The Magical Quality of Number Theory: A Conversation with John Pardon

Interview by Simon Donaldson
Edited by Maria Guetter and Keating Zelenke

In September 2022, the Simons Center welcomed John Pardon, SCGP's fifth permanent faculty member. He joins the Center from Princeton University where he was a professor of mathematics since 2016. In November, Pardon met with fellow Simons Center faculty member Simon Donaldson for a discussion.

Photo: Joshua Klein

Simon Donaldson: First, it's a real pleasure, and an excitement for me to welcome you to the Simons Center as our new permanent member, and to have this chance to talk to you in this interview.

I thought I'd begin by… Well, one of the remarkable features of your career is that you began publishing research papers at a very young age. I think, in fact, maybe your first paper went back to a high school project?

John Pardon: Yes, well, my dad suggested I do a research project to submit to the Intel Science Talent Search. I wasn't really sure what I wanted to work on. I spent the summer thinking about various possible projects…

SD: What age were you at this point?

JP: It was the summer before my senior year of high school, which was 2006, so I would have been seventeen. I was always fascinated by number theory, but part of the reason for that 'magical' quality was that I never really understood the 'why' behind any of it. I find it easier to think visually and spatially, so geometry and topology come much more naturally to me, and I eventually settled on a very concrete geometry question. This was something I could actually make progress on.

JP: I read lots of math articles I found via random online searches. One of these was an open problems list compiled by Mohammed Ghomi, a Professor at Georgia Tech. This was the source of the problem that I worked on.

The problem asks, given a closed curve in the plane, can it be convexified in a motion during which every pair of points always move farther apart?

SD: It's a very appealing geometric problem.

JP: I can summarize all I really did in just a single sentence, which is to say that the result was known already for polygons by work of Connelly, Demaine, and Rote — you approximate your curve by polygons, and the space of expansive motion is compact, so you can take a limit.

SD: That was a big achievement for a seventeen-year-old to make that advance. Were you sure at that time that you were going to become a professional research mathematician?

JP: Not really. In fact, in high school, I was very interested in algorithms and computer programming. I went to the International Olympiad in Informatics. No one in the U.S. knows what 'informatics' means, but it means computer science, computer programming.

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I never was able to make the U.S. IMO [International Mathematic Olympiad] team. I wasn't particularly close either. Going into undergrad, I thought I'd probably do one of the two — either math or computer science.

I also, at the time, played cello pretty seriously. I've sort of dropped off practicing as much as I used to. But that wasn't really a career goal for me, just personal enjoyment.

SD: While you were at Princeton as undergraduate you also worked on quite a number of different problems and produced papers, for example, on group actions on manifolds, and random polygons and quite a range of other topics. Do you have a favorite paper from that period?

JP: Probably the last one about the distortion of knots, if you can call that the last paper of that period. It's actually sort of a funny story—I'm not sure when I started thinking about the problem—but I also first read about it on the list of open problems in geometry by Mohammed Ghomi. I hadn't been thinking about it for four years, but I'd known about it for four years. In the spring of junior year, there was a two-week period where I thought I'd solved it.

I had just finished taking two 3-manifold topology classes with Zoltán Szabó and Dave Gabai and those are exactly the techniques which are relevant [for this problem].

I thought I'd solved it, I was writing this very short argument, and eventually I realized I was making one of the standard mistakes in 3-manifold topology. I don't quite remember now what it was.

SD: Could you describe in a few words roughly what this problem was about? This distortion problem?

JP: Yes. If you have a closed loop in three-space, there are many ways to prove that it's knotted, many invariants one can calculate: the fundamental group of the complement, its representations or knot polynomials, or various other more sophisticated invariants.

There are also geometric measures of complexity. For example, there's something called the rope-length. It's a mathematical formalization of the following simple idea: if you want to tie a knot with a rope, how long of a rope do you need?

A geometric complexity measure like the rope-length is quite strong in the sense that it's pretty easy to show that, no matter how long your rope is, there are only a finite number of knots you can tie with it.

Now Gromov asked a question about a very weak measure of the geometric complexity of a knot. This measure is called the distortion. It's about comparing the arc length on the knot with the straight-line distance in the ambient Euclidean space.

The distance along the knot between a pair of points on it is, of course, always at least as long as the straight-line distance. The maximum ratio of those two quantities, over all pairs of points on the knot, is called the distortion.

JP: Gromov proved a very beautiful theorem, which says that for any circle embedded into Euclidean space, the distortion is always at least the distortion of the standard round circle, with equality only for the round circle.

Now, it's hard to get other lower bounds on the distortion, and it's basically because it's scale invariant—you can take any sort of knot you like and then scale it down, the distortion is the same, and then implant it into any other knot. And you can do this infinitely many times, with infinitely many knot types, and create some sort of very complicated fractal pattern. And the distortion doesn't go to infinity when you do this—it's roughly just the maximum (not the sum) of the distortions of the constituent knots.

SD: So, there's no control of the length of the knots in this discussion?

JP: Yeah. Well, I mean, you could, but it wouldn't give you anything because you can just scale it down.

SD: You could just scale it, yes.

JP: Gromov's question was to show that there are knots which require arbitrarily large distortion to realize. I had sort of given up on this problem, but I had an idea in the summer, while walking in Royal Victoria Park in Bath, UK.

It took four or five months to figure out what the final bound on the distortion would actually be, and to write everything up carefully.

SD: That's a great result. It appeared in one of the top journals.

Your main research interest, since your PhD, has been in symplectic topology. What attracted you to that field initially when you went to Stanford?

JP: I would say that I'm not attracted to fields, I'm attracted to problems. It's sort of a random coincidence really. I was not being a very good first year graduate student. I was not talking to professors very much at all. I was thinking about a question about counting holomorphic disks in the symmetric product of a Riemann surface, as in Heegaard Floer homology, and emailed my future advisor, Yakov Eliashberg, to meet and discuss it.

So, we met, and I asked my question, which was very specialized and probably hopeless anyway. But, he told me some wonderful things about symplectic geometry and mentioned a few problems, and I think I probably then worked on something completely different than what he wanted me to work on.

SD: One of your best-known achievements goes back to your thesis of the construction of the virtual fundamental class in holomorphic curve theory. I always wonder, was it your background in topology which somehow gave you the new idea—the new input there? Tell us about that.

JP: Well, I should say there is a large body of work on this problem. In particular, work by our colleague Kenji Fukaya and collaborators.

This is a problem in which a great many ideas have already been proposed, by many different people, and shown to work in certain settings. Putting everything together in a logically compatible way which treats the problem in general, is somehow the most difficult part. This situation is somewhat rare in mathematics—usually it's the elemental ingredients which are most important.

This really contrasts with the knot distortion problem we discussed earlier, where there was one simple idea which could be worked out in a matter of a half an hour and shown to give a nontrivial result. Even though it took me another few months of work to write everything out precisely, you could summarize the essential content very quickly. So, you can convince someone you have solved the problem in a short amount of time.

The situation with virtual fundamental cycles is quite the opposite. I would almost say there's really not one key idea at all, nor is it surprising that a construction exists. Doing the construction is sort of like being an architect. The difficult part is to make everything work together correctly.

SD: Maybe perhaps you could tell us something about your current projects… I know they're quite technical, but is there some way you could say some words about them?

JP: My most recent project is the work with Vivek Shende and Sheel Ganatra about wrapped Fukaya categories.

SD: But it also involves — as [far] as I understand it, which is not that very far — it involves building a big edifice, a big project. Is that true?

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JP: Yes. Well, all papers in this area are quite long… What we prove is a local to global principle for the Fukaya category. The first invariants one learns in algebraic topology are defined by cutting up your space into pieces, doing some algebra with those pieces, and then re-assembling to get, say, the homology.

The Fukaya category of a symplectic manifold is a much more global invariant because it is defined from Lagrangian submanifolds and holomorphic disks, both of which can travel anywhere in the manifold. We showed, however, that in some specific settings, the Fukaya category can be recovered from local information glued together algebraically.

SD: This may not be a fair question to ask because you've only been with us at Stony Brook for a couple of months, but do you have some ideas or vision for the development of the Simons Center and how your work will fit in in the future? Or the particular things you hope to see developed?

JP: I'm looking forward to interactions between the two fields. I've always been fascinated by quantum invariants of 3-manifolds arising from Witten's path integral reformulation of the Jones polynomial based on the Chern-Simons functional. I've not been able to prove anything about it ever, but maybe someday (and this is certainly the right place to work on Chern-Simons theory!).

SD: You're intrigued about whether one could make that into a rigorous mathematical formulation, is that what you are saying? The path integral?

JP: That's certainly one very interesting problem. But there are other problems such as giving rigorous proofs of things which you can derive, by assuming some properties of path integrals, but for which there's no mathematical proof. So that might go by rigorizing the path integrals, or maybe not. Maybe some other way.

SD: The work you have been mentioning a few minutes ago, about the Fukaya category— that's quite related to string theory and things in some ways.

JP: That's what I'm told. I can't say I understand any of it at the moment, but maybe sometime.

SD: Well, thanks very much John. I think it was very interesting to hear about you and I look forward to talking to you more in the months and years to come. I'm sure we'll have more interesting discussions.

JP: Yes, thank you, it was nice talking to you too.

John Pardon works on geometry and topology, with a special focus in symplectic topology. He first became well-known for solving, as an undergraduate at Princeton, Gromov's problem on distortion of knots, for which he was awarded the 2012 Morgan Prize. In 2013, Pardon proved the three-dimensional case of the Hilbert-Smith conjecture; his further work opened new avenues in the study of moduli of pseudo-holomorphic curves, and of Fukaya categories. John Pardon received the Alan T. Waterman Award (2017), the highest scientific honor granted by the NSF, and more recently he received the Clay Research Award (2022).

Pardon obtained his PhD in 2015 at Stanford University, under the supervision of Dr. Yakov Eliashberg.