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Mark Braverman Interview

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When did you realize that you want to be a mathematician? Who and what inspired you to become a mathematician? Was it an easy or difficult decision for you?

As a child, math was just easier than other things, so going into something to do with math or computer science was a relatively easy decision for me. On a professional level, the decision to become a professional mathematician (as opposed to doing industry work) has been much less obvious. On some level, it is a decision that I am continuously making.

Working on the academic side of theoretical computer science, we have plenty of connections to industry and applied problems, but keep the main features that make the academic environment attractive: free exchange of ideas (without having to worry about intellectual property and other corporate issues), the ability to interact with students and postdocs, and the ability to switch or even alternate between different directions in the field.

What kind of mathematics did you like the best in school and at the university? Do you have a favorite math problem from back then? How did you find "your" area of mathematics? What makes it particularly attractive for you?

I must say that it is very difficult to pinpoint which areas of mathematics I find attractive and why. For example, I like computational complexity, but find mathematical logic very difficult, even going back to university courses. Similarly, I find functional analysis natural, but struggle with differential equations. It is hard to generalize from this, even for me. I would say that I like it when there are general principles animating an area, and when subject to these principles, most things fall into their places without too many special subcases (even if actual proofs are very difficult or out of reach).

What I like about theoretical computer science is that it is clearly an important field that is still young enough that core concepts are only now being developed. Most of the original motivation for it had been applications to computation and communication, but it has since found connections to many areas of mathematics and other sciences. More importantly (for me), even though it took us until the 20th century to discover computation as an abstract concept, it is clear that basic questions about limits of computation are fundamental natural philosophy questions. If we were to encounter sentient aliens, they would probably have asked questions about computation in the same way as they would have probably asked questions about black holes or prime numbers.

How do you choose the problems to work on?

Broadly, there are two kinds of problems I work on.

The first kind are problems that I believe will help me learn a new area. For me, the best way to learn a new area is to work on solving problems in that area. This is a bit like being a PhD student again: my collaborators teach me about the area, and I help them with the math problems that come up. I am typically not very discerning about problem choice here, as long as they are interesting to the field I am studying, and it looks like working on them is a good way to learn the area.

The second kind are problems that come from one of my longer-term research programs. Broadly, my goal is to build and expand connections between theoretical computer science and other disciplines. In the past, I worked on connections with analysis and dynamics, with information theory, and with mechanism design in economics. I should note that in most cases these connections are not new. The big challenge is typically not so much solving some major "old" open problem, but rather asking the right questions to drive the research agenda forward. Here, most of the energy goes toward thinking about what are the right questions to ask.

Do you feel that the progress in mathematics is sometimes very fast and sometimes very slow? What do you do when it goes fast? What do you do when it is slow?

On the problem-solving side, when I see that some research direction or problem that has originated with us (i.e., my research group) is moving very quickly, we double down and see it through. That said, my personal preference is not to participate in a race to get the same result. If someone else I respect is working on a problem I trust that it will get done and I find it better to focus on things that wouldn't happen without me. When an area is overheated, I prefer to step back, and return when the dust settles.

When progress is very slow and we don't see a breakthrough for a long time I see it as a cue to do some theory-building – consolidate conjectures, prove equivalences. Even just writing a list of "smaller" open problems may be enough to stir progress. If we are completely stuck, we just postulate a conjecture (such as P\neq NP), and build on top of it.

On the more applied side, I must say that slow progress hasn't really been an issue. Technology is moving very fast (by math standards), and even just keeping up with changes in hardware, applied algorithms (such as in machine learning), and social applications of computing already means that the corresponding math will have to be moving at breakneck speed.

What was the first real mathematical result you obtained? Tell us about the result itself, the circumstances, and what effect it had on you.

I would say that my first "serious" mathematical result was on the existence of non-computable Julia sets. When I started my Master's degree in computer science in Toronto, I knew that I wanted to study the computational complexity of continuous problems. I started by studying computable analysis. I've obtained some new abstract results, but I felt that I needed "hard" specific examples of continuous structures to apply the theory to. Looking at Julia sets that occur in the area of complex dynamics seemed like a natural choice – there was a large number of computer programs for drawing Julia sets both by amateurs (the fractals are beautiful!) and by very serious mathematicians (e.g. implementing Milnor's distance estimator algorithm). My PhD adviser Steve Cook suggested that I look for an expert in complex dynamics and ask them about Julia sets (we expected them to all be computable). I had reached out to Michael Yampolsky from the math department – one of the world experts on one-dimensional complex dynamics. I thought I had a quick question asking for a few pointers, but it ended up being a long-term collaboration (we eventually wrote a book on the computability of Julia sets together).

The first "real" result we obtained was on the existence of non-computable Julia sets. It turned out that despite the "common" computer-generated pictures being accurate in most cases, there are cases in which the Julia set corresponding to a parameter c cannot be computed, no matter how long the program is allowed to run. The proof used (what was then) cutting-edge tools from complex dynamics. I took one course in complex analysis during my undergraduate studies, and had minimal background in dynamical systems, so I learned a lot. More importantly, I learned that it is difficult to predict the technical or conceptual depth of a question: I was looking for an example to work into my Master's thesis, and ended up with a project that occupied the majority of my PhD work.

Can you tell us about your biggest "Eureka!" moment?

I must say that in my experience, there is little correlation between "Eureka!" moments and actual (correct) results. Most of my "Eureka!" moments end up either as "would-be proofs" (i.e., proofs that would be really neat if it weren't for the fact that they turn out to be wrong), or well-known facts from some other field (but experiencing the joy of discovering them is a reward in itself).

The closest thing to a Eureka moment I've experienced where the proof actually worked was the 2009 proof of the Linial-Nisan conjecture. It was a conjecture about approximating certain boolean functions computed by simple circuits with polynomials. After listening to a talk (and hearing about the conjecture for the first time) I thought about a completely new way of combining two existing (seemingly unrelated and conflicting) schemes for approximating boolean functions with polynomials. While the combination seemed neat and promising, it took me another several weeks (and a 25 page write up) to come up with a proof of the conjecture based on the idea. Then, as I was about to post it, it occurred to me that the proof can be significantly simplified into 2 pages. Still, it took several weeks and a lot of work to get from what looked like a neat idea to a proof "from the book". There have been many similarly promising ideas that have not led me anywhere, at least not yet.

What did you feel and what did you do when you learned about being awarded the prize?

The first thing that happened was that I got an email from Carlos Kenig, the president of the IMU, that said that he needed to speak with me on the phone. Because of the timing of that email and due to the fact that I had no ongoing affairs with the IMU that would necessitate a phone call, I experienced a giddy mix of nervousness and excitement and immediately replied to the email, saying that I can talk any time. I then immediately told my wife, who shared my emotions. Thankfully, he called right away and told me the news. I was shocked and extremely grateful. My wife, for whom this is a less complicated thing, became extremely happy and did a little dance. My emotions were more contemplative. I can't say exactly how I felt, because I think I was in a bit of shock, but I definitely felt a lot of gratitude and experienced a sort of flashback to the entire journey that brought me to this point. I think I also felt a heightened sense of responsibility for the future of my field.

Who are the people who contributed the most to this success?

I want to open by saying that I don't like this question when taken literally because there are very many people who contributed to my success and if I were to start mentioning names I couldn't avoid missing someone who deserves to be mentioned. However, I will answer this question in a wider sense.

There are two broad groups of people who contributed to my success. The first group consists of people who created the conditions for even being in the position to do the work that is being recognized. This includes people who sustained my life, such as my parents, my wife, and the people who invented a particular chemotherapy drug that saved my life some years ago and the doctors who treated me then. Also, in this group are people and institutions who may not have given me literal life, but they gave life to my aspirations, and who gave me foundational knowledge. These include teachers and mentors of all kinds (starting with my parents and grandparents), institutions that gave me the freedom to advance at my own pace and pursue my interests (these include the Technion, where I received my undergraduate degree, the University of Toronto, where I received my doctorate, and Microsoft Research, where I did my postdoc). I am grateful to various funding agencies and foundations that have been supporting my work generously. Their support allowed me to run a vibrant group with brilliant students and postdocs, of whom, I hope, you are soon to hear for their own achievements.

The second group consists of people with whom I had the privilege to do the work, and who taught me most of what I know about the various advanced branches of math and theoretical computer science. These are my students, postdocs and long-term collaborators, from whom I continue to learn and to draw inspiration. Without them, much of my work would have never happened, and perhaps I wouldn't have stayed in the field if not for the continuing opportunity to work with and learn from such an inspiring group of people.

What are the new horizons, new problems, new goals for you now?

I'm currently working in two main directions. The first direction is algorithmic mechanism design – the problem of converting algorithms (where inputs are given) into mechanisms where inputs come from potentially self-interested or strategic participants. Algorithms are rapidly entering essentially all domains of our lives, which makes mechanism design a highly applied and urgent problem. Even more than developing mechanisms for specific important problems, I hope to design some generic principles for converting common types of modern distributed algorithms into mechanisms.

The second direction is more theoretical, and grows out of the information complexity work. Here my goal is to develop new invariants for models of computation. Abstractly speaking, information is one such invariant – if k bits of information are not present at some location, you need to transmit at least k bits to get them there. Classical information theory was developed as part of the digital communication revolution, and most of the hard work on it was done outside of computational complexity theory. For the next set of invariants (e.g. for strong lower bounds on data structures) we might not be so lucky and might need to develop them ourselves within computational complexity theory.

Do you draw inspiration from teaching mathematics? Do you wish people knew more about what is happening in mathematics? Is there something mathematicians should do to help people appreciate the importance and beauty of what they do?

One of my favorite theorems to teach is Godel's First Incompleteness Theorem. Colloquially, it says that in any "interesting" axiomatic system, there are statements that are true but cannot be proved. In particular, there will always be correct statements in math that we can't prove (and which will need to be added as axioms if we want to use them). The proof sketch is pretty simple to explain to students with some background in programming, yet it is philosophically very deep. This theorem was arguably the first important "gift" from modern computational thinking to the philosophy of knowledge.

Mathematics is capable of producing such philosophical insights, but it is “also” useful (and necessary) in pretty much all engineering undertakings. Understandably, the applied side receives significantly more attention (as it should be). When explaining the significance of some of my more abstract work, say, in computational complexity theory, it is always tempting to connect it to current applications such as cryptography or hardware acceleration. It takes more effort to make the case for the philosophical depth of the questions: we want to understand properties of natural numbers or of computational “randomness” for the same reason we want to study distant stars or ancient creatures – they are there, and we (humans) are curious.

How important are interactions with a computer for your work, now and in the future? Do you get more from interactions with a computer or interactions with people?

When it comes to the “social” function of computers, I mainly use computers as communication tools to connect with collaborators elsewhere (or, during the pandemic, next door). I also use computers to obtain resources, such as research and survey articles. I view reading papers as a form of communication as well, because it is a form of interaction with other people’s ideas.

Beyond communicating, I have used computers for simulations, and to formulate hypotheses. For example, our paper with Makarychev, Makarychev and Naor, showing that the commonly conjectured value of the Grothendieck’s constant was wrong, started with computer simulations that stubbornly gave us “wrong” results that we weren’t expecting.

Still, by far the most impact computers have had so far has been in organizing information – a quick search allows one to find an obscure paper from 30 years ago using a couple keywords. There is still a lot of room for improvement in terms of “just” knowledge organization (e.g. answering queries of the form “give me all articles that independently claim to prove Theorem 2.6”). Note that a tool capable of answering such queries will probably lead to a lot of fast progress by connecting open problems to existing results. After that, it is hard to predict how effective automated theorem provers would be, and what impact they would have on mathematics overall. There will be a significant impact, it is just hard to predict how exactly it will play out. That said, pure mathematics is at its core a philosophical discipline, so I don’t see humans being taken out of the loop completely.

Outside of mathematics, what are your favorite things to do, interests, pursuits? Do you approach them as a mathematician, or are you happy to forget about mathematics while you are on a break?

I actually have a pretty normal work day (i.e. the equivalent of 9-5 most days, with the hours sometimes shifted a little). Outside of work I am a husband and father of two young children and I spend a lot of time with my family. I spend a lot of time on activities with my kids. There are things like birthday parties, playdates, and other things you would imagine a parent of young children would do. Other than that, I like to read widely. In recent years I have read lots of nonfiction (on topics such as history and sociology). Lately, like many in the pandemic, I have been playing a lot of recreational chess. I am by no means good at it, but I do enjoy playing flash matches online with other people like myself. My wife and I also started studying Talmud together this year. The Talmud is a (very complicated) body of Jewish texts that annotate the Hebrew Bible and go through what amounts to legal arguments regarding various practices – a very interesting and illuminating glimpse into the development of Jewish thought. These are just some of my recent pursuits, but of course these things change over time.

As far as approaching things as a mathematician, I think that any person who is immersed in his profession (provided that the profession is interesting to them), in some ways becomes one with the way of thinking that his profession requires of him. It is almost impossible to notice in oneself because it is so natural. Also, I understand mathematics to be just another language that I speak natively. I think I use it internally to talk with myself and to understand the world just like I use my actual language. With that, there are some pitfalls one has to watch for. For example, in math, you can build a very long chain of arguments, and, as long as each link is valid, the chain will be perfectly correct. In other fields, such as history, such chains typically fail – sometimes spectacularly – because the links are only "somewhat correct", and cannot be chained together.

As a prize winner, you also become an ambassador of mathematics to the society and its leaders. What would you say in a meeting with, for instance, a Science and Education Minister?

I am guessing that (just as an actual ambassador), much of what I would say would be restating obvious points ("Do fund math education"; "Make advanced math classes available as broadly as possible"; "Do fund basic research in mathematics and computer science, both for its edifying value and because it has great return-on-investment" etc.).

Of the less obvious points, I would argue that good (not necessarily great or deep) school-level math education coupled with some historic and philosophical background should be an important part of raising engaged adults. This is an argument typically made in favor of humanities, while mathematics emphasizes its engineering applications, but the argument applies to mathematics as well.

There are at least two distinct ways in which even elementary math education can help citizens. Firstly, numbers play an important role in public arguments (recall how statistics and probabilities were being thrown around during debates around covid policies). Math confidence coupled with some relatively basic skills (basic probability, understanding orders of magnitudes) go a long way toward being able to parse such arguments, and deflect clearly invalid ones. Secondly, mathematics is a safe environment for experiencing *not knowing*. Not knowing is a uniquely unpleasant experience, and we usually make up for it, by inventing stories (Earth resting on giant turtles), or conspiracy theories. Importantly, in public debates, demagogues are able to take advantage of this aversion by offering a false sense of certainty. Most people don't associate the study of math with building tolerance for not knowing, but in fact, for many people it is the only place where they ever experience getting a wrong answer and not seeing why, or being completely stuck. Even at a broader level, the Collatz conjecture is one of the easiest examples of something that we (humanity) are stuck on (but hope to find out one day) that one can explain to an 8th grader.