Success is a Rare Event in the Failure of Research — In Conversation with Alessio Figalli

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Figure 1. A. Figalli (m) receiving a Frontiers of Science Award at the ICBS on July 16th 2023. Next to him C. De Lellis (l) and Nanhua Xi (r).

Biography. Alessio Figalli, Fields Medal recipient of 2018, is an Italian mathematician working in the field of partial differential equations. He graduated 2006 from the Scuola Normale Superiore in Pisa under the supervision of Giovanni Alberti and Luigi Ambrosio. Only one year later he obtained his PhD under supervision of Luigi Ambrosio and of Cédric Vilani at the École Normale et Supérieure de Lyon. After another two years in France he left for University of Texas

at Austin in 2009, and became full professor there in 2011. In 2016 he returned to Europe and became professor at the ETH Zürich.
The interview took place at the ICBS in Beijing, during lunch break. Figalli was there as one of the Basic Science Lecturers and Frontiers of Sciences Award winners. We were very fortunate that he could find time for this interview during his only 3 days stay in Beijing. We met directly after lunch and walked a bit though the huge BIMSA campus to get coffee, before the actual interview started. According to him, the top priority when you want to hire Italians is to ensure the flow of proper caffè.

LH: *Welcome, it is a pleasure to have you here in Beijing. We know you have been to so many places during your career: Italy, France, the U.S., and now Switzerland. You have built a very international background. How important do you think is it to have this international experience?*

AF: For me, it was very useful. I learned a lot. Everything I do is going to always be a combination of different traditions.

LH: *What made the most impact?*

AF: Each of them in a different way. When I got the Fields Medal, I would say some of the main topics that can be mentioned in relation to the prize are due to the different places I've been. I mean, of course, the first part of my production was largely influenced by the Italian school, but then there was the French influence, and finally the Austin influence.

LH: *What was it actually like to win the Fields Medal? When did you know? How do you keep the secret?*

AF: It was tough to keep the secret. I knew it at the end of February.

LH: *Did you tell the family? And they had to keep silent.*

AF: For the family, it's easier, right? No one will ask them. It's not like people on the street will stop them and ask about my research and my awards. They don't even know, right? The burden is mostly on you because your colleagues will wonder. Once your name is among the candidates, friends and colleagues will try to figure it out. I also had some friend mathematicians who told me: "If you got it, you have to tell me, because I need to be there. I want to be there when you receive the award, so I need to know."

LH: *How do you do it then?*

AF: Well, they knew that I had to keep the secret, so my answer to them was: "Please come to the Fields Medal award ceremony." That was the same when my advisor Cédric won it.[1] I was young back then — I think it was 2010. For instance, in that case, I was not planning to attend the ceremony. I was young and still early in my career, and I was not applying to go to ICM; I didn't think of going to ICM myself. But there were some friends of Cédric that told him very clearly: "If you got it, I want to be there."

[1] Ndr: Cédric Villani was awarded the Fields Medal in 2010. In that year, Figalli was a young associate professor at Austin.

So Cédric did the same with those who asked him. In fact, myself, Clément Mouhot and François Bolley, the former students of him, told him "If you got it, you need to let us know because we want to organize a conference in your honor."
LH: *I see.*
AF: That's what we did in the January of 2011: we celebrated. He got the Medal in August 2010, but we couldn't wait for the official act to start organizing. There were a lot of rumors, people saying "We think he got it," but we were not sure. So we asked: "Cédric, tell us: did you get it or not? Because if you got it, the same day it's official, we're sending invitations to people." We secured the funding for the conference, we organized, we prepared the schedule, and then we made the invitations quickly after the announcement. We had to do everything in advance.
LH: *Important.*
AF: I mean, it's good that you keep the secret because it's nice, right? It's nice that people don't know officially. But there is no 100% secret. For instance, there are journalists coming to prepare the videos for the Fields Medal ceremony. You know, if you see your colleague with journalists in his office, with a camera, something is going on.

[laughs]

AF: I mean, you're not stupid, right? So people saw me working inside ETH with a camera in the months before. So those colleagues who saw me figured it out, but they didn't say anything. They were very kind.
ZZ: *Now we understand where the rumors came from.*
AF: Exactly. I think there are many ways in which rumors originate, and this is probably the easiest one, right? Because people in the department will realize, but you didn't tell them.

It would be easier, if you just told them, because you would say "I got it, please don't say it". Instead, the moment you just sit in the corridor, someone could say: "I think my colleague got it because I saw something." From there people start to make speculations. This way, the rumors and speculations go around more easily, right?
LH: *For me, one fascinating part about the Fields Medal is that there are always so many speculations.*
AF: I think it's fun because some are always wrong. If it was all sure, then it's not speculation, right?
LH: *Did you start to give out wrong rumors about people? Just to distract?*
AF: No, no, no, not agree with those statements. I think that most people who started rumors that were wrong, they did it always in good faith. People just genuinely think a certain person got the prize. Maybe someone says, "I believe this person may have got it" and then after four more people, the news has become like "It's sure that person is the winner." And then it goes on. That's why I think fake news is so common.

In general, a rumor about a math prize is never too fake. I think that most of the names that come up in the rumors are serious contestants for the medal. I mean, to award the Fields Medal, there is a prize, there is a committee, and they have to make choices. There will be more people who decide who gets the prize. So I think people start to speculate the winner starting from what the short-listed names are. Then, the last moment is when there's a choice to be made, the judges have to agree on a name. There is no absolutely right choice you can make, right? It's a matter of taste, a matter of being lucky at the right moment.

LH: *How did your career change from before the Fields medal to after the Fields medal? Or just your personal life? Maybe that might actually suddenly change quite significantly as well.*

AF: All right, so let's see career-wise. You get more responsibilities, right? On the one hand, with respect to your own community, not much changes in the sense that people who value you and know you are independent of the medal. They know what you do, and they invite you to give seminars and lectures because they want to hear about your work. If I think just people close by and people would like the same topic as me, it won't change too much.

But the big difference is with the all the rest of the community, because people didn't know who you were, suddenly know who you are. This is even more noticeable outside the mathematical community because then you start to become a person who is considered a role model for the kids. For instance, I gave many lectures to high school students, a thing that I never did before because, of course, there was no reason no one would care.

LH: *So, you improved on that, or how did you prepare for the first high school student lecture?*

AF: Yeah, it was a lot of work. The first time, you try to balance, and then with time, you just prepare it; maybe you have some material, and then you realize, after all, that part was not important because they cannot grasp it that much, or maybe you'd spend too much time.

I gave talks even to 11, 12 years old kids. I mean every age, well I never went to very young ones. I don't think at age 6 to 10, what I have to say can make a difference and it's too specific, too advanced in some sense. But when you're early 12, 13, you really got a bit of your passion. Then I think it makes a difference, not really what they say about mathematics, is more what they say as personal experience, right? In reality, any of my colleagues could be the same, it's not that I'm better than them. I'm sure that many of my colleagues would give better talks to high school students.

But then they see you. They ask you questions. They ask, how did you get there? What did you do? What were your challenges? And then you show the human part of yourself, which I think is the most important part to transmit to young people, who remind me of myself before the medal. Well, to be honest, before I got the medal, not anymore because I really knew many Fields Medalists, but if I think of myself as a student, the Fields Medal looked like something unattainable. The people who got it, they looked like they were from another world, not the same planet. And then you start to meet them and then you humanize them, right? The moment you meet someone, you humanize the person, it's no more just a name. It is a person that you meet and talk to. Suddenly things become more... you wouldn't say 'normal', but at least you can relate a bit more.

LH: *And who is the personal hero you always wanted to meet? Like the one who's like this is a giant size and then suddenly becomes human.*

AF: Let's put it this way. When I started mathematics, I didn't know anything. I started very late to be passionate about math. I was lucky because I was at a really nice university [Scuola Normale in Pisa]. In fact, when I really understood mathematics is what I wanted to do, the professors that taught me the most were top mathematicians. First Luigi Ambrosio, my advisor, and then Cédric Villani. But I didn't know who they were. For me, they were just the professors I had in my course at Scuola Normale [Ambrosio], and then when they sent me to Lyon I met Cédric [Villani] in 2005. At the time he was not yet close to the Fields medal. So I met him in a phase where I didn't realize how lucky I was to meet these people, right? But then, as time passed by, there was a name in Italy that always was kind of a sort of legend for us as students, which was De Giorgi.

De Giorgi was a very famous Italian mathematician, who really shaped the old school of mathematics in Pisa, where I was a student. He was a professor at Scuola Normale Superiore, but he died in 1996. People in the mathematics department were talking about him like a legend, so I always wished I had met him. I wished I had the opportunity to see how he would talk about mathematics, and how he thought about problems. Then in 2008... okay, let me say first that there was this other mathematician that I admire a lot because I read some paper, which is Luis Caffarelli in Austin. Anyway, for me, he was a fantastic mathematician, but as many others of whom I read top papers. I remember very well in 2008, I was in Princeton, IAS. I was visiting there for 2 weeks. Almost every day, Enrico Bombieri, who is the other Italian Fields medalist, would come to my office, and stop by because he was very happy that there was an Italian in this program.[2] He would ask me to go with him to have a coffee.

LH: *Did you find a place where you two could have a good coffee? I heard that Italians have a secret nose on where to find coffee.*

AF: In that case, we just went to the cafeteria, one floor upstairs to drink American coffee. But he liked to talk, probably he just was happy to have an Italian to converse with. I think there weren't many Italians at IAS at that moment, probably he was just happy to chat.

So we chatted a lot, and I used that opportunity to ask him about the famous De Giorgi, because the two worked together for many years when Bombieri was in Pisa, before getting the Fields Medal. And then he told me something that I will never forget. He told me: "De Giorgi had a vision, which I would say essentially no one else had. I mean the way De Giorgi had of thinking about mathematics. There is only one other mathematician for me that kind of reminds you of De Giorgi, and this is Luis Caffarelli." At that time I was working in France. I was

[2] Ndr: Anecdotes on E. Bombieri also appeared in the conversation with Sergio Cecotti in the previous ICCM issue.

a professor in France and then UTAustin made me an offer to move and become faculty there. I thought this was my chance I could go there and be a colleague with Luis Caffarelli. In this way I would have the opportunity to talk to him and learn from him. So that's why I moved to Austin in 2009. I decided: let's go to the US, let's pack everything and move to the US. It was a very quick decision.

And then spent 7 years there. Caffarelli and I were essentially office mates, you know, office neighbors, my door was next to his door.

LH: *That's beautiful!*

AF: In this way we saw each other every day for 7 years. This continuous interaction has been important for me, for instance, all the things he has been doing in the last 5,6 years about free boundaries are really the continuation of what he did from the 70s until 2000. So, a very big chunk of his career was devoted to this class of problems. And this now what I'm doing, kind of curiously, I started to do that, mostly, once I left Austin. That's the funny part. So I think in my years in Austin I just got very inspired by him and the people around him, what people were saying, the seminars I would hear. With time I started thinking that all these problems were cool, and I felt I could do something there. Then, once I moved back to Europe and relocated to Zurich, after a year, I started to do so.

LH: *Did you stay in contact with Luis Caffarelli? Because you were probably working on the same kind of problems?*

AF: He wasn't working anymore on that, but of course I always share with him the results once I get them. So I gave talks in the US, and he was there, and he knew I could prove these or prove that, and he was very happy. He wasn't working anymore on that. He was very happy that we could keep improving this line of research. He really revolutionized that area, although there were some points still to be addressed. However these things were very tough, and people thought there was not much more that could be done. It was thought that Caffarelli did virtually everything. Then we recently realized that, in fact, that was not the case. There was much more we could do. In fact, that has been also the content of my talk today.

ZZ: *Actually, in your talk, at the end, there is a Hausdorff n-4 measure zero theorem. I'm just curious because there's also this $n-4$ in the Ricci limit space. Is there any kind of connection between them?*

AF: Not really. We looked into that and that's where we got stuck, our method stops there. The approach Serra and I used in 2019 is to have a one-parameter family of solutions. Then you do the numerology, and you get stuck at 4. Actually, if you do the numerology properly, you will get stuck at 4.5, but there is no dimension of 4.5 — It's either 4 or 5, and we get stuck in between them. That's why. Let's say 4.5 is the borderline now, it is as far as we can get with our approach. It's not clear that there is no way to get around it with a different approach to prove the Schaeffer conjecture.

Anyway, to answer your question: I don't see a connection. It's more of an accident: you have some numbers and some powers, you put things together and get 4.5.

For instance, I have also worked on the Stefan problem, which is the ice melting into water. As the ice melts, there will be times when it breaks, and every time the ice breaks in more pieces, or it creates singularities, it creates holes, whatever. These singularities of the dynamics are what we call *singular points*. Then you ask yourself: how big is the set of singular points? We proved that it has Hausdorff dimension 1/2, which by coincidence is the same as the currently bound known for Navier-Stokes. The difference, however, is that for Navier-Stokes, no one knows if this bound is optimal because there is still a 1 million problem out of it. Maybe there is no singular time at all. In the Stefan problem, instead, we know that the set of singular points exists, and we get Hausdorff dimension 1/2. In our case, we get 1/2 really out of the problem, and we get it in three places. Essentially we split the singular points into three types, we check the Hausdorff dimension separately for each type, and we get 1/2 in all three cases.

There are so many reasons why we get 1/2, you could believe that maybe there is some deep reason beneath. But I understood, especially low numbers are always around, the numbers that you get in this kind of situation are not many. For instance, you know, yesterday I got two awards[3]. There is the other about stable solutions that are smooth up to dimension 9, and that is sharp. We proved that stable solutions are smooth up to dimension 9, and in dimension 10 there is a counter-example. Why is it 9? Just no matter what, that had to be a number and it happens to be 9.

LH: *So, have you ever thought about going into another field?*

AF: Outside of mathematics, you mean? No.

LH: *Actually even in mathematics, like anything else than analysis? Anything else you are interested in?*

AF: Yeah, I played around with other stuff. Well, I think that, in general, what we choose to do is due to a combination of two things. Of course, you are shaped by your early education. In my case, in Pisa, I got a very strong analysis education. But also you are shaped by what you find more close to you, in terms of personal interest but also in terms of feeling. There are some problems you feel, they naturally attract your attention. I wouldn't know how to say better. On the contrary, other problems simply don't match my brain, or the way I think about math. That's how I ended up doing analysis. I'm very, very happy about my choice. I always try to branch out. One instance is the interface of analysis and probability. My papers, which were moved to more dynamical systems, still with analytic ideas, but in dynamical systems. I also have papers where I touched the question more from the perspective of mathematical physics. I even have a paper where I had to study a bit of combinatorics and use it.

However, in all these instances, every time I branched out was because I had someone else, a coauthor who was the expert. And that's a lot, right? because then we could really have complementary expertise that combines. In this way, we were able to do something interesting. In my career, I changed a lot, I went

[3] Figalli got two Frontiers of Science Awards at the ICBS.

back and forth, but I would say I had always three core areas. First, I started with the isoperimetric problems, like isoperimetric inequalities, function inequalities, and so on. Then I added the optimal transport part to it. Finally, I added some nonlinear PDE to it, towards Monge-Ampere, which is non-linear PDE, and then I started to move toward free boundaries. Of course these are not things that I have been doing constantly. Free boundary I started six years ago. I was doing a lot of optimal transport but left for a few years. I didn't do much of optimal transport recently, but I decided to go back because these problems have a lot of fun applications.

Now there is also a lot of interesting optimal transport from the machine learning community. It's very interesting for me to see how that unfolds. Now it has touched so much outside, there are some questions that are still very close to my interest because they're applied, but very mathematical. So I think I am changing for me, but okay, it's not that I jump to algebraic geometry or something, you know, on the other side.

LH: *Then, on the other side, what fascinates you about estimates and regularity theory?*

AF: I think it's for the mathematical beauty in them, right? You see how important they are, right? The more you learn, the more you understand them, and the more you give meaning to stuff, right? Very often, there is. I think sometimes we believe we don't like some stuff, just because we didn't put enough time into it.

I also think, as I've said, that I am biased. I studied a lot of analysis as a student and suddenly analysis looks cool to me. Then I never spent enough time on other areas of mathematics to decide that they were not my taste. But once you get an expert in something, you tend to find the problems there. You know, analysis is so rich. There are really so many things to do. It's like how much time and effort I should even to try to do something else. It is not easy, and it's not clear, right? Because then what? Algebraic geometry, symplectic geometry, mirror symmetry, name one. Then there is group theory and low dimensional topology, there are so many things in mathematics. It's very rare that someone suddenly decides to change to another topic. Why one over the other?

ZZ: *You mentioned some potential new applications of optimal transport. There is actually one question I want to ask mainly because of my personal interest. You know how optimal transport is, giving us some new insight into the lower bound on Ricci curvature. I was wondering if this would lead to a different way of understanding the Einstein equation in general relativity. Perhaps some way to study non-smooth relativity in some sense?*

AF: There are works that now have done it a bit. For example, the recent work by Mondino and collaborators [7], and also McCann [6] did something along these lines. Essentially, they have done Lorentzian optimal transports that have developed Ricci bounds in the Lorentzian context. It's definitely extremely interesting. I think the real question here is: in this kind of problem, can you really get fully rid of hard analysis? In this context, you have singular objects, which is what you need to describe singularities and very complicated spaces. But at the same time, there are also smooth objects; it's not that the spaces are singular everywhere, right?

This theory proved to be very powerful, for instance, in the Riemannian case. Now, this is a new theory; it is difficult to say how powerful it would be in the Lorentzian case. For sure, it's very natural to try. And I think it's not only very natural but also important to attempt because even just the act of trying will force you to learn to develop new mathematics, which so often has its own value. Then, will it give something that can change our understanding? It is early to say. I think sometimes it's good just to keep in mind that there is a smooth picture and a singular picture. Maybe the two communities should try to collaborate more in some sense, right? That's why you need both visions, I think, to really understand the mechanism behind it. Because the singular doesn't come out of nothing. It comes from something smooth.

So, there is a selection principle behind it. It's like when in PDE — I'm talking in a macro-sense now — there are these singular solutions to PDEs and very weak solutions and so on. But then the real ones are limits of a smooth solution because usually, the singularity does not come out of nowhere. It comes from something nice that somewhere develops the singularities.

Thus, often, the question is: which weak solutions come from smooth ones? Is it everything or not? Otherwise, you enlarge space, you make it much more general, but maybe you make it too general.

If you think about the Riemannian setting, the classical setup is the fact that at the beginning, Lott, Strum, and Villani [8], [9] used the optimal transport to characterize lower Ricci bounds.

Then Ambrosio-Gigli-Savaré [1] managed to characterize Riemannian lower Ricci bounds, right? So they managed to encode the information of being also Riemannian. This was a very important achievement because back then, it was not known how to keep the Riemannian structures in some way. They understood the way to do that, and from that, many, many more results followed. The lesson here is that you need to go by steps. We will see (laughs).

ZZ: *You have many amazing results in PDE and optimal transport. Do you have a favorite one?*

AF: I wouldn't say I have a favorite one. There are those to which I am more attached sentimentally because I was younger, and then I could feel the wonder. I don't know, as the year passed, I have been very happy about the results.

LH: *There's a difference between what I consider I'm happy about and what people appreciate, right?*

AF: Of course. So I think there are results that maybe you were very happy about, and maybe they didn't get the admiration you hoped for and vice versa. But there are at least a couple of them. One of them is the isoperimetric inequalities that I did with Maggi and Pratelli [5] which was a very fun project that started out of nothing.

After a talk, we started just to discuss what you said, because Francesco Maggi was giving a talk in Italy. I was there. I asked at the end of his talk: "You mentioned that optimal transport applies to this problem; what do you mean?"

He started explaining what he meant, we started to chat, and then collaboration started. It was a very fun collaboration. The final idea came up after going to a bar in Edinburgh at 1 A.M. It was really out of nothing. In research, you need these moments of luck. That's true, sometimes you are lucky. I have a very fond memory of that moment.

The other one, for instance, is the paper on the Sobolev regularity for Monge-Ampere, which we did with Guido De Philippis [3]. In that case, as well the idea came almost out of the blue. We spent seven years chatting on and on without getting into anything. And then, one afternoon, out of nothing, we got the key idea. From that point, things just came out and the project proceeded quickly, very quickly.

I think these are two projects where things in the end had a happy ending out of a bit of surprise and luck. It's nice to do mathematics in a friendly environment, it is very relaxed, just chatting with friends and discussing in the pool. Sometimes, it works, and good things come out.

So that's how they came out, these two. I keep very good memories of these two moments. These two and also the research we did in the obstacle program, with Joaquim Serra and Xavier Ros-Oton [4]. It was fantastic. These are fantastic projects that took us many, many years of work. That's a lot of blood.

LH: *Is there a project on which you worked for 10 years and still nothing?*

AF: Many. Most. Most projects sound appealing at the beginning, and that's the point, right? We all have to understand that success is a rare event in the failure of research. The question is just how to cope with that.

LH: *So, what's your strategy?*

AF: I try to have different ideas and different projects, and I hope that at least something progresses here and there. That's what happens when I have multiple-line research. Some project looks a bit easier, some projects may be a little more long-term, and some projects, are shorter-term.

I tried to keep a balance, and not to get depressed when everything was stuck. Once you are more established, things are easier. You can take things with more perspective. So I'm now in a lucky position because if not many things work, it's okay. It can happen, right? A few years ago it was a bit tougher, but sometimes I always tried and also worked with collaborators with whom I have a good personal relationship. It makes the research work smoother: when you and your collaborators are in the same frustration mode, you support each other.

LH: *I guess that in these cases after you work six years on a project, you almost give up. And then suddenly you have the idea that unlocks the situation.*

AF: You are right because the project went on for so long, we started thinking maybe we'll never solve it. At some point, we thought we should move forward. We asked ourselves: "Should we try to find a counter-example? Should we put out a theorem without the application we had in mind?" It's a bit of luck when you find the right idea after 6-7 years.

LH: *It must be a beautiful experience.*

AF: In this case it was because this story has a happy ending. Many don't have a happy ending. I think they are a lot of fun. I just have ideas that got stuck here and there. That's fine. I think the point is not to take it personally. Actually, my philosophy is that you learn much more in a project where you fail than in a project where you succeed. If you get success too quickly, you are happy, but you didn't learn too much in the process. *If it takes a lot of years, whether it's a success or not, in the process, you learn much more.*

When my initial attempt didn't work, I had to come up with a lot of ideas. Many of those seem useless now, but maybe they will be useful in some other way, at some other time. Maybe not, but in any case I learned a lot. That's also part of the process.

LH: *Did it happen? So, you basically learned something along the way of another project. Suddenly, it helped you with something completely different.*

AF: Absolutely. That can happen. So that's also what I mean by the ideas, the inspiration, even for this kind of obstacle problem that I was discussing, the revolution, sometimes in our approach, was that we realized that some formulas that were present in the minimal surfaces could be applied in obstacle problems.

And it wasn't obvious. At some moment, I remembered, I was working with Joaquim Serra, we were discussing and said "It would be so cool if this formula was true in the obstacle problem setting, too. It would be fantastic." However, our initial response was: "It's not going to happen. Come on, it's not going to work. Someone else would have found it." If this formula that everyone has known for 40 years would apply to this case, someone would have found it. But then we thought, "OK, let's try it". We tried, and...

LH: *Did it work?*

AF: No, it didn't work. We tried. We did all the computations we liked, but there was an extra term, in our case, with respect to the classical setting where people apply this formula.

In our solution, there were some extra terms that came up, and we didn't know what to do with them. We didn't know what to say about that additional part in the equation.

Essentially, we had this expression and we wanted to prove that a quantity had certain positivity properties. We looked at it and observed: "This term is positive, this other term is positive, this third term is also positive, and this last one? I don't know." It is funny to think about it now, because I was always so frustrated, and I kept thinking: "Come on, it should work, it would be so cool!"

I think I went on for over five months of writing the same formula over and over. I tried plenty of ways to rearrange the terms, trying to organize them differently to manifest the positivity I was hoping for.

And then one afternoon, I was rewriting the same formula again, and Joaquim Serra and I realized, but this term, isn't that what they saw in that other formula in the other paper?" We looked at that and went on: "This term corresponds to that positive quantity of that paper, but this second term is also equal to that quantity, but then from this expression you can get this other piece and then, wait, it is on the right side. This is also positive. Thus this last term, about which I didn't know what to say for months, is also positive like every other term in our equation. This works." In conclusion, we were looking at the same formula over and over for five months before realizing that it worked. The only reason we realized that was because we, in the meanwhile, were trying to think about what people had done. We were looking at other expressions. So it wasn't something obvious. It's not that you say something is positive because it's the square of something, for example. It was really subtle. And then we put everything together, no one before had realized that property.

LH: *So, once things works, it becomes easier, right?*

AF: Well, more or less. When you have the right insight, you still have to put in a lot of blood. If the first paper we wrote is 40 pages, the second, the one of generic regularity, its published version is 105 pages of very dense, very tough computations. I mean, it was very painful to make everything work.

So technically, it was very, very hard, but we felt it was going to work. Now we have the path, we have the tools. We have to understand how to make it work, and there will be lemmas that we will spend months to prove it. But you feel that you are going, and then you have the energy. Of course, maybe, as I said, it took us some time. For that project, it took three years from the moment we had the formula till the paper was completed. So it's not easy.

The difference is that you believe you are getting somewhere; you feel it. It's more of a feeling. When you have it, you are positive, right? Something is moving. We didn't know which theorem we could prove. I believed we were going to have a solid theorem, then we tried to get the best theorem we could. So that also gives you positive energy, right?

LH: *In particular, for hard computations.*

AF: Yeah. For hard computation, we need that. You need some positive energy. Otherwise, you give up.

LH: *Normally we like to ask some educational questions. So, what do you think would be the optimal way of educating students? Let's say starting from high school?*

AF: I think you need to stimulate the curiosity first so that they don't do math because they have to, but if they do it is because they like it. It's not forcing. It is also important to give them a reason to learn, in the sense that they have to be excited, but they also have to see a purpose. It's not just saying to young students they have to learn math because math is important.

I mean, they are 14. They're in the moment of their lives when they want to rebel against society, and you want them to tell them that they have to study because something is important. I mean, that's not a good reason for them. I think you have to show them a purpose, which could be something cool. Why do more kids want to do physics? Because they can do experiments and they see it's cool. Children can understand and learn from a home-made experiment. There are many cool experiments on YouTube. So, for mathematics, you need to show young people the purpose, right? You may say, for example, “Let me explain to you how cryptography works. How is it based on number theory? How is it based on factorization?” I’m not saying that students should learn only exciting topics, but at least they can see something, a purpose behind it. I think it is important to stimulate curiosity in students. In any way, you can give a reason.

And also another thing, that is a big problem in mathematics, is that people try to categorize students, especially teachers. They say you’re good at math or you’re not good at math. Right? One is very quickly classified: these are the good ones, and these are not good ones. I don’t think it’s good because, in reality, every mathematician is different. There is no one good way of mathematics. I’m multitasking. I follow many projects at the same time. There are people who are not like me who have to concentrate on one project at a time, and they do super well. Who is doing better? None of us. I do my way, you do your way, and each of us follows its own way. But in school, we characterize. If you are very fast in doing computation, then you’re good at math. If you are slow in computation, then you’re bad at math. Is it really what it means to be good at math that you’re doing computation fast? Calculators do faster than me, and they’re not doing math. This is also how training students works: speed is training; it is not how good you are.

So I think the point is to stimulate and encourage everyone to develop their passions, whether is math or something else. And some of you need to create a reason to do stuff. And also, knowledge by itself is overrated in the sense that knowledge changes very quickly. What the students need to know is to think critically, develop critical skills in analyzing stuff, and be able to learn stuff quickly and effectively.

I think it’s a very bad habit to categorize early. If I think of myself as a student, when I started, I was not the best in my class. I started with very little mathematical knowledge at the university. I had done classical studies in high school, I learned a lot of Greek and philosophy and very, very little math. When I finished high school I didn’t know how to compute derivatives. I didn’t know how to compute anything; I didn’t know any calculus while all my friends knew it already in my first undergraduate year. And yet, it worked out for me — I’d say it worked out very well, right?

If, in the first semester of university, someone had chosen the best ones of my course, I would not have been there. And why? Just because someone studied more, they were more passionate before. In those early stages, those who are passionate at five years old are going to be better, but are they really going to be the best mathematicians later? Maybe they will, maybe not. I think we put too much stigmatization too quickly. I think everyone is able to do mathematics, I don’t believe that people are unable to do mathematics. However, it is socially accepted to believe that some people are unable to. Then it’s natural for a young person to think: “They told me that I’m not able, why should I study math? The teacher told me I could not do it.” So, in primary school, you already give up.

LH: *It’s true, that may happen.*

AF: That's why also you have so little gender balance. Young girls are more thoughtful; they think more. So if a teacher tells the girl "You're not good at math", they will believe they're not good, and they quickly doubt themselves. Boys that age never doubt themselves. You'll see much more saying, "No, I'm going to do it anyway." They are more rebellious, right? But it doesn't mean they are better.

LH: *What's your advice for young people who would like to decide whether to go into academia or not? It's a very difficult path.*

AF: At which moment?

LH: *Like students, PhD students, and postdocs.*

AF: It's a very difficult and competitive path. We academics are losing people because now private companies offer a lot of jobs, more than what we can offer. They pay much better than what we can pay.

My advice is: if you're still a student, like at a university, it's too early to know whether it's going to be a work out or not, right? You have to try.

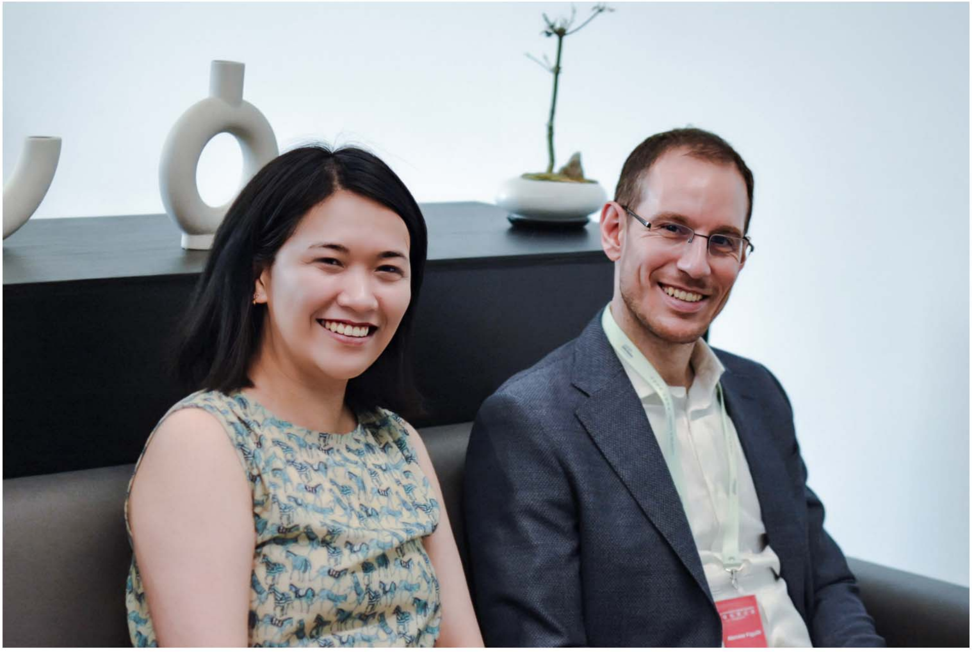
I think PhD is a very beautiful, interesting training moment. You challenge yourself. If it works out and you are passionate about it, you like it, you can keep going with the academic career. You can try. If it doesn't, you learn new skills; they're valuable, no matter what, because you learn how to think critically about problems, and you can go to a private company. For those who are admitted to the PhD school, I think it's worth trying. It's a very nice experience. Then there are not many positions, and there are many students. But I think if that's what you like, you should try your best to do it with some sacrifice, but that's a choice, right? That's a very personal choice which I will never regret.

I also tell my students that Switzerland is a small country. You cannot stay here. There is no way you will find PhD and postdocs and positions here. You have to go out. You have to change the country. That's a fact. If you want to stay here, you have to go to the private sector. Many people want to remain in Zürich. It's a nice city, you live well. It's a life choice and I don't judge it. The same way, like myself, I was in Italy. I moved to France, then I moved to the US. I made sacrifices. It's necessary to be willing to do them, and it's not forever. If it's just the first step of your career, then you should try it, if you like it.

If then you feel it's not working — you are not happy, it's too much sacrifice — no one forces you. The good thing about mathematics now is that it is very well recognized in the private industry. You can get a very rewarding job. So I think now actually is a good moment to try because there are very good alternatives if things don't work out.

LH: *Thank you, Alessio, for your insights. One last question: do you have a book to recommend? We are working on a list of book suggestions from all the people we interview. For all sorts of people, you can think of PhD students or even younger undergrads. If they want — you know — just to read some beautiful math, what would you recommend?*

AF: I started a lot of books as a first-year, and second-year, PhD student, but they were mostly Italian when I was studying them.



When I moved later, I was always reading many books, but they were already advanced. Sometimes I must say that I'm a big picky about books.

There are some beautiful, classic books like Functional Analysis by Brezis [2]. That's a great classic for functional analysis. It's very concrete and to the point.

It's not abstract functional analysis with complex analysis and then PDEs. It is going to the point where you see why you do function analysis with applications. I like it because quickly you get it.

And then for other topics, when I couldn't find some books, sometimes, I wrote one by myself. I recently wrote a book on optimal transport. Now, every time I teach classes, if I can write a short book, I try.

There are two sorts of books, right? There are the big books which are like bibles. You should never use them to study. These are for reference. Very big ones, with a lot of material, too much. What I like is the short books based on graduate courses, for instance, the American Mathematical Society, but also the European Mathematical Society.

Now, they publish a lot of books based on the graduate courses. It's a very nice story, usually because it's really a collection of notes from the professor and students on what they taught in a class. So these are the best ones I think to learn. But those with very long structures have too many of too many things. These are not good ones to learn because it's too much and it is too advanced. The technicality, the technical results, you learn them when you need them. You can't learn everything; it's not worth trying. You'll forget anyway. My suggestion is: you just should try to get books that are 100-150 pages, short books which give you a vision of the subject. Then, you decide what level of knowledge you need, and from there, you can go for specialized readings. My advice is always to start with the easy books, whatever it means.

LH & ZZ: *Alessio Figalli, thank you very much for your time.*

Interview held on July 17, 2023

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