A Probability-Rich ICM and Wendelin Werner's Work >>>

Wendelin Werner at work   © J. F. Dars / CNRS Photothèque

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The 2006 International Congress of Mathematicians in Madrid was exceptionally rich in probability theory. Not only was the Fields Medal awarded for the first time to a probabilist, namely Wendelin Werner, it was also awarded to Andrei Okounkov whose work bridges probability with other branches of mathematics. Both Okounkov and Werner had been invited to give a 45-minute lecture each in the probability and statistics section before their Fields Medal awards were announced.

The newly created Gauss Prize (in full, the Carl Friedrich Gauss Prize) for applications of mathematics was awarded to Kiyosi Itô, another probabilist whom we all know. The objective of the Gauss Prize is to honor scientists whose mathematical research has had an impact outside mathematics, such as in technology, in business, or simply in people's everyday lives. A presentation of Itô's work was made by Hans Föllmer in a plenary address to the audience of the congress, in the presence of Itô's daughter, who received the prize and gave a speech on behalf of her 90-year-old father who was prevented by ill health from attending.

The Nevanlinna Prize was awarded to Jon Kleinberg who uses probability in his work. Much of his lecture was about small worlds for which probability was used to formulate the model.

Among the plenary lectures, apart from those delivered by probabilist Oded Schramm and statistician Iain Johnstone (on the use of random matrices in statistics), Percy Deift's lecture on "Universality for mathematical and physical systems" was about random matrices and Avi Wigderson's lecture "P, NP and mathematics" was in part about probabilistic algorithms. Richard Stanley's plenary lecture elaborated on the famous Baik-Deift-Johansson result on the longest increasing sequence in a random permutation (which incidentally has been connected by Andrei Okounkov to another famous result about the largest eigenvalue of random matrices). Even the plenary lecture of Terence Tao, another Fields Medalist at this same congress, was entitled "The dichotomy between structure and randomness" and contained several examples from probability. Finally, in the logic session Rod Downey's 45-minute talk was about algorithmic randomness and computability. These were just a sample of lectures we attended and there could be more talks that reflected the growing importance of probability theory in science and mathematics.

Although the Fields Medal was awarded to a probabilist for the first time, it was not surprising that Wendelin Werner was the one. Werner was born in Germany in 1968, but

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Random Graphs and Large-Scale Real-World Networks >>>

[Editor's Note: In May and June of 2006, the Institute hosted a program on "Random Graphs and Large-Scale Real-World Networks", of which Béla Bollobás was the chair of the Organizing Committee. In this article, he gives his perspectives on the field of random graphs as well as the program organized at IMS.]

The classical theory of random graphs was founded by Erdõs and Rényi almost fifty years ago. Erdõs and Rényi studied random graphs as fascinating and intricate objects in pure mathematics, and used their theory to show the existence of graphs with paradoxical properties. Since then, this theory has gone from strength to strength, with thousands of papers written on the topic.

Not surprisingly, although the theory of random graphs is an area of pure mathematics, possible applications have never been far away — after all, many large-scale graphs occur in real life. For example, the World Wide Web can be viewed as a graph, and so can metabolic and protein networks, food webs, the system of telephone calls, the network in the brain, traffic flows, acquaintances in a society, economic networks, and so on. These graphs resemble the classical binomial random graphs in the sense that they do not seem to have clear-cut structures that are easy to describe — at

a glance, the connections seem to be 'random'. However, they are unlike any of the random graphs of the classical models: most networks occurring in the world are far from homogeneous; in particular, their degree distributions are rather different from those in the classical models. Also, real-world networks do not tend to be chosen from reasonably well-defined distributions, but arise as the result of dynamical processes that add and remove vertices and edges from the network.

Although a fair amount of empirical work had been done on real-world graphs for many decades, in particular, on acquaintance and citation networks, mathematical work on them started only in the last decade. For instance, Watts and Strogatz drew attention to the 'small-world phenomenon', and Barabási and Albert noted the 'scale-free' nature of many of the networks concerned, evidenced by, for example, power-law degree distributions. They suggested that such distributions arise in a graph growing by acquiring more and more vertices and edges if the newly arrived vertices get joined to old ones according to some preferential attachment rule. Thus, for example, the World Wide Web seems to be essentially scale free: viewed as a directed graph, the distributions of the in-degrees and out-degrees are well approximated by power law distributions.

his parents settled in France when he was one year old, and he acquired the French nationality a few years later. After studying at the Ecole Normale Supérieure de Paris, he defended his PhD thesis in Paris in 1993, shortly after getting a permanent research position at the CNRS. He became a Professor at University Paris-Sud Orsay in 1997. Before winning the Fields Medal, he had received many other awards, including the 2000 Prize of the European Mathematical Society, the 2001 Fermat Prize, the 2005 Loève Prize and the 2006 Pólya Prize.

Wendelin Werner's work lies at the interface between probability theory and statistical physics. The fact that the models in consideration enjoy asymptotic conformal invariance properties also leads to using sophisticated tools from complex analysis. Werner's most famous results come from his collaboration with Greg Lawler and Oded Schramm on applications of the so-called SLE (stochastic Loewner evolution) processes. SLE processes are obtained by introducing in Loewner's equation of complex analysis a random driving function which is just a scaled linear Brownian motion. The work of Werner and his co-authors has produced extraordinary applications of SLE processes to long-standing open problems, such as the rigorous calculation of the non-intersection exponents for random walk or Brownian motion.

Such exponents govern, for instance, the asymptotic behavior of the probability that two independent planar random walk paths up to time n will have no intersection point.

Another remarkable application was the proof that the Hausdorff dimension of the exterior frontier of a planar Brownian path is equal to 4/3. This fact, which had been conjectured by Mandelbrot more than 20 years ago, was one of the most fascinating open problems of probability theory. SLE processes have many other spectacular applications to different models of statistical physics, such as percolation, self-avoiding random walks or spanning trees on the lattice. The development of these applications, by Wendelin Werner and his co-authors, represents a giant step in the mathematical understanding of these models.

Louis Chen
National University of Singapore
and
Jean-François Le Gall
Ecole Normale Supérieure

29 January 2007

People in the News >>>

New Deputy Director

Denny Leung, who served as the Institute's Deputy Director from 1 August 2004 to 31 December 2006, relinquished his position to resume full-time duties at the Department of Mathematics. He had contributed much to the institute during his service and will remain as an editor of *Imprints*. He is succeeded by Ka Hin Leung from the Department of Mathematics.

The latest addition to the IMS extended family is baby Lee Zi Jun, born to the Institute's Lab Officer, Jolyn Wong, on 14 February 2007.

Baby Zi Jun

Direct studies of real-world networks themselves (measuring various properties such as degree-distribution, diameter, clustering, etc.) have led to suggestions for mathematical models of these networks. These new models are often rather far from the standard models of random graphs, and are easiest studied by computer simulations and heuristic analysis.

Of necessity, the rigorous mathematical study of these new models lags behind the empirical observations and computer experiments: although over the decades many tools and methods have been invented to tackle random graph problems, the new models often need very different methods.

The aim of the program "Random Graphs and Large-Scale Real-World Networks" was to bring together many of the foremost experts on the new random graph models: combinatorialists, probabilists, computer scientists, and physicists. The excellent facilities at the Institute and the friendly and helpful staff were instrumental in creating an inspiring atmosphere; all the participants were delighted to take part in the Program and benefited greatly from their stay in Singapore.

Béla Bollobás
University of Cambridge and
University of Memphis

Programs & Activities >>>

Past Program in Brief

Geophysical Fluid Dynamics and Scalar Transport in the Tropics (13 November–8 December 2006, Special Lectures 18–22 December 2006)

Website: http://www.ims.nus.edu.sg/Programs/geophysical/index.htm

Chair

Tieh-Yong Koh, *Nanyang Technological University*

Members

Peter Haynes, *University of Cambridge*
Pavel Tkalich, *National University of Singapore*
Hock Lim, *National University of Singapore*

The program addressed the dearth of knowledge in tropical dynamics. Over two workshops and one lecture-tutorial series, an international gathering of scientists and applied mathematicians reviewed the recent theoretical ideas on geophysical fluid dynamics (GFD) and scalar transport within the tropics. The ideas reviewed had helped to organize and elucidate information in datasets generated by weather or sea-state forecast and pollutant dispersion analysis in Southeast Asia. Thus, the program benefited participating applied meteorologists and oceanographers who handle datasets on a day-to-day basis.

A good atmosphere for discussion: (from left) Jun-Ichi Yano, Martin Skote, Rogerio Manica

A group transported by turbulence

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Bernard Legras: Mastering chaos and turbulence

Intent on modeling the tropical atmosphere

Current Program

Moving Interface Problems and Applications in Fluid Dynamics (8 January–31 March 2007)
Website: http://www.ims.nus.edu.sg/Programs/fluiddynamic/index.htm

Chair
Boo Cheong Khoo, *National University of Singapore*

Members
Weizhu Bao, *National University of Singapore*
Zhilin Li, *North Carolina State University*
Ping Lin, *National University of Singapore*
Tiegang Liu, *Institute of High Performance Computing*
Le Duc Vinh, *Singapore-MIT Alliance*

The program discusses recent developments in the modeling and simulations of biological flow coupled to deformable tissue/elastic structure, shock wave and bubble dynamics in biological treatment (occurring in shock lithotripsy, lipoplasty, phacoemulsification and others) with experimental verification, multi-medium flow or multi-phase flow involving cavitation/supercavitation (arising from large pressure changes) and detonation problems. It addresses (mathematical) issues arising from these areas, including

i. how to efficiently deal with interfacial topological change,
ii. how to overcome the unphysical oscillations,

iii. how to suppress the numerical instability when a fluid coupled to a stiff material or when the density ratio of two media is very large,
iv. how to efficiently deal with stiff chemical reactions in computations,
v. whether and when one should consider using isotropic or anisotropic models, considerations of thermal and friction effect, and other factors during the modeling of multi-phase flows with relevance to the bio-medical field and physical environment.

This three-month program brings together leading physicists, computational scientists and applied mathematicians internationally, and local experts from NUS, NTU, A*STAR institutes and local hospitals to discuss and interact as well as collaborate. The program consists of two workshops, four tutorial sessions, and collaborative research.

As part of the program, a joint Department of Mathematics/ IMS Winter School took place from 8–26 January 2007. Twelve students from the ASEAN region were offered financial support to take part in the Winter School.

Ready to interface

Next Program

BRAIDS (14 May–13 July 2007)
Website: http://www.ims.nus.edu.sg/Programs/braids/index.htm

Co-chairs
Jon Berrick, *National University of Singapore*
Fred R. Cohen, *University of Rochester*

Members
Mitch Berger, *University College London*
Joan S. Birman, *Columbia University*
Toshitake Kohno, *University of Tokyo*
Yan-Loi Wong, *National University of Singapore*
Jie Wu, *National University of Singapore*

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To date, most mathematical interest in braids has come from algebraists, topologists and mathematical physicists. As well, braids are also engaging the attention of computer scientists, as a basis for public-key cryptosystems. Probabilistic algorithms are being employed to search for solutions to word problems in the braid group. Relevance to robotics, cryptography and to magnetohydrodynamics is also to be explored during the program. The main theme of the program is the mathematical structure of the braid group, together with applications arising from this structure both within mathematics, and outside of mathematics such as (a) magnetohydrodynamics, (b) robotics and (c) cryptography.

Activities

- **PRIMA Summer School:** 4–29 June 2007
  … *Jointly organized with Department of Mathematics (PRIMA = Pacific Rim Mathematical Association)*

- **Tutorials:**
  Week 1: 4–8 June 2007
  (a) Preliminaries in topology and algebra, by E-Jay Ng: 4 hours
  (b) Braids — definitions and braid groups, by Dale Rolfsen: 4 hours

  Week 2: 11–15 June 2007
  (a) Simplicial objects and homotopy groups, by Jie Wu: 4 hours
  (b) Configuration spaces, by Fred Cohen: 2 hours

  Week 3: 18–22 June 2007
  (a) Magnetohydrodynamics, by Mitch Berger: 4 hours
  (b) Configuration spaces and robotics, by Robert Ghrist: 2 hours
  (c) Braid groups and cryptography, by David Garber: 2 hours

- **Conference:** 25–29 June 2007

- **Public Lecture:** Braids and robotics by Robert Ghrist (University of Illinois, Urbana-Champaign), 26 June 2007

Programs & Activities in the Pipeline

Summer School in Logic (1–31 July 2007)
… *Jointly organized with Department of Mathematics*
Website: http://www.ims.nus.edu.sg/activities/logicss07/index.htm

Organizing Committee
Chi Tat Chong, *National University of Singapore*
Qi Feng, *Chinese Academy of Sciences, China and National University of Singapore*
Yue Yang, *National University of Singapore*

Invited Speakers
Theodore A. Slaman, *University of California at Berkeley*
W. Hugh Woodin, *University of California at Berkeley*

The 2007 Logic Summer School will consist of two parts, one in recursion (computability) theory and the other in set theory, running in parallel. The lectures will be conducted by Professors Theodore A Slaman and W Hugh Woodin of the University of California at Berkeley. In addition to lectures, there will be classroom discussions of mathematical problems for participants led by senior graduate students. The Logic Summer School is a collaboration between researchers at the University of California, Berkeley, Chinese Academy of Sciences and the National University of Singapore.

Computational Methods in Biomolecular Structures and Interaction Networks (9 July–3 August 2007)
Website: http://www.ims.nus.edu.sg/Programs/biomolecular07/index.htm

Co-chairs
Yu Zong Chen, *National University of Singapore*
Vladimir Kuznetsov, *Genome Institute of Singapore*

Members
Xiang Yang Liu, *National University of Singapore*
Boon Chuan Low, *National University of Singapore*
Louxin Zhang, *National University of Singapore*

This program will discuss recent progress and facilitate the exchange of new ideas in the development and application of mathematical algorithms and computational methods for studying biomolecular structures, their interactions and networks. It is also intended to promote stronger communication and collaboration among mathematical, computational and biological scientists in order to examine essential and unsolved mathematical problems arising from structural and network biology. The program will be structured around two workshops and two tutorials designed to bring together researchers from a wide spectrum of mathematical and computational biology.

Activities
- **Workshops:**
  I. Probabilistic and deterministic models of structure and complexity dynamics of large-scale biomolecular interaction networks: From concept to analysis and validation, 16–20 July 2007
  II. Data analysis and modeling of protein-DNA, protein-RNA, protein-protein and RNA-DNA interactions: identification and prediction of molecular structures and their biological functions, 30 July–3 August 2007

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- **Tutorials:**
    I. New approaches to mathematical modeling, simulation and analysis of biomolecular interaction networks, 9–13 July 2007
    II. Models and computational algorithms of structural determination of macromolecules, their interactions and bio-imaging, 23–27 July 2007

Bose-Einstein Condensation and Quantized Vortices in Superfluidity and Superconductivity (1 November–31 December 2007)

Website: http://www.ims.nus.edu.sg/Programs/bose07/index.htm

Co-chairs

Weizhu Bao, *National University of Singapore*
Fanghua Lin, *Courant Institute, New York University*

Members

Jiangbin Gong, *National University of Singapore*
Dieter Jaksch, *University of Oxford*
Baowen Li, *National University of Singapore*
Peter Markowich, *University of Vienna*

This two-month program will bring together leading international applied and pure mathematicians, theoretical and experimental physicists, computational scientists, and researchers from NUS Departments of Mathematics, Physics, Material Sciences and Mechanical Engineering, and from A*STAR institutes IHPC and IMRE, to review, develop and promote interdisciplinary research on Bose-Einstein condensation and quantized vortex states and dynamics in superfluidity and superconductivity.

The program participants will:
i.   review the most recent and advanced developments in research on Bose-Einstein condensation and quantized vortices in superfluidity and superconductivity, from experiment to theory, simulation and application;
ii. present recently developed mathematical theories, including modeling, analysis and computational techniques, that are relevant to BEC and quantized vortices;
iii. discuss and compare different recently proposed scientific models for BEC, especially for BEC at finite temperatures, and fermion condensation;
iv. identify critical scientific issues in the understanding of BEC and quantized vortices and the difficulties that are common to both disciplines;
v.   accelerate the interaction of applied and computational mathematics with physics and materials science, and promote this highly interdisciplinary research that has emerging applications;
vi. develop and foster international collaborations in a new era of scientific research.

Activities

1. Collaborative research: 1 November – 31 December, 2007
2. Workshop 1: 12–16 November, 2007
   Title: Bose-Einstein condensation: modeling, analysis, computation and applications
3. Workshop 2: 10–14 December, 2007
   Title: Quantized vortices in superfluidity and superconductivity and kinetic theory

Highlights of Other Activities

Biostatistics Workshop (25 October 2006)

... Jointly organized with Department of Statistics and Applied Probability

Website: http://www.ims.nus.edu.sg/activities/wkbiostatistics/index.htm

Organizing Committee

Anthony Kuk, *National University of Singapore*
Kwok Pui Choi, *National University of Singapore*

This one-day workshop was a sequel to the Biostatistics Workshop held at the Department of Statistics and Applied Probability, NUS, on 18 August 2006. It was a forum for working biostatisticians to share experiences on the practical applications of biostatistics in their work (including the modes of interface with clinicians and biomedical scientists), to discuss the challenges and opportunities that lie ahead for the profession, and to formulate strategies to increase the effectiveness and impact of biostatistics. A total of 3 overseas and 4 local speakers delivered lectures. The workshop was attended by 51 participants.

Biostatisticians enjoying a workshop

Talking shop, biostatistically

Mathematical Conversations

Hans Föllmer: Efficient Markets, Random Paths >>>

Hans Föllmer

Interview of Hans Föllmer by Y.K. Leong (matlyk@nus.edu.sg)

Public Lectures
The Institute organized two public lectures in February and March.

Hans Föllmer of Humboldt University lectured on "Mathematical Aspects of Financial Risk". The lecture was jointly organized by IMS and the Risk Management Institute in conjunction with the Department of Mathematics.

Hans Föllmer: Analyzing risky business?

The second lecture, on "Computers and Genomes", was delivered by Michael Waterman of the University of Southern California. It was organized in conjunction with the Bioinformatics Institute.

Michael Waterman: Computation is in his genes

Hans Föllmer is renowned for fundamental contributions to statistical mechanics, stochastic analysis and mathematical finance. He is also known for his indefatigable energy and enthusiasm in actively promoting the applications of mathematics, especially to financial markets.

Having undertaken a broad education in philosophy, languages, physics and mathematics in four European universities, he obtained his doctorate (Dr. rer. Nat.) from University of Erlangen under the supervision of Konrad Jacobs. Except for a 3-year stint in the U.S. at MIT and Dartmouth College, his career was essentially cultivated to fruition in Europe — at University of Erlangen, University of Frankfurt, University of Bonn, ETH Zurich and Humboldt University in Berlin. At Bonn, he was professor twice, first at the Department of Economics and later, after a period of eleven years in Zurich, at the Department of Mathematics. Since 1994, he has been Professor of Mathematics at Humboldt University, Berlin.

His extensive publications cover several interdisciplinary areas. In addition to the influence of his pioneering research, he has made numerous contributions to the scientific communities in Europe and elsewhere through his active involvement in scientific committees and advisory boards. For his deep and wide-ranging contributions, he received the following awards: Emmy Noether award of the University of Erlangen, Science Prize of the GMÖOR (Gesellschaft für Mathematik, Õkonomie und Operations Research), Prix Gay-Lussac/Humboldt, the Georg Cantor Medal of the German Mathematical Society and an honorary doctorate from the University Paris-Dauphine.

He was also elected as member of the following scientific bodies: Academia Europaea, Deutsche Akademie der Naturforscher Leopoldina, and Berlin-Brandenburgische Akademie der Wissenschaften.

Besides giving invited lectures at major scientific meetings and universities throughout the world, he is actively engaged in the training of scientists and mathematicians both inside and outside of Europe. Among other activities, he is involved in the International Research Training Group (IRTG) Berlin-Zurich and the DFG Research Center "Mathematics for key technologies".

Since 2000, Föllmer is a regular visitor to NUS and has rendered valuable service to the Department of Mathematics and the Institute. He is a founder member of the Institute's Scientific Advisory Board (SAB) which successfully charted the direction of the Institute during its first five years. During a 3-year period in 2000-2003, he helped to develop the

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Department's new financial mathematics program, and visited the department for short periods of 4 to 8 weeks to advise and give courses on the subject.

It was during his visit to the Institute as a member of the SAB that Y.K. Leong interviewed him on behalf of *Imprints* on 4 January 2006. The following is an edited and enhanced account of this interview, in which he spoke with passion about his intellectual path from philosophy to mathematics, and gave us a rare glimpse, from the view of a pioneer at the interface of probability and finance, of the somewhat unexpected impact of stochastic analysis (an esoteric branch of mathematics) on stock markets (one of the most practical activities of an industrial society).

Imprints: Your university education seems to have been rather unusual in the sense that it was taken in many places in Germany and France. Please tell us something about it and about how you became interested in stochastic analysis.

Hans Föllmer: In the German tradition, the fact that I went to several universities is not unusual but it's quite normal and even expected traditionally. My father, for example, as a student, went to four different universities. The idea was to get to know different schools of thought in different parts of the country. In that sense, I did the normal thing. I started out in Cologne, then I went to Göttingen, and the reason that I went to France was that at that time I had already focused on one special area, and my advisor for the diploma thesis asked me to go to Paris for a year in order to learn more about it. In the meantime, he had moved from Göttingen to Erlangen, and then I joined him in Erlangen, and that was university number four - just like my father.

I: Who was your supervisor?

F: My supervisor was Konrad Jakobs. He was working in ergodic theory, and the reason that he went to Erlangen was that he wanted to establish a joint center in probability with Hans Bauer who had at the same time moved from Hamburg to Erlangen. Bauer and his students were working on the potential theory of Markov processes, and my own interest then was, in fact, closer to Bauer's than to Jakobs'. The reason I went to Paris in 1965 was that I was supposed to learn some potential theory from the sources in France — Choquet and Brelot, for example. Of course I also took other courses, and I particularly enjoyed the lectures of Laurent Schwartz and Jacques Neveu. After my year in Paris I went to Erlangen for three years. During that time there was a lot of activity in probability. Robert Blumenthal came for a year and gave a graduate course on the book on Markov processes which he was writing with Ron Getoor. There were visitors such as Paul-André Meyer, Joe Doob, Shizuo

Kakutani, Alexandr Borovkov, Kiyosi Itô and Kai Lai Chung. For us graduate students that was an exciting time.

I: Were you interested in stochastic analysis right from the beginning?

F: No, I even didn't start in mathematics at the beginning. I first started to study philosophy and literature. Then I became interested in the philosophy of language, linguistics, and I thought it would be good for me to understand how formal languages like mathematics work. So I thought I would sit in on mathematics classes, and then I got interested, and slowly I got drawn into the subject. One reason was that mathematics was much better organized as a curriculum than philosophy. Philosophy was very free floating. So I got sucked into the mathematics program and started to enjoy it. There were several occasions that I thought of going back to my original interest, but I stayed on. The reason I got interested in probability rather early, in my third year of study of mathematics, may have something to do with my original motivation in philosophy because I was intrigued by the notions of probability, entropy and uncertainty. That probably played some role in my decision.

I: Was there any single person who was quite decisive in making you work in probability?

F: The reason that I decided to specialize in probability had certainly something to do with my teacher at that time, Konrad Jakobs. He is a very impressive person and has very wide interests in mathematics and beyond. I liked him a lot as a teacher, and he immediately helped me and supported me. That probably played a role, too.

I: Was it a tradition to have broad interests?

F: Yes, that was the intellectual tradition. You were encouraged to take a broad approach and I liked that. Nowadays, it is much more focused. In retrospect, it was a luxury spending time on philosophy and so on. It would be harder to do the same thing now, also in Germany, because now there is more pressure on students to proceed quickly.

I: You taught briefly for three years in the United States immediately after your doctorate. Was it a cultural or intellectual pull that made you return to Europe to establish your career in Germany?

F: That was a very difficult decision. After one year in the States, I thought, "Okay, it was all very interesting, but, no, I really want to go back to Europe." After the second year, I was no longer so sure, and in the third year I was strongly tempted to stay. Clearly the scientific situation in the United

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States was very attractive. But I was already married and we had a child, and finally we decided to go back to Germany, mainly for cultural reasons. Soon after, I had another option to go back to North America, but I also had attractive offers in Germany, and so we decided to stay. But it was more the cultural pull, not so much the intellectual in the professional sense.

I: Your research work was initially in stochastic processes, which is theoretical probability theory, and you soon began to work on stochastic problems in other fields. Why mathematical finance and not other areas like biology?

F: My research was primarily in stochastic processes for 30 years, not only initially. But I had one first contact with mathematical finance already in 1971 when I was at Dartmouth. At that time, I had an undergraduate student who wanted to write a senior thesis in probability, and he proposed to work on an optimal stopping problem related to insider information in finance. This was David Kreps, who went on to become professor of economics at Harvard and Stanford and to receive the Clark medal in 1989, and who is now dean of the business school at Stanford. I learned from him what an option is. That was my first contact with mathematical finance. But for a long time I continued to work in probability, on problems in martingale theory, in interacting particle systems, and in stochastic analysis. For several years, I worked on questions motivated by the interface between probability and statistical mechanics, especially probabilistic approaches to phase transitions, Gibbs measures, and large deviations. My interest in mathematical finance became more systematic only much later, in the mid-80s. Actually, it was again triggered off by David Kreps. David spent a sabbatical in Cambridge and he came over to ETH Zurich, where I was teaching at that time, and gave a seminar related to the Black-Scholes pricing formula for options. I got intrigued and started to think about it. Then Dieter Sondermann, a colleague from Germany, visited ETH Zurich for a month. At the same time, he was doing consulting work with a major Swiss bank, and we started to work together on some mathematical aspects of option pricing. From that time on, I took a more systematic interest.

I: Did you work on a specific problem with this colleague of yours?

F: Yes, we looked at the problem of hedging financial derivatives in situations where the Black-Scholes paradigm of a perfect hedge breaks down, and we used arguments from martingale theory. By the way, Dieter Sondermann was professor of statistics in the economics department at the University of Bonn. He was holding the same position that I had held from 1974 to 1977 before I went to ETH

Zurich. At that time, I had a position as professor of statistics at the economics department of the University of Bonn. That was from 1974 to 1977. In 1974, I had three options — two offers for positions in mathematics and one from the economics department. At that time, I decided to take the economics offer because I wanted to learn what those guys were doing. The experience of three years in the economics department was probably responsible for my later decision to pursue questions in mathematical finance. After three years, however, I had an offer from ETH Zurich and I thought it was a good time to go back to mathematics. One aspect of this was that in doing research with students on questions which I liked, the conditions were better in the mathematics department than in the economics department. But I never regretted the decision to go to the economics department for some time because it was a very enriching experience to get to know this other culture. At that time, Gerard Debreu (who later received the Nobel Prize in economics for work in microeconomic equilibrium theory) was visiting the economics department in Bonn for a year to work with Werner Hildenbrand. He came with a strong group of young economists from Berkeley which included Truman Bewley (later at Yale), Mukul Majumdar (later at Cornell), Alan Kirman (later at Marseille) and Andreu Mas-Collel (for a long time at Harvard before he became minister of universities and research in Catalunya). That was a very stimulating environment, and I enjoyed that a lot.

I: How much of the field of mathematical finance has been accepted as an integral part of economics?

F: The fact that some of the Nobel prizes have been awarded to work in quantitative and even mathematical finance shows that the field has a lot of acceptance within the community of economists. I was more concerned with the other side — how well accepted is mathematical finance as a part of mathematics? My main interest was always in questions which are motivated by the financial applications, but which also have some intrinsic mathematical interest and can be treated as research problems in their own right from the mathematical point of view.

I: Decades ago, the general public would associate financial mathematics with more commercial activities like accounting and book-keeping. Do you think that this general public perception has been significantly raised to a higher level?

F: Several decades ago, before the early 70s, I would have had the same perception. Since then there has been really a spectacular change and a dramatic increase in mathematical sophistication. Mathematical finance has become a new source of appreciation and esteem for mathematics in the

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eyes of the general public. In the financial industry, the number of professional mathematicians working there has become much higher than what it used to be. I think that has generated a lot of respect for mathematics within that community and also in a wider public. When the Nobel Prize was given to Mertens and Scholes for their famous option pricing formula, this was one of the rare occasions where a mathematical formula appeared on page 1 of the *New York Times*. Yes, public perception of mathematics has been significantly raised.

I: If I'm not mistaken, some kind of empirical stochastic studies of the stock market were actually carried out before probability theory was rigorously established. We know that sophisticated mathematical tools are now used to deal with problems of the stock market. Have those problems also contributed to and possibly influenced the theoretical development of probability theory? If so, could you give us some examples?

F: I think there is an interplay between direct concerns with the stock market and the development of probabilistic concepts and methods. One very basic mathematical object in probability theory is Brownian motion, which plays a fundamental role for a number of reasons. Brownian motion was proposed (not under that name) in 1900 by Bachelier in his thesis with Poincaré in Paris as a model for price fluctuations in the stock market. Thus the aim to describe price fluctuation in mathematical terms has motivated a very important step in the development of the theory of stochastic processes. From then on, the original financial input to the theory of Brownian motion was for a long time forgotten. The theory of Brownian motion was developed on its own for intrinsic mathematical reasons and it was only in the 60s that the original work of Bachelier was taken seriously again from the financial point of view. The group of Paul Samuelson at MIT started to use it systematically in the mid-sixties, and since then Brownian motion (on the logarithmic scale) serves as a benchmark model in finance.

I: Was it at a rigorous level?

F: The original work of Bachelier contained a number of important ideas. From the modern point of view, it was not as rigorous as what you would like to see nowadays. The fundamental mathematical problem of constructing Brownian motion rigorously as a measure on the space of continuous paths was only solved 23 years later by Norbert Wiener. But on the more qualitative level, some very important ideas, for example the reflection principle for Brownian motion, already appeared in Bachelier's work. It also contained a formula for option pricing. It's not the one

which later became the canonical pricing formula because it was based not on a logarithmic Brownian motion but on the original Brownian motion itself, and one crucial argument for the Black-Scholes formula was missing, namely the construction of a perfect hedge.

You asked whether those problems contributed to and possibly influenced the theoretical development of probability. My answer would be "yes". I have already given the first example. The introduction of Brownian motion was motivated by the financial interpretation. Another example is the revival of martingale theory in the late 80s. Martingale theory had flourished in the 60s and 70s. The financial interpretation suddenly provided a fresh look and new questions. Several theoretical developments are due to that financial interpretation. One example is the pricing theory in incomplete financial markets. Let me explain. From the mathematical point of view, the Black-Scholes formula simply reduces to the following basic fact about non-linear functionals of Brownian motion. A fundamental theorem of Kiyosi Itô says that such a functional can be represented as a stochastic integral of Brownian motion. In the financial interpretation, the integrand can be interpreted as a trading strategy. The non-linear functional describes the payoff of a financial derivative, for example a call option. Thus Itô's representation theorem shows how to represent the payoff as a result of a trading strategy involving the underlying financial assets. This leads to a recipe for pricing. The initial constant which generates, using the trading strategy, the payoff may be viewed as the cost of replicating the financial derivative. This implies that the initial cost is the right price for that option. Otherwise there would be an arbitrage opportunity. That is the key to what is known as the Black-Scholes formula. From the mathematical point of view, one could say that it is simply an application of a basic representation theorem in stochastic analysis for functionals of Brownian motion.

I: Who was the first to make this observation?

F: Originally, the Black-Scholes formula was not derived by a representation theorem. It was derived by a direct argument using the Itô calculus and the solution of an appropriate partial differential equation. The full power of the representation theorem is needed if you pass from simple financial derivatives such as call options to more exotic options. Then you need the functional on the full path space. The connection to the representation theorem was clarified by David Kreps, whom I've mentioned earlier, Michael Harrison and Stan Pliska in the 80s. They recognized the relevance of previous work on the representation problem which had been done in martingale theory. It is known that

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the representation theorem holds if and only if there is a unique martingale measure. How to explain a martingale measure? Typically one fixes a probabilistic model for the price process, for example, a geometric Brownian motion. Such a model is specified by a probability measure on path space. If you now change the model by switching to another probability measure which is equivalent to the original one such that the given process becomes a martingale under that new measure, then that measure is called a martingale measure. To be a martingale means to behave like a fair game with respect to that measure. This notion of a martingale measure is very fundamental in mathematical finance. That the representation theorem holds is equivalent to uniqueness of the equivalent martingale measure. This had been shown, quite independently of the financial interpretation, already in the 70s and early 80s in the French school of probability; in particular by Jean Jacod and Marc Yor.

New questions which arose had to do with the fact that the martingale measure may not be unique. Then the situation becomes more complicated. The question arises: which martingale measure should one choose as the pricing mechanism. How should one construct a reasonable hedging strategy? This question leads to a projection problem for martingales and, more generally, for semi-martingales. It triggered off a new development in probability theory where the projection theory of Kunita-Watanabe for martingales was extended to semi-martingales. So that was a new version of a basic projection problem in probability which was motivated by finance.

Another example is the following. If you want to hedge the financial derivative, you may insist on staying on the safe side and make sure that there is no shortfall at the end of the day. Mathematically, this leads to the theory of super-hedging which can be seen as a new generalization of the classical Doob-Meyer decomposition for supermartingales, a fundamental theorem in martingale theory. This new version, now often called the optional decomposition theorem, was developed first, in a special context, by Nicole El Karoui and then in full generality by Dima Kramkov, a former student of Albert Shiryaev in Moscow, at that time a postdoc in Bonn, and now professor at Carnegie-Mellon. For this work he received a prize of the European Mathematical Society for junior mathematicians in 1996. This is another example where a question in finance led to a new problem in probability and triggered off a significant advance on the theoretical level.

I: The concepts and ideas are totally new?

F: The optional decomposition is definitely a new step. It is not a straightforward generalization. You can see that in discrete time. There the Doob-Meyer decomposition can be written down in three lines, but the extension to the optional decomposition, even in discrete time, takes several pages. It involves a new combination of martingale arguments and arguments from convex analysis. It is not just a technical refinement; it is a conceptual advance.

I will give you a third example. In applying arguments from mathematical finance, you usually fix a probabilistic model. Typically, there is a significant amount of model uncertainty. You cannot be sure that the chosen probability measure really describes the objective situation. One way of dealing with that is to take into account a whole class of possible probability measures. Then many new problems arise. For example, the classical problem of optimal portfolio choice translates into a new projection problem. You have to project the whole class of model measures on the class of martingale measures. In the usual case, you would simply project one single measure on a given convex class of measures. This problem is well understood, especially if the projection problem is formulated in terms of relative entropy. The question of model uncertainty leads to a new robust version of the classical projection problem, which has been treated only recently. It has been solved last year in joint work with Anne Gundel while we were both at the IMA in Minneapolis for a program in financial engineering.

I: Does the computer play a significant role in your work on stochastic finance? Do you rely on the empirical data to shape your ideas?

F: I am staying on the theoretical side. I do not work myself on the computer or use simulations, but I follow some of the developments on the empirical side. Some of my own work is motivated by empirical work on the microstructure of financial time series. There are new modeling issues which arise. For example, if you look at financial data on a tick by tick basis, it provides the motivation to model the dynamics of an order book. So you do not immediately switch to the mesoscopic level of description by means of stochastic differential equations but you try to model the dynamics of the market microstructure. That also raises the question of how do you model in mathematical terms the interaction of many agents who trade and place their orders. To develop mathematical models for the microstructure of financial markets is a very challenging research program which calls for methods developed in the theory of interacting particle systems. I've recently been involved in some related issues in joint work with Ulrich Horst, a former PhD student in Berlin who is now at UBC in Vancouver, and with Alan Kirman, whom I have already mentioned before.

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I: It seems that mathematical finance is built on axiomatic and abstract principles (like the efficient market principle). Have these principles been tested and verified?

F: The efficient market hypothesis comes in different forms. In its strong form, it says that the price fluctuation you observe behaves like a martingale. As a special case, it would be the random walk hypothesis, which assumes that price moves like a random walk. What you see is usually not so far from the martingale property, but there is a lot of evidence that you should not take it literally. In fact, that form of the hypothesis is too strong. If you move away from that hypothesis, it means that due to a basic systems theorem of Doob there are strategies that generate a positive expected gain. But there is a weaker form of the hypothesis which is much more flexible. It says the following. There may be strategies with positive expected gain but it is not possible to have positive expected gain and zero downside risk. In other words, there are no free lunches. That makes economic sense because if free lunches were available, there are enough clever people around to seize the opportunity and to wipe them out. In this more flexible form, the hypothesis is widely accepted. There is a broad consensus that you don't find free lunches even though you may be able to make profits with positive expected gain accepting some downside risk. In this weaker form, the hypothesis has been a rich source of interesting mathematical developments. It has been shown that the absence of arbitrage opportunities is mathematically equivalent to the fact that there are equivalent martingale measures. That is an existence theorem. Modern mathematical finance starts on that basis.

I: Can mathematical finance be considered a science?

F: If you translate the question into German, the answer would be clearly "yes". In German, "science" is "Wissenschaft" and "Wissenschaft" is rather broad. It's not just natural science, it also includes the social sciences, economics, and finance as well.

I: If it is a science, one should be able to falsify principles or hypotheses in mathematical finance.

F: Yes. For example, there is a lot of empirical evidence that the efficient market principle in its strong form does not hold. Personally, I do not work under that strong hypothesis. I do work under the weaker one. There is no significant evidence that I know of which would refute it.

I: Does it mean that in finance there are such things as laws that govern the behavior of stock markets?

F: I do believe in the relevance of probabilistic laws in finance. It's reasonable to describe price fluctuations in terms of probability measures on certain path spaces. The absence of free lunches amounts to the existence of an equivalent martingale measure, and this implies that continuous price fluctuations are nowhere differentiable. This can be viewed as a law which explains the erratic price behavior of a liquid stock which you actually see on a mesoscopic time scale. If you take the problem of pricing financial derivatives, you can show that a price must satisfy certain bounds if it does not create arbitrage opportunities. Such arbitrage bounds can be seen as a law, too. Mathematical finance is certainly a science, by my understanding of science.

I: Have you done consultation work for any financial organization?

F: No, I have not done that personally. But some of my co-authors have been involved in that. I have former students who are involved in that. I am following some of their activities, but I try not to get involved myself.

I: It's very lucrative.

F: It may be lucrative, but it also may change your life. I had occasion to watch while working on a joint paper how my co-author was, every once in a while, called to the phone because the program had to be urgently modified in some bank where his ideas were being implemented. I would not like that kind of pressure.

I: What is your advice to graduate students who are keen on a career in mathematical finance?

F: My advice to my own students is to get a broad and solid education in mathematics and not to specialize too early. Even if you decide to work in this area of finance, either in academia or the financial industry, it's a field that evolves rather fast. You need a lot of flexibility, also on a mathematical level. It's not clear that it will be enough to know the tools, for example, needed to understand the Black-Scholes formula. Other challenges may come up which may require very different techniques. I already gave you one example — the microstructure of financial markets. You have to be a good probabilist to react efficiently and to use other methods as well. To my own students, I recommend them not to narrow down too early but to make sure that they are comfortable with a wide range of techniques in probability and analysis.

Avner Friedman: Mathematician in Control >>>
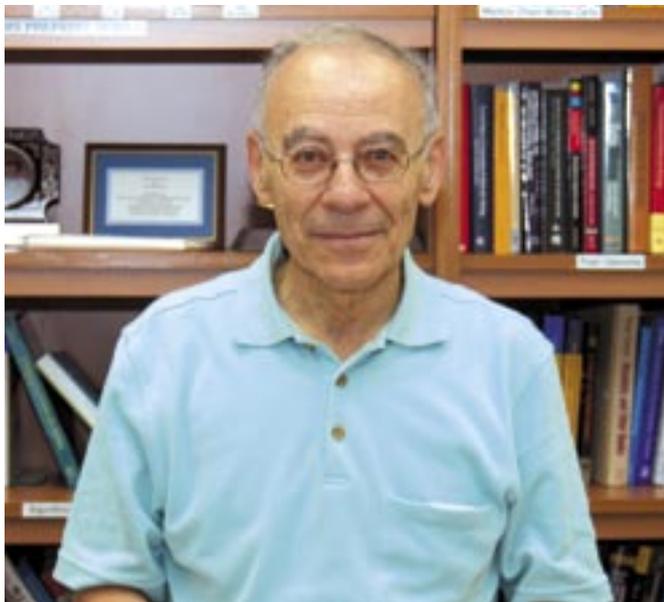
Avner Friedman

Interview of Avner Friedman by Y.K. Leong (matlyk@nus.edu.sg)

Avner Friedman has made important contributions, both in theory and applications, to partial differential equations, stochastic differential equations and control theory. His career, especially during the past two decades, epitomizes a personal mission and relentless drive in bringing the tools of modern analysis to bear in the service of industry and science.

His distinguished career began at the Hebrew University, Israel and weaved, in a somewhat colorful way, through Kansas, Indiana, Berkeley, Minnesota, Stanford, Northwestern and Purdue, culminating in the directorship of the Institute for Mathematics and its Applications (IMA) , Minnesota (1987–97), Minnesota Center for Industrial Mathematics (MCIM) (1994–2002) and Mathematical Biosciences Institute (MBI) of the Ohio State University (2001–). He is also the Distinguished Professor of Mathematical and Physical Sciences at Ohio State University, the latest in a chain of numerous distinguished professorships in the universities he has passed through.

His service on many U.S. national boards and advisory committees is an indication of his boundless energy and selfless efforts in promoting the applications of mathematics and advancing the mathematical sciences. Among the honors and awards he received for his wide-ranging contributions are the Stampacchia Prize, NSF Special Creativity Award, and membership of American Academy of Arts and Sciences and of the U.S. National Academy of Sciences, He has served and continues to serve on the

editorial boards of numerous leading journals in analysis, applied mathematics and mathematical physics. His prolific research and scholarly output has resulted in more than 400 publications, written singly and jointly, and 20 books. He has always been in demand for invited lectures in and outside the U.S. Even at the biblical age of three score and ten and beyond, he is directing a concerted effort to bring problems of the biosciences within the reach of the mathematical sciences.

As a founding member of the Scientific Advisory Board (SAB) of IMS since 2000, Friedman has contributed to the development and success of the Institute in its first five years. On his annual visit to the Institute, he was interviewed by Y.K. Leong on behalf of *Imprints* on 6 January 2006. In the following edited and vetted account of the interview, one can feel the palpable excitement of applying mathematics to the real world and of being drawn into the personal world of a creative and gregarious personality.

Acknowledgment. *Imprints* would like to thank Dr Lynn Friedman for assistance in the preparation of this version.

Imprints: What was the topic of your Ph D thesis? Did it set the general direction of your future research?

Avner Friedman: My thesis was in partial differential equations. It dealt with several different subjects. I have been involved in differential equations my entire career, but have also diversified to other areas.

I: You didn't change fields?

F: I didn't change fields in the sense of going from partial differential equations to algebra. But within partial differential equations, I diversified to a number of areas. Partial differential equations are used in, for example, control theory, applications to industry and, recently, mathematical biology.

I: You went to University of Kansas immediately after your doctorate. Was there any specific reason for this decision?

F: One chapter in my thesis dealt with the so-called problem of unique continuation. Professor Nachman Aronszjan, at the University of Kansas, had done some very important work on unique continuation. I wrote him about my results, and, soon afterward, he invited me to come as a research associate to his department. I was there for one year.

I: From your publications, it seems that initially you were primarily interested in the theoretical aspects (analysis) of partial differential equations but very soon afterwards, you also did and continue to do a lot of work in applied areas

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like control theory and stochastic differential equations. When and how did that happen?

F: I have always worked on partial differential equations, and I have looked for areas where they can be applied. That's why I started to migrate into areas of applications such as control theory. For a while, I went completely into stochastic differential equations because there were interesting problems in game theory, that is, stochastic games: problems of pursuit of objects when only partial information is known. It turned out that this topic was very well connected with partial differential equations, and I came back to doing partial differential equations through stochastic differential equations. By exploring these applications, I enriched my areas of knowledge and research.

I: Did those applied problems contribute new insights or new developments in partial differential equations?

F: Absolutely. They were very exciting problems. I started to be interested in real applications in the late 1980s when I was exposed to problems in industry that some of my colleagues, especially in England, were tackling. Later on, I moved from Northwestern to Purdue and then to Minnesota to be the director of IMA (Institute for Mathematics and its Applications). By that time I was completely immersed in problems from industry, and I found out that a large number of theoretical problems in partial differential equations came out of industrial problems.

I: From your large number of publications, it seems that not only are you prolific in writing papers on your own but you also enjoy collaborating with a lot of people. How much of this is due to your own personal temperament and how much to a research philosophy that is consciously pursued?

F: I think that if you look at the trend in mathematics, you will see that increasing numbers of papers are co-authored by two, sometimes three people. More and more, mathematicians and mathematical scientists are talking among themselves. It is extremely stimulating to do so, especially in applied areas. Many of my first papers were done alone, but most of my work now is joint. I often collaborate with others, particularly my former students.

I: Unlike the term "applied mathematics," the term "industrial mathematics" is a relatively new one. Could you tell us briefly what exactly is "industrial mathematics"?

F: In applied mathematics, you pick up problems from the sciences, engineering and other academic disciplines; you may look at the literature to find out where the problems are and try to solve them using mathematical tools. You may discover new mathematics. In industrial mathematics,

by contrast, you go to industry to find the problems. The problems are not usually published, and you have to talk to people. You have to find out what those in industry are interested in *today*, because tomorrow they may be interested in something else — or they may be out of job. Find out what they are doing now, what is interesting to them and what the time horizon is for solving the problems. Then you may talk to them, or to your colleagues, or simply think by yourself to come up with suggestions for a solution. You don't necessarily need to find complete solutions. If you publish a paper in mathematics, you must present complete proofs. In industrial mathematics, you may get a 90 percent instead of 100 percent solution, but you must get it in a timely fashion.

I: Is work in industrial mathematics usually acceptable to journals in mathematics for publication?

F: Oh, yes. In the IMA, we had a seminar for industrial mathematics, and we had about 25 speakers every year coming from industry. Each one came with a different set of problems. I wrote up, and sometimes rewrote, the problems. There are 10 volumes of these, each containing about 25 sets of problems in a particular subject. About 50 publications were based directly on these problems. There are another 50 papers that might be called second-generation. For example, there was a lot of work done in optics, in scattering, that came from my contacts with Honeywell and some other companies. This has been pursued by some of the people at the IMA, some of my students and postdocs, and they are still working on them with Maxwell's equations. There is a stream of papers that has come out of industry.

I: You mentioned that you published a series of volumes on industrial mathematics. They are not papers but actually books.

F: Yes, in each chapter, there is an introduction to the industrial problem, and then I formulate open problems for mathematicians.

I: It's quite encyclopedic in scope, isn't it? This must have required tremendous energy.

F: Yes, but energy is a function of enthusiasm.

I: Has any of your applied research been used in industry?

F: Absolutely. Work that we have done in optics, called "diffractive grading," was used by Honeywell in order to get grants from the defense department. Also in collaboration with postdocs, I did some work that led to patents at Ford Motor Company. Work that I did myself involving semiconductors and modeling was used by Motorola in chip

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design for instrumental control to control the acceleration of a car.

I: You are also working on problems in biology?

F: Well, that's what we do at the MBI. I am personally fascinated by the mathematics of cancer, which happens once again to involve partial differential equations.

I: What is your most satisfying piece of applied research work?

F: Well, I think the most satisfying piece of research is whatever I'm working on now. Whenever you work on a problem, it is the most exciting thing in your life for the time you are working on it. If you work in a field that is rapidly developing, it's not just one paper but a sequence of papers. Right now, we have a very interesting line of research that is motivated by cancer, but is nonetheless pure partial differential equations. This is the question of bifurcation problems in free boundary problems. The solid tumor is a moving region, and you don't know how it's going to move and grow. It develops fingering and so on. We try to prove theorems for moving boundaries with fingers, developing fingers as bifurcations. This is really an open area of problems.

I: Do you have to talk to other people like biologists?

F: I would say that I *get* to talk to biologists, specifically to experimentalists who work as oncologists. Of course, I also talk to other mathematicians working in partial differential equations.

I: Do the biologists seek you out to solve their problems?

F: At first, I go to them. When they are convinced that we are actually useful to them, then they also come to us. That has been my experience.

I: But it's not very easy to convince a pure mathematician to go and solve those problems.

F: It's not easy at first, because you have to do a lot of work before you can be useful to the biologists. You have to learn a lot. But I started the MBI because I was certain that mathematicians could make key contributions in the biosciences. Now, it's my personal research interest and my administrative role combined. And we have 14 postdocs involved in different fields of the biosciences. Some of them work on cancer and others on neuroscience, physiology, ecology, genomics, etc.

I: Can you tell us something about the IMA and MBI?

F: The IMA was started in 1982. There was a national competition for mathematical institutes. The NSF decided to have two — one in Berkeley in core mathematics, and the other in Minnesota in more applied work. Hans Weinberger was the director of the Minnesota institute for its first five years. I succeeded him as director. At that time I started to emphasize interaction with industry in addition to general applied mathematics. My point of view was that applied mathematics could only gain wide acceptance, say, in industry, if those doing mathematical research in industry knew you actually could connect with and care about the problems with which they were dealing. In addition, I thought you would find very interesting problems in industry, so I started to visit companies. Typically, I would spend two days in one company and talk to about 20 people. Out of these, I would identify one or two people whose problems might benefit from mathematical input. I would then invite them to talk in my seminar.

After 10 years, I stepped down from IMA, and started the Center for Industrial Mathematics in University of Minnesota. It is a degree program. Graduate students who want degrees in applied and industrial mathematics spend a summer internship in a company and come back to author a masters thesis. Some of them continue to write PhD theses supported by industry.

When NSF called for new proposals, I was already interested in the opportunities biology was bringing to mathematics. I was in Minnesota at that time, and you can't expect NSF to support two institutes in one department, so I worked together with people at Ohio State University to write a proposal in mathematical biosciences. It was a good time for OSU: the medical school was hiring many new people in biological sciences, and people in statistics were very active in biology. Our proposal was successful, and I became the first director of the MBI.

I: What is generally understood as "applied mathematics" in the United States?

F: Keith Moffat and I have talked about the fact the "English applied mathematics" has a different flavor from "US applied mathematics." To give you a flavor of US applied mathematics, materials science is an important area of applied math in the United States. You can use mathematics in the modeling of it. For example, car companies want to increase mileage per gallon — it's a government requirement. To do so, they want to replace steel with lighter material, say aluminum. But aluminum is not strong enough, so they add carbon particles to make it stronger, and it turns out that partial differential equations can be used to predict how this new material will behave. Ford Motor Company actually came up with a problem and we did some work on it at the IMA.

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Michael Todd: Optimization, an Interior Point of View >>>

Michael Todd

Interview of Michael Todd by Y.K. Leong (matlyk@nus.edu.sg)

Michael Todd is well-known for his fundamental contributions to continuous optimization, both in the theoretical domain and in the development of widely-used software for semidefinite programming.

His research work has left a deep impact on the analysis and development of algorithms in linear, semidefinite and convex programming; in particular, on interior-point methods, homotopy methods, probabilistic analysis of pivoting methods and extensions of complementary pivoting ideas to oriented matroids.

He did his B.A. at Cambridge University and Ph.D. at Yale University. Except for a two-year stint at the University of Ottawa, his scientific career began and developed into prominence within Cornell University, where he is now the Leon C. Welch Professor in the School of Operations Research and Industrial Engineering.

He has been invited to give talks at major scientific meetings and universities throughout the world. He held special appointments at leading universities and centers of research in economics and operations research, such as the Fields Institute (Toronto), Carnegie-Mellon University, the Cowles Foundation for Research in Economics (Yale), the OR Center (MIT), the University of Washington, BellCore (US), Cambridge University and the Center for Operations Research and Econometrics (CORE, Leuven, Belgium). He has served, and continues to do so, on the editorial boards of leading journals on optimization, operations research and computational mathematics. Among the honors and awards given in recognition of his important research are Guggenheim and Sloan Fellowships, the George B.

I: Are these predictions successful mathematically?

F: Yes, it turns out that the predictions have been very useful to the engineers. As a result, the field has completely changed since our first materials science program in 1985. The mathematical community of people working in materials science has increased tremendously. Other examples of US applied mathematics come from applications in control theory, computational science, applied linear algebra, fluid dynamics, scattering theory, nonlinear waves in oceans and materials, polymeric materials and polymers.

I: What about operations research?

F: Operations research applications have ranged from manufacturing to finance, and there is so much more. Imaging has developed rapidly in many aspects: imaging distant targets is a different problem than imaging at the

molecular level. Speech recognition — we have a volume at the IMA in speech recognition — involves Markov processes. Applications even come in from traditionally pure mathematics. The field of U.S. applied mathematics is vast and diverse. We had programs in applied number theory, in coding, communications, graph theory, scientific computation as well as fluid dynamics.

In England, by contrast, fluid dynamics used to be the crowning theme, because England is surrounded by water. Traditionally, England is very strong in computational fluid dynamics, and they are looking at all kinds of phenomena in waves and fluids. Many of the mathematicians working on these problems inspired me to get involved in applied mathematics in the first place and ultimately to bring industry to the table to expand the kinds of problems mathematicians are involved in solving.

Dantzig Prize of the Mathematical Programming Society and SIAM, the John von Neumann Theory Prize of the Institute for Operations Research and the Management Sciences (INFORMS) and INFORMS Fellow.

Todd has close research links with NUS faculty in the Department of Mathematics and was Chair of the Organizing Committee of the Institute's program on "Semidefinite programming and its applications" held in 21 December 2005–31 January 2006. During his visit for this program, Y.K. Leong interviewed him on behalf of *Imprints* on 9 January 2006. The following is an edited version of the interview in which he gives us a stimulating glimpse of the theoretical insights behind one of the most important applications of the mathematical sciences to operations research, engineering, economics and industry.

Imprints: You did a B.A. in mathematics at Cambridge and went to Yale to do a Ph.D. in administrative sciences. Was the thesis topic a mathematical one?

Michael Todd: Yes. I took a course from Herbert Scarf in mathematical economics at Yale. He described his recent work in computing approximate fixed points, and I got very fascinated by his work and, in general, by complementary pivot algorithms which use purely combinatorial arguments to solve optimization problems. I wanted to understand the combinatorial background to these methods. That was the basis of my thesis. It was indeed a mathematical one. "Administrative sciences" is a strange name. There aren't too many departments of administrative science, and they chose it so that it didn't sound too much industrial, too much business school. Basically, it's about the science and mathematics of decision-making.

I: Why didn't you go to the mathematics department instead?

T: I had been supported in Cambridge by Shell. They had a fellowship for me and they suggested that I go abroad for a couple of years to a business school. With a fellowship between my college in Cambridge and Yale, I went there mainly to see America for a couple of years and then I decided to stay because it was fascinating. Choosing the department was sort of difficult, and it was really an accident.

I: Were you interested in pure or applied mathematics right at the beginning?

T: At Cambridge, my work was basically in pure mathematics, but towards the end of it — and especially when I was at Yale — I decided that the applications were interesting. I got

fascinated by the applications, in particular, by algorithmic questions.

I: Is semidefinite programming a generalization of linear and convex programming?

T: Semidefinite programming is a generalization of linear programming. In linear programming the variable is a vector whose components all have to be non-negative. In semidefinite programming, you have a symmetric matrix and all its eigenvalues have to be non-negative, so it has to be positive semidefinite. So it is more general than linear programming but it is a subclass of problems in convex programming.

I: Could you give us some examples of problems that involve semidefinite programming?

T: One of the nicest things about semidefinite programming is the wide range of areas in which it has been applied. I think that the first interest probably came from people in control theory who wanted to study ways of controlling dynamical systems optimally and making sure that they were stable. That led to inequalities that required certain matrices to be positive semidefinite. There are also applications in a completely different area related to combinatorial optimization problems connected with graph partitioning. Another source of semidefinite programming is robust optimization, which has been a hot topic recently. All of these different areas lead to an interest in efficient algorithms for semidefinite programming.

I: Is there an optimally efficient algorithm for solving linear programming problems?

T: That's the holy grail of linear programming research. It's a very intriguing situation. Now we have two different classes of algorithms — simplex algorithms and interior-point methods, and there is wide disparity between them on some classes of problems — sometimes one is much faster than the other. They are very different theoretically. The simplex method in the worst case is exponential but seems to perform very well in practice. Interior-point methods have a polynomial time bound and they perform much better than that bound in practice. For large-scale problems, it is not clear which one is the more efficient. There may be some new methods that will do even better. We're still waiting to hear about that.

I: Are these two methods connected?

T: Not very closely. They are based on very different geometric views of linear programming. The set of feasible

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solutions in linear programming is a polyhedron and the optimal solution always lies at a vertex. So it's natural to consider algorithms that just go from vertex to vertex and that's what the simplex method is based on — an algorithm that traces the skeleton of this polyhedron. Interior-point methods move through the interior and make smooth approximations. So they ignore much of the combinatorial structure and look at the analytic structure.

I: So one is discrete and the other is continuous.

T: Exactly. Interior-point methods never get an exact solution unless you do a special rounding procedure, but they get very, very close, and incredibly fast: you have to solve a very small number of systems of equations which are more complex than the equations in the simplex method.

I: How do you know which method to use?

T: It could be based on the software that you have. Most efficient commercial software allows you the option to use either one. I think people look at their class of problems and decide which one works better for their problems.

I: Which one is more popular?

T: I think for historical reasons the simplex method is more popular, but if you want something jazzy, the interior-point method is certainly a wonderfully efficient method for solving these problems.

I: Are there any probabilistic methods?

T: There are but we should distinguish two viewpoints. First of all, some algorithms make random choices and there are some very interesting theoretical ideas that have been used in low-dimensional problems that have much better computational complexity on certain classes of problems than the more usual ones. But there are also probabilistic analyses of the deterministic algorithms that people typically use on large-scale problems. Simplex and interior-point methods work in practice much, much better than their worst-case bounds. We would really like to understand that. One way to do it is to assume that the problem is random and to understand the average behavior of the algorithm on random problems. Some very interesting results have been obtained along those lines.

I: I noticed that there is a mention of homotopy in one of your papers. Is there something topological about it?

T: I think it is more a question of how the methods are based on different geometric views, and earlier I described a little bit how the simplex method is based on the combinatorial

geometry and the interior-point method on the convex geometry. My earlier work was related to algorithms for computing approximate fixed points: homotopy ideas come up, but also the combinatorial topology and geometry of triangulations. Those algorithms were very interesting but not too much can be said about their computational complexity. They tend to be useful for small dimensions, up to maybe 50, on very nasty nonlinear problems, whereas linear programming and semidefinite programming problems are often much, much larger and more highly structured.

I: Is the software for implementing the algorithms freely available?

T: That really depends on whether you are talking about linear programming or semidefinite programming. Linear programming is very widely applicable and has huge commercial implications. So the very best codes cost you some money, but there are some very good codes that you can obtain freely. There are a couple of websites where you can get some good codes for linear programming. But for semidefinite programming, the market is probably more in the scientific and engineering community; so you can't charge them a lot of money. Most of the algorithms are freely available, and several of those are available on the web. A good starting point is the NEOS Solver for Optimization site.

I: Have you written some of those yourself?

T: Yes, actually with one of my National University of Singapore colleagues and another colleague: Kim-Chuan Toh, who's in the Mathematics Department here, and Reha Tütüncü of Carnegie-Mellon University. We have a package for semidefinite programming, and it can also be used for linear programming.

I: Could you give us an idea of the complexity involved in semidefinite programming?

T: I'll give you some sort of an idea. First of all, these interior-point methods have been extended from linear programming to semidefinite programming. They typically take a very small number of iterations, perhaps 10 to 50, but each iteration involves a lot of work. Even if you have a problem with sparse data, in the semidefinite case you have to solve a generally dense large linear system of equations and that can be very costly. So these methods are typically very computationally burdensome, and the number of linear constraints can only get up to a thousand or two. These algorithms can give very accurate solutions. Other classes of algorithms, based more on first-order methods, can solve much larger problems with tens of thousands of constraints. They get much less accurate answers and don't have such

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good complexity bounds, but can be quite fast in practice. I'd say a thousand to ten thousand is the order of the matrices involved and the number of constraints that you can handle with these methods.

I: Can all linear programming problems be solved in principle by quantum computers or a theoretically most powerful computer?

T: I don't know a huge amount about quantum computers. From what I understand, I think it is possible to solve linear programming problems in one step. There's only a finite number of possible options, the vertices of the polyhedron, and the quantum computer is allowed to examine them all simultaneously and pick out the best. Similarly for biological computers based on DNA and so forth. I don't know how practical these methods are. For semidefinite programming, I don't see that you can get an immediate solution, but it will be interesting to find out.

I: What happens if one day we really get quantum computers? Will linear programming problems be trivialized?

T: Yes, but maybe also all NP-complete problems too. It's not clear that these methods can really push all problems that are currently considered interesting to become totally trivial. I don't know whether such computers will really ever become that practical.

I: Do you believe in quantum computers?

T: I think it is a nice theoretical concept to consider, but I'm not expert enough on computers to comment on that.

I: Do you consider yourself to be an applied mathematician or a pure mathematician?

T: I'd say applied mathematician — that's what I say to people I meet on the plane. But just as with pure mathematics this generally gets the same response, "That was my worst subject. I don't understand it at all," which is very unfortunate. Sometimes I try to explain some of the nice things that mathematics can do.

I: Do you think algorithmically or geometrically?

T: I think geometrically a lot of the time. There are so many different ways of looking at optimization problems, from optimality conditions, to the theory of the algorithms and the modeling. I try to keep computational concerns in the back of my mind, but I'm still very interested in the theory as well. The geometric viewpoint on optimization problems really attracts me.

I: But at the end of the day, you still have to do the computations.

T: Yes, you do, and it's nice to be within, say, six degrees of separation, or fewer, from people who are actually practically solving applied problems. Even if you are not producing the software, you are motivated by improving the algorithms so that people can actually solve larger problems faster.

I: Except for two years in Canada, you have been at Cornell right from the beginning of your career. Have you ever thought of moving to other universities?

T: There have been a few times when I thought about it. But overall, Cornell has been a very attractive environment for me. The School of Operations Research and Industrial Engineering has some wonderful colleagues, both in optimization and more generally in operations research. The university as a whole, and mathematics and engineering, have wonderful people, and the quality of the graduate students has been terrific. I really enjoy working with the students in operations research and applied mathematics. It's also a wonderful place to live and very naturally beautiful.

I: Do you talk to people in economics?

T: Economics, once in a while, probably less than people in engineering, computer science, mathematics, but still occasionally, yes. My interest in economics was more during the 70s, a long, long time ago. I have sort of lost touch with the latest things that have been done now.

I: What advice would you give to a graduate student who is interested in applied mathematics?

T: You really need to find a problem where you feel so excited about it that you have a fire in your belly to keep working on it. You should look at all options, keep your options as open as possible, find an advisor to help you see the right approach at the right time and to let you do what inspired you to work in the area, and hope you find the way ahead of you.

I: Have you gone back to Britain?

T: I've gone back socially, for family reasons or whatever, every year or so. I spent one sabbatical back there and I've been back for several conferences. I find in the area I'm working in there are interesting people in many places in the world: in England, but also in Belgium, France, Germany, Japan and Singapore besides the US that I work with as well.

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I: The name of the school you are in — "School of Operations Research and Industrial Engineering" — seems to give people the impression that it has very little to do with mathematics.

T: It's more of a question of how it evolved. We have people who are much more involved with practical work and consulting, but I think many of us regard ourselves as a mathematical sciences department within engineering. We have people working in applied probability, statistics, and optimization, from quite a theoretical viewpoint to a more practical viewpoint. It's nice to have that full spectrum, but many of the faculty were very well-trained mathematically. A lot of us have appointments also in the Center for Applied Mathematics, and some people have appointments in mathematics as well.

I: How is your relation with the engineers?

T: Pretty good. Some fields of engineering are closer than others. We are not too much involved in the experimental side, but for example our relations with electrical engineering and computer science are very good.

I: Do you try to educate the engineers mathematically?

T: I try. I very often have students from other parts of engineering taking my classes. Along with the modeling and computation involved, I try to make them understand that the abstract viewpoint can be valuable. I hope they appreciate the beauty of mathematics. I think that in a strong engineering college, the students are pretty much aware of the advantages of having good mathematical training, particularly the graduate students.

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