A Conversation with Timothy Gowers

David Richeson

Timothy Gowers is a highly accomplished and celebrated research mathematician (see page 12 for more about his research). He received the prize of the European Mathematical Society (1996) and the Fields Medal (1998). He is a fellow of the Royal Society (1999) and was knighted by Queen Elizabeth (2012). He holds the Rouse Ball Chair at Cambridge University.

He is also an excellent expositor. He has written about mathematics, the philosophy of mathematics, and the culture of mathematics. He wrote *A Very Short Introduction to Mathematics* and was editor of *The Princeton Companion to Mathematics*. He blogs regularly at *gowers.wordpress.com*.

In a 2009 blog post Gowers asked whether massively collaborative mathematical research was possible—crowdsourcing mathematics, so to speak. To test whether research could be conducted in this 21st-century way, he posed a problem that mathematicians of all levels could contribute to. Work was carried out in the comments section of his blog and on a wiki. The theorem was proven in seven weeks. Thus the "polymath project" was born.

On March 26, Gowers received the Joseph Priestley Award from my institution, Dickinson College. Afterward we chatted about his life and work. This interview has been edited for length and clarity.

David Richeson: You were on the UK's Math Olympiad team as a high school student. Do math competitions prepare good mathematicians? Is being successful at competitions a good indicator that someone will be a good mathematician?

Timothy Gowers: It is not necessary or sufficient. But it is still a good indicator. It correlates surprisingly strongly. When I say surprising, it is because the requirements to be successful at competition problems appear to be very different from the requirements for being successful at research.

The nature of the problems is a bit different, but also there is more of a premium on speed if you're doing a competition than if you are doing research. If you're doing research, it doesn't matter if you're slow. In fact, it can almost be an advantage.

Also, you get a slightly false impression of what maths is like from a carefully set problem that has one little trick that you have to dig down and find and then the problem gets unlocked. A research problem occasionally has that flavor, but much more often it is not like that.

But [competition] gives you at least one very important experience, which is the experience of looking at a problem and thinking "I have no idea how to solve that," and then thinking quite a lot about it, chewing over it, and finally managing to solve it. That's an experience that some people never have. But when you've had it once or twice, it is very powerful. You lose your fear of that feeling, which everybody gets.

In fact, if I could make one recommendation for teaching—which I apply to my teaching all the time—I have almost a pathological dread of telling anybody the answer to anything. If somebody can't do a problem, I try very hard to think of the minimal nonzero hint that tells them something. Because it is terribly important to have the experience of finding things difficult but then managing to get past that stage. If the habit of thinking for yourself and solving problems even though they are difficult could be instilled at a very early stage, it would make a huge difference.

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DR: I've been reading some of your research articles. To a non-expert like me, it seems as if there's a remarkable amount of breadth. Is there a common theme among all your research endeavors?

TG: I don't feel that my interests are all that broad, actually. It is true that I've worked in areas like analysis and combinatorics, which are normally considered quite far apart. But the Banach space problems I was working on were basically combinatorial, and the fact that they were in the continuous realm rather than the discrete realm was not very important.

I would describe my interest as being something like asymptotic combinatorics. I like combinatorial problems that involve inequalities and estimates and bounds and things like that. When you look at combinatorial structures and let n get large, a combinatorial structure sort of approximates a continuous structure. In fact, there are very precise statements along those lines. But in an imprecise sense, the feel of the subject is very much like the feel of analysis.

The areas that I study, which to the outsider may look quite spread out, are not, really. I've got my little bag of tricks just as everyone else has their bag of tricks.

DR: One thing I found fascinating about your work in Banach spaces were the crazy counterexamples that you came up with. What is the role of examples and counterexamples in building mathematical theory?

TG: I suppose I have a personal interest in leaping to their defense. The rough story is that I became obsessed with a problem called the distortion problem of Banach spaces. The distortion problem had been an open problem for a long time. It was a big question in the subject. I put a lot of effort into trying to prove it.

I developed various ideas, and then I turned my attention to another problem called the unconditional basic sequence problem. I was trying to prove it, but then I realized that my attempts to prove it would not work if there were some sets that were a bit like counterexamples to the distortion problem.

Around that time, Thomas Schlumprecht defined a space that wasn't a counterexample to the distortion problem, but which gave me the ingredients I needed to get me on the path to a counterexample to the unconditional basic sequence problem.

The main point I want to make is that I wanted to prove a theorem and then, realizing that the proof wouldn't work if such-and-such happened, got from that realization to a counterexample. Once it arose, it led to counterexamples to a number of other things, and a general method for producing counterexamples, which I exploited for a while.

I was worried at the time of getting typecast as a counterexample specialist [laughs]. Actually it was quite a relief when a little bit later I came up with an actual proof of a conjecture instead of a crazy counterexample.

DR: Do you have any advice for undergraduates who are beginning their research career or are heading to graduate school to start a research career?

TG: It is very tough to decide what to work on. You have to find an adviser you trust who can set you suitable problems to start with.

But you should also not be afraid of just trying things. If you go to a research seminar and someone mentions an open problem and you find it interesting, don't just think, "Oh, well this person couldn't solve it, so I certainly couldn't do it." It is often not like that. Give it a go.

You haven't got much to lose if you spend three or four days really trying hard to solve the problem. If you get absolutely nowhere, then you should probably stop and go back to what you were doing before. And if you get somewhere and make a little bit of an advance, then maybe you should stick with it a bit longer.

If you work on a hard problem, all sorts of benefits can flow from that, even if you don't solve it. One possible benefit applied to me when I worked on the distortion problem, which was eventually solved by other people, by the way. If I hadn't thought very, very hard about the distortion problem, in an ultimately fruitless attempt, I wouldn't have solved the unconditional basic sequence problem—I wouldn't have had the ideas I built up from thinking about the distortion problem, which were crucial for the solution.

Another thing working on a hard problem can give you, which is very important, is a readiness if somebody at some point says some idea that is relevant to the problem. If you hadn't thought hard about the problem, you wouldn't notice that this idea could help you—that it happens to be the missing ingredient.

Solving a problem is a probabilistic process. One good way of increasing your chances in research is to think quite hard about a lot of problems. If you spend a week of serious effort on 10 different problems, then the chance that at some point some little piece of luck will happen that will enable you to solve one of them is surprisingly high.

So, when you're just starting out, you want to cultivate a general interest in mathematics and a readiness to think about things. If you just say, "My adviser has suggested this problem. I've got to solve this problem and there's nothing else," then you're putting all of your eggs in one basket and your probability of success, while I hope that it is not zero, is smaller than it could be.

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DR: You received the Fields Medal in 1998. Did that change the way you approach your work, what problems you choose, and so on? For instance, did it put more pressure on you, or was it a liberating experience that allowed you to take risks?

TG: It was a bit of both, actually. Part of me thought, I really, really do not want to have the reputation as somebody who got the Fields Medal and never really did anything else [laughs]. So that was a source of pressure. It was a huge relief when I proved my first reasonably good result after [laughs].

That said, more recently I've been working quite seriously on automatic theorem proving. [Editor's note: he's creating algorithms that enable computers to prove theorems and produce human-readable proofs.] If I hadn't already become very established as a mathematician, I might have worried that people would think, "Oh, gosh, he's given up, and is not doing proper maths anymore."

I am somewhat liberated to work on something I've

A Sampling of Timothy Gowers's Mathematical Work

Banach Spaces

A basis for a vector space V is a set of vectors $\{\mathbf{v}_1, \mathbf{v}_2, \ldots, \mathbf{v}_n\}$ such that every vector in V can be written uniquely as a linear combination of the basis vectors. That is, for any vector $\mathbf{w}$ in V there are unique scalars $a_1, a_2, \ldots, a_n$ such that $\mathbf{w} = a_1\mathbf{v}_1 + a_2\mathbf{v}_2 + \cdots + a_n\mathbf{v}_n$. The number of elements in the basis is the dimension of V.

A Banach space is a vector space that satisfies other criteria. By the 1990s finite dimensional Banach spaces were well understood, but infinite dimensional spaces were more mysterious. For infinite dimensional spaces we must allow linear combinations of infinitely many basis vectors to obtain all vectors. But we know from calculus that if we allow infinite sums, we are inviting trouble—rearranging a conditionally convergent series can yield a different sum.

But some Banach spaces have nice infinite bases. A Banach space has an *unconditional basis* $\{\mathbf{v}_1, \mathbf{v}_2, \mathbf{v}_3, \ldots\}$ if every vector $\mathbf{w}$ can be written uniquely as a linear combination of the basis vectors, $\mathbf{w} = \sum_{i=1}^{\infty} a_i\mathbf{v}_i$, and the order of the summation does not matter.

The famous unconditional basic sequence problem asked if every infinite dimensional Banach space has an infinite dimensional subspace with an unconditional basis. In 1992 Timothy Gowers and Bernard Maurey independently discovered examples showing that the answer is no.

In the next few years, Gowers went on to prove more foundational results about infinite dimensional Banach spaces. In 1994 he proved a dichotomy theorem that split infinite dimensional Banach spaces into two classes. Using this theorem he proved that certain Banach spaces, the so-called homogeneous spaces, must be isomorphic to the space of sequences ℓ_2. This answered a question posed by Banach in the early 1930s.

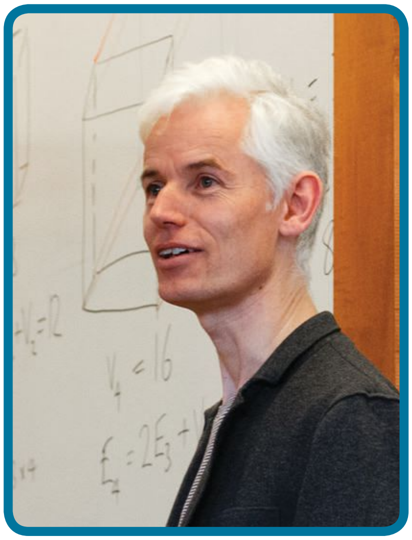
Arithmetic Combinatorics

In 1927 Van der Waerden proved the following beautiful theorem. If we have r colors and are given any k, then there exists an N such that any coloring of always been interested in. I've reached a stage where I can afford to say, "I'm not going to work on conventional maths problems for six months, and I'm just going to work on this." There have been some periods like that in the last few years.

DR: How did you get the idea for the polymath project? And how did you choose the first problem?

TG: I can't remember exactly how I got the idea. But I remember that I had the idea by early January 2009, because I remember telling people that I thought it might be an amusing thing to try.

I had this realization that what would initially seem to be an obstacle—which is that people wouldn't want to give away their ideas—that this impulse could be turned on its head if it was all happening sufficiently publicly in small units of discourse. Because then if you keep your ideas to yourself, you run the risk that someone will have those ideas and you'll lose them. If you think of the unit of discourse as not being the full article with the proof, but just as the comment on the blog post, then people's instinct to establish priority would work the other way and they'd want to give away their ideas as soon as they had them. I think that



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worked, basically.

Then I discovered that the idea of open science was not a new one, and I figured that if I didn't do something fairly soon, then someone else would beat me to it [laughs]. Exactly the sort of "wishing to establish priority" impulse led me to go from thinking "wouldn't that be a fun thing to try?" to actually doing it [laughs].

That still left the question of what problem to choose. I could have said, "Let's try to solve the Riemann hypothesis," but then nobody would have wanted to give away a good idea. And people would have said, "Well, it is such a hard problem, there's zero chance of success,"

1,2,...,N, contains an arithmetic progression of length k all of whose terms are the same color.

For example, if we have $r = 2$ colors and we need to be guaranteed an arithmetic progression of length $k = 3$, then we must take at least the first $N = 9$ natural numbers. For example, 1 2 3 4 5 6 7 8 9 is a coloring with two colors and it has a 3-term red arithmetic progression: 2, 5, 8.

In 1936 Erdős and Turán made a more general color-free conjecture: Given k and $0 < \delta \leq 1$, there is an N such that every subset of $\{1,\ldots,N\}$ of density δ (that is, the subset has at least $\delta \cdot N$ elements) has an arithmetic progression of length k.

In 1973 Szemerédi proved the conjecture, and the result is now known as Szemerédi's theorem. In 2001 Gowers gave an important new proof of Szemerédi's theorem and provided a much smaller upper bound for N than had existed previously.

The first polymath project took these ideas one step further. The Hales-Jewett theorem is like a high-dimensional version of Van der Waerden's theorem in which we are not coloring integers, but cells of a high-dimensional tic-tac-toe board. And instead of looking for arithmetic progressions, we are looking for lines of the same color (like winning configurations of the tic-tac-toe game).

Just as Szemerédi's theorem is a density version of Van der Waerden's theorem, there is a density version of the Hales-Jewett theorem. The first polymath project found a new proof of this density theorem. And from this proof, they produced a new proof of Szemerédi's theorem.

Further Reading

For more on the first decade of Gowers's mathematical work, see Joram Lindenstrauss's entry in "The Mathematical Work of the 1998 Fields Medalists," *Notices of the AMS* (January 1999) 17–26.

For more about the polymath project, visit *polymathprojects.org* and read "Massively Collaborative Mathematics," by Timothy Gowers and Michael Nielsen, *Nature* **15** (October 2009) 879–881.

Read Gowers's article (with M. Ganesalingam) "A fully automatic problem solver with human-style output" (*arxiv.org/abs/1309.4501*) for information on his work in automatic theorem proving.

which would have been an accurate assessment.

So it seemed better to choose a compromise. I didn't want a boringly easy problem—I wanted it to be a genuine problem that we'd be very pleased if we solved, but not an absolutely notoriously hard problem. I also thought that it would be good if it was something where a start had been made on it.

I happened to have a problem that someone else and I had discussed, and we'd had some thoughts about it that seemed quite promising. We had a general idea of how to get started, but had abandoned it quite some time earlier because we'd run out of steam.

So I reconstructed the ideas and wrote an initial blog post. The advantage of doing that was that the first question was already split up into smaller subquestions that people could have a quick opinion about. It turned out to be enough to get things started. It went from there, although not in exactly the direction I was expecting.

DR: Where do things stand now? Have there been many successful polymath projects?

TG: Not too many, but there have been a few, most notably, recently, the improvements to the prime gaps theorem. [Editor's note: The twin prime conjecture says that there are infinitely many primes two numbers apart—3 and 5, 5 and 7, 11 and 13, and so on. In 2013 Yitang Zhang surprised the mathematical community by proving that there are infinitely many prime pairs at most 70 million numbers apart. (See Jordan Ellenberg's "The Beauty of Bounded Gaps," *Math Horizons*, September 2013.) The polymath project got the gap down to 246.]

We're still feeling our way around and trying to see what it is good for and what it is not good for. Initially people started discussing whether this was going to be the future of the way we do research. They started worrying about what would happen if everything was done publicly online, but this was going a bit too far too quickly.

The first project had the advantage that it was the first one. The prime gaps one had several things that made it work, such as that it was a very sexy problem [laughs]. Also, it was clear that progress was possible right from the start.

DR: The bound was clearly bigger than it had to be.

TG: Zhang had very understandably not made significant efforts to optimize the bound, and so people were breaking the record once a day, practically. Terry Tao said something like, "It rescued the literature from having 50 papers that each gave a new slight improvement to the record" [laughs]. It was just a very good way of people pooling their resources.

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There's been one other polymath project that I see as a success, but of an unconventional kind. That was the one to solve the Erdős discrepancy problem. It didn't solve the problem, but it led pretty quickly to a significant improvement in understanding the problem.

That's a possible role of the polymath project in the future. It can hugely speed up that initial stage when you're thinking about a problem and you have a lot of ideas, and afterward you view the problem in a much more sophisticated and deeper way. Sometimes you're lucky and can push on and solve the problem. But sometimes you hit what feels like a genuine barrier rather than the initial barrier, which was largely due to unfamiliarity with the problem.

DR: What are your interests outside of math? I know you come from a family of musicians and you play jazz piano.

TG: Music is definitely the thing outside mathematics—leaving aside family and things like that—that is most important to me. It is not my profession, but I couldn't imagine a life without it. It's not a little hobby. It's not like going to the movies, which I hugely enjoy and would be sorry not to do, but which is not part of my identity the way music is.

DR: Do you play only for yourself or do you play out?

TG: Occasionally I play in small jazz groups in public. My musical activities have somewhat dwindled. I played a lot when I was at school and university. Then the demands of family life and just the fact of not being a student made it all that much harder to do.

My father was a composer. I've done some composing, and it's something that still interests me. If I were told that it was my destiny never to write a serious piece of music, that's something I'd be quite disappointed by. It might have to wait until I retire. I feel that I have got unfinished business with composition. ■

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