00:00:00 Intro

[Mira] And I am Mira. we are associated members of ML4Q

[Chris] and you're listening to ML4Q&A – a show where members from the Matter and Light for Quantum Computing Cluster talk about their careers, their research and the future of quantum.

[Mira] We are excited to have ML4Q professor Mario Berta on the podcast. Chris, ever thought whether you want to work in academia or big tech?

[Chris] I'm a very senior postdoc by now, so I absolutely have. But Mario actually joined RWTH Aachen at the end of 2022 coming from Amazon Web Services in California.

[Mira] Well, maybe ML4Q's academic excellence makes up for the weather and money. I was quite surprised to learn that he almost didn't go for a PhD.

[Chris] We will talk with him about quantum algorithms, benchmarking, and what it takes to bridge the gap between theory and industry. But also about starting a research group, teaching theoretical physics and the saga of the quantum version of Stein's Lemma.

[Mira] So let's jump in and hear from Mario himself about his fascinating path through academia, industry, and back again.

00:01:19 Welcome Mario

[Chris] Mario Berta, welcome to the ML4Q&A podcast.

[Mario] Thank you. Thank you for having me.

[Chris] We wanted to start a bit with your background and obviously you are from Switzerland, which many people will find cute. Is Swiss German your native language?

[Mario] Yes, it is.

[Chris] And would a German person understand you or did you try?

[Mario] Generic German people not I think, but you know, at the ETH there are also lots of Germans, my PhD advisor - German with his first language. So he would adapt and then learn and so it's possible.

[Chris] So we wanted to give people a very quick sample. So could you just say the Feynman quote "nature isn't classical damn it. And if you want to make a simulation of nature, you'd better make it quantum mechanical" in Swiss German.

[Mario] Okay, let me think about it. Okay, so, "Natur isch nöd klassisch, verdamm nomal. Aber si wird simuliert, und das sött mer mit quantenmechanische Methode mache." Is that good enough?

[Chris] Yeah, fairly understandable for a German like me.

[Mario] Yeah, I mean, this is the Zurich accent, so there are different flavors, of course.

00:02:39 Getting into quantum mechanics during the diploma thesis

[Mira] So, yeah, as a student were you already like mathematically oriented? Was mathematics your favorite subject or physics?

[Mario] Yeah, it was kind of mixed. So I should say that my father was a high school physics teacher. So I had a lot of exposure there. And then when it came to university, I was thinking about either math, physics or computer science, basically. But I was never very talented in a lab or these kind of experiments. So you are kind of naturally gravitated towards math, I guess.

[Chris] So people usually say, don't worry about the foundations of quantum mechanics until you're well established. But you did your diploma thesis in the lab of Renato Renna, who's kind of known for also, among other things, this foundation of quantum mechanics. So, did you go into this lab more on the foundational level or more on the mathematical level, what got you into this?

[Mario] Yes. So actually, when I graduated at ETH at the time, Renato wasn't there yet. He was just about to start. He was still in Cambridge as a postdoc. So I emailed him. I knew he would be coming. And then I started my thesis the same day he started there, actually, which was very nice.

Yeah, I guess I was interested in different areas of physics, also at very large scales, like general relativity, cosmology, and things like that.

But the reason I went into quantum is because I thought there's the most happening there. There's the newest science, you know, it's kind of a dynamical area and that spoke to me, so to speak.

I don't think I had a very good idea of “okay, we can do more mathematical stuff, we can do more foundational stuff”. I just heard quantum mechanics one and two and then I went into it.

[Mira] Your master thesis is quite compact and mathematics can be a very compact language. So did you on purpose try to keep it that way.

[Mario] Well, it's very short time only at ETH. I don't remember even like four or six months compared to one year in Germany. So that's one reason why it's shorter. I have to recall now but I think there were some results but the full results were only done very late so there was not a lot of time to write this up.

Generally, if you can say what you want to say in a certain number of words or pages I think one shouldn't stretch it so I would probably do it the same way again.

00:05:25 **Short hesitation before returning to academia for the PhD**

[Chris] And there's a little break between your master thesis and the start of your PhD. Can you tell us about this whole story?

[Mario] Yes. I found it a bit too difficult to do science after my diploma thesis back then. You have to think very hard, right? Like progress is slow, like of course it can be very rewarding.

But yeah, I basically decided not to do a PhD and, you know, went into finance in Switzerland. I didn't like that at all and thought maybe I should stick with science and see where it goes. Put a bit of effort into it and so I got the type of work and kind of working conditions so I didn't look back.

[Mira] And then you did your PhD with Mathias Christander. And whose PhD supervisor was Arthur Eckert. So the acknowledgement of your PhD thesis read like a who's who of mathematical oriented quantum information scientists. How was it to grow into this community?

[Mario] Yeah, it was a lot of fun. I mean, I have to credit Matthias for that because he really introduced me very early on to everyone. There was lots of travel funding available. And I really like the social aspects of this, and I like to travel, meet people. So I would say, part of the reason I'm in science are these interactions.

[Chris] And it's this community, it's really a sub-community of the quantum information community. Would you agree that there's sort of the broad, high-peak quantum information ecosystem, but there's also this core of mathematical physicists

[Mario] Yes. I mean, of course, the whole area grew a lot, like they're still growing every year, like a lot. But I think, I don't know, maybe the 80s and 90s, like the way it started, this is this kind of core quantum information theory, which started from a lot of mathematical principles. I think this quantum information community is what is nowadays this core community.

There's also this Quantum Information Processing (QIP), which is the most important annual event, which is assembled around that community to a certain degree at least. But of course, these days there are lots of other communities, there's all the computational things, there's industry. Now it's actually quite a small and also a bit older community in some sense.

00:08:26 **Quantum Information between Physics and Mathematics**

[Mira] So given all that background, we also were trying to understand what is the border between mathematics, computer science, and physics. Do you consider this very mathematical study of quantum mechanics still physics or is it a branch of mathematics?

[Mario] Yes, of course, there's a lot one could say about that. But my principle is kind of that it's really an interdisciplinary area and I think if you live it that way it's most productive.

So for example when I was postdoc at Caltech there's this Institute for Quantum Information and Matter (IQIM) and this is really between departments, between faculties and maybe if you're a professor it's a bit different but if you're like PhD student or postdocs. You don't even know what department people are coming from. And this is super nice. You can have discussions. It's really like a merging of ideas. A big reason why our area was and still is very successful.

And I think we should do more of that in Germany also. I am trying to do that a bit in Aachen with the Computer Science Department, but there are lots of barriers, let's say, because of the organizational structure of the university. But anyway, we're working on it.

And now having said all of that: I mean, I'm not finding new physics. Maybe I should put it that way. In that sense, it's math. I work with the mathematical framework of quantum mechanics. I show maybe new things in there, but this typically doesn't lead to new physics; which is also sometimes challenging if you're at a grant interview with a physics panel. So, that's a distinction.

And then I think it relates to the question: is this at the math or the physics department? I don't know it depends on the country actually. So yeah, it's somewhere there.

[Mira] You also have a lot of students which have background in computer science, right?

[Mario] Computer science and even like electrical engineering

[Mira] What do you think of this year's Nobel Prize do you think it was justifiably awarded to the category?

[Chris] So we should say it's the neural networks. Is this physics or is this computer science or even mathematics?

[Mario] The thing is with the Nobel Prize, right? Like there's only one in physics. There's none in computer science. There's none in mathematics. And then the Nobel Prize kind of stands out amongst all prizes in science or engineering or whatever, right? So it's kind of like this like black and white type of thing, which maybe it's not the best thing ever.

But still, I think this is perfectly justified, like if you look at the criteria for a Nobel Prize, in my opinion. Maybe some computer scientists are a bit offended. I don't know. But I mean, physics is also really heading in that direction, like if we like it or not, I think.

[Chris] So you work in this strongly mathematically rigorous, but there's very many different tasks in this broad quantum field. I guess it's very different to work on optimizing a concrete algorithm than to try to prove bounds on more broad tasks. I mean, quantum hypothesis testing, which you have worked on, for example, to me seems like a very broad thing where you can prove bounds and then there's, you know, concrete algorithms. Where do you fall on the spectrum and what do you prefer?

[Mario] So interestingly, I think at the moment I'm kind of doing a bit of both in some sense. So originally I was working more on core quantum information theory, a lot of mathematical conceptual equations, and I still do that. My ERC grant is also for that type of topic.

Since I also worked in industry for a couple of years there, I picked up working on quantum algorithms. And I think there you can also make kind of broad statements or propose kind of some meta algorithm and try to argue about how the performance, but it's indeed true that then in the end if you want to really solve problems, concrete problems of interest, you need to dive into much more details and tune different type of things. We do that also in collaboration with other groups or with industry. I think in that sense I'm also involved on that front.

My personal preferences, I still like to do core quantum information theory a little bit better, maybe because it's cleaner. It's kind of more slick or beautiful in my opinion. Because if you have to write an algorithm and really run it end-to-end, there's a lot of not-so-clean stuff. You just have to fine-tune it, you have to massage it.

[Chris] Especially with error correction right? It seems that thinking quantum algorithms and quantum error correction together is also probably important and there's many tricks.

[Mario] Yes, so error correction would even be an additional kind of direction and of course if you're serious about doing stuff on quantum computers you have to ultimately combine all of these things and also kind of usually optimize these things. I agree.

I think, for example, in this Google Quantum AI team, they do a lot of that where they think about both sides. I usually don't think about error correction myself. And the reason being that if you want to optimize your algorithm for your error correction architecture, you need to have some concrete systems, some concrete hardware in mind. And I don't have access to any of that. This is, for example, different from the Google lab, where they have an experimental and a theory team.

So that's why I would talk about algorithms usually independent kind of error correction overhead.

[Chris] So you're more still on the pen and paper side or Mathematica?

[Mario] Yes, yes, yes. I mean, usually what we do is you propose an algorithm and then you try to approve kind of rigorous worst case performance guarantees, which say whatever problem you throw at it, it will solve it with that type of complexity, with that type of runtime, with that type of scaling.

But then, of course, you also want to test it on typical instances of the problem that you are interested in. And maybe you want to make heuristic arguments, why it would perform well. So this is also part of the work. I think you want to do both. Because ultimately, what you actually want to do is to try the algorithm on real computers, real quantum computers, in the same way we do this for classical algorithms. But we can't do that. So we need to approach it from as many sides as possible.

00:15:45 On the potentials and limits of quantum information processing

Mira: So are we getting better at understanding the potentials and the limits of quantum information processing?

[Mario] Yeah, I think we're getting better in the sense that, we know better where the advantages might be and where they're not. I don't know where they're not, if this is getting better.

We have a better understanding, but not necessarily only positive, right? In the sense that, you know, some areas where we might have believed that quantum is good, maybe it's not as good as we thought.

But interestingly, like the original algorithms was kind of in the 90s, maybe. And then for a long time, there wasn't that much progress. But now, maybe let's say in the last 10 years, there was a lot of progress again. And I think it's partly fueled by industry also. So that's very nice to see.

[Chris] You come from this more mathematical side. But do you think people just playing around with quantum computers are going to stumble into things?

[Mario] Well, I think if we would have quantum computers that are better, that actually allow for a decent number of steps of computation and stuff. Then, yes, indeed. I think as for classical theoretical computer science, you know, people propose algorithms, they look at runtimes, and then later people implement them for the problems that test them, they fine-tune them, there's even decades of experience of how you do these things and then in the end, you know, you have products, you have software that people use in the industry, right? So we would want to have the same chain in quantum.

But if you work with nowadays quantum computers, it's sometimes much more difficult, right? Because basically what you're seeing is noise. You can argue about noise tolerance but not really about the core working of the algorithm in my opinion.

00:17:32 Going to Caltech (IQIM) for a postdoc

[Mira] Then so after your PhD, you did postdocs as well or a postdoc. You worked at Caltech with John Preskill and that's very much at the center of the quantum hype. Also close to the Silicon Valley. So how was this experience?

[Mario] Yeah, I mean, it was awesome for me. I was really excited to go to California and LA. I have a good time, more broadly.

But IQIM; I really loved the time there, as I already mentioned, it is this very interdisciplinary place. And other things I really liked about it, I had this complete academic freedom.

I think I was there nearly four years. And there was not a single thing I had to do, not a single meeting I had to attend, like really had to, just lots of fun, do whatever you want, go wherever you want, right? And this is how good new science comes into existence, I think, and should have more places like that also in Europe, I think.

[Chris] I'm asking for the German postdoc community. How much teaching did you have to do?

[Mario] None, none.

[Chris] I rest my case.

[Mario] No, no. But again, four years, not a single thing I had to do, right? This is very interesting. Of course, it's a competitive place, right? You have to get into it. But that was very nice.

The other thing that is great is that basically because it's such a well-known place, all the people that have good results, they basically pass through there, they go there, they present the results, so you have access, you're like in the middle of things.

So maybe the thing that was more difficult at first is that everyone's very smart, right? So you realize where you stand and where you don't stand. So this is something you have to cope with, I guess, but you can take a lot of positives from that, I think.

[Mira] But was it still with the idea that you wanted to stay in academia at that point?

[Mario] No, actually not. My plan was then to go indeed to Silicon Valley and take a job at Google or whatever. Back then, it wasn't so much of a thing that you would work in the quantum industry. I mean, there were some teams, but it wasn't really a viable career option per se.

No, I thought I would... Actually, even my post-doc, I just applied to a couple of schools and I thought, if this works out, if I can go to a good place, I'll continue. Because I think on paper, like being an academic, it's of a very risky kind of adventure and you don't make your life easy in some sense, whereas if you go work for a tech company.

[Chris] I mean, worst case, you earn a lot of money.

[Mario] Yeah, exactly. The answer is no, this was not necessary the plan, but I also didn't exclude it.

[Chris] Did you get an injection of this tech pro optimism there?

[Mario] The tech pro optimism. No, but just kind of let's say the California optimism. It feels very different. And of course, the Institute is very international, but just life generally is a bit more optimistic. I guess Europeans say it's superficial, but you know, now we're just superficially not optimistic. Like, it's the same way, right? Like we're equally superficial, but that's my take on it anyway.

00:21:00 Transitioning to work in industry while being at Imperial College

[Mira] Then you joined Amazon AWS. What was it like working for AWS?

[Mario] Yes. I should say that I went to London also. Imperial College. I mean, maybe that's not so important. But for me personally, it was important because I was faculty there in the computer science department actually.

And basically with what they pay, you can't pay rent. You know, Imperial College is like a really good school, like really good people there. But then I complained, you know, had all the department and they were like “You know, we expect you to also do some consultancy in industry, like that's also good for your research anyway. And we won't pay you more, but you know, we'll approve whatever.”

And that's how I started working in the quantum industry, actually, with Cambridge Quantum Computing back then in London. And that's when I realized that the industry is actually doing very interesting work as well. And there can be lots of fun indeed.

And then after I was three years in London, I was made permanent. And that meant I could also take a sabbatical or just like a year somewhere else. So that's when I said I want to try out industry more. And then I had lots of connections with the Amazon Web Services quantum computing team. And that's how I got into it. It was very difficult because there was COVID also at the time. So it was very non-trivial to arrange things.

[Chris] Just briefly Cambridge Quantum Computing is now Quantinuum. Maybe people today might even not have heard of it anymore because they merged with Honeywell maybe?

[Mario] I think it was Honeywell.

[Chris] But now it's one of the big players

[Mario] Actually I'm trying to work for them again but in Germany, it's a bit more difficult to do part-time consultancy stuff when you're a professor. There are lots of legal things that have to be sorted, but I mean the process, let's say.

00:23:08 Joining Amazon AWS

[Mario] Anyway, so then Amazon, right? Well, for me, it was a bit of a shift of topic, as I briefly mentioned before. There was a couple of months where I had to get accustomed to things. I will say I had much more time for research there versus in academia as a professor. So in that sense, I really enjoyed it.

Of course, it's less personal. Like I had a lot of freedom also to explore things, but in the end, you know, there's some line manager that tells you like, okay, we stop doing this now, we do that. So these are the two biggest differences for me compared to academia.

One is you have more time for research in industry if you have a really like a top notch job as a scientist in the industry. But of course there are political things that can happen and then you know you have to adapt.

[Chris] I mean AWS on the one hand is developing quantum computers in-house. But they're also sort of already one of the main cloud providers for quantum computing. Do you think that, like, do they win either way? Do they have to have their in-house solution win or is it like, will they sort of be okay in any case?

[Mario] I mean Amazon is going to be okay in any case.

[Chris] Yeah, exactly.

[Mario] So even if you spend one billion on this year, even a couple of percentages at most from their research and development budget. So in that sense, it's just kind of like a high risk fun type of project, but it has significant size indeed. And there are, as you say, there are different aspects, right?

There's kind of the actual research team that also builds a quantum computer, the whole cloud management and then there's also a third team which does like customer projects and kind of consulting solutions. So I don't know. I haven't been there now for a for a while. I will say though that this is a very non-typical project for Amazon, right? Because you have other companies like, let's say Microsoft or IBM or also Google to a certain degree. They have this kind of track record of doing kind of basic research and just see where things land. You have this kind of research. Amazon did not have that. They were always like, they're very like product or customer oriented company. So it's a bit of a new thing. So I am not sure where it will lead eventually.

[Chris] But I mean, they even hired people like you who are more on the very theoretical side, I think, right?

[Mario] Yes, yes. I mean, hiring decisions, of course, are also made based on who you know already in practice, right? So maybe they would have liked my profile a bit better if I would have been a bit more applied. But, you know, they knew like I could do good work, like we could work together.

But I think that there's still a difference on how the whole project is set up, without wanting to talk too much about company politics. There's really the idea that we have a product with cloud access to computers, sell these to customers, pay for it. That's one thing.

And then, I mean, also with their in-house quantum computer, the idea is to put that in a cloud and sell access. This is the main motivation, I think, for them.

00:26:33 Will cloud computing be a viable business?

[Chris] One of the things with quantum computing is that there's technically blind cloud computing? Even for classical tasks to some extent, do you think that will be a viable business?

[Mario] Well, okay, so you can do the same actually also classical cloud computing, right? So I'm a cloud provider. I want to provide services where people don't want us to know what are the algorithms that we perform.

And the problem with that is there is just a huge overhead in terms of inefficiency if you want to run computations like that. And so that's why at least in the classical world, this is more of a niche business, let's say. And I'm not sure why they should be different in a quantum setting like long term.

I'm not sure what computations will be so important and people think very shockingly little of all security online, generally, I think.

[Chris] Maybe the people that have such a deep security requirement usually also have the money to buy something and put it on premises.

[Mario] Yes, that is true. I mean, in some sense it's a nice area because it's at the intersection of cryptography and computing. And everything cryptography in the quantum realm is a bit more near term. So that's why this is maybe developed a bit more and talked about a bit more about this.

But long term, I would still like to see an argument why in the quantum regime, it's more important to have private computations versus in the classical regime, but it's at most a niche business.

00:28:28 Possible applications of mathematically driven research

[Mira] What kind of applications does the research that is more theory or mathematically driven have in the industrial setting?

[Mario] Well, yeah, this is a long-term game, right, kind of. I mean, in some sense, information theory kind of started in the modern sense with Shannon in the 40s and 50s. And this was the very first time a mathematical approach to information processing.

And I don't know what people would have answered back then if you would have asked this question. But in some sense we're now working on kind of the same type of a quantum version of what Shannon did for information processing, trying to understand the quantum concepts there and how they're different from classical or not different. So in that sense, I think I'm confident that there's a clear long-term impact.

But otherwise, I think the theory impact is on the one hand for error correction. I think you know people come up with better codes and there are lots of classical coding theorists, even like 4G-5G mobile code and stuff like this kind of expertise is now coming more into the quantum error correction regime. I think there you usually start very mathematical, but then it can be applied quite fast. So error correction, I think, would be one.

And of course, algorithms, right? Like Shor's algorithm, it was like a kind of pure math paper originally. And so there is that pipeline, if you want. But yeah, it's very indirect. And of course, these examples are also kind of very post-selected now, right? There were lots of ideas and lots of them were maybe useful, but not for applications. And now we see a few that are derived.

[Chris] I think both Shor and Shannon might have been on company payrolls while they did these discoveries, right? Certainly, Shor was working for IBM and Shannon was working for Bell Labs, I think. So, I mean, maybe there's a history of deep theory.

[Mario] Yes, yes, that's actually an interesting comment. I mean, at least in the foundations of quantum algorithms you can really see that a lot of people go to industry. So I would expect in the coming years, I don't know if there are any breakthrough results, but a lot of important work, also on error correction tools to certainly come out of companies.

I do worry though a little bit, right? I don't know how this was in the 40s, 50s, 60s or later. There was probably more like job security than now at these tech companies, things can change very quickly, which then of course is difficult because you need long kind of timeline, right? You need to have safety; you need to have consistency to kind of develop these big ideas. But I guess, yeah, we'll see.

[Chris] Yeah, we'll get to your transition back to academia, but maybe just one very quick question that I thought of was: did you meet in your time at Amazon some algorithm specialists from sort of the industry side that work on relevant problems? How many of these people are running around and do they run into like the quantum department at Amazon?

[Mario] So you mean other industries?

[Chris] Like people at Amazon working on sort of algorithm optimization for like more generic classical stuff like, you know, optimization.

[Mario] Yeah, so at Amazon that the theory the algorithms is kind of small. So this was just kind of us if you want. Sometimes we worked with this customer facing team. So we did projects for people that would pay for them. Probably shouldn't give any names. So there were these interactions.

And then, well, in California, there were lots of events where you would meet other teams like Microsoft or Google. And then there would be, actually, there's quite a lot of exchange. So I think overall, it's a very kind of, friendly field still also in industry around algorithms at least because everything so abstract so people are still willing to share if you want. But when it comes to more relevant problems I don't know like I mean like really problems that people care about in industry.

[Chris] Yeah exactly like the people who I don't know optimize Amazon's optimization algorithms like there must be some people there in the basement who just really try to get the most.

[Mario] Yeah, the classical part. I mean we did talk to these people but the gap between, you know, what they're doing and how good quantum computers are, it's just huge. I mean, you can get some guidance, right?

Like, if you look at, some whatever NP hard problem. Like a problem we know it's hard in general, but they need to solve it anyway, right? Because it needs to be solved. So what can you do, right? And then they have very specific instances and methods. And you can take inspiration from that, but it's kind of one way. I don't think quantum can give back a lot because you know devices are just not there yet.

[Chris] Yeah, basically Amazon is probably one of the companies in the world with the most you know logistics and optimization problems that they are solving on a daily basis, right?

[Mario] Yes, yes, but again quantum doesn't help at this time at all like this is a huge gap and so there is this in-house teams, actually I can give you another example.

I also talked a lot to like the cryptography teams at Amazon because I was also doing a bit of cryptography that's a part of my background, the original background partly. And even there, right?

They have very practical problems and they have customers that ask for certain products, they have certain needs and then there's the question, okay, what can quantum do better and does that fit any of these problems or security bottlenecks? Because you know, maybe you have like a chain and then there are some bottlenecks where things are like least safe let’s say and if quantum doesn't fit these bottlenecks we just make something that's already safer.

It's not really useful application-wise. So it's really difficult in my opinion to match because you can't pick what quantum is good at. It's just it's good at certain things. But if that matches what people need? It’s difficult.

00:35:12 Coming back to academia and joining RWTH

[Mira] Then you came back to academia. Was there a reason why you did that?

[Mario] Yes, so let me start by saying that it was a very kind of close call. It was not obvious at all to me what to do. There are some personal reasons, some financial reasons and some I guess scientific reasons.

But okay, so I had this ERC starting grant from the European Union and I could have postponed it for a while, but at some point it was like, you either come back to Europe or you lose this grant and you're not serious as an academic so it's like either I come back now or you I stay.

I mean there were also other grants but it was this kind of situation and so that's why I applied you know because in London I knew that it's going to be difficult long term, just to make a living in UK's complicated place.

And then I applied at a couple of places, and then I got the offer from Aachen and negotiated, and I was kind of happy with the package. So that's kind of maybe on the scientific side.

The other thing was also personal. My long-term partner, she's from Europe. She decided at some point that she's not going to move to the US. Also because of incompatibility of medical degrees and stuff like that. So this was a very personal thing. So it's kind of that played into it as well.

I will say also that financially it was kind of a hard decision because, I mean, the difference in salaries is so huge, right? Like if you work five to 10 years in tech, you earn more money than all your life long as a German academic, right? Let's just stay five more years and I'm good, right? There were thoughts like that.

But in the end, you know, you want a meaningful life, you know, and I much believe in the German academic system.

00:37:14 Diving immediately into teaching

[Chris] Yeah, speaking of the German academic system, you did hit the ground running there, and you immediately went into teaching. If we look into your teaching load, it's not insignificant, right?

[Mario] No, no, I am immediately in full teaching load. Yeah, yeah, yeah. This is, well, I hacked it a little bit in the sense that I arrived like, I don't know, like one week after the term starts, so I could avoid at least teaching the very first time.

But, normally they give you a teaching reduction. This was not foreseen in this case. I do have a degree in physics, but at 20 years I was basically not much in contact with this and then, you know, teaching this kind of basic theory lecture in physics. Yeah, it still is a challenge, let's say.

[Chris] But do you also enjoy it? Does it inspire you occasionally? I guess there's this Feynman thing of like you only know something if you teach it.

[Mario] Yeah, yeah, it's very interesting. I learn or relearn a lot again. That's definitely true. So in that sense, it's also satisfying. Sometimes there's too much. I mean, as a professor, I don't even have working hours. I just have tasks, but there are just too many tasks. And so, yeah, sometimes it's a bit too much.

And then the question is, what does the university want from me? Like, what should I prioritize? So this I find difficult sometimes, you know, research, funding, teaching, this kind of thing.

Then what's also kind of... Probably not unique, but special in Germany is that you can study for free, right? You can study for many years also. And so that means you have a lot of students and it means you will have very good students; best students, I think they're as good as at any of the best schools.

But you also have a lot of, let's maybe call them less motivated students, right? And they're part of the lecture as well. So you need to tailor. And this is something I struggle a bit with sometimes because I feel if you study physics, you should be committed. There are easier degrees to get if you just want to get through it. So I'm still adapting, I guess.

[Chris] One of the things I think, I mean, I also studied in Aachen and I always thought that the theory professors who come in and then they, you know, have these nine blackboards and then this big lecture hall and they, you know, fill all of them twice in a lecture. Like it's a very specific skill to do this blackboard lecture, right? And it's a skill that has nothing to do with the day-to-day work as well, not as much to do with the day-to-day work. How was it for you to get into this? Like, do you have to practice?

[Mario] Yes, I guess every year hopefully gets a bit better. But I mean, it's generally true. If you become a professor, you know, you become a professor because you excel at science in a very kind of narrow direction, maybe you were successful in raising some money. And that's what you get hired for, right?

But then you get tasks. And many of them have nothing to do with what you trained for, what you're, you know, educated in. And I think there's a bit of a challenge here. And I think one should also kind of be more aware of that issue, indeed.

I mean, for example, also if you start as a professor, I have very nice colleagues that help me with things, but like a priori, no one tells you how to do anything. You just go there, like, oh yeah, this is your office but it's kind of a harsh transition, yes, yes. Indeed.

And the teaching specifically, I did teach a lot also when I was at ETH already during my undergrad, basically to make some money on the side to like finance to get my degree. And so this was all old school blackboard kind of style. So I have some experience from there.

But yeah, I mean, I also get rated, right? The students rate my teaching, right? And yeah, there's certainly areas where I can improve.

[Mira] You also take this course on quantum algorithms. You teach the master students, but that is related in some way to your work. Do you also have to take bachelor's courses or things that are completely different?

[Mario] Yes, exactly. So I have this quantum algorithms lecture, which I designed from scratch. This is also fewer students, smaller rooms. I think there's more intimate interactions and stuff. It's easier in some sense. Everyone's interested anyway. They wanted this, they picked this.

Then there's the quantum information kind of lecture. It's also a master lecture, but everyone that goes through this quantum technologies degree has to take it. That's something in between, like a bit more students.

And then there are bachelor lectures. So basically the way it works in Aachen is that all the theoretical physicists share this kind of theory one, two, three, and four, which is just covering all the bachelor stuff.

And yeah, I'm currently teaching this theory IV which is statistical physics. and this is a challenging part. This is the nine big blackboards kind of … even the organization. Like you know you get 15 people like assistance and it's like a lot of a lot of stuff and again It's just like: “oh you do it!”

00:42:40 Comparing working in industry to academic life as a professor

[Mira] But it's naturally more relaxed as people think that as compared to working in industry, academia is a bit more relaxed, but not necessarily.

[Mario] I would say it's 100% the other way around.

[Mira] Oh, OK!!

[Mario] So these two years at Amazon, I mean, I was still working on top of the 100% there, 20% in London, Imperial College. I had a group there and everything, but nevertheless, in industry, you just work on a project, right? You are part of a project, you know, you have kind of working hours at least and just do your stuff.

If you like lead a research group as a professor, I think there's much more like pressure like responsible for everything, right? So I guess it depends on the person and the exact job, but I would say working in industry is much, much less stressful than being, let's say, a competitive professor.

Also, I found that many professors, at least in this kind of theory, are very smart. It's a competition of the best ideas. In tech, they are very smart people. They're also like, let's say more normal people. So the competition is kind of different. So, I found it quite easy and relaxing to, you know, blend in, let's say.

[Chris] I mean, it's true that professor is a very selective thing.

[Mario] There's a lot of selection, right?

[Chris] Your research group, so you said you got this ERC grant and so I guess you got more grants because your research group for a theory group that is sort of starting out started out as a large research group. Like how many people do you have at the moment?

[Mario] Well, it depends a bit how you count, right, like master students for example, but maybe it's around 15 people.

[Chris] Exactly. And I would say like let's say the normal start as a theoretical physics junior professor or something you would start with maybe a PhD student and a postdoc but you are starting with this with this bigger group. Is this like I guess this is a management challenge also not just the scientific one right.

[Mario] Yes, I mean, I was already a professor in London for three plus two years, the two years when I was also working at Amazon. So I have some experience from there and maybe they're like five, six people at most. But so I had some experience in that sense.

But yes, there's a lot of hiring challenges, like how do you get a work visa in Germany and stuff like that, right?

[Mira] Yeah.

[Mario] If this one Chinese guy is from Harbin, which is a kind of military school in China; we got a visa in the end, so it's good. But there are lots of these aspects. And yeah, again, I think I'm just kind of growing into it, maybe.

I agree that in, like, at least theoretical physics people usually have very small groups, but at least in quantum, if you have like a junior professor, they might be happy to hire more people. They just don't necessarily have more funding, right?

In my case, there were a couple of grants, also this excellence cluster here at ML4Q is providing quite a bit of funding and then of course it would be a pity not to use it, even if it's a bit stressful.

00:46:10 Getting introduced to quantum cryptography

Mira: So we can talk about your research interests. One of which is quantum information theory and cryptography. Did you hear about quantum computing first or quantum cryptography?

[Mario] Oh, a good question. As an undergraduate, there was a lecture. I don't know what it was called, but it was taught after this Nilsen and Chuang book, right? It was taught by Atac Immamoglu. That was my first interaction. I think it was a bit of everything.

And then, Atac told me that Renato would be coming to ETH. So I emailed Renato and this was the story. I didn't necessarily pick, oh, I want to do information theory and not computing. It was just kind of like, okay, that's how things went. That's how I got into it.

[Chris] So did you then, I guess then quickly, one also encounters this Shannon canon of information, like classical information theory, because it's very important for cryptography. Did you like to study up on that very quickly or did you have lectures there?

[Mario] Well, I'm still studying up on it, I guess. That's what my electrical engineering students are also very handy for. There were no lectures. At least at ETH it is not really foreseen. I mean, you can take lectures. But I think, so once you start a PhD, you do something like professionally all day long, I think you just catch up on it on your own right like you have all day long so lectures can be very good if you have a good teacher but for me I would usually anyway just like learn stuff a bit independent of the actual lecture you know just read a book or some online course and that's how I would that would catch up on it.

I always found it easier if you start out very mathematical theory and then you want to go more applied like to go that direction. Versus the other so for example, I didn't study math originally right so whenever I go more into math I find it more struggling than going the other way in the more applied direction.

[Chris] Yeah from my perspective interviewing you as an experimental physicist; it's not easy to get into all of this and for example I mean in this information theory stuff this entropy and as a guy with a physics education, I have a rough idea of what entropy is, but there's, you know, there's this more quantum version of entropy. And it doesn't really have that much to do with thermodynamics, right? Do you think a lot about this? Does it all become quite obvious how this fits together when you think about it long enough?

[Mario] I think entropy is a very interesting concept precisely because of what you said, namely that it appears in different scientific disciplines and this is a priori completely disconnected. This is just kind of a certain mathematical function that pops up. And then, oh, it's the same function. So let's also call it entropy.

I think there's also this famous story when the von Neumann entropy in quantum dynamics was discovered, like, or first talked about in the 30s, 1930s. And then Shannon, you know, he came up with theory of information, but then there was the question how you should call this function. And then von Neumann said “call it entropy, because it's the same function”. But very different scientific disciplines, so indeed.

And this makes it very exciting, because then you can ask, is there a deeper meaning to that and yeah maybe there is but I think looking at this kind of interdisciplinary questions I find very exciting indeed. I wouldn't say I understand it all now.

00:50:08 Thoughts on entanglement

[Chris] We have lots of questions, but maybe we can like one of the key things in also quantum communication is entanglement. And you have worked a lot of a lot with this.

Do you think we are still learning new things about entanglement? And is there like a new sort of discoveries of usage in multipartite systems?

[Mario] Yeah, I think we're still learning a lot, at least on a kind of theory perspective. Even this year there has been quite a lot of exciting progress, I find.

If this translates into, you know, applications, let's say for quantum internet or something like that, this is of course a little less clear, let's say.

But I mean short answer is I think there is still a lot of progress partly because it's so kind of a poorly understood in some sense once you have multi-partite systems.

[Chris] And you have this whole debate about post quantum cryptography and I guess some companies are now switching to that. But it's costly, right?

Like how do you think Shor's algorithm will be pushed into the NISQ era and there will be some things happening or is it right? I mean, from my experimental point of view, it looks like at least 10 years down the line or something until somebody does anything meaningful. That's optimistic.

[Mario] Yes, probably. I mean, post quantum cryptography is mostly classical cryptography. It's not quantum safe. I mean, you can use also quantum solutions, but they're actually classical solutions kind of that we're happy with.

And I mean, this is, even in some of the products they're using, this is, I think, in Chrome also, they use it at some point, like, you know, it works. It's fine.

[Chris] We don't need the quantum internet to be safe against Shor's algorithm.

[Mario] That's at least our current understanding of things. So this is, if there is a strong incentive to make this transition; transitions always cost, but I think the technology is basically there if you would want to.

Now, Shor, and short-term, I'm not sure how well this is understood, but I think generally, Shor is quite difficult to implement in a noise-resistant way. There are certain algorithms that are believed to be maybe more noise resistant or noise can even be like be made use of. Shor is not necessarily of that type. So I don't expect like any early applications to be going that direction.

But maybe the last point to say is that of course, if you record all the conversations that are happening nowadays, you can decrypt them later. So that maybe gives you some urgency of changing your system.

[Chris] That milk has already been spilled, let's say.

[Mario] Yeah, but that's going to be very interesting like if you can listen to all the world wide whatever central intelligence conversation.

[Chris] Yeah, I mean it's the story is that NSA somewhere in the desert or somewhere has some data centers where they're basically just sucking up the internet and putting it on hard disks right.

[Mario] Yeah, exactly. And I mean, other countries are doing that as well probably.

[Chris] So yeah, this is, yeah, I don't know. This is happening, certainly.

00:53:43 On algorithms and algorithms benchmarking

[Chris] Let’s switch to algorithms. What’s your favorite quantum algorithm, like softball?

[Mario] My favorite quantum algorithm. Well, they are the ones that are very elegant, let's say, and cool.

But recently, I think a very important development has been made around quantum Gibbs samplers. So you want to basically create thermal states of quantum many-body systems. So not the ground state at zero temperature, but at some non-zero temperature.

And then the question is how you can do that efficiently or more efficiently, like learn stuff about these Gibbs states. And there has been a lot of progress last year and this year on this question. I think this is currently the most exciting avenue to explore further.

Not necessarily because we already know it's very good, it's just, it's promising and not explored enough yet. So that's maybe currently my favorite.

[Mira] Part of your work also involves benchmarking these quantum algorithms. How important are classical simulations for this?

[Mario] Yes, you can benchmark in different ways. You can either run your algorithm actually on a quantum computer. So if you don't do it in a very smart way, you only see noise.

You can run it on a simulator of a quantum computer. And then, of course, classical computations are very important. So these simulations would maybe be mostly for typical use case analysis that you try to simulate. But it’s very restricted in the sense that you can only look at a very small instance sizes.

But then a lot of the benchmarking is more of a resource analysis like you don't actually run the algorithm you just like count how many quantum gates or elementary steps you think you will need and what type of gates.

And then that gives you kind of an estimate, you know, if you want to – with this quantum algorithm – solve a problem that is classically hard, that maybe you can't solve classically, it gives you an estimate of like how many gates you need, like how powerful your hardware needs to be.

And of course they also need classical computers but you don't actually simulate the algorithm; you just kind of try to estimate how many gates you would need to actually run it.

[Chris] So when you work on these things do you work on the fully abstract level or do you think already about gate sets and like compiling the algorithm?

[Mario] There you can go down the chain as far as you want or you don't want, but what I'm familiar with is the quantum circuit model.

So there you count elementary gates, but then of course people make this distinction between Clifford and non-Clifford gates and that's basically how far we would go. So they are the type of like easy quantum gates or clipper gates and the non-clipper gates.

[Chris] This is basically due to error correction like I mean essentially the reason we call Clifford easy is because we think that error correction will be Clifford.

[Mario] Yes, this already makes a lot of assumptions about your system architecture, indeed, but it doesn't make assumptions about the concrete way you realize it, right?

So then you would count these non-Clifford gates, and this is usually the number you use, like, okay, you need 10 to the 6 of those, and this gives you an estimate on how many qubits you need and stuff like that, but this is how far down the chain I would usually go.

This is not necessarily very near-term relevant, I should add. But I think it gives you good pointers like where you should look for quantum advantage, like realistic quantum advantage, like in the long run. And this is the whole reason, you know, billions are poured into this field. And so that's why I think these are useful pointers.

00:57:33 The Quantum Algorithm Zoo

[Chris] Yeah, one of the projects you've been involved with here is this sort of big review article on quantum algorithms. How was it to put together; that was originally this website “the quantum algorithm zoo”, but you guys are now making it more concrete writing more details and you have really a big group of theorists to look into this. How did this come together?

[Mario] Yeah, there are, of course, a lot of things I could say about that. So, it started when I was at Amazon. And basically, it started out of very many negative results, right? Because we're told by management, you have to look at good algorithms for quantum advantage, like in practice for problems of relevance, whatever.

So we look at A, B, C, D, and it would actually maybe invest significant time to understand it much better, but then it would always be like maybe there's some quantum advantage somewhere, but not for what we call end-to-end complexity, right?

So you have a certain problem that you want to solve in industry or whatever. And then in the end, you have a whole chain of computations. And in the end, you want the results, like usually classical thing, like a number. And then you need to count all the computational resources used in between. And the question is, if you replace some of these resources with some type of quantum versions, does it help?

And if you make that kind of analysis, let's say it's very challenging to find good algorithms, good problems for quantum algorithms. And so then we internally started writing these things up, and that's how it started actually and then it got bigger and bigger, and the idea was actually originally to have a Wikipedia-like kind of thing.

And it's different from this quantum algorithm zoo, because the quantum algorithm zoo just focuses on this complex theoretical results for certain subroutines, right?

So you have a certain mathematical problem; you can solve it that well with quantum algorithms. But this is not in this end-to-end fashion, in the sense that down this chain of computations maybe you have some subroutine for which quantum is good, but you know, you don't look at it holistically.

For example, one of quantum algorithms also looks only looks at it in terms of computational cost, but you know, computer is not only the CPU, right? You have like, maybe GPU, then you have quantum version, you have RAM, you have quantum RAM. You have to look at all the resources.

So that's kind of what we're trying to do in this review article. First on the kind of primitives and then really the application side. But yeah, once I left Amazon, I was no longer in charge. I mean, first I was in charge, but then I left and so all the people took over. I was still part of the team, of course. But yeah, it was a mess, as you can imagine, right?

And also the next difficulty is that new results come out all the time. So you have to do like a cutoff at some point. But I think it's a very, very valuable resource and the hope or the goal of this is to guide the broader scientific community, especially in industry also to work on the right type of problem that really promise long-term progress.

[Chris] It's written not only for physicists but also for people who are just more generally interested in algorithms.

[Mario] It's a technical document so it's in that sense for specialists but you have like gazillions of startups that work on algorithms; for people like that maybe don't work on these, there's no long-term promise; like look at these problems these are usually hard problems but if you tell people explicitly this is not leading anywhere look at this instead, I think this can be a good very good service for the community.

01:01:17 Thoughts on scalability

[Mira] You mentioned that new things keep coming up. So this year also has been really great in terms of, hardware development in quantum computing and things like that. So could you comment on complexity versus scalability for today's architecture and what's your prediction for the skeptics?

[Mario] Yes, so I think you recently also had Markus Müller, right, on this podcast? And you should probably ask him about the hardware breakthroughs and the scalability of those.

[Mira] But in terms of applying quantum algorithms to now use this.

[Mario]The devices, actually. Yes, yes. I mean, we barely have one logical qubit, right? And we can't do any computations on it. So in that sense, we can't do any logical computation at all yet, right? But that's not necessarily the message I want to give.

It's kind of hard to say because there are also these competing technologies, right? And then I think certain technologies go to point X, but maybe not further, unless you have an additional scientific breakthrough. I think this is kind of an interesting way of measuring it.

These people that develop hardware, do they believe with their current ideas and technologies, how far can they push it? And how far they think they cannot push it? Because then some other effect pops up and you need to fundamentally do things differently.

And I think right now with these ideas, you can still push considerably, which is very nice. But there's still a huge gap right between actual hardware and what the complexity theories talk about and then the question is also do you want to do a quantum computation that is classically hard that you cannot do on a classical device because then. We're talking for our future. Or are you just happy to do some logical computations like some proof of principles? This might be sooner.

01:03:26 Promising quantum linear algebra type of algorithms

[Chris] So there's a lot of algorithms that are sort of sub-exponential but related to generic linear algebra problems because there's a link between quantum mechanics and linear algebra from doing the review of all the algorithms is there things that stick out to you do you think there's potential there?

[Mario] Yeah, this is kind of an involved question, I guess, because there's a lot of different types of quantum linear algebra type of algorithm.

But I will say that I think it's very challenging to find significant quantum speed up compared to, like, state-of-the-art classical methods. Both in terms of theory and asymptotic complexities as well as in terms of actual finite size relevant instances kind of performance. The thing is just because of the error correction overhead you need large quantum advantages for them to become relevant in any reasonable kind of time frame and this is very challenging. I think there is this kind of wisdom out there in the community that you need at least a super quadratic speed up; this is the threshold in the sense that quadratic is on the one hand relatively generically available. So if you have a quartic speed over something you might already be happy. But even that is very rare and usually if people find something like that then it's also because it's such a contrived problem and instance that you know classical algorithms have not been sufficiently looked at for that problem.

So it's actually quite cool sometimes to find a quantum algorithm; people find a classical one and I mean there's good progress but the pattern I see is that people make good proposals and a lot of smart people look into the problem a lot. Then we learn a lot about it but often the message is actually quantum has a small speed up but not that useful.

[Chris] And sometimes people have even discovered that the sort of quantum speed up can be implemented completely classically as well.

[Mario] That can also happen. I think scientifically – again, this is a great adventure. We learn a lot, right? It's very productive.

[Mira] Yes.

[Chris] It's also one of the reasons why academia is maybe a good place to be in that sense, because if you work in industry, ultimately they want you to find these holy grail applications.

[Mario] Yes, I mean, especially these big tech companies, right? They are known for pouring tens of billions into something and then one day to another they don't feel like it anymore so that can happen. I do feel though that in academia it's taxpayers' money in the end so we need to really justify what we're doing versus in industry it's some shareholders that lost this money then I care less

[Chris] Maybe we can also just build in this quick disclaimer about NP hard problems and so on and traveling salesman like I don't know what you think about this but there's sort of still even though there's very little evidence people are still saying you know quantum computing will solve logistic logistical challenges.

[Mario] Yeah I mean there's the wisdom that probably we can't find super polynomial quantum speed-ups for NP-hard problems. I think people don't think that this is the case so there's no like one-for-all solution.

If your optimization problem has a very specific structure, maybe quantum computers are good. It's just that if you simulate physical systems, then you have certain structure and maybe quantum is amenable to that, but if you just look at kind of general linear algebra problem, general optimization problem, it might have nothing to do with quantum at first. Maybe it's a less good structure for quantum, but you really, you have to work on that.

You cannot just say it's going to be good. You really, you have to tell me why, right? It's not enough to just have this kind of wishful thinking here.

01:07:45 On the mathematics of quantum information

[Mira] So we can talk a little about the mathematics of quantum information. First of all, do you think there is such a thing as an atomic unit of information?

[Mario] Yes. I mean, there's the unit of information which is in classical physics. It's a bit and in quantum it's a qubit. So in that sense, there is such a unit.

But of course, one of the interesting findings of quantum is that this unit depends on the underlying physics. So Shannon's idea was that you can kind of define information independent of its physical representation. But it was implicitly also assuming that it's independent of the physical theory behind it. But now is nowadays knowledge that it's actually not independent of the physics of whatever physics you take you can define this unit of information. I'm not sure what other philosophical implications come with that word.

But yeah, I mean, the interesting part is the quantum unit of information is fundamentally different than the classical one in many aspects.

[Chris] We have quantum mechanics, and it is sort of this generalization of probability theory. But like when quantum mechanics was discovered, the math for it was sort of to a large extent there. Do you think in this whole pushing for mathematical quantum information, we are discovering some of the properties of this math? But sort of the framework, complex numbers, linear algebra, it was all sort of readily available, right? And we're now moving around in it. What is your feeling for this?

[Mario] So back then I think there are objects called von Neumann algebras, right? They were introduced by von Neumann partly because of quantum theory.

So some math, at least in this very general abstract sense, was developed also because of the discovery of quantum mechanics. But then given that we mostly work within the framework, just the mathematical rules of quantum mechanics, right? We explore those, maybe we understand more aspects of it, or more consequences, but we rarely go outside.

But there was a couple of years ago now this interesting result, right? I'm not sure if this is published by now, probably still not, but that is operator algebra theory. That is con embedding, conjecture, whatever is wrong. And this was work out of quantum information, so to speak. So, I think there is kind of a back action also to mathematics again.

And for example, also the mathematical physics community, like the old school mathematical physics community, they are very interested in quantum information. So it's interesting in that sense. And it also goes both ways, I would say.

[Mira] You actually work in a way that's very close to mathematics with lemmas and theorems. What do people who look at quantum mechanics this way see that others might miss?

[Mario] Well, I'm not sure. I mean, usually, there's a whole spectrum of physicists, you know, experimentalists, and more like applied hands-on physicists, and then you get more and more theory and mathematics. And then what people always joke is if you're really a mathematical physicist, everyone already knows how it works.

But then in the very end, you know, the mathematical physics can also prove it, right? So what did you actually learn from that? I mean, it's a good question, you might ask. In certain settings. Like some people try to prove that atoms are stable. If you want to prove that rigorously from first principles, you can only do it for very small atoms and molecules. When you do stuff like that, you can ask what do you actually learn? Of course, you learn mathematical things. But I think when you talk more about these foundational aspects and what is the very core of information, what are the ultimate limits of information processing, you can really gain some insights by fully formalizing things.

And often what also happens is that if it's come up with some idea, some conjecture, they seem certain phenomena for certain systems, but then you want to put this in a whole framework and prove things as general as you can. think you bring in a lot of structure to the situation, to the knowledge.

But it is true that it's more on the rare side that new physics will be discovered because of mathematical prediction. There are examples, but often it goes the other way. I acknowledge that.

01:12:50 The story of revisiting Stein's lemma

[Chris] We are running towards the end of the podcast, but we can just very quickly – because we wanted to give an example – there's this Stein's lemma and there's been a lot of recent debate and you wrote a comment I believe on it. So can we just very quickly tell the story and maybe on the one hand it is mathematical, but on the other hand it's also related to many concrete problems, right?

[Mario] Yes. So there was many years ago, I guess 15 years ago or so, there has been this kind of well-known result on entanglement testing, which is called quantum Stein's lemma.

The reason people are very interested in that, it's around the question if entanglement transformations are reversible. So if you have a certain state, maybe maximum entangled, another one, maybe less entangled, like when can you convert one state to another and back? This is like kind of the general framework and it has this connection to quantum hypothesis testing, to quantum statistics and this goes under this name Stein's lemma. So it's about understanding entanglement.

So, there was this series of papers in Communication Mathematical Physics which is a very prestigious journal in Mathematical Physics. And there were multiple papers in Nature Physics also, which obviously is a high-impact journal.

These papers are very difficult to understand and I think very few people even tried including myself. Actually I spent two years in London with a postdoc trying to understand some of these things and we just couldn't. We couldn't see the proof. This was just out there but people were using these results all the time.

And then at some point there was another group that wrote a follow-up paper on the original works, that used these original results in a certain way. And then we went through this follow-up work. And this just seemed to be wrong. Like the implications seemed to be too strong. And then we went back, basically.

And then one day I got up in California, and my collaborator Marco Tomamichel in Singapore, he had this like counter example to the proof. And then everything kind of fell apart. So the papers were just – the proofs are wrong. And this was kind of a big thing in the community.

Lots of interesting things happen. Also, what do the journals do? What do the editors do of these papers that are peer-reviewed? What happens, right? There were like important people on these papers.

Anyway, then, there were a couple of papers written. We wrote a note also and a comment in Nature Physics about the situation. But then, luckily this year there are two solutions to the original problem that appeared that seemed to be correct. So it's fixed now, if you want.

[Chris] So, the lemma stands, the original proof didn't.

[Mario] The original proof didn't, yes. Exactly. And it's a very also socially interesting situation, right? You can kind of question the whole publication, peer-review system. And because what often happens is actually papers are slightly wrong, but it's always fixable. And then people are always like, okay, I know this is wrong, but you know, we can do it a bit differently.

But in this case, it wasn't. So this is why I had big implications for once. It was a nice communication in the sense that people were open about it, and it ultimately led to good scientific progress. So in that sense it's a happy end, at least for science. But yeah, I think there are lots of questions that one should ask, right?

How were all these papers peer reviewed and no one noticed mistakes? It's kind of a simple mistake, you know.

[Chris] Yeah, I think, I mean, we have this in experimental physics as well, that, you know, there was lots of scandals recently, also in the quantum information field. And I mean, there it was much more serious. It was not mistakes, but even, you know, problematic practices, let's say. Maybe not fraud, but you know, at least problematic practices in the science. And I think the problem is peer review, you know, in the end, somebody gets a paper and maybe has a week to spend on it, right? And if you have a complicated mathematical proof, maybe you would need a month or...

[Mario] Yeah, so this is actually also a problem because the field is so interdisciplinary, right? Because, for example, in math or mathematical physics, it's not uncommon to have 12 months' time to review a paper you get 12 months like a year like this is normal because you need a time. You want to invest that time and now everything is mingled and you have these math papers and people anyway don't review them.

It's challenging of course. It's a bigger question about the whole publication system, but I think it's interesting. And all the original papers are wrong. They're still online. I mean take from that whatever you want.

[Chris] I mean, another problem. Indeed. I mean, the journals are very slow to like, if even if you want to fix your own work, I mean, if you want to ask, ah, can we please, you know, change these, uh, this formula? We made some typo. It takes a while, right? Like these journals are not fast.

[Mario] Some even don't do it. So I have other papers. I know they're wrong. I'm like, okay, I made a mistake. I want to let you retract it. They say no, addendum erratum impossible. Sorry. So this is not something that was thought about properly. I also don't have like immediate solutions, but something we could think about also as a community.

01:18:36 Interested in joining Mario's group?

[Mira] So before we come to the end, I just also want to, since you have joined recently at RWTH as a professor and you're growing your group, do you have any positions you'd like to advertise?

[Mario] We have open positions at any level. I mean, like Bachelor, Master, PhD or postdoc. It has to fit, of course. I think I also have a call on my homepage, but I encourage people to send me their documents and I would get back to them if I think I see a good fit for an interview. That would be great. I am always looking for people. Thank you.

[Chris] So Mario, thanks a lot for being on the podcast.

[Mario] Yeah, thank you guys. That was very nice.