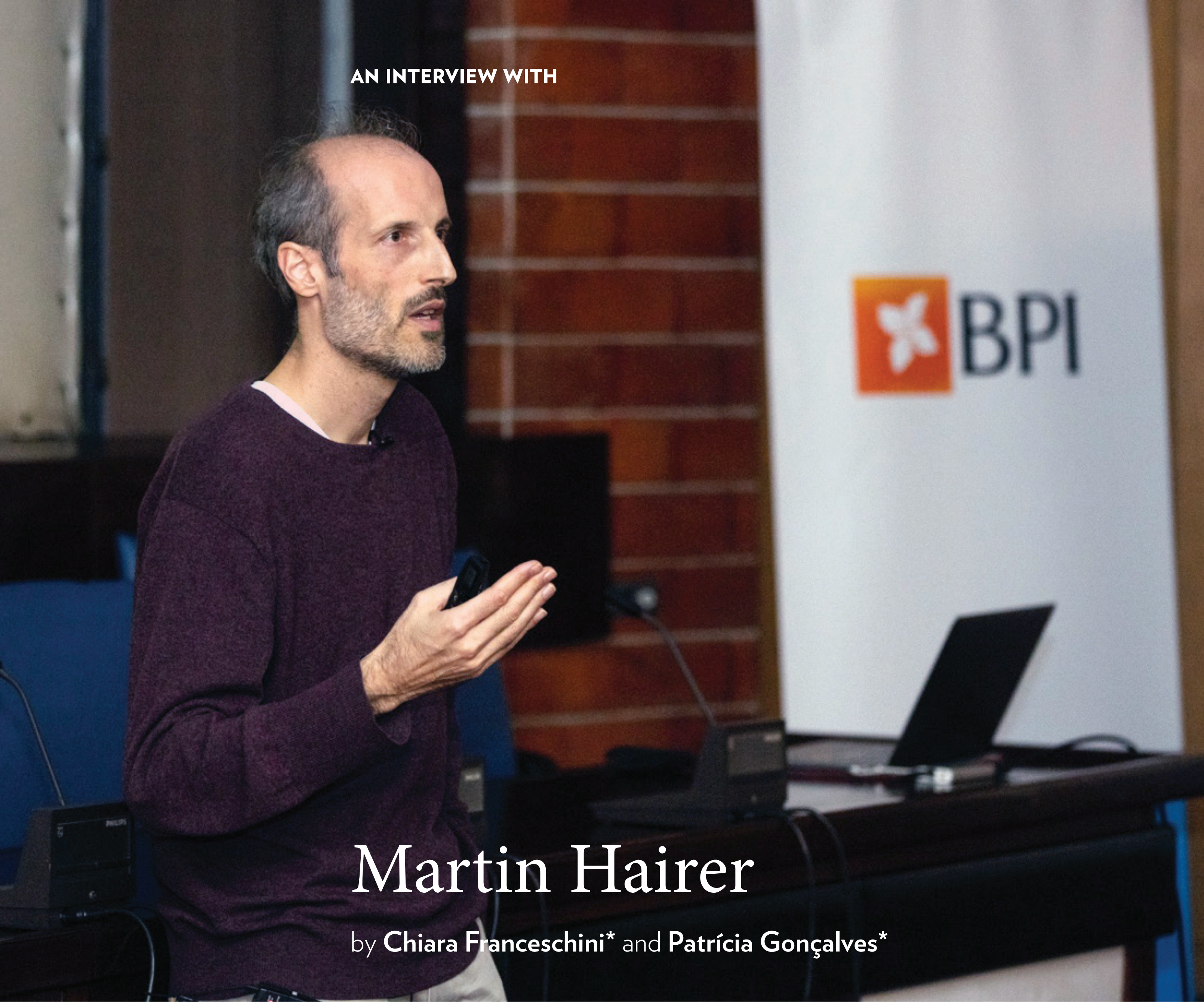

AN INTERVIEW WITH

Martin Hairer

by **Chiara Franceschini*** and **Patrícia Gonçalves***

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Martin Hairer is a British-Austrian mathematician working in the field of stochastic analysis, who was awarded a Fields medal in 2014, the most prestigious prize in the career of a mathematician. He holds a chair in Probability and Stochastic Analysis at Imperial College London and is considered to be one of the world's foremost leaders in the field of stochastic partial differential equations (SPDEs) in particular, and in stochastic analysis and stochastic dynamics, in general. By bringing new ideas to the subject he made fundamental advances in many important directions such as the study of variants of Hörmander's theorem, the systematization of the construction of Lyapunov functions for stochastic systems, the development of a general theory of ergodicity for non-Markovian systems, multiscale analysis techniques, the theory of homogenization, the theory of path sampling and, most recently, the theory of rough paths and the newly introduced theory of regularity structures. Besides the Fields medal, he was awarded several highly reputed prizes and distinguished with several honors and distinctions. Among those, we mention the LMS Whitehead prize, the Philip Leverhulme Prize, the Fermat prize, the Fröhlich prize. He was distinguished as a fellow of the Royal Society, the American Mathematical Society, the Austrian Academy of Sciences, the Berlin-Brandenburg Academy of Sciences and Humanities, the German National Academy of Sciences Leopoldina and was also distinguished as an Honorary Knight Commander of the British Empire. He was awarded several fellowships and grants including an ERC, a Leverhulme Leadership Award, EPSRC; he is editor of several leading journals and delivered talks in many highly reputed institutions worldwide.

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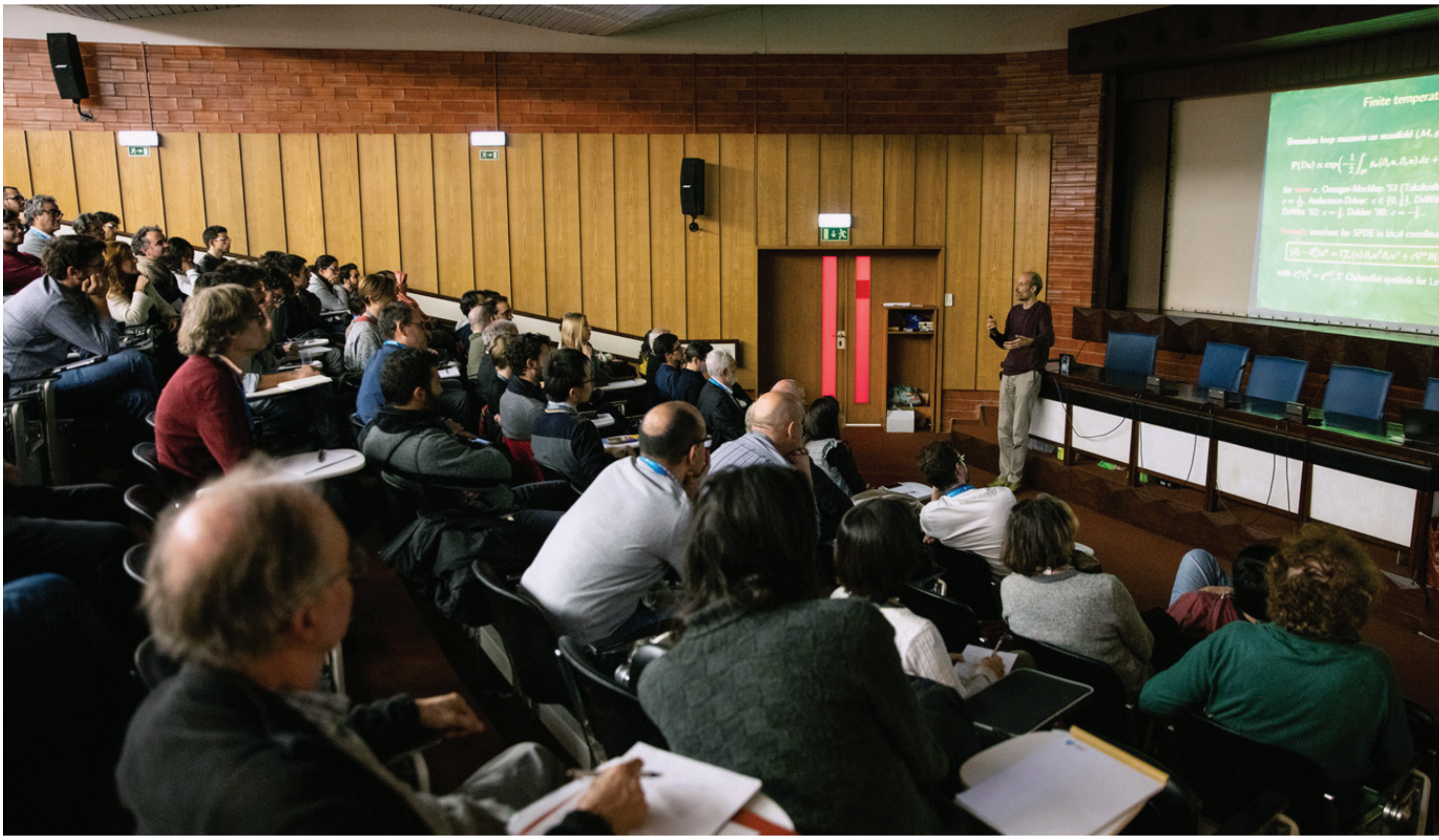

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We have interviewed Martin on the first week of December 2019 while he was participating in the conference "Particle Systems and Partial Differential Equations XIII", that was held at Instituto Superior Técnico of the University of Lisbon.

How did you end up studying mathematics? How much do you think you father's job influenced you?

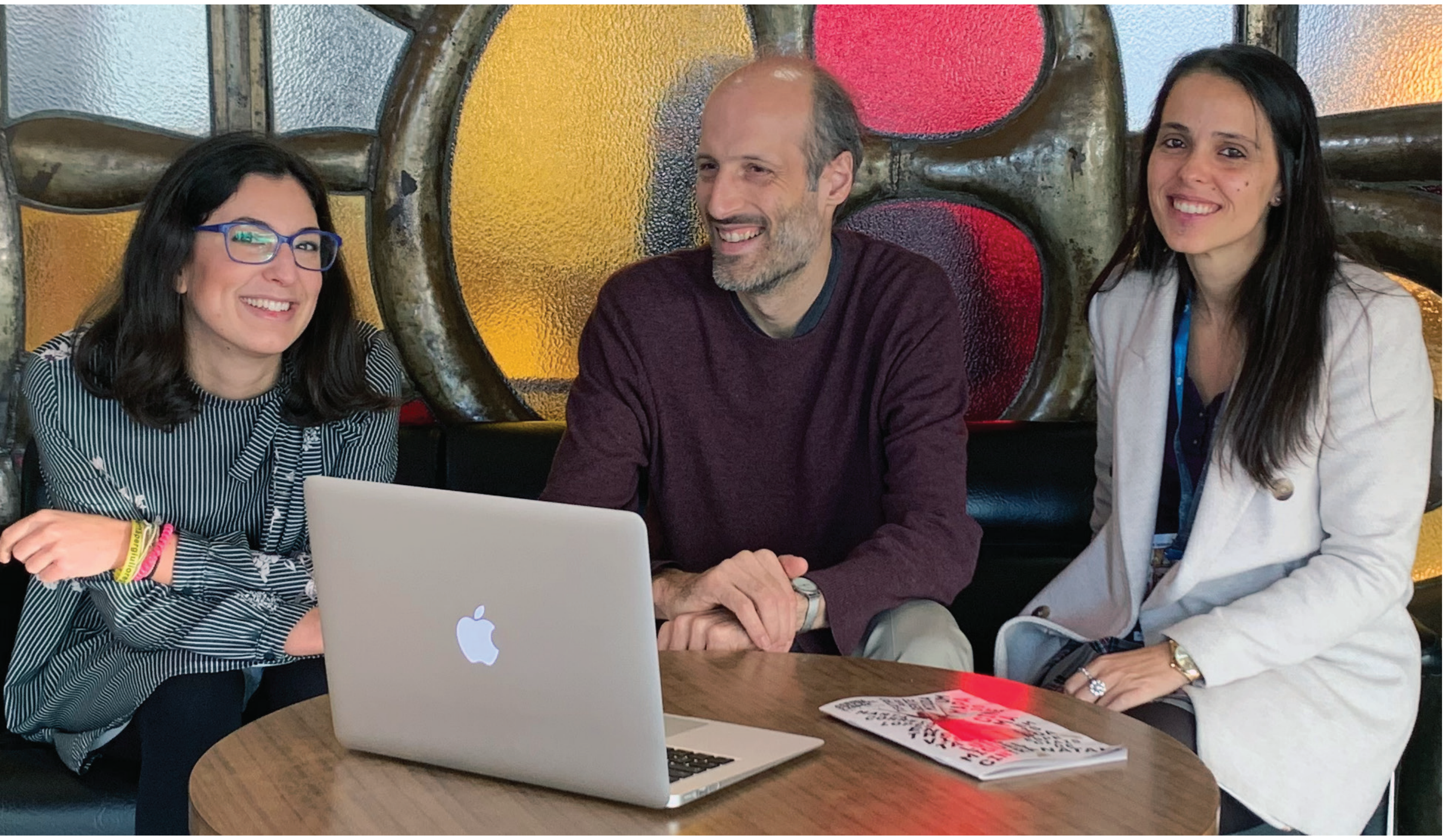
I have certainly been influenced by my dad. Thanks to him I was exposed to maths in an informal way at an early age: I knew what a differential equation is, in some sense, when I was about 12 years old, and this is something that normally you do not learn at school. My dad would explain to me if I asked about his work, but he did not impose anything on me. On the other hand, my mother was a primary teacher for a couple of years, and then decided to work in a toy's library, and actually she is still doing that today.

Did you also know that you wanted to become a mathematician? Can you tell us your path into math?

It wasn't always clear to me that I would do mathematics, I was interested in programming as well, so it could have been a possibility for me to become a computer scientist or an independent developer. I didn't study mathematics for my PhD, I actually have a physics diploma from the University of Genève, but both maths and physics backgrounds overlap for most of the courses of the first years. The reason I have switched from physics to maths is because I was never fond of the experimental/laboratory works, nor the data analysis coming from there, I was more interested in the theoretical aspects, so it was a natural path to go to the theoretical physics rather than the experimental physics side; and even there, I sometimes found the arguments too handwaving. I didn't feel sufficiently secure to make such arguments, so I felt that, if I wanted to publish something, I wanted to reason with arguments that are rigorous: this translates into a future in mathematics.

How did you get the idea of singular SPDEs and regularity structures for such equations that lead to the Fields Medal in 2014?

The first work I did in this direction is an older paper I have with Andrew Stuart. He was interested in path sampling and wondering how to simulate in a computer a stochastic differential equation (SDE) conditioned on hitting a specific point. It is not easy to do this because it might never hit the point, so we thought of finding an SPDE such that its invariant measure is the bridge of that equation. If you do that for a diffusion with additive noise and gradient drift, then the SPDE that you get is a reaction-diffusion equation. It was natural to start



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wondering what happens in higher dimension by taking an arbitrary drift that is not necessarily gradient. In this case what you find is a kind of a reaction-diffusion equation but with a Burger's type drift. We wanted to give a notion of solution to those SPDEs, but it was not clear what would be the interpretation of this drift in terms of the solution. The article solving this problem was my first work in the area of singular SPDEs. Later, while I was working at the Courant Institute in New York, Gérard Ben Arous suggested to me to actually look into the Kardar-Parisi-Zhang (KPZ) equation and to try to give some meaning to its solutions.

Is there a point in your life when you realized that you actually had good chances to win the Fields medal? How did things change for you after?

The first time someone mentioned this to me was after my paper on the KPZ equation where I prove uniqueness of solutions, but to be honest I didn't think it was possible at that stage to get the medal. When I got the idea of regularity structures, I thought that it might be possible, but that work came out when I was almost forty, so it seemed pretty unlikely, since there was not much time left. After winning it, my life didn't change all that much: clearly now I do more public initiatives (public lectures, interviews like this), but it is not all that much, maybe three or four times per year now. I already received many invitations after my original paper on regularity structures came out, so the medal didn't really change the amount of travel I do.

You won the Fields medal in 2014, together with the first Latin-American, the first woman and first Iranian. Nowadays there is a lot of talking about minorities in our community and in the world in general. How do you think minorities should be regarded?

Well, I was the first Austrian (laughing). This is a tricky question. Thanks to my wife, Xue-Mei, who is also a mathematician, I can have some idea of what it means to be a woman in mathematics. I can see she gets a bit annoyed if she has the impression of being asked to do something or to be part of something only because she is a woman. Of course, you want to be asked/invited because of your mathematics, not because you are a woman. Nowadays, there is a lot of pressure on having more women in scientific committees as this would increase female participation. At the level of committees like workshop organisation, I can see how this helps to improve women's representation which is a good thing, but in some other cases it can also be counterproductive. For example, at the senior level, the proportion of women in the maths community is lower that the proportion of women you would like to have in committees, which translates into extra administrative work for women. This is also unfair, maybe they'd rather spend that time doing research. I am not sure there is a good solution, I hope at some point things, hopefully, will become more balanced.

The History of Mathematics has a lot of mathematicians that we admire. Do you have a particular admiration for one that you would like to mention? Do you have a professor that is was/is an idol for you or that inspired you, by the time when you were at the university?

I think that one of the most impressive mathematicians is Von Neumann, he was one of the last mathematicians who knew everything: both on the analytic side, the algebraic side, also in computer science, he was extremely broad in many things. In terms of professors' influence, probably the strongest influence is from my PhD advisor, Jean-Pierre Eckmann, I like his way of thinking about problems: he is fascinated by problems regardless of the mathematics he knows. In fact, he would learn new mathematics to understand and solve new problems. Moreover, he has 'good taste' in the sense that he can recognize what makes a problem interesting.

Among your many results and achievements, is there one that you are particularly proud of? And is there any other problem that you tried to attack in the past, but it is still open?

In terms of achievements, the paper I am most proud of is the one regarding regularity structures, I would also mention my paper regarding the Navier-Stokes equation, which was the complete answer to my PhD problem and came out few years after I defended my PhD thesis. In terms of open problems, one thing that is embarrassing is that I still haven't managed to solve the problem Jean-Pierre gave me for the diploma thesis! (Laughs) I was able to prove that the question he asked was wrong, but I still don't know how to find a proper answer. The setting regards a finite chain of anharmonic oscillators with nearest-neighbor coupling where the first and the last oscillators are coupled with two thermostats at different temperatures. Even to show that there exists an invariant measure is not obvious and it can be done only in specific situations, but not in general.

Have you ever visited Portugal before? What is your impression about the country?

My first visit to Portugal was in 2003 for a conference, but it was a short visit and I did not have enough time to form any impression on the country, this time is also short, so maybe on the next visit!

Is there one thing that you would have liked to know when you were younger, that now you would say to a younger Martin Hairer? Any advice for the young mathematicians who have just finished their PhD?

After the PhD I wasn't completely sure to continue in academia. I believe Jean-Pierre wanted me to go to Courant Institute for a post-doc position, but I went to England to conjugate my personal and my professional life. I decided to give a chance to academia, I had a fellowship from the Science Foundation for two years and after that I would have seen if I could get something decent. If not, I would have probably continued to work as a freelancer for software development, which is something I still do in my spare time. It took some year for the academic salary to be comparable to the one of a software developer!

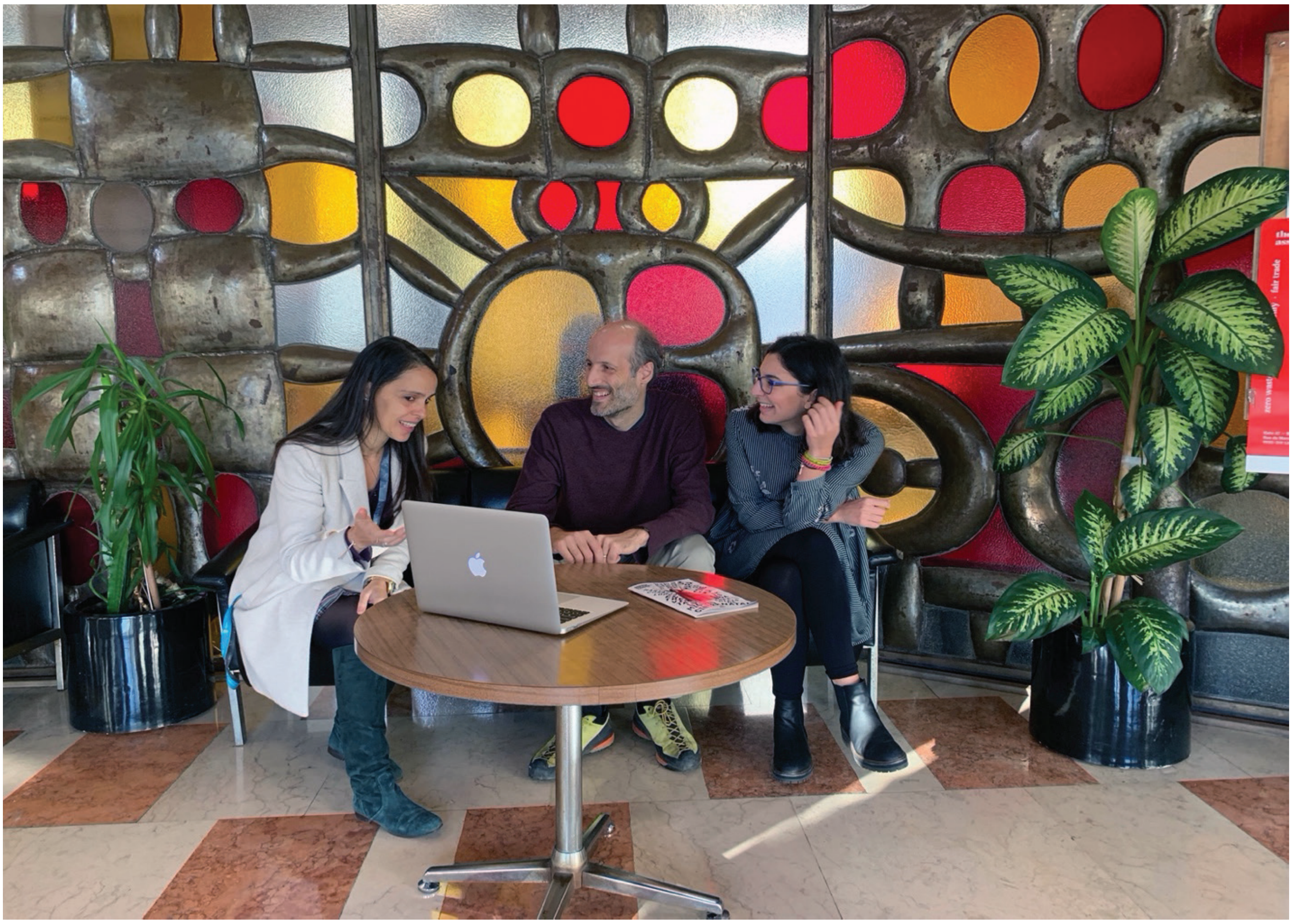
What do you think is the biggest problem of our academic system (mathematics) and what would be the first thing that you do to improve it?

For sure the issue that you are forced to move quite a bit before finding a permanent position. This is related to what we have already discussed regarding women in mathematics. Naturally, people drop out of going into academia, because it is difficult to combine both family and work. Another problem regards the funding mechanism. You try to pick some winners in a competition and you give them more money than what they can actually spend. In our research fields, we don't need so much money, all the equipment we need usually is just a laptop and some money to travel, with additional funds going into hiring postdocs. My impression, is that it would be better to split some of the big grants among several people. This would be relatively inexpensive and would make more people happy. Clearly you give money to someone who has a sort of masterplan, with a big global vision. However, there are many researchers that don't have it, but who nevertheless produce very good mathematics. For this reason, I think there should be more money available in small grants. For example, the money of one large grant (say €300k/year) would be sufficient to provide 50 mathematicians with a travel grant of €6k/year. As a consequence of the current system, most post-doctoral are funded through these grants. At least in the UK, there are relatively few positions for researchers that are independent of such grants, although in the USA they are more common, an example is what they call instructor positions.

How do you see your research field in 20 years from now? What do you think will be the hottest problems for the community? Would universality classes' problem be solved?

I am not very optimistic regarding the last question, I don't think all the questions regarding universality will be addressed, but, on the other hand, I believe people will still be interested in such problems because many of the probability questions tend to have this flavor, i.e. to produce scaling limits of some objects of interest. It is hard to make specific predictions regarding which results are going to be cracked: if you do, it probably means you are about to crack it.

In the recent years, the Mathematical and the scientific community in general have been overwhelmed with the use of bibliometric data to assess and evaluate individu-



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als and institutions. What do you think about that?

I think in general it is very bad to have specific metrics. Or at least it wouldn't be as bad if you don't say which bibliometric data you are using for evaluating. If you use an unknown metric, people cannot game it; however, as soon as you publish which metric you are using to assess people, they are going to crack it. Of course, the aim is to design the metric in such a way that people will produce better research in gaming it, but unfortunately no one has figured out which sort of metric has this feature. You will always find an easier way of gaming it, instead of doing the things you are supposed to. There is a funny example regarding this in the UK. The government wanted to have trains running on time, so it started to impose fees on companies which had more than 5% of the trains running late. They defined what it means for a train to be on time, namely that it has to reach the final destination within 5 minutes of the scheduled time. This produced several consequences: the train companies rewrote the timetables, so that the journey would take 10 extra minutes. That's not too bad, as it would make the timetable more realistic. However, another thing that happened is that, since the rule for being on time just regarded the last station, they started to jump stations as this would take several minutes for people to get in and out. Clearly, in this way companies reached the target, but not in the way it was intended to be. Another funny story on how metrics produce unintended results regards the league tables for best university. One publication in the UK produces a ranking every year and some time ago they changed the criteria for what they call "research impact" to make the evaluation more objective. The outcome of that was that University of Alexandria in Egypt turned out to be number four worldwide. How is that? It was because of one single guy. He was a mathematician who produced a lot of papers with tons of self-citations published in a journal where he was editor-in-chief. I sent an email to the guy who came up with the methodology to point this out and I noticed that the year after they basically changed the methodology in a way to just rule out University of Alexandria with an ad hoc procedure.