Pittsburgh Interdisciplinary Mathematics Review **3** (2025), 102–116

An interview with Professor James Maynard

Leonardo Finzi, Nina McCambridge, and Lark Song

Biographical sketch

James Maynard is a mathematician known for his influential work in analytic number theory. Born in 1987, he earned his bachelor's and master's at Queen's College, Cambridge, and his DPhil from Balliol College, Oxford in 2013, advised by Roger Heath-Brown. He gained international recognition for his 2013 breakthrough on small gaps between prime numbers, introducing new methods that significantly advanced and simplified earlier results. He proved the Duffin–Schaeffer conjecture and, more recently, improved Riemann zeta function zero density estimates with collaborators. Among his many honors are the 2014 SASTRA Ramanujan Prize, 2022 Fields Medal, and a 2023 election as a Fellow of the Royal Society.

Leonardo Finzi/Nina McCambridge/Lark Song: *To start off with, what questions in math — or what kinds of math — were you interested in as a child?*

James Maynard: I think I was interested in lots of the math questions I could understand at the time, just lots of the basic patterns of numbers and things. I would not quite say that would have to be number theory problems, but I really liked looking at patterns in numbers and multiplication tables, and simple questions asking about that. I always found that very interesting: looking at things like, "How often does a number come up in a multiplication table?" — simple things like that.

I started getting connected to cool questions, number theory — even basic things. I really liked the way that a math problem at high school or something had one answer, but there were many different ways of getting that answer. And so you could look at the problem from multiple different viewpoints, and all of those viewpoints would feel like rather different ways of thinking about the problem, but they would all arrive at exactly the same answer. I always found that super compelling. There were lots of different paths, but they all led to the same place.

LF/NM/LS: *It sounds like it was always natural for you to go into number theory.*

JM: Yeah. Maybe it never felt like that as I was going through it at the time. Looking back? Yes, totally. I think if you looked at how I went through and the sorts of things I was interested in, I was always interested in number theory. I sort of learned for myself about number theory before it was ever formally taught. I remember when I applied to Cambridge University, they asked you to put down a couple of your favorite math subjects, and normally you just pick a couple of subjects that you are best at in your high school exams and things. But I put down number theory even though I had never taken any, which is potentially a bold move, but it was just because I found it super compelling because I had read about it. I was slightly dreading that they would ask me some technical question about it, because I had never learned number theory before, but I put it down nevertheless because I was super interested in it.

From an outside perspective, it looked like I was always interested in number theory, but as I was going through it, I think I did not have any grand vision of where any of my choices would lead to, necessarily. I am not sure I even really knew that being a mathematician was a job.

For me, it did not feel like, "Oh, I was always born to do number theory." It was just always something that I thought was cool. And it just so happens that this thing I thought was cool when I was a high school kid was something I could actually turn into a job. I still sometimes find it crazy that people will pay me to play around with problems that I think are cool — since I was a high school kid.

LF/NM/LS: *Is there a branch of math that makes you want to close the textbook and walk away?*

JM: I have a love-hate relationship with lots of areas of applied math. I actually think that some perspectives, some statistical physics, some numerical analysis and things like that, can be super important to incorporate into pure mathematics. I think that has been a feature of various bits of my work: that actually, it has taken some basic principles from these very applied areas of math and used them in pure math contexts.

But at the same time, I feel like I intrinsically think like a pure mathematician, and so I struggle a lot with the lack of formal structure and things, as I think lots of pure mathematicians do. I feel that this is unfortunate, that there is this slight divide at Oxford.

I taught an undergraduate class on the calculus of variations, which is normally an applied math course, but it is a topic I really love, and I think it is super cool. But I just could not teach it in an applied math style, so I completely rewrote the lecture notes — formal proofs, statements — which covered all the same topics, but I had to teach it in my own way, because even though I thought the ideas were super beautiful, I had to think and teach it like a pure mathematician.

I definitely have a love-hate relationship with applied math. There are definitely some times when I read an applied math textbook and I get super frustrated, and I say, "Just write down a specific statement of what you really mean! Be concrete!" But at the same time, there are often beautiful ideas — they just need a little bit of processing to sort of bring that out.

LF/NM/LS: *You talked about closing the textbook, but we can talk about the things that you would open the textbook for as well. How do you choose the topics you work on when you are doing research?*

JM: The main thing is that I want whatever research I work on to be in some way connected to some big problem that I really care about. That is the main way that I find myself motivated about the problems that I think about, and the main way that I try to make sure that the research that I do is interesting and worthwhile from my point of view.

I tend to take some big problem that I think is really cool and interesting, no ideas about how to solve that, but I will read up a bit on what sort of partial progress people have made, and try to come up with super simplified toy problems that isolate some of the difficulties, to try to hopefully overcome one of those specific technical difficulties, to make some kind of baby steps in the direction of this big problem. I am not necessarily expecting to make big progress, but provided it is in some way connected, if you squint hard enough, big problem — that is how I like to choose my research.

LF/NM/LS: *Do you tend to want to work alone or with other people, either at Oxford or at whatever institution?*

JM: It is a big mix. I learn a huge amount, often from collaborations. Particularly, one thing that I never appreciated when I was younger was how much of a human subject and process math is, and how much personality actually influences things.

Sometimes, when you have the right kind of collaborator, working in a super complementary way to one another can be great. There have certainly been problems where my collaborator and I just think in completely different ways about a problem, and this is great because I will kind of make some progress, then get completely stuck on one thing, see no way of making any further progress, and my collaborator will just think in a completely different way. That, to me, seems like lunacy, but somehow that will be precisely what is needed to overcome the point where I got stuck.

Often, the best way of learning a new field is to work on a concrete problem with someone who is more of an expert in that field, because they can show you how the basic principles that you have learned about in a textbook can really be thought about and used in a concrete problem.

I feel collaborations can be super beneficial in those ways. But nevertheless, when it comes to me doing my creative inputs, either for a project that I am working on my own on, or for a collaboration, I tend to feel that I need to sit down at a desk and really think things through.

When we work on our collaboration, we will often bounce ideas off one another, we will think things through a bit, but then I will say, "Okay, now I need to go off to my desk, and I need to sit and bang my head against this problem, to really think on my own for it, and then when I have something to show for it, I will come back to you and talk about what bits I thought I could make progress on and what bits I got stuck on." It is very much a balance of collaborations and solo work.

I think that in terms of my published papers, it is almost 50/50 between papers that I have that are solo published and papers that are collaborations. I think that is kind of representative.

LF/NM/LS: *Now to move away from your start in math and more into your career thus far and the things you have seen that emerge from your work: what are some recent implications of papers you have published that you really like and/or that you find really interesting?*

JM: One thing that I very much like is this paper I had on small gap twin primes, primes that come close together, and I was very proud of this result, but I was more proud of the kind of ideas that went into it. One thing that gives me a lot of personal satisfaction is that lots of other mathematicians have taken this new technique that I developed and applied it in lots of other contexts. Those ideas and that kind of motivation have led to various other results by people. That is a really good feeling, when you feel that your work has been absorbed by the community, and then there is a whole generation of often younger people who are taking your ideas and driving them further, and using your ideas to make advances in lots of other fields.

Similarly, one cool thing was that I had this paper about primes with missing digits. There were some other people who were interested in a — at first completely different — problem about whether random polynomials are irreducible or not. They realized that there were some similarities between the sorts of ideas that they were thinking about and some of the ideas that I was thinking about. I think my work was a useful motivation for them to think about these problems further, and they have now done super cool work that seemed completely out of reach beforehand. Maybe it is not using my work so much, but the fact that my work was a slight motivator for them to think more about some of these ideas is a really cool feeling. They have made big advances, and to whatever extent my work had some influence on that, that is really cool — that there are advances in completely different fields that are motivated by some of my similar ideas.

LF/NM/LS: *What are some results in number theory or mathematics you generally believe deserve more attention?*

JM: I think there was a lot of harmonic analysis that was done quite a long time ago that has now been largely forgotten, and a lot of this was very high-quality work. It was done by very strong people. But somehow, lots of it has fallen slightly out of fashion, and I feel that even if the fundamental problems they were working on are now not so much of current interest, lots of the sorts of techniques and things that they developed, I am sure, have uses elsewhere. It is unfortunate that they have been forgotten to quite the extent that they have.

Similarly, I feel that there are some ideas that have been very important in certain fields, but have not percolated across to other fields where they will surely have uses. In my own field of analytic number theory, I think there are a lot of possibilities for incorporating more sophisticated ideas coming from probability theory into some of the standard toolkit of analytic number theory. Somehow, it is very common to use very basic concepts from probability theory, and there are good reasons why some of the more sophisticated techniques from probability theory do not translate so easily.

But I nevertheless feel that there are ideas — particularly connected to probability theory and, more recently, computer science — which surely should find analogs in analytic number theory and should be able to produce lots of exciting new solutions. I think that is certainly an area with growth potential. It is not that those original ideas are necessarily underappreciated in the grand scheme of mathematics, but they may be underappreciated in terms of their potential applications within number theory.

LF/NM/LS: *How much did the applications of analytic number theory motivate you? It sounds like your main motivation is the structure of prime numbers themselves. How much do you care about the applications, especially cryptography?*

JM: For me, it is definitely the case that my primary motivation is the inherent mathematical interest. I view prime numbers as inherently fundamental mathematical objects, and that is why, at the end of the day, I really care about them. It is nice that they have uses and applications outside of number theory. It definitely is an easy way to demonstrate their importance — that prime numbers show up in all kinds of different areas of math and physics. Cryptography is the obvious example of applications of prime numbers. But for me, that is not really the main motivation. It is definitely a nice add-on, but it is secondary to the fact that I just view these as fundamental objects in pure math.

LF/NM/LS: *Do you ever find the applications to cryptography concerning from a security perspective? I do not know if that is something you ever think about.*

JM: Most of the things that I think about, at least, tend to be on the line where we have very good guesses as to what we think should be true, and the difficulty is just proving that what we think should be true is, in fact, true. The only security implications would really arise if somehow our guesses about what we think is true were wrong and there were some conspiracy going on, and we actually found numbers that behave in a way that is rather different from how we expect them to behave. I think that is super unlikely, and so in that sense, I feel that my bread-and-butter research is much more in the direction of reassuring cryptographers that the things internet security is based on are generally well founded, rather than threatening them that I am suddenly going to come along with something that is going to break cryptography.

LF/NM/LS: *Here comes the AI question. This is Pittsburgh, home to pioneers in formal proof and automated reasoning including Thomas Hales and Jeremy Avigad. What is your outlook on future advancements in mathematical methods or tools, such as AI or the Lean proof assistant?*

JM: Over a moderate time frame — say in the next 10 to 20 years — I imagine AI will have growing importance, particularly as a proof assistant for mathematics. Often, when high-level researchers talk to one another, they are really not talking on the level of rigorous formal proofs at all, but they are talking on a much higher, more heuristic level. And lots of the proof is trying to work out stepping stones on a very high-level heuristic and pretending that complicated objects are actually simple — and you are lying to try and simplify the problem so it can fit in your brain, and then think about how these things can work.

Then once you have a high-level proof, there is a technical exercise of turning this into a real mathematical thing. And sometimes you realize that the sort of intuition you had is slightly wrong — things do not work out quite the way you hoped they would. But normally, when high-level mathematicians really believe something should work on a high level, it is just a technical exercise to turn this into a formal mathematical proof — but often a very tedious one.

And that is a process which you could easily imagine AI being very good at helping to automate. Often the sort of moral argument you come up with initially is essentially true, but there are always exceptions, and you need to show that the exception is not too bad, or that they are not too common, or that they are fairly exceptional in some way. And there are lots of extra additional technical details that are fundamentally routine, but still take a long time to go through. Automating some of those routine details has a huge potential benefit, as well as recognizing when problems that you encounter are routine in a slightly different language.

One problem that math has is that often the same sorts of problems are encountered in several different fields, but every field has its own field-specific language, so people do not realize that the problem they are studying — only if you kind of squint a bit and call all the objects the same — is actually a problem that has been studied by ten different fields already, each of which has come up with its own sort of solution and can do different things. Sometimes I encounter a problem, and I know that people have studied this, but I do not know the right buzzwords for what people actually call the object. I think AI has lots of potential for easing these sorts of translational difficulties. I can tell AI, "Hey look, I am dealing with these objects, I know people have studied them beforehand — what is the buzzword that I need to use?"

I can suddenly tap into the literature on this, where people have done all the hard work for me beforehand. Similarly, lots of mathematicians are fundamentally realizing that a problem in one area has been solved already in another area, and so you can just import the techniques of this different area into this new area. And AI, I feel, has a big potential for just realizing that once you have a little bit of a dictionary, these are more or less the same problem.

Those sorts of connections, I think, offer lots of potential for AI. At least over a 10 to 20 year time frame, I am a bit more skeptical as to whether AI is really going to be doing the high-level, creative side of math. But I view AI as much more of a proof assistant — it has lots of potential for helping mathematicians focus on what the really important creative mathematical ideas are, and that means they do not have to spend so much time worrying about tedious technical details that are not really where the big work is, but can often take up a huge amount of time to write up in the right way.

LF/NM/LS: *Kevin Buzzard is currently leading an open-source initiative formally proving Fermat's Last Theorem using Lean. Are there any problems you have solved that would be suitable or doable for graduate students working in formalization to formally prove?*

JM: In many ways, many of the results that I have proven are closer to first-principle proofs than Fermat's Last Theorem, which very much built upon many layers of architecture that mathematicians have developed on top of one another for several generations. In that sense, a proof from first principles of most of my results would probably be a small undertaking in principle, compared to Fermat's Last Theorem.

My understanding at the moment is that the way Lean works is slightly better suited to certain aspects of mathematics than others. More algebraic aspects, for example, often allow quite clean black-box proofs that plug and play with one another, and this fits very nicely. In slightly more analytic areas of the subject, you have these common themes, but often there are slightly messy estimates, and we are not very good in analytic areas at coming up with clean black-box statements that you can just use off the bat.

Often you have to slightly reinvent the wheel each time, because you need a technical variation for each statement, which correspondingly means that, from my understanding, the Lean library is not so advanced on the more analytic side of things. You often need lots of parameters, and every time I try to prove a general theorem that I think should apply to all contexts I can think of, I get an email six months later from someone saying, "I need a version of your theorem, but with these slight things changed. I think that a small modification of the proof should work, and is that the case?" And I say yes, it is. I completely failed, therefore, in my attempt to have a general theorem.

I think it will be easier in the sense of not having quite such large architectural structures building upon one another, but it would maybe be a bit more painful in the sense that you need to do all these analytic arguments, and it is not totally clear what the right, general way of doing them is that will make them useful for projects.

Ideally, and the purpose of Kevin's project, is not just to formalize Fermat's Last Theorem, but to have a library where you can use all the intermediate ideas as well, as part of the whole Lean set-up. Whether it is a worthwhile thing or not, there is a little bit of a chicken-and-egg situation here, and at the moment, it is still the case that formalizing a proof in Lean requires quite a lot of work hours, and it is a lot harder to formalize some proof in Lean than it is to just write up the mathematical paper. This is why most mathematicians are not writing up their papers in Lean when they are producing new results.

If no one is doing anything, then obviously in that situation it is not going to progress — unless you suddenly have some big AI advance that is much better at turning sketchy proofs into formal Lean code. So you need a little bit of both. I do not think there is anything in particular making any of the results that I have proved impossible to do, other than the slight difficulty of how you formalize an analytic argument without having to reinvent the wheel every single time.

LF/NM/LS: *Very insightful. Staying on your big picture of math and going into analytic number theory specifically, do you believe that proving something like the twin prime conjecture or advancing the first Hardy–Littlewood conjecture, if that is possible in the near future or the far future, would be the beginning of something in analytic number theory? Or do you think that would lead toward the end?*

JM: The currency in math is always ideas. We often motivate things and frame things in terms of, "I am trying to solve this concrete problem." But at the end of the day, people do not care so much about whether the problem is just true or not true. They care about why it is true or not, and they care about the proof. The excitement when someone proves a big result is slightly less now that we have a formal verification of something that we always believed to be true and that is indeed true — but it does have value.

Normally the excitement is really this: to be able to prove something, someone has kind of understood what is going on behind the scenes to a much greater extent than they were able to before. This will give us much better knowledge and understanding that will often have much wider implications than just the result itself. And so I would imagine that any proof of one of these big results, like the twin prime conjecture — whatever method you use to prove it — would open up a whole new avenue of techniques that would have a huge number of different consequences.

Somehow, you would be able to overcome these technical difficulties that exist in many different sets of problems and combinations. If you merely had some sort of huge, long, technical proof that I did not understand at all, showing that the twin prime conjecture is true, I would not, at the end of the day, really care much at all. My feeling is that at least if someone does come up with it, they will come up with fundamental ideas that can be distilled into simple principles and that have all kinds of other implications — and that will advance the theory, possibly.

LF/NM/LS: *Thinking about your attitude towards math and math research, do you think math is discovered or invented?*

JM: Yeah, the fundamentals of math, I think, are definitely discovered. To me, I could just about imagine universes where the laws of physics are different or something. I could just about imagine the idea that everything I see and touch is somehow a simulation and I am being tricked into thinking those things. But I cannot possibly comprehend a world where 1 plus 1 is not equal to 2, or where prime numbers are not prime numbers in some way. So to me, somehow, the fundamentals of math are almost more real than the universe itself, and they are just super fundamental to nature and the universe itself.

Because of this, prime numbers exist regardless of whether humans exist or not. If you go back to the time of the dinosaurs — the dinosaurs, T. rexes, might have had no concept of what a prime number is. But that does not mean that the Riemann Hypothesis is not true, of course.

The Riemann Hypothesis is clearly true, of course, regardless of whether T. rexes know what a prime number is. So to me, maybe you could argue that a lot of the techniques of mathematical proof are invented by mathematicians. But the fundamental objects themselves are just inherent to the universe.

LF/NM/LS: *Now we are going to ask some more personal life questions rather than philosophical questions. I mean, you were basically talking about this at the beginning — but when did you know that you wanted to be a mathematician?*

JM: I think it was only maybe midway through grad school that I really thought, "Hey, this is what I want to do," properly. I was not necessarily convinced that I was good enough or able to do it. But I think that was the first time that I really thought, this is what I can see as a career.

I went into grad school because I enjoyed math and math research, but I was still not super settled, I think, on where I saw myself going. I was happy to just take things one step at a time, but I think it was midway through grad school that I was like — this is really cool, I want to make a career in this.

And I at least tried to be a mathematician.

LF/NM/LS: *And what qualities have you found are most important for a mathematician — like personality traits? Have you seen any unexpected trends among mathematicians?*

JM: In terms of personality traits, I think perseverance and resilience are super important. The nature of math research is that you will go six months without really making much progress on the problem, and you have to just be completely okay with that.

That requires a certain kind of personality. It is unfortunate that there are some people who, I think, in principle could make great research mathematicians in terms of their creativity and their ability to do things, but you need to have the right sort of psychological mindset to be able to deal with the highs and lows of the idea of math research.

If you are not struggling and not understanding things, you are not really working on interesting problems, so it definitely requires a certain type of person to just embrace that uncertainty of not knowing when the next result will come, but to enjoy it nevertheless.

There are lots of things you can do to help yourself. Being fun and interested and passionate about what you are doing is a big way of doing that, as well as having ways of taking a break from it. Maybe that is one unusual thing I find about mathematicians — that mathematicians often, I feel, have very niche, specific outside interests, where they have some often quite peculiar thing that they are amazingly good at.

LF/NM/LS: *I found that to be true, as well.*

JM: I think mathematicians are often quite obsessive. You need to be if you are a research mathematician — obsess about your problem. And I certainly find that it is not only in math where I am obsessive. I am very bad at being only slightly interested in something. I have to either not really care about what I am doing, or I have to be obsessively interested in it.

You look at my colleagues — there are people who just have these sorts of passion projects that they are super passionate about, that have got nothing to do with math. It is their way of breaking up the math, but they are often amazing at whatever really specific other interest they have.

LF/NM/LS: *What are some examples of cool passion projects that you have found inside?*

JM: One of my colleagues has two passion projects. He does hang gliding very seriously and does that very frequently. And he also breeds butterflies.

LF/NM/LS: *Oh, that is an interesting one!*

JM: He loves flying in general. He is a sort of expert in lots of different planes and things like that. These are things that you would not expect at all from just meeting him. He seems like a pretty typical mathematician. You would not guess it talking to him.

Other colleagues are super talented at musical instruments or something. It is not uncommon for people who have the potential to be professional musicians to decide that math is going to be something they actually do instead. It can be a nice thing to go to a conference and find that the conference has a piano or something, and then there is someone who is essentially a professional-level pianist who will suddenly just perform really well.

LF/NM/LS: *That is amazing!*

JM: Yeah, so things like that are pretty cool.

LF/NM/LS: *You have done a vast amount of research and published a lot of papers. You did a lot of different research on a lot of different questions. Is there a favorite moment in any kind of research that you did?*

JM: I think one pretty special moment for me was when I was a first-year postdoc in Montreal, and I realized that the ideas I had been thinking about, concerning small gaps between prime numbers, looked like they would actually work out. I think that was the first time I had proven a result that I had the feeling the rest of the field would particularly care about.

So it was the first thing that really proved to me, as much as anything else, that I maybe had the talent to really do research properly at a high level. And so I think that is a bit special to me, because that was the first time I really started thinking, "Hey look, I can do something that other people cannot do."

LF/NM/LS: *Which of your published papers are you most proud of?*

JM: The Small Gaps paper is somehow special to me because that was the first time I felt like I was doing something that other people cared about. And there have been various results since then that I am also very pleased and happy with. But somehow that first hit feels like it is a big expression for me.

LF/NM/LS: *When you take time off, what is your day like?*

JM: Right now my life is quite busy because I have got two young kids. They keep me pretty busy outside of work time — and sometimes during work time as well.

LF/NM/LS: *How old are they?*

JM: The younger one is 11 months old, and the older one is two and three-quarters. That is super fulfilling and a lot of fun and very rewarding, but it definitely takes up a lot of time. So that is a big part of my life these days.

Outside of that, I like photography. I like being kind of geeky and pretentious about coffee. I would like one now. Often when I travel — for example, one of the big perks of being a mathematician is that you get to go to all these cool different places — typically on my itinerary is to check out modern art museums in the places I am going to, and to go around with my camera and take photos of where I am visiting. It is a way of trying to engage with all these cool places I get to see, and to balance between different sorts of overpriced coffee.

LF/NM/LS: *Do you have a favorite artist?*

JM: Not necessarily an individual favourite artist, but I'm particularly fond of lots of the early to mid-20th century art movements. Maybe starting with the Surrealists and moving into Abstract Expressionism, and things like that from that sort of period, I find lots of the pieces of art just really good. They have, I think, a really nice balance between being abstract but also very concretely speaking to me in some way.

LF/NM/LS: *Speaking of photography, is there a public place where you regularly upload your photos?*

JM: No, for me photography is somehow a very personal activity. It is slightly less about what photos I do or do not take, and much more about the process of taking photos. As I said, I get to travel to all these fun different places, but definitely for a while I felt it was a little bit of a shame that, when I am a tourist on my own, I felt like I was not really engaging with the places I was visiting. And so I was not seeing them properly in being there. Photography for me is very much a way of experiencing the cities and the places that I am conscious I am going to. I try to be creative, and I pick out some of my favourite photos at home. But mainly, it's the process of photography that is engaging with my environment that I enjoy.

LF/NM/LS: *What would you have done if you had not become a mathematician? Might you have become a photographer?*

JM: I do not think I have the talent to be a photographer. If I look at some of the really great photographers, they just have this creative eye that I think I lack. It is difficult to know — there are clearly math-related fields that I could have gone into potentially. I did an internship at an investment bank when I was trying to work out things to do. I did some sort of math-related work of a sort for the UK government.

LF/NM/LS: *Was that related to cryptography?*

JM: Some were related to cryptography and things like that. Clearly, there has been a huge boom in the tech sector with the rise of AI and related developments that are likely to shape the world in lots of different ways. I think there is a huge role for mathematically minded people to play in trying to understand and work things out. That's another closely related field that I could imagine a different version of myself being interested in and going into.

LF/NM/LS: *If you had gone into AI, do you think you would have gone into more development or safety?*

JM: I think I fundamentally have a researcher's mindset. Ever since I was a little kid, I always completely wanted to understand how things work. I always wanted to take things apart, and it was that understanding process that was always the important thing for me. I only have a limited understanding of how AI and lots of these big platforms work, but I think I imagine myself as being very much on the theoretical end of things, and I would not have liked just the tuning-parameters aspect of AI, but rather the effort to step back and get a bigger-picture idea of what architectures work for what sorts of problems, and why we should expect these sorts of architectures to be well suited to certain problems and not well suited to others. Why, with certain presentations of data, AI can solve and make big advances, but if you just present the same data in slightly different ways, it cannot. There are some fundamental features of the nature of what we are feeding into the AI systems that are, in some sense, specific.

When AI is processing a photo, it is very important that this is a photo of something in the real world, rather than some random collection of color pixels. The fact that it looks like it relates to something in the real world is something that the AI is very much picking up on. Something is either a picture of a cat or it is not a picture of a cat — it is not some weird, hybrid cat-like thing where half the pixels are cat-like and half the pixels are not cat-like. So it would be that theoretical understanding — which I think exists both on the safety side of things and on the development side — that would be the key for me. I could imagine myself working behind all the safety design of things, but I would have very much been trying to reverse-engineer what is really going on, why it is working the way it is, and why it is not working when it does not work.

LF/NM/LS: *If you were to start your career over again, as a mathematician, would you have done anything different mathematically or career-wise?*

JM: I am not sure. One thing was that I was quite naive going through, and that I did not look very far ahead. I did not have a clear career projection of where I imagined myself ending up. I did not have an appreciation of how competitive the job market and related things after grad school can be. In that sense, I lucked out — fortunately, I proved results that people cared about at just the right time, which meant that I was set up and was going to do fine on the job market and related opportunities.

But I think, at least looking backwards, I was pretty naive compared to lots of the students nowadays. I am not sure if that was a good thing or a bad thing, but I think in some ways it helped me because I focused on the math and I did not get too distracted by worrying about whether I would get a job or not. That is clearly good from the point of view of proving theorems, but equally, there is a danger of survivor's bias here. Maybe I got lucky in some way, and 90% or 99% of people who take my approach face stark difficulties at some point. But you only interview the 1%, right? So that is one thing where I am not sure whether I took the right approach or not. I think about this quite a lot with my own graduate students — trying to get that right balance between not worrying too much about the practicalities of what results you need to get and what kind of job, and making sure they are fundamentally focused on the research and research statements, versus the practicalities of getting a job, what you need to do, what the important steps are. It is important to have a bit of both.

LF/NM/LS: *Have you noticed any differences in mathematical culture across institutions?*

JM: I think there are huge differences in the kind of mathematical culture, in terms of how mathematicians are encouraged to interact, present things, and talk about ideas. This is certainly why I very much encourage my students, as they are progressing in math and in their careers, to try and experience a variety of mathematical institutions. Some styles people love, and some styles people hate. You have to work out what works for you, but I think it has been very beneficial for me to experience different institutions and, in particular, different mathematicians. I am always shocked at how different mathematicians are in terms of how they think about problems and what their intuition is for different things. This diversity of perspectives on the same problem is incredibly valuable. It often means that when people change institutions, there is going to be a little bit of a culture shock. But the important commonalities and these different experiences are very, very valuable for getting a broader idea of how to think about math, how math works, what are good ways to communicate with other mathematicians, and — as part of this overall social endeavor — what are less effective ways of communicating with one another.

In March 2025, Maynard delivered the inaugural Pittsburgh Mathematical Horizons Lecture and presented his work at a joint colloquium by Carnegie Mellon University and the University of Pittsburgh.

Editors of the Pittsburgh Interdisciplinary Mathematics Review
E-mail: pimr.editors@pitt.edu