IMPRINTS
INSTITUTE FOR MATHEMATICAL SCIENCES
JUNE 2004

At the Confluence of Economics, Mathematics and Statistics >>>

Public forum at SMU: from left - Robert Engle, Kenneth Wallis, Lawrence Klein, Roberto Mariano

Courtesy School of Economics and Social Sciences, SMU

In recent years, the most quantitative part of economics that is called econometrics has seen, with the aid of extensive computer calculations and sophisticated mathematical and statistical techniques, fundamental and dramatic advances in both theory and practice for financial economics. Outstanding examples of the richness of multidisciplinary cooperation in three major areas of human knowledge (economics, mathematics and statistics) were given during seven weeks of activities at IMS and the School of Economics and Social Sciences of Singapore Management University (SMU).

A joint program on econometric forecasting and high-frequency data analysis was held from 5 April to 22 May 2004. (A more detailed report of the program may be found inside this newsletter.) There were (two) Nobel Laureates, eminent scholars, active researchers, graduate students and practitioners. Some came from academia, others from industry. Many participants were based in Singapore and many came from different parts of the world. Ideas flowed, expertise was shared and many an experience enriched. It was an academic event that was also noted by the local and foreign news media. It was the fruition of an idea shared by Director Louis Chen and Founding Dean Roberto Mariano, both of whom first met each other more than thirty years ago when they went to Stanford University in the United States from their home countries in the East in search of knowledge, truth and a future.

A highlight of the program was the jointly organized one-and-a-half-day symposium held on 7 and 8 May. Among the fourteen speakers who presented papers at the symposium were Nobel Laureates Lawrence Klein and Robert Engle. Klein gave a symposium lecture on "Interpreting multi-sectoral versus multi-time-period analysis in forecasting" while Engle gave a symposium lecture on "Downside risk and its implications for financial management". Earlier on (6 May), Klein gave a seminar at IMS on "The University of Pennsylvania models for high-frequency macroeconomic modeling". Another very distinguished speaker at the symposium was Kenneth Wallis of University of Warwick, who was editor of the prestigious economics journal *Econometrica*. A bonus of the symposium came in the form of a special public forum on "Econometrics Today" in which these three eminent economists treated more than 120 forum participants (who were postgraduate

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From the Editor >>>

The cognition of randomness must have somehow been engraved into the evolving consciousness of the earliest living creatures, if only as an instinctive response, as they compete to avoid extinction. For many of them, the struggle to stay alive is almost synonymous with the management of risk in a world teeming with predators. The human mind, on the other hand, has been able to view randomness not just as uncertainty to be feared but as opportunities to be exploited to one's advantage.

Since the early ruminations of Pascal and Fermat on games of chance, scientists have continued to be intrigued by the realm of the random in nature. Although mathematicians have been able to provide a coherent theory of probability more than seven decades ago, it is the computer that allows one to use (and see the immediate results of) the theory in playing around with randomness in the most amazing and ingenious ways. The physicists were the first to exploit this avenue of scientific experimentation. Perhaps not so well-known is the fact that economists have been building computer-aided models with mathematical and statistical sophistication for a long time.

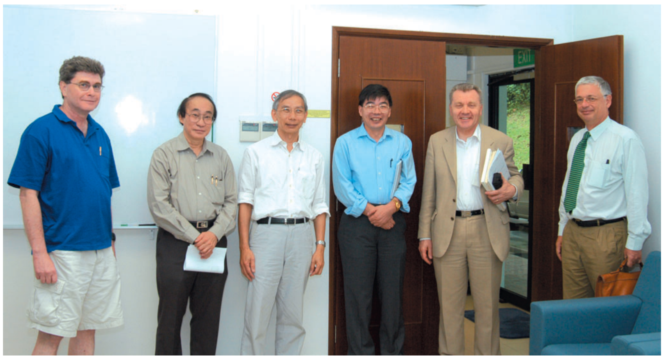
A dramatic example of the impact of computers on economics is the fairly recent successful use of computers in studying randomness in financial econometrics. This is something that is visible, tangible and practical. A combination of computer technology and innovative statistical ideas in time-series analysis provides the breakthrough tools in understanding random fluctuations (called volatilities) permeating financial markets. However, the computer by itself is not able to make sense of the vast amount of data that it churns out. It is the human mind that makes the fundamental leap of intuition and imagination. Theoreticians and practitioners of computing tell us that there are indeed logical limits to what the algorithms that drive the computations and simulations can achieve. They are not just technological limits. So, ultimately, we have to rely on the most vital and reliable computing tool we have – the human brain.

If no man is an island, it would follow that no area of knowledge is an island since knowledge is basically generated by man. Although the payoffs of multidisciplinary interactions among economics, mathematics and statistics have yet to be measured and quantified, no one doubts that they are mutually enriching and are of intrinsic and permanent value, Evidently, in the world of knowledge, there are no rigid and permanent boundaries, and no boundaries that can block the flow of ideas into or out of an area of knowledge. There are no closed boundaries of knowledge, only closed minds of the intellect.

Y.K. Leong

People in the News >>>

Professor Jean-Pierre Bourguignon (Institut des Hautes Études Scientifiques and Centre Nationale de la Recherche Scientifique) paid a short visit to the Institute on 24 May 2004 and met the Chairman of the Institute's Management Board, the Director and the organizers of the program on Geometric Partial Differential Equations. He also gave a colloquium lecture on "Is there a mystery behind the Ricci curvature?" during his one-day visit.



Bourguignon visits IMS: from left - William Abikoff, Louis Chen, Chi Tat Chong, Xingwang Xu, Jean-Pierre Bourguignon and Dominique Aymer-de-la-Chevalerie

Pauline Han left the Institute on 23 May 2004.

Claire Tan joined the Institute as the new housing officer on 17 May 2004 and William Chen as technical support officer on 24 May 2004.

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students, theoreticians and practitioners) with their views on the state of econometric methodology and its impact on economic research.

The presence of Klein and Engle added a touch of Nobel glamor to the program. The legendary stature of an 84-year-old successor and pioneer of a grand economic tradition can only leave one in awe while the mathematical sophistication of an econometric master will never fail to mesmerize the listener. Creative minds will always be tapped wherever they go. TV news media like BBC Asia interviewed them on an early Monday morning. SMU students were given the rare opportunity to interact with them at a special session. And, of course, we too would never miss the rare chance of picking the brains of first-rate thinkers. Some insight into their thinking may be glimpsed from the interviews published inside this newsletter.

Programs & Activities >>>

Past Programs in Brief

Markov Chain Monte Carlo: Innovations and Applications in Statistics, Physics and Bioinformatics (1 - 28 March 2004)

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

Chair:
Wilfrid Kendall, *University of Warwick*
Co-chairs:
Faming Liang, *National University of Singapore and Texas A&M University*
Jian-Sheng Wang, *National University of Singapore*

The aim of this program was to bring together people who work on innovative developments and applications in statistics, physics, and bioinformatics, with the intention first to encourage cross-fertilization between workers in rather different developments, second to challenge the theoretical capacity of these methods by exposing them to statistical and bioinformatical applications. About 20 research leaders from overseas were invited to participate in the program.

Tutorials took place throughout the program, which culminated in a research workshop in the last week. Seven tutorial lecturers each presented a set of lectures on their specialty: Bernd Berg (Florida State University), Julian Besag (University of Washington, Seattle), Rong Chen (University of Illinois at Chicago), David Landau (University of Georgia), Wilfrid Kendall (University of Warwick), Robert Swendsen (Carnegie Mellon University), and Elizabeth Thompson (University of Washington, Seattle). Under the broad title of Markov Chain Monte Carlo (MCMC), subjects ranged over statistical inference, genetics, sequential Monte Carlo, and simulation physics. The tutorials were complemented by 20 research lectures presented by leading scientists from across the world, mostly taking place in the research workshop at the end of the program, and representing a wide range of current research in MCMC. As well as formal lectures and research talks, there was a poster session / reception which helped to generate many informal discussions. Attendance at tutorial sessions averaged 25; attendance during the research workshop averaged around 30.

The interdisciplinary nature of the program was strongly appreciated by the participants who have all learned a great deal from the variety of ways in which MCMC is forming a vital component of problems ranging from protein modeling through criticality phenomena of interacting systems and applied statistics to genetic pedigree analysis. It is clear from the feedback received from invited visitors that a variety of research collaborations have been started or brought further as a result of the opportunities offered by the program.

The following is some encouraging feedback from the participants.

"I've had a tremendous time! First class local organization."

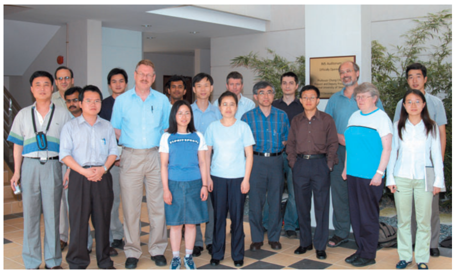
"Thank the organizers for a wonderful job. It is really interesting to hear different perspectives, and to look at both the theoretical side and the application side of different algorithms."

"I think the organizers and the IMS Staff did a great job! It was so interesting to hear people from various disciplines; and the relatively small scale of the conference made it easy to have in-depth discussions with other participants."

"This workshop on MCMC methods is quite astounding by its quality and the diversity of the participants; I can only wish I had stayed longer! I also want to thank the IMS Staff for their incredible hospitality and help in organizing my stay here. I have rarely seen such a well-run institute. Bravo!"

"It was great getting to learn about the issues faced by other communities (such as Physics) in using MCMC methods. Also, the organization of the conference and helpfulness of the staff were all top notch!"

Elizabeth Thompson: G(enetics) = $(MC)^2$



Snapshot of a long-run equilibrium

Simulating tea break

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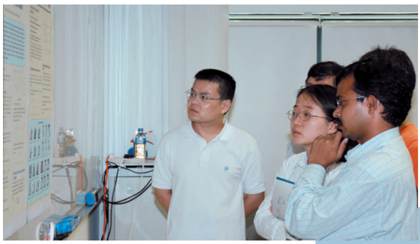

Pictures at an MCMC exhibition

Robert Swendsen: Count of MC Monte Carlo

Econometric Forecasting and High-Frequency Data Analysis (5 April - 22 May 2004)

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

Co-chairs:
Tilak Abeysinghe, *National University of Singapore*
Roberto S. Mariano, *Singapore Management University and University of Pennsylvania*
Yiu Kuen Tse, *Singapore Management University*

This was an intellectually stimulating program attended by econometricians and statisticians from all over the world. Participants benefited greatly from the six tutorial series, covering topics on forecasting seasonal time series, forecast evaluation, multivariate time series forecasting, econometric analysis of financial high-frequency data and affine processes in finance. In addition, there were 13 seminar paper presentations by visiting and local participants. The highlight of the program was the two-day symposium with 14 invited papers, ending with a forum on "Econometrics Today" featuring Nobel Laureates in Economics Robert Engle (New York University) and Lawrence Klein (University of Pennsylvania), and former editor of *Econometrica* Kenneth Wallis (University of Warwick).

Philip Hans Franses (Erasmus University Rotterdam), surveyed models in analyzing seasonal variations in means, variances and also in correlation structures. He argued that there is overwhelming evidence that out-of-sample forecasts improve for models that include seasonal variation in a proper way. Kenneth Wallis' tutorials on forecast evaluation are especially enlightening. His stress on reporting of uncertainty and interval forecasts (central intervals, shortest intervals), density forecasts (histograms, fan charts) and evaluation of forecasts sends important messages to practitioners in macroeconometric prediction. Wolfgang Breymann (ETH-Zentrum) and Jeffrey Russell (University of Chicago) provided comprehensive surveys on analyzing high-frequency financial data. Various stylized facts were presented, with works extended to study important issues in micro-market structure, including measures of transaction costs, multivariate correlations and tail behavior. Christian Gourieroux (CREST, CEPREMAP and University of Toronto) rigorously integrated a literature of asset-pricing models under an affine class of processes and explored the full potential of affine models. Building upon the traditional ARMAX model, Manfred Deistler (Technische Universität Wien) elegantly presented the factor model with interesting empirical examples. An average of 56 participants attended the tutorials.

The program culminated in the symposium, with invited speakers presenting their frontier research. Researchers paid due attention to forecasting macroeconomic variables (such as inflation rates) and their implications for the martingale-difference hypothesis, as well as forecasting financial data, especially the predictability of stock prices. Realized volatility took center stage in modeling high-frequency financial data, with interesting improvement with respect to unbiased estimation and optimal sampling. 61 people attended the $1^1/_2$ day symposium. The forum in the symposium was open to the public, with an attendance of over 120 people. The panelists shared their reflections on the state of econometric methodology. It was indeed a very rare opportunity in which top researchers in the discipline engaged in an open dialogue with their fellow researchers as well as laymen on some fundamental questions on the discipline that forms the basis of their career. The web cast of the forum can be viewed at http://www.sess.smu.edu.sg.

The following is some encouraging feedback from the participants.

"Many thanks for the excellent conference – one of the very best in my professional life. The mix of very informative tutorials and original papers made this a very insightful experience."

"The NUS-SMU conference on forecasting and HFD was excellent, not only thanks to the impressive coverage of speakers but especially due to immaculate organization and overwhelming hospitality. The interaction between math/stats and finance is very important and conferences like these stimulate this. I hope to remain in close contact with many people here and will follow the developments of NUS and SMU in the future."

Sign rule from Tilak Abeysinghe

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Econometrics symposium at IMS

An economical break

Christian Gourerioux's WAR on term structures

Current Program

Geometric Partial Differential Equations
(3 May - 26 June 2004)

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

Co-chairs:
Xingwang Xu, *National University of Singapore*
Paul Yang, *Princeton University*

There will be four sets of tutorial lectures spread throughout the program and the speakers are Thomas P. Branson (The University of Iowa), Neil Trudinger (Australian National University), Frank Pacard (Université Paris XII) and Alice Chang (Princeton University). About 27 researchers from overseas have agreed to participate in the program.

Neil Trudinger on PDEs

Next Program

Wall-Bounded and Free-Surface Turbulence and its Computation (July - December 2004)

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

Co-chairs:
Mohamed Gad-el-Hak, *Virginia Commonwealth University*
B. E. Launder, *University of Manchester Institute of Science and Technology*
Chiang C. Mei, *Massachusetts Institute of Technology*
Olivier Pironneau, *University of Paris VI (Pierre et Marie Curie)*
Khoon Seng Yeo, *National University of Singapore*

To date, 28 overseas visitors have agreed to participate in the program, which will comprise a series of seminars, tutorials and workshops, including workshops on the following sub-themes:

(a) Computation of turbulence I (13 – 15 July 2004)
(b) Computation of turbulence II (3 – 5 August 2004)
(c) Turbulence at a free surface (31 August – 2 September 2004)
(d) Transition and turbulence control (8 – 10 December 2004)
(e) Developments in Navier-Stokes equations and turbulence research (13 – 16 December 2004)

The following have agreed to conduct tutorials: Tim Craft (University of Manchester Institute of Science and Technology), Hector Iacovides (University of Manchester Institute of Science and Technology) and Pierre Sagaut (LMM - University of Paris VI (Pierre et Marie Curie)/(CNRS)).

Programs & Activities in the Pipeline

International Conference on Scientific and Engineering Computation (IC-SEC 2004) (30 June - 2 July 2004)

Website: http://www.ic-sec.org/index.html.

The IC-SEC 2004 is jointly organized with Faculty of Engineering, Faculty of Science and Institute of High Performance Computing (IHPC), Singapore.

Invited speakers to the conference include Shiyi Chen (The John Hopkins University), Yonggang Huang (University of Illinois at Urbana-Champaign), Michael M. Humphrey (Altair Engineering Inc.), Shaker A. Meguid (University of Toronto) and Kenji Ono (University of Tokyo).

This year, Computational Science and Engineering (CSE) Symposium 2004 will be held in conjunction with IC-SEC 2004. It aims to highlight new developments and applications of CSE techniques and methodologies to industry and the applied research community in Singapore, as CSE is fast becoming an integral part of the product and

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process innovation cycles in industry.

The 6th International Chinese Statistical Association (ICSA) International Conference (21 - 23 Jul 2004)

Website: http://www.statistics.nus.edu.sg/ICSA.htm

Co-chairs:
Louis Chen, *National University of Singapore*
Zhiliang Ying, *Columbia University*

The conference is jointly organized with the Department of Statistics and Applied Probability and is co-sponsored by Institute of Mathematical Statistics. Plenary speakers of the conference include Jianqing Fan (Princeton University & Chinese University of Hong Kong), Kung-Yee Liang (National Health Research Institute, Taiwan & Johns Hopkins University) and David Siegmund (Stanford University).

Mathematics and Computation in Imaging Science and Information Processing (Continued Program) (August 2004)

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

Co-chairs:
Amos Ron, *University of Wisconsin-Madison*
Zuowei Shen, *National University of Singapore*
Chi-Wang Shu, *Brown University*

The following upcoming activities form a continuation of this program:

(a) Workshop on "Functional and Harmonic Analyses of Wavelets and Frames" (4 - 7 August 2004)
(b) International Conference on "Wavelet Theory and Applications: New Directions and Challenges" (10 - 14 August 2004)
(c) CWAIP-IDR-IMS Joint Workshop on "Data Representation" (16 - 20 August 2004)

Tutorials will also be conducted by Emmanuel Candes (California Institute of Technology), Palle Jorgensen (University of Iowa), David Larson (Texas A&M University) and Denis Zorin (New York University). About 50 overseas visitors have agreed to participate in the program and give invited talks at the conference or workshops.

Nanoscale Material Interfaces: Experiment, Theory and Simulation (24 November 2004 - 23 January 2005)

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

Co-chairs:
Weizhu Bao, *National University of Singapore*
Bo Li, *University of Maryland*
Ping Lin, *National University of Singapore*
Jian-Guo Liu, *University of Maryland*

This two-month program will bring together leading international physicists, material scientists, computational scientists and applied mathematicians, and experts from NUS Departments of Physics, Materials Science, Mathematics and Computational Science, and from A*STAR institutes IMRE and IHPC. The program participants will:

(i) review recent developments in the research on materials surfaces and interfaces, from experiment to theory to simulation;

(ii) identify critical scientific issues in the understanding of the fundamental principles and basic mechanisms of interfacial dynamics in different kinds of materials systems, particularly those that are characterized by fluctuation, multiscale, and non-equilibrium;

(iii) accelerate the interaction of applied mathematics and computational science with physics and materials science, and promote the highly interdisciplinary research on new material interface problems with emerging applications.

Activities:
(a) Research collaboration (24 November 2004 - 23 January 2005)
(b) Workshop 1 (25 - 29 November 2004)
(c) Tutorial (3 - 7 January 2005)
(d) Workshop 2 (10 - 14 January 2005)

3rd Asia Pacific Workshop on Quantum Information Science (3 – 15 January 2005)

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

Co-chairs:
Artur Ekert, *University of Cambridge*
Choo Hiap Oh, *National University of Singapore*
Kok Khoo Phua, *SEATPA and National University of Singapore*

This workshop is jointly organized with Department of Physics, NUS and deals with the interface between quantum mechanics and computer and information science. It is currently one of the most vibrant areas of scientific research worldwide and has attracted many researchers from physics to mathematics to computer science to engineering. One of the primary aims of the workshop is the promotion of interest in quantum information among mathematicians and computer scientists in Singapore. Tentatively, the confirmed list of invited speakers are: Yakir Aharonov* (Israel); Hans Briegel (Innsbruck); Mo-lin Ge (Nankai); Daniel Greenberger (CCNY); Gerald Milburn (Queensland); C.P. Soo (NCKU) and Reinhard Werner (Braunschweig).

* Subject to further confirmation.

Semi-parametric Methods for Survival and Longitudinal Data (1 February – 15 April 2005)

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

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Co-chairs:
Yougang Wang, *National University of Singapore*
Zhiliang Ying, *Columbia University*

This program focuses on certain aspects of modern statistics and biostatistics in semi-parametric modeling and analysis; in particular, those arising in health and biomedical sciences, genetics and economic studies. It will cover the following topics: non-proportional hazards regression; multivariate survival analysis; semi-parametric models for limited dependent variables in cross-sectional studies and panel data; longitudinal data analysis; computer-intensive methods and analysis of large data sets. Leading experts will present state-of-the-art developments and identify new research problems and directions.

Activities:
(a) Computationally intensive methods (13 - 26 February 2005)
(b) Interaction/collaboration (27 February - 5 March 2005)
(c) Survival analysis (6 - 19 March 2005)
(d) Interaction/collaboration (20 - 26 March 2005)
(e) Longitudinal data analysis (27 March - 2 April 2005)
(f) Semi-parametric models for duration and panel data in econometrics (2 - 9 April 2005)

Uncertainty and Information in Economics
(9 May – 3 July 2005)

Website: http://www.ims.nus.edu.sg/Programs/uie

Co-chairs:
Robert Anderson, *University of California at Berkeley*
Parkash Chander, *National University of Singapore*
Peter Hammond, *Stanford University*
Yeneng Sun, *National University of Singapore*

Introducing probabilistic analysis to manage uncertainty and limited information in basic microeconomic models has enriched our understanding of economic behavior and made fundamental advances in the mathematical theory of economics. This program will have three sub-themes: game theory, information economics, and finance, with uncertainty and information as the underlying thread connecting these sub-themes. Specific topics include basic game theory, coalition formation, auctions, incentive compatibility, automated and algorithmic mechanism design, equilibrium and asset pricing. In addition to participating in the conference and tutorials, program visitors will give seminars related to the core themes and engage in research interactions and new collaborations.

Activities:
(a) Tutorials (30 May - 3 June; 13-17 June, 2005)
(b) Conference on Uncertainty and Information in Economics (6-10 June 2005)

Computational Prospects of Infinity
(20 June – 15 August 2005)

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

Co-chairs:
Chi Tat Chong, *National University of Singapore*
Qi Feng, *Chinese Academy of Sciences, China, and National University of Singapore*
Theodore A. Slaman, *University of California at Berkeley*
W. Hugh Woodin, *University of California at Berkeley*

The program will consist of tutorials and seminars given by researchers from Europe, North America and Asia. The tutorials will touch on topics on Ω-logic, fine structure, recursive enumerability and effective randomness. Program visitors will also give seminars on recent results and news related to the core themes of the programs.

Asian Mathematical Conference 2005
(20 July - 23 July 2005)
Venue: National University of Singapore

Website: http://ww1.math.nus.edu.sg/AMC/index.htm

This is jointly organized with the Department of Mathematics, Department of Statistics and Applied Probability, Singapore Mathematical Society (SMS) and South East Asian Mathematical Society (SEAMS). It has an International Scientific Committee chaired by Kenji Ueno (Kyoto University), a Steering Committee chaired by Eng Chye Tan (National University of Singapore) and an Organizing Committee chaired by Eng Chye Tan.

Semidefinite Programming and its Applications
(15 December 2005 – 31 January 2006)

Website: http://www.ims.nus.edu.sg/Programs/semidefinite/

Chair:
Michael J. Todd, *Cornell University*
Co-chairs:
Kim-Chuan Toh, *National University of Singapore*
Jie Sun, *National University of Singapore*

Semidefinite programming (SDP) problems are linear optimization problems over the cone of positive semidefinite symmetric matrices. SDP has applications in engineering; in particular, in systems and control, structural optimization and signal processing, and more recently, in NP-hard combinatorial optimization problems and in polynomial programming. The widespread applications of SDP have led to great demands on quality solvers for SDP, especially solvers for large-scale problems. The program workshop will provide a forum for the exchange of ideas between researchers working on SDP applications and those working on algorithms and software development.

Activities:
(a) Tutorial (9 - 10 January 2006)
(b) Workshop (11 - 13 January 2006)

Mathematical Conversations

Wilfrid Kendall : Dancing with Randomness >>>

Interview of Wilfrid Kendall by Y.K. Leong

Wilfrid Kendall followed in the scientific footsteps of a distinguished father (probabilist and statistician, David Kendall) and established himself as a well-known expert in probability theory who has made significant and wide-ranging contributions to random processes, stochastic geometry, stochastic calculus and perfect simulation. His interest in the use of computers in teaching and research has also led him to develop computer algebra software in statistics and probability.

He is on the editorial boards of numerous leading mathematics and scientific journals, among them *Annals in Probability*, *Statistics and Computing*, and the *London Mathematical Society Journal of Computation and Mathematics*. He has been invited to give lectures at major scientific meetings and conferences, and has served on the committees of international scientific organizations. He is a professor in the Department of Statistics of the University of Warwick.

He was the Chair of the Organizing Committee of the Institute's program on Markov Chain Monte Carlo (MCMC) held in March 2004. The Editor of *Imprints* interviewed him at the Institute on 17 March 2004. The following are edited and vetted excerpts of the interview, in which he talks about the early formation of his career interest, the role of randomness in computer simulation, the close connection between probability and statistics and his views about the place of computers in statistics and intellectual thought.

Imprints: I'd like to thank you for giving us this interview in spite of your busy schedule. Where did you do your PhD and what was it on?

Wilfrid Kendall: I got my PhD, or DPhil as it is called there, at Oxford. I was an undergraduate at Queens's and then a graduate student at Linacre College. My thesis ended up with the title "Three problems in probability theory", which was very naughty of me because I had been told that the PhD title should be informative about what the PhD is about. But I was so anxious to get it submitted that I forgot all about the title until the last minute. There were, of course, three problems in the thesis. One was to do with early work on the knotting of Brownian motion, one concerned contours in random fields and one related to work I had done with my father on the statistics of shape. They were probability or statistics topics and they all had some kind of geometry attached to them, which has continued a fairly common theme in all the work I have done. My supervisor was John Kingman. He, in fact, was almost supervised by my father. Well, he was supervised by my father, but he never got around to submitting his PhD – never needed to! So my father is also my academic grandfather except for that small technicality.

Wilfrid Kendall

I: Your father is a famous probabilist and statistician. How much were you influenced by your father?

K: It's a very interesting question to me. In one sense, enormously - the fact that he was a working mathematician, that research was clearly exciting and interesting for him. That had a great influence on me. On the other hand, he was very wise, and he knew then what I know now; that if you are following in your father's footsteps, then it can be a difficult path sometimes. And so he never pressed me at all. Occasionally he would tell me a little bit about mathematics but I never felt any compulsion from him to do mathematics or statistics. It was all a choice of my own free will. In fact, my free will was really well informed! At the time when I came to choose my subject for a PhD, my tutor at Oxford, whom I admire very much, warned me that it could be difficult to follow in one's father's footsteps. He gave me very sound, very helpful advice, and he said afterwards that I had listened to him very politely and then I went away and did just what I was going to do before. But I really did take what he said very seriously. However I found I hugely enjoyed doing not just mathematics but also probability and statistics. So I was led that way. I was doing it because it was interesting and engrossing. I didn't want to do it because it was something my father had done. I would be quite strong on that point to anyone in the same position as myself. You really must be sure that you are doing what you want to do because inevitably there are going to be times when it is difficult, and then you'll need to know that you made your own choice for yourself.

I: What is the difference between applied probability and statistics?

K: That's a tricky question! It's like asking what's the difference between strawberry and cream. They are very close, and it's really nice to have both of them together. In

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statistics, the questions are different: you are saying that there are things you want to know about, so you estimate and you test your hypotheses and so on. In probability, you are saying, "The system is behaving randomly and I want to know how it's going to behave." It's a different kind of question. It's going the other way, if you like. There is not a clear cut line of division.

I: What are some of the hottest topics or developments in applied probability?

K: I asked people at lunch about this question and we all agreed that this is a very hard question! Certainly I can give a personal answer: what is hot for me is all the things that I like doing at the moment. The whole interface between probability and computing is very interesting. What we are doing now (in the Institute's program) is only a small part of that. There's a lot going on. Some of the work going on in random matrices is absolutely brilliant, and there's some lovely stuff to do with percolation theory. It is a difficult question to answer, particularly about applied probability. Some parts of science, and even of mathematics, are like a huge factory. You just have one or two products, and everybody involved is somehow working on the same products. It may take a long time before they eventually produce something really big. There are other parts of mathematics, and probability is one of these parts, which are extremely creative and vigorous, but there is no great master plan to which everybody tries to contribute a little. Instead, it's a very rich and fertile field and there are lots of different problems coming up all the time with a lot of premium on being original and trying to find your own way to do things. Temperamentally, I find that much more exciting. But it's difficult to say what the hottest development is in probability. You can say what you like to do right now but it's probably unwise and counterproductive to try to have much influence on what everybody else chooses to do.

I: Or shall we say, what are the central problems in applied probability?

K: Well, I think there are central problems that people are looking at and getting intrigued by. I'm not sure if you should talk about probability problems. They typically are problems to do with mathematical science generally. For example, at the moment some of my friends are extremely interested in random matrix theory because they think it might have something to do with the Riemann Hypothesis. Sometimes people think there is something there, and sometimes people think it's a mad dream. But it is interesting in its own right. There are other questions which have really been there a long time in statistical mechanics – whether there is some universal structure hiding behind things like percolation. There are people who have done some exciting work related to that. There are certainly big questions that people would like to think about. But I think that it's true generally that's what makes probability an attractive and vigorous subject, why a lot of people are attracted to it; there are lots of things to do and they are all very interesting. No one can quite tell what will be the next new development.

I: Could you tell us how the term "Markov Chain Monte Carlo" came about?

K: Monte Carlo refers to the process where you want to calculate something and it may be too difficult to do either by hand or by using a computer to find the integral directly, and you try to do it instead by doing a random experiment, which involves the probability of interest. It actually goes back a long way – the famous Buffon needle problem. You drop a needle onto a lattice of lines. Find the probability of overlapping the lattice. (Clue: it is related to pi.) But Monte Carlo itself is a term coined probably during the war because of computational demands in the development of the atomic bomb. Why Monte Carlo? Well, because the method had to do with the roulette wheel and randomness. Markov chain Monte Carlo is a particular way of doing Monte Carlo. If you like, you could read it as "Monte Carlo with Markov chains". So when you are doing these random experiments, the question is how are you going to do the randomness? For example, like tossing a dice, tossing a coin, or running a roulette wheel. You may do it indirectly, you may say let's build a stochastic system, a Markov chain, and let's design it so that it has an equilibrium distribution which is what we are interested in. Then you run it for a long time and you observe the outcome and that gives you a way of handling the calculations. The adjective "Markov chain" describes a way of doing a Monte Carlo.

This idea goes back a long way, one of the first ideas people were using. There are many complicated problems for which the quickest approach is to relate them to the probability of long-run behavior of Markov chains. There was a very famous paper by Metropolis and others which goes back to the 1950s, but almost certainly they were doing a lot before that. The physicists who have a lot of money to buy big computers have always been interested in computing and developed it. Relatively recently, statisticians started to persuade people to buy them computers too. And the computers got flown in and sit on the statisticians' desks. At that stage, a large number of statisticians started to get involved using computers. Once they have the computing power, then it started to become a more feasible way to solve problems. It is pretty effective and has a tremendous influence upon the way people are doing statistics now.

I: How much of the new developments in probability and statistics have been dictated or influenced by the advances in computer technology?

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K: I think, a huge amount. Here is a very simple example: the sort of questions that I used to mark for undergraduate examination papers when I started lecturing have largely gone out of fashion because they had to do with hand calculations but now you simply use a statistical package. I think that had a very big influence on the sort of things one does because some things have become very easy. One no longer thinks about them. But then, that means you can pose much harder questions. Markov chain Monte Carlo is another kind of example; computations that would have been inconceivable without accurate computing power. And then there are other applications, not really applications, but problems stimulated by the presence of computers and computation. You get interested in different kinds of questions. Back at Warwick I have a number of people I like to spend time to talk to – many of them are in the statistics department, many in the mathematics department, and also a significant number in computer science because probability is now important if you want to understand how to analyze the behavior of computer algorithms.

I: What about the theoretical aspects? The computer is good for computations, but will it have any influence on the theoretical development in probability?

K: As soon as you know how to do something, that there is a possibility of an answer, then your theory changes because your theory is about how you do things and you have just acquired a whole new way of doing things. That means you need a whole new theory. You can trace that all through statistics. What people are interested in theoretically is very often driven by the things that they can already do on the computer, which suggests theoretical questions. And then people on the theoretical side are motivated to do new things.

I: There seems to be a prevalent faith in some kind of order underlying every random, if not chaotic, behavior. Do you see this as a new paradigm in science or even in mathematics?

K: It's a very old paradigm. For example, in the book of Genesis, God builds order out of chaos. I think the idea of order coming out of chaos is not particularly new! Indeed, the mathematical notion of chaos can be viewed as saying there is a randomness in the choice of initial conditions right at the beginning, but you only see it bit by bit as the system evolves. I don't think there is any real conflict between randomness and systems with a great deal of order. Adrian Smith once said that probability is about what you don't know. You make probabilistic statements about the things you don't know are happening. It is perfectly compatible with ordered complex systems. Some things you don't know about, maybe you'll find out bit by bit as the system evolves. You can even use probability to do it. In fact, we had a conference in Durham in the summer which was to do with Markov chains in the overlap in between many different areas. And one of the things that was very interesting to see is that the group of people using Markov chain Monte Carlo in statistics were often working towards the same end as people who study deterministic systems with no randomness whatsoever and who are finding that the theory of Markov chains is a good way to describe how the initial conditions propagate through the system.

I: Does probabilistic modeling require the design of a "perfect" random number generator or some similar "random process generator"? Is that achievable in practice?

K: The answer to the first part is "no", and the answer to the second is "probably not". Practically, what you need is something which generates random numbers which are good enough. You don't want a number generator that produces a periodic sequence 0, 1, 0, 1, …. That's not good enough. How good is "good" enough? It's good enough if it does what you want it to do. If it has done its job, then it's good enough. A lot of work goes into the design of an arithmetical random number generator. From time to time, it gets replaced by one that is thought to be better and sometimes one can indicate rigorously how good the properties of these random number generators are. Indeed we have just had an example in the workshop: someone was talking about the case where you can show, a bad choice of random number generator leads to errors in certain complicated Markov chain Monte Carlo calculations. So you have to be careful. There is no replacement for the computer in your head. You've got to think about these things.

Suppose you want to produce a perfect random generator. Maybe you go to quantum theory, but there are all sorts of ways that things can go wrong. For example, suppose you built it wrong. That's embarrassingly easy to do if it's of complicated design. I recall a friend of mine who tried to build random number generators using thermal noise. He said that it was going to be perfect. He set up the stuff which electronically converted the thermal noise into noughts and ones, and it had a subtle correlation in it. He showed it to me and we agreed "It's wrong. There is not enough random deviation." Eventually he traced the problem to some subtle kind of electrical feedback.

This morning, somebody was talking about the design of generators of random bits based on a Geiger counter but they failed to take into account the fact that the Geiger counter worked better at higher voltages and there was a 24-hour period fluctuation in the voltage supply to the Geiger counter. So in a technical sense it wasn't doing the job it set out to do, producing more random bits at some times than at other times. You have to realize that in the

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black box you are using to produce a string of random numbers, there's probably going to be some factor there which you can't quite control and which you might have left out. When you take that perspective, then it doesn't seem so crazy, on the other hand, to use what we call a pseudo-random number generator using an arithmetical sequence because at least, you understand the properties of that. One of the criteria in the practice of random number generation is that you should prefer a random number generator whose defects you know to one whose defects you don't know. There is a nice quote about this. John von Neumann said back in the fifties, "Anyone attempting to generate random numbers by deterministic means is, of course, living in a state of sin." You have to do it, you are using a random number generator, at the back of your mind there may be something wrong with the generator, or maybe it's something wrong with the way you code the thing. One of us was just estimating coding error probabilities this morning. He reckons that the programs he writes have a 40 percent chance of being wrong in a first working draft. For my programs the chance is probably higher. Once you take that into account, you start looking for the bugs you *know* must be there!

I: Are there any limits to the levels of computer simulation? Do you think that computer simulation can shed some light on some of the mysteries of life such as the way the brain functions or even the origin of life itself?

K: The answer to the first question is: "Yes, there are limits". The answer to the second question is clearly yes and clearly no. The first question is interesting. My friends in computer science tell me about some very interesting theorems which show that there are practical limits to what we can do with computer simulation and which are related to algorithmic limitations to do with the phrase "NP-complete". You are looking at a world of problems of scale. In other words, when you double the size of the problem, does the work you do double or quadruple or worse ... or much, much worse ... and hence you can derive notions of hierarchies of difficulty of algorithmic problems. You can get the same sort of hierarchy for problems to do with computer simulation. So there appear to be logical limits as to how much can be done with computer simulation.

Now to your second question. Science certainly can shed much light on amazing things. Everyday, for example, I read about new progress in understanding and control of diabetes. On the other hand, you just have to look into the eyes of a new-born baby to realize that there is something about which science remains silent. If, by the mysteries of life you mean something like that, the answer is: No.

I: Could you tell us something about the role and position of computers in mathematics education at your university?

K: Our department was one of the early UK statistics departments to use the computer in a big way in teaching statistics, so we were early starters. At Warwick, we have a center which tries to encourage innovation in the use of computing and it has taken on a very practical strategy. It recognizes that there are people using computers in all sorts of different ways across the university. It produces a newsletter which reports on these ways. It encourages people to experiment a bit and to report what is useful. Now, for example, whenever I give a talk or a course, I make sure that my lectures have notes on the web which are highly hypertexted so that they have all sorts of links in them. Increasingly, people say they like them and find these helpful. But I think that, while innovation and experimentation are good things, it's important always to bear in mind that actually education is ultimately about what is going on in people's heads.

I: Is it compulsory for Warwick statistics students to do some computer programming?

K: As a matter of fact, it is. Our students are all exposed to a course using the computing package Mathematica. But the point I'm making is that in the end what matters is when people walk out of the classroom or computer room, have they changed their way of thinking? Have they actually learned anything? You don't need a computer to make a difference to that and sometimes the best thing we can do to help people learn is to put the computer in a quiet corner of the room and switch it off. What matters is what's going on in people's heads.

I find the computer a great aid in making illustrative material available to students when I talk on some topics. It makes a tremendous difference if they can actually learn how to do things and see them afterwards. However it's important not to get lost in all those technology.

We teach our students to use computer packages rather than programming as in such flexible packages you can learn how to program. We don't teach them programming as a primary activity. Typically, when they come out into the world, what they need to know is how to use the computer as a tool. That is clearly the way things are progressing. Programming is done by some people but what is most important is for people to know how to develop the qualities of systematic thought and care that are required for programming.

I: Do you have any connections with the Warwick mathematics department?

K: Yes, I have a lot of friends there. In particular the Warwick probabilists are almost equally divided between mathematics and statistics. Probability is at the boundary and it is a good and interesting place to be in.

Lawrence Klein : Economist for all Seasons >>>

Interview of Lawrence Klein by Y.K. Leong and K.S. Tan

Lawrence Klein

Lawrence Klein is a pioneer in the creation of computer models for econometrics and economic forecasting using mathematical techniques. From the formative years of his education at University of California at Berkeley and at MIT during the early war years, he moved to the Cowles Commission for Economic Research (then at University of Chicago) where he formulated a model of the United States economy and predicted an economic upturn after the war. He is well-known for the enhanced economic model called the Klein-Goldberger model and for the famous "Wharton Econometric Forecasting Model". He has built economic models of the United Kingdom, Canada, Japan and other developed and developing countries. He has served as a consultant to the governments of many countries; in particular, to China as it opened up to the west. The impact of his work on modern economics and his influence on present day economists are well recognized. He was awarded the Nobel Prize for economic science in 1980.

He was president of the Econometric Society and of the American Economic Association in the late sixties, and founded Wharton Econometric Forecasting Associates (now Global Insight) in the sixties. In the seventies, he started "Project LINK" to connect the models of some international countries in one of the first attempts to produce a "world economy" model. Besides being a practitioner, he is a scholar who has written extensively on econometric and economic models.

He joined the Department of Economics at University of Pennsylvania in 1958 and became the Benjamin Franklin Professor of Economics and Finance at its Wharton School of Business in1968. He is now an emeritus professor at Pennsylvania and continues to be active in research and consultation.

He was a key speaker at the program organized jointly by IMS and the School of Economics and Social Sciences of SMU in April and May 2004 on econometric forecasting and high-frequency data analysis. The Editor (Y.K. Leong) of *Imprints* and Kim Song Tan of SMU interviewed him on 6 May 2004 at the Institute. In the following edited and vetted excerpts of the interview, the 84-year-old distinguished economist talks about his early university education, the scientific challenges of economics and a life-long dedication to the application of economics for the welfare of humanity.

Imprints: You mentioned in your Nobel Prize autobiography that you were attracted to mathematics and economics when you were in college. What made you decide to do your PhD in economics rather than in mathematics?

Lawrence Klein: For one thing, I started thinking about a long-term career as I entered college. I thought first about being an economist, and I stayed with that. I thought of mathematics as a tool to gain better understanding of economics. Also, when I was very early in university, we used to go out in teams to different colleges in the area to participate in mathematics competitions. I decided that I wasn't really going to be good enough a mathematician to win those competitions and that there were young people of my age who were better mathematicians. So I stayed with economics.

I: Was your early mathematical training sufficient for your later work in economics or did you pick up most of the required mathematical methods as you went along in your research?

K: As an undergraduate in university, I had about half my classes in mathematics and about half in economics. But when I went to graduate school, I went further in mathematics. It was a very good graduate school at MIT and the mathematics was very good. So I picked up more mathematics and this was a period when mathematics was just beginning to be used in economics on a bigger scale, while most of the early work followed what we might call classical methods. When John von Neumann and Oscar Morgenstern introduced the theory of games, one had to go to set theory and other kinds of mathematical reasoning. I made the shift. And a lot of work dealing with dynamic systems in economics requires stochastic studies of dynamics that involves stability properties of systems and differential equations of more complicated sorts; so that I had to, for some time, keep studying mathematics. But then I got more and more involved in the applications of mathematics to economics and in the applications of economics to real world problems. Gradually over the years, mathematics got more and more complicated and deeper for economics and professionals. So I didn't keep up studying mathematics endlessly. I shifted more towards doing things with economics and the mathematical basics that I had already started.

I: Do you think that mathematical rigor is necessary for economic training?

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K: Rigor is important. You could be wrong, thinking you are right by not being quite rigorous and finding that there are exceptions and things that you have missed. I think what one really wants is imagination above rigor. And then you go to your friends who are mathematicians and check to see if your imagination and intuition took you in the right direction.

K.S. Tan: In that context, how do you find the current trend in economical study which a lot of people complain is becoming too mathematical and not relevant or practical enough?

K: I won't say it's too mathematical but it's often too abstract. I think that some of the theoretical work in mathematical economics has drifted away from the important problems.

T: Do you see that as a potential problem in the sense that those who are in the university teaching economics are so well trained in mathematics that they feel compelled to continue along this path and go further and further away from the real problems that they might see?

K: They really do, but, of course, we have an important obligation to teach and give students ideas about economics. We should keep in mind that we are doing economics and not pure mathematics.

I: How much were you influenced by the style and philosophy of Paul Samuelson in economics?

K: That was important for me. When I was an undergraduate, I independently had the interest in finding how mathematics could be used in economics as a tool. One day I went to the library at the University of California and was thumbing through issues of "Econometrica" when I found in early issues (just beyond the first decade of the journal) articles by Paul Samuelson. I was so impressed by them that when I had a chance to go study under him at MIT, I realized that he was at that time, and still is, the greatest American economist of that period.

T: There are also people who say that people like you, Samuelson, Solow in your generation, are really great thinkers, not just economists, whereas the current crop of economists are just economists, technicians. Do you buy that argument?

K: The people you mentioned and some others like them are interested in mathematics to deal with the problems of the world we live in and to quantitative economics as a tool, but there are many people today who get away from that concept.

I: It seems that economics is, in some sense, observational and empirical and yet one cannot conduct controlled "economic experiments" in the sense of experiments in the physical sciences. Does it then make sense to talk about "economic truths"? If it does, are there objective "laws" in economics in the sense of scientific laws in physics or chemistry or even biology?

K: Of course, physics, chemistry and biology are very different. The broad concepts of science include some sciences that are very respectable but have no controlled experiments: meteorology (it's not an experimental science), seismology (it's hardly an experimental science) and astronomy. Yet they go far with mathematics. Apart from meteorology and seismology, astronomy is *very* precise. Now, in addition to controlled experiments, which are important, the defining thing is the ratio of noise to signal. In astronomy the noise to signal ratio is very low. Meteorologists have gone very far, but if you judge meteorology by looking ahead as far as one month or more, the findings don't look impressive. But if you judge meteorology by the next minute, the next day, or two days, it looks impressive and it's getting better. Economists should follow some of the techniques that meteorologists use. They tie in to the computer much more intensively. They send balloons into the atmosphere, fly aeroplanes through the hurricane's eye and learn more. We don't get enough of that extreme information flow in economics. Seismology understands what happens during an earthquake but they don't understand how to control or predict it. Now to some extent people are trying to introduce controlled experiments in economics. I often thought about that issue, say, like going to an institution such as a prison, change the economic values and look at the outcomes. It's possible to have some experimentation in economics. Nobel Prizes were awarded to some economists who did experimental work and went further than collection of data. But, by and large, economics is not an experimental science, and we must try to do the best we can with that limitation. The lecture I have just given was an attempt to show how we might improve our ability to forecast the economy by small steps even though we can't experiment.

I: Who were the economists who were given Nobel Prizes for their experiments?

K: Vernon Smith of George Mason University. He shared the prize with Daniel Kahneman of Princeton.
[*The citation for Smith reads: "for having established laboratory experiments as a tool in empirical economic analysis, especially in the study of alternative market mechanisms". - Imprints*]

I: Economic phenomena appear to be governed by random decisions in human behavior in a way not unlike those encountered in the history of mankind. Yet economics, but

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not history, has been hugely successful in making economics into a science. Is this due to a gigantic leap of faith on the part of economists in the methodology of mathematics?

K: In the work that I do in economics and econometrics, when something very big happens, like the OPEC decision in1973 to limit oil production, to limit oil exports, to raise the price of oil four-fold or eight-fold, it is almost an arbitrary decision, unexpected. Now I say we cannot predict that OPEC would do that, but once OPEC has done that we can predict the outcome. I think we did very well with that. A number of the predictions that I have been involved with were of that sort. During the closing days of the Second World War, I shifted from MIT and was asked to help build a model to predict whether the United States will revert back to the Great Depression as soon as demobilization and peace were achieved. We made such a prediction. It was against almost everyone else's view, and it turned out to be right: America *would not* go back to the times of the Great Depression. And there have been similar times - after the Korean War, the Vietnam War, and even now, the present war. So we say we can't do a really good job in predicting those events but when those events have occurred we can do a reasonably good job in judging the outcome.

I: Are there such things as economic laws? Do economic laws exist?

K: I wrote a paper once on "Some Laws of Economics". One of the interesting laws I looked at from time to time is called "Engel's Law", which he (Engel) found by studying social groupings of people in Europe – the percentage of a family's income spent on food declines as income rises. It may be a fairly weak law, but it holds. At the 100th anniversary of Engel's Law, the econometrician Professor Houthakker wrote a paper surveying countries all over the world to see if Engel's Law held. And then there was a very important event. When it came to China, he said he couldn't get data from modern China (that is to say, the beginning of the communist regime in China) but he found some Chinese family budgets from around 1920, or so, like the ones Engel found, and he said, "Yes, Engel's Law held." I was fascinated by those remarks. When I went to China for the first time in 1979, I got hold of a paper by a Chinese American economist surveying consumers in Tianjin and he got almost the same coefficient that Houthakker had found from the twenties in China for Engel's Law. So that particular observation by Engel had great longevity. There are many others that I cited like that. They don't give you enough information to know as much as you want to know about the economy. They don't cover a big enough part of it. There are some laws like that which have held up through centuries or decades.

I: When was Engel's Law formulated?

K: In the 19th Century, in 1857. The people in the sample were Belgians, and Engel (Ernst Engel) was German or Prussian.

I: To what extent do the non-quantifiable elements of politics and culture contribute to the economic modeling of a country?

K: They contribute a lot. My last example in the lecture that I just gave was the use of sample surveys, not completely non-quantifiable, but not very quantifiable, of people's attitudes after the attack on the World Trade Center. The way we use surveys for consumers: to determine if you were better off, worse off (on a 5-point scale), much better, much worse or about the same. Let's say there is limited quantifiability. We found these very important in giving us guidelines on what consumers were going to do after that big event. There are many such things like that, and I claim that it's important to study subjective attitudes in decision making, political structure, and legal structure of politics and culture. Yes, they contribute. We should be aware of them and we should take them into account to the extent possible. Sometimes that extent possible can be stretched because we learn new methods of finding out about political and cultural events.

I: You constructed economic models for several countries like United States, Japan, Canada, United Kingdom and others. Were these models used by the various governments in planning their national economic policies? Have you done any economic model for a developing country?

K: The answer to the first question about whether they were used: yes; many governments have used these models or models evolved from them. I don't think you should use the word "planning"; I think it's in formulating their economic policies, such as interest rate policy or tax policy. It is not planning but it is using the models for doing the government's work and definitely used in that respect.

Any economic models for developing countries? Yes, for many. Right now, I have been involved, for a number of years, in modeling of China and I'm working on models for Russia. Russia is a transition country moving from planned economy to market economy. I have worked on Mexican models a great deal. I've helped a lot with different African models and different Asian country models.

I: Do you think that there is still a gap in communication, if not in interaction, between the majority of economists and the majority of mathematicians?

K: There are definitely gaps. If mathematicians are broad-minded enough and the economists are patient and careful enough, we still communicate quite well. But at the

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extremes, there have been major debates in our National Academy of Sciences whether the social sciences (that includes not only economics but also political science, sociology, anthropology) - whether some of them - should be included in the National Academy of Sciences. They have been there for a long time, but the academy had to make a conscious effort starting in the sixties or seventies to open up class groupings for social scientists, and some mathematicians have been very harsh in complaining about that. There were fights in the academy with mathematical members over the election of some social scientists. There was a very big fight at the Institute for Advanced Study in Princeton, over the hiring of a sociologist, by some mathematicians at the Institute. So these things happen.

I: Do you think that a mature mathematician could learn enough economics to make a non-trivial contribution to economics or do you think that he or she should possess some innate "economic acumen or intuition" in order to do so?

K: I don't know if it should be innate but I think that if mathematicians want to comment on the role of economics in social and political life, or economics as a social scientific discipline, then they should learn something about the way the economy functions and the way economic decisions are made, and then there will be better communication.

I: I will wrap up my questions with one question that is quite philosophical. You have dedicated your life to creating and developing a whole generation of economic models. Other than the Nobel Prize, what is the greatest satisfaction that your life-long work has given you?

K: Well, to see the models used. For me, one of the great things was that when I started in the faculty in the University of Pennsylvania, we knew the models and the application of models by the business and the public and government communities, and in doing so, we raised enough financial support so that over the years, between 10 and 15 PhD students every year were being supported by us. We paid their university fees and living expenses, and now they have gone out into the world and many have been very successful. That gives me a lot of satisfaction. We were able to use our approach to apply economics using mathematical, statistical, numerical methods to support enough students so that they have successful careers.

T: In your view, do you think that economic theorizing has reached a fairly mature stage, or do you expect to see another revolution coming in the same way that the Keynesian revolution, rational expectations, real equilibrium studies changed the thinking of economists?

K: I think that there is plenty of open room for creative thinking of a major sort to come and I think that the Keynesian revolution was very important. I think Leontief's work put activity up a bit; that is very important; and Samuelson's work was partly Keynesian. There were many others like that. But I feel that some of the work being done now is not getting far. I think that economists accept "rational expectations" as though it is realistic and correct; it is a hypothesis and I don't think it has been validated. There are others. I don't see a big event or a big change in the way of thinking among the most modern branches of economics that have the same impact as the Keynesian revolution had.

I think the information technology revolution had a very big impact, certainly a very big impact on what I do. It's not an economic theory, but it enables us to judge economic theory and principles much better. That was the basis of the lecture that I just gave. How can we improve economic forecasting by drawing upon the computer, the flow of information, the dissemination of information and the dissemination of policy preferences? There may also be a breakthrough, eventually, using the new kinds of techniques and facilities in the same way that I claim that meteorologists have definitely added, one day, two days, sometimes just half an hour, to the validity of meteorological forecasts. It helps utilities, helps the airlines, helps state planning. The economist will use the same information facilities to develop more accurate judgments and predictions.

T: You were involved over the years in work on China. From our understanding of economics, do you think that China's becoming a super economic power on par with America is a certainty? Can we say that it is a certainty that the Chinese economy will be a super economic power equal to the US?

K: I think you can say that China's catching up to the United States by aiming at a moving target would be unusual. I don't see China overtaking on a per capita basis eventually. I won't say it's impossible but it's not my judgment. On the other hand, China's present projection, I regard as plausible. China's leaders say that since reform (since 1978) China more than quadrupled in GDP by 1980. The new target is to quadruple again between 2000 and 2020. In looking at that, I'd say there's an excellent chance of doubling by 2010. I don't say they won't double between 2010 and 2020, but they will have to work harder than they have. I've been combing records. No country has had 40 years of that size growth in terms of established statistics. One reason why I think the decade will show whether it is favorable for China's plan is the preparation for the Olympics in 2008 and the preparation for World Expo in 2010. I think that those are going to keep China very busy providing the infrastructure and facilities for those major events, and China wants to show the world what she can do in those events that will give China the opportunity.

Robert Engle: Archway to Nobel >>>

Interview of Robert Engle by Y.K. Leong and K.S. Tan

Robert Engle started his university education as a physicist at Williams College and Cornell University but switched to economics for his PhD at Cornell, specializing in the use of time series in econometric analysis. In 1982, he formulated a model, known as an ARCH (acronym for "autoregressive conditional heteroskedacity") model, to study time-varying volatility in inflation. Soon afterwards, it was realized that his model could be applied to financial econometrics. In subsequent work and in collaboration with others, he extended his model to the so-called GARCH (generalized ARCH) and GARCH-M models, and introduced fundamental concepts which have set new directions for modern econometrics. His ideas and techniques have become indispensable tools in risk management in the financial sector. For his fundamental contributions, he was awarded in 2003 the Nobel Prize in economic science with his collaborator Clive Granger.

Engle taught at MIT and University of California at San Diego (UCSD), and in 2000 joined the Stern School of Business at New York University, where he is now the Michael Armellino Professor of the Management of Financial Services. He is active both in academic research and in consultancy work for financial institutions. He is a member of the American Academy of Arts and Sciences and a Fellow of the Econometric Society and of the American Statistical Association. He has given prestigious lectures like the Fisher-Schultz lecture, the William Phillips lecture, the Pareto lecture, the Frank Paish lecture, the Journal of Applied Econometrics Lectures and the first Econometric Institute/Princeton University Press Lectures at Erasmus University.

He was a key speaker at the program organized jointly by IMS and the School of Economics and Social Sciences of SMU in April and May 2004 on econometric forecasting and high-frequency data analysis. The Editor (Y.K. Leong) of *Imprints* and Kim Song Tan of SMU interviewed him on 10 May 2004 at SMU. In the following edited and vetted excerpts of the interview, Engle talks about his intellectual passage from the sequestered "basement realm" of superconductivity to the gregarious, if not glamorous, world of economics and finance, how the seeds of his Nobel Prize winning work were planted and his views on academic research and consultancy work.

Imprints: Thank you, Professor Engle, for kindly agreeing to be interviewed by us. Your bachelor and masters degrees were in physics. What made you switch to economics for your PhD degree?

Robert Engle: I went to graduate school in physics without being sure that I wanted to continue in physics. I've always loved physics but after I started my graduate work in the basement of the physics building studying superconductivity, I decided that I didn't really want to spend my life doing research on a topic which only a handful of people would ever understand. So I went to talk to people in the economics department because economics is the most quantitative of the social sciences and I thought that there was a possibility of doing something useful and interesting for a large number of people. To my amazement, they were interested in having me switch. And so I did. That was in Cornell.

Robert Engle *Courtesy School of Economics and Social Sciences, SMU*

I: Did your doctorate work set the direction for your later ground-breaking work in econometrics?

E: There were connections. My doctoral work was in time series and some of the mathematics I learned in physics was involved with spectral representations and things like that. That was carried forward into my thesis. The work on the ARCH model was rather different although it's still time series. It was about second moment properties rather than first moment properties. It was a different class of models, but there is a relationship.

I: What led you into formulating the innovative ARCH model?

E: I was on sabbatical at the London School of Economics at that time. I was interested in a question that Milton Friedman had posed. That was a macroeconomic question. He said that he thought that the cause of business cycles was not just the level of inflation but the uncertainty of inflation. The argument is that businesses try to invest in the future. If they don't know what the price level or wage level is going to be (and there's a lot of uncertainty about it) they are likely to withhold their investments. So that will lead to a downturn in the economy. If that is really the case, then you will expect to see the uncertainty of inflation forecast changing over time and being correlated with business cycles. So that was the question I was trying to solve.

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I always say that there are three inputs to the ARCH model. I brought two ideas from time series. I had done a lot of work on Kalman filtering and using predictive densities to write likelihood functions. The third input was that Clive Granger, my long-time collaborator and friend with whom I shared the Nobel Prize, had just proposed a test for a bilinear process which is a type of time series model that involves looking at the correlations of the squares of the residuals of an econometric model. One day I was on the computer and Clive came by and said, "Let's take a look at your residuals, square them, fit an autoregression." And lo and behold, that was very significant, and I said, "Wow, isn't that interesting? The data really had evidence of this sort of thing in it." But I didn't really believe that it was evidence of a bilinear model. I thought that it was evidence of something else – I didn't know what. It turned out that if I were working with this data evidence, I was able to come up with a model which could be used for convoluting volatilities to answer the Friedman hypothesis.

I: Did your physics training contribute towards some kind of insight?

E: I think my physics training was particularly important in the relationship between theory and evidence. Sometimes it starts with a theoretical hypothesis and then you look for empirical evidence. Sometimes there is empirical evidence first and the theorist looks for a model that works. I feel that whichever way it happens, that's the role the econometrician takes. He is the person who really must strive to relate the data that we see for the economy with the theoretical models to make it move. I think that econometrics is a natural way for a physicist to approach the world.

I: Would it be correct to view your ARCH model as the mother of all econometric models? In retrospect, are you surprised that it led to so many ramifications?

E: I don't think it's the mother of all econometric models. It's really the first model to be interested in volatility and it is the mother of all volatility models, but econometrics is much wider that that. So it's not at all the mother of all econometric models. I'm quite surprised how popular it was. I knew it was a good idea at that time but I thought that if econometrics is the size of a table, then the part that is interested in predicting volatility and uncertainty is only a small part of the table. But it has turned out to be very important for so many applications that are still growing.

I: Which is more important in creating models: technical mastery or intuition?

E: I think they are both important. I tend to try to prove theorems with my intuition before I get technical about them. They have to make sense to me how this could be true and then I say, "Ok, now, how can I prove it?" To me, the intuition comes first. But when I say the intuition, you have to have the technical skills to rewrite your intuition in such a way that it looks like you can understand where it fits. It's very hard to develop a new idea, because you can look at it in so many different ways. Unless you've got a wide technical background, you don't know how to begin proving the theorem. How do you phrase this theorem? You need a lot of technical background before you can even formulate the question. I'm better at intuition than I am at the technical details.

I: It seems that econometrics uses a lot of statistical theory and methods. Do you think that behind the algorithms and computations there are some fundamental economic concepts that could be subject to some kind of objective economic laws?

E: You know, when physicists talk about laws, they think about Newton or Einstein or something like that. These are inexorable laws. I don't think that there are going to be economic laws in that sense for economists because what we are looking at when we build models for the economy is the average behavior of a lot of people. By averaging you can get a lot closer to a law, but it isn't clear that it is amenable in the same way as physical laws are going to be, so I think probably not. We find general principles, tendencies and patterns that are preserved over time.

I: You mention principles, but a principle is some weak form of a law.

E: Yes, I suppose it is. When I said that, I wondered whether you would point that out. A lot of economic models are based on very strong optimizing results and general equilibrium results. Rational behavior gives you very strong hypotheses about how the world is going to be. Many of those are good descriptions of how you see behavior. So in a sense, I suppose you would think of those as economic laws but it's not that they explain things exactly. There's a lot of dynamics and adjustment that you have to make to the system that you see.

I: Modern physics deals with random behavior and so does econometrics. Do you think that there could be some physical analogies that may be useful in economics and econometrics? In particular, what are your views about quantum finance?

E: Well, I have not found that interesting – the finance theory that the physicists are doing – "econophysics", that's what I would call it. I think that it is, in an interesting way, mechanical. It tries to apply mechanical principles to economic systems and doesn't recognize that there is behavior and that it is not actually a physical model. These

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are agents with dual optimizing and behaving in ways that atoms and molecules don't do. So I think that while there may be interesting things that could come up out of this, I think it's not obvious that there's something very useful that physical principles can be applied to economics. I don't think that quantum mechanics has any direct implications for finance because quantum mechanics is a probabilistic statement about the future evolution of particles and atoms. It doesn't talk about the fact that in every price movement there is a buyer and a seller and somehow sellers and buyers have to agree to this kind of outcome. It isn't that one person can push the market without somebody agreeing to sell it to them. There is an optimizing character of the economy which is really not present in quantum physics.

I: You teach at the university and do research and at the same time run your own consultancy services for industry. How do you manage that?

E: I manage it by keeping them working together. So when I do my consultancy, I make sure that what I do in my consulting work is actually going to be an important part of my research, and I have had some wonderful problems that come out of consulting projects. I think that this is a way of keeping your research focused on problems which people are interested in. I think it's important to do that but I do not like doing my consulting on things that would never end up as part of my research.

I: Do you have students?

E: I have students. I am now in the finance department of NYU (New York University). I have some finance PhD students and I have some economics PhD students, and I still have some students from UCSD (University of California at San Diego) whom I'm working with. I have a range of students.

I: How has the Nobel Prize affected or influenced your life?

E: In a way it changes everything and, on the other hand, it doesn't change anything. I have lots of things which are different. The press was never interested in talking to me before that. Now I have lots of interviews with the news media. They wanted to know about things that I never thought I was expert in. But I ended up talking about them anyway. I'm now more of a generalist. I've met so many interesting people from different areas of science, economics and journalism and so forth. It's fascinating about the people you meet. I meet finance practitioners. I have an interesting experience that people like hedge fund traders and so forth tell me their strategies which nobody would want to reveal in the past.

I: It's their trade secrets or something.

E: That's right, trade secrets. I don't quite know what that is but I think it's got something to do with the Nobel Prize. In that sense, a lot of things have changed. In many ways, I do my best to keep my research and my life the same as before. I'm continuing to give talks and do my research and I think I don't want that to stop.

K.S. Tan: You were saying that after the Nobel Prize you are in a way forced to speak as a generalist in many contexts. Do you find you are more influential as a generalist in that context than as an econometrician?

E: Well, people always want to know some things like "Is the stock market going to go down?" I don't know whether that's being influential. I haven't actually taken on any causes. Sometimes Nobel Prize winners do say, "I want to do this thing." I haven't done that yet. It could happen. I think I reach a bigger audience because I'm speaking about more general things. My general comments would be about financial management and risk assessment and that sort of things. Now I end up talking about general macroeconomic issues in the US and international issues. In about five minutes, the BBC is going to broadcast whatever I said this morning on the BBC Asia Report.

T: The reason why I ask that question is because you must have heard many times that economists these days are so focused and so specialized in their fields that they cannot deal with larger economic or even for that matter political issues. Yet economics and econometrics are part of the social sciences. How do you respond to that?

E: Well, I think specialization is natural. It's a lot to ask people to be expert in a particular area and making innovations that are valuable to the profession and to people in that particular area and still be able to speak as a generalist. However, I think a lot of times making advances in a particular area is aided if you've got a little broader interest so that you can bring things from other disciplines. You can bring stuff from mathematics, from statistics and from other areas of economics to answer problems in your particular area. So I think that some amount of generalism is a good thing anyway, but it's a lot to expect anybody in any particular area to be able to comment widely on economic issues of the day.

T: That brings us another question. Do you think that when a student wants to do economics, he should be someone who has some interest in general social, economic phenomena first, or should he just approach economics as a form of science? I'm asking you this question because you came from physics, and yet you were able to deal with economic questions and issues. I don't know whether all

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econometric students are of this type today.

E: I think actually it's probably true they are not. It took me a long time. When I started graduate school as a student in economics, I could do problems that someone would set but I couldn't figure out what the problems should be, what should be an interesting problem. I think it took me probably ten years of my time teaching at MIT and so forth. I was continually trying to develop and understand this economic intuition that so many other people had taken so easily. But it was hard for me to grasp. So I don't know whether in the beginning you should expect that. But I think people should try and develop it. Of course, that's what graduate education is about. That's why you go to meetings and you listen to talks. You try to develop your economic intuition.

T: Would you go so far as to say that without it you would not be able to make it as a successful econometrician?

E: No, I don't think so. I think actually there are a lot of successful econometricians who are very narrow, technical people. They have to pick good problems. That's where you make your name. You solve a good problem and it's a kind of intuition which makes you choose the problem. I like to take problems from the world around me and figure out what actually is the nature of this problem and how you can solve it. But people who take problems from the current state of econometric research realize there is a problem here, they formulate it and they solve it. I think that's a valuable contribution.

T: A question on consultancy. Do you often find yourself in a conflicting position where the private sector tends to look for definitive answers to their questions and think there are some numerical answers to their questions and we know that it's not possible in all cases to provide this kind of answers. How do you deal with that?

E: I'm more of a tool builder. The ARCH model is a tool which allows you to study risk and a lot of consulting that I have done is not actually so much looking for answers as looking for tools. How do you build the tool that's good for measuring risk in this kind of setting? How do you build the tool that helps people form their portfolios? You build one and it helps a little bit but maybe not enough. So then there's another one you might want to develop along the way. It's not so much getting an answer. It's advancing our real understanding and our ability to solve these problems.

T: How do you find from your experience how useful econometric solutions are to hedge fund strategy or general financial trading strategy, treasury and other types of trading?

E: I'm not so involved in trading strategy. I've avoided that because that actually doesn't ever lead to publishable research. Either it works, in which case you can't publish it, or it doesn't work and nobody cares. Even if you do publish one that does work, no one will really believe you because then people will ask, "Why did you publish it?", and it goes away as soon as you publish it anyway. I have tried not to get involved in trading strategy. But if you talk about strategies like what is the best way to forecast risk or something like that. I think those are not proprietary typically. Maybe initially you wait a little bit before you put it in the academic discipline. I try not to get involved in things that have too much conflict. Another set of consulting that I did for a long time (although I'm not doing it any more) is energy research, electricity modeling. What is the demand for electricity at different times of the day, how do you forecast that, how does it depend on appliances and things like that. This is another example of how you develop statistical methodology. People build these models for utilities and forecast what the needs will be in the future. You know it's not proprietary. There's a lot of non-proprietary stuff you can do both in the financial sector and more broadly in the industrial sector.

T: A lot of people in the finance industry these days, especially when it comes to training, tend to be engineers by training. In fact, many of them have no economic qualifications whatsoever. They seem to be doing well and form a large group in the finance industry. Do you think that in that sense finance might be closer to physics than to economics?

E: I think that finance education is typically not as quantitative as what financial practitioners require. In financial practice you need to handle an enormous amount of data, a lot of computing tasks. Finance PhDs are often not that well trained in econometrics or in computer methods and they are often trained in particular corporate finance theory or something like that. I think that finance service sectors hire a lot of engineers, physicists, chemical engineers and mathematicians because they have skills that they need and they cannot get a finance person to do. I think that academic finance is not as close to practitioner finance as you might think. In fact, practitioner finance does have a lot of economics and econometrics in it.

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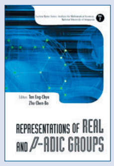

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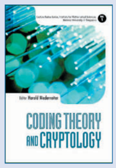

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