IMPRINTS
INSTITUTE FOR MATHEMATICAL SCIENCES
SEPTEMBER 2007

Mathematics: The Movies >>>

Weavers of math and digital magic

One fine Sunday morning (1 July 2007) an animated part of Hollywood came to town (Raffles City, Singapore) to tantalize and mesmerize a large gathering of scientists, academics, professionals, educationists, civil servants, students and retirees with a rare and unusual revelation of blow-by-blow snippets of animation used in "disaster" movies like *Poseidon* and recently produced war movies. It was, to use the well-worn cliché, eye-opening and mind-blowing to catch a glimpse of some of the trade secrets of the movie industry as unveiled by computer scientist and digital media expert Douglas Roble, Director of Creative Software of Digital Domain. To the layman, the black box holding these secrets is inaccessible, being shrouded in the abstraction of mathematics and computer science. The visual results are stunning – anyone would have been impressed – but only the initiated is aware that the road from theory to applications is a long and arduous one.

The day-long symposium, officially titled "Symposium on mathematics and science in digital media, technology and entertainment" literally began with screen "fireworks" which one would expect as highlights of the day's festivities. Although the Chairman of A*STAR, Lim Chuan Poh, had said in his opening speech how impressed he was with the computer-generated animation when he visited Lucasfilm at San Francisco recently, there was little hint of the visual treats that would follow soon afterwards. True, the publicity poster for the symposium had mentioned that two movies *(Poseidon* and *Pirates of the Caribbean: Dead Man's Chest)* out of the three nominated for the special effects Academy Award at the 2007 Oscars award ceremony used numerical simulation. But it sounded all too scientific and academic. It seems that seeing is not necessarily believing – one could not believe that what one sees in the movies is not real. The work of practitioners like Roble and of theoreticians like the following three speakers has blurred, almost obliterated, the line between reality and illusion. The art form of movies is now invaded by the scientific, in fact mathematical, preciseness of computer graphics.

After the initial excitement of Roble's presentation, one is brought down to mathematical reality with the hard stuff by Peter Schröder of the California Institute of Technology. We learn that in the maps of the 16th century cartographer Mercator, designed originally for the practical purposes of navigation, are sown the seeds for the geometric ideas needed to animate modern movies. Schröder gave the audience a geometrically dazzling and dizzying crash course (minus the esoteric details) from the existence theorem of the Riemann Mapping Theorem in complex analysis to the representation of surfaces using networks of meshes.

Next, Stéphane Mallat of École Polytechnique, who is also the Chairman of the start-up *Let it Wave*, gave the audience an insight into how he and his colleagues took the plunge

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People in the News >>>

Congratulations to Sergio Verdú

IMS wishes to express its heartfelt congratulations to Sergio Verdú for being named the 2007 recipient of the Claude E. Shannon Award of the IEEE Information Theory Society. The award honors "consistent and profound contributions to the field of information theory." For his "contributions to multiuser communications and information theory", Sergio was also elected to the U.S. National Academy of Engineering in 2007. For some fascinating insights into Sergio's personal and professional journeys, readers will certainly not want to miss his interview in this issue of *Imprints*. IMS is delighted to have had Sergio Verdú as an invited speaker at the Institute's program *Random Matrix Theory and its Applications to Statistics and Wireless Communications* (26 February – 31 March 2006).

National Science and Technology Awards

Jon Berrick and Jie Wu, two of the organizers of the recently completed program Braids (14 May - 13 July 2007), have received the National Science Award (NSA) for "uncover[ing] deep connections between algebraic topology and the theory of braids". The NSA is Singapore's highest honor in science. Bravo, Jon and Jie!

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Emmanuel Candes: The story of compressive sensing

Captivated by movies – and mathematics

Panel discussion: (From Right) Zuowei Shen, Peter Schröder, Douglas Roble, Eng Chye Tan, Carl de Boor (hidden), Emmanuel Candes, Stéphane Mallat

from academia to industry and successfully developed and commercialized a revolutionary system of image compression technology (bandlet technology) for HDTV (high-definition television). It was a veritable first-hand lesson from a researcher-turned-entrepreneur.

The last talk by Emmanuel Candes of California Institute of Technology dealt with "compressive sensing", a rapidly expanding new field of which he is one of the main architects (the others being Tao, Romberg and Donoho). It described how conventional wisdom in signal processing (as encapsulated in the Shannon Sampling Theorem) was overturned by new mathematical techniques which will have tremendous applications in medical imaging and optical and sensing devices by reducing memory space, time and cost involved in storing and reconstructing images.

The symposium was wrapped up with a forum, chaired by NUS Provost Eng Chye Tan, in which the earlier four invited speakers, Carl de Boor (the pioneering master of splines) and Zuowei Shen (of NUS Mathematics Department) answered questions that ranged from technical details to down-to-earth questions about how to study mathematics. The passion of research, the perspiration of mastering the subject, the excitement of scientific discovery and the power of mathematics were vividly portrayed right up to the end. After coming out of the symposium, one would probably watch the movies in a different light and enjoy them even more.

Y.K. Leong

Programs & Activities >>>

Past Programs in Brief

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*

Fluid mechanists, physicists, biological scientists, computational scientists, applied and computational mathematicians and engineers were brought together in this program, which aimed to develop and promote interdisciplinary research on modeling, theory, and simulations in the area of fluid dynamics involving moving interfaces, with emphasis on applications towards the bio-medical field and the physical environment that are of relevance to industry and the defense community. It provided a platform for local and international researchers to exchange ideas, conduct collaborative research and identify future directions and developments in these fields.

35 overseas and 20 local speakers gave talks over 3 week-long workshops. The workshops were attended by a total of 163 participants. In conjunction with the program, a joint Department of Mathematics/IMS Winter School took place from 4 – 29 January 2007. The activities consisted of a workshop and tutorial conducted by Professor Zhilin Li (North Carolina State University). Visits were also arranged to Department of Mechanical Engineering, Department of Mathematics, Institute of High Performance Computing and Nanyang Technological University. The Winter School workshop also served as a component of the graduate course, "MA6251 Topics in Applied Mathematics I" given at the Department of Mathematics.

Interfacers interfacing

Eagerly imbibing fluid dynamics

Kazuyoshi Takayama: Shock and bubbles therapy

A dynamic group captured

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*

The main theme of the program was the mathematical structure of the braid group, together with applications arising from this structure both within mathematics, and outside of mathematics such as magnetohydrodynamics, robotics and cryptography. The program attracted 77 local and overseas participants.

A summer school of the Pacific Rim Mathematical Association (PRIMA) was incorporated as part of the program. It was jointly organized with the Department of Mathematics. The summer school, which was held from 4 – 29 June 2007, attracted 31 students from Canada, China, Japan, Korea, Malaysia, Mexico, Singapore, UK and USA.

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The students attended 3 week-long tutorials conducted by graduate student E-Jay Ng (National University of Singapore), Professor Dale Rolfsen (University of British Columbia, Canada), Professor Jie Wu (National University of Singapore), Professor Fred Cohen (University of Rochester, USA), Professor Mitch Berger (University College London, UK), Professor Robert Ghrist (University of Illinois, Urbana-Champaign, USA) and Professor David Garber (Holon Institute of Technology, Israel).

Wrapping up the program was a conference held from 25 - 29 June 2007. 22 international speakers each gave a half hour to an hour's talk to an audience size of about 60. There was a public lecture entitled "Robot Swarms and the Topology of Coordination" given by Robert Ghrist (University of Illinois, Urbana-Champaign) on 26 June 2007. Besides benefiting from the talks, the participants also enjoyed themselves during the sightseeing trips to Bukit Timah Nature Reserve, National Museum and the Night Safari.

What is that braided thing over there?

Alejandro Adem: Homotopy and cohomology

In celebration of a grand dame of mathematics: Joan Birman's 80th

A great link up of braid theorists

Dale Rolfsen bringing braid groups to life

Summer School in Logic (2 - 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 and National University of Singapore*
Yue Yang, *National University of Singapore*

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

A second summer of logic at IMS

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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*

The program brought together researchers from a wide spectrum of mathematical and computational biology. It discussed recent progress and facilitated 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 also promoted stronger communication and collaboration among mathematical, computational and biological scientists in examining essential and unsolved mathematical problems arising from structural and network biology. Structured around two workshops and two tutorials, 18 overseas and 10 local speakers were invited to give talks over the four week long program.

All set to unleash the power of computation on biology

Xiang Yang Liu: Understanding and design of bio-materials

Edison Liu: Protein-DNA interactions

Sticking to the theme – interacting and networking

Fruit of a program – a meeting of minds

Upcoming Activity

Workshop on Mathematical Models for the Study of Infection Dynamics of Emergent and Re-emergent Diseases in Humans (22 - 26 October 2007)

Website: http://www.ims.nus.edu.sg/Programs/infectious07/index.htm

Chair
Eduardo Massad, *University of São Paulo, Brazil*

Co-chairs
Stefan Ma, *Ministry of Health, Singapore*
Paul Anantharajah Tambyah, *National University of Singapore*

Members
Anthony Kuk, *National University of Singapore*
Kah Loon Ng, *National University of Singapore*

This program focuses on the mathematical models used in the study of several classes of infections. The program content is intended to (and likely to) attract international interest. There is much scope and urgent need for research in this area as classical epidemiological methods offer limited help towards the understanding of the transmission dynamics and, in particular, for the designing of control strategies.

The program will be structured around workshops designed to bring together researchers from a wide spectrum of mathematical and statistical epidemiology. The main themes to be covered include

i. Emerging and re-emerging vector-borne infections,

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ii. Emerging and re-emerging directly transmitted infections,
iii. Emerging and re-emerging sexually transmitted infections,
iv. Emerging and re-emerging antibiotic resistant strains.

The field of mathematical and statistical epidemiology is wide open for the development of new methods which carry enormous potential impact. This program will join researchers from around the world to integrate and synergize the strengths of mathematics, statistics and epidemiology to the understanding of disease dynamics and to propose control strategies.

Next Program

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. Tutorial 1: 5 - 10 November 2007
   Speakers: Dieter Jaksch, Oxford University
   Weizhu Bao, National University of Singapore
3. Workshop 1: 12 - 16 November 2007
   Title: Bose-Einstein condensation: modeling, analysis, computation and applications
4. Tutorial 2: 2 - 7 December 2007
   Speaker: Fabrice Bethuel, University of Paris VI
5. Workshop 2: 10 - 14 December 2007
   Title: Quantized vortices in superfluidity and superconductivity and kinetic theory
6. Public Lectures

Programs & Activities in the Pipeline

Data-driven and Physically-based Models for Characterization of Processes in Hydrology, Hydraulics, Oceanography and Climate Change (7 - 27 January 2008)

... Jointly organized with Pacific Institute for Mathematical Sciences, University of British Columbia

Website: http://www.ims.nus.edu.sg/Programs/ocean07/index.htm

Co-chairs
Sylvia Esterby, *University of British Columbia*
Hans-Rudolf Künsch, *ETH Zurich*
Shie-Yui Liong, *National University of Singapore*

Members
Vladan Babovic, *National University of Singapore*
Wolfgang Kinzelbach, *ETH Zurich*
Pavel Tkalich, *National University of Singapore*
Jim Zidek, *University of British Columbia*

The program will focus on improvements of description of physical, environmental and water quality processes through hydrodynamics, morphology, hydrology, water quality, ecology as well as numerical methods and techniques such as finite difference methods, finite element methods and boundary element methods, with applications to physically based modeling of lakes and reservoirs, prediction of runoff in poorly gauged catchments using physically based models, and flood modeling.

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In addition, it will also consider recent development in statistics relevant to the topical areas. Considerable efforts have been made to assess uncertainty by comparing and combining different physical models (especially in weather prediction and climate modeling) and on calibrating complex computer models with observations, taking non-identifiability and structural model deficits into account. It should be noted that these topics are currently the object of a program at SAMSI (Statistical and Applied Mathematical Sciences Institute).

The program will concentrate on bridging the gap and establishing bridges between two approaches (and two scientific communities) by addressing several specific topical areas: water resources management, down-scaling in climate change and non-linear wave and tsunami modeling.

The three main topics that will be covered are:

i. Development of a fully integrated data driven and physical-based models for water resources management,
ii. Dynamic and statistical downscaling on climate change study,
iii. Nonlinear wave dynamics and tsunami modeling.

Activities
The first week of the program will be dedicated totally to seminars/lectures on the three topics described above. Each of the following two weeks will start with two days of presentations, by a number of invited speakers, focusing on the topics described above. The remaining three days of each of these two weeks will be reserved for work in smaller multi-disciplinary groups. The groups will address a number of concrete challenges associated with the three topical areas. The general idea is to arrive at possible research collaboration in the immediate future; and to draft scientific publications by the end of the workshop.

Mathematical Imaging and Digital Media (5 May – 27 June 2008)

Website: http://www.ims.nus.edu.sg/Programs/imaging08/index.htm

Co-chairs
Tony Chan, *University of California, Los Angeles*
Zuowei Shen, *National University of Singapore*

Members
Say Song Goh, *National University of Singapore*
Hui Ji, *National University of Singapore*
Seng Luan Lee, *National University of Singapore*
Andy M. Yip, *National University of Singapore*

The purpose of this program is to conduct multidisciplinary studies involving mathematical perspectives and foundation of imaging science and digital media. In particular, the program emphasizes on the applications in imaging science and digital media of recent developments in the areas of approximation and wavelet theory, numerical analysis and scientific computing, and statistical and data analysis. The focus will be on the following themes.

i. Mathematical Imaging and Digital Media: Mathematical methods for computer graphics, computer vision, mesh generation, image restoration and reconstruction, image enhancement, image segmentation, object detection, image decomposition, image representation, image compression.
ii. Wavelet Theory and Applications: Sparse data representation and approximation by wavelets and redundant systems, noise removal, stochastic wavelet analysis, inverse problems via wavelet methods.

Activities
1. Workshop I: 9 – 13 June 2008
   Chinese-French-Singaporean Joint Workshop on Wavelet Theory and Applications
2. Workshop II: 16 – 20 June 2008
   Workshop on Mathematical Imaging and Digital Media
3. Summer School: 19 May – 6 June 2008

7th World Congress in Probability and Statistics (14 - 19 July 2008)

Jointly sponsored by the Bernoulli Society and the Institute of Mathematical Statistics
Jointly organized by the Department of Statistics and Applied Probability, Department of Mathematics and Institute for Mathematical Sciences of the National University of Singapore.

Website: http://www.ims.nus.edu.sg/Programs/wc2008/index.htm

Chair of Scientific Program Committee
Ruth Williams, *University of California, San Diego*

Chair of Local Organizing Committee
Louis Chen, *National University of Singapore*

This meeting is a major international event in probability and statistics held every four years. It features the latest scientific developments in the fields of probability and statistics and their applications. The program will cover a wide range of topics and will feature thirteen keynote lectures presented by leading specialists. In addition there will be invited paper sessions highlighting topics of current research interest as well as many contributed talks and posters. The venue for the meeting is the National University of Singapore. Singapore is a vibrant, multi-cultural, cosmopolitan city-state that expresses the essence of today's New Asia.

Mathematical Conversations

Sergio Verdú: Wireless Communications, at the Shannon Limit >>>

Sergio Verdú

Interview of Sergio Verdú by Y.K. Leong (matlyk@nus.edu.sg)

> *In the beginner's mind there are many possibilities, but in the expert's mind there are few.*
> – Shunryu Suzuki (1904 – 1971), Japanese Zen Master

Sergio Verdú is world-renown for pioneering the field of multiuser detection in wireless communications and for fundamental work on data transmission and compression in information theory.

His theoretical doctoral research has a tremendous impact on communications technology with numerous applications in mobile cellular systems, fixed wireless access, high-speed data transmission, satellite communication, digital television and multitrack magnetic recording. His book *Multiuser Detection* published in 1998 is now a modern classic. His research papers have received many awards from scientific and professional bodies. He has also received several awards for professional education and outstanding teaching. The prizes, awards and accolades bestowed on him are indeed too numerous to list; the latest in 2007: election to the US National Academy of Engineering and the Claude E. Shannon Award, the highest honor in information theory.

On the faculty of Princeton University since 1984, Verdú is Professor in the Department of Electrical Engineering since 1993. He is also a core faculty member of the Program in Applied and Computational Mathematics. He is known for his personal zeal in advisory and organizational work (not only in the United States, but from South America across Europe to Asia) in advancing and promoting science and technology. He has served as President of IEEE Information Society and he serves on the editorial boards of leading journals in his field, in particular, for IEEE. His scholarship and scientific charisma have led to many visiting appointments and invited lectures around the world.

Verdú was an invited speaker at the Institute's program *Random Matrix Theory and its Applications to Statistics and Wireless Communications* (26 February – 31 March 2006). He was interviewed on 26 February 2006 by Y.K. Leong on behalf of *Imprints*. The following is an edited and enhanced version of the interview in which he traced his scientific path from humble beginnings in Barcelona, Spain to prominence in the world's leading centers of communications research in the United States. It resembles a classic Spanish narrative that spans a wide spectrum from human passion to intellectual vision set on a scientific stage for exploring the physical possibilities of communications at the edges of the theoretical limits of information theory.

Imprints: In your undergraduate training in Spain, you had already specialized in telecommunication engineering. Why did you choose this particular branch of engineering?

Sergio Verdú: I decided to become a telecommunications engineer when I was 7 years old. My father gave me a toy – a kit with which you could build radios and all sorts of electrical devices – and I was hooked. My father was very good with electrical gadgets. As a child I was always immersed in electronics. At that point I decided to be an electronics engineer and I never wavered from that.

I: Was your father an engineer?

V: No, my father had very little formal education. His childhood was spent during the Spanish Civil War. He suffered a lot and went through tragic circumstances. He was a self-made man, a very fine man. He really had a lot of influence on me even though he died in an automobile accident when I was 11 years old.

I: What attracted you to go to the United States (in particular, the University of Illinois at Urbana-Champaign) for your graduate studies?

V: Going back to my early youth, I guess that's where you would find the traces for all these decisions. My parents decided that I would get an English tutor when I was 6 or 7 years old. From then on until when I was in high school, I had an English tutor: a Spaniard, she was not a native English speaker and I guess that accounts for my less than perfect accent. It gave me an edge over everybody else who was just learning English in school. I was fascinated with things American and in particular with the space program. When

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I was 14, I jumped at the opportunity to spend a summer in Rockville Center, Long Island in 1973. It was an excellent opportunity to see a world completely different from the backward country where I had grown up: Spain was under a fascist dictatorship from 1939 to 1975. At that time things like color television were completely new to me. I remember being fascinated by the Watergate affair that was going on at that time. The fact that a country could be so open politically while undergoing a painful episode and still able to do it with a sense of humor was a revelation. Although I tried, practically the only American activity that to this day I never could really get interested in was baseball. At that time it was certainly not very common for Spanish students to do their graduate studies in USA. In fact, I didn't know anybody who had done that. I ended up at the University of Illinois at Urbana-Champaign partly because as an undergraduate I had worked a lot on computer-aided design. I was very much into the design and analysis of electrical circuits using the computer. A lot of prominent people in that field have been at Urbana-Champaign. I was also admitted at Stanford and, of course, I knew some of the professors there but perhaps not as much as the ones at Urbana. But at that time, I already knew that I had done enough programming and hacking in my life. I really wanted to do theoretical work and I really wanted to do communications theory and information theory. Of course, as an undergraduate I had already heard of Shannon, and one day when I was discussing my options to go to graduate school in the US, one of my professors said, "Well, you know, Claude Shannon was at the University of Illinois." I said, "Oh, okay." That clinched the decision for me.

I arrived in the United States in1980 during the Presidential campaign between Ronald Reagan and Jimmy Carter, and Urbana-Champaign was pretty shocking to me. It was so unlike the atmosphere in the New York area that I had seen in `73 and, needless to say, very different from the big European city life that I had been exposed to. In addition to the geographical isolation, the religious atmosphere of the place was really striking. When I got there, I asked, "So when did Claude Shannon teach here?" and nobody knew about Claude Shannon having been there. One day, browsing in the university bookstore, I picked up a copy of Shannon's *The Mathematical Theory of Communication*. It had been reprinted by the University of Illinois Press.

I: Then you went to Princeton immediately after Illinois?

V: Yes, the day after I defended my PhD thesis. My wife Mercedes and I drove our Chevy to New Jersey.

I: You didn't go back to Spain?

V: No. I always wanted to remain in the United States. I had a Fulbright Fellowship. That gave me a lot of trouble because Spain, concerned about the brain drain, refused to give me permission to stay in the United States. But, after a long, complicated process through the State Department and the Department of Justice involving senators and so on, I was granted a waiver of the requirement to return.

I: Your doctoral research pioneered the field of multiuser detection. Could you tell us something about it? Were you excited and surprised by your work at that time?

V: Yes. At that time (in the early 80s), I had worked for my masters' thesis in minimax robustness. This was a field that originally started in statistics with the work by Huber in the 70s. Then there was a lot of work in engineering (particularly by my advisor Vincent Poor) applying Huber's theory to robust estimation, robust detection and so on. Vincent Poor mentioned that in spread-spectrum communications, they were modeling the multiaccess interference as white Gaussian noise, and although this seemed to be a pretty good modeling assumption, perhaps there was some room to apply robust statistical methods to account for the deviation from the central limit theorem. I started to look at it from that angle, but then I quickly realized that that was not the right approach and that a completely new approach had to be taken. Then I obtained the optimum multiuser detector, and that became the beginning of my PhD thesis. The interesting thing was not only the structure of the receiver but the fact that in many cases you could achieve single-user performance. The gain was remarkable and much more than what we expected. That was the beginning of multiuser detection. At that time, nobody was paying any attention to it. Spread-spectrum research was pretty much dominated by military funding and did not have the vibrancy it acquired later on, thanks to the ascent of wireless telecommunications and CDMA wireless commercialized by Qualcomm.

I: Your doctoral work was not classified?

V: The university would not allow any classified research. It was actually good for me that at the beginning it did not attract any interest. It was only years later that multiuser detection became a very vibrant research field with a lot of research citations to the early work I had done in the early 80s. In 1998, I published a book, essentially a compilation of my work and my teaching of the subject. But sometime in the late 1980s it ceased to be my primary research focus. Perhaps if the success had been immediate, then I would have devoted a lot of my efforts into that and less into information theory, which eventually became my primary field of interest. I think it was propitious, and interestingly, the time constant from inception of ideas to implementation of these ideas in that particular field was very, very long. It's only recently that there has been motivation and success

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in industry implementing multiuser detection. One of the drivers has been multi-antenna systems where there is interference between signals transmitted by different antennas. Qualcomm, the proponent of CDMA cellular wireless, came up with a second-generation cellular wireless with rather old signal processing algorithms. It didn't use any multiuser detection, but they have announced recently that they are using these methods in their third-generation chips. These are channels where bandwidth and power are resources to be conserved. And one of the lessons that Shannon taught us is that you have to exploit the fine details in your model (in this case multiuser interference) to squeeze the most out of the channel resources.

I: It is now commonplace?

V: It depends on which area. Although the systems were not designed with the idea that you would have sophisticated receivers taking into account multiuser interference, both in third generation CDMA and in Digital Subscriber Loops (high speed data through telephone copper wires), they are starting to implement it. Multiuser detection is commonplace in the multi-antenna receivers where you can get substantial gains in capacity taking into account interference proceeding from different antennas. There are also chips that take into account intertrack interference in magnetic recording. It is always just a matter of time until the maturity of technology puts a stop to the waste of bandwidth/power.

I: Have you ever considered working in industry?

V: No, I was always very much an academic type. I like the freedom to pursue my own ideas and my own work. I also like to interact with young people. Not having a boss is nice too.

I: You seem to be equally comfortable with mathematics and engineering. How do you manage to reconcile their two different approaches to problem-solving – approaches that are apparently poles apart?

V: Strangely enough, they are not very different because the way you approach problems is essentially the same in both fields: going back to the basics. As much as I can, I always try to avoid carrying a bag of tricks that I can apply from one problem to another. I have actually moved quite a bit from problem to problem, and there's a lot of pleasure starting on a problem from scratch that I really didn't know anything about. Like the Zen philosophy says, in the mind of the beginner the possibilities are endless. A lot of important contributions are made by people who have just entered the field. Learning new mathematics is a delightful reward. Technology points out what next to learn; for example, my work on random matrices – which is why I am here now – was motivated by wireless communication systems. The type of research that excites me is mathematically challenging and relevant to the real world. Claude Shannon was the archetypical seamless combination of mathematician and engineer.

I: Do you think that, in general, engineers have as much mathematical training as they should have?

V: Mathematical training is like wealth, nobody has enough of it. The thing about this discipline that we call electrical engineering is that its unifying theme (electricity) goes back to the 19th century and is now completely obsolete. But our engineering training gives you a lot of versatility to deal with very different problems. To give you an example, two of my graduate students are finishing their PhDs in information theory this summer and are joining Goldman Sachs and Credit Suisse. Electrical engineering undergraduates may not get as much mathematical training as they would need to be professors doing research on say telecommunications. That mathematical training they will have to get later on in graduate courses and on their own. But electrical engineering undergraduates do get very strong training in problem-solving, and this gives them a lot of options.

I: It seems that one perception about the mathematical training for engineers is that they are more interested in sort of recipes or a bag of tricks for solving problems.

V: The training in engineering is very different around the world. Some of the European systems tend to have a kind of dichotomy. In the first two years of engineering, they are very mathematically oriented, and then later the subjects become very practically oriented. For example, I had to take two semesters of television – something that would be completely unheard of in the US. I think that in the US, perhaps because the professors are much more research oriented than in other places, we tend to be more mathematically oriented, at least those of us on the applied mathematics side of electrical engineering, like communications, control and signal processing.

I: Do you work directly with hardware engineers to create the technology?

V: No, not really. By the way, the dichotomy between hardware and software is fading. It is always important to be aware at any given time what the technology can deliver so that you know whether the solutions you are coming up with are solutions that can be implemented now or in 20 years' time or perhaps the technology in a certain field has progressed so much that you can implement things that are much more sophisticated than what people are

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implementing right now. So it's very important to have a sense of what technology can deliver even if normally we don't collaborate in research with people working in hardware.

I: What is the "biggest" unsolved theoretical problem in communications technology?

V: The biggest success story of Shannon's theory has been in point-to-point communications. Shannon's theory has been instrumental in anything that has to do with modems, wireless communications, multi-antenna and so on. But network information theory has proved to be a particularly tough challenge. Shannon was the first to formulate the problem, or at least the building blocks, in 1961. Instead of having one transmitter and one receiver, you have a bunch of transmitters and a bunch of receivers, and you may also have some nodes in between that act as relays, and some of those nodes may also be sources or sinks of information. You could think of a very general topology and you would like to know what are the best rates of information, what are the distinguishable signals that you can send. This is something that we still don't know.

Another important technological challenge is data compression of audio and video signals, which in my view is still in its prehistory. Even though Shannon also gave the fundamental principles of this discipline, information theory has not had nearly as much impact as it has had in channel transmission or in text/data compression. I think the reason is that we do not yet have a good understanding of human vision and hearing, and even the little we know is hard to marry with the available theory.

I: The point-to-point problem is solved?

V: We understand it a lot better. Shannon gave us the point-to-point framework, but he didn't give us all the solutions. Finding the capacity of a particular point-to-point communications channel may be extremely challenging and, in fact, the capacity of some very simple channels is still unknown.

I: Do you agree that engineers are very focused in their research in the sense that they try to solve only problems that are of immediate practical concern in contrast to physicists who try to answer fundamental questions that are not immediately applicable?

V: No, I don't agree. Shannon was the primal example of an engineer who would explode this myth. Many of us who are working in theory are accused, more often than not, of doing exactly the opposite: of solving problems that are of no immediate practical concern and that may become relevant only in the distant future or never. Those of us who have followed in Shannon's footsteps have an appreciation for beauty and elegance and for the fact that beautiful and elegant results sooner or later become practical. So you need to have some faith even though what you are working on now is not of immediate practical concern. You may be interested in it not because of some technology out there clamoring for solution, but because of its beauty.

I: It appears that you are a mathematician first and then an engineer.

V: I would say first an engineer, then a mathematician and then an engineer. I have come full circle. My doctoral thesis had an important component in developing algorithms, and also a lot of analysis but I had this nagging feeling that it was not mathematical enough for my taste. When I got into information theory I became quite theorem-proving minded. But, the thrill of coming up with new algorithms is something I have come to appreciate later, more so in recent years. When I was younger, I had the idea that if I cannot prove a theorem about something, then I don't want to do research on it. Now my outlook has evolved. Of course, I still like to prove theorems, but I have also done some recent work that is algorithmic and I enjoyed it very much.

I: Can you tell us something about your present research interests and the problems you are working on?

V: People say that the interesting problems are at the boundary between disciplines. This is actually true sometimes. One of my current interests is the boundary between information theory and estimation theory. A couple of years ago, we found a very basic formula that connects some basic quantities from information theory and estimation theory. Capitalizing on this formula, we gave some simple proofs of a probability theory result on the monotonicity of the non-Gaussianness of the sum of independent random variables as well as a famous result from Shannon's 1948 paper, called the entropy-power inequality. We also came up with a new universal formula in continuous-time nonlinear filtering, as well as an algorithm to minimize transmitted power. All those come from this innocent-looking formula. As usual, there is nothing more insightful and practical than a pretty formula.

Random matrix theory has been very rewarding. I got into random matrix theory around 1997. When I was finishing my book, I was fortunate to become acquainted with Marchenko-Pastur's theorem and I included it in Chapter 2. Since then there has been an enormous interest and excitement. It is challenging to get into this theory. Even though its early history developed in the 50s and 60s, the core results are quite recent. It's only in the last 10 years or so

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that there has been a lot of interest in it from contemporary mathematicians.

I: Is random matrix theory applicable in engineering?

V: Very much so. It's applicable and fundamental in wireless communications. The first application was in the capacity of multiple antenna systems. In Bell Labs, Foschini and Telatar realized that in the presence of electromagnetic scattering when you have multiple antennas the channel capacity can be much larger than if you have single-antenna transmitter and receiver. Random matrix theory is fundamental in this realization, and also in the analysis of the fundamental limits of spread-spectrum in wireless communications.

I: Have there been any breakthroughs in random matrix theory?

V: The great breakthrough, at least for applications in wireless communications, was in 1967 in the work of Marchenko and Pastur in the Soviet Union. That was an amazing piece of work. It was completely unknown for many years. In 1986, I looked at a random matrix problem that I wanted to solve. I looked in the literature (of course, that was before Google) and I could find nothing. The work on random matrices that I could find was completely orthogonal to what we needed. Physicists and mathematicians were rediscovering the Marchenko-Pastur result in the late 80s and 90s. Lately there has been a lot of excitement in a new mathematical field called "free probability" and one of its main applications is in random matrix theory. Wireless communications and information theory have been one of the main propellers of work in this theory. We are not just consumers of this kind of result; we have also been able to pose new questions and solve some of these problems.

I: So, in a sense, wireless communications has affected the development of random matrix theory.

V: Oh, yes, for sure. You see this pendulum of interaction in other fields. Information theory was very much influenced by ergodic theory, and also the other way around. Kolmogorov made a fundamental discovery in ergodic theory thanks to information theory.

I: Do you do much consultation work for industry?

V: I occasionally have done work with people in research labs such as Bell Labs, Hewlett Packard, and Flarion, which was recently acquired by Qualcomm. When I was doing work in Hewlett Packard, the group there was very theoretically inclined. I was actually kind of like the guy who was pushing for us to do more algorithmic work rather than theorem-proving. It was particularly rewarding to be associated with Flarion because you see the thrill of seeing brilliant ideas being implemented in a very short period of time.

I: Do you have any patents?

V: Traditionally, being more academically oriented towards peer publication, I have not pursued patents at Princeton. But yes, I do have quite a few patents granted or pending both through Bell Labs and Hewlett Packard.

I: Do you think that technology will be able to catch up with the theoretical advances in science and technology or even mathematics?

V: Well, Shannon's theory is a good example of a theory that at the beginning created a lot of enthusiasm. Shannon became an instant celebrity. Then, for a few years, people were asking the question, "If this is so good, how come it hasn't seen the light?" Of course, what happens is that it came well before its time, well before technology was ripe to be implemented. It took a long, long time for implementable codes to achieve Shannon's limits. In data compression they did not appear till the 70s and in data transmission until the 90s.

I: When was Shannon's theory put forward?

V: 1948, so it took a long time. That's a powerful lesson because everybody knew that these were very powerful ideas. For decades, there was a lot of unsuccessful work in trying to design codes that would approach Shannon's limits. When there are theoretical breakthroughs and when we are able to solve problems of a fundamental nature, then just because technology doesn't seem to be on the near horizon to be able to implement those ideas or formulas, it doesn't mean we should give up and say, "Okay, this is a dead field because we have given it long enough time and technology has not implemented it, and therefore it is hopeless." I think information theory is a great lesson in that respect. In communications, we have a limited piece of spectrum that we can only use with given resources, and there is an enormous economic incentive to use that spectrum as efficiently as possible. So when you have a theory like information theory that sets fundamental limits, there is an enormous incentive to get as close as you can to those limits.

I: How would one make the theoretical work drive the technology faster? Is that possible?

V: Well, it is possible. In certain areas, it has been enormously successful, for example, in modems, in work that was published in the *Information Theory Transactions*. Four

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years later, you could buy modems that were implementing those ideas for a hundred dollars. That is a field where the time constant is much faster. In fields like cellular wireless, the technology transfer has been a lot slower. Developed in the late 80s, second-generation wireless systems were predicated on technology that was really old (a lot of it 50s, 60s). A revolution happened in the 1990s with the advent of a class of channel codes called the turbo codes. In the beginning, they were not very appealing to the theoreticians because these codes came very close to the Shannon limit, but nobody could explain why. We couldn't come up with theorems that would say. "Hey, of course, this is why they do work." Now we understand them better. Actually, they vindicate Shannon because he came up with a theory for what the best code could do without the benefit of knowing a single code except possibly for the simple Hamming code that was developed at the same time. He said, "Well, I don't know how to construct the optimum code but I can show that a construction where the codes are chosen blindly at random, performs close to optimum on the average." The problem is that if you choose a code at random, it cannot be implemented because it doesn't have structure. So these new codes that go back to the 1990s turn out to have enough structure that you can implement and encode them in linear time and at the same time they have enough randomness like Shannon originally said in 1948 to be close to the best.

I: So without those codes the cellular revolution would have been impossible?

V: The first digital systems used codes that were really far from capacity. Early in the game in the design of codes, people took a turn away from Shannon's random codes. Coding theory became a geometric discipline, very combinatorial, not so probabilistic. Now we are going back to the roots. Those geometric constructions that emphasize minimum distance properties of codes are not the ones that achieve capacity. They are very interesting mathematically but are not the ones that turn out to come closest to Shannon's fundamental limits. With the new codes you can increase the efficiency quite a bit. What is surprising is that there was nothing inherently there to prevent people in the 1960s to come up with these codes. Actually, Gallager at MIT had come up with random-like codes in the early 60s but he abandoned them because they thought they could never be implemented and that they were too complicated. They are actually not too difficult to implement. The key is not to attempt optimum decoding because that is too expensive. With a judicious choice of code, the life of the decoder is a lot easier, and near optimal decoding is feasible. The bottom line is that in linear time you can come very close to the Shannon limit. People are now using cellular phones that incorporate these codes.

I: Is the Shannon limit a real physical Heisenberg-type limit or is it a Gödel-type logical limit?

V: The short answer is: information theory is a chapter of probability theory, which in turn is a chapter in mathematics. The starting point is a stochastic model for the information source and a stochastic model for the channel. Are those models relevant to the real world? If they weren't, your cellphone would not work. Having said that, since 1948 there have been enormous strides in information theory dealing with uncertainty in nonprobabilistic ways. An example is the theory of algorithmic complexity which is devoid of any probability, and was put forward by Kolmogorov, the father of modern probability theory.

I: Do you think that a revolution in wireless communications would follow in the wake of breakthroughs in nanotechnology?

V: The radiofrequency spectrum usable in wireless communications is rather limited. Information theory tells us the fundamental capacity of the medium. We cannot go beyond it no matter how fast the computing technology. But let me address the question from a broader perspective: why can a DVD contain a lot more music than a CD? The compression technology of the CD dates back to the 1930s. By the time the DVD was developed 15 years after the CD, lossy compression was much better understood, and the optical recording devices were also quite a bit more advanced. So the engineer reaps benefits from both applied physics and applied mathematics. For the information theorist, new physical devices mean new communication channel models, with a capacity to be discovered. So I think information theorists are going to be around for a long time.

Béla Bollobás: Graphs Extremal and Random >>>

Béla Bollobás

Interview of Béla Bollobás by Y.K. Leong (matlyk@nus.edu.sg)

> *As long as a branch of science offers an abundance of problems, so long is it alive; a lack of problems foreshadows extinction or the cessation of independent development.*
> - David Hilbert (1862 – 1943)
> International Congress of Mathematicians, Paris, 1900

Béla Bollobás is well-known for a wide range of significant contributions to graph theory, combinatorics and functional analysis. His recent work on applications of random graph techniques to percolation theory is a ground-breaking contribution to the theoretical basis of a newly emerging field motivated by physical phenomena and first explored by computer simulation.

He may be regarded as a leading exponent of the Hungarian school of graph theory, having paved the way for the current widespread applications of random graphs in numerous areas in applied mathematics, physics and engineering. In addition to more than 350 research papers, he has written 10 books and edited 9 volumes. He is also well known for his mathematical exposition and for championing the cause of the combinatorial approach in mathematics. His two books *Extremal Graph Theory* and *Random Graphs*, published in 1978 and 1985 respectively, were the first books to systematically present coherent theories of early results in those areas. His latest book *Percolation* is written jointly with Oliver Riordan. Bollobás's personal and mathematical connections with his mentor, the prolific and consummate problem-solver Paul Erdős (1913 – 1996) and with his intellectual mainspring Trinity College in Cambridge are the stuff of legends of contemporary mathematics.

A Fellow of Trinity College since 1970, Bollobás has a long and distinguished career at the Department of Pure Mathematics and Statistics in Cambridge University from 1971 to 1996; from 1982 to 1994 he paid long visits to Louisiana State University at Baton Rouge. In 1996, he accepted the Jabie Hardin Chair of Excellence in Combinatorics at the Department of Mathematics of the University of Memphis, Tennessee, while keeping his Fellowship at Trinity College. Since 2005, he has been a Senior Research Fellow of Trinity College. He is also a foreign member of the Hungarian Academy of Science. He has held visiting appointments in various countries throughout the world and has been invited to give lectures at major conferences and scientific meetings. He has supervised over forty PhD students, some of whom have gone on to distinguished careers, notably Tim Gowers, 1998 Fields Medalist and Rouse Ball Professor of Mathematics at Cambridge University. Bollobás excelled not only in mathematics but also in sports: he represented Oxford University in the pentathlon, and Cambridge University in fencing.

Bollobás's connections with NUS date back to 1994 when he was visiting professor from June to August. During his second visit from May-June 2006 for the Institute's program *Random Graphs and Large-scale Real-world Networks*, of which he is chair, Y.K. Leong interviewed him on behalf of *Imprints* on 17 May 2006. The following is an edited and enhanced version of the transcript of the interview, in which he traces his mathematical journey from a closed Hungarian communist system to an eclectic academic environment in Cambridge and speaks passionately about his personal mission in spreading the philosophy of combinatorics within mathematics, his reminiscences giving us glimpses of the richness of modern mathematical traditions.

Imprints: You did your first doctorate in Hungary. Who was your supervisor then?

Béla Bollobás: I should be able to answer this question very easily, but I cannot, since in the Hungary of the 1960s we didn't have well-defined supervisors: we would join a group of mathematicians, attend the right seminars, talk to the right people, and work on our dissertations on problems we picked up. The group I joined was that of László Fejes Tóth, who worked on discrete geometry and had written the famous book on the subject, so I wrote my dissertation on packings, coverings, and tilings. However, my real supervisor was Erdős. I had got to know him when I was 14 or so, and from then on he gave me lots of mathematical problems; over the years he kindly stayed in touch with me and inspired me. Of course, he was not in Hungary all that much, but even when he wasn't there, he wrote letters

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with problems and so he was my real supervisor from the very beginning.

I: Was your education a typically traditional Hungarian one?

B: Yes. I always went to school, didn't stay at home and was not home-schooled like Erdős, for example. But I did have lots and lots of private tutors, not for school work, but for extra-curricular activities. I grew up 5 – 10 years after the war, and in the communist Hungary of the day many people who had played prominent roles before the war lost not only their livelihoods but even their homes: they were sent into "exile". So I was taught at home by a former general, a count, a baroness and a former judge. They were excellent people, but in those days, they were deemed to be nobodies. So with my education I was exceptionally lucky: I couldn't have had better people to tutor me. The judge was not allowed to remain in the judiciary, so he took up teaching. The general was pretty famous – he was the head of Hungarian fencing. Fencing was actually popular in Hungary for many years. This great-uncle of mine had the wonderful idea of setting up a Fencing Academy in the army, so that able recruits had a chance of being trained to be coaches, rather than go on mindless drills. Within a few years Hungary produced more coaches than the rest of the world put together. Before the war in Central Europe fencing was much more important than it is now: for doctors, judges, lawyers and civil servants fencing played a role somewhat similar to that of golf today. The three countries that were great in fencing were France, Italy and Hungary.

I: Were you spotted by Erdős?

B: In some sense, yes. In Hungary there are many competitions; in fact, the idea of having mathematical competitions at all was born in Hungary. When I was 14, I won the national competition, and, as luck would have it, Erdős just returned to Hungary for a week or so: he sent word to me that I should go and meet him. I met him in a fancy hotel in Budapest, on a hill-top. We had lunch and it was amazing that he was willing to talk to a 14-year-old boy. He was 45 but to me he looked ancient. Throughout his life he was extremely good to youngsters. His favorite was Louis Pósa, whom he got to know when Pósa was 10 or 11. Erdős was very disappointed when, after a good start to his career, Pósa didn't continue in mathematical research but chose to nurture very talented teenagers.

I: It seems that Hungary has produced a disproportionately large number of mathematicians.

B: That is certainly true. I'm pretty sure it's due to two things. Firstly, in Hungary we had a journal for secondary school pupils. It's a monthly based on attractive and challenging problems. Readers are invited to send in their solutions which are then checked, marked, and the best of which get published. That made a huge difference. The other reason is that there are annual mathematical competitions: three-hour long exams testing your ingenuity on a handful of problems. I believe that the existence of the journal was even more important than the annual competitions, since the competitions in the journal went on throughout the year. All the time, you have problems that you wanted to solve – elegantly. The judges gave you bonus marks if you gave several solutions or you generalized a problem or you sharpened the bounds, which generated much research. Practically everybody I can think of went through this system – Marcel Riesz, Alfréd Haar, Eugene Wigner, von Neumann, Pólya, Szegő, von Kármán. But Erdős was never good at those competitions; von Neumann and von Kármán were very good at them. Wigner and von Neumann were in the same school, and Wigner considered von Neumann to be the only genius he had ever met, although he had known Einstein as well.

I: What made you go to Cambridge to do a second doctorate after your fist one in Hungary?

B: Hungary was a very closed-in country. You were not allowed to travel outside, and going abroad was always a tremendous feat. From an early age, I felt claustrophobic. At the beginning of my university studies, I asked Erdős whether I could go and study abroad. I knew that he was allowed to live abroad and came back to Hungary for only short periods. He spent a lot of time in Israel and even had a job there. I asked him whether I could go to Israel for a semester or even a year to study mathematics. Then he said, "Why Israel? You are not even Jewish. Why not Cambridge? I have a very good friend who had just gone there to work with Davenport and maybe he can help you." Of course, going to Cambridge was beyond my wildest dreams. So Erdős wrote a good recommendation to Harold Davenport to try to get me into Cambridge. By then I had a joint paper with Erdős which I wrote when I was still at high school. But we needed permission from the communist authorities. That took ages and ages, and was very humiliating, but eventually I did get the permission and was allowed to go to Cambridge for a year. That was in the middle of my undergraduate studies. After a year in Cambridge I returned to Hungary but very soon I had a scholarship to go Cambridge to do a PhD. I applied to the authorities for permission to go there, but I was refused. Next, I had a scholarship to Paris but was again refused permission to leave the country.

I: You went to Moscow?

B: Yes. After I had got my degree, I spent a year in Moscow

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to work with Israel Moiseievich Gelfand. My year there was a wonderful mathematical experience. After Moscow, but not quite immediately, the communist authorities allowed me to go to Oxford on a scholarship (from Oxford, of course, not Hungary). By then, I said to myself, "If I ever manage to leave Hungary, I won't return." So when I arrived in Oxford, I decided to take up my old scholarship to Cambridge rather than return to Hungary. That way I didn't have to apply for anything because it had been sitting there for years. But then within a year, I got a fellowship from Trinity College, which was better than getting a PhD. There was no pressure on me whatsoever to submit for another PhD. But I thought that as the College had given me a scholarship to do a PhD, it was my duty to get one.

I: I notice that your PhD in Cambridge was done with Adams, who was a topologist.

B: Yes, Adams was my official supervisor but in reality I worked by myself, getting my problems from the Functional Analysis Seminar. When I was in Moscow, Gelfand said that it would be very good to work with Michael Atiyah or Frank Adams, the great topologists. However, when I was in Oxford, Atiyah was on his way to the Institute for Advanced Study, and when I arrived in Cambridge, Adams was still in Manchester although on his way back to Cambridge. By the time Adams arrived a year later, I already had a fellowship at Trinity College. Nevertheless, Adams remained my official supervisor; in fact, I learned a fair amount of algebraic topology from him and I did work on some of his questions. During my first year in Cambridge I joined the functional analysis seminar, where I found several beautiful problems, some of which I solved, so my Cambridge PhD thesis was on Banach algebras.

I: Was your interest in graph theory shaped by your early years in Hungary?

B: It was certainly due to Erdős. If he hadn't been there to give me lots of attractive problems, I'm sure I would have ended up doing either number theory with Turán or probability theory with Rényi.

I: You have published several books on graph theory, including *Extremal Graph Theory* in 1978 and *Random Graphs* in 1985. What made you write these books?

B: It really goes back to the picture I had of graph theory, not only picture but reality. For some peculiar reason, in the early 1970s or later, graph theory came in two flavors; one was done in Western Europe and America, and the other in the East, mostly Hungary by Paul Erdős, Tibor Gallai, Gabriel Dirac and others. In the west, they didn't do any extremal graph theory. On the other hand, in Hungary, graph theory was almost exclusively extremal graph theory. I very much wanted to show that extremal graph theory was a pretty serious subject and not only a collection of random problems that Erdős thought up and popularized. The usual charge against graph theory is, "Ah, it is made up of ad hoc problems that have nothing to do with each other. What's the point?" To some extent, at the very beginning, this is true, but slowly, slowly all these results do gel into a single theory, so my aim was to show that there is such a theory – extremal graph theory. I started to write this book very soon after I arrived in Cambridge but it took me ages to finish. I had to take a sabbatical to find enough time to finish it.

The theory of random graphs was founded by Erdős and his good friend Alfréd Rényi in the late 1950s and early 1960s. At the beginning, they wrote several joint papers but the whole theory didn't take off. People didn't jump on it and said "How exciting! Let's try to continue it." The climate started to change in the 1970s. In particular, Erdős came to visit me in Cambridge for a term as a Visiting Fellow Commoner in Trinity College – perhaps the longest period he spent in one place for many decades, since he never stayed anywhere for more than a week or so. He suggested that we work on random graph problems. I got interested and from then on, I was doing random graphs. I had this urge to showcase the classical theory together with lots of new developments and show that it is not only a beautiful subject but also very important. Really, random graphs became more and more active in those days. Once you write a book, parts of it became outdated almost immediately. It was the first serious book on random graphs just as the book on extremal graph theory was the first book on the subject. They are on different aspects of graph theory but they are closely connected.

I: How do you see the future of combinatorics, especially random graphs?

B: Hilbert, I think, said that a subject is alive only if it has an abundance of problems. It is exactly this that makes combinatorics very much alive. I have no doubt that combinatorics will be around in a hundred years from now. It will be a completely different subject but it will still flourish simply because it still has many, many problems. The same applies to random graphs. In fact, the field of random graphs has connections with statistical physics, percolation theory and even computer science. It's very strange that just at about the same time that random graphs were founded, Broadbent and Hammersley founded percolation theory. These two subjects are all about random subgraphs of certain graphs. They should be about the same – okay, one is finite and the other is mostly infinite and lattice-like, but still, they have about the same questions. For many, many years there were no interactions between the two subjects, none

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whatsoever. Now this is changing quite a bit. Quite a few combinatorialists are doing percolation-type problems.

I: Are random graphs applied to biology?

B: Yes. In the last 10 years or so many new spaces of random graphs have been defined in the hope of modeling phenomena in various areas, including biology. People have realized that large-scale real-world networks resemble random graphs. You can't really say that they have this structure or that structure. But this random graph is very different from the classical Erdős-Rényi model of a random graph. It has different characteristics: for example, the degree distribution may follow a power law, unlike in the classical case. One of the main advocates of using new models of random graphs is László Barabási, who also proposed several interesting models.

I: Was the power law discovered empirically?

B: Yes, it was observed that several graphs seem to obey a power law, but there were no proofs that they really do. Physicists and experimentalists have a very different attitude from that of mathematicians: much of the time they are not very interested in rigorous proofs. For a mathematician it is rather annoying that proving even the basic results about these new models can be pretty tough. Oliver Riordan and I have done a fair amount of rigorous work on properties of power law graphs.

I: You mentioned that there is an abundance of problems in combinatorics. It seems that combinatorial problems are very easy to formulate but very hard to solve.

B: For me, the difference between combinatorics and the rest of mathematics is that in combinatorics we are terribly keen to solve one particular problem by whatever means we can find. So if you can point us in the direction of a tool that may be used to attack a problem, we shall be delighted and grateful, and we'll try to use your tool. However, if there are no tools in sight then we don't give up but we'll try to use whatever we have access to: bare hands, ingenuity, and even the kitchen sink. Nevertheless, it is a big mistake to believe that in combinatorics we are against using tools – not at all. We much prefer to get help from "mainstream" mathematics rather than use "combinatorial" methods only, but this help is rarely forthcoming. However, I am happy to say that the landscape is changing.

When Erdős and Rényi started the theory of random graphs, they had to make do with basic probabilistic results concerning sieves and moments, but combinatorics changed the landscape of probability theory considerably. In order to answer questions in probabilistic combinatorics, results of a different flavor had to be proved in probability theory: results concerning sharp thresholds, isoperimetric inequalities, rapidly mixing random walks, and so on.

There are many other tools as well: algebraic, analytical, and even topological. For example, Borsuk's theorem has been used to prove several beautiful results in combinatorics. The achievement is not in applying such a theorem, after all, every schoolboy knows the theorem, but in discovering that it can be applied, and how it can be applied.

A totally ignorant and unfair way of judging a result in combinatorics is to ask the author: "What have you used to prove your theorem?" Then, upon being told that such and such a theorem was used, comes the retort: "Oh, that's very easy. I could have done it". What nonsense. Yes, of course it's easy once you are told what to do. The achievement is in finding the tool that can crack the problem after a series of clever manipulations that make the problem amenable to the application of the tool.

I: Could the difficulty of combinatorial problems be due to the discreteness of the objects?

B: Not really. Frequently, it is fairly easy to change a discrete problem into a continuous one but more often than not this change does not bring us any closer to a solution. The trouble with the combinatorial problems is that they do not fit into the existing mathematical theories. They are not about functions, topological spaces, groups or operators. More often than not, we simply do not have the machinery to attack our problems. This is certainly not the situation in other branches of mathematics. In fact, it may happen that first a wonderful machine is built and then the search starts for a worth-while problem that this machine can be applied to. This attitude is totally foreign to combinatorics. In combinatorics we have our problem which at the beginning looks like a Chinese box: there seems to be no way in, there is no indication as to how to start it. Here's the problem: we want to solve it and we don't care in what way we solve it.

I: So you are almost starting from nothing or from the bare minimum . . .

B: To some extent, yes, but of course, these problems are also built on top of each other. Once a problem gets solved, another one arises, and the theory does build upwards as well, not only sideways. A problem I certainly love and I'm sure is very deep is the problem of conformal invariance in percolation theory. I also love the related problems about the existence of various critical exponents. I have no doubt that these beautiful problems are so hard that they'll be around for many, many years. The original problems are

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combinatorial although they can also be considered to be problems in analysis or probability theory. I'd be surprised if we didn't need totally novel ideas to solve them.

I: Going the other way, are there any problems in more traditional areas of mathematics that can be solved by combinatorial methods?

B: Oh yes. It is frequently the case that once you have applied all the tools at your disposal, at the end you have to solve an essentially combinatorial problem in the traditional sense: you have to argue from the bits of information you have better than anybody else.

I: I think that the perception of combinatorics has changed considerably.

B: I hope that it is changing, for it should certainly change. Combinatorics is becoming a more "serious" subject, closer to the traditional branches of mathematics – there's no doubt about this. Combinatorics has many really hard questions, like number theory, algebraic topology and algebraic geometry.

I: Is there a single result or discovery of yours that has given you the greatest satisfaction?

B: I wonder how many people can say "Yes" to such a question. There are quite a few results that made me very happy at the time, but not one that I would trade for the rest. Let me tell you about some of my favorite results. Not surprisingly, people often like results they proved when they were young. Thus, I rather like a certain lemma of mine that I proved when I was an undergraduate. It is still one of the very few proper exact extremal results about hypergraphs. (Hypergraphs tend to be nastier than graphs, so this may not be so surprising.) Also, it can be applied in lots and lots of ways. It can be proved very easily: some years after I discovered it, Gyula Katona gave a ridiculously easy and very beautiful proof. But still, I am happy that I found it when I was an undergraduate.

Also, in the early 70s, I wrote a paper with Erdős in which we greatly improved a 30-year-old fundamental result of his, the so-called Erdős-Stone theorem. This theorem says that if a graph G on n vertices has εn^2 more (so, really, very few more) edges than the number guaranteeing a complete subgraph on r vertices, then suddenly it has a complete r-partite graph with t vertices in each class, i.e., r disjoint classes of t vertices, with an edge joining every pair of vertices belonging to different classes. (A little more precisely, we take $r \geq 2$ and $\varepsilon > 0$ fixed, and let $n \to \infty$.) This is very much a "phase transition" type result: once the number of edges increases beyond the point at which a "very thin" complete r-partite graph can be guaranteed, we can guarantee a rather "thick" (t-thick) complete r-partite graph as well. The question is all about the largest t one can guarantee. Erdős and Stone proved that the largest t one can guarantee is at least the $(r-1)$th iterated logarithm of n, the order of the graph. Erdős conjectured in numerous papers that the correct bound is precisely this iterated logarithm. To our great surprise, in the early 70s, almost thirty years after the publication of the Erdős-Stone theorem, we proved that the bound is log n, much larger than we imagined.

Another result I do like very much is about the scaling window in the phase transition of a random graph. Let us take a set of n vertices and add to it edges one by one, at random, with the uniform distribution, so that at "time" t we have t edges. The question we are interested in is "What does this random graph look like at various times?" (Here and elsewhere, all assertions are claimed to hold "with high probability", i.e., with probability tending to 1.) We are mostly interested in one of the crudest properties of our random graph: the number of vertices in the largest connected component. The greatest discovery of Erdős and Rényi was that at time $n/2$ there is a sudden *phase transition* in the sense that if the number of edges is a little less than $n/2$ then there is no large component, in fact, every component has at most order log n vertices; however, if the number of edges is $cn/2$ for some constant $c > 1$, then suddenly there is a *giant component*, a component of order n, in fact, a component with about $\alpha(c)n$ vertices, where $\alpha(c) > 0$. So the size jumps from order log n to order n.

Although at first sight this is a sharp result, it is far from so. Let us look at the point of phase transition through a magnifying lens. What magnification should our lens have to enable us to see the continuous emergence of the giant component? More formally, let us look at our process at time $t = n/2+s$. For what values of s is the largest component much larger than the second? Here are two rather different scenarios consistent with the theorem above. (1) If $s > n/\log n$ then with high probability the maximal component is at least 10^{10} times as large as the second, while for $s < n/(2\log n)$ this is false. (2) If $s > n^{1/2}$ then with high probability the maximal component is at least 10^{10} times as large as the second, while for $s < n^{1/2}/\log n$ this is false. Now, in the first case we would say that the *window* of the phase transition is about $n/\log n$, while in the second the window is about $n^{1/2}$.

About a quarter century after Erdős and Rényi proved their famous result, I proved that the size of the window is, in fact, $n^{2/3}$. Furthermore, if s is substantially larger than $n^{2/3}$, say, $s \geq n^{2/3} \log n$, but is still $o(n)$, then the largest component has about $4s$ vertices, and all other components are *much* smaller. This was the very first rigorous result about the size of a nontrivial window. All this is, of course, very close to

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percolation.

Let me finish with two more results. First, a lovely little theorem I proved with Andrew Thomason, which really should have been proved 150 years ago by Steiner or another geometer. Take any d-dimensional body of volume one. In that case, I can give you a box (a rectangular parallelepiped), also of volume one, so that no matter on which plane you project your body and the box, the projection of the box has at most as big a volume as the projection of the body. Note that we are talking about projections into $2^d - 2$ nontrivial subspaces: d subspaces of dimension 1, $d(d-1)/2$ subspaces of dimension 2, and so on. It is a little surprising that there is a body that in this sense minimizes all these projections.

And the last. Very recently, Oliver Riordan (one of the co-organizers of this program) and I proved that the critical probability of random Voronoi percolation in the plane is one-half. Of course, everybody who knows a little about percolation would have sworn that this critical probability must be $1/2$ and nothing else, but proving it was a very different matter. There is a strong similarity with the events in the 1960s and 70s, when everybody in percolation theory knew that the critical probability of bond percolation on the square lattice was $1/2$, but nobody could prove it; eventually, after a ten-year gap, Harry Kesten found a proof. The question concerning Voronoi percolation turned out to be much more complicated than that about the square lattice; my paper with Oliver will be published soon. Actually, our hope was that it would be the first step towards proving conformal invariance for random Voronoi percolation. The trouble is that even the "preliminary step" of showing that the critical probability is $1/2$ was much more difficult than we had bargained for, so we haven't yet managed to make much progress with conformal invariance.

I: It seems that you are a counter-example to the belief that good results can only be obtained before the age of forty.

B: Maybe, maybe, but, of course, the belief that a mathematician is dead after the age of forty is very much the figment of G.H. Hardy's imagination. Hardy loved to say that only young man can do real mathematics when, in fact, he himself was a very strong counter-example to that. Hardy after 40 was much, much better than Hardy before 40.

I: You have quite a few research students. Do you like teaching them?

B: I love to have good students. One of the many reasons why I love to be in Cambridge is that Cambridge has by far the best research students in Britain. I have had over 40 research students, many of them extremely good. It would be wrong to list them because whomever I wouldn't mention would be right to feel slighted. But let me just say that four of my students are professors in Cambridge. One of them is a Fields Medalist – Tim Gowers. His is the only name that I consider legal to mention because he's the only one to have got a Fields Medal.

I: Who are the people who influenced you most?

B: Paul Erdős is clearly the man who influenced my mathematical career the most. He was at almost every conference that I attended for 25 years. And one of my jobs at these conferences was to look after him. I really enjoyed his company very much. I would not have imagined how much I would miss him: I am really surprised that even a decade later I miss him very much.

When I was at Cambridge as an undergraduate, I got to know the great physicist Paul Dirac and his wife very well; I became very much part of their family. Mrs Dirac was from Hungary: she was a sister of Eugene Wigner, the Nobel-prize-winning physicist. It was wonderful to be around the Diracs. Mrs Dirac was the best hostess I have ever seen: she was very well read, had a great appreciation of art, loved antiques of all kind, and was extremely skilful to move the conversation to interesting, unconventional topics. Paul Dirac was an absolutely "free man", the free man *par excellence*, free in the sense that he was free of convention, and didn't have any baggage to carry, as he didn't want to prove himself, and did not mind what people thought about him. He was very polite and considerate, but he could say quietly his own opinion which was often different from that of other people's.

I: He was well-known for not saying too much, wasn't he?

B: That's true, but he did talk quite a lot when he was among friends. He talked to me quite a lot; I could never complain that he didn't. He is someone I have always respected tremendously. Unfortunately, precisely when we moved to Cambridge from Oxford in 1969, he retired to live in Tallahassee, Florida. It was a great blow to us because the Diracs were the people we knew most intimately in Cambridge. From then on, we always went to visit them in Tallahassee and stayed there for a week or even a month. People in Cambridge could never understand what Dirac could be doing in Florida, how he could "put up" with Florida after Cambridge. However, Dirac loved to be in Tallahassee and often told me that he should have moved there much earlier.

When I arrived in Cambridge for good, to become a fellow of Trinity College, I was surprised that J.E. Littlewood was still alive, as to me he was quite legendary. I was amazed

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that he was still around in the college. It was mostly through my wife, Gabriella, that I got to know him very well, and I am very lucky that I did. Gabriella, who is a sculptor, made several busts of him; one of these is now in the Combination Room of Trinity College. Littlewood had the reputation of being totally unapproachable, but by the time I got to Cambridge, he had mellowed much. Unfortunately, most of his former students and colleagues still respected him too much and were also a little afraid of him, so they very rarely visited him. He came to have dinner with us a lot; many times. When we had people for dinner, we asked him as well; his presence lent a weight to the evening as everybody was honored to be at dinner with Littlewood.

I: How old was he then?

B: He was 85 when we got to know him, and died at 92. He loved mathematics and had many stories about his friends, including Hardy, Russell and Wittgenstein. While sitting in the Combination Room, sipping claret, he would start his story with "Before the war..." Whenever somebody would ask "Which one?" the answer was always "The first". That was really wonderful.

When he died, I became his literary executive and inherited all his letters and papers; many of these papers originally had come from Hardy. I edited a collection of his stories, *Littlewood's Miscellany*, which is a delightful book, about twice as long as its predecessor *[A Mathematician's Miscellany]* and has many more stories. Of course, the stories were not new, but he remembered them after he had published that book. The extended version was published only after he died.

I: What do you think about Erdős's idea of the "ideal proof from the Book"?

B: Not very much. Actually that was really a joke of his – I talked about this with him many times. He was interested in proving good results; he did not set out to find the proof from the Book, as has been said about him many times. Of course, he was particularly pleased to find beautiful proofs of *simple results*. He always said, "Look, such gems, such really simple, beautiful proofs can only be found in the Book." You don't expect the Riemann Hypothesis to have a proof from the Book that one can give in 5 minutes. Of course not. You would expect an infinitely more complicated proof. So he always used "The Book" as a joke to enliven his lectures; it should not be taken seriously.

I: You have positions at Memphis and Cambridge. Isn't that a strange combination?

B: I must admit that it is. Everybody thinks it is. Actually I love both places very much. Cambridge is our true home: that's where we have been for close to forty years, and that is where our real house is – I'm sure that eventually we shall live only in Cambridge, with occasional trips to Budapest. But we also love to be in Memphis.

When I say that I love Memphis, people tend to be puzzled, but they don't know what they are talking about. In the first instance, we went to Memphis because my wife got absolutely fed up with Cambridge, finding it claustrophobic, and Erdős suggested that I go to Memphis, which he had visited many times, often several times a year. In Memphis I have a really wonderful job – no lecturing, no administration, a great assistant to look after me, funds to invite visitors, funds to travel, very clever and kind colleagues, an excellent gym, and so on. Although I do not have to lecture, I always give a graduate course on a topic I hope to write a book on. I view Memphis as a mathematical training camp, where the first thing to do is mathematics, and there is no second. Erdős had very good friends at Memphis – Ralph Faudree, Dick Schelp, Cecil Rousseau, Chip Ordman – mathematicians who helped him a lot: they are still in Memphis and now they are my friends as well; since my arrival they have been joined by several other excellent people like Paul Balister, Vladimir Nikiforov and Jenő Lehel.

On the other hand, when I say that I love Cambridge, nobody is surprised: "Of course, Cambridge is great." And Cambridge is great. I don't know whether you have been to any of the Cambridge colleges. For me one of the best aspects of my own college, Trinity, is that academics from different disciplines mix: we have outstanding people from all kinds of different subjects at our fairly informal lunches and rather formal dinners. You may find yourself sitting next to a physicist and an economist, and opposite a historian and a physiologist. These are wonderful occasions: you can talk about a great variety of topics to real experts in those fields. Also, it is flattering to be in a place where so many excellent people work. Of course, many a first-time visitor misses this aspect of a college entirely since with him the conversation tends to be shallow: "How long are you staying in Cambridge?", "Have you been here before?", "Where do you come from?", and so on. Thus, Vladimir Arnold got it completely wrong when he imagined that this kind of conversation goes on all the time. This couldn't be further from the truth.

I: Do you have a special position in Memphis? Was it created for you?

B: I'm the first occupant of a rather special chair, the Jabie Hardin Chair in Combinatorics. This chair was not created for me, but Erdős persuaded me that I should accept it, and my colleagues in Memphis were kind enough to be happy about it.

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Jennifer Tour Chayes: Basic Research, Hidden Returns >>>

Jennifer Tour Chayes

Interview of Jennifer Tour Chayes by Y.K. Leong (matlyk@nus.edu.sg)

. . . Bill Gates says research "is key to our long-term position."
- Dan Richman in *Seattle Post-Intelligencer*

Jennifer Tour Chayes has made important contributions to a newly emerging and rapidly growing multidisciplinary field that straddles mathematics, physics and theoretical computer science. Her current theoretical work on auction algorithms, self-engineered networks and phase transitions in combinatorics and computer science has found applications in the Internet and the computer industry.

After her BA in biology and physics, Chayes did a PhD in mathematical physics at Princeton. After some

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I: Do you travel a lot?

B: Yes, I do: too much. I'm sure the urge to travel goes back to my childhood. In Hungary I grew up feeling imprisoned, and I was always longing to travel, especially to the South. I still find the South very romantic.

I: Erdős traveled a lot too.

B: Yes, Erdős traveled an awful lot. He traveled in a different way, he traveled alone, and almost always went for rather short periods. I frequently go for several months, and then I take lots of people with me, mostly my students and former students from Cambridge and Memphis. I feel that I have to take my current students with me if I want to take care of them: it would be very unfair to leave them at home.

I: I understand that you have taught our present Prime Minister Lee Hsien Loong.

B: I certainly taught him more than anybody else in Cambridge. I can truthfully say that he was an exceptionally good student. I'm not sure that this is really known in Singapore. "Because he's now the Prime Minister," people may say, "oh, you would say he was good." No, he was truly outstanding: he was head and shoulders above the rest of the students. He was not only the first, but the gap between him and the man who came second was huge.

I: I believe he did double honors in mathematics and computer science.

B: I think that he did computer science (after mathematics) mostly because his father didn't want him to stay in pure mathematics. Loong was not only hardworking, conscientious and professional, but he was also very inventive. All the signs indicated that he would have been a world-class research mathematician. I'm sure his father never realized how exceptional Loong was. He thought Loong was very good. No, Loong was much better than that. When I tried to tell Lee Kuan Yew, "Look, your son is phenomenally good: you should encourage him to do mathematics," then he implied that that was impossible, since as a top-flight professional mathematician Loong would leave Singapore for Princeton, Harvard or Cambridge, and that would send the wrong signal to the people in Singapore. And I have to agree that this was a very good point indeed. Now I am even more impressed by Lee Hsien Loong than I was all those years ago, and I am very proud that I taught him; he seems to be doing very well. I have come round to thinking that it was indeed good for him to go into politics; he can certainly make an awful lot of difference.

I: Do you have any books in the pipeline?

B: I have two books coming out for the International Congress in August. One of them is a collection of problems – lots of beautiful problems, exactly what we discussed over coffee in Memphis with Paul Balister and others. It will be published by Cambridge University Press and is called *The Art of Mathematics* with the subtitle *Coffee Time in Memphis*. The other one is a book I wrote jointly with Oliver Riordan: its title is just *Percolation* – short and punchy.

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postdoctoral work at Harvard and Cornell, she was all set for a distinguished career in academia at UCLA until one fateful day in 1996 when Nathan Myhrvold, then chief technological officer at Microsoft, approached Chayes and her husband Christian Borgs with an offer for them to join Microsoft Research. The rest, as they say, is history. Since then this famous husband and wife team co-founded and co-manages the Theory Group of Microsoft Research, one of the most active and vibrant groups of theoretical research in industry. In addition to the impact left by the collaborative work of Chayes and Borgs with others, the Theory Group has attracted many leading mathematical scientists as visitors, spawning fundamental research in a way that is rarely seen in industry. This unique phenomenon has been highlighted in a recent (March 2007) issue of *Scientific American*.

Chayes is probably the most striking counterexample to the myth that women is not cut out for science or that science has no place for women. Co-author of more than 80 research papers and co-inventor of 11 patents, she is the Research Area Manager for Mathematics and Theoretical Computer Science at Microsoft Research, Affiliate Professor of Mathematics and Physics at the University of Washington, a Fellow of the American Association for the Advancement of Science and a National Associate of the National Academies. She has also served as Chair of the Mathematics Section of the American Association for the Advancement of Science and as Vice-President of the American Mathematical Society. She serves on the Board of Trustees of the Mathematical Sciences Research Institute, the Scientific Boards of the Banff International Research Station and the Fields Institute, the Advisory Boards of the Center for Discrete Mathematics and Computer Science and the Miller Institute for Basic Research in Science, the Communications Advisory Committee of the National Academies, the Committee on Facilitating Interdisciplinary Research, the U.S. National Committee for Mathematics, the Association for Computing Machinery Advisory Committee on Women in Computing , the Leadership Advisory Council of the Anita Borg Institute and the International Union of Pure and Applied Physics Commission on Statistical Physics. Her capacity for research and organizational work is indeed legendary.

Chayes was invited by the Singapore Mathematical Society for its Distinguished Visitor Program in July 1999 and by IMS to give a public lecture at the Institute's program on *Random Graphs and Large-scale Real-world Networks* (1 May – 30 June 2006). During her short stay (7 – 16 June 2006) at the Institute, she was interviewed on 12 June 2006 by Y.K. Leong on behalf of *Imprints*. The following is an edited and unvetted version of this interview in which she talks with exuberance about her passion for science and mathematics, conveying forcefully the time-tested faith, if not axiomatic truth, in the inevitable and unstoppable benefits of basic research in mathematics and science.

Imprints: Your BA was in biology and physics and you waited till graduate school before deciding to specialize in one of them (mathematical physics). How difficult was it for you to make this decision?

Jennifer Tour Chayes: I have always liked many different sciences. I started out wanting to do biology, and then I did a little bit of physics – I love physics, so I decided to double-major in physics and biology. I also did a lot of chemistry as an undergraduate, one course short of a chemistry major. Mathematics was my hobby, I just enjoyed doing mathematics as science, but I didn't think of it as a profession. I thought it was fun to do mathematics. I suppose I was better in theory than in experiments, so it was probably a better idea to go into physics than into biology because at the time that I entered graduate school (in 1979) there was not a lot of theoretical biology. There was theoretical physics and there was mathematics. So I could do a lot of mathematics as well. One of the things I feel is that you don't have to make a decision to stop doing some subject in order to do another subject. I feel that I can still choose later in my career. I chose to do some computer science, and I keep thinking that maybe one day I will go back to biology. Now, more than 25 years later, there are a lot of interesting questions in theoretical biology – the field has matured so that there really is a vibrant field of theoretical biology. It has been impacted by mathematics, physics and computer science. It's always difficult for me to make a quick decision but I don't feel these decisions are permanent until you can do everything.

I: What was your area of research in your PhD thesis?

C: I proved theorems about several different systems in solid state physics. The questions were very mathematical, having to do with random statistics. A lot of what I did in graduate school, even what I still do, has to do with phase transitions – special points in a system where there is a qualitative change in what is going on in the system.

I: You applied these ideas to algorithms too. It's very surprising, isn't it?

C: Yes. Any system, when it is large enough, starts to exhibit a kind of average behavior. When I change my parameters in the system, the behavior of the system sometimes changes dramatically. That's a mathematical definition of what happens at a phase transition. The nature of algorithms changes very dramatically when you change certain parameters. A system can go from being solvable (a very efficient algorithm) to not being solvable in a short period of time. So I find phase transitions in algorithms also. At

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first, I was a little surprised, but I was also very excited by the connection because when I saw the connection, I had already been working on phase transitions in physical systems for 15 to 20 years. It was very exciting to me that some of the phenomena that I understood very well were manifesting themselves in very different applications.

I: Were you first attracted to problems in theoretical computer science through the mathematics or was it the other way around?

C: If it has to be one or the other, I suppose it was the mathematics first and then the theoretical computer science but, in fact, it was the physics first, and then the mathematics, and then the theoretical computer science. Finding systems that have very interesting phase transitions, I was picking them up because I love phase transitions. They seem to have some new applications for theoretical computer science.

I: You taught in the universities before joining Microsoft. Did you experience any kind of "culture shock" in this career transition?

C: Yes, I suppose you could call it "culture shock". I think it's good to experience culture shock … it was very different from the university. Things happen on a much faster time scale. One day the company is interested in one thing, and then the Internet comes along and we shift. It's a much faster time scale than that of mathematical physics. Also there are people who really care about tearing the door apart … I find all of these very, very interesting. I could choose to participate in it or I could have a more academic group at Microsoft. I feel that I got the best of both worlds. Actually when I first told my colleagues from academia that I was leaving academia to go to Microsoft, everyone of them thought I was crazy. Now many of them think that I am very lucky, but at that time almost all of them thought I was crazy because they didn't believe that I could continue to do fundamental research. But Microsoft is very interested in fundamental research. Last week, we were giving a presentation to Bill Gates on some of our research. He was very interested in the mathematical details and he asked all kinds of questions about the mathematical details. I think there's real benefit for a company to have fundamental research because you never know what is going to be important.

I: Was there any time frame for a product or objective?

C: No, we are a very theoretical group. Microsoft has a huge development organization. There are thousands and thousands of developers. They are the ones who worry about the product time scale. In research, we worry much more about trying to expand the horizons to see where the world is going to be 10 years from now, 50 years from now. I don't think it makes sense for a company to try to have its research organization compete with its development organization. We don't have a pressure to do anything on a product cycle time scale. But sometimes, once in a while, we do get things into products, and I find that exciting too.

I: If it's not considered confidential, could you tell us a little about how you came to be involved in the founding of Microsoft's Theory Group and something about its structure – for example, is it localized in one particular place? Are there many permanent members and so on?

C: The way it started at Microsoft is that I was doing what I thought was very theoretical research on phase transitions and computer science. I told the chief technology officer at Microsoft about this research. He was a classmate of mine at Princeton when I was getting my PhD and he actually did his PhD in quantum gravity, which is much more theoretical than anything I did. But he left quantum gravity and did more classical things. I was telling this to him and he was saying to me, "Oh, you should come to Microsoft. You should do this at Microsoft." And I said, "Oh, that makes no sense." Then he kept encouraging me. Finally, my husband Christian Borgs, who is also a mathematical physicist, and I looked at Microsoft and we thought that this was a company that did care about fundamental research, even though at that time there was no research lab there. We believed them when we looked at some of the other research that was being done there. The only thing was that they thought we would have a very small group and we thought we would have a larger group. So there was some talking to do to make sure that we would have a group large enough to cover mathematics and physics effectively. This was started in 1997.

I: Like Bell Labs – they have fundamental research labs too.

C: Actually, at the time that Bell Labs was getting less fundamental in research, Microsoft was becoming more fundamental. The structure of the group … we have a relatively small number of permanent members (10). We have 8 postdocs who stay for a period of about 2 years. We have about half a dozen long-term visitors who stay anywhere from a few months to 2 years. Our visiting professors may come and spend a year or two years. Just like the IMS here, we have many short-term visitors (about 200 short-term visitors per year) – people who stay from one day to one month. We don't have workshops but we thought that if we were going to cover mathematics, physics and theoretical computer science and not hire hundreds of people, the best thing to do would be to bring in a lot of visitors and talk to them, do research with them and tell

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them to send their students to us. We have a lot of summer interns also. So it feels more like an institute than a normal research group.

I: Is there a place where the whole group is stationed?

C: We are basically stationed in Redmond. When I first went to Microsoft, we only had research in Redmond, the company's headquarters. After I was there for a few years, Microsoft opened up a few other research labs. There's a lab in Cambridge, England and in Silicon Valley. There's now a large lab in China. But the vast majority of the research is done in Redmond. Bill Gates feels it's better to have most of the researchers there so that they can interact more with the policy people and with him.

I: Can anybody apply to visit for a long term?

C: People can apply, but we don't have so many long-term visitors. If you are working with someone at Microsoft, it's more likely that you will get long-term visitor status, or if someone at Microsoft is very interested in what you are doing. Other people often come for short-term visits and if we find that there is common interest, then they come back for longer-term visits. Microsoft funds the visitors. Visitors are paid for various reasons. One of them is that some of our visitors have come up with some very valuable intellectual property for Microsoft. We find that discussing a problem – even a theoretical problem – with someone may turn out to have applications for Microsoft and if we weren't paying them, we wouldn't have a right to that idea.

I: What happens if a person develops an idea while that person is at Microsoft but doesn't fully develop it until the person has left Microsoft.

C: Well, while they are at Microsoft when they develop a valuable idea, then we can file a patent with them on the basis of what they did at Microsoft. Patents don't have to be on fully blown ideas. A patent is usually less than a paper. In an academic paper, you try to work everything out. In a patent, even if you have an idea but you haven't worked out everything, you can still get a patent for it. Now if someone starts something at Microsoft and we feel that it's very interesting, we will sometimes ask them if they would like to stay under a contract with us, maybe one day a week they develop that idea even when they go home, and we pay them for that.

I: Are you talking about patenting of ideas? That's unusual.

C: Well, you patent algorithms but algorithms are really just ideas on how to do something.

I: There's no hardware involved?

C: No, there's no hardware involved. Now, I was surprised as a mathematician, a basic scientist, when I did my first patent. You probably heard stories of patents that have very little in them, like the "one-click patent" at Amazon – people always use that as an example. But there are more substantial ideas than that even though not every detail is thought out. In fact, when an idea is very broad, it's often more valuable. Ideas in their early stages are more valuable because they are broad and then little pieces can be patented as refinements of that. For many years at Microsoft, I patented almost nothing. In my first 8 years, I think I did very little patents. Now in the last year, I have done 12 patents because I happened to be working on something that has a lot of applications for Microsoft. We look at every idea and ask, "Does it make sense to patent it to the extent it is involved?" If we think that it might be used in a Microsoft product, then we just protect ourselves with a patent.

I: Does it mean that if you have patented an idea, you may be constrained not to reveal the details when you write a paper about it?

C: Not at all. That's why you should patent it. Once you have patented it, you can tell it to the whole world because then you own the rights to it. Different companies have different ways of dealing with it. There has been a lot of criticism against certain technology companies because they don't patent. They just keep secrets. That's very hard on their scientists because then their scientists are not able to publish and not able to talk to people and not able to be scientists. For us, we make the decision. We look at something and each individual makes his own decision. No boss ever tells them. If you think this is useful for the company, then you patent it. Sometimes the day before I submit a paper, I would give something to the lawyer and say, "File a patent on this before tomorrow because I'm submitting this paper to a workshop tomorrow." This allows you to pass it to anybody because your rights to that idea are protected. In fact, it gives you much more freedom than being secretive about it.

I: Have any of your patents brought in any personal wealth?

C: It's hard to tell what the direct relationship is. Most of the patents I have done are very recent and some of them have to do with new ideas on the web. I think that some of those ideas are valuable to the company. It surprises me. I didn't think I was going to do math that is going to be passed onto the bottom line. It shows that it makes sense for a company to have a basic research outfit because you don't know what's going to be important. It turns out that

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algorithms are important and mathematicians are good at doing algorithms.

I: So it benefits the company more than you personally? The company has the rights to the patent.

C: I feel that it's fair. I don't teach. I get to travel. I'm well-compensated. I have freedom to invite collaborators. For me it's a very good trade off. I love my life. I'm happy that I'm able to do something that is worthwhile to the company to justify the expenses.

I: You mentioned that there are only 10 members. Are they mostly mathematicians or physicists?

C: Mostly mathematicians and theoretical computer scientists.

I: What about logicians?

C: We don't have any logicians, but we do have some combinatorialists. Certain parts of combinatorics are very close to logic. In Microsoft Research, there is one group that was started by someone who does logic. He's now doing other things but he was a logician. He's James Gorbit. He came from the University of Michigan and he started a group on abstract state machines but he did logic for many years. So there were people who did logic at Microsoft.

I: Any plans to get a logician into your group?

C: We try to get smart people into our group. If there's a brilliant logician, then we'll hire a logician. If there's a brilliant topologist, then we'll hire a topologist. I think it's much more who the person is, the quality of their work, rather than the subject because what people work on changes.

I: Does the Theory Group select only problems that are immediately relevant to computer software and technology?

C: No, absolutely not. We do basic research just like what you would do in a math department or a computer science department or a physics department. Sometimes we would talk to people in products and if the problems that they have are interesting mathematically, then we will look at those problems. We are really motivated by basic research.

I: Is collaboration more important or is the individual encouraged to work freely according to his or her own interests?

C: Definitely we want everybody to follow their own interests. On the other hand, we really do like collaboration. So you might have a couple of people going off doing something that doesn't have anything to do with what everybody else is doing. But when we hire people, we try to hire people who like to collaborate because we feel that there is a lot to be gained by collaboration. Also, we have many visitors who take advantage of our visitor programs and we expect people to collaborate a lot. I think that in trying to cover so many different fields with a small number of people, it's important to have people who like to collaborate because they can then bridge the gap.

I: Which do you think is more decisive for advances in computing – a conceptual revolution in theoretical computer science or a technological revolution in computer hardware?

C: I think they actually go hand in hand. As in many other sciences, when you work in experimental sciences, you see that there is an advance in experiments and then there is an advance in theory and then there is advance in experiments, and they go hand in hand. You find the theoreticians being inspired by the changes in the hardware and the people who build hardware inspired by the software revolution. Something has come up in hardware now – Intel and some other companies have produced the so-called "multi-core" chips. In such a chip there is a potential for parallel computation. That requires a true revolution in software. Intel was really like Microsoft in coming up with new software for multi-core chips so that people will want to buy those chips. Here is a hardware revolution and now we have several groups at Microsoft trying to figure out how to use these multi-core chips. There are a lot of very interesting theoretical problems and they ask people from the Theory Group to come to talk to them. Also there are other changes that are brought on by changes in software. A lot of the revolutions we have seen in computer science recently have been done by theory people who work at search engines. The two young guys who started Google were theory students at Stanford and they came up with the first algorithm for search engines. If you look at the whole field of web hosting, which is how to deliver content rapidly and is very important to the web, the web would not be as big as it is were it not for web hosting. Size would be going down left and right whenever people try to log on to them. Akamai, the biggest web hosting company in the world, was formed by a theoretician Tom Leyten and his students. These were revolutions in theory and software, and made hardware follow along. We are building this whole structure of the Internet on the web because there were some software ideas.

I: You mentioned the chip by Intel. That is a technical achievement. Was it necessary to do that? That's just making it smaller, isn't it?

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C: Well, it's not just making it smaller. Having many processors on one chip, the multi-core chip is qualitatively different from the old chips. Within each multi-core chip, you can do parallel computation. It requires a completely different kind of software on a machine language level. It is a real revolution, and I think that as we learn how to take advantage of that, we will find many incredibly new applications, just like now we learn to take advantage of increased inexpensive storage. We come up with voice applications and video applications to take advantage of Moore's Law – the increase in storage and increase in computability.

I: Do you think there is an intrinsic limit to computing power, either theoretically or technologically.

C: It would be interesting if there were a theoretical limit, something like a Heisenberg uncertainty principle for computation. It is certainly true that we will never be able to stay on more bits of information than the number of atoms in our universe. At a certain point we are going to be limited at the atomic scale. If you try to think of something along the Heisenberg uncertainty principle, you might think of the limitation in speed. However, with parallel computation, which is so much faster, and quantum computation which is a kind of parallelism, I'm not sure if there is an intrinsic limit beyond the atomic scale. That's a very interesting question.

I: What about quantum computers?

C: We actually had for many years in our group some people working on quantum computing. They have now spread out to form their own larger group. I think it's a very interesting idea. The error-correction aspect of quantum computing is the most challenging aspect. Mike Friedman, who is within our group for many years and has now formed his own quantum computing group, is working on a different model for quantum computation in which you build the physical system so that it doesn't generate errors and so that it automatically corrects for errors. He's working with experimental physicists who are trying to build these things. Nanoscience in computing is also very fascinating. There are a lot of experimental advances in nanoscience and theoretical advances in quantum computation that will help us with our computing power in the future.

I: Do you know whether there is anyone who has built a prototype of a quantum computer?

C: I know that there are some quantum gates that people have made, but unfortunately those are the ones in which errors have to be corrected in the gates. So they have very limited power at the moment.

I: So the quantum computer is more like a dream rather than a reality.

C: Quantum cryptography, I think, will be used before quantum computers. It is actually a rather promising method of cryptography.

I: What about biological types of computers? Has anybody come up with anything like that?

C: Using DNA and things like that? There are a number of people working on them. I sense more excitement about them a few years ago than I sense now. I think that there are some limitations to those things. I think they can help us possibly in the next few generations of micros. Beyond that we need something more than biological computation.

I: You are also working on auction theory. Are you more concerned with the optimal algorithms for auction strategies rather than with auction theory per se?

C: I'm working on algorithms for auctions and for game theory in general. I'm looking at algorithmic game theory. It's a very interesting field and that's the field in which I've been filing a lot of patents. There's a lot of interest to Microsoft – very much an area in which we're competing with Google and Yahoo and some other companies. I think there are fascinating questions there. You have to come up with methods for dealing with auctions very quickly. Whenever you put a search term into a search engine, there is an auction that takes place in a millisecond. You don't even notice the time, but all of these ads that appear on the web site of the search engine are a result of an auction having taken place when you enter that term. So you need very efficient auction algorithms. Our group came up with some methods that help to prevent click fraud by coming up with algorithms in which we understand what the incentives are to commit click fraud and getting rid of those incentives.

I: What is click fraud?

C: Click fraud occurs when, for example, you are one book seller and I am another book seller, and we are both putting ads on a search engine under the term "book". Now you don't pay for your ads unless somebody clicks on your ads, and I don't pay for mine unless somebody clicks on mine. So if I go and click on your ad, then it costs you money. But I'm not a real buyer or a potential buyer. I'm just trying to run my competitor out of business. There are a lot of problems with click fraud, but we came up with certain algorithms which get rid of a lot of click frauds. There are a lot of interesting problems like puzzles, and it's really a lot of fun. In fact, just last week we were showing some of

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that to Bill Gates. It's of interest to Microsoft and it's also nice mathematics.

I: Does this type of problems generate very theoretical mathematics?

C: Yes. In auction theory, if you try to auction different items to different people who want different bundles of items, those kinds of auctions are very complicated mathematically. They are called combinatorial auctions, and the number of combinations blow up very rapidly. There are fascinating deep theoretical questions – very difficult mathematical problems, NP-hard to approximate. We were also showing Bill some of the answers to those problems, which are still at the theoretical level, but it is important that we try to understand them.

I: What about applying those things that you are doing to economics?

C: Yes, we are working economics into all of this. We are doing algorithmic game theory, which brings together computer science and economics. Actually, we have had several good economists as visitors. We have been talking to them a great deal because I think there is very interesting mathematics there.

I: Have you ever thought of going back to biology?

C: I have thought about it actually. I talked to some biologists about it. There are several areas, all kinds of things in network theory, pathways to various enzymes that are close to the network questions that I'm working on in the context of the Internet and the World Wide Web. The state of diseases is certainly a biological question. There are all kinds of fascinating data-mining questions when you look at the genome. If we could use those data more efficiently, there is no question we would have cures for a lot of the diseases that plague us. I'm definitely thinking about that. I certainly want to go back to biology before my career ends.

I: One final mundane question. Did you face any kind of barriers generated implicitly by the "traditional gender mind-set" when you first joined academia or industry?

C: The first thing is that I try to ignore it. I try not to pay attention to that. I think I became more and more aware of them when I began to have students and when I began to have to make important judgment, because then I study the facts of the barriers in other people, for other people. I think there are two types of barriers: one is that there are a few people, not too many, who don't think that women are cut out for science. The president of Harvard made some very incorrect and politically stupid comments about that. I would have a pretty easy time dealing with that because if somebody thinks I'm stupid, I very quickly show them that I'm not stupid. If someone thinks you are stupid and you are not, it makes them very foolish. That is very easy to take care of. You just do very good work and no one can question that. There is another aspect of it, which is the leadership aspect. Are people comfortable with women leaders? I think that that happens in every male-dominated field. Over the years you have very confident women who take on leadership roles. After that happens, then you see changes. The changes are brought about by individuals who go in there and do such a good job that it's a moot point.

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