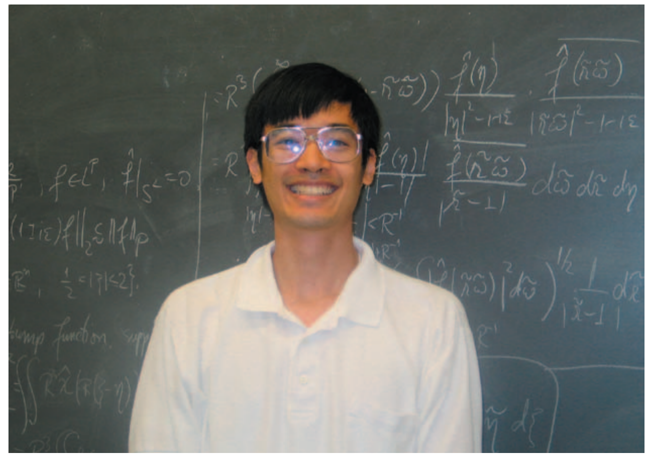
CMI Profile

# Interview with Research Fellow Terence Tao



*Terence Tao (b. 1975), a native of Adelaide, Australia, graduated from Flinders University at the age of 16 with a B.Sc. in Mathematics. He received his Ph.D. from Princeton University in June 1996 under the direction of Elias Stein. Tao then took a teaching position at UCLA where he was assistant professor until 2000, when he was appointed full professor. Since July 2003, Tao has also held a professorship at the Mathematical Sciences Institute Australian National University, Canberra.*

*Tao began a three-year appointment as a Clay Research Fellow (Long-Term Prize Fellow) in 2001. In 2003, CMI awarded Tao the Clay Research Award for his contributions to classical analysis and partial differential equations, as well as his solution with Alan Knutson of Horn's conjecture, a fundamental problem about the eigenvalues of Hermitian matrices. Tao is the author of eighty papers, concentrated in classical analysis and partial differential equations, but ranging as far as dynamical systems, combinatorics, representation theory, number theory, algebraic geometry, and ring theory. Three-quarters of his papers have been written with one or more of his thirty-three collaborators.*

## Interview

*From an early age, you clearly possessed a gift for mathematics. What stimulated your interest in the subject, and when did you discover your talent for mathematical research? Which persons influenced you the most?*

Ever since I can remember, I have enjoyed mathematics; I recall being fascinated by numbers even at age three, and viewed their manipulation as a kind of game. It was only much later, in high school, that I started to realize that mathematics is not just about symbolic manipulation, but has useful things to say about the real world; then, of course, I enjoyed it even more, though at a different level.

My parents were the ones who noticed my mathematical ability, and sought the advice of several teachers, professors, and education experts; I myself didn't feel anything out of the ordinary in what I was doing. I didn't really have any other experience to compare it to, so it felt natural to me. I was fortunate enough to have several good mentors during my high-school and college years who were willing to spend time with me just to discuss mathematics at a leisurely pace. For instance, there was a retired mathematics professor, Basil Rennie (who sadly died a few years ago), whom I would visit each weekend to talk about recreational mathematics over tea and cakes. At the local university, Garth Gaudry also spent a lot of time with me and eventually became my masters thesis advisor. He was the one who got me working in analysis, where I still do most of my mathematics, and who encouraged me to study in the US. Once in graduate school, I benefitted from interaction with many other mathematicians, such as my advisor Eli Stein. But the same would be true of any other graduate student in mathematics.

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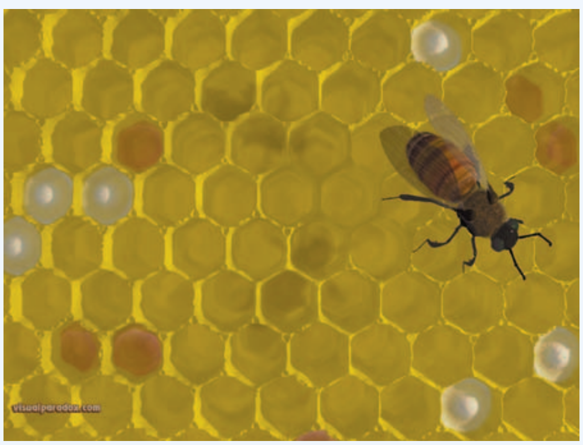
*What is the primary focus of your research today? Can you comment on the results of which you are most fond?*

I work in a number of areas, but I don't view them as being disconnected; I tend to view mathematics as a unified subject and am particularly happy when I get the opportunity to work on a project that involves several fields at once. Perhaps the largest "connected component" of my research ranges from arithmetic and geometric combinatorics at one end (the study of arrangements of geometric objects such as lines and circles, including one of my favorite conjectures, the Kakeya conjecture, or the combinatorics of addition, subtraction and multiplication of sets), through harmonic analysis (especially the study of oscillatory integrals, maximal functions, and solutions to the linear wave and Schrödinger equations), and ends up in nonlinear PDE (especially nonlinear wave and dispersive equations).

Currently my focus is more at the nonlinear PDE end of this range, especially with regard to the global and asymptotic behavior of evolution equations, and also with the hope of combining the analytical tools of nonlinear PDE with the more algebraic tools of completely integrable systems at some point. In addition, I work in a number of areas adjacent to one of the above fields; for instance I have begun to be interested in arithmetic progressions and connections with number theory, as well as with other aspects of harmonic analysis such as multilinear integrals, and other aspects of PDE, such as the spectral theory of Schrödinger operators with potentials or of integrable systems.



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Finally, with Allen Knutson, I have a rather different line of research: the algebraic combinatorics of several related problems, including the sum of Hermitian matrices problem, the tensor product muliplicities of representations, and intersections of Schubert varieties. Though we only have a few papers in this field, I still

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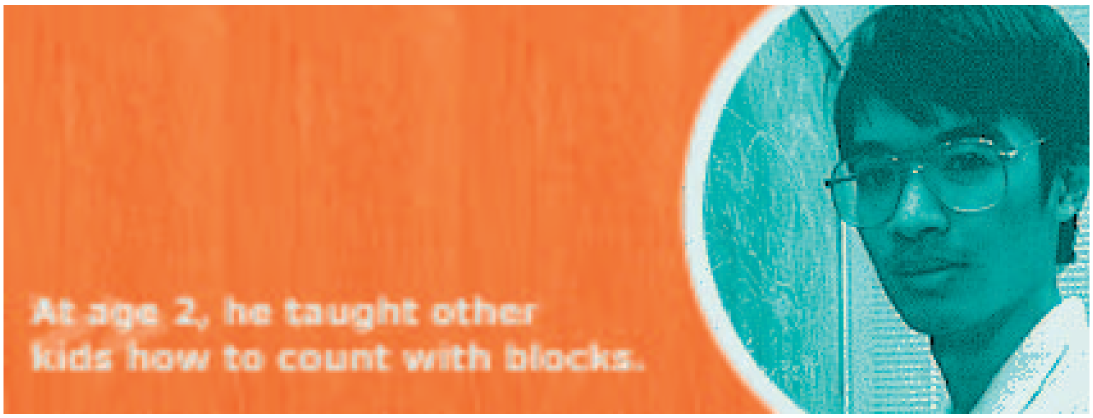
count this as one of my favorite areas to work in. This is because of all the unexpected structure and algebraic "miracles" that occur in these problems, and also because it is so technically and conceptually challenging. Of course, I also enjoy my work in analysis, but for a different reason. There are fewer miracles, but instead there is lots of intuition coming from physics and from geometry. The challenge is to quantify and exploit as much of this intuition as possible.

In analysis, many research programs do not conclude in a definitive paper, but rather form a progression of steadily improving partial results. Much of my work has been of this type (especially with regard to the Kakeya problem and its relatives, still one of my primary foci of research). But I do have two or three results of a more conclusive nature with which I feel particularly satisfied. The first is my original paper with Allen Knutson, in which we characterize the eigenvalues of a sum of two Hermitian matrices, first by reducing it to a purely geometric combinatorial question (that of understanding a certain geometric configuration called a "honeycomb"), and then by solving that question by a combinatorial argument. (There have since been a number of other proofs and conceptual clarifications, although the exact role of honeycombs remains partly mysterious.) The second is my paper on the small energy global regularity of wave maps to the sphere in two dimensions, in which I introduce a new "microlocal" renormalization in order to turn this rather nonlinear problem into a more manageable semilinear evolution equation. While the result in itself is not yet definitive (the equation of general target manifolds other than the sphere was done afterward, and the large energy case remains open, and very interesting), it did remove a psychological stumbling block by showing that these critical wave equations were not intractable. As a result there has been a resurgence of interest in these equations. Finally, I have had a very productive and enjoyable collaboration with Jim Colliander, Markus Keel, Gigliola Staffilani, and Hideo Takaoka, culminating this year in the establishment of global regularity and scattering for a critical nonlinear Schrödinger equation (for large energy data); this appears to be the first unconditional global existence result for this type of critical dispersive equation. The result required assembling and then refining several recent techniques developed in this field, including an induction-on-energy approach pioneered by Bourgain, and a certain interaction Morawetz inequality we had discovered a few years earlier. The result seems to reveal some new insights into the dynamics of such equations. It is still in its very early days, but I feel confident that the ideas developed here will have further application to understanding the large energy behavior of other nonlinear evolution equations. This is a topic I am still immensely interested in.




*UCLA Spotlight Feature from the UCLA Website, Courtesy of Reed Hutchinson, UCLA Photographic Services*

*You have worked on problems quite far from the main focus of your research, e.g., Horn's conjecture. Could you comment on the motivation for this work and the challenges it presented? On your collaborations and the idea of collaboration in general? Can a mathematician in this day of specialization hope to contribute to more than one area?*

My work on Horn's conjecture stemmed from discussions I had with Allen Knutson in graduate school. Back then we were not completely decided as to which field to specialize in and had (rather naively) searched around for interesting research problems to attack together. Most of these ended up being discarded, but the sum of Hermitian matrices problem (which we ended up working on as a simplified model of another question posed by another graduate student) was a lucky one to work on, as it had so much unexpected structure. For instance, it can be phrased as a moment map problem in symplectic geometry, and later we realized it could also be quantized as a multiplicity problem in representation theory. The problem has the advantage of being elementary enough that one can make a fair bit of progress without too much machinery – we had begun deriving various inequalities and other results, although we eventually were a bit disappointed to learn

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that we had rediscovered some very old results of Weyl, Gelfand, Horn, and others). By the time we finished graduate school, we had gotten to the point where we had discovered the role of honeycombs in the problem. We could not rigorously prove the connection between honeycombs and the Hermitian matrices problem, and were otherwise stuck. But then Allen learned of more recent work on this problem by algebraic combinatorialists and algebraic geometers, including Klyachko, Totaro, Bernstein, Zelevinsky, and others. With the more recent results from those authors we were able to plug the missing pieces in our argument and eventually settle the Horn conjecture.

Collaboration is very important for me, as it allows me to learn about other fields, and, conversely, to share what I have learned about my own fields with others. It broadens my experience, not just in a technical mathematical sense but also in being exposed to other philosophies of research, of exposition, and so forth. Also, it is considerably more fun to work in groups than by oneself. Ideally, a collaborator should be close enough to one's own strengths that one can communicate ideas and strategies back and forth with ease, but far enough apart that one's skills complement rather than replicate each other.

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It is true that mathematics is more specialized than at any time in its past, but I don't believe that any field of mathematics should ever get so technical and complicated that it could not (at least in principle) be accessible to a general mathematician after some patient work (and with a good exposition by an expert in the field). Even if the rigorous machinery is very complicated, the ideas and goals of a field are often so simple, elegant, and natural that I feel it is frequently more than worth one's while to invest the time and effort to learn about other fields. Of course, this task is helped immeasurably if you can talk at length with someone who is already expert in those areas; but again, this is why collaboration is so useful. Even just attending conferences and seminars that are just a little bit outside your own field is useful. In fact, I believe that a subfield of mathematics has a better chance of staying dynamic, fruitful, and exciting if people in the area do make an effort to write good surveys and expository articles that try to reach out to other people in neighboring disciplines and invite them to lend their own insights and expertise to attack the problems in the area. The need to develop fearsome and impenetrable machinery in a field is a necessary evil, unfortunately, but as understanding progresses it should not be a permanent evil. If it serves to keep away other skilled mathematicians who might otherwise have useful contributions to make, then that is a loss for mathematics. Also, counterbalancing the trend toward increasing complexity and specialization at the cutting edge of mathematics is the deepening insight and simplification of mathematics at its common core. Harmonic analysis, for instance, is a far more organized and intuitive subject than it was in, say, the days of Hardy and Littlewood; results and arguments are not isolated technical feats but instead are put into a wider context of interaction between oscillation, singularity, geometry, and so forth. PDE also appears to be undergoing a similar conceptual organization, with less emphasis on specific techniques such as estimates and choices of function spaces, and instead sharing more in common with the underlying geometric and physical intuition. In some ways, the accumulated rules of thumb, folklore, and even just some very good choices of notation can make it easier to get into a field nowadays. (It depends on the field, of course; some have made far more progress with conceptual simplification than others).

*Godfrey Harold Hardy (1877–1947)*
*reproduction from **Remarkable Mathematicians** by Ioan James, © Ioan James 2002, University Press, Cambridge.*

*How has your Clay fellowship made a difference for you?*

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The Clay Fellowship has been very useful in granting a large amount of flexibility in my travel and visiting plans, especially since I was also subject to certain visa restrictions at the time. For instance, it has made visiting Australia much easier. Also I was supported by CMI on several trips to Europe and an extended stay at Princeton, both of which were very useful to me mathematically, allowing me to interact and exchange ideas with many other mathematicians (some of whom I would later collaborate with).

*Recently you received two honors: the AMS Bôcher Memorial Prize and the Clay Research Award, for results that distinguish you for your contributions to analysis and other fields. Have your findings opened up new areas or spawned new collaborations? Who else has made major contributions to this specific area of research?*

The work on wave maps (the main research designated by the Bôcher prize) is still quite active; after my own papers there were further improvements and developments by Klainerman, Rodnianski, Shatah, Struwe, Nahmod, Uhlenbeck, Stefanov, Krieger, Tataru, and others. (My work in turn built upon earlier work of these authors as well as Machedon, Selberg, Keel, and others). Perhaps more indirectly, the mere fact that critical nonlinear wave equations can be tractable may have helped encourage the parallel lines of research on sister equations such as the Einstein, Maxwell-Klein-Gordon, or Yang-Mills equation. This research is also part of a larger trend where the analysis of the equations is moving beyond what can be achieved with Fourier analysis and energy methods, and is beginning to incorporate more geometric ideas (in particular, to use ideas from Riemannian geometry to control geometric objects such as connections and geodesics; these in turn can be used to control the evolution of the nonlinear wave equation).


*James Clerk Maxwell (1831–1879)*
*Courtesy of Smithsonian Institution Libraries, Washington, DC*

The Clay award recognized not only the work on wave maps, but also on sharp restriction theorems for the Fourier transform, which was an area pioneered by such great mathematicians as Carleson, Sjolin, Tomas, Stein, Fefferman, and Cordoba almost thirty years ago, and which has been invigorated by more recent work of Bourgain, Wolff, and others. These problems are still not solved fully; this would require, among other things, a complete solution to the Kakeya conjecture. The relationship of these problems both to geometry and to PDE has been greatly clarified however, and the technical tools required to make concrete these connections are also much better understood. Recent work by Vargas, Lee, and others continue to develop the theory of these estimates.

The Clay award also mentioned the work on honeycombs and Horn's conjecture. Horn's conjecture has now been proven in a number of ways (thanks to later work by Belkale, Buch, Weyman, Derksen, Knutson, Totaro, Woodward, Fulton, Vakil and others), and we are close to a more satisfactory geometric understanding of this problem. Lately, Allen and I have been more interested in the connection with Schubert geometry, which is connected to a discrete analogue of a honeycomb that we call a "puzzle." These puzzles seem to encode in some compact way the geometric combinatorics of Grassmannians and flag varieties, and there is some exciting work of Knutson and Vakil that seems to "geometrize" the role of these puzzles (and the combinatorics of the Littlewood-Richardson rule in general) quite neatly. There is also some related work of Speyer that may shed some light on one of the more mysterious combinatorial aspects of these puzzles, namely that they are "associative."

*What research problems are you likely to explore in the future?*

It's hard to say. As I said before, even five years ago I would not really have imagined working on what I am doing now. I still find the problems related to the Kakeya problem fascinating, as well as anything to do with honeycombs and puzzles. But currently I am more involved in nonlinear PDE, with an eye toward moving toward integrable systems. Related to this is a long-term joint research project with Christoph Thiele on the nonlinear Fourier transform (also known as the scattering transform) and its connection with integrable systems. I am also getting interested in arithmetic progressions and

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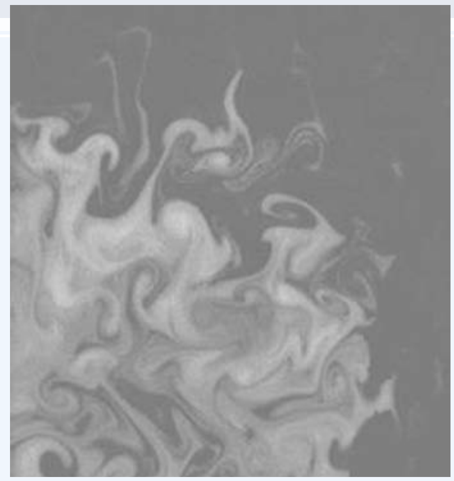
their connections with combinatorics, number theory, and even ergodic theory. I have also been learning bits and pieces of differential geometry and algebraic geometry and may take more of an interest in those fields in the future. Certainly at this point I have more interesting directions to pursue than I have time to work with!

***What are your thoughts on the Millennium Prize Problems, the Navier-Stokes Equation, for example?***

The prize problems are great publicity for mathematics, and have made the recent possible resolution of Poincaré'sconjecture – which is already an amazing and very important mathematical achievement – much more publicized and exciting than it already was. It is unclear how close the other problems are to resolution, though they all have several major obstructions that need to be resolved first. For Navier-Stokes, one of the major obstructions is turbulence. This equation is “supercritical,” which roughly means that the energy can interact much more forcefully at fine scales that it can at coarse scales (in contrast to subcritical equations where the coarse scale behavior dominates, and critical equations where all scales contribute equally). As yet we do not have a good large data global theory for any supercritical equation, let alone Navier-Stokes, without some additional constraints on the solution to somehow ameliorate the behavior of the fine scales. A new technique that would allow us to handle very turbulent solutions effectively would be a major achievement. Perhaps one hope lies in the stochastic models of these flows, although it would be a challenge to show that these stochastic models really do model the deterministic Navier-Stokes equation properly.


*Flourescent dye dispersed by a two-dimensional turbulent flow Courtesy of Marie-Caroline Julllien and Patrick Tabeling, Ecole Normale Supérieure, Paris*

Again, there are many sister equations of Navier-Stokes, and it may well be that the ultimate solution to this problem may lie in first understanding a related model of equations – the Euler equations, for instance. Even Navier-Stokes is itself a model for other, more complicated, fluid dynamics. So while Navier-Stokes is certainly an important equation in fluid equations, there should not be given the impression that the Clay prize problem is the only problem worth studying here.

**The full text of Tao's interview can be found at: www.claymath.org/interviews/**

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