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Hugo Duminil-Copin

by **Jorge Milhazes de Freitas***

I read that you were keen on sports, and particularly handball. So my first question is: how close were we to having a world champion handball player rather than a Fields Medalist?

Well, you know, at the time I didn't really know what the chances were on either side when I made the decision. I chose quite early, when I was 15. I basically had to decide between going to a special sports and studies school or going to Lycée Louis le Grand, which was a special school for mathematics, in some sense. So I didn't know what the expectations were.

Later on, though, I stopped growing. So I'm pretty sure I would never have made it into the national team, which would have made becoming a world champion rather difficult. At the time, France was the leading nation in handball—it was the generation of Karabatić and company. They were Olympic champions and so on. Making it into the national team was essentially the key step, and with my physical strength I would never have gone that far. So, in retrospect, it was a good choice.

But you also don't go into mathematics thinking you're going to get a Fields Medal. There is so much randomness. Very, very few people could reasonably presume it. Maybe for someone like Terence Tao, one could guess early on that there was a good chance. Personally, I didn't even know about the Fields Medal at the time.

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When did you realise that you wanted to do mathematical research for a living, or that you might have a future in mathematics?

It happened in a very strange way. I always loved physics and mathematics, even though I was never as good at physics as at maths. When I arrived at the École Normale Supérieure, the level of the people around me was really intimidating. I ended up dropping physics and, seeing the level of others, I thought I would probably end up teaching—which was something I wanted anyway. I come from a family of teachers, so I was perfectly happy with that.

Then I took the agrégation, the national exam to become a teacher, and it went extremely well for me. I even remember how people told me my ranking: I finished second. They said, "We have the results, and nobody can believe it—you were second." Not because they thought I should have been first, but because they simply never imagined I would be second.

That gave me a bit of confidence. The agrégation is officially an exam for teaching, but in practice it is really an exam in mathematical training. After that, I did a one-year master's program, which included a long stay in Vancouver. There, I had a lot of time, and in some sense some of my former weaknesses became strengths: working for a very long time on a problem, becoming totally obsessed, trying many different approaches because I have a hard time focusing on just one thing.

By the end of that year, I realised that this way of working was actually very effective. I ended up with, in some sense, two research papers, and it became clear that I could start a PhD. But it really came out of the blue. I didn't go there thinking, "Let's become a researcher." I went because it seemed like a good experience and I needed it for my master's degree.

It worked so well that I almost became a mathematician by accident. When I came back, I was already one, without having planned it. After that, I was hooked. I realised that my way of thinking was well suited to research, and from then on I never really doubted it.

And what about your choice of probability theory?

That was a bit simpler, and it is probably related to my inclination toward physics. At ENS, I had a teacher, Jean-François Le Gall, who is famously an outstanding lecturer. His courses were extremely clear and well organised. And yet, for the first two months, his class had a catastrophic effect on me.

There was this notion of a random variable. It was explained very well, but in a very formal way, and I simply didn't get it. I kept thinking: it's just a function—why do they call it a random variable? A function from one space to another.

At some point, I finally understood the subtlety: what information you want to extract, and how randomness is encoded. Then I was completely hooked. I went from total confusion to a global understanding, and suddenly this beautiful course became crystal clear. At first it had felt too perfect, too foreign to my way of thinking; then it became very natural.

That pushed me to enrol in a very focused master's program in probability. I then had fantastic courses, including one by Wendelin Werner. That's where I learned about percolation theory and the Ising model. It became clear to me that this was mathematics deeply connected to physics—I felt like I was bringing physics back into my mathematical life. From then on, it was obvious that this was my field.

It also helped that I was good at it. You can like something and still be bad at it. In my case, it really matched my intuition, and things worked very well.

Do you have a role model in mathematics—someone you particularly admire or who inspired you?

Interestingly, I didn't really have any role models until quite late. When I was young, I didn't even know that being a mathematician was a real job. I wasn't the kind of student who read Thurston's essays or Feynman's lectures.

Later on, I was surrounded by people I deeply admired, such as Wendelin Werner and my PhD advisor, Stanislav Smirnov. More generally, I was inspired by a certain way of doing mathematics.

In that sense, I would single out Harry Kesten. He was not necessarily flashy, but he had an extraordinary ability to drill through problems where nobody else could make progress. His proofs were often not elegant at first—his papers are, frankly, horrible to read. They are extremely technical. But the core ideas are almost always the right ones. After years of polishing by the community, they turn into incredibly powerful tools. I have infinite respect for that ability.

One of the best moments of my mathematical life was meeting him. I met him twice: once at the ICM in 2010, where he gave the laudation for Stanislav Smirnov, and once when I gave a lecture for his birthday. Even though he was already ill at the ICM, he discussed recent work of Stanislav's with me. To have someone I considered a kind of god talk seriously about the work of a young mathematician was incredibly impressive. He is probably the person who impressed me the most, mostly through his mathematics.

We are now in an era in which artificial intelligence is exploding. It is everywhere and changing our habits and the way we interact with computers. Do you feel an impact on your research? Do you use computer-aided proofs or other AI tools? And do you think AI will have a lasting impact on mathematical research?

Yes—I was expecting that question. Let me start with the first part: do I use it?

The daily life of a mathematician is not limited to research. For everything else, I use AI quite a lot, as a very powerful assistant. I always dreamed of having an assistant to handle tasks I am not particularly good at. Nowadays, it is extremely useful for polishing writing. I am still a very basic user, and I don't trust AI at all for mathematical content, but for form and style it is already very helpful.

This year, we have started to hear about AI being able to help prove easy lemmas and similar things. It is still borderline, and in my area—where arguments are often less formal—the impact remains limited. For PDEs and very formalised areas, it seems to be becoming quite efficient. For percolation, where you work a lot with drawings and geometric intuition, it is essentially impossible at the moment. But what is impossible today may become easy in one or two years.

As an assistant for mathematics—just as it already is for writing—I think this will arrive very soon. I try to stay open-minded and not reject it outright, because it is clearly going to be part of our lives. One thing that already works remarkably well is turning handwritten notes into polished LaTeX. For teaching, I still like to write things by hand when I prepare a course, but now you can convert notes into a rough LaTeX file very quickly and then use AI tools to clean it up. That is already a reality, and with some practice it can save a huge amount of time.

As for AI helping to prove things, I am still waiting—but I suspect it will come soon. Whether AI will generate genuinely new ideas is a different question, and that leads to a deeper issue: what is mathematics, really?

If the goal is simply to prove that something equals zero, then I have no doubt that an AI will eventually produce such proofs. But if the proof is 1,000 pages long and unreadable, it does not give me what I want, which is understanding. I am also deeply interested in the journey of proving something, not just the final statement. So I expect that AI will very quickly become better than us. There may not even be a long intermediate phase in which we can enjoy discussing mathematics with AI as equals. We will move rapidly from thinking that AI is a bad mathematician to realising that we are the bad mathematicians.

This forces us to rethink our role. What do we bring to science? Already today, much of modern mathematics is too complicated for non-mathematicians. Our role may increasingly be to prepare conceptual ground, to clarify ideas, and perhaps to act more like philosophers of abstract thought.

For the next series of questions, feel free not to reveal anything you don't want to or can't disclose. Regarding the Fields Medal, did you have a sense at some point that it might happen? Did you know you were on some kind of shortlist—assuming such a list exists? Were you anxious?

I can be quite open about this, because I actually think the way the process works isn't very considerate of the feelings of people who are clearly on some kind of shortlist—let me be generous, but I don't think that's an exaggeration.

Roughly speaking, you can identify maybe the top twenty mathematicians of a generation. Even though that notion of "best" is strange, of course. Sometimes there's one person who stands out so clearly that everyone knows they're going to get it. I'm pretty sure Peter Scholze, for example, had zero doubt that he would get the Fields Medal—though he probably didn't care much. There are people like that.

But beyond those obvious cases, there's usually a group of about twenty people, and within that group maybe three or four really stand out. So pretending that you don't know you're among those twenty is a bit disingenuous. In my case, I knew I was in that group.

I also knew that, even within probability, there were probably four or five people who could reasonably get it. Any of those choices would have made sense. So yes, you live with that. That part is fine. What you don't know is when—or whether—you'll be told.

Not having a fixed date is actually a bit perverse. People who haven't heard yet are waiting. That's human. It's not so much that you want to have it; you just want to know.

I was lucky because I was told quite early. But I think about others—people I respect enormously, and who I believe were at least as deserving as I was—who were probably waiting, thinking there was still a chance. That feels a bit indelicate. I really think there should be a fixed date: say, "We tell people on January 8."

Then, if you don't get the call on January 8, you know. And your life will be perfectly fine. I can tell you: the Fields Medal is wonderful in some respects, but it's also a heavy load. You can absolutely be very happy without it.

This is something that isn't said often enough. The process isn't considerate enough of people's feelings. I remember a conversation in April 2014 with a mathematician—I won't name them—who said, "You know, I don't have it." And I could see the relief in saying that. The list had become too short; at least things were clear. In some sense, knowing helped them make peace with it. I really think this part of the process should change.

Either do it like the Nobel Prize, or keep the secrecy—but at least give a date. By the way, the secrecy itself can be quite funny. I have a few good stories about keeping a secret for six months.

In my case, things were a bit more complicated. Out of kindness, some people—not on the jury, but you know how information circulates—told me back in 2018 that I was on the shortlist. That was actually not easy to navigate.

In 2018, it didn't even cross my mind that I might be shortlisted. I went to the ICM to give my sectional talk, very happy, not thinking about the medal at all.

But once someone tells you that kind of information, you can't un-know it. Of course, it's not the same jury, not the same four years—it's a long process. I had to tell myself: be serious, keep doing mathematics the way you like to do it, not in a way that optimizes your chances.

There's a temptation to go on an "advertising tour," visiting all the top universities. But what I really like is doing math and working on hard problems. Still, it required some mental strength not to think about it too much.

One funny consequence is that, in 2018, I told my wife and my parents—because, well, I'm human. And they became stressed. They're not in control, right?

You're not supposed to tell your family—your spouse is allowed, but not your parents. So in February 2022, they knew I should hear something around February or March.

My father kept saying, "Hugo, you look tired. You look sad. You can tell us if you didn't get it. You know we love you, we don't care."

And I was like, "No, I couldn't be happier." Inside, I was thinking, I look sad? I feel great. But my father was convinced my face looked tired and miserable. I was completely confused.

Eventually, I told them—mostly because they were so stressed that I was sad, even though I wasn't. I was stressed, yes, but not sad. It was funny to see them so worried about my well-being that they didn't realise I had actually won it.

And then, after I told them, they said, "Oh no, that's not what we meant. You just look tired for other reasons." And I was like, "So you think I look tired all the time?" That was also kind of funny.

So how far in advance are you told?

I found out in January.

That's quite early—the ICM is only in July. It's not useless, though. For instance, I could quietly read work by other medalists.

France is also peculiar in this respect. Someone like Cédric Villani really embraced public life—he was brilliant with the media, and the media loved him. There was a clear "Villani effect," and I knew there would be something similar: the Fields Medal is much more visible in France than in many other countries.

So I knew I would get significant media coverage, and that I'd have to deal with it—especially given the comparison with Cédric's very polished public presence. That worried me a bit. I probably should have done media training, but because I wasn't allowed to tell anyone, I couldn't even get advice from my university.

At least I could read and prepare things quietly—my ICM proceedings, for example. That wasn't so bad.

A funny thing about secrecy: I made my entire group read my proceedings without them realising they were the proceedings. I told them, "Some journal asked me to write a review," and they completely bought it. It was ridiculous, and at some point I was afraid they'd catch on.

Your daughter was one year old at the time, right? So she didn't understand…

Well, she still doesn't.

We actually had a math argument the other day about triangles. She showed me a triangle with her fingers and said, "This is a triangle, Dad." I was very proud. Then she turned it upside down and said, "This is not a triangle."

I tried to explain that orientation doesn't matter. She said, "No, it's not." After ten minutes, she concluded, "My teacher said it's not a triangle. So it's not." And she walked away.

So I lost a math argument to my three-year-old daughter—who certainly lied about what the teacher said.

She has absolutely no idea about the Fields Medal. It will probably be funny when she does, but that will be much later. In fact, this is a good summary of my life overall. Among people I know, I'm not really a "Fields medalist." My status hasn't changed much. Even within my research area, colleagues don't treat me differently, which is good—it means the mathematics comes first.

What did change is media attention, politics, and to some extent interactions with students—especially in large lectures, where behaviour can be different.

I had a shocking experience at Harvard. It was a conference in honor of Elliott Lieb's 90th birthday. Elliott Lieb is a god in mathematical physics—his influence is everywhere, especially close to my field. I have infinite respect for him.

During a break, I was talking with Elliott, and a young guy came up and asked for a selfie with me, almost pushing Elliott aside to take the photo. I thought: "You have no idea you're standing next to one of the most accomplished mathematical physicists of the 20th century". I would love my work to shine even a fraction as much as his.

Instead, it was like: "Please move, we want the picture with the Fields medalist." That's a completely ridiculous side of this prize.

My next question was whether it had an impact on your life and whether it changed things significantly—but I think you've already answered that.

What definitely changed is the media exposure.

I feel like I'm constantly being asked to defend mathematics. You suddenly become an ambassador for the field, and that can be overwhelming. Of course, you can choose not to engage too much, but I often feel compelled to give something back. And that comes at a price. It's a very heavy load.

Sometimes it's absolutely exhausting. You're pushed far outside your comfort zone in many ways, and I really think institutions could provide more support for that. It requires a lot of energy, and in some sense you're quite alone. This is something that genuinely changed my life: there are many things I now do because I feel it's my responsibility—even though, honestly, I would sometimes prefer not to do them.

That said, it's demanding but it also has very positive aspects. In 2023, for instance, I gave about a hundred public lectures. That was completely crazy. At the same time, speaking in front of 500 kids is incredibly cool—there's so much energy. You're exhausted, you can't really do much research, but there are real upsides.

You feel useful, in a way. I think I felt useful for the first time in my life when I started interacting with the public. There's much more to be done in that direction. As a community, we should be far more involved in public engagement, because people really enjoy it. A large part of the public actually loves mathematics.

What advice would you give to a young researcher who isn't sure whether to pursue a career in mathematics? What would you say to someone in that situation?

It's a difficult question, but an important one. For me, mathematics has never been easy. I'm not someone who learns it effortlessly. Even when I grasp an idea, translating it into a formal proof can be really hard. I've always had to find ways to keep going, even when things were tough.

One approach that worked for me—and that I'd recommend—is to work on multiple problems at the same time. I always try to keep at least three in mind. First, I like to have a problem that I know I can solve—something below my level. This is my safety net. On days when I feel drained or stuck, working on that problem helps me regain confidence and energy.

Second, I need a problem that's roughly at my level. This is the one that balances effort and reward: it challenges me enough to grow and also contributes meaningfully to the community. Ultimately, we are judged on our output—not just papers, but also talks, ideas, and how we reuse knowledge. Writing ten easy papers isn't going to get you anywhere. You need something at the right level.

And third, it's crucial to have problems that seem too hard—problems that you dream about solving, even if they seem out of reach. In my experience, the most difficult problems I've tackled were ones I initially thought I couldn't solve. If I had only worked on problems that felt achievable, I'd never have attempted them. By balancing the too-easy, right-level, and too-hard problems, the very difficult ones often become approachable over time.

I've tried focusing entirely on a single problem once, and it was frustrating. I felt my energy wasn't being used effectively. Having multiple problems prevents that kind of

burnout.

Another key point: mathematics is hard, and it's normal not to understand everything right away. Success comes only if you are truly passionate about the area you're working in. Don't chase what looks shiny or glamorous; follow what excites you. That passion is the only way you can accept, and work through, periods of confusion and difficulty.

Finally, choose your advisor for human reasons, not just academic prestige. The relationship with your PhD advisor—or with your students, if you're supervising—is crucial. We learn far more by observing and mimicking behaviour than by following instructions. A strong human connection allows for that kind of learning. Thurston expressed this even better than I can, but the essence is that direct, personal interaction matters more than formal guidance.

So when picking an advisor, look for someone you feel a genuine connection with. The same goes for students: one of my main concerns when taking on a PhD student is ensuring there's good rapport. Without it, the work will suffer for both sides.

So, last question: have you ever found any real-world application for any of your results?

Honestly, I'm not really interested in the applications of my results, even in areas where I could have some. For example, percolation theory does have applications in other fields. In principle, if you impact theoretical physics, it can ripple into applied physics and engineering. But that has never been my motivation.

I don't even aim for problems with potential applications. I accepted that early in my career. My drive comes from aesthetic purposes—a kind of pure mathematical coherence and harmony. I think it's important that some people are motivated this way, because it complements those who are driven by applications. You don't have to be a superhero, able to produce highly original mathematical ideas and simultaneously find their practical applications. Society benefits from people with different strengths. My strength is trying to produce ideas.

Very often, I work on a problem and get an idea that doesn't quite fit. Because I'm driven by the idea itself, not just the problem, I'll explore whether the idea can work elsewhere. If it resonates with me, I'll try to find another problem for which it's useful. My career is basically built around that approach.

For example, early in my career, with my PhD advisor, I solved a problem proving that the number of self-avoiding walks on the hexagonal lattice grows at a specific rate—a very elegant result. It's a readable and satisfying paper even if you're not into probability. People often asked us, "Where did that original idea come from?"

Here's the story: Stas, my advisor, had an idea in a completely different context. We tried to apply it to self-avoiding walks, and it didn't work. Later, I realised a variant of the idea could be applied to a percolation problem, so we pursued it there. It evolved, leading to two papers in that area—but still didn't work for self-avoiding walks. Eventually, the idea stalled in percolation; six months of effort went nowhere.

Then one day, I realised that version 2.0 of the idea actually fit perfectly back in the original problem—self-avoiding walks. And in two minutes, I had the full proof. Zero doubt. It was one of those rare moments where you see how ideas can travel, evolve, and eventually land in exactly the right place.