# Interviews with Three Fields Medalists

Interviewed by Vicente Muñoz and Ulf Persson

Andrei Okounkov, Terence Tao, and Wendelin Werner received Fields Medals at the 2006 International Congress of Mathematicians in Madrid, Spain (Grigory Perelman was also awarded a medal but declined to accept it). These interviews with Okounkov, Tao, and Werner were conducted via email by Vicente Muñoz and Ulf Persson in the fall of 2006 and originally appeared in the December 2006 issue of the *European Mathematical Society Newsletter*.

# Andrei Okounkov

*Muñoz & Persson:* How did you get interested in mathematics?

**Okounkov:** The most important part of becoming a mathematician is learning from one's teachers. Here I was very fortunate. Growing up in Kirillov's seminar, I had in its participants, especially in Grisha Olshanski, wonderful teachers who generously invested their time and talent into explaining mathematics and who patiently followed my first professional steps. I can't imagine becoming a mathematician without them. So it must be that in this respect my professional formation resembles everybody else's.

What was perhaps less usual is the path that led me to mathematics. I didn't go through special schools and olympiads. I came via studying economics and army service. I had a family before papers. As a result, my mind is probably not as quick as it could have been with an early drilling in math. But perhaps I also had some advantages over my younger classmates. I had a broader view of the universe and a better idea about the place of mathematics in it. This helped me form my own opinion about what is important, beautiful, promising, etc.

It also made mathematics less competitive for me. Competition is one of those motors of human society that will always be running. For example, we are having this interview because of the outcome of a certain competition. But I believe it distracts us from achieving the true goal of science, which is to understand our world.

*M* & *P*: So, you wouldn't say that competition is the best way to do mathematics?

**Okounkov:** I think it is a mistake that competition is actively promoted on every level of math. While kids take solving puzzles perhaps a bit too seriously, grown-ups place the ultimate value on being the first to prove something. A first complete proof, while obviously very important, is only a certain stage in the development of our knowledge. Often, pioneering insights precede it and a lot of creative work follows it before a particular phenomenon may be considered understood. It is thrilling to be the first, but a clear proof is for all and forever.

*M & P:* How do you prefer to work on mathematical problems? Alone or in collaboration?

**Okounkov:** Perhaps you can guess from what I said before that I like to work alone, I equally like to freely share my thoughts, and I also like to perfect my papers and talks.

There may well be alternate routes, but I personally don't know how one can understand something without both thinking about it quietly over and over and discussing it with friends. When I feel puzzled, I like long walks or bike rides. I like to be alone with my computer playing with formulas or experimenting with code. But when I finally have an idea, I can't wait to share it with others. I am so fortunate to be able to share my work and my excitement about it with many brilliant people who are at the same time wonderful friends.

And when it comes to writing or presenting, shouldn't everyone make an effort to explain? Wouldn't it be a shame if something you understood were to exist only as a feeble neuron connection in your brain?

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*M & P:* Do you prefer to solve problems or to develop theories?

Okounkov: I like both theory and problems, but best of all I like examples. For me, examples populate the world of mathematics. Glorious empty buildings are not my taste. I recall my teacher Kirillov saying that it is easier to generalize an example than to specialize a theory. Perhaps he did not mean this 100 percent seriously, but there is a certain important truth in those words. Understanding examples links with ability to compute. Great mathematicians of the past could perform spectacular computations. I worry that, in spite of enormous advances in computational methods and power, this is a skill that is not adequately emphasized and developed. Any new computation, exact or numeric, can be very valuable. The ability to do a challenging computation and to get it right is an important measure of understanding, just like the ability to prove is.

*M & P*: Much of your work has deep connections to physics. Does that mean that you find it essential that mathematics is related to the natural world, or that you would even think of it as subservient to the other natural sciences?

Okounkov: When I said "our world" earlier I didn't mean just the tangible objects of our everyday experience. Primes are as real as planets. Or, in the present context, should I say that celestial bodies are as real as primes? Throughout their history, natural sciences were a constant source of deep and challenging mathematical problems. Let's not dwell now on the obvious practical importance of these problems and talk about something else, namely the rich intuition that comes with them. This complex knowledge was derived from a multitude of sources by generations of deep thinkers. It is often very mathematical. Anyone looking to make a mathematical discovery needs a problem and a clue. Why not look for both in natural sciences?

This doesn't make mathematics a subordinate of other sciences. We bring, among other things, the power of abstraction and the freedom to apply any tools we can think of, no matter how apparently unrelated to the problem at hand. Plus what we know we really do know. So we can build on firmer foundations, hence higher. And look—mathematics is the tallest building on campus both in Princeton and in Moscow.

*M & P:* There is a common view of the public that computers will make mathematicians superfluous. Do you see a danger in that? And in particular what is your stand on computer-assisted proofs? Something to be welcomed or condemned?

**Okounkov:** Computers are no more a threat to mathematicians than food processors are a threat to cooks. As mathematics gets more and more complex while the pace of our lives accelerates, we must delegate as much as we can to machines. And

I mean both numeric and symbolic work. Some people can manage without dishwashers, but I think proofs come out a lot cleaner when routine work is automated.

This brings up many issues. I am not an expert, but I think we need a symbolic standard to make computer manipulations easier to document and verify. And with all due respect to the free market, perhaps we should not be dependent on commercial software here. An open-source project could, perhaps, find better answers to the obvious problems such as availability, bugs, backward compatibility, platform independence, standard libraries, etc. One can learn from the success of  $T_{\rm E}X$  and more specialized software like Macaulay2. I do hope that funding agencies are looking into this.

*M & P:* The age of the universalists is gone. Nowadays mathematics is very diverse and people tend to get mired in subspecialities. Do you see any remedy to this?

**Okounkov:** Mathematics is complex. Specialization, while inevitable, doesn't resolve the problem. Mathematics is a living organism; one cannot simply chop it up. So how do we both embrace and resist specialization?

We can be better neighbors. We shouldn't build high fences out of sophisticated words and a "you wouldn't understand" attitude. We should explain what we know in the simplest possible terms and minimal generality. Then it will be possible to see what grows in the next field and use the fruits of your neighbor's labor.

Good social contact makes good neighbors. Effective networks are hard to synthesize but they may be our best hope in the fight against fragmentation of mathematics. I, personally, wouldn't get anywhere without my friends/collaborators. I think there is a definite tendency in mathematics to work in larger groups, and I am certain this trend will continue.

# **Terence** Tao

*M* & *P*: When did you become interested in mathematics?

Tao: As far as I can remember I have always enjoyed mathematics, though for different reasons at different times. My parents tell me that at age two I was already trying to teach other kids to count using number and letter blocks.

*M* & *P*: *Who influenced you to take the path of mathematics*?

Tao: I of course read about great names in mathematics and science while growing up, and perhaps had an overly romanticized view of how progress is made; for instance, E.T. Bell's *Men of Mathematics* had an impact on me, even though nowadays I realize that many of the stories in that book were overly dramatized. But it was my own advisors and mentors, in particular my undergraduate advisor Garth Gaudry and my graduate advisor Eli Stein, who were the greatest influence on my career choices.

*M* & *P*: What was your feeling when you were told about being a medalist?

Tao: I had heard rumors of my getting the medal a few months before I was officially notified—which meant that I could truthfully deny these rumors before they got out of hand. It was still of course a great surprise, and then the ceremony in Madrid was an overwhelming experience in many ways.

*M* & *P*: Do you think that the Fields Medal will put too high expectations on you, thus coming to have an inhibiting influence?

Tao: Yes and no. On the one hand, the medal frees one up to work on longer-term or more speculative projects, since one now has a proven track record of being able to actually produce results. On the other hand, as the work and opinions of a medalist carry some weight among other mathematicians, one has to choose what to work on more carefully, as there is a risk of sending many younger mathematicians to work in a direction that ends up being less fruitful than first anticipated. I have always taken the philosophy to work on the problems at hand and let the recognition and other consequences take care of themselves. Mathematics is a process of discovery and is hence unpredictable; one cannot reasonably try to plan out one's career, say by naming some big open problems to spend the next few years working on. (Though there are notable exceptions to this, such as the years-long successful attacks by Wiles and Perelman on Fermat's last theorem and Poincaré's conjecture respectively.) So I have instead pursued my research organically, seeking out problems just at the edge of known technology whose answer is likely to be interesting, lead to new tools, or lead to new questions.

*M* & *P*: Do you feel the pressure of having to obtain results quickly?

Tao: I have been fortunate to work in fields where there are many more problems than there are people, so there is little need to competitively rush to grab any particular problem (though this has happened occasionally, and has usually been sorted out amicably, for instance via a joint paper). On the other hand, most of my work is joint with other collaborators, and so there is an obligation to finish the research projects that one starts with them. (Some projects are over six years old and still unfinished!) Actually, I find the "pressure" of having to finish up joint work to be a great motivator, more so than the more abstract motivation of improving one's publication list, as it places a human face on the work one is doing.

*M & P: What are your preferences when attacking a problem?* 

Tao: It depends on the problem. Sometimes I just want to demonstrate a proof of concept, that a certain idea can be made to work in at least one simplified setting; in that case, I would write a paper as short and simple as possible and leave extensions and generalizations to others. In other cases I would want to thoroughly solve a major problem, and then I would want the paper to become very systematic, thorough, and polished, and spend a lot of time focusing on getting the paper just right. I usually write joint papers, but the collaboration style varies from co-author to co-author; sometimes we rotate the paper several times between us until it is polished, or else we designate one author to write the majority of the article and the rest just contribute corrections and suggestions.

*M & P:Do you spend a lot of time on a particular problem?* 

**Tao:** If there is a problem that I ought to be able to solve, but somehow am blocked from doing so, that really bugs me and I will keep returning to the problem to try to resolve it. (Then there are countless problems that I have no clue how to solve; these don't bother me at all).

*M & P:* Do you prefer to solve problems or to develop theories?

**Tao:** I would say that I primarily solve problems, but in the service of developing theory; firstly, one needs to develop some theory in order to find the right framework to attack the problem, and secondly, once the problem is solved it often hints at the start of a larger theory (which in turn suggests some other model problems to look at in order to flesh out that theory). So problem-solving and theory-building go hand in hand, though I tend to work on the problems first and then figure out the theory later.

Both theory and problems are trying to encapsulate mathematical phenomena. For instance, in analysis, one key question is the extent to which control on inputs to an operation determines control on outputs; for instance, given a linear operator T, whether a norm bound on an input function f implies a norm bound on the output function Tf. One can attack this question either by posing specific problems (specifying the operator and the norms) or by trying to set up a theory, say of bounded linear operators on normed vector spaces. Both approaches have their strengths and weaknesses, but one needs to combine them in order to make the most progress.

*M & P:* How important is physical intuition in your work?

**Tao:** I find physical intuition very useful, particularly with regard to PDEs [partial differential equations]—Ineed to see a wave and have some idea of its frequency, momentum, energy, and so forth, before I can guess what it is going to do—and then, of course, I would try to use rigorous mathematical analysis to prove it. One has to keep alternating between intuition and rigor to make progress on a problem, otherwise it is like tying one hand behind your back.

*M* & *P*: *What point of view is helpful for attacking a problem?* 

Tao: I also find it helpful to anthropomorphize various mathematical components of a problem or argument, such as calling certain terms "good" or "bad", or saying that a certain object "wants" to exhibit some behavior, and so forth. This allows one to bring more of one's mental resources (beyond the usual abstract intellectual component of one's brain) to address the problem.

*M* & *P*: Many mathematicians are Platonists, although many may not be aware of it, and others would be reluctant to admit it. A more "sophisticated" approach is to claim that it is just a formal game. Where do you stand on this issue?

Tao: I suppose I am both a formalist and a Platonist. On the one hand, mathematics is one of the best ways we know to try to formalize thinking and understanding of concepts and phenomena. Ideally we want to deal with these concepts and phenomena directly, but this takes a lot of insight and mental training. The purpose of formalism in mathematics, I think, is to discipline one's mind (and filter out bad or unreliable intuition) to the point where one can approach this ideal. On the other hand, I feel the formalist approach is a good way to reach the Platonic ideal. Of course, other ways of discovering mathematics, such as heuristic or intuitive reasoning, are also important, though without the rigor of formalism they are too unreliable to be useful by themselves.

*M* & *P*: *Is there nowadays too much a separation between pure and applied mathematics?* 

Tao: Pure mathematics and applied mathematics are both about applications, but with a very different time frame. A piece of applied mathematics will employ mature ideas from pure mathematics in order to solve an applied problem today; a piece of pure mathematics will create a new idea or insight that, if the insight is a good one, is quite likely to lead to an application perhaps ten or twenty years in the future. For instance, a theoretical result on the stability of a PDE may lend insight as to what components of the dynamics are the most important and how to control them, which may eventually inspire an efficient numerical scheme for solving that PDE computationally.

*M & P:*Mathematics is often described as a game of combinatorial reasoning. If so, how would it differ from a game, say like chess?

Tao: I view mathematics as a very natural type of game, or conversely games are a very artificial type of mathematics. Certainly one can profitably attack certain mathematical problems by viewing them as a game against some adversary who is trying to disprove the result you are trying to prove, by selecting parameters in the most obstructive way possible, and so forth. But other than the fact that games are artificially constructed, whereas the challenge of proving a mathematical problem tends to arise naturally, I don't see any fundamental distinctions between the two activities. For instance, there are both frivolous and serious games, and there is also frivolous and serious mathematics.

*M & P:* Do you use computers for establishing results?

Tao: Most of the areas of mathematics I work in have not yet been amenable to systematic computer assistance, because the algebra they use is still too complicated to be easily formalized, and the numeric work they would need (e.g., for simulating PDEs) is still too computationally expensive. But this may change in the future; there are already some isolated occurrences of rigorous computer verification of things such as spectral gaps, which are needed for some arguments in analysis.

*M* & *P*: Is a computer-assisted proof acceptable from your point of view?

**Tao:** It is of course important that a proof can be verified in a transparent way by anyone else equipped with similar computational power. Assuming that is the case, I think such proofs can be satisfactory if the computational component of the proof is merely confirming some expected or unsurprising phenomenon (e.g., the absence of sporadic solutions to some equation, or the existence of some parameters that obey a set of mild conditions), as opposed to demonstrating some truly unusual and inexplicable event that cries out for a more human-comprehensible analysis. In particular, if the computer-assisted claims in the proof already have a firm heuristic grounding then I think there is no problem with using computers to establish the claims rigorously. Of course, it is still worthwhile to look for human-readable proofs as well.

*M & P:* Is mathematics becoming a very dispersive area of knowledge?

Tao: Certainly mathematics has expanded at such a rate that it is no longer possible to be a universalist such as Poincaré and Hilbert. On the other hand, there has also been a significant advance in simplification and insight, so that mathematics that was mysterious in, say, the early twentieth century now appears routine (or more commonly, several difficult pieces of mathematics have been unified into a single difficult piece of mathematics, reducing the total complexity of mathematics significantly). Also, some universal heuristics and themes have emerged that can describe large parts of mathematics quite succinctly; for instance, the theme of passing from local control to global control, or the idea of viewing a space in terms of its functions and sections rather than by its points, lend a clarity to the subject that was not available in the days of Poincaré or Hilbert. So I remain confident that mathematics can remain a unified subject in the future, though our way of understanding it may change dramatically.

*M* & *P*: What fields of mathematics do you foresee will grow in importance, and maybe less positively, fade away?

Tao: I don't think that any good piece of mathematics is truly wasted; it may get absorbed into a more general or efficient framework, but it is still there. I think the next few decades of mathematics will be characterized by interdisciplinary synthesis of disparate fields of mathematics; the emphasis will be less on developing each field as deeply as possible (though this is of course still very important), but rather on uniting their tools and insights to attack problems previously considered beyond reach. I also see a restoration of balance between formalism and intuition, as the former has been developed far more heavily than the latter in the last century or so, though intuition has seen a resurgence in more recent decades.

*M* & *P*: There are lots of definitions of randomness. Do you think there is a satisfying way of thinking of randomness?

Tao: I do see the dichotomy between structure and randomness exhibiting itself in many fields of mathematics, but the precise way to define and distinguish these concepts varies dramatically across fields. In some cases, it is computational structure and randomness that is decisive; in other cases, it is a statistical (correlation-based) or ergodic concept of structure and randomness, and in other cases still it is a Fourier-based division. We don't yet have a proper axiomatic framework for what a notion of structure or randomness looks like (in contrast to, say, the axioms for measurability or convergence or multiplication, which are well understood). My feeling is that such a framework will eventually exist, but it is premature to go look for it now.

*M* & *P*: If you were not to have been a mathematician, what career would you have considered?

**Tao:** I think if I had not become a mathematician, I would like to be involved in some other creative, problem-solving, autonomous occupation, though I find it hard to think of one that matches the job satisfaction I get from mathematics.

## Wendelin Werner

### M & P:Were you always interested in mathematics?

**Werner:** Well, as far as I can remember, math was always my preferred topic at school, and I was a rather keen board-games player in my childhood (maybe this is why I now work on 2-dimensional problems?). As a child, when I was asked if I knew what I wanted to be later, I responded "astronomer". In high school, just because of coincidences, I ended up playing in a movie and having the possibility to try to continue in this domain, but I remember vividly that I never seriously considered it, because

I preferred the idea of becoming a scientist, even if at the time, I did not know what it really meant. When it was time to really choose a subject, I guess I realized that mathematics was probably closer to what I liked about astronomy (infiniteness, etc.).

*M* & *P*: Have you known about the Fields Medal since an early age, and did it in any way motivate you? In particular what was your feeling when you were told about being a medalist?

Werner: I learned about the existence of the Fields Medal quite late (when I graduated roughly). In fact, I remember some friends telling me halfjokingly, half-seriously, that "you will never get the Fields Medal if you do this" when I told them that I was planning to specialize in probability theory (it is true that this field had never been recognized before this year). It is of course a nice feeling to get this medal today, but it is also very strange: I really do not feel any different or "better" than other mathematicians, and to be singled out like this, while there exist so many great mathematicians who do not get enough recognition is almost embarrassing. It gives a rather big responsibility, and I will now have to be careful each time I say something (even now). But again, it is nice to get recognition for one's work, and I am very happy. Also, I take it as a recognition for my collaborators (Greg Lawler and Oded Schramm) and for the fact that probability theory is a nice and important field in contemporary mathematics.

I guess that all these feelings and thoughts were present in my head when I hung up the phone after learning from John Ball in late May that I was awarded the medal. I knew that it was a possibility, but nevertheless it took me by surprise.

*M* & *P*:*A*re there some mathematicians who you admire particularly?

**Werner:** I am not a specialist of history of mathematics, but I find it amazing what the great nineteenth century mathematicians (Gauss, Riemann, ...) managed to work out—I certainly feel like a dwarf compared to giants. I have also the greatest respect for those who shaped probability theory into what it is now (Kyoshi Itô, Paul Lévy, Ed Harris, Harry Kesten, to mention just a few). Also, I owe a lot to the generation of probabilists who are just a little older than I am (just look at the list of Loève prize winners for instance. I really felt very honored to be on that list!) and opened so many doors.

*M* & *P*: Do you fear that the Fields Medal will inhibit you by putting up too high expectations for future work?

**Werner:** It is true that in a way, the medal puts some pressure to deliver nice work in the future and that it will probably be more scrutinized than before. On the other hand, it gives a great liberty to think about hard problems, to be generous with ideas and time with Ph.D. students for instance. We shall see how it goes.

*M & P:* As you pointed out, your chosen subject has never been awarded a medal before. Is it because it has been considered as "applied mathematics"? Would you call yourself an "applied mathematician"?

Werner: Probability theory has long been considered as part of applied mathematics, maybe also because of some administrative reasons (in the U.S., probabilists often work in statistics departments that are disjoint from the mathematics departments). This has maybe led to a separate development of this field, slightly isolated. Now people realize how fruitful interactions between probabilistic ideas and other fields in mathematics can be, and this automatic "applied" notion is fading away (even if probability can be indeed fruitfully applied in many ways). In a way, the field that I am working on has been really boosted by the combination of complex analytic ideas with probability (for instance Schramm's idea to use Loewner's equation in a probabilistic context to understand conformally invariant systems). I personally never felt that I was doing "applied" mathematics. It is true that we are using ideas, intuitions, and analogies from physics to help us to get some intuition about the concepts that we working on. Brownian motion is a mathematical concept with something that resonates in us, gives us some intuition about it. and stimulates us.

*M & P:* Is there any risk that computers will make mathematicians obsolete, say by providing computerized proofs? Or do you believe this will stimulate mathematics instead?

**Werner:** Well, one of my brothers is working precisely on computer-generated or computer-checked proofs. I have to be careful about what I say, especially since my own knowledge on this is very thin. I personally do not really use the computer in my work, besides T<sub>E</sub>X and the (too long) time spent with emailing. It can very well be that some day soon, computers may be even more efficient than now in helping understanding and proving things. The past years have shown how things that looked quite out of reach ended up being possible with computers.

*M & P:* Do you have any other interests besides mathematics?

**Werner:** I often go to concerts (classical music) and play (at a nonprofessional level, though) the violin. Often, I hear people saying "yes, math and music are so similar, that is why so many mathematicians are also musicians". I think that this is only partially true. I cannot forget that many of those I was playing music with as a child simply had to stop playing as adults because their profession did not leave any time or energy to continue to practice their instruments: doctors usually have many more working hours than we do. Also, music is nicely compatible with mathematics because—at least for me—it is hard to concentrate on a math problem more than 4-5 hours a day, and music is a good complementary activity: it does not fill the brain with other concerns and problems that distract from math. It is hard to do math after having had an argument with somebody about non-mathematical things, but after one hour of violin scales, one is in a good state of mind.

Also, but this is a more personal feeling, with the years, I guess that what I am looking for in music becomes less and less abstract and analytical and more and more about emotions—which makes it less mathematical...

But I should also mention that, as far as I can see it, mathematics is simultaneously an abstract theory and also very human: When we work on mathematical ideas, we do it because in a way, we like them, because we find something in them that resonates in us (for different reasons, we are all different). It is not a dry subject that is separate from the emotional world. This is not so easy to explain to nonmathematicians, for whom this field is just about computing numbers and solving equations.