/they run a group of postdocs and research students. Funding is needed, projects are needed, facilities are needed, budgets are considered, and proposals are written. You can't do that without a bit of entrepreneurial spirit. So, when I switched to operating a start-up company, basically I ran it as a university research group, and it worked well. However, as an entrepreneur, instead of writing proposals, you write quotes; people come to you saying that they want things. Sometimes they want things that are not working yet, and those become customer-industry joint development projects. An example is our monochromator, which we wrote a theoretical paper about, describing how the monochromator should be designed. Ray Carpenter at Arizona State University read the paper and said, "That's the monochromator I want," but apart from the theoretical design, the thing didn't exist yet. He came to us and said, "I have money for one. Can you make it for me?" And we said, "OK, great. But, you know, it's probably going to take us about three years because we haven't yet started on the detailed design." We went ahead, and it turned out to be the highest-performing monochromator that anybody's ever made.

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What have you found to be a really effective way to communicate the importance of science and research and to inspire young scientists and engineers?

You have to draw on how things are relevant to people's everyday lives. For instance, people just take something like the iPhone completely for granted. If you look at the circuit diagram, it's an incredibly complicated computer. And none of it would be possible if we didn't know how to manufacture semiconductor circuits really well, which requires electron microscopes for visualizing the nanoscale and quality control. So, electron microscopy may look esoteric to the "real world," but Snapchat or TikTok would not be happening without it.

In a couple of the articles about you, it was said that you're a bit of a daredevil and you're always up for a good adventure. How do you find the time to go on these adventures while running Nion and making technology breakthroughs?

I don't know; that came naturally. There's a lot of people in our fields, and in science in general, who do a lot of different things. And the ones who are really good at science will probably be very good at something else too. You can't work 24 hours a day and expect to keep on getting fresh ideas. You need to take a break every now and then; it restores the mind beautifully. So, when you're young, you try to see what kind of a peak you can climb and stuff like that, and when you get a little older, it becomes a bit more sedate, but it's important to keep at it (Figure 3). The Greeks invented this idea; in their academies, you were not supposed to sit around the whole day discussing the works of Plato. You were expected to do something athletic, too. And it does help.

Figure 3: Ondrej Krivanek and Eda Lacar at Jackson Lake in Grand Teton National Park.

With all of the things that you've looked at in the TEM, do you have a favorite or a most exciting thing that you were able to image?

There were several episodes where the results surpassed my expectations, but one of them does stand above the rest. That was the magic evening we had at Oak Ridge, where we were imaging graphene and nanotubes. We were working with a new microscope that we delivered [to Oak Ridge] six months earlier. At the time, graphene was hard to get hold of, but I had secured a good collection of graphene and monolayer boron-nitride samples, and also some nanotubes. We brought them to Oak Ridge, and the images were just *spectacular*. They were so much clearer than anything I had seen before. And it was like, "Wow, this all really came together, the theoretical performance of the microscope has worked out in practice. Here's the living proof." All those years that went into designing the aberration-corrected microscope, from the proof of principle corrector, to the second-generation corrector, to the third-generation corrector with a microscope to go with it, here's the proof that it works.

I'm also very interested in biological things, but because I'm not a biologist, I don't know that much about them. When we ran workshops at Gatan on energy loss spectroscopy, if a biologist came along, I took them under my wing and worked with them, looking at mitochondria, endoplasmic reticulum, and all the other wonderful components of living cells that we would have no idea were out there if we didn't have electron microscopes. They are truly fascinating. So, for somebody from the outside like me, it's a new continent that seems really exciting to explore.

Any parting advice for young professionals?

I don't know if you guys had that experience where you go to a workshop and you come out of it thinking, "This is a field I need to go into," or "This was really fantastic." I had that experience at the 1978 Cornell workshop organized by John Silcox and various other people. It changed the way I viewed what was important in electron microscopy and what needed to be done next.

It's so important to get people together and exchange ideas . . . it moves the field forward. So, do attend conferences and workshops, places where you can trade ideas. You want to listen to more than one person's perspective, but figuring out what research path to take is something you have to decide yourself. So, stay active, go to conferences, talk to people, learn from them, and form your own opinions. And strive for that moment when you realize: "When it comes to this particular field, I probably understand it better than anybody else." That's when you're really making progress. It's hard work, and perspiration is a major component, but luck comes into it as well. When you get to *that* moment, you'll know. Then, 20 or 30 years later, you'll have young people asking *you* to give a summary of your experiences.

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