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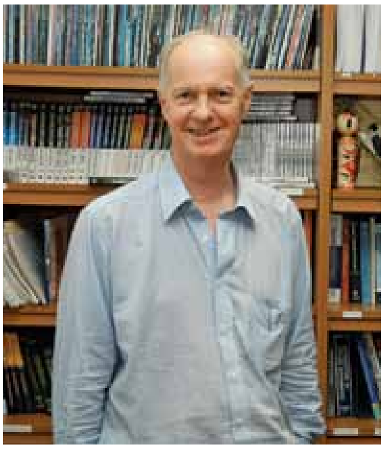
Probability and Discrete Mathematics in Mathematical Biology >>>



Andrew Barbour

[Editor's note: From March to June 2011, the Institute hosted a program on "Probability and Discrete Mathematics in Mathematical Biology". Andrew Barbour, Co-chair of the Organizing Committee, reflects on the growing importance of mathematics in modern biology, and on the IMS program.]

Probability theory has come to play an increasing role in modern biology. In contrast to natural sciences such as physics and chemistry, in which randomness is important only at molecular level, biological organisms of the same species usually differ markedly from one another, for reasons that may be obscure, and can most satisfactorily be described as being random. One of the major achievements of probability as applied to biology is the Mendelian explanation of the patterns of inheritance, involving probability statements at two levels: the independent selection for an offspring of one of the two alleles in each of the paternal and maternal pairs, independently of what happens for other offspring, and the independence of the choices within an offspring for different loci (which is true for pairs of loci that do not happen to be too close to one another on the same chromosome). These laws, put forward in the early 1860's, were largely ignored until the early 20th century; yet they predicted that inheritance was determined by genes that are present not singly but in pairs, very much earlier than experiment was able to confirm the fact. This striking success formed the basis of the modern theory of genetics, whose development has been accompanied every step of the way by probabilistic and statistical modelling. For instance, Kingman's introduction of the coalescent process in 1982 represents one of the major advances in the modern analysis of genetic data.

Another area in which mathematical modelling became popular in the early 20th century is that of population ecology and the spread of epidemics. Although behaviour at an individual level in such processes can be very different from individual to individual — for example, an individual's number of offspring, or whether or not an individual contracts a disease — the early models were used to describe what happens in a 'large' population, where the random fluctuations are relatively unimportant, and differential equation models were used with considerable success. However, in situations such as those encountered in conservation biology, the smallness of the populations and their susceptibility to 'random' fluctuations is the feature that is of greatest interest, making probability models essential. The development of the theory of stochastic processes from the 1950's onwards brought with it a corresponding increase in activity in stochastic population models. One feature of particular interest is that of random spatial variation. Even when a population as a whole is large, population densities

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may not be so, and the local behaviour of a population process may well take place under small population conditions; thus randomness once more becomes a matter of key importance. The household models developed in epidemiology also reflect this fact. Indeed, the success of the traditional large population models, in describing the evolution of epidemic disease in human populations, is not something that should necessarily have been expected.

The way in which interaction patterns influence many such processes has become a central topic since the emergence of the HIV epidemic. Here, it is not only spatial location that determines interaction; there are many other, much less easily measured, factors that determine affinity, so that concepts such as dimension may not even be the right ones to be thinking about. A major problem is that, for public health purposes, it is global outcomes that are the most important, and that these may not be well determined by the sort of local features of an interaction network that it is feasible to try to investigate empirically.

Based on such considerations, we designed our program around the fields of population genetics, stochastic processes in ecology, and networks, and the interactions between them; these are three areas in mathematical biology that we feel are of essential importance for the development of the field, and in which both probability and discrete mathematics play an important part. Our aim was to use the program to promote the use of probability theory and discrete mathematics in addressing problems currently arising there. To do so, we arranged two series of tutorial lectures, each followed by a research workshop, and invited colleagues to spend time conducting research with us outside these main blocks. A central theme was to stimulate young researchers to become interested in topics in mathematical biology, and to provide a means for them to interact both with established members of the community and among themselves. We were also keen to encourage the formation of new research partnerships between experts in the field, and to provide an environment within which research projects could be undertaken and pursued.

The first series of tutorial lectures and the first workshop focused on population biology and genetics. A frequently recurring theme was the influence of ecological processes, such as competition for resources, spatial distribution of populations, spatial and temporal variability in habitat, and (large scale) catastrophes, on inferences that are drawn from the standard population genetical models. In the second tutorial series and the second workshop, the emphasis was on networks in population biology. The areas covered included a variety of topics, ranging from models describing the growth of networks to the statistical analysis of data, such as protein interaction networks, that arise in the form of graphs. A particular example of the latter was a paper discussing alternatives to trees as descriptions of evolution, that are needed when horizontal gene transfer and hybridization play a significant role, and how one can detect whether a tree is an inadequate description. There was also considerable interest in the influence of network structure on random processes that evolve on a network, such as the spread of epidemics and social dynamics. The student participants were also encouraged to give talks on their own research during the workshops, and several of them took the opportunity.

Captivated by the mathematics of biology

In the part of the program outside the tutorial and workshop fortnights, the activities were less intensive, but no less productive. A changing number of participants spent time on collaborative research, and there were typically two or three research seminars each week. In addition, there was more time to spend in discussion with and mentoring of the student participants, some of whom were very happy to take advantage of the opportunity presented. Indeed, the friendly and relaxed environment at the institute was extremely conducive to research, and was universally appreciated by the participants — the whole staff are to be congratulated for their friendly and untiring attention to the welfare of all concerned.

Andrew Barbour
University of Zurich

People in the News >>>

Béla Bollobás elected as Fellow of the Royal Society

The Institute offers its congratulations to Professor Béla Bollobás of University of Memphis and University of Cambridge for his election as Fellow of the Royal Society in May 2011. Professor Bollobás was the Chair of the Organizing Committee of the IMS program Random Graphs and Large-Scale Real-World Networks (1 May – 30 June 2006). An interview with Professor Bollobás was featured in Issue 11 (September 2007) of *Imprints*.

Weinan E, Newly Elected CAS Academician

Congratulations to Professor Weinan E of Princeton University who has recently been elected as Academician of the Chinese Academy of Sciences. Professor E has served as a member of the organizing committees of the IMS programs Nanoscale Material Interfaces: Experiment, Theory and Simulation (24 November 2004 – 23 January 2005), Mathematical Theory and Numerical Methods for Computational Materials Simulation and Design (1 July – 31 August 2009) and Multiscale Modeling, Simulation, Analysis and Applications (1 November 2011 – 20 January 2012).

IMS Associates honored by SNAS

The IMS Director, Professor Louis CHEN, and the following associates and friends of IMS, Professors CHONG Chi Tat, LUI Pao Chuen, SHEN Zuowei, TAN Eng Chye, Bernard TAN Tiong Gie and Andrew WEE Thye Shen, have been anointed as inaugural Fellows of the Singapore National Academy of Science. They were among a group of eleven scientists in Singapore who received the honor in a ceremony held at NUS on 24 November 2011. Congratulations to all of them!

Professors Chong Chi Tat, Shen Zuowei and Andrew Wee Thye Shen are serving members of the IMS Management Board (with Professor Chong as the Chair). Professor Lui Pao Chuen is a former member of the IMS Scientific Advisory Board. Professor Tan Eng Chye served in the organizing committees of several IMS activities, including as Co-chair of the Organizing Committee of the program Representation Theory of Lie Groups (July 2002 – January 2003). Professor Bernard Tan Tiong Gie composed a piece of music *Remembrance* specially for the Institute's tenth anniversary celebration on 24 June 2010 (the musical score is available at http://www2.ims.nus.edu.sg/tenthanniversary/music.php).

Programs & Activities >>>

Past Programs & Activities in Brief

Computational Prospects of Infinity II: AII Graduate Summer School (15 June – 13 July 2011) and Workshops (18 July – 5 August 2011)

… *Jointly funded by the John Templeton Foundation*

Website: http://www2.ims.nus.edu.sg/Programs/011aiic/index.php

The objective of the AII Graduate Summer School was to bridge the gap between a general graduate education in mathematical logic and the specific preparation necessary to do research on problems of current interest in the subject. There were also three workshops with different themes, providing forums for discussion on latest research developments in the respective areas.

The AII Graduate Summer School consisted of intensive short courses with problem sessions by four leaders in the field, namely Richard Shore (Cornell University), Theodore Slaman (University of California at Berkeley and National University of Singapore), John Steel (University of California at Berkeley) and Hugh Woodin (University of California at Berkeley and National University of Singapore). In addition, there were also lectures delivered by four post-docs. A total of 73 participants (including 49 graduate students) attended the summer school. The lectures would be published as a volume of the IMS Lecture Notes Series.

The first workshop on Set Theory had 14 invited talks on the latest developments in the area and a problem session where participants gathered to discuss on the problems in their research work. A total of 44 participants (including 17 students) attended this workshop. The workshop on Infinity and Truth had 10 invited talks, a half-day panel discussion and a concluding informal discussion session. A total of 36 participants (including 10 students) attended this workshop. A proceedings for this workshop would be published as a volume of the IMS Lecture Notes Series. The third workshop on Recursion Theory featured 17 invited talks and a concluding discussion session. A total of 35 participants (including 8 students) attended this workshop. The workshops on Set Theory and Recursion Theory were a sequel to the highly successful workshop *Computational Prospects of Infinity*, held at IMS in 2005.

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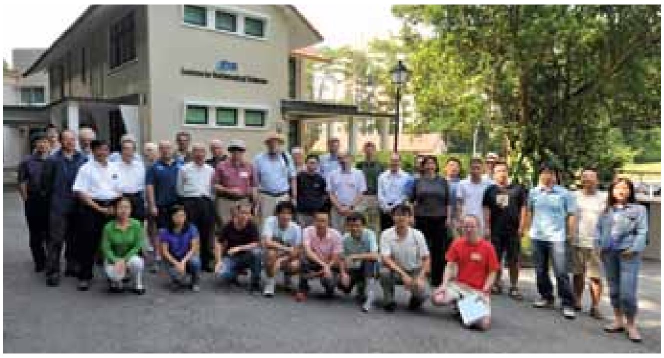
Scholars of infinite prospects


Summer school tutorial speakers (From left: Hugh Woodin, Richard Shore, John Steel, Theodore Slaman)


Logical discussions over coffee (Foreground from left: Ilijas Farah, Menachem Magidor, Steve Simpson)


Ekaterina Fokina: Computable structures


Seekers of infinity and truth

Automata Theory and Applications (1 – 30 September 2011)

Website: http://www2.ims.nus.edu.sg/Programs/011auto/index.php

Chair:
Frank Stephan, *National University of Singapore*

The program brought together researchers from various fields related to automata theory to enhance research in the following five areas of applications: (1) Automatic structures (2) Automatic groups in the framework of Thurston (3) Automata theory and genericity and randomness (4) Applications of automata theory in inductive inference (5) Hybrid systems. It consisted of a one-week workshop (12 – 16 September 2011) and two phases of informal discussions, seminars and collaborations.

The one-week workshop featured 22 invited talks on a range of topics related to the above five areas; including a presentation by Todor Tsankov (Université Paris Diderot - Paris 7) on his recent breakthrough in automatic structures. In general, the workshop gave insight into automata theory from various perspectives and stimulated the participants to think about new questions and connections. The workshop was attended by a total of 33 participants.

Three quarters of the program were dedicated to informal discussions and seminars. From the feedback gathered from some of the participants, this arrangement worked well as they got to interact, discuss quite diverse questions and open problems, work on research papers and got into more detailed technical aspects. A number of new collaborations were established among the local and overseas participants, and they would likely lead to joint research and publications in the future.


Automata theorists


Robert Gilman: Rationality in groups


Automatic exchanges (From left: Klaus Reinhardt, Dietrich Kuske, Thomas Zeugmann)

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Workshop on Phylogenetics for Infectious Diseases — with a focus on DNA viruses (10 – 14 October 2011)

Website: http://www2.ims.nus.edu.sg/Programs/011wphylogenetics/index.php

Co-chairs:
Vincent T. K. Chow, *National University of Singapore*
Swee Hoe Ong, *Genome Institute of Singapore*
Gavin J. Smith, *Duke-NUS Graduate Medical School Singapore*
Julian W. Tang, *National University Hospital*
Von Bing Yap, *National University of Singapore*

The aim of the workshop was to discuss how to improve the analysis of evolutionary and epidemiological aspects of viruses (mainly focusing on their genomic sequences) and the infections they cause, using mathematical tools, with a focus on DNA viruses (e.g. varicella zoster virus, human papillomaviruses, adenoviruses, etc.) which are less often analysed with these techniques; though RNA viruses, such as influenza and HIV are also included. By the end of the workshop, the participants had a better understanding of alternative and possibly newer mathematical approaches to analyse such viral gene sequence data. Notwithstanding, the workshop fostered greater collaboration between the various specialists in different disciplines.

The invited speakers comprised scientists from a wide range of disciplines. Some of them are virologists/microbiologists who had access to virally-infected clinical samples, and these included Julian Tang, Vincent Chow, Charles Grose (University of Iowa), Ulrich Bernard (University of California at Irvine), Yi-Mo Deng (WHO Collaborating Centre for Reference and Research on Influenza), Patrick Tang (BC Centre for Disease Control), Jennifer Gardy (BC Centre for Disease Control), Gavin Smith, Justin Bahl (Duke-NUS Graduate Medical School Singapore). There were also mathematicians, statisticians, and computer scientists, who could write the analytical software to analyse these sequences in a statistically robust manner, and these included Marc Suchard (University of California at Los Angeles), Alexei Drummond (University of Auckland), Sergei Kosakovsky-Pond (University of California at San Diego), Philippe Lemey (Katholieke Universiteit Leuven), David Welch (Penn State University), Stéphane Hué (University College London), Tommy Lam (University of Oxford). Many young participants, as well as several post-docs and PhD students, also had a chance to present their research findings. The workshop attracted a total of 53 participants.

It has been planned to publish some of the topics of this workshop in a special themed edition of Infection, Genetics, Evolution (IGE), which is one of the main journals within the field of phylogenetics.

Group with infectious enthusiasm for viruses

Phylogeneticists with style
(From left: Marc Suchard, Jennifer Gardy, Julian Tang, Charles Grose)

Hans-Ulrich Bernard: On human papillomaviruses

Workshop on the Design and Analysis of Clinical Trials (24 – 28 October 2011)

Website: http://www2.ims.nus.edu.sg/Programs/011wclinic/index.php

Chair
Weng Kee Wong, *University of California at Los Angeles and Singapore Clinical Research Institute*

Research in clinical trials is on the rise, in part due to advances in genetic research which is gaining in importance in the medical sciences. The goal of the workshop was to bring together statisticians, biostatisticians and experienced trialists together to exchange cutting-edge research ideas and discuss emerging developments in the design and analysis of adaptive clinical trials. The four-day workshop consisted of a one-day short course on *Adaptive Methods for Clinical Trials* and software lecture with demo conducted by Professors Tze Leung Lai (Stanford University) and Balasubramanian Narasimhan (Stanford University) and 12 invited talks by leading experts in the field covering vaccine trials, pragmatic and regulatory issues and recent statistical advances in the design and analysis of clinical trials. The short course was very helpful to graduate students and young scientists in attendance and the workshop had further stimulated their research interest in clinical trials. The workshop attracted a total of 87 participants, ranging from experts in clinical

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trials, researchers from academia, pharmaceutical and related industries, practitioners running clinical trials and graduate students interested to learn recent advances in adaptive techniques in clinical trials.

Designers of clinical trials

Speaking of (clinical) trials
(From left: Nancy Flournoy, Chris Jennison, Zhiliang Ying)

Tze Leung Lai: Adaptive methods for clinical trials

Public Lecture:

Samuel Safran: Understanding the effects of elastic forces on cells

Professor Samuel Safran of Weizmann Institute of Science delivered a public lecture titled "Order and Rigidity Sensing by Biological Cells" at NUS on 14 November 2011. Professor Safran talked about the role played by the elasticity of cells and their environment in the regulation of cellular processes such as proliferation, differentiation and tissue development. First he gave a brief review of current experiments on cellular sensing of substrate rigidity and stress and how this related to orientational (nematic) and layer (smectic) ordering that occurred in the cellular cytoskeleton of nascent tissues derived from stem cells. Then he presented some simple, theoretical models that integrated the active, elastic forces exerted by cells with liquid-interactions that characterized living matter. He concluded the lecture with some inspiring speculations on how environmentally responsive, physical forces in the cellular cytoskeleton could affect the long-term fate of stem cells.

Next Program

Multiscale Modeling, Simulation, Analysis and Applications (1 November 2011 – 20 January 2012) and Winter School (12 December 2011 – 13 January 2012)

*... Co-sponsored by Institute of High Performance Computing, A*STAR*

Website: http://www2.ims.nus.edu.sg//Programs/011multi/index.php

Co-chairs:
Weizhu Bao, *National University of Singapore*
David Srolovitz, *Institute for High Performance Computing and National University of Singapore*

The three-month program will bring applied and computational mathematicians, theoretical physicists and chemists, computational materials scientists and other computational scientists together to review, develop and promote interdisciplinary researches on multiscale problems that often arise in science and engineering. It will provide a forum to highlight progress in a broad range of application areas, within a coherent theme and with greater emphasis on mathematical analysis and numerical simulation for multiscale modeling and emerging applications in quantum physics and chemistry and material sciences.

Activities

- Collaborative research: 1 November 2011 – 20 January 2012
- Workshop I — Challenge and Modeling of Multiscale Problems in Mechanics and Materials: 14 – 18 November 2011
- Winter School: 12 December 2011 – 13 January 2012
- Workshop II — Multiscale Modeling and Simulation for Defects and Their Dynamics: 19 – 21 December 2011
- Workshop III — Mathematical Theory and Computational Methods for Multiscale Problems: 9 – 13 January 2012
- Public Lectures
  By Samuel Safran, Weizmann Institute of Science, Israel on 14 November 2011
  By Boris Yakobson, Rice University, USA on 10 January 2012
- Special Seminars: 16 – 18 January 2012

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Upcoming Activity

Workshop on Non-uniformly Hyperbolic and Neutral One-dimensional Dynamics (23 – 27 April 2012)

Website: http://www2.ims.nus.edu.sg/Programs/012whyperbolic/index.php

Co-chairs:
Juan Rivera-Letelier, *Pontifical Catholic University of Chile*
Weixiao Shen, *National University of Singapore*
Mitsuhiro Shishikura, *Kyoto University*

This workshop aims to bring together some of the leading experts working on parabolic renormalization, statistical properties and thermodynamic formalism of one-dimensional dynamical systems and related topics, to disseminate and explore possible research collaborations.

There will be four or five lectures in each of the five days of the workshop. Plenty of time will be left for informal discussions among the participants.

Programs & Activities in the Pipeline

Branching Laws (11 – 31 March 2012)

Website: http://www2.ims.nus.edu.sg/Programs/012law/index.php

Chair:
Chengbo Zhu, *National University of Singapore*

The program aims to examine important recent progress on branching problems, with special attention to the following topics: (1) Invariant theory and toric deformation; (2) Unitary representations and branching laws; (3) Gross-Prasad conjectures.

The activity will consist of a series of seminars by the overseas and local participants.

School and Workshop on Random Polymers and Related Topics (14 – 25 May 2012)

Website: http://www2.ims.nus.edu.sg/Programs/012randompoly/index.php

Co-chairs:
Frank den Hollander, *Leiden University and EURANDOM*
Rongfeng Sun, *National University of Singapore*
Nikos Zygouras, *University of Warwick*

The aim of this school and workshop is to bring together researchers working on random polymer models and related problems, as well as young researchers interested in this area, to foster learning, exchange of ideas, and collaboration, and to promote further progress in our understanding.

The topics to be covered in the school and workshop include, but are not restricted to: random pinning models, charged polymers, copolymer models, directed polymers with bulk disorder, Kardar-Parisi-Zhang (KPZ) universality class, and dynamics of polymers.

Activities

- School with three mini-courses: 14 – 18 May 2012
- Workshop on Random Polymer Models and Related Problems: 21 – 25 May 2012

Financial Time Series Analysis: High-dimensionality, Non-stationarity and the Financial Crisis (1 – 22 June 2012)

Website: http://www2.ims.nus.edu.sg/Programs/012hidim/index.php

Co-chairs:
Ying Chen, *National University of Singapore*
Piotr Fryzlewicz, *London School of Economics*
Qiwei Yao, *London School of Economics*

The program will invite world-leading experts in the areas of stationary and non-stationary modelling of low- and high-dimensional financial time series, and encourage them to use data covering the period of the recent financial crisis to discuss the impact of the crisis on their proposed models, methods and theories.

Activities

- Workshop: 4 – 7 June 2012
- Special Lecture Series and Graduate and Graduate Student Poster Presentation: 11 – 15 June 2012
- Workshop: 19 – 22 June 2012

Random Matrix Theory and its Applications II (18 June – 15 August 2012)

Website: http://www2.ims.nus.edu.sg/Programs/012random/index.php

Chair:
Ying-Chang Liang, *Institute for Infocomm Research*

The two-month program will provide mathematicians and engineers with a unique platform to discuss interesting fundamental problems, results and explore possible solutions related to random matrix theory and its applications in wireless communications and statistics.

Activities

- Informal seminars, ad hoc talks and discussions: 18 June – 6 July 2012
- Tutorial 1: 9 – 13 July 2012
- Workshop 1 — RMT Applications in Wireless Communications: 16 – 20 July 2012
- Informal seminars, ad hoc talks and discussions: 23 – 27 July 2012
- Tutorial 2: 30 July – 3 August 2012
- Workshop 2 — RMT Applications in Statistics: 8 – 15 August 2012

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Mathematical Conversations

Wendelin Werner: Probabilistic *Tour de Force* >>>

Wendelin Werner

Interview of Wendelin Werner by Y.K. Leong

Wendelin Werner was awarded a Fields Medal at the International Congress of Mathematicians in Madrid in 2006 "for his contributions to the development of stochastic Loewner evolution, the geometry of two-dimensional Brownian motion, and conformal field theory".

Werner's work embodies a new trend or style of mathematical research which combines a personal approach with fruitful collaboration with other researchers in breaking new ground and advancing existing achievements. This is exemplified, in particular, by his work with Gregory Lawler and Oded Schramm on the development and application of stochastic Loewner evolution (SLE). Another example is his work with Stas Smirnov in their work on the critical exponents for two-dimensional percolation on the triangular lattice.

Werner and his co-workers established rigorously what physicists have long believed to be true, if only physically justifiable, within what physicists call "conformal field theory". Werner's research provides a geometric connection between probability theory and classical complex analysis, and helped to close a hitherto wide gap between mathematics and physics in general, and statistical physics in particular. His ongoing research with his students and co-workers continues to probe the geometric depths of Brownian motion in the plane in terms of complex analysis and relate them to physical phenomena.

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Asian Initiative for Infinity (AII) Graduate Summer School (20 June – 17 July 2012)

... *Jointly funded by the John Templeton Foundation*

The Summer School bridges the gap between a general graduate education in mathematical logic and the specific preparation necessary to do research on problems of current interest in the subject. The main activity will be a set of three intensive short courses offered by leaders in the field. The invited lecturers are Stevo Todorcevic (University of Toronto), Gerald Sacks (Harvard University) and Ilijas Farah (York University). Selection of students and postdoctoral scholars for participation will begin in early 2012.

Meeting the Challenges of High Dimension: Statistical Methodology, Theory and Applications (13 August – 26 October 2012)

Website: http://www2.ims.nus.edu.sg/Programs/012statheory/index.php

Co-chairs:
Peter Hall, *University of Melbourne*
Xuming He, *University of Michigan*
Yingcun Xia, *National University of Singapore*

The topic of high-dimensional data analysis has many aspects, motivated by many applications, sometimes relying heavily on dimension reduction and variable selection, and sometimes co-habiting happily with more conventional multivariate methods. The program's first workshop (13 – 24 August 2012) will address all of these aspects. The program's second workshop (1 – 12 October 2012) will continue to address challenges of high dimensional data analysis with more focuses on the methods and applications where sparsity is present.

Activities
- Workshop 1: 13 – 24 August 2012
- Tutorials: TBA
- Workshop 2: 1 – 12 October 2012

Optimization: Computation, Theory and Modeling (1 November – 23 December 2012)

Co-chairs:
Defeng Sun, *National University of Singapore*
Kim Chuan Toh, *National University of Singapore*

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Born in Germany in 1968, he became a French national in 1977. He studied at the École Normale Supérieure and obtained his doctorate from the Université Pierre-et-Marie-Curie in 1991. He was a Leibniz Fellow at the University of Cambridge and a research officer at CNRS (Centre National de la Recherche Scientifique or National Center for Scientific Research). He is a professor at Université Paris-Sud (Orsay) since 1997 and also teaches part-time at École Normale Supérieure (Paris).

Other than the Fields Medal, he has received numerous awards, notably the Rollo Davidson prize, Prix Paul Doisteau-Emile Blutet (Académie des Sciences), European Mathematical Society prize, Fermat prize, Prix Jacques Herbrand (Académie des Sciences), Loeve prize and Pólya prize.

He is on the editorial board of the *Annales de l'Institut Henri Poincaré* and *Annals of Probability*. He is actively involved in scientific committees and advisory/ administrative boards in and outside of France. He also considers it to be part of his mission to reach out to the general public and to increase public understanding and awareness of mathematics, and has been giving regular talks for the general audience since 2002.

He is a member of the French Academy of Sciences and honorary fellow of Gonville and Caius College, Cambridge. He has been invited to give prestigious courses like the Cours Peccot and Mark Kac seminar, and named lectures like the James and Marylin Simons, Davidson, Lévy, Tom Wolff and Göran Gustafsson lectures, as well as plenary lectures at various major scientific meetings world wide.

Werner was invited to give a BS-IMS (Bernoulli Society-Institute of Mathematical Statistics) Special Lecture on "Are frontiers always symmetric?" at the 7th World Congress in Probability and Statistics held in Singapore, 14 - 19 July, 2008. It was jointly sponsored by the Bernoulli Society and the Institute of Mathematical Statistics and jointly organized by the Department of Statistics and Applied Probability, Department of Mathematics and Institute for Mathematical Sciences of the National University of Singapore. A special reception was held on 15 July 2008 to honor Werner for being the first probabilist to receive the Fields Medal at the International Congress of Mathematicians 2006 in what could be considered a watershed event for the recognition of probability theory as an area in its own right within mathematics.

On behalf of *Imprints*, Y.K. Leong took the opportunity of Werner's presence at the World Congress to interview him at the Department of Mathematics, NUS on 17 July 2008. The following is an edited and vetted version of the transcript in which he shares with us his views about mathematics and physics, his own style of research and the need for mathematicians to reach out to the public. His youthful and gentle demeanor belies the impact of his work on mathematics and statistical physics.

Imprints: You were born in Germany and became a French national at the age of 9. Could you tell us how this came about?

Wendelin Werner: I was born in Germany and my father was going to do a PhD in German literature. It turned out that the French National Library had acquired some manuscripts of Heinrich Heine the German poet. Because he wanted to work on those manuscripts, the family moved to Paris when I was one-year-old. Then we stayed on in France because my parents decided to settle there. I therefore grew up entirely in the French school system and I regard French as my most natural language, even if I speak German at times, with my parents for example.

I: You mentioned Heine [Heinrich Heine (1797 – 1856)].

W: Yes, he lived the last 25 years of his life in Paris, for political reasons.

I: What was the topic of your PhD work under the supervision of Jean-Francois Le Gall?

W: It was about the properties of two-dimensional Brownian motion, the continuous version of random walks. At that time, Professor Le Gall had proved some important results in that subject. When I started my thesis, he just moved on to another subject - random trees, branching processes, superprocesses and so on. In a way it was very good for me because he left me very nice questions.

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I: Was real physics a motivation for you?

W: At that time, no; I was treating Brownian motion as a purely mathematical object. Of course, it has a physical interpretation and relevance, but most of the questions I was first involved in were more "for mathematicians only". However, the reason for which I like Brownian motion is probably related to the fact that it is an abstract mathematical object that has some sort of concrete intuitive incarnation.

I: You were a research officer at CNRS from 1991 to 1997 even before you got your PhD. Was it a special arrangement and did your position at CNRS have any influence on your PhD research topic?

W: It was not a special arrangement. At the time when I was hired by CNRS, I had written a couple of research papers. I guess that at that time the general policy of the committee of CNRS was to hire promising very young people. Some of my friends were also in the same situation. It was more a general political decision about hiring strategy. Then, of course, when you are on a research position that young, it leaves you a lot of time and freedom (no need to worry about writing applications etc.), which is great.

I: Were you on some scholarship for your PhD?

W: I was a student in the École Normale Supérieure, which was some kind of scholarship, and I was hired at CNRS immediately after finishing ENS.

I: CNRS is not just an applied establishment, is it?

W: No, no, not at all. It is sometimes even caricatured as being a stronghold of pure and non-applied science, which is very wrong (many applied projects take place in relation to CNRS as well).

I: Is there close collaboration between CNRS and the universities?

W: Oh, the relation and intertwining between CNRS and universities is a very complicated issue... The answer to your question is clearly yes because most CNRS researchers (and I think all of the mathematicians at CNRS) work within the university labs. But now, if you follow the French news, there is some debate about reorganizing research in France and what the role of CNRS should be in the future. I guess this is a very French problem and I do not think this is very interesting to detail here.

I: How did you get into collaboration with Gregory Lawler and Oded Schramm on two-dimensional Brownian motion?

W: The first one of the two I met was Greg Lawler. He had done important work on two-dimensional Brownian motion. It was natural for me to discuss and get in contact with him. I met him at a couple of conferences. The real work started with him basically after one of his lectures in Oberwolfach, when we discussed in front of the blackboard. And then, when Oded Schramm invented his SLE using the Loewner equations for evolutions, it was clear that his tools were complementary to my work with Greg and provided the important missing link in the chain. Therefore, it was very natural for us to start discussing with Oded. I went to see him at the Weizmann Institute to see if one could combine all these ideas together. And everything worked out very well.

[Oded Schramm died unexpectedly in an accident on 1 September 2008 (less than two months after this interview) at the age of 47 while hiking at Guye Peak in Washington State. A towering figure in mathematics, he had been with Microsoft Research's theory group since 1999. Like Werner, he was also invited to give a BS-IMS Special Lecture at the same 7th World Congress. - *Imprints*]

I: Did Schramm work through email with you?

W: Yes; in fact, we worked mostly by email. We wrote quite a few papers with Greg, but we didn't actually meet up that much — which is, in a way, really strange because I guess we feel quite close [to each other].

I: Your research work involves geometric visualization in the plane. How important is geometric insight and intuition to the solution of those problems?

W: Ah-hah, this is a tricky question. It is true that my work can be explained or shown by pictures in two dimensions (this is what I'll have to give in my talk). On the one hand, it is really pure mathematics and I would not say that geometric intuition is the key to understanding these things. On the other hand, I should say that many people who work in two-dimensional structures - it could be complex analysis, or

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dynamical systems in the plane - are also people who played chess or Go when they were children or even later. So they like playing with these two-dimensional pictures. But these pictures are not the type of geometry that mathematicians do in the area that is usually called "geometry".

I: I remember in your website there are some very nice pictures of fractals.

W: Yes, I have a couple of pictures.

I: Do you work with the computer?

W: Not much. Well, in fact I spend probably too much time with email.

I: Maybe it's an advantage not to work too much with the computer.

W: Yes, I tend to think so. I'm considering removing the computer from my office in order to really think. That will be more efficient. We have email anyway at home and on the cellphone now, so maybe the office should be the one place without email...

I: Do you know whether there are any analogous phenomena of phase transitions in areas of economics and psychology?

W: I guess phase transitions occur in many, many different places like biology and economics; psychology is less obvious... On the other hand, to understand them and to really apply the mathematical model is another question. What we have been working on are some very specific two-dimensional problems and they are directly relevant to almost none of the real-world phase transitions. But understanding deeply one particular model sheds some lights on the others. And some theoretical physicists who first worked on such questions then applied ideas from statistical physics to economics in a seemingly successful way.

I: Phase transitions were first considered by physicists. The mathematicians came in much later, isn't it?

W: Yes, in a way when the physicist sees something occurring, they try to find a model and understand many things about the model and the [underlying] ideas. Several theories [about phase transitions] were invented and set up during the second part of the 20th century, and some of them became real mathematical subjects.

I: Do you work with physicists?

W: I wouldn't say that I work directly with them. I go to some of their talks and sometimes I speak at their conferences. I don't understand everything that they are saying, but it is clear that many of them have a powerful insight. And interacting with them is very useful for us.

I: They are not worried very much about mathematical proofs, isn't it?

W: I wouldn't think like this. I have a lot of respect for what they are doing and the way they function. It's true that according to our mathematical rules many of their arguments are not rigorous, but this is not what they are after. They want to understand the general features of phenomena, not necessarily all the details. But we can nevertheless be useful for them, because as we mathematicians know, one has sometimes to look at the details in order to see hidden mistakes or misunderstandings.

I: So it looks like physicists have to study more mathematics now.

W: Not all of them! It is true that they often do not have a solid curriculum in all aspects of mathematics and in probability theory in particular. So, when they use elaborate ideas or techniques, they often have to learn these things from scratch. While the discrete probabilistic models are easy to understand and [you] have a feeling about it, when you go to the next step to try to study continuous probabilistic structures like Brownian motion say, which is the first example of a continuous random object, then the mathematical definition is not so obvious, and there are some non-trivial things to master. Very often, physicists like to think about probabilistic objects using discrete intuition. In many cases, this is no problem because the discrete model is basically the same as the continuous one. But in some cases, the continuous object is actually much richer, and contains subtleties that one cannot capture in the discrete setting. That is precisely the case of the two-dimensional structures considered by Schramm.

I: The impression one gets is that probabilistic ideas provide powerful tools in solving problems in physics,

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economics, biology and engineering but not so much in pure mathematics. For example, do you think that probabilistic methods will one day be used to prove long-standing problems like the Goldbach Conjecture or even the Riemann Hypothesis?

W: I have no idea. I would be a little bit surprised in the case of these two problems. Well, I mean, there is interesting work going into the relationship between random matrices and the zeros of the Riemann zeta function, but proving the Riemann Hypothesis is another business.

I disagree with your statement that tends to disconnect probability theory from the rest of mathematics. There are numerous examples of recent and ongoing work in pure mathematics that involves probabilistic ideas and techniques. I would say that it is precisely because of the fact that probability theory was developed a bit later than many other parts of mathematics that it is only recently that interactions between what people do in probability theory and other parts of mathematics have been pointed out, and this led to many exciting fruitful results. Of course, I wouldn't say that probability theory will explain everything. It is more [evident] that it is a part of mathematics, and therefore very naturally interconnected to the other parts of mathematics. After all, mathematics is a whole and separation into subfields is always somewhat artificial.

I: Kiyosi Ito used to say that one should follow a great master. Do you have any guiding principle or philosophy that directs your research work?

W: I don't think I will have a good answer to that question. Each one, you know, has his/her own individual way to proceed and get good ideas. So, I definitely have no idea about the general philosophy on what other people should be doing except that they should do what they feel is right in their case. Personally, I don't have any special guiding principle or general ideology or philosophy. I try to do things that I find nice and that I like, and I just try to do my best.

I think that for the general public and also other mathematicians, it is important not to intimidate them but to explain that there is no mythical mathematician who understands everything and lives in a different world from the others. I'm always very suspicious, you know, when someone says, "This person is a genius." I don't believe it. And as I said, there are as many good ways to do mathematics as there are mathematicians. One also has to be lucky. I happened to work in a subject I like and where there are nice questions that turned out to be do-able and solvable using current tools.

I: Do you believe in working on hard problems?

W: In a way, yes. One trend of science in general, and mathematics in particular, is that for people who work in academia there is pressure to write papers, which led to an explosion in the number of papers that have been published. Also, as a result of this, it becomes risky to invest time to work on difficult problems. Because the problem is so difficult and other people have not solved it, there is some chance that you will not be able to solve it and you will not write anything about it in that length of time. On the other hand, I think it's important to know that these problems must be tackled. It is much more important to have one big problem solved rather than writing papers on something that is hardly surprising or gives no new idea. I think it would be better for the community as a whole if people were to take on the big challenges.

I: You once wondered whether the Fields Medal would change the way students listen to your lectures. Two years after ICM 2006, have you found the answer to that question?

W: As soon as you start really discussing mathematics, you just forget about it [the recognition]. If the students wonder whether my lectures are good, it means that they are thinking about the mathematical content, and not about me and the medal. I guess, in general, it's a fact that with fellow mathematicians who work closely with me and understand what we're doing, it doesn't change anything. And I'm very happy about it. But when you talk to people in other circles, people who see you from far away or even people within the university, like president of the university, then they change their attitudes. Some friends who didn't know anything about my profession suddenly look at you in a different way. But not the students, I think; they are happy if you give a good lecture and they are not happy if you don't give a good lecture, and they don't care who is giving the lecture.

I: You acted in a French film *La Passante du Sans-Souci* in 1982 at the age of 14. Did you ever have any ambition to take up acting as a career?

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Alexander Semenovich Holevo: Quantum Information, Quantum Computation >>>

Alexander Semenovich Holevo

Interview of Alexander Semenovich Holevo by Y.K. Leong

Alexander Semenovich Holevo made pioneering and fundamental contributions to quantum information theory, non-commutative probability and statistics and the statistical structure of quantum theory.

His ground-breaking paper of 1973 gave us what is called the Holevo bound which sets a limit on the amount of information that can be carried in quantum systems. Then in 1980, his original insights into the statistical foundations of quantum theory were set out in a monograph *Probabilistic and statistical aspects of quantum theory*. From then on, he used algebraic and probabilistic methods to solve difficult problems in the dynamical theory of open quantum systems and quantum stochastic processes. His work has exerted much influence on both the theoretical and experimental advances in quantum computing and quantum cryptography that have been steadily made since the end of the 20th century. His work continues the tradition of A.N. Kolmogorov in developing the theoretical foundations of probability theory and statistics and applying them to information theory and mathematical physics. In this direction, he has collaborated with both theoretical and experimental researchers in quantum optics, precision experiment, quantum cryptography and quantum computing.

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W: No. It was actually at the age of 13. It was a nice and interesting experience. I got some proposals to continue doing some movies other than this one, but it was clear that I wanted to do something scientific. I didn't know precisely what scientific research was about, though - it was not clear that I wanted to do research - but I just wanted to learn more stuff and it was clear from the very beginning, even before they shot the movie, that I did not want to take up acting as a profession.

I: Do you think mathematicians should try to communicate with the public?

W: It is important to communicate to the public what mathematics is and who mathematicians are, what sort of things they have in their minds and also to show that we are not a separate class of people. We are like most people. We are, for instance, not necessarily distant, dry people without emotions who live in an abstract world.

I: Maybe it's time for mathematicians to make an effort to communicate with the public?

W: Yes, yes. Of course, it's difficult because we are not really trained to do that.

I: Some people would consider talking to the public to be a waste of time.

W: I think that there should be more professional recognition for this activity and that outreach activities should count as regular teaching hours. Maybe this will help... Another thing is that interacting with the media (radio, television, newspapers) requires some skills that we are not really acquainted with. The relation with journalists can be quite difficult, because they may filter out the part of the message that we feel is the essential one. So we have to be trained a little in order to prevent being misunderstood and to give a faithful and positive image of mathematics.

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He has worked in the Steklov Mathematical Institute in Moscow for many years and continues to lead a vibrant research team there. He is also known for his lecturing skills which have been demonstrated in the Moscow Institute of Physics and Technology and Moscow State University. Arising out of his courses are two well-known books *Statistical structure of quantum theory* and *Introduction to quantum information theory.*

He serves on the editorial boards of several major scientific journals and has been active in the organizing committees of many international conferences. For his deep scientific contributions, he received the International Quantum Communication Award, Alexander von Humboldt Research Award and Markov Prize of the Russian Academy of Sciences.

He was invited to give an address on *Gaussian optimizers* and a tutorial on *Characteristics of quantum channels and the additivity problem* in August 2008 during the Institute's program on *Mathematical Horizons for Quantum Physics* (28 July – 21 September 2008) which was jointly organized with the Center for Quantum Technologies and partially supported by Lee Foundation and Faculty of Science. During his visit, he was interviewed on 25 August 2008 by Y.K. Leong on behalf of *Imprints*. The following is an edited and vetted version of the transcript of the interview. In it, he conveys the excitement of being one of the early pioneers of quantum information theory and the passion with which experimental physicists and mathematical scientists work together in pursuit of the elusive and "entangled" quantum computer. He also gives us a glimpse of the impact of the Russian mathematical tradition that is becoming clearly visible in modern mathematics and physics.

Imprints: What was the topic of your doctoral dissertation and who was your doctoral advisor?

Alexander Holevo: In Russia there are two scientific degrees: one is Candidate of Sciences which is equivalent to PhD in the West and another is Doctor of Sciences which is, to some extent, equivalent to Habilitation. Probably you are asking about the first degree which is similar to PhD. In this case, the topic was the statistics of continuous-time random processes and my supervisor was Yu. A. Rozanov who descended from the school of Kolmogorov and was one of his best students. So I can say I'm a mathematical nephew of Kolmogorov. The second dissertation was about non-commutative statistics and I came to this topic myself. It was called *Investigations in the general theory of statistical decisions* and was translated into English and published by the American Mathematical Society in 1978. The second dissertation should be completely independent research. After defending the Candidate or PhD degree, I changed the topic of my research. I wanted to be closer to mathematical physics and to problems which were considered in the seventies. I must say that my first published paper was even earlier and I did it under the supervision of another outstanding mathematician M. A. Naimark [Mark Aronovich Naimark (1909 – 1978)]. He gave us lectures on functional analysis when I was a student in the Moscow Institute of Physics and Technology. These lectures impressed me very much, and he gave me a problem on indefinite metric spaces, which I solved, and that was my first publication. But I was a little bit dissatisfied with the abstract approach to the subject as I had read [John] von Neumann's book and I knew that operator theory and functional analysis have profound motivation from quantum mechanics.

I: Von Neumann's book?

H: Yes, von Neumann. Naimark also has excellent books but they were purely mathematical. After the defense of my PhD, I somehow arrived at the huge area of non-commutative probability and statistics. Quite interestingly, at the same time, at the end of the sixties and beginning of the seventies there appeared the physical papers of American communication scientist C. W. Helstrom who started to develop the quantum analogue of mathematical statistics called quantum detection and estimation theory. In particular, he could prove the quantum analogue of the Cramér-Rao inequality. I was very much interested in that and I started to develop mathematically non-commutative statistical decision theory. In a sense, it is an analogue of Wald's theory [Abraham Wald (1902 – 1950)] but on the basis of operator theory. Here I applied positive operator-

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valued measures which turned out to be very useful for a mathematical description of quantum statistical decision processes. This was my start of the topic for the second dissertation. At that time I also became interested in quantum communication theory. Here I must first say that Dobrushin [Roland Lvovich Dobrushin (1929 – 1995)] was very supportive of my interest in this while he was a purely classical information theorist. This work was motivated by these people.

I: Was your 1973 paper motivated by Shannon's work on information theory?

H: After all, yes, but directly I was more motivated by the work of Helstrom and by the interest of such people as Dobrushin, Sinai and Chentsov [Nikolai Nikolaevich Chentsov (1930 – 1992)]. Also Kolmogorov himself, although he already retired at that time, expressed interest in the work that was going on. He attended some of my lectures and this encouraged me to do research in quantum communication theory.

I: Was this before quantum information theory was formally created?

H: One can say that there were two stages. What I'm speaking about is the first stage. At this stage, it was more quantum communication but already the notion of channel and the estimate of capacity appeared. But the coding theorem was proved only in the nineties with the new ideas of quantum computation due to [Peter] Shor, quantum data compression in the work of [Benjamin] Schumacher and other remarkable achievements.

I: Is your work primarily theoretical and mathematical in nature? How much do you interact with the experimentalists?

H: You know, conferences in quantum information theory have up to now a very positive feature - they join theoreticians and experimentalists. This is no longer the case in many other branches of mathematical physics. They do their conferences apart. But in conferences on quantum information you can meet people like [Anton] Zeilinger or [Nicolas] Gisin who do real work in experiments. I think an example of this combination of theory and experiment is the Centre for Quantum Technologies here in Singapore under the guidance of Professor A. Ekert. In Russia, I also have good contact with physicists like Academician K. A. Valiev [Kamil Akhmetovich Valiev (1931 – 2010)] and his group in the Russian Academy of Sciences and Professor S. P. Kulik in Moscow University and also in other countries, like Professor E. Polzik in the Niels Bohr Institute in Copenhagen. So we understand each other.

I: It seems that quantum information theory is mathematically rigorous but physicists are generally not fettered and obsessed by rigor. How do they react to this kind of rigor imposed by mathematicians?

H: It's a very nice question. It reminds me of a famous lecture by L.D. Landau [Lev Davidovich Landau (1908 – 1968)] which I attended when I entered my university. He gave a lecture especially for the first year physics students. One strong impression was his painful reaction on existence and uniqueness theorems. He blamed this kind of activity as "scholastic". This may be an extreme example of this kind of attitude. But in my opinion, rigor is important in mathematics but it is not the very essence of mathematics. You know that Euler, Newton, Hamilton, Ramanujan and Levy (in probability) have some papers which do not meet strict standards of rigor, but they were very deep mathematically. So the essence of mathematics is in something else, in depth of insight to the problem. Also I think what is important is conceptual clarity. This is important as well for physical theory: certainly at the frontier of modern theoretical physics it is a lack of conceptual clarity which prevents from creating a unified theory for quantum mechanics on one side and gravitation on the other side.

I: Generally, physicists depend more on intuition.

H: Of course, they rely on intuition, but mathematicians also rely on mathematical intuition, maybe a different kind of intuition. Physicists have an image of an object and they give this image all properties which they believe it to have

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while mathematicians think in terms of notions or concepts. Maybe this is the difference, but in both cases, intuition is quite important, especially in the first step of research.

I: Is it possible to combine these two modes of thinking in one person?

H: Kolmogorov was a brilliant example of such a combination. Bogolyubov [Nikolai Nikolaevich Bogolyubov (1909 – 1992)] is another example. There are other examples. When this happens, it's the best.

I: The ideas of quantum theory are rather counter-intuitive. How do we know that these ideas may not be mere shadows of a deeper reality that is yet to be discovered?

H: It's also a very good question. Based on my research in quantum information theory, I wrote a book in 1980 which was translated into English and published by North-Holland [2nd Edition: Edizioni della Normale, Pisa 2011]. It is called *Probabilistic and statistical aspects of quantum theory*. In the book I tried to explain as far as possible mathematical foundations of quantum theory based on probabilistic notions in a way which would be accessible to mathematicians who know probability and functional analysis. To some extent it is possible to develop the reasoning of quantum mechanics as the non-classical interaction of classical devices. You have preparation devices, you have measurement devices and they are described completely classically. But then they interact in a non-classical way. In this book, I base on classics of mathematical quantum physics like Dirac, Fock and von Neumann and later Feynman and Faddeev. You see, it is not possible to push this concept of non-classical interaction of classical devices to the very end. There is one key feature of quantum systems which is called "entanglement". There is no way to explain it in classical terms. One has to either accept it or to make war with it like some people did, including Einstein.

I: It is not just an abstract idea, isn't it? Is it something physical?

H: Yes, entanglement is a kind of correlation between parts of a composite quantum system, say a system consisting of two particles. Then it's possible that they interacted in such a way that you cannot model this correlation by any classical mechanism of randomness. So it's essentially a quantum random physics which cannot be modeled by what is called "local realism". Some people think that a deeper reality exists. This was the idea of Einstein - that there should be a more detailed description from which this quantum mechanical description follows, but it was shown later that it is not possible without creating contradictions with another fundamental property of locality. So local realism is not compatible with predictions of quantum theory. In this sense there is a deeper reality yet to be discovered, but it may not be reality of Einstein-type.

I: There is a lot of very exotic theories like multiple dimensions. Could they be the reality you are looking for?

H: One of them could be. For example, it can be string theory or non-commutative geometry, another candidate, but at present, one does not know what it would be. It's one of the major enigmas of modern science.

I: You have mentioned that the additivity problem is a very challenging problem in quantum information theory. Could you tell us roughly what this problem is and what kind of consequences its solution will have?

H: I would add that it is a very challenging mathematical problem in quantum information theory because it can be formulated in purely mathematical terms but of course it is easier to understand if you use the language of information theory. Essentially it concerns two parallel quantum channels. Both in classical information theory and quantum information theory, you can consider transmission of messages in parallel when you have two channels.

I: Are the channels independent of one another?

H: Two channels are completely independent one from another, they are called parallel channels. But messages may be dependent like letters in a message created or quantum states in quantum information that may be created in tandem. There are several numerical characteristics for channels and one says they have the additive property

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if the value of the characteristic for a pair of parallel channels is equal to the sum of the values for separate channels. One important example is Shannon capacity in classical information theory. It's possible to define notion of quantum channel which is just a mathematical notion of completely positive map. One can define analogue of Shannon capacity and one can ask whether it is additive. The problem is already 12 years old. It is still not known if it holds for all channels. It is proved for some channels, but so far there is no counterexample. We would be happy to have even a non-rigorous but correct proof of this because then it would be easy to do it rigorously. There is idea or intuition which is lacking in the solution of this problem. This is about what is important for mathematics - rigor or depth of intuition. There are two possibilities. Either it is additive for all pairs of channels. In this case, the consequences will be the following. Mathematically, it will enable us to easily compute what is called the classical capacity of quantum channels, that is, capacity of transmission of classical messages. Then you have one-letter expression and it's easy to compute, but it would be very unnatural from the physical point of view because, as I told you, the input for the parallel channels can be entangled, and entanglement usually increases capacity. In this case, you would have the mysterious phenomenon that entanglement somehow does not appear.

I: So, physically it's not likely to be true?

H: Yes, it's precisely what I want to say. Physically, most likely, you would expect it to be non-additive, but so far there is no example. [Existence of counterexample that solves the additivity problem in the negative has been shown by M. Hastings following A.Winter and P. Hayden shortly after the interview took place. — Holevo]

I: Do you mean mathematical example?

H: Also, there was extensive computer modeling trying to find a non-additive example. But to a high precision, they didn't succeed.

I: What about experiments rather than simulation?

H: It is possible to do numerical simulation and they don't find it. Capacity is not a good quantity for experiments. Even in the classical information theory, capacity gives you only an ideal, an upper limit to which you can transmit information, and even in classical information theory, the codes which attain this limit are not known. You can approach it, you cannot prove it by physical modeling. You can disprove it by computer modeling, but this does not happen.

I: In 2006, the first 12 qubit quantum computer was benchmarked. How far are we to the first prototype of a working quantum computer?

H: Let us look when the present information technology age started. It is a general belief that it was born in 1948. This was the year of two important discoveries. One was the physical discovery of the transistor and this started the technology revolution and the miniaturization of computer techniques and so on. Precisely in the same year and same place in Bell Labs, Shannon wrote his paper on information theory where he substantiated his idea of digital representation of information. This gave rise to software. So you see, the modern hardware and software had this year as a starting point. Now, in connection with quantum information theory and quantum computation, we already have an intellectual revolution and it happened in the 1990s, at the end of the 20th century, but there is still no technology revolution. There are some experiments of physical work, as you mentioned, but this is not technology. This is very perfect physical experiment, very unique, but when to expect this technology revolution, nobody knows. The only thing one can say is that progress of microelectronics is such that approximately in 12 to 15 years the size of logical gates in classical computers will reach atomic scale and then quantum noise will appear and will interfere with the operation of the classical logical elements. And then by necessity people will have to look for solutions and ideas of quantum information and quantum computation will offer solutions to this problem.

I: Will there be a limit to the computing power that can be reached?

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H: I said only that there should be some fundamental limit and there are such estimates made, for example, by S. Lloyd from MIT. He wrote several papers estimating the ultimate power of computers.

I: But the quantum computer is very different. There seems to be practically no limit to the quantum computer.

H: There is a common illusion that the quantum computer can do everything. Not at all. The quantum computer, even if it is realized, can do better only for isolated problems like factoring, for example, and maybe modeling complex quantum systems. This was the initial idea of Feynman. He suggested the idea of a quantum computer for modeling of complex quantum systems. For other problems, there is no proof and even no belief that it is stronger than any system.

I: In your 1973 paper you set some kind of limit to the performance of quantum computer.

H: Yes, precisely. You see, quantum information theory had started not from quantum advantages but from limitations. So quantum theory first puts formal limitations and the limitations are fundamental. You cannot supersede that.

I: There is a strong Russian tradition in probability dating from Chebyshev, Markov and Lyapounov that has survived the excesses of the Revolution especially during the Stalin period. What do you see is the strength of this tradition and what is the direction of this continuing tradition?

H: It was a very difficult time on the one hand, but on the other hand, science developed. For the record, I would add to the names of Chebyshev, Markov and Lyapounov the names of Kolmogorov and Khinchin. They were people of intermediate generation. They were young when the Revolution came and they took advantage of the new possibilities which were opened by the Revolution. After the communists came to power there was a famous meeting between Lenin and the heads of the Academy, including V. A. Steklov [Vladimir Andreevich Steklov (1863 – 1926)]. The Academy of Sciences started to develop very quickly and on a much bigger scale than when it was in Czarist Russia. Kolmogorov and Khinchin became famous in the thirties, which are known as a time of repressions. But one should have more colors for this period.

I: Russian scientists were working in isolation from the rest of the other scientists during that period.

H: No, I don't think so. Maybe in the thirties, especially in the forties due to the War. You know, in the twenties, several Russian scientists were sent abroad to establish and revive contacts with Western colleagues. One example is P. L. Kapitza who went to Rutherford in Cambridge and established a laboratory and became famous there. There were very good contacts, but later he came to the Soviet Union and was not allowed to go back. But the Soviet government bought for him the equipment from the Cambridge laboratory and built an institute, and he became the director of this institute. For sure, I would like to add Kolmogorov and Khinchin who had grown as famous mathematicians during this period. I think one of the strengths of the tradition in probability is in the combination of a purely mathematical approach and an interest in the problems of the real world. This is characteristic of both Kolmogorov and Khinchin as well as of Chebyshev, Markov and Lyapounov. This interest is important not only for applications, but it is also important for mathematics. It brings new ideas and ideas which are not artificial, which are well motivated. I try to continue this tradition in my work: in particular, quantum information theory, on the one hand, is closely connected to non-commutative analysis, algebra and non-commutative probability in modern mathematics, and on the other hand, it is connected to quantum physics. So it makes a bridge between these two disciplines.

I: Did the real life problems bring advances to the theoretical knowledge?

H: Certainly, I think so. In the case of quantum information theory, I can say so.

I: The Russian school of mathematics has produced people like Kolmogorov, Manin, Sinai and many others who are able to bridge pure mathematics and physics, chemistry, biology and other applied disciplines. What do you think are the factors that contribute to this phenomenon?

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H: I think there are several factors. But for mathematicians, one very important factor and, in some cases, the decisive factor is the support and guidance from senior mathematicians. If you have a person from a senior generation who has a broader vision and who supports you in your interest in applications of mathematics, that is very important. One can add the names of other former students of Kolmogorov who became famous, like V. I. Arnold [Vladimir Igorevich Arnold (1937 – 2010)], Ya. G. Sinai, as you already mentioned, A. N. Shiryaev and others. They took much inspiration personally from Kolmogorov. If you speak about later times, in the seventies, this kind of activity shifted to mathematical physics, and you can see several names of top Russian mathematicians who understand very well the important connections between mathematics and real world problems. For example, they develop quantum field theory and modern geometry quite successfully. I would give a more recent example of A. Yu. Okounkov, a Russian mathematician who got the Fields Medal [in 2006]. He was working on representation theory and combinatorics but he was also studying random surfaces and partitions.

I: He is from Moscow and now working in the United States, isn't it?

H: He is from Moscow, but he's now in Princeton.

I: That brings up the problem of brain drain. A lot of talented young Russians are going overseas. Is that a real problem?

H: Yes, it's a real problem.

I: Do you ever think of working overseas?

H: No. I have a number of offers overseas, but I never seriously thought about moving from here [Moscow].

I: Sinai is now in Princeton. Manin is no more in Russia.

H: Ya. G. Sinai comes every year to Moscow to conduct his famous seminar.

I: There is no shortage of very talented people coming back?

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H: There is a shortage of people of the most active generation, between 30 and 55.

I: They could always go back to Russia on visits.

H: In some cases, they come but it's not the same as working permanently in Russia.

I: The products of the recent genomic revolution are bringing out various ethical and environmental issues that were not seriously considered before. As a result, there have been recent concerns about possible adverse effects of future commercial products of nanotechnology on the environment that may not have been carefully thought through. What is your view on such concerns?

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H: I think that the term nanotechnology is very broad. It includes many different things, for example, biochemistry. It is easy to imagine that progress in biochemistry could have dangerous consequences if not properly regarded. If you are speaking about quantum technology, I think it is a bit early to speak about it. I told you there is still no technology revolution. When technology revolution comes, this question will arise. In particular, in what sense might quantum computers be dangerous? They might break secrets and pose real threats to information security, but on the other hand, quantum cryptography itself produces physically secure protocols. I don't see any effect of quantum information theory on the environment, but one should be very careful.

I: Do you have many students?

H: There are many good students but I must say that there is a serious problem. Surprisingly, in spite of the very difficult years in the nineties, we still have very good and brilliant young people who come to the university. But the problem now is that the new economic conditions make them earn money. Starting from the third year [in university] and later, they look for some additional jobs and this distracts them from science. So this is a problem.

I: If we look at the history of science, some of the major discoveries are made during difficult social times.

H: Yes, A. N. Kolmogorov [(1903 – 1987)] and A. Ya. Khinchin [(1894 – 1959)] give examples from tough times, but knowledge was then much more important than money.

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