Opinions Libres
le blog d'Olivier Ezratty

Decode Quantum with young scientists at Lindau

Welcome to the 76th episode of **Decode Quantum**. In our series of three episodes recorded in Lindau where dozens of physics Nobel laureates met with young scientists, we picked a few of them who are specialized in the broad field of quantum computing to head their thoughts about it: Caroline Tornow (ETH Zurich), Francesca Pietracaprina (Algorithmiq), Yaroslav Herasymenko (TU Delft, QuSoft) and Adam Shaw (Caltech).

This podcast was recorded on July 1st, 2024, in Lindau, Germany during the **Lindau Nobel Laureate Meetings 2024** with Fanny Bouton (OVHcloud) and myself. The two other podcasts were recorded and already published with **David Wineland** and **Bill Phillips**.

Caroline Tornow is from ETH Zurich and just started a PhD in the Condensed Matter Theory and Metamaterials group. Beforehand, during her Master's in Quantum Engineering at ETH, she worked in the Quantum Computational Science group at IBM Research Zurich and the Quantum Information Theory group at ETH. She therefore has some ideas on where we are with regards to the maturity of quantum computing in general, and, we would guess, also with superconducting qubits.

Francesca Pietracaprina is a software engineer and researcher at Algorithmiq, a quantum computing software company based in Helsinki Finland. She did her PhD in between statistical and quantum physics and two postdocs in Rome and Toulouse where she focused on localization in quantum systems, a phenomenon that involves a breakdown on thermalization in presence of strong disorder. She then obtained a Marie Curie fellowship with which she moved to Dublin and continued her research on quantum disordered systems and quantum thermodynamics. At Algorithmiq, she is focused on creating full stack solutions for drug discovery and simulation.

Yaroslav Herasymenko is a post-doc at TU Delft and at QuSoft in Amsterdam. He did his PhD at the University of Leiden. He currently works on the development of fermionic (in other words, of electrons) quantum simulations. He started to work in condensed matter theory and then became interested in math and computing methods of condensed matter. It drove him to the field of quantum computing. He is a native from Kyiv, Ukraine.

Adam Shaw has a PhD in physics in 2024 from Caltech, where he studied quantum computing with Rydberg atom arrays. He is part of the team who broke recent records with the control of 6,100 atoms using lasers, working on both experimental and theoretical aspects to these systems, working on gate fidelities, large scale quantum simulation, and showing how certain quantum errors can be erased. He is now a post-doc at Stanford, still working on cold-atoms computing.

(podcast)

What brought you in quantum science?

Adam Shaw: I was always interested in science from a pretty early age. My uncle was a physicist working at Lawrence Berkeley National Lab in the U.S. And then in my undergrad, I worked on condensed matter and sort of material science type problems. When I went into graduate school, I wanted to work on what I thought were cleaner type systems. Systems where we could really isolate down to the very low level, sort of most pristine quantum physics that there was. Because I was always interested in condensed matter physics, interested by materials physics. But I always felt like there was so much sort of emergent complications that it made it difficult to really get down to the underlying quantum physics. And so that's why in graduate school, I started working on these so-called Rydberg atom where essentially we use laser light to trap materials. manipulate, move around an image, single atoms at a time, and then make them interact with each other for purposes of doing quantum computing, quantum metrology, information science in general.

Francesca Pietracaprina: I basically was guided by curiosity. I think I started, like, I knew that I wanted to do something with science even when I was like a child. And then when I started the university, initially, I wanted to do more astrophysics, I think, because I was really passionate about astronomy and that kind of things. And then, while studying at the university, I gradually switched to quantum topics, to actually more, like, statistical physics topics, and then those led me to disordering quantum systems and to quantum systems themselves.

Caroline Tornow: I also always wanted to become an astrophysicist. This changed during my bachelor's studies when I attended the atomic and molecular physics class by Prof. Immanuel Bloch. There we learned in the very last lecture that one can trap single atoms using laser light which I found really fascinating. I then had the opportunity to do my bachelor's thesis in the group of Prof. Immanuel Bloch in one of his labs and after that I became more and more interested in quantum simulation and quantum computing. I moved on to ETH Zurich for the Quantum Engineering master's degree, which was established only one year before I started, so it was quite new and I was interested in how this program is. It turned out to be highly interdisciplinary, with people having a background both in engineering and physics. In this program you also have the opportunity to do an internship in a company. I did my internship at IBM Research in Zurich and after that also my master's thesis. Both of course further increased my interest in quantum computing.

Yaroslav Herasymenko: From an early age, I was interested in many different things but then I figured I could do physics because why not and because I was good at physics at school, and then I was interested in fundamental physics. Somehow for PhD, I moved to condensed matter which is a study of complicated many body quantum systems and then I realized that, like, I'm interested in physics, but equally so I'm interested in the methods of approaching it, like methods of organizing information and solving the problems with computers. And it was very nice that quantum computing is now on the rise, because there are a lot of interesting questions in that domain, and that's how I got into that. So now, I'm mostly interested in sort of, the way I tell it at the parties is that I use mathematics to explain how computers can be used to solve physics problems. So, it's a kind of a tripartite thing, which links back to my multiple interests. Now in this podcast, I guess I can say I'm interested in *quantum* physics problems, not just physics problems.

What was the topic of your thesis?

Yaroslav Herasymenko: it was called **Strategies for braiding and ground state preparation in digital quantum hardware**, 2022, which is a handful. That essentially reflected me changing the subject. The first six papers I wrote in condensed matter, and then mid-PhD I decided that that's a little bit… It's an old field, to be honest. Condensed matter is a 100-year old field. And quantum information is generating a lot of interesting questions. So braiding is something that, like, an old school condensed matter person will understand what it means. And the ground state preparation is something that a quantum algorithms person will understand.

Some of his published work:

Measurement-driven navigation in many-body Hilbert space: Active-decision steering by Yaroslav Herasymenko, Igor Gornyi, and Yuval Gefen, PRX Quantum, June 2023 (21 pages).

Optimizing sparse fermionic Hamiltonians by Yaroslav Herasymenko, Maarten Stroeks, Jonas Helsen, and Barbara Terhal, Quantum, August 2023 (34 pages).

Efficient learning of quantum states prepared with few fermionic non-Gaussian gates by Antonio Anna Mele and Yaroslav Herasymenko, arXiv, February 2024 (38 pages).

Solving Free Fermion Problems on a Quantum Computer by Maarten Stroeks, Daan Lenterman, Barbara Terhal, and Yaroslav Herasymenko, QuTech and QuSoft, arXiv, September 2024 (12 pages).

Caroline Tornow: the topic of my master's thesis was scaling quantum computing using a technique called circuit cutting and knitting. And that is about the problem that quantum computing is facing at the moment, the scalability problem. And circuit cutting and knitting allows you to run larger quantum circuits. on smaller quantum devices. I can give maybe an easy example. If you just have two 5-qubit quantum computers at hand and want to run a 10-qubit quantum circuit, then this is of course at first glance not possible. But using this technique, you can cut really this large quantum circuit and run different fragments, the smaller fragments on the two quantum computers. So this, in general, allows you to compute observables that you would only get if you run the larger quantum circuit. But it also does not come for free. You have to run the circuits more often than if you had a larger quantum computer. And also, it was about error mitigation because the operations that we used for circuit cutting and netting. were quite noisy, and there we also had to develop quantum error mitigation techniques for these operations.

Some work from her:

Directly related to the master's thesis project:

Scaling quantum computing with dynamic circuits by Almudena Carrera Vazquez, Caroline Tornow, Diego Riste, Stefan Woerner, Maika Takita, and Daniel J. Egger, arXiv, February 2024 (16 pages).

Additional work:

Scaling of the quantum approximate optimization algorithm on superconducting qubit based hardware by Johannes Weidenfeller, Lucia C. Valor, Julien Gacon, Caroline Tornow, Luciano Bello, Stefan Woerner, and Daniel J. Egger, Quantum **6**, 870 (2022).

Minimum quantum run-time characterization and calibration via restless measurements with dynamic repetition rates by Caroline Tornow, Naoki Kanazawa, William E. Shanks, and Daniel J. Egger, Phys. Rev. Applied 17, 064061 (2022).

Adam Shaw: I just defended a couple of months ago, my thesis title was **Learning, verifying, and erasing errors on a chaotic and highly entangled programmable quantum simulator**, April 2024, with Manuel Endres. So, a lot of different things went into it, but essentially it was all based on these Rydberg atom arrays that I mentioned a moment ago where we sort of control single atoms, you know, make them interact with each other in order to perform gates and quantum operations. And using this basic quantum platform, we were able to realize a whole host of interesting quantum physics across the range of sort of studying many-body physics, which is sort of a quantum simulation type point of view, doing actually quantum metrology, where we were trying to use a quantum computer to improve our ability to measure things about the world, in our case, improving the ability to perform an atomic clock. And then finally actually doing quantum computing with these systems. And specifically, you know, introducing new methods for eliminating errors in these systems and improving the fidelity of doing quantum operations and achieving sort of state-of-the-art gate fidelities for both Rydberg atoms but also competitive with other more long-standing quantum computing platforms like superconducting qubits and trapped ions. And so in general, it was sort of the broad spectrum of what can you do with a Rydberg atoms or generally what can you do with a quantum computing platform married with the sort of atomic physics ideas that we've known about for 20-30 years at this point.

Francesca Pietracaprina: the title of my thesis is **Investigating Localization Transitions with the Forward Approximation**, 2015. It was basically a topic at the meeting point of statistical physics and condensate matter. The idea was to try to understand this, what at the time was actually underactive discussion, these transitions that happen in systems with a lot of disorder. And in particular, I studied both interacting systems, which were like, especially active at the time, but also non-interacting quantum systems that had this forward approximation that works particularly well in higher and higher dimensions. This is basically at the foundation of what I did then in the later years, because I've always kind of mixed topics from statistical physics, often also from the classical side, and quantum physics.

Other notable work from Francesca:

Forward approximation as a mean-field approximation for the Anderson and many-body localization transitions by Francesca Pietracaprina, Valentina Ros, and Antonello Scardicchio, Physical Review B, February 2016 (17 pages).

Shift-invert diagonalization of large many-body localizing spin chains by Francesca Pietracaprina, Nicolas Macé, David J. Luitz, and Fabien Alet, SciPost Physics, 2018 (27 pages).

I would like to have one last question, normally I ask at the end of this show, but I would like to know… What is this opportunity to be in Lindau today for you? What do you expect of this event?

Francesca: I expect to meet people, to be honest. I see that there are very nice lectures and a lot of side events, but I think the value of these kind of meetings is really to have a direct contact with the people who are doing the talks, but also with the other participants and I've been in other meetings that are in this style, let's say, of course not with 30 plus Nobel Prizes, but I came out of them with a group of people whom I still keep in contact so I think the richness, the advantage of this kind of setting is really to build your own network, but also a group of friends. I already have booked a lunch with J. Michael Kosterlitz and look forward to it.

Adam: I'll sort of add on to it that I think another real benefit of a broad-spectrum conference like Lindau, in addition to meeting a lot of people, specifically those people are from a broad spectrum of different physics. You know, we had talks today from Nobel laureates who are working in biophysics, mathematical physics, areas where I haven't done a whole lot of research, but I still find general interest, and I think that you can oftentimes find new research directions when you bring together multiple different types of physics, different types of science in general. So besides just meeting other young scientists, I'm looking forward to really sort of talking with people from different points of views, different backgrounds, and trying to find some mutually new, exciting ways to do research going forward.

Caroline: I also totally agree with Adam and Francesca. I actually don't have anything to add, just yeah, I mean I also met already people from backgrounds like astrophysics, quantum information theory, quantum computing, both experimentalists and theorists, soft matter, condensed matter, medical physics. It's very fascinating to talk to all these people and normally when you go to conferences you just meet people that work in the same field as you, so that's actually very, very good to be here.

Yaroslav: I'm passionate about physics in general, and about many things, and it's very nice to learn from people here. So in addition to potentially striking up new collaborations or new ideas it's just very nice to actually know… people are in different subjects of physics, they almost live in different worlds, and it's nice to just make contact with these different alien worlds, like somebody has an issue with, you know, chemical dropping on their glove in the lab or something like that, it's just very different ways of living your science life, and I think it enriches your experience because, in particular, quantum computing is itself a very interdisciplinary field. It's good to know how different people live, in this big physics world.

I would like to start the discussion with something else. You started your scientific career as being specialized in at least one or two domains, sometimes you shift from one domain to the other. How do you do to cope with the sheer diversity of technologies of science in that field, even if you stay just in quantum physics or quantum computing, it's so broad, it's very difficult to track, that's my job, my daily job, I imagine when you have a full time job or something, how do you track that? Do you have tricks? or kind of trade-offs you make for your own, I would say, scientific lifestyle?

Yaroslav: my personal choice, which is by no means the only one or advisable one, is to basically understand, well, to have a broad understanding of what's going on to the point where you can start your work without anxiety that you're going to miss something. At this point, when I see a title of a paper, an abstract of a paper, there are different categories. One is like, okay, I'm really not interested personally, it could be good research for some, not for me. Other is, I'm interested, I'm not working on this, but I'll skim the paper because I'm interested to learn, maybe it will be useful later. But it's a minority of papers that are directly relevant to what I'm interested in. So, developing a very sharp taste, I think I'm lucky to have developed a very clear sense of what I like in science. It involves both liking the type of questions and liking the process of studying, the way you get to those answers, which is something you wouldn't know if you don't try. And then also the culture in the community. If you combine all these things, you end up with a very narrow subfield, and then you can only focus on that. So that's my approach, and I know other people have different opinions. I guess also I focus on long-term questions, where I should not be worried to be scooped by a month or by a week because somebody did something that's so much on the surface, but more try to focus, which is also risky of course, try to focus on more big picture questions or deeper, more challenging questions such that if I do it in a year, I'll be fine. It will not be like nobody else will do it in another year or two. And then you're not so worried of keeping track of what's going on every day.

Caroline Tornow: It's very complicated to cope with the amount of research that is published every day, just on the arXiv even, but I also have to say that I always had very excellent supervisors who guided me to important and interesting questions, and that helped a lot to find out what I'm interested in.

I would guess that even in your own field, the superconducting qubits around IBM, it's even difficult to track just that one.

Yeah, that's true, because there are so many applications. Of course, then I was focused more on IBM's technology, right?

But still, you worked with Immanuel Bloch on different fields, a new perspective.

Yeah, that's always nice to have different perspectives.

Interesting, I've seen a lot of scientists who were at least in two domains. I've seen many French scientists who went into Delft. like you, working on Majorana fermions with Leo Kouvenhoven and so on, and now they are in a different field, superconducting or spin qubits or whatever. So you have this kind of, but it's interesting in your case, Caroline, to move from atoms to superconducting. It's a different kind of atom.

Adam Shaw: I'll somewhat echo what Jaroslav said, which is that I think that once you sort of establish yourself in terms of thinking about what sorts of problems are you interested in, you sort of just naturally fall into this position where you delve deeply into the problems and the papers and the talks, etc., which you sort of know are going to be interesting to you. And then it's a bit a matter of taste of just sending out sort of a survey of what else to sort of look at. And so, for instance, I try to be familiar with the sort of top-line results from other, experimental platforms, theoretical results, etc., while then getting into the nitty-gritty details of, for instance… since the Rydberg atoms platform. I think that in general it's just very important to remember that no matter what sort of research, you're doing, you are working in a large field, you're working with a lot of people publishing at once, and you can obviously get swamped and feel like there's sort of too much work going on around you, but as long as you focus on the problems that you're interested in and sort of find like-minded individuals who are working on similar things, it's relatively straightforward to keep track of your own little hemisphere, and then as you become more established, that becomes larger and larger, but your ability to keep all that together also becomes better.

I've got a case study for you. Last December, there was this famous paper from Mikhail Lukin' team and the MIT and others. I mean, there were at least three organizations behind that on top of Harvard. I know people who work in the cold-atom space, it took them a while to understand what was in that paper, because it was so diverse there, but there were many different kinds, of co-occurring codes, different systems. So how did you proceed to learn what was in that paper?

That experiment, the Harvard-MIT experiment, is very similar to my own experiment. And so I didn't find too much trouble reading that paper, because it was quite similar to my own work. You have a quick reading, I would say. So you know which figures are for you to look at, and you know where are the problems and stuff like that? Yes, exactly. I think that for that paper in particular, I don't think I even read the whole paper. I think I just sort of glanced through it and sort of knew what the plot of it was. I had also heard talks about it, though, so there was some amount of understanding already. But I think that's kind of the point, that once you are in the field, what appears to be like a very complicated paper to somebody who's even slightly outside of that field, if it's something that's in your interest and in that regime that you actually care about, it becomes, I think, a lot easier to grasp, even despite the complications.

OK, you have a head start, like we say.

I guess, well, it's a head start. Head Start born from years of slowly inching forward. I wouldn't know if I would call it a Head Start, but it's certainly, there's some amount of knowledge gained over a long time.

Yeah, because how many such papers do we have every year? About, let's say, 12, no? I mean, Fundamental Paper was a big experiment. There was also the Quantum Utility paper from IBM in June last year.

Yeah, I would say on the order of less than 10 papers, where it's really attention grabbing to the entire field. Yeah, around that order. So did you find such a paper,

Francesca: I wanted to comment on one thing that Adam said, that is, in the end, you follow your interest. Because in the end, it is true, we cannot know everything. So at some point, you really need to cut through and to look at the specific topics that you are interested in. even if that means switching from fields to fields and do your best to keep up with them. And I remember one trick that the director of my Ph.D. program taught to everyone, was one of the standard recommendations that he gave to everyone, is to pay attention to talks. Have someone explain the things to you. Don't try to read papers, because reading papers takes a lot of time, and if you have wide interest, at most what you can do is skim through them. But if you can have someone explain the key aspects of a work in like 30 minutes, then go and listen to that talk. And if that fails, even just call them, set a meeting, and have them explain something to you.

When reading papers and then talking to a specialist, I wonder, could it be written in a different manner? So that everybody could understand quickly, is there a kind of style, the rules of the game which is to follow some kind of rules when you write a paper and then it makes it not understandable even by knowledgeable people?

I kind of completely agree. As scientists, we are really bad at writing. We follow some conventions.

Why is that? Do you think some conventions are driving this? I've seen some differences in the past. I've seen a lot of differences in the way scientists, I don't know if it's a culture or country-based thing, I'm not sure about it, maybe linked to the PI rules and the way, the main author is driven by his own team. Any ideas on that?

Yaroslav: I'm not sure about conventions, maybe cultures, yes. I don't think there are any explicit reasons for people to act one way or another. I think there are just accepted ways of expression. And one thing in particular I want to bring up is that there is a little bit of an inflation of hype, let's say, attractive words that you use to describe your own research. Like previously, I think 30 years, 40 years ago, you can find papers that are much more readable. I think also for the sheer fact that people just describe what happens, they're happy to share something that people now would be insecure to share. Like, "OK, this didn't work". But also, there are individual people. There are some people who break the mold. There are some very particular authors, for example, who are working in some domains where people write incomprehensible papers. And they just write straight to the point; they're humorous. And I think people just are afraid to express themselves in ways that feel a little bit informal. But at the same time, everybody wants to read informal papers. I think it's just this disadvantageous kind of equilibrium that we end up with, but it's not even official. People maybe want to make their results sound better, on top of them just being afraid of sounding informal in some very vague meaning of this word. They want to sound formal, they do it, and then it's a downward race to the bottom situation.

When you look at the hype phenomenon, which we could discuss for a long time, there's an interesting phenomenon. There's a scientific paper being published, and then the communication of the research lab, or the company that was behind that, is usually having a title, Researchers found X way to create a scalable quantum computer. When you see the details, it's far from that, but it's so funny that it's a gimmick. It's coming out everywhere. Whatever the technology, whatever the science behind the paper. You observe that?

Yaroslav: definitely. It's often that something that is in the title is essentially something that was already done 50 years ago, and that was the foundational result in whatever is done in the new paper. At least for the common reader, it would read that they discovered that thing that was discovered 50 years ago, but actually they discovered some modification on top of that thing. That would be a typical example. I think it's partially a challenge of science communication, but partially it's also a challenge of staying honest. I think it's a little bit like if everybody is dishonest, it's disadvantageous to be honest. If everybody is honest, it's advantageous to be honest. I find that we just should work. It's a little bit in our own hands as a community to stay honest and do good science.

Adam: I would add on to that. I think from an experimental perspective, it's slightly different just because with the way that these experimental systems are developing, where each of these systems are developing in a different way, even the sort of longest term experimental systems have only been around for something like 20 years, maybe 10 years for others, only like 5, 6 years for the most recent, like these systems that I work on. Most papers are sort of a combination of technology and physics in one. And so there's this mixture of sort of saying that you have produced either some new technology which will push the field forward, while also using it to do some interesting physics problem. And so I think that this creates this potentially tricky situation where you somehow want to make claims both on the physics side and the engineering side, where sometimes it can come off as being hyped in one or the other, when sort of together I think that you're making a valuable contribution. But I think that that can be difficult to distinguish sometimes, like where is something just engineering to push a quantum computer forward, where is it doing engineering, and then looking at some older physics result for the purpose of value.

It's a big overarching question I have in my head. I've been having that for a long time. It's what the difference between science development and technology and engineering. Interestingly, when we had this discussion with David Wineland and Bill Phillips, they believed it was mostly engineering when sometimes other physicists say it's a physics and pure science challenge. How do you view that with your eyes of fresh young scientists?

Adam: I'll take the easy route and just say it's some combination of both of them. To be a little bit more controversial, I guess what I'll say is that I do think that the quantum computing field right now is heavily engineering dominated. The fact that you have companies which are trying to commercialize these things points to the fact that at least some people think that quote unquote science problems are solved and that this is a matter of scalability entirely. and that that's just an engineering challenge. I do push back on that to a certain extent. I think that there is a lot of, certainly you can just engineer and try to scale, but there is a lot of physics that has to go into actually improving this. So for instance, I'll provide a particular example, which is that, and I think that Caroline has a similar experience to this, is that, when you try to build a larger quantum system you have these errors which are occurring and you could just try to make more pure quantum systems, make better control, etc. But one thing that we did in my PhD was for instance introduce a new error mitigation technique, which was not at all sort of known about, not at all thought about, but a new way of interrogating quantum systems. A new way of looking for if errors have occurred in them, which were not previously known error correction methods. And I think that, scientific discovery of how we can interrogate quantum systems then allowed us to improve our control over our quantum computer. And so I think that there is that development in both. And I will say that once you do have a quantum computer, which is even you know, some order of tens of qubits, something on that size or larger, I do think that there are interesting physics problems that can be explored and have been explored across the range of different experimental systems over the last several years.

You mentioned control error correction, it's a very interesting field because when you look at the details, whatever the qubit modality, you have to look at many things, I mean, you've got errors, initialization, the gates, the readout, then you have leakage error, then you can leverage errors, it's very broad as a

scientific domain, and even in that space, of course, when you involve the mathematics behind QEC, it's unbelievably, I would say scientific on top of being an engineering problem. You observe that with cold atoms, I presume.

Adam: one thing that people are sort of discovering right now as they try to build better quantum computing systems to work towards that future dream of error correction is that co-design is incredibly important between the experiment and the theories that go into it. When people were coming up with sort of the baseline theories of quantum error correction in the 90s, late 90s, 2000s, surface codes, etc., they were sort of designing for these idealized quantum systems with certain sets of controls over them. But what we're learning now is that when we co-design an experiment along with its error correction potential, we can find more efficient ways to get to an error correction frontier. We can also find ways in which certain capabilities of an experiment can be exploited. You mentioned erasure errors, which is what I was just mentioning a second ago, but for instance also arbitrary connectivity, which some platforms have access to. Just recently, there's a lot of hype in the field about these new types of so-called LDPC codes, which are an error correction code, which really weren't thought about too much over the past several decades, but then really have gotten a lot of hype in the last year because people have realized that there's ways to realize them in modern quantum systems. I do think that there is a very broad set of error correction challenges that are present.

Your characteristic is you've been working on physics and software. So that **makes you a good person to have two kinds of perspectives on what you can do with those systems**.

Francesca: I kind of understand the hype that is behind quantum computing because, I mean, I work in a company right now, so, like, in this space, of course, the bigger claim that you do, the more funding you receive. At the same time, this is really still a research problem. It's not something that you just need to engineer and make a solution. It's really an interaction between the two, in everything, there's a feedback loop between the research, the experiment, the development of the solutions, and so on. From the software perspective, I would say that right now, especially my work is to help the quantum computer, the hardware, to reach some results by using classical computers, so high-performance computing. And already, working on this topic, you see that there is so much that we can do on the classical side.

You mean using quantum-inspired algorithms, or variational?

You can, of course, define better quantum algorithms, but you can also do a lot more for error mitigation.

So, if you come back to this issue, I would like to ask two questions for you, the three of you are more in the physics side. How do you fight noise? Because it's still a scientific problem. You want to fight noise, whatever it comes from, whatever its impact. We nearly have three lines of fidelities with trapped ions, two and a half nines with cold atoms recently, 99.667 with superconducting qubits, silicon qubits are much lower, I would say, right now, 99. So it's still a physics problem, it's not just engineering, how do you fight that noise?

Caroline: I would say that came up already: quantum error mitigation, that's for the near-term quantum computers. It's only a near-term solution, I would say. Yes, so here you basically use the noise that is there, for example, in zero-noise extrapolation. You control the noise and increase it, and then you are, under certain assumptions, able to extrapolate to a noise-free expectation value. There are also other techniques like probabilistic error cancellation. In addition I would like to mention dynamical decoupling which is one important error suppression technique. Here, using echo sequences of pulses, you are able to refocus unwanted dephasing terms in your Hamiltonian that you would otherwise have and that could destroy your quantum information.

It’s for NISQ systems?

Dynamic decoupling can be also used in FTQC.

Pulse level control is kind of an optimization technique you use when you have ansatzes with the phase, 00:40:09 arbitrary phase rotations that you use in NIST but not in FTQC where you use magic state distillation and T-gates and stuff like that. So, there’s a big difference there.

Yaroslav, have you worked on noise a little bit in your various models?

Yaroslav: The short answer is no, although I did encounter noise in one of my early works on quantum computing where I was trying to make sense of near-term computation. I had more hope about that than I have now. And there, just the sheer sampling noise and just the most basic considerations about fidelity, they hit you in the face pretty quick. Even with the best mitigation techniques, still, noise is essentially just a big limiting factor for anything that you want to do pre-fault tolerance. So, that’s what I realized and that’s the role it’s played in my life. I basically switched my work to working assuming that there will be fault-tolerant quantum computers at some point and then, okay, what can you do with that, yeah.

Adam: I have a lot of thoughts on noise. I think that, in general, I think that error mitigation techniques, as I mentioned before, as Caroline mentioned, are extremely important. But I also think that, you know, we are getting better at learning how to characterize quantum systems, control these quantum systems. You know, during the course of my PhD, we went from a CZ gate fidelity that went from something like 92% to then 99%, 99.5% and now in our lab we have 99.7% fidelity for just like CZ gate in these Rydberg atom systems. And those improvements aren’t just, you know, random, they’re not tricks. So to give like one particular example, you know, one thing that I think that we’re very proud of and where I’m very proud of my own work is sort of having a very concrete error model for how our system works and having basically a digital twin of our system that can replicate all the different sorts of errors that occur in it. And once you have an error model like that you can try to figure out how to actually improve it. So for instance, we know that our gate fidelities of 99.7% are limited by laser phase noise for instance. So how do you improve that? Well, there’s clear ways to improve laser phase noise. You build a method for saying, okay, how do we get from 99.7% to 99.9%? How do you get from 99.9% to 4 nines of fidelity, etc? And I don’t think that these are insurmountable challenges in almost any case.

I can only speak about the Rydberg atom arrays, not other systems, but there are not really fundamental barriers which are going to hit at some point. There are problems of laser power, laser control. As is the case with most things in quantum science, the sort of more funding that you put into solving a problem, it generally will get solved fairly easily if it’s a technical problem. So I think that, to me, I don’t see noise as like an insurmountable thing. I think that most of these problems will be solved in the coming years. And I do think that even in the near term, with sort of noisy systems, you can still learn a lot from them. You can still study interesting science applications which are not so susceptible to noise. And you can also study the creation of large-scale entanglement, creating large-scale entangled states which have practical applications either in simulation and computation, but also, for instance, in metrology, where, by using entanglement, you can improve these systems, improve atomic clocks, improve sensors in general. So my perspective is just that the technical noise sources which we face will get solved, in my opinion. I don’t think that there are technical noise sources which are going to turn out five years from now to be insurmountable.

Let’s come back to the nature of the problem between engineering and science. Let’s come back to solid-state qubits. It looks like there’s a need to better understand the science of materials themselves. I mean, the purity of the elements, the way you manufacture the systems. It’s a mix of science and engineering, again. I visited a while ago, it was in April, I was in Slovenia. I visited a team. Their specialization was

understanding the purity of materials to make sure that the qubits were doing well. And they were doing other stuff, teaming with Google. So, it looks like the scientific part of this research area is maybe underestimated. Because we hear from the vendors and not enough about the science that we have. But all of you, whether you are in Yaroslav, in Qutechand QuSoft, you in the US or Caroline at ETH, you at Algorithmic, you all are working with academics. There's always an academic side and a vendor side. So, Caroline, in your view on this importance of science behind the fidelities of those qubits?

Caroline: it's clearly important, right? I would also agree that it's a combination of engineering and physics. But yeah, it's very important.

Since we're nearing the end of the session, I will ask the most stupid question nobody could ask you, anybody could ask you, and I've been asked every day, is when will we have a functional quantum computer? How do you respond to that question? I've got a hard time because I say we don't know, it's maybe 15 years. Some are optimistic, some are pessimistic. When you ask people, the response fits a Gaussian curve. So what's your views on that? You're young, so you've got all the world ahead of you.

Caroline: I don't know, maybe in five years. I mean, we had a similar discussion at the Quantum Luncheon, I think we all were there. There one had to raise their hands if one thought that we already have a functional quantum computer, and then many people raised their hands, and yes, I mean, it also depends on the definition. We have quantum computers that can compute useful things already, or they are there to learn about a quantum system, learn about the noise. It depends on the definition, yes.

Usually the question is more stupid, but the question is about "the" quantum computer, like if there was only one, so we know there are various breeds, and even paradigms, quantum. 00:46:45 annealing, quantum simulations, gate-based systems, so it's a diverse world. We have to explain that to some extent.

Adam: I'm of the viewpoint that we already have functional quantum computers for some definition of quantum computer. Obviously, we don't have full error correction, but I think that that is, you know, some number of years away that I won't care to guess. But I think that current quantum computers are already achieving, you know, useful scientific results. Maybe not in ways that classical computers could not also solve with, you know, some great effort in some cases. But certainly, quantum computers and the development of them has led us to investigate problems of interest. And there are many times where a quantum computer or a quantum simulator has, you know, shown us an answer, shown us some new way of thinking about some problem. And then afterwards, the classical computation lists can say, OK, we can also solve this and actually push this even further. But there are several cases where this has occurred, where, yes, there are problems where classical computers still might have an advantage over quantum systems. But by building the quantum system, working on it in its NISQ state, we're actually learning more about how quantum systems in general work. And interesting, important, physically relevant problems are being tackled in a way which then classical computers can give more insight into. And as these systems develop more and more, I think that there will be more and more problems where classical computers will struggle, even more to keep up. But I'm very much of the opinion that we already have functional quantum computers. I think that quantum simulation, quantum computation, hybrid approaches, these are all very valuable near-term approaches to really study the world around us in a very different way than I think we ever have been able to do so before the last ten years.

We have to explain the difference between a, it exists and it's got a quantum advantage, which is a different thing.

Yaroslav: quantum advantage is indeed sort of a big ask, and I think it's okay. I think what will happen in the next, let's say, ten years is that about every one or two years something exciting will happen in this domain experimentally which will keep the interest going. So we had the quantum advantage, then we had the error correction result, now we suddenly also have this new cool kid in town, which is the neutral atoms. Every year or every two years something exciting happens, which means that I don't think quantum computing will become like a boring thing anytime soon. But it has to be admitted that in the next maybe five to ten years the progress will be a spectacular feat of engineering and spectacular feat of physics rather than quantum advantage. But this is okay, I mean it feels like right now humanity is willing to invest into this kind of thing just because it's so exciting and it is going somewhere. I mean, you know, if we are there in 15-20 years, it's completely fine. Electrical cars, you know, for example… there are these very long hauls in the history of venture capital that somehow succeeded in something nice. And I think right now humanity has gotten used to the idea, long time investment is actually not so bad.

We are more in the field of fully autonomous cars which is a different field.

Francesca? when I started learning about this field, this was like a PhD and two or three postdocs depending on how you count ago, there was no quantum computer. Then at some point, it was completely like science fiction. And then at some point, ah, there is a quantum computer with 10 qubits, with 5 qubits, with 10, with 50. I mean, it was a development that was very rapid. It's something that has gone so fast that, like, if you say yes, in 10 years, I have no idea what in 10 years we will have. We might as well have one in 10 years because we went from science fiction to reality to a quantum computer that you could connect through the Internet and actually use in a very short amount of time. Even with respect to the imaginations of people. people. And another thing that I wanted to say is that Yaroslav correctly said something about the quantum advantage idea. So you want a quantum computer, you are asking, OK, is it useful? But useful in what sense? Because, yes, we can do something useful with a quantum computer. The point is that we are also extremely good at doing things with classical computers. So like right now, the current status of the technology is that we can do something useful with a quantum computer. We can also do that same thing by spending like one tenth, one, thousandth less money because we can run it on a supercomputer, on a supercomputing cluster instead. The quantum computer is super expensive right now. Yeah, so like you can do basically the same thing using much cheaper resources. I think this is going to change, yeah, I think this is going to change very, very soon, like immediately. Yes, yes.

Switching the cost, the economical value, which is another way to look at this?

Francesca: yes, and also like finding problems that you actually cannot solve, because sometimes you are able to solve these problems, because you can use tricks, you can use, I don't know, you can use the specifics of the system to use approximations, yeah, simplifications, and in the end you are able to actually get results on a classical computer, even if you need to use like a supercomputing cluster. That is not true for all systems, of course, all problems that you can imagine, and there will be some problems, there is probably there, and we just need to find it, and that's good. It's going to be the problem that proves the quantum advantage, but that's us looking for a problem, not…

What's amazing, I would say, when comparing the quantum computing, let's say, revolution, with other digital-based revolution, is the legacy is fighting, to some extent, because you have tensor network solutions, which are even more powerful right now. We've got those NVIDIA and other GPUs and TPUs and NPUs, which are even more powerful. They don't bring any exponential speed up, of course, because they are limited by the way it's organized, but still, it's making a lot of progress, so it's a big challenge. One last question for you guys. It happens that one of our pride with Fanny is many of our listeners decided to go in quantum because of the podcast itself. So what would you say to convince a young student, let's say, a bachelor level or undergrad. level, to get interested in that field? Any idea? What would you say? Even though… People have their own passion and they make their choice, but you are probably passionate about what you do, so how do you convey that message?

Francesca: everyone should follow their passion, because you should follow your heart basically and that will always give you satisfaction, but if I need to convince someone about this field I would say that this is a field that is extremely active, there is a lot of new things. Again, saying what I was saying before, like 10 years ago it was science fiction, now it's reality, so if you want to see something change radically during your work life, this is going to be a very interesting topic to invest in.

That's a very good slogan: "turn science fiction into reality"!

Adam: I would say is that it is important to be passionate about what you. What I would encourage people to think about is that we say this field of quantum science, quantum computing, quantum information science, whatever you want to call it, this is an incredibly broad field where if you're interested in chemistry or mathematics or physics or computer science, even biology, even further fields than that, I think that there are aspects of quantum information science, quantum science in general, which you might find interesting. And I think that what's really nice about this field is because it is so broad, you can sort of find your niche in the community but still be a part of this very active community that's growing so quickly and still really make progress towards these landmark goals that we're all working towards. So that's what I would say is that don't think of quantum science as just some esoteric physics thing that maybe you're not very interested in the physics aspects of it, so you don't want to look into it for those reasons. Really think of it as an interdisciplinary way to build a new way of thinking about the world with other fields, a ton of different overlap with different fields which people might have a lot of interest in, which I think is an important thing to point out.

Caroline: it's a very interdisciplinary field, I really enjoyed working together with chemists, software engineers and physicists during my time at IBM for example, and also not just looking at applications, but also running experiments on a quantum computer and understanding fundamental questions, how open quantum systems behave, for example.

Would you say that to some extent it helps develop new or general problem solving skills?

Caroline: Definitely and it's really highly enjoyable to work in an interdisciplinary field.

Yaroslav: I really want to echo what Adam said about the… that there are all these different subdomains in quantum computing, and that's not just one thing. I really want to reiterate that. So, you might be a student, you might be at the university or at an institute, where you maybe have contact with one or two quantum information related groups. So, what I would really encourage people is that, also based on my personal experience, is if you don't find those particular groups interesting or what they're doing, so interesting, look beyond that. Find out what other things are out there, because it's easy to form impression about quantum computing based on just two or three people you met. Because you will be surprised, you might be surprised how there might be this part of quantum computing arena, which really will appeal to you directly. The reason, the motivation to look for that is because it's pretty obvious there are so many open problems there. If you're scientifically minded, there are so many open problems there compared to some other fields which are much older. So I would really recommend trying to see maybe there are open problems that directly appeal to you, to your mindset, and also depending on what you look forward to in the future, if you want to stay in academia, quantum computing is a good place. If you want to move to industry and you right now want to do a PhD somewhere, quantum computing is a good place, you'll easily move to industry. Basically, it's a pretty versatile place. There are a lot of open questions, and you should learn more about it if you're doubting.

Adam: I've heard from some students, younger students in the past, that they're worried about getting into quantum science because they have a lot of questions about it. I have heard of, it's all over-hyped, like, you know, is this really a field that we want to study because, you know, this is all just hype claims and there's not actually anything behind it. And I would just say that, you know, whether or not, you know, we are able to build a quantum computer in the next decades, whether or not, like, a fault-tolerant quantum system is able to be made, there are existing, there's a huge rich arrangement of existing problems in all of those fields that we've been discussing, which, on the academic side, have been interesting for decades and will be interesting for decades. And so I just don't want people to, you know, think that because this is hyped from an industrial point of view and because companies are trying to build this, there is a huge amount of academic interest, which, you know, maybe that is where you find the most interest for you. And so I just don't want people to think that because it is becoming commercialized, it has this sort of negative aspect about it, the hype aspect, I'll say.

But what you describe is a key phenomenon, which you could mention as being side effects in innovation. Sometimes you have innovation that comes from an enabling technology, or some software, or some combination of tools, which was not planned, even not in the plans of the companies doing it.

Yeah, I think that I heard a good line at this quantum luncheon that we were at a few days ago, which is that hype not only brings money into it, but hype also brings interested people. And so maybe the hype is undeserved behind quantum computing, but it's at least bringing a lot of people into the field who are all very smart, all have a broad range of backgrounds, and whether or not the eventual goals of the field are realized, I think they will be, but whether they're not, I think there's still a huge amount of scientific knowledge that we'll gain along the way, and we'll gain for quite a while to come.

Well, thank you, Francesca, Adam, Caroline, and Yaroslav. It was a very interesting discussion, and it's the last of our three podcasts recorded at Lindau.