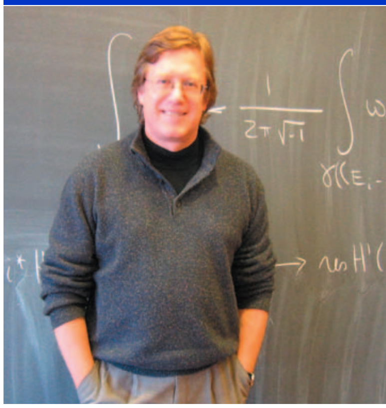
Contents

Clay Mathematics Institute

The Year 2003

Letter from the President



Dear Friends of Mathematics:

It has been a great pleasure working at the Clay Mathematics Institute the past eight months. Rarely does a mathematician have the opportunity to serve the discipline in such a capacity, and I look forward with great anticipation to what we can accomplish during the next few years. There are three areas to which I am devoting special attention: increasing public awareness and understanding of mathematics, support of individual researchers, and programs that attract talented young people to mathematics.

CMI offices in Cambridge

Concerning the first point, our goal is a gradual but definitive change in the public perception of mathematics. It should be part of our shared culture that mathematics is not a static, finished subject, but one that abounds in unsolved problems and grand intellectual challenges. It should be common knowledge that mathematics provides a deep and powerful way of thinking about the world, one that is extraordinarily useful in fields as diverse as physics and finance, mineral exploration and medical imaging. The Millennium Prize Problems take a significant step toward this goal: With almost no expenditure of resources, the level and frequency of public conversation about mathematics has been raised. But there is much more to be done, and, of course, much more than any single institution can do. For its part, CMI will continue to sponsor public lectures, commission publications, organize events, and contribute to other initiatives. With time, we hope any schoolchild, layman, or professional will be able to call to mind an important mathematician or an important mathematical result, just as today we call to mind the names of Einstein or Edison when physics or invention is mentioned. With time, we should all have a notion of the mathematics which lies behind technologies like those of the Google search engine or internet commerce – or at least we should all know that behind each of these technologies stands, invisible but powerful, a piece of mathematics.

Who is it that solves the difficult problems and creates new mathematical knowledge? It is, of course, the individual mathematician, conversing with colleagues, perservering to find insight and bring it to fruition. Therefore no effort can be more important than the identification and support of promising mathematical researchers. It is to such support – support of its Research Fellows and Scholars – that CMI devotes, and will continue to devote, by far the greatest part of its budget. Fellows, currently ten in number, are appointed for a period of two to five years and select whichever host institution best suits their needs. Fellows define their own research program, travel and pursue collaborations anywhere in the world, supported by CMI.

Research mathematicians do not, of course, arise by spontaneous generation or intervention of miracle. At one time each was a student with a budding interest in this beautiful, challenging, and rewarding subject. It is vital that we identify such students and give them the intellectual sustenance and stimulation needed to thrive, to grow to their full potential. It is obvious in our society that this should be done in athletics and in music, but it is just as true in mathematics. Through its partnership with the Ross and PROMYS programs, its involvement in the Mathematics Olympiad, through the Clay Research Academy held each spring in Cambridge, and through other initiatives, CMI continues to nurture young people's interest and development in mathematics.

We look forward to the coming year and the new mathematics that it will bring.

Sincerely,

James A. Carlson, President

The 2003 Clay Research Awards:

Richard Hamilton

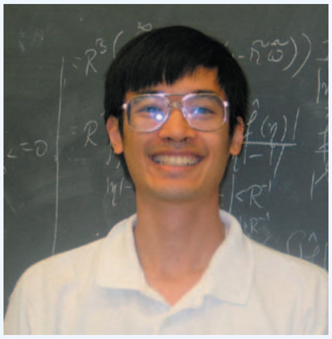
Terence Tao

Each year CMI recognizes outstanding achievements in mathematics with the Clay Research Award. In 2003, the award went to Richard Hamilton for his visionary work on Ricci flow and to Terence Tao for his fundamental contributions to analysis and other fields.

Richard Hamilton of Columbia University was recognized for his introduction of the Ricci flow equation and his development of it into one of the most powerful tools in geometry and topology. The Ricci flow equation is a system of nonlinear partial differential equations somewhat analogous to the classical (scalar) heat equation. However, the quantity that evolves is not temperature, but rather the metric on a manifold, that is, its geometry. In his seminal 1982 paper, Hamilton showed that a compact manifold with positive Ricci curvature evolves toward a state of constant curvature. This theorem is the basis of his visionary program to prove William Thurston's geometrization conjecture, of which the celebrated Poincaré conjecture is a special case. Recent work by Grigori Perelman of St. Petersburg has spectacularly advanced these ideas and brought us much closer to an understanding of the conjectures.

Terence Tao of UCLA was recognized for his groundbreaking work in analysis, notably his optimal restriction theorems in Fourier analysis, his work on the wave map equation (the hyperbolic analogue of the harmonic map equation), his global existence theorems for KdV type equations, as well as significant work in quite distant areas of mathematics, such as his solution with Allen Knutson of Horn's conjecture, a fundamental problem about hermitian matrices that goes back to questions posed by Hermann Weyl in 1912.

Richard Hamilton
© 2003 Allison Evans



Terence Tao

The awards were presented at the CMI Annual Meeting on November 14, 2003. MIT hosted the event, which was held at its Media Lab.

Richard Hamilton receiving the award from CMI President Jim Carlson
© 2003 Allison Evans

Shing-Tung Yau recognizing the contributions of Richard Hamilton
© 2003 Allison Evans

David Jerison, who received Terence Tao's award on his behalf, joined by Landon T. Clay. © 2003 Allison Evans

Birch and Swinnerton-Dyer Conjecture

Hodge Conjecture

Navier-Stokes Equation

P versus NP

Poincaré Conjecture

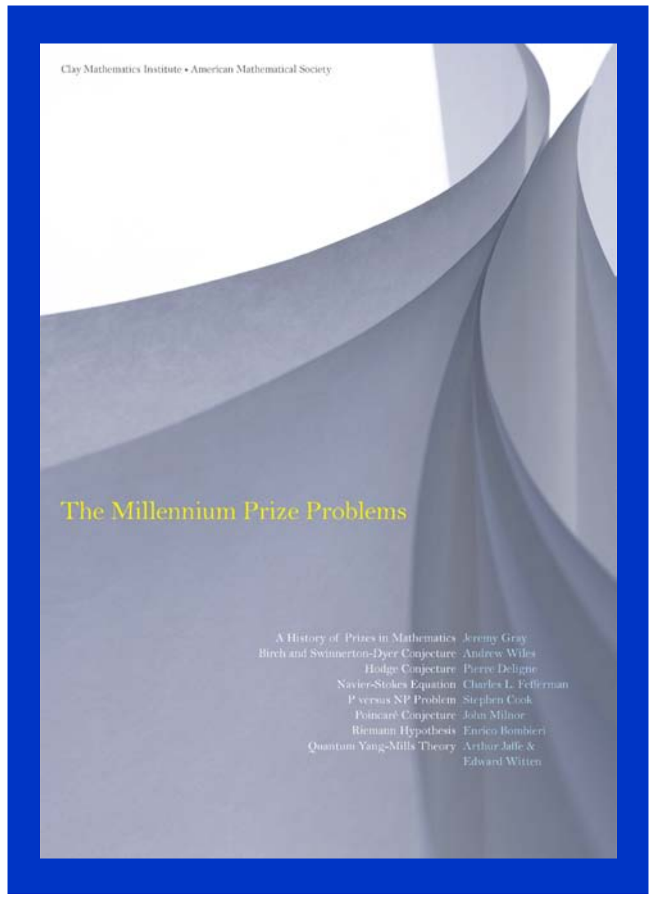
Riemann Hypothesis

Yang-Mills and Mass Gap

HISTORY

On August 9, 1900, at the second International Congress of Mathematicians in Paris, David Hilbert delivered his famous lecture in which he described 23 problems that were to play an influential role in the mathematical research of the coming century. One hundred years later, on May 24, 2000, at a meeting at the Collège de France, the Clay Mathematics Institute announced the creation of a $7 million prize fund for the solution of seven important classic problems that have resisted solution. The prize fund is divided equally among the seven problems, and there is no time limit for their solution.

The Millennium Prize Problems were selected by the founding Scientific Advisory Board of CMI – Alain Connes, Arthur Jaffe, Andrew Wiles, and Edward Witten – after consulting with other leading mathematicians. Their aim was somewhat different than that of Hilbert: not to define new challenges, but to record some of the most difficult issues with which mathematicians were struggling at the turn of the second millennium; to recognize achievement in mathematics of historical dimension; to elevate in the consciousness of the general public the fact that in mathematics, the frontier is still open, and abounds in important unsolved problems; and to emphasize the importance of working toward a solution of the deepest, most difficult problems.



PUBLICATION

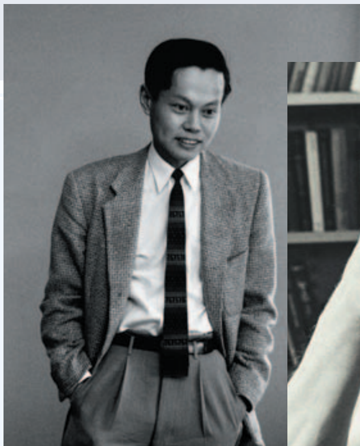
Later this year, CMI and the American Mathematical Society will jointly publish *The Millennium Prize Problems*, which gives the official descriptions of each problem as well as the rules for awarding the prizes. The book includes an essay on the history of prize problems in mathematics by Jeremy Gray.

Problems have long been regarded as the life of mathematics. A good problem focuses attention on something mathematicians would like to know but presently do not. This missing knowledge might be eminently practical, it might be wanted entirely for its own sake, its absence might signal a weakness of existing theory – there are many reasons for posing problems. A good problem is one that defies existing methods, for reasons that may or may not be clear, but whose solution promises a real advance in our knowledge.

–Jeremy Gray

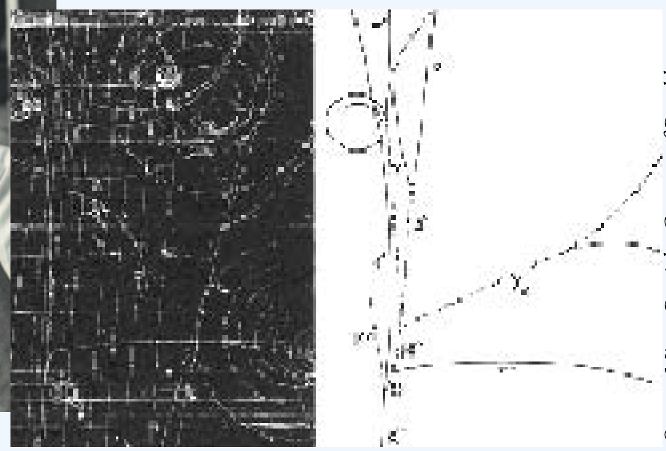
Report on the status of the Yang-Mills problem

by Michael R. Douglas, Professor of Physics at Rutgers University



Chen Ning Yang (b. 1922)
© Estate of François Bello, Science Photo Library, Photo Researchers, Inc.

Robert Leroy Mills (b. 1927)
Ohio State University Photo Services



Omega Minus Particle, Courtesy of Brookhaven National Laboratory

Yang-Mills Existence and Mass Gap: *Prove that for any compact simple gauge group G, quantum Yang-Mills theory of $\mathbb{R}^4$ exists and has a mass gap $\Delta > 0$.*

As explained in the official CMI problem description set out by Arthur Jaffe and Edward Witten, Yang-Mills theory is a generalization of Maxwell's theory of electromagnetism, in which the basic dynamical variable is a connection on a G-bundle over four-dimensional space-time. Its quantum version is the key ingredient in the Standard Model of the elementary particles and their interactions, and a solution to this problem would both put this theory on a firm mathematical footing and demonstrate a key feature of the physics of strong interactions.

To illustrate the difficulty of the problem, we might begin by comparing it to the study of classical Yang-Mills theory. Mathematically, this is a system of non-linear partial differential equations, obtained by extremizing the Yang-Mills action,

$$S = \int \mathrm{Tr}\ F \wedge *F,$$

where $F = dA + A \wedge A$ is the curvature of the G-connection A. Perhaps the most basic mathematical question here is to specify a class of initial conditions for which we can guarantee existence and uniqueness of solutions. Among the qualitative properties of these solutions, one with some analogies to the "mass gap" problem would be to establish or falsify the existence of solitonic solutions, whose energy density remains localized for all times (in fact, these are not believed to exist). Such questions have seen a great deal of mathematical progress in recent years, using techniques that are founded on the well-developed theory of linear PDE's.

By contrast, there is at present no satisfactory mathematical definition of the quantum Yang-Mills theory, because of the famous difficulties of renormalization. Conceptually, the simplest starting point for

Continued on page 16

Summary of 2003 Research Activities

The researchers engaged by CMI and the programs it conducts are selected and approved by CMI's Scientific Advisory Board. The Board consists of Jim Carlson, Simon Donaldson, Gregory Margulis, Richard Melrose, Yum-Tong Siu, and Andrew Wiles.

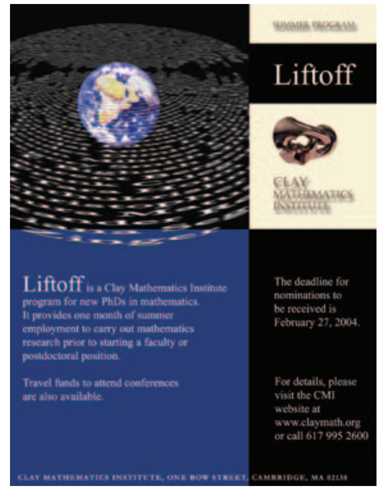
In 2003, CMI increased to 10 the number of its Clay Research Fellows (formerly Long-Term Prize Fellows). Elon Lindenstrauss of Stanford (Ph.D. from The Hebrew University of Jerusalem) was appointed for two years, and began his work at the Courant Institute of Mathematics at New York University. Maria Chudnovsky, the first woman to hold the position, was appointed for five years and is continuing her work in Princeton. Fellows conduct their work at whatever institution is most suited to the advancement of their research. CMI defrays their research expenses, including travel to conferences and work with collaborators.

Elon Lindenstauss
© 2003 Allison Evans

Maria Chudnovsky
© 2003 Allison Evans

CMI also appointed several distinguished Clay Research Scholars (formerly Prize Fellows) in 2003. Among them were Steven Zelditch (Johns Hopkins University), who organized a program at MSRI on semi-classical analysis, and Manindra Agrawal, winner of the 2002 Clay Research Award for his work on primality testing. Agrawal spent the past year at the Institute for Advanced Study.

In 2003, CMI established a new Clay Senior Scholars program. Senior Scholars are distinguished mathematicians who play a leading role in a topical program at an institute or university away from their home region, generally for a period of one to six months. The first senior scholars will be Richard Stanley (MIT) and Bernd Sturmfels (UC Berkeley), who will lead the program in geometric combinatorics at IAS/Park City Mathematics Institute (PCMI) in July, 2004. Nominations for the program are due August 1, 2004.



CMI appointed fifteen Liftoff Fellows in the summer of 2003, the same number as the previous year. Selected from nine institutions across the country, Liftoff Fellows are young mathematicians of exceptional promise who have just completed their Ph.D.

CMI appoints Book Fellows to write monographs on topics of current interest. Among the Fellows supported in 2003 were Yan Soibleman of Kansas State University and Alexander Braverman of Harvard University. Professor Braverman is working on a monograph on D-modules which is expected to appear in late 2004.

Among the Research Programs organized and supported by CMI in 2003 were the following:

- IPM String School and Workshop in Iran.
- Arnold Diffusion, a ten-week program at Princeton University.
- Non-commutative Geometry and Applications, a conference and school at Vanderbilt University.
- Arithmetic, Geometry and Topology of Algebraic Cycles, a conference in Morelia, Mexico.
- Unity of Mathematics, a conference in honor of I.M. Gelfand, Cambridge, Massachusetts.

Workshops & Conferences

High School Students

The 2003 CMI Summer School on Harmonic Analysis, the Trace Formula and Shimura Varieties was held at the Fields Institute in Toronto, June 2–27. 104 participants from more than ten countries attended. See page 19.

Fields Institute Summer School participants. Courtesy of Sonia Houle

The Clay Mathematics Research Academy, directed by Dr. David Ellwood and organized with Vida Salahi, was the first program held at CMI's new offices in Cambridge, Massachusetts. Twelve exceptional high-school students worked on original research problems for ten days with faculty leaders Richard Stanley (MIT) and Roger Howe (Yale), and four graduate and postdoctoral assistants. The program focused on combinatorics and geometry, and drew participants from across the country. CMI supported two student summer programs: PROMYS at Boston

Clay Research Academy session

Clay Research Academy students (left) and lecturer Sergei Gukov (right)

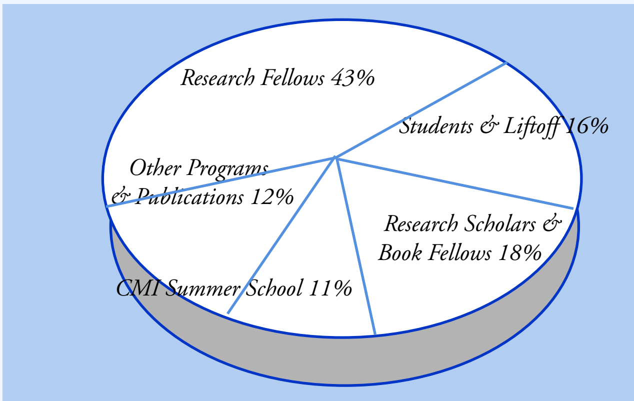
University and the Arnold Ross Program at Ohio State University. Both programs encourage talented high-school students to explore mathematics at a deeper level than in their regular courses and to consider mathematics as a career. CMI again presented an award at the USA Mathematical Olympiad ceremonies in Washington, DC, to the student with the most original solution in the American Mathematics Competitions. The 2003 Clay Olympiad Scholar was Tiankai Liu of Phillips Exeter Academy. Kwok Fong Tang, also of Phillips Exeter, and Anders Kaseorg of Charlotte Home Educators Association in Charlotte, NC, each received Honorable Mention.

Olympiad Scholar Tiankai Liu
© Robert Allen Strawn, 2003

CMI supported a project on dynamical systems at the Institute for Mathematical Sciences at SUNY Stony Brook led by John Milnor. Notable among the activities was a workshop on the work of Grigori Perelman.

Through its only direct grant, CMI provides substantial support to the mathematics program at the Independent University of Moscow (IUM). See p. 8.

The pie chart below sets forth research expenses for the fiscal year ending September 30, 2003, and reflects most of the programs and activities described above.



Research Expenses for Fiscal Year 2003
(comparative allocations change annually based on scientific merit)

The Independent University of Moscow

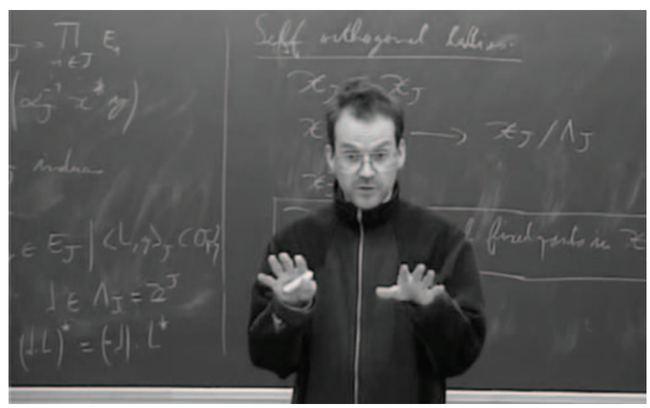
IUM students with Professors Paramonova and Lando

The Independent University of Moscow (IUM) is a small, elite institution in the Russian capital dedicated to the development of future research mathematicians. Founded in 1991 by distinguished mathematicians, many of whom are members of the Academy of Sciences, the IUM's philosophy of education follows the successful tradition of the Russian School in which students rediscover much of the material they learn. V.I. Arnold is the Chairman of its Academic Council. One of the principal aims of the IUM is to counteract the effects of Russian mathematical diaspora, both through retention of talent in Russia and through enhanced interchange with other mathematical communities.

> *We welcome everyone who has a passion for mathematics, is commited to a hard work and open to new ideas. – IUM*

Although the Russian government provided the IUM with a magnificent building in 1996, it does not receive permanent state operating funds. In the aftermath of the Russian financial crisis of August of 1998, faculty salaries dropped by 50%, and in 2000 CMI stepped in with support sufficient to make up the difference. Since that time CMI has increased its annual grant. CMI has also supported the research work and scientific exchanges of many mathematicians associated with IUM. During the 2002–2003 academic year, IUM conducted seventy courses and research seminars. No tuition is charged for attendance at IUM courses. Indeed, a large fraction of the students, one hundred twenty in number, received small stipends. Their education is supported by a library of 10,000 volumes.

Fields Medalist Laurent Lafforgue of IHES is a regular visitor to the IUM and a regular speaker in its Globus seminar, most active in Moscow. Following a fourth visit in November 2003, Professor Lafforgue commented "I am quite impressed by the mathematical activity there, especially by the young and extremely bright students, and the professors who teach them." As for the future, he adds "it is extremely important for world mathematicians that the Russian school flourish."



Laurent Lafforgue at IUM

Alexei Sossinski

Irina Paramonova

The IUM now sponsors a one-semester Math in Moscow program for foreign students and publishes the *Moscow Mathematical Journal*, distributed by the American Mathematical Society in the United States. These activities and a broadening base of financial support have stabilized IUM as a leading mathematical research institution.

Additional information about IUM, including its faculty and programs, can be obtained from its website, http://www.ium.mccme.ru/english/.

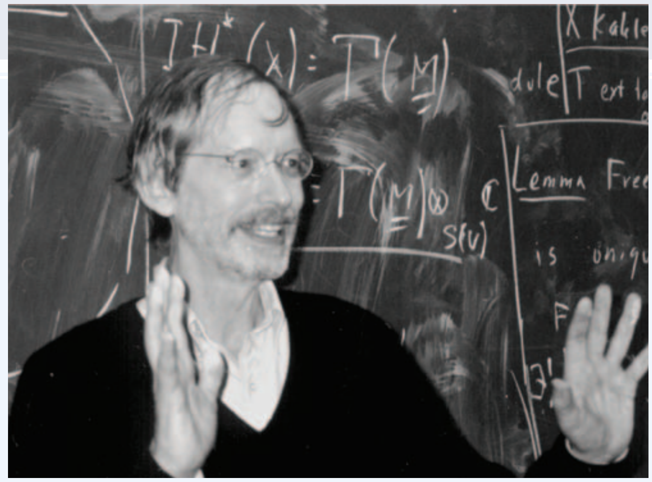
Sergey Lando

Sergei Novikov

In the Soviet Union pure mathematics was concentrated in Moscow and Leningrad, with only two places to get a good education: the State Universities of the two cities. Compared to the US, the training was very concentrated. Undergraduate training provided serious general mathematical education and research (for the last three years) under the guidance of the advisor of your choice; the next stage was graduate school (three or four years) which was pure research. It was common that good undergraduate students were producing research articles beginning with the third year of study.

Despite the fact that too many mathematicians left Russia – as with Germany in the 1930s – mathematics still lives there. The IUM deserves great credit for this.

— Sasha Beilinson



Bob MacPherson at IUM

The quality of the faculty and students at the IUM is extremely high. I taught two undergraduate classes there about ten years ago. The students comprised one of the two most stimulating groups of undergraduates I have had contact with. I would give them problems, and they would immediately start solving them, talking to their neighbors in the room. (The other undergraduate group was at the Ecole Normale Supérieure. The ENS has a program in which visiting mathematicians explain some of their work to first-year students. Of course, their first-year students are pretty advanced, by US standards.)

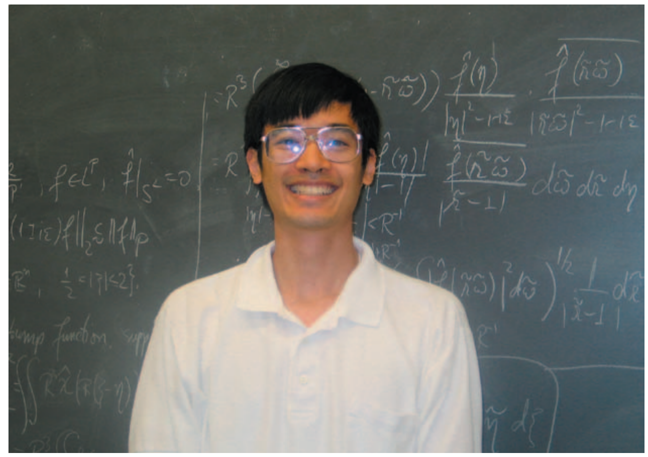
The operation at the Independent University depends on the Moscow tradition of teaching for the love of the subject, rather than for money. The ratio of quality and quantity of the work done to the total budget must be much higher at the Independent University than at any other institution in the world.

— Bob MacPherson

Our professors have designed and developed their own methods of teaching mathematics. We value independent thinking, open discussions, student initiative and hard work committed to the exploration of the universe of mathematics.

— IUM website

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.

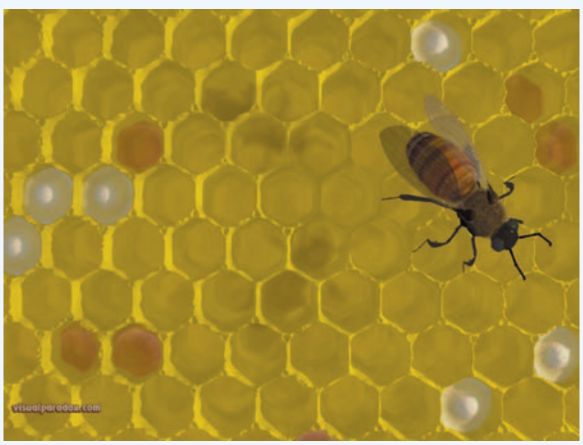
My parents were the ones who noticed my mathematical ability, and sought the advice of several teachers, professors, and education experts; I myself didn't feel anything out of the ordinary in what I was doing. I didn't really have any other experience to compare it to, so it felt natural to me. I was 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.

> Ever since I can remember, I have enjoyed mathematics; I remember being fascinated by numbers even at age three.

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).

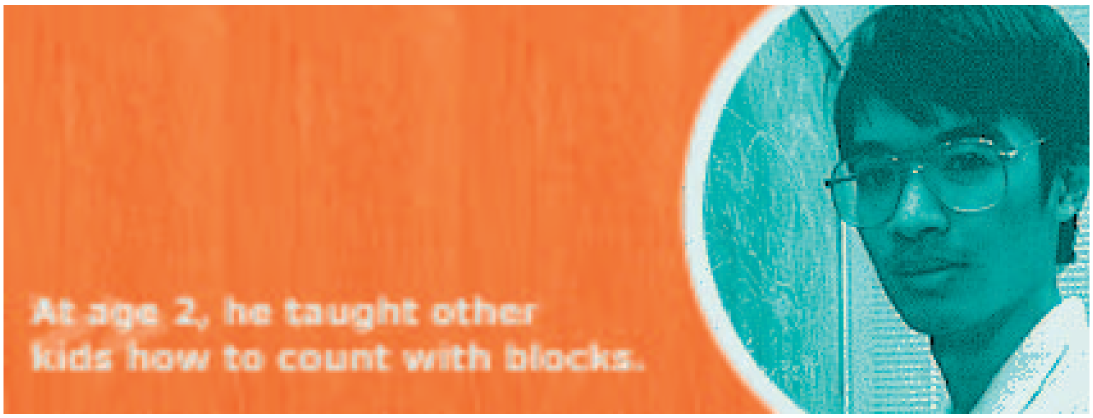
Currently my focus is more at the nonlinear PDE end of this range, especially with regard to the global and asymptotic behavior of evolution equations, and also with the hope of combining the analytical tools of nonlinear PDE with the more algebraic tools of completely integrable systems at some point. 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.



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 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.

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.

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



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

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.

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

> Collaboration is very important for me, as it allows me to learn about other fields, and, conversely to share what I have learnt 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 and exposition.

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.

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...

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

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).

How has your Clay fellowship made a difference for you?

Also, counterbalancing the trend towards increasing complexity and specialization at the cutting edge of mathematics is the deepening insight and simplifications of mathematics at its common core.

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?

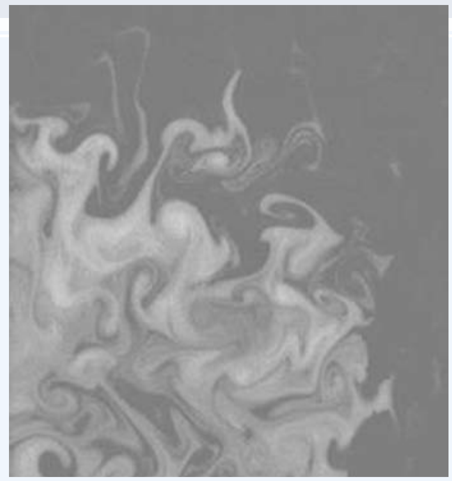
It's hard to say. As I said before, even five years ago I would not really have imagined working on what I am doing now. I still find the problems related to the Kakeya problem fascinating, as well as anything to do with honeycombs and puzzles. But currently I am more involved in nonlinear PDE, with 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

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.

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's conjecture – 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/

Recent Research Articles:

The primes contain arbitrarily long arithmetic progressions. Ben Green and Terence Tao (arXiv:math.NT/0404188)

Global well-posedness and scattering for the higher-dimensional energy-critical non-linear Schrödinger equation for radial data. Terence Tao (arXiv:math.AP/0402130)

Global well-posedness and scattering for the energy-critical nonlinear Schrödinger equation in R3. Jim Colliander, Mark Keel, Gigliola Staffilani, Hideo Takaoka, Terence Tao (arXiv:math.AP/0402129)

Report on the status of the Yang-Mills problem

by Michael R. Douglas

Continued from page 5

discussing this is probably to consider Yang-Mills theory on a "lattice," in other words a graph Γ with vertices, edges and a set of faces or "plaquettes," each of which is a closed loop in Γ. For example, we could take the vertices to be integral points $\mathbb{Z}^4 \subset \mathbb{R}^4$; the edges to be the straight lines connecting pairs of points at unit distance, and the plaquettes to be the loops of total length four. A G-connection on Γ is then a map from edges into G, and the curvature of the connection on a specified plaquette is (minus) the trace of the holonomy around that loop minus the identity matrix. The Yang-Mills action can again be taken to be the sum of squared curvatures.

Given this explicit description of the space of configurations, we can define "quantum lattice Yang-Mills" on a finite subgraph γ of Γ in terms of a functional integral,

$$Z[\gamma, g^2, G] = \int \prod dU_i \; e^{-\frac{1}{g^2}S},$$

where the integral is over all holonomies in γ, in other words all maps from edges into G; the measure is the product of Haar measure for the holonomy on each edge, and S is the Yang-Mills action. Finally, the real parameter g^2 is the "bare coupling constant," which much like Planck's constant controls the strength of quantum fluctuations.

The quantity Z is known as the partition function. The other quantities of physical interest are expectation values under this measure; for example, the joint expectation value of the curvatures on a pair or set of plaquettes.

Taking these finite-dimensional integrals as our starting point, the question of the existence of quantum Yang-Mills is essentially whether there is any sensible way to define the limit of the $Z[\gamma, g^2, G]$ over successively larger subgraphs $\gamma \subset \Gamma$, and thus to define a functional integral on Γ. Taking this limit will clearly involve renormalization, and this example was Wilson's primary motivation for his study of the renormalization group.

A great deal is known about the expected properties of the limit from a variety of physical arguments. Most importantly, in the limit of $g^2 \to 0$ and large γ, it is believed that the specific choice of Γ approximating $\mathbb{R}^4$ will not matter; the resulting expectation values will converge to "correlation functions in a continuum quantum field theory," satisfying formal properties that include invariance under the isometry group of the flat metric on $\mathbb{R}^4$, and others formalized in the Osterwalder-Schrader axioms. The additional axioms provide sufficient conditions for the construction of a Hilbert space and operator interpretation of the theory, analogous to the standard operator interpretation of quantum mechanics.

Establishing these axioms is the "existence" part of the problem, while the "mass gap" claim involves the fall-off of correlation functions with distance, as explained in more detail in Jaffe and Witten's problem description. We should say that starting with lattice Yang-Mills is not essential, and a variety of other starting points, such as other approximations to the functional integral, have been considered. The point is to define the limiting continuum field theory. In principle, this might even be done without taking limits.

So far as I know, no breakthroughs have been made on this problem in the last few years. In particular, while progress has been made in lower-dimensional field theories, I know of no significant progress toward a mathematically rigorous construction of the quantum Yang-Mills theory. The state of the art remains the works of Balaban and of Magnen, Rivasseau and Sénéor cited in the problem description.

There has, however, been interesting progress on various related problems. First, there is much to be learned from the mathematical study of more general lower-dimensional field theories. A recent overview of constructive quantum field theory is Rivasseau, math-ph/0006017.

There are two classes of quantum field theories which are generally believed to bear a close similarity to four-dimensional Yang-Mills theory. The first is the two-dimensional nonlinear sigma model with target space a group manifold G, or, more generally, a symmetric space M of positive Riemannian curvature.

This is a theory whose fields are maps from two-dimensional space-time into M. Apparently, even these theories have not yet been constructed to the standards required in the problem description. On the other hand, the mass gap has been exhibited in a regulated version of the $O(N)$ sigma model [3]. Physically, these theories are known to be integrable, a point we will return to below.

The other broad class of models with great similarity to Yang-Mills, but significantly simpler, are the four-dimensional supersymmetric Yang-Mills theories. These are modifications of Yang-Mills for which the fields include, in addition to the connection of our previous discussion, various "fermionic" and other fields chosen to realize the following property: the Hamiltonian, the operator on the quantum Hilbert space which generates time translations, has a square root, called the "supercharge."

Many wonderful properties and simplifications follow from supersymmetry, as discussed in [1, 2]. Physically, the most important is that the renormalization problem is mitigated, which one would hope could make defining the theory easier. Mathematically, the most important is the relation to "topological field theory," first proposed by Witten, and discussed in some detail in the references we just gave.

Furthermore, although one has changed the problem, one still has a fairly close relation to the original problem using the ideology of the renormalization group. Namely, one can start with a supersymmetric theory, and add supersymmetry breaking terms to the action which only become important at long distances (many lattice spacings in the earlier discussion). This will produce a theory with the better renormalization properties of the supersymmetric theory at short distances, but which reduces to conventional Yang-Mills theory at longer distances. Thus, a solution to the problem in a sufficiently general class of supersymmetric theories would in fact imply the solution of the original problem.

While at present we appear no closer to this goal than the original one, there has been significant progress in the past few years in understanding supersymmetric gauge theory. Perhaps the most interesting recent results are Nekrasov's rederivation of the Seiberg-Witten solution using instanton methods [4, 5], and the recent evidence for integrability of the large N supersymmetric $N = 4$ gauge theory [6] (for a recent review, see [7]).

The first of these starts from the celebrated 1994 Seiberg-Witten solution of $N = 2$ supersymmetric gauge theory. While mathematicians are perhaps more familiar with the consequences of this work for four-dimensional topology, the original physics motivation and significance of this work was in fact that it addressed the "mass gap" and related problems in a supersymmetric analog of Yang-Mills theory. Indeed, the basic reason for the simplicity of the Seiberg-Witten invariants, compared to the Donaldson invariants, is the mass gap property, which allows reducing the computation of the invariants to a lower-dimensional problem.

The original work of Seiberg and Witten obtained the mass gap property by an ingenious argument that involved a clever ansatz for the "supersymmetric effective action" describing the long-distance properties of the theory. Subsequent work showed that their ansatz was the only consistent possibility out of a large class, but this still left the justification of the overall framework more or less as pure physical intuition.

On the other hand, their ansatz had precisely the form expected from a direct computation of the sum of an instanton expansion, defined as a succession of integrals over moduli spaces of Yang-Mills instantons. Thus one could hope to validate it by direct computation. Furthermore, these moduli spaces have an explicit description, due to Atiyah, Drinfeld, Hitchin and Manin, so the necessary computation was rather explicit. It was, however, too difficult to perform directly.

This problem was solved in a *tour de force* work of Nekrasov, which started from the problem of instanton sums in the noncommutative deformation of the gauge theory originally introduced by Alain Connes and Marc Rieffel. In the noncommutative theory, instanton moduli spaces are smooth and significantly simpler, but many further clever tricks were required to come to a result. In a sense, this result establishes Seiberg and Witten's result as a sort of four-dimensional analog of mirror symmetry, in which the explicit instanton sum is the analog of numbers of holomorphic curves on a Calabi-Yau, and the mirror is the Riemann surface encoding the Seiberg-Witten solution.

One can argue that this is the simplest result in four-dimensional gauge theory that carries any of the physics of the mass gap, and as such might be the best way in to the complexities of the problem.

The second result, integrability of $N = 4$ super Yang-Mills, is harder to explain in mathematical terms, but we mention it because of its novelty and the possibility that it will drastically change how we think about these problems. Its starting point is the "AdS/CFT correspondence" of Maldacena, according to which this version of Yang-Mills theory can be reformulated as a string theory in anti de Sitter (AdS) space. This is a difficult string theory to work with, even by physicists' standards, and few explicit results are known. But the striking claim of Minahan, Zarembo and others is that, at least for the $SU(N)$ gauge group in the limit of large N, this string theory becomes exactly solvable (or integrable) in precisely the sense that we mentioned above for the two-dimensional sigma model. This is essentially because of the high degree of symmetry of AdS and the other spaces involved. The full picture is still rather mysterious at present, but might lead to a detailed connection between the much better understood two-dimensional field theories, and the original Yang-Mills problem in four dimensions.

To summarize, while I would not bet on this problem being solved in the next few years, it remains a fertile ground for mathematical discovery.

References

[1] P. Deligne, P. Etingof, D. Freed, L. Jeffrey, D. Kazhdan, J. Morgan, D. Morrison, E. Witten, eds., *Quantum Fields and Strings: A Course for Mathematicians*, American Mathematical Society, Providence, RI , 1999, two volumes.

[2] D. Freed, *Five Lectures on Supersymmetry*, American Mathematical Society, Providence, 1999.

[3] C. Kopper, Comm. Math. Phys. 202, 89 (1999).

[4] N. A. Nekrasov, "Seiberg-Witten prepotential from instanton counting," Proceedings of the ICM, Beijing 2002, vol. 3, 477–496, arXiv:hep-th/0306211.

[5] N. A. Nekrasov and A. Okounkov, "Seiberg-Witten Theory and Random Partitions," arXiv:hep-th/0306238.

In the late 1960s, I began a new formulation of gauge fields through the approach of nonintegrable phase factors. It happened one semester I was teaching general relativity, and I noticed that the formula in gauge theory,

$$F_{\mu\nu} = \frac{\partial B_\mu}{\partial x_\nu} - \frac{\partial B_\nu}{\partial x_\mu} + i\epsilon\left(B_\mu B_\nu - B_\nu B_\mu\right)$$

and the formula in Riemannian geometry

$$R^{\ell}_{ijk} = \frac{\partial}{\partial x^j}\left\{\frac{\ell}{i\ k}\right\} - \frac{\partial}{\partial x^k}\left\{\frac{\ell}{i\ j}\right\} + \left\{\frac{m}{i\ k}\right\}\left\{\frac{\ell}{m\ j}\right\} - \left\{\frac{m}{i\ j}\right\}\left\{\frac{\ell}{m\ k}\right\}$$

are not just similar --- they are, in fact, the same if one makes the right identification of symbols! It is hard to describe the thrill I felt at understanding this point.

Excerpts from an interview with C. N. Yang in the Mathematical Intelligencer, Vol. 15, No. 4 (Fall 1993) 13–21.

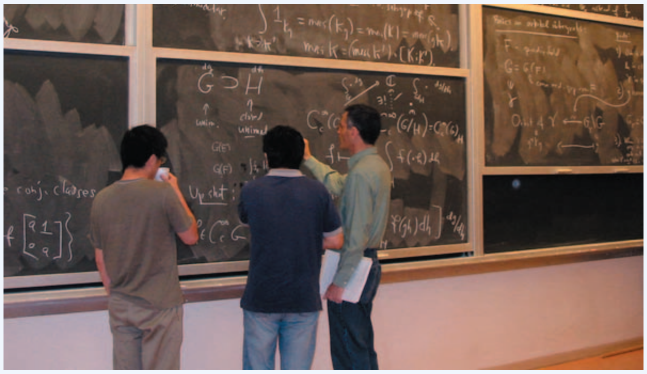
Harmonic Analysis, the Trace Formula, and Shimura Varieties at the Fields Institute in Toronto

Each year the Clay Mathematics Institute seeks to bring a generation of young researchers to the frontiers of mathematical research through its summer school program.

The 2003 School was held in June at the Fields Institute in Toronto on the related subjects of Harmonic Analysis, the Trace Formula and Shimura Varieties. Aimed at graduate students and mathematicians within five years of their Ph.D., the school began with three weeks of foundational courses centered around the trace formula: one course on the statement and proof of the trace formula, two courses providing background material on reductive groups and harmonic analysis on those groups, and a fourth course on Shimura varieties, which provided an illuminating application of the trace formula. The fourth week consisted of five short courses on more specialized topics related to the main themes of the school.

> Mathematics is an international language. Students attending came from universities in Canada, the U.S., France, Austria, Israel, Italy, Germany, Holland, Hong Kong, the UK, and China. Their countries of origin were still more varied, including India, Iran, Korea, Macedonia, Peru, Romania, Pakistan, Turkey, and Vietnam.

While there are were no formal prerequisites, preference was given to applicants with some prior knowledge of



Robert Kottwitz's course on Harmonic Analysis on Reductive Groups and Lie Algebras. Courtesy of Sonia Houle

Fields Institute Summer School participants. Courtesy of Sonia Houle

algebraic groups or number theory. The organizers reviewed and ranked the applications independently, and agreed on the final selections. The school had a very international flavor, with 104 participants from institutions in more than ten countries and of at least twenty nationalities. The response and feedback from the school was extraordinary, and the hospitality of the Fields Institute was outstanding. In light of the school's success, CMI is now working on a book for publication in the CMI/AMS Monographs series based on the subject matter of the school. Course notes and streaming audio for all the lectures is already available through the school's web site.

> One of the major benefits of the summer school is having many highly motivated people working through dialogue with many experts. The last time I was in a math course with so many people was in my first year of undergraduate work. I never had so many top mathematicians available at one spot for such a long time before. My idea of doing math is to collaborate with others as much as possible. The summer school was a rare perfect chance to do so.
>
> – *school participant*

See: http://www.fields.utoronto.ca/

2004 Research Fellows

On February 23, 2004, the Clay Mathematics Institute announced the appointment of four Research Fellows: Ciprian Manolescu and Maryam Mirzakhani of Harvard University, and András Vasy and Akshay Venkatesh of MIT. These outstanding mathematicians were selected for their research achievements and their potential to make significant future contributions.

Ciprian Manolescu

Ciprian Manolescu (b. 1978), a native of Romania, is completing his Ph.D. at Harvard University under the direction of Peter B. Kronheimer. In his undergraduate thesis he gave an elegant new construction of Seiberg-Witten Floer homology, and in his Ph.D. thesis he gave a remarkable gluing formula for the Bauer-Furuta invariants of four-manifolds. His research interests span the areas of gauge theory, low-dimensional topology, symplectic geometry and algebraic topology. Manolescu will begin his four-year appointment as a Research Fellow at Princeton University beginning July 1, 2004.

Maryam Mirzakhani

Maryam Mirzakhani (b. 1977), a native of Iran, is completing her Ph.D. at Harvard under the direction of Curtis T. McMullen. In her thesis she showed how to compute the Weil-Petersson volume of the moduli space of bordered Riemann surfaces. Her research interests include Teichmuller theory, hyperbolic geometry, ergodic theory and symplectic geometry. As a high school student, Mirzakhani entered and won the International Mathematical Olympiad on two occasions (in 1994 and 1995). Mirzakhani will conduct her research at Princeton University at the start of her four-year appointment as a Research Fellow beginning July 1, 2004.

Andás Vasy

András Vasy (b. 1969), a native of Hungary, received his Ph.D. from MIT in June 1997 under the direction of Richard B. Melrose. Vasy is currently an associate professor at MIT. The focus of his research program is scattering theory, specifically the theory of N-body quantum Hamiltonians. Vasy has proved several deep results in this field concerning the structure of the scattering matrix and asymptotic behaviour of generalized eigenfunctions. More recently he has extended these techniques to study analysis on symmetric spaces. In 2002, he received an Alfred P. Sloan Research Fellowship. Vasy intends to work at Northwestern University and MIT during his two-year appointment as a Research Fellow beginning July 1, 2004.

Akshay Venkatesh

Akshay Venkatesh (b. 1981) was born in New Delhi, India, and raised in Australia, where he attended the University of Western Australia in Perth. In 2002, he received his Ph.D. from Princeton University, where he worked under the direction of Peter Sarnak. Since that time he has held an C.L.E. Moore Instructorship at MIT. Venkatesh has made major progress in counting and equidistribution problems in automorphic forms and number theory. His research areas include representation theory, number theory, locally symmetric spaces, and ergodic theory. Venkatesh has chosen to carry out his research at the Courant Institute of Mathematical Sciences during his two-year appointment as a Research Fellow beginning September 1, 2004.

The above mathematicians join the current Clay Research Fellows: Manjul Bhargava (Princeton University), Daniel Biss (University of Chicago), Alexei Borodin (Caltech), Maria Chudnovsky (Princeton University), Dennis Gaitsgory (University of Chicago), Sergei Gukov (Harvard University), Elon Lindenstrauss (Courant Institute of Mathematics), Mircea Mustata (Harvard University), Igor Rodnianski (Princeton University), and Terence Tao (UCLA).

Call for Nominations, Proposals and Applications

Nominations for Clay Senior Scholars, Clay Research Fellows, Clay Liftoff Scholars and for participants in the Clay Research Academy are accepted once a year and should be sent to the attention of Maria McLaughlin at:

nominations@claymath.org

They can also be mailed to:

Clay Mathematics Institute
One Bow Street
Cambridge, MA 02138

Nomination Deadlines:

Senior Scholars: August 1
Research Fellows: October 30
Liftoff Scholars: February 27
Research Academy: February 12

David Ellwood, CMI Research Director. Courtesy of Sonia Houle

The Clay Mathematics Institute invites proposals for conferences and workshops, its annual summer school, and book projects. Proposals will be judged on their scientific merit, probable impact, and potential to advance mathematical knowledge. The Scientific Advisory Board generally meets four times a year and will consider proposals at each of these meetings. Most decisions, however, are made in early Fall; complete material for consideration at that meeting should be received by August 1.

Proposals need not be long, but should address the scientific rationale, benefit, and impact of the project. A budget and the standard coversheet should be attached and sent to the attention of Maria McLaughlin at:

proposals@claymath.org

They can also be mailed to:

Clay Mathematics Institute
One Bow Street
Cambridge, MA 02138

CMI encourages short pre-proposals consisting of the cover sheet and a letter expressing interest and commenting on the value of the project for the mathematical community.

Please see: http://www.claymath.org/proposals/

Proposal Deadlines:

Summer Schools: August 1, February 15
Conferences: August 1, February 15
Book Fellows: August 1, February 15

Selected Articles by Research Fellows

Below are listed the two most recent publications for each of CMI's Research Fellows (Long-Term Prize Fellows). The complete list is available at www.claymath.org/fellows. Especially noteworthy was the result of Green and Tao posted at arXiv:math.NT/0404188. The authors show that there are arbitrarily long arithmetic progressions of prime numbers.

DANIEL BISS

Decomposing Out(F_n). D.K. Biss
CD + PL implies smooth. L.M. Anderson and D.K. Biss

ALEXEI BORODIN

Random partitions and the Gamma kernel. Alexei Borodin, Grigori Olshanski (arXiv:math-ph/0305043)
Continuous time Markov chains related to Plancherel measure (announcement of results). Alexei Borodin, Grigori Olshanski (arXiv:math-ph/0402064)

MARIA CHUDNOVSKY

Detecting even holes. M. Chudnovsky, K. Kawarabayashi, P. Seymour
Chudnovsky is also finishing the publication *Non-zero A-paths in graphs with edges labeled by group elements.* with J. Geelen, B. Gerards, L. Goddyn, M. Lohman and P. Seymour

DENNIS GAITSGORY

On de Jong's conjecture. Dennis Gaitsgory (arXiv:math.AG/0402184)
D-modules on the affine Grassmannian and representations of affine Kac-Moody algebras. E. Frenkel, D. Gaitsgory (arXiv:math.AG/0303173)

SERGEI GUKOV

Equivalence of twistor prescriptions for super Yang-Mills. S. Gukov, L. Motl, A. Neitzke (arXiv:hep-th/0404085)
An Index for 2D field theories with large N=4 superconformal symmetry. Sergei Gukov, Emil Martinec, Gregory Moore, Andrew Strominger (arXiv:hep-th/0404023)

ELON LINDENSTRAUSS

Invariant sets and measures of nonexpansive group automorphisms. E. Lindenstrauss, K. Schmidt (arXiv:math.DS/0303121)
Rigidity of multiparameter actions. Elon Lindenstrauss (arXiv:math.DS/0402165)

MIRCEA MUSTATA

Multiplier ideals of hyperplane arrangements. Mircea Mustata (arXiv:math.AG/0402232)
Asymptotic invariants of base loci. Lawrence Ein, Robert Lazarsfeld, Mircea Mustata, Michael Nakamaye, Mihnea Popa (arXiv:math.AG/0308116)

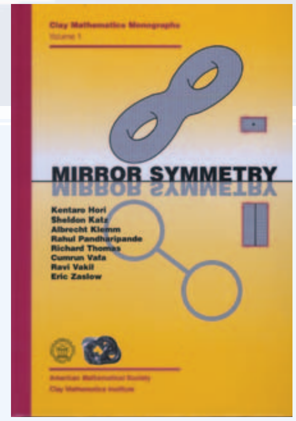
IGOR RODNIANSKI

Global existence for the Einstein vacuum equations in wave coordinates. H. Lindblad, I. Rodnianski (arXiv:math.AP/0312479)
A geometric approach to the Littlewood-Paley theory. Sergiu Klainerman, Igor Rodnianski (arXiv:math.AP/0309463)

TERENCE TAO

The primes contain arbitrarily long arithmetic progressions. Ben Green and Terence Tao (arXiv:math.NT/0404188).
Global well-posedness and scattering for the higher-dimensional energy-critical non-linear Schrödinger equation for radial data. Terence Tao (arXiv:math.AP/0402130)

Books & Videos

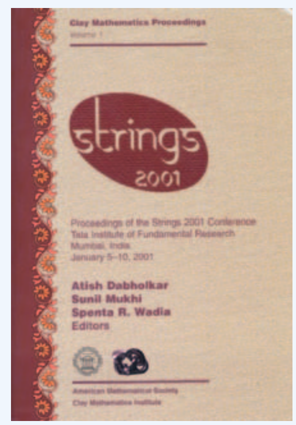


Mirror Symmetry. **Authors: Kentaro Hori, Sheldon Katz, Albrecht Klemm, Rahul Pandharipande, Richard Thomas, Ravi Vakil. Editors: Cumrun Vafa, Eric Zaslow. CMI/AMS publication, 929 pp., Hardcover. ISBN 0-8218-2955-6. List: $125. AMS Members: $99. CMIM/I. To order, visit: www.ams.org/bookstore.**

"This book, the product of the collective efforts of the lecturers at the school organized by the Clay Mathematics Institute, is a valuable contribution to the continuing intensive collaboration of physicists and mathematicians. It will be of great value to young and mature researchers in both communities interested in this fascinating modern grand unification project." – *Yuri Manin, Max Planck Institute for Mathematics, Bonn, Germany*

"The book is an excellent starting point for learning the subject. [It] provides an extensive background for both mathematicians and physicists. It is also the most up-to-date book on the subject, covering recent developments like the Gopakumar-Vafa invariants and geometric transitions." ***from Marcos Marino's review of the book in MathSciNet.***

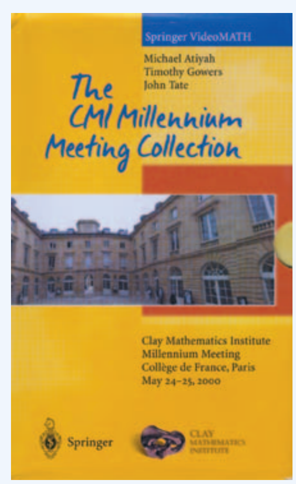
Strings 2001. **Authors: Atish Dabholkar, Sunil Mukhi, Spenta R. Wadia. Tata Institute of Fundamental Research. Editor: American Mathematical Society (AMS), 2002, 489 pp., Paperback, ISBN 0-8218-2981-5, List $74., AMS members: $59. Order code: CMIP/1. To order, visit: www.ams.org/bookstore.**



This book summarizes recent results in string theory from the perspective of world renowned experts, including Michael Green, David Gross, Stephen Hawking, John Schwarz, Edward Witten and others. Individual articles discuss D-branes, black holes, string dualities, compactifications. Calabi-Yau manifolds, conformal field theory, noncommunative field theory, string field theory, and string phenomenology.

Global Theory of Minimal Surfaces. **Proceedings of the 2001 CMI Summer School at MSRI. Editor: David Hoffman. Forthcoming joint CMI/AMS publication. 800 pp., Hardcover. Available in late 2004. Available soon at: www.ams.org/bookstore.**

Strings and Geometry. **Proceedings of the 2002 CMI Summer School held at the Isaac Newton Institute for Mathematical Sciences. Authors: Paul Aspinwall, Tom Bridgeland, Alastair Craw, Robert Henricus Dijkgraaf, Michael Douglas, Mark Gross, Nigel Hitchin, Anton Kapustin, Gregory Moore, Miles Reid, Richard Thomas, Pelham Wilson. Forthcoming joint CMI/AMS publication. Available soon at: www.ams.org/bookstore.**



The CMI Millennium Meeting Collection**. Authors: Michael Atiyah, Timothy Gowers, John Tate, François Tisseyre. Editors: Tom Apostol, Jean-Pierre Bourguignon, Michele Emmer, Hans-Christian Hege, Konrad Polthier. Springer VideoMATH, © Clay Mathematics Institute, 2002. Box set consists of four video cassettes: *The CMI Millennium Meeting*, a film by François Tisseyre; *The Importance of Mathematics*, a lecture by Timothy Gowers; *The Millennium Prize Problems*, a lecture by Michael Atiyah; and *The Millennium Prize Problems*, a lecture by John Tate. VHS/NTSC or PAL. ISBN 3-540-92657-7, List: $119, EUR 104.95. To order, visit: www.springer-ny.com (in the United States) or www.springer.de (in Europe).**

These videos document the Institute's Paris meeting at the Collège de France where CMI announced the Millennium Prize Problems. For anyone who wants to learn more about these seven grand challenges.

2004 Institute Calendar

JANUARY FEBRUARY	Topological Aspects of Real Algebraic Geometry. Research Scholars Kollár and Sottile at MSRI. January 5–May 14. Special Week on Ranks of Elliptic Curves and Random Matrix Theory, Isaac Newton Institute. February 9–13. Ciprian Manolescu and Maryam Mirzakhani of Harvard University, and András Vasy and Akshay Venkatesh of MIT are named Clay Research Fellows.February 25.
MARCH APRIL MAY	Complex and Symplectic Geometry Conference, University of Miami. March 11–15. The Clay Mathematics Reseach Academy at CMI. March 19–27. Clay Public Lecture by Timothy Gowers, *Is there such a thing as Infinity?* Harvard University Science Center. March 22. CMI/IAS Workshop on Mathematical Aspects of Nonlinear PDEs, Institute for Advanced Study, Princeton, NJ. March 23–26.
JUNE JULY AUGUST	Summer School on Floer Homology, Gauge Theory, and Low-Dimensional Topology in Budapest at the Alfréd Rényl Institute of Mathematics (Hungarian Academy of Sciences). June 5–26. Retrospective in Combinatorics, MIT. June 22–26. K-Theory and Noncommutative Geometry, Institut Henri Poincaré. July 5–17. Geometric Combinatorics. CMI Senior Scholars Richard Stanley and Bernd Sturmfels at IAS/Park City Mathematics Institute. July 11–31. Strings, Branes, and Superpotentials, Aspen Center for Physics. July 19–August 15.
SEPTEMBER OCTOBER NOVEMBER DECEMBER	Algebraic Cycles, K-Theory, and Modular Representation Theory at Northwestern University. September 16-19. CMI and MSRI Conference on Recent Progress in Dynamics at MSRI. September 27–October 1. Automorphic Forms and Trace Formula, The Fields Institute, Toronto. October 13–16. CMI Annual Meeting, Cambridge, MA. November 5. Harmonic Analysis, Ergodic Theory & Probability, Stanford University. December 12–14.