NUS
National University of Singapore

IMPRINTS

INSTITUTE FOR MATHEMATICAL SCIENCES

OCTOBER 2006

IMS Initiates Summer School Program >>>

A logical summer at IMS

Earlier this year, groups of eager budding mathematicians could be seen on the grounds of the Institute going to and from lectures, speaking with professors and talking excitedly about mathematics at the IMS lounge. They were the first ever groups of students to attend Summer Schools at the Institute for Mathematical Sciences.

The Summer Schools were jointly organized with the Department of Mathematics at the National University of Singapore. The Summer School in Combinatorics was held in the second half of May, in conjunction with the IMS program on "Random Graphs and Large-Scale Real-World Networks". Lectures were delivered by Paul Balister, Béla Bollobás, Svante Janson, Yuval Peres, Oliver Riordan, Devavrat Shah and Sanjay Shakkottai. Additional talks were arranged at the Departments of Mathematics and of Statistics and Applied Probability to introduce the participants of the Summer School – who came from the Philippines, Thailand, UK, USA, Vietnam as well as Singapore – to the departments and their research activities. The Summer School in Logic was a month-long program in July. Ted Slaman and Hugh Woodin of the University of California at Berkeley were the principal lecturers who delivered mini-courses in recursion theory and set theory respectively. The participants, hailing from China, USA and Singapore, contributed to the intense and active learning process by delivering lectures at a student-run seminar which met once a week. The Summer School was the first of a planned annual series under a tripartite cooperative arrangement among the logic and set theory groups at Berkeley, Chinese Academy of Sciences and NUS.

A number of favorable circumstances had arisen to enable and encourage the IMS to bring the idea of Summer Schools to fruition. As was reported in the last issue of *Imprints*, IMS participated in the founding of the Pacific Rim Mathematical Association (PRIMA), one of whose action points was to organize summer schools for graduate students in the Pacific Rim. The two summer schools mentioned above were included under PRIMA's coordinating umbrella. The Institute will continue to work with PRIMA on future summer schools for information dissemination as well as to tap on its network for possible exchanges. Funding for the summer schools came about when IMS and the Department of Mathematics agreed to cooperate. Additional partial support was obtained from the Faculty of Science.

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People in the News >>>

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Sharing logical insights

A determined group of random graph-ists

Organizing Summer Schools is a natural complement to existing activities of IMS and helps to further its objectives. The IMS, like the mathematical sciences community in general, is and must be globally oriented in its outlook. Nevertheless, it is cognizant of the opportunities and responsibilities it has towards the advancement of the mathematical sciences in the region. For its Summer School program, the Institute makes a special effort to identify and invite promising mathematics students from the region to participate and assists them with financial support. It is an important step towards promoting exchanges and strengthening cooperation in mathematical sciences in the region. An increase in the level of mathematical activities as a result of the summer schools will be of benefit to mathematical communities in the region as well as the world beyond.

More "Schools" are already being planned at IMS: a "Winter School" in January 2007 in conjunction with the IMS program on "Moving Interface Problems and Applications in Fluid Dynamics" and Summer Schools in 2007 and 2008 respectively as part of the IMS programs on "Braids" and "Imaging Science".

Denny H. Leung

Centennial Professorship for Director

Louis Chen, Director of IMS, has been named to a Tan Chin Tuan Centennial Professorship by the National University of Singapore. In celebration of its centennial, eight Centennial Professorships were created by NUS and endowed with donations from beneficiaries and matching government grants. Louis plans to use part of the research funds from his Centennial Professorship to partially support the Summer School program run jointly by IMS with the Department of Mathematics. Congratulations to Louis on the piece of good news for him and for IMS.

New IT Manager

Aung Naing Sunn, IT manager, left the Institute on 18 April 2008. Sunn, as he was known to all at IMS, had been with the Institute since 2001. He was the person who was primarily responsible for planning and setting up the IT infrastructure of IMS. We wish Sunn all the best in his future endeavors, and thank him in appreciation of the fine IT working environment that he had built up. Stephen Auyong joined the Institute on 28 June 2006 as its new IT manager.

Past Programs in Brief

Random Graphs and Large-Scale Real-World Networks

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

Chair
Béla Bollobás, *University of Memphis and Cambridge University*

Co-chairs
Khee-Meng Koh, *National University of Singapore*
Oliver Riordan, *Cambridge University*
Vikram Srinivasan, *National University of Singapore*
Chung-Piaw Teo, *National University of Singapore*

The program brought together people who have done much work on the rigorous mathematical theory of random graphs and experts (mostly physicists and computer scientists) on measuring real-world graphs, modeling them and studying them experimentally.

44 overseas speakers participated in this program. Tutorial Lectures were conducted by Béla Bollobás (University of Memphis and University of Cambridge), Paul Balister (University of Memphis), Svante Janson (Uppsala University), Yuval Peres (University of California at Berkeley), Oliver Maxim Riordan (University of Cambridge), Devavrat Shah (Massachusetts Institute of Technology) and Sanjay Shakkottai (University of Texas).

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Programs & Activities >>>

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Svante Janson: Inhomogeneous random graphs

Yuval Peres: Probability into the breach

A public lecture by Jennifer Chayes (Microsoft Research) on "Epidemics in Technological and Social Networks: The Downside of Six Degrees of Separation" was held on 9 June 2006. It was attended by 89 people.

Jennifer Chayes: Understanding epidemics

A math camp was conducted by Béla Bollobás (University of Memphis and University of Cambridge), Paul Balister (University of Memphis), József Balogh (University of Illinois at Urbana-Champaign), Jonathan Cutler (University of Nebraska-Lincoln), Robert David Morris (University of Memphis) and Amites Sarkar (University of Memphis). 15 students from several junior colleges and NUS High School had a stimulating time learning about topics ranging from isoperimetric problems to *p*-adic numbers to mathematical games such as Nim.

Getting into the spirit of the camp: In dialog with Béla Bollobás

Happy campers

A joint Department of Mathematics/IMS Summer School in Combinatorics was held from 18-26 May 2006. A total of 14 Students from Thailand, Vietnam, USA, Philippines and Singapore attended the Summer School.

Algorithmic Biology: Algorithmic Techniques in Computational Biology

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

Co-chairs

Hon Wai Leong, *National University of Singapore*
Pavel Pevzner, *University of California, San Diego*
Franco Preparata, *Brown University*
Ken W. K. Sung, *National University of Singapore*
Louxin Zhang, *National University of Singapore*

The program brought together researchers in algorithmic biology from a wide spectrum of application areas including, but not limited to, sequence comparison and analysis, microarray design and analysis, whole genome alignment, motif finding, recognition of genes and regulatory elements, gene network, phylogeny reconstruction, phylogenetic networks, molecular evolution, computational proteomics, and systems biology.

The Third Annual RECOMB Satellite Workshop on Regulatory Genomics was held on 17th and 18th July 2006. This workshop continued the tradition of previous RECOMB Satellite Meetings as focused workshops on topics of particular interest to the computational biology community.

Alberto Apostolico: Finding motifs

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A gathering of bio-algorithmics

Ron Shamir: Pioneering pathways

The other workshop of the program, "Workshop on BioAlgorithmics" took place from 12-14 July 2006. This workshop consisted of 34 invited speakers and attracted 95 participants. Two tutorial sessions were conducted by Mehmet M. Dalkilic (Indiana University) and Haixu Tang (Indiana University).

Captivated by the allure of mathematics, computer science and biology

Color commentary on mathematics in biology

Other activities besides the workshops included a public lecture on "The Role of Mathematics and Computer Science in Molecular Biology Research" by Martin Tompa (University of Washington) on 19 Jul 2006 and a Math-CS Camp for students from junior colleges and NUS High School. Students at the camp learned about genetic coding and moved on to the pancake flipping puzzle. It was conducted by Guillaume Bourque (Genome Institute of Singapore), Mikhail Gelfand (Research and Training Center on Bioinformatics, Russia) and Hon Wai Leong (National University of Singapore).

The genetic code and its history: Mikhail Gelfand (seated) and Hon Wai Leong

Dynamical Chaos and Non-equilibrium Statistical Mechanics: From Rigorous Results to Applications in Nano-systems (1 Aug - 30 Sep 2006)

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

Organizing Committee
Leonid Bunimovich, *Georgia Institute of Technology*
Giulio Casati, *University Insubria, Italy, and National University of Singapore*
Lock Yue Chew, *Nanyang Technological University*
Baowen Li, *National University of Singapore*
George Zaslavsky, *New York University*

This two-month program brought together leading international scientists in the fields of mathematics, theoretical, computational, and experimental physics, and local experts from Departments of Physics, Mathematics, Material Science, Mechanical Engineering, Electrical and Computer Engineering, DSO labs, Temasek Labs, and A*Star Institutes. The program participants reviewed recent developments in dynamical chaos theory and

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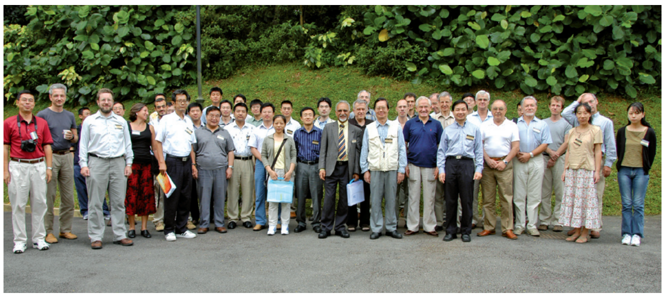
non-equilibrium statistical mechanics and its applications to quantum systems and, in particular, to nanosystems. The participants discussed basic scientific topics for the understanding of the fundamental laws of physics as well as applications to nano and quantum systems. The program provided a platform for the participants, in particular the mathematicians and physicists, to dialogue and collaborate in the fast developing field of nano science and technology.

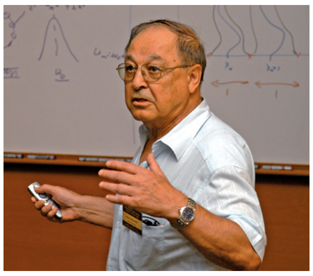
The program began with The First International Workshop on Transmission of Information and Energy in Nonlinear and Complex Systems (TIENCS). Tutorial lectures and seminars took place throughout the two-month duration of the program. The tutorial speakers were Konstantin Khanin, John Hood Lowenstein, Vered Rom-Kedar, Vasily Tarasov, Vladimir Tseitline and George Zaslavsky.

Leonid Bunimovich: Escape artist from circles

Getting physical over coffee



A dynamic group in non-chaotic equilibrium



George Zaslavsky: Persistent fluctuations

Next Program

Geophysical Fluid Dynamics and Scalar Transport in the Tropics (13Nov – 8 Dec 2006)

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

Chair of Organizing Committee
Tieh-Yong Koh, *Nanyang Technological University*

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

This one-month program is a small effort to address the dearth of knowledge in tropical geophysical fluid dynamics. Over two workshops interspersed by two mini-courses, an international gathering of scientists and applied mathematicians would review recent theoretical ideas on geophysical fluid dynamics (GFD) and scalar transport within the tropics and incubate new ideas. The ideas discussed would help organize and elucidate information in datasets generated by weather or sea-state forecast and pollutant dispersion analysis in Southeast Asia. Thus, the program would also benefit participating applied meteorologists and oceanographers who handle datasets on a day-to-day basis. The topics listed below will be covered.

I. Dynamical Models of Tropical Atmosphere and Ocean
II. Chaotic and Turbulent Scalar Transport
III. Hamiltonian and Lagrangian Approach to GFD and Transport

Activities

Workshops:

1. Dynamical Models of the Tropical Atmosphere and Oceans, 13 - 17 Nov 2006
2. Chaotic and Turbulent Scalar Transport in the Tropics, 27 Nov - 1 Dec 2006

Lecture-Tutorial:

1. Lagrangian and Hamiltonian methods in Geophysical Fluid Dynamics and Transport, 4 - 8 Dec 2006

Programs & Activities in the Pipeline

Biostatistics Workshop (25 October 2006)

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

Organizing Committee
Anthony Kuk *(National University of Singapore)*
Kwok Pui Choi *(National University of Singapore)*

Translational medicine and clinical research have been identified as key areas in the next phase of Singapore's

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Biomedical Initiative (2006-2010). Biostatistics will play an increasingly vital role in research efforts in these fields. Indeed, recognition of the importance of research in biostatistics is evidenced by the establishment of the Singapore Consortium of Cohort Studies and the NUS/NUH General Clinical Research Centre. Within Singapore, biostatisticians hold diverse positions in universities, research centers, government ministries and the private sector. The present workshop is a sequel to the Biostatistics Workshop held at the Department of Statistics and Applied Probability, NUS, on 18 August 2006. It will be a forum for working biostatisticians to share experiences on the practical applications of biostatistics in their work (including the modes of interface with clinicians and biomedical scientists), to discuss the challenges and opportunities that lie ahead for the profession, and to formulate strategies to increase the effectiveness and impact of biostatistics.

Moving Interface Problems and Applications in Fluid Dynamics (8 Jan - 31 Mar 2007)

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

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

In this program, we will discuss recent developments in the modeling and simulation of biological flow coupled to deformable tissue/elastic structure, shock wave and bubble dynamics in biological treatment (occurring in shock lithotripsy, lipoplasty, phacoemulsification and others) with experimental verification, multi-medium flow or multi-phase flow involving cavitation/supercavitation (arising from large pressure changes) and detonation problems.

The purpose of this program is to bring together fluid mechanists, physicists, biological scientists, computational scientists, applied and computational mathematicians and engineers to develop and promote interdisciplinary research on modeling, theory, and simulation in the area of fluid dynamics involving moving interfaces, with a view to applications in the biomedical field and physical environment and with relevance to industry and the defense community. It will provide a platform for local and international researchers to exchange ideas, conduct collaborative research and identify future directions and developments in these fields.

Activities:

Winter School
(jointly organized with Department of Mathematics)

- **Workshop:** 8 - 12 Jan 2007
- **Tutorial :** 15- 19 Jan 2007
  Speaker: Zhilin Li
- **Other activities :** 22 – 26 Jan 2007

Workshop I: 8 - 12 Jan 2007
Title: Moving Interface Problems and Applications in Biological Flows.

Workshop II: 12 - 16 Mar 2007
Title: Multiphase Physical Flows and Applications

Braids (14 May - 13 Jul 2007)

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

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

The main theme of the program is the mathematical structure of the braid group, together with applications arising from this structure both within mathematics, and outside of mathematics such as (a) magnetohydrodynamics, (b) robotics and (c) stereochemistry.

The interests of the organizers lie mostly in topology. Therefore it is likely that most long-term visitors will be from that area. Reflecting the theme of the program, it is intended to have tutorials that would:

- introduce outsiders (e.g. graduate students) to the mathematics of braid theory
- facilitate communication between those working in the mathematical theory of braids and those who apply braids elsewhere, specifically in magnetohydrodynamics, robotics and stereochemistry.

Activities:

Tutorials:

Week 1 (4 - 8 Jun 2007)
(a) Braids - definitions and braid groups: Dale Rolfsen (4 hrs)
(b) Simplicial objects, homotopy groups (Part 1): Jie Wu (2 hrs)

Week 2 (11 - 15 Jun 2007)
(a) Simplicial objects, homotopy groups (Part 2): Jie Wu (2 hrs)
(b) Stereochemistry: Kurt Mislow (2 hrs)
(c) Configuration spaces: Fred Cohen (2 hrs)

Week 3 (18 - 22 Jun 2007)
(a) Magnetohydrodynamics: Mitch Berger (4 hours)
(b) Configuration spaces and robotics: Robert Ghrist (2 hours)

Conference: 25 - 29 Jun 2007

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

Mathematical Conversations

David Siegmund: Change-point, a consequential analysis >>>

David Siegmund

Interview of David Siegmund by Y.K. Leong (matlyk@nus.edu.sg)

David O. Siegmund is widely acclaimed for his fundamental contributions to the theory of optimal stopping time in sequential analysis and for his recent work on the application of analysis to genomics. He is well-known for his philosophical delight and mathematical ability in commuting between the theoretical heights of probability theory and the murky depths of statistical applications.

He taught for about 8 years at Columbia University, where he obtained his doctorate under the supervision of Herbert Robbins. Since 1976, he has been at Stanford University, where he was Chair of the Statistics Department twice, served as Associate Dean of the School of Humanities and Sciences and is now the John T. and Sigrid Banks Professor. He has been a visitor to The Hebrew University, University of Heidelberg, University of Cambridge and Oxford University. He was at NUS in 2005 as the first Saw Swee Hock Professor of Statistics.

He has been invited to give lectures at major scientific meetings; in particular, the Wald Lectures, Hotelling Lectures at the University of North Carolina, Taiwan National Science Council Lecture, and Bahadur Lectures at the University of Chicago. Among the many awards he received are the Guggenheim Fellowship, Humboldt Prize, Wilks Medal and membership of the American Academy of Arts and Sciences and of the National Academy of Sciences of USA. He has served extensively on professional committees in the US. He has also been on the editorial boards of leading journals, such as the Annals of Statistics and the Annals of Probability. He was president of the Bernoulli Society and of the Institute of Mathematical Statistics. His numerous papers deal with theoretical questions in probability theory as well as concrete applications concerning clinical trials and gene mapping. He wrote two books (the first jointly with Y.S. Chow and H. Robbins) which are now classics in sequential analysis.

David Siegmund's long association with NUS dates back to the 1980s (as external examiner for the University of Singapore) and continues as a founding member of the Scientific Advisory Board of the University's Institute for Mathematical Sciences since 2001. When he visited the Department of Statistics and Applied Probability (DSAP) from October to December 2005, Y.K. Leong interviewed him at DSAP on behalf of *Imprints*. The following is an edited and revised transcript of this interview in which he talks passionately about his early attraction to mathematics, his subsequent search for the relevance of the mathematical sciences and a calling which he finds fascinating and challenging in theory and application. Here he also shares with us his rich experience in research and administration.

Imprints: Were you already fascinated by statistical mathematics in your school days? Were your school teachers instrumental in attracting you to statistics?

David Siegmund: The answer to the first part is clearly "no". In my school days, I had one mathematics teacher whom I liked very much, but at that time I was more interested in the foundations of mathematics. I found a book describing Cantor's set theory, the cardinality of infinite sets, the non-denumerability of the real numbers, etc. I thought that was a beautiful subject. I did have a university teacher who was instrumental in my attraction to statistics. In some sense, I became interested in statistics because I became disenchanted with the way mathematics in the 20th century had divorced itself from science. I took up an interest in this science and that science, shopping around, and at one point tried the social sciences. After deciding that none of these was exactly right for me, but with an interest in the social sciences, I was drawn to statistics as an area of mathematics closely related to the social sciences. Ironically, I have never done anything specifically related to the social sciences since then, but it did play a role in helping me find the field of statistics.

I: Were you more interested in applications than theory?

S: I've always been interested in theory. At heart, I would love to be a pure mathematician. At the same time I always wanted problems that seem to be related to some kind of applications, but they certainly don't have to be applied

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problems in the sense that working applied statisticians would recognize them as applied problems.

I: How did you get interested in sequential analysis?

S: During my last year in the university, I took a course that involved reading Volume 1 of Feller's book on probability theory (at that time there was only one volume, now there are two), and I thought that the chapter on gambler's ruin was both fascinating and mysterious. The problems were fascinating, and while setting up difference equations was very natural, pulling solutions out of the air, as it seemed to me at that time, was very mysterious. In my first year at graduate school, I took a course in sequential analysis from Herbert Robbins and found the same problems were considered there from a completely different point of view. The methods of solution seemed more satisfying, and the connections to statistical applications added to my interest in the classical problem of gambler's ruin. Since then I have been interested in sequential analysis.

I: Was your PhD thesis on a topic in sequential analysis?

S: It was – on optimal stopping theory. One of the first things I read on my own during the first summer I was a graduate student was the chapter in Doob's book, *Stochastic Processes*, on martingale theory. I thought that it was the most beautiful mathematics I had seen up to that time, and it was naturally related to optimal stopping theory. Conceivably, I had the motivation from sequential analysis at the time but I don't recall. I think I just wanted to learn stochastic processes and that was one chapter that particularly appealed to me. Since my PhD thesis advisor, Herbert Robbins, was interested in optimal stopping theory, and it was naturally related to martingale theory, it was the subject for me.

I: That was at Columbia?

S: Yes, that was at Columbia.

I: Is Columbia near your home town?

S: No, I grew up in St Louis which is right in the middle of the United States. I started to think about Columbia because my wife was interested in going to the Columbia School of Social Work, probably the best known school of social work in the United States. When I mentioned this to Paul Minton, who advised me as an undergraduate, he became excited and said, "Oh, Herbert Robbins, now at Columbia, would be a wonderful advisor. He is very creative. You would love to work with him." So my wife's interests and my interests seem to coincide, and we went off to New York.

I: Robbins was originally a topologist?

S: He wrote his PhD thesis at Harvard in topology, but then before he really developed as a topologist, he was led during the war to problems of operations analysis. After the war he was invited to become a professor of statistics even though he had never taken a statistics course in his life.

I: In your scientific career, you have moved between Columbia and Stanford. What made you decide on Stanford as your eventual choice?

S: From a professional point of view, I found different advantages at Columbia and at Stanford, but my wife was an unequivocal spokesperson, on behalf of our children too, in favor of Stanford. I think she was completely correct – it is a much nicer place to live in than New York City. The scientific advantages became clear to me later on, though early in my career I liked very much to be in Columbia. But Columbia is not as strong a scientific university as Stanford is, and the statistical applications one naturally comes across in New York outside the university have to do with the financial community, the legal community and so forth. Those were interesting but I did not naturally gravitate to them the way I gravitate to some of the scientific things at Stanford. And Stanford's statistics department was larger and certainly, on average, a better department. So that seems to have been a good choice in the long run.

I: I believe you were at Columbia for quite a while.

S: I went there for three years as a graduate student and beginning assistant professor, with a one year hiatus at Purdue University, where Y.S. Chow was on the regular faculty and Robbins and Aryeh Dvoretzky were visitors. After two years at Stanford as an assistant professor, I went back to Columbia for seven years. But since 1976, I have been at Stanford.

I: Do you consider your work from sequential analysis to change-point analysis a natural development of your scientific interests? Could you tell us something about the origin of change-point analysis?

S: It was certainly a natural step. I really didn't know much about change-point analysis; but Bruce Macdonald, who headed the statistics section of the Office of Naval Research asked me to give a seminar in Washington, because he thought some of my research might have applications to change-point analysis. I went there with a few of my own thoughts, but in ignorance of the existing literature. Some of the questions asked by the audience and some of the references they mentioned made me aware that there was this field of change-point analysis. I realized that it was

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indeed closely related to what I had been doing in sequential analysis and that it was quite interesting. In a sense, change-point analysis began with quality control at Bell Telephone Laboratories in the 1920s and 30s, but the real breakthrough, which ushered in the modern period, involved a couple of papers by E.S. Page, a British statistician, in 1954 and 1955 when he introduced the CUSUM test as a means of quality control. He didn't understand the relationship of the CUSUM test with the likelihood ratio test of statistics. That understanding came later, but since that period in the 1950s the subject has grown quite a bit. Initially, it was the result of the seminar questions that I didn't know the answers to, but then later the very rich theory and applications, that have held my interest.

I: You mentioned change-point analysis as a quality control thing. Was it empirically motivated?

S: Yes. The conceptual scheme is that we have a process, some kind of industrial process, that produces items in a complicated way that amounts to a black box. We can't look inside the black box to see if it is operating correctly. What we can do is to make measurements on the products to infer indirectly if it is operating correctly. The change-point philosophy was that you are careful in the beginning when you set things up, and the black box will initially operate correctly. Then after a while, someone gets careless or machinery wears out, and there is a change in the product, and you have to spot that change and then make adjustments to the system so that it starts operating correctly again.

I: Is there a theoretical foundation for this?

S: There certainly is a mathematical foundation. From the point of view of applications, there is always a debate whether a particular model is the best model that you can use. There are models where changes occur instantaneously by a discrete amount and others where changes occur gradually. There is a debate on which kinds of models are better. In spite of a certain level of implausibility, by and large the model that posits abrupt changes is very successful.

I: Do I understand that there are many change-point models?

S: Yes. There is no canonical problem. A problem has a certain structure to it but there is not a single mathematical formulation. In fact, I am sometimes at a loss for terminology. The term "change-point" is embedded in people's minds, but there are many problems with the same essential mathematical structure that don't really fit the change-point idea. So I sometimes use the phrase "change-point-like problems" to convey the idea that we are doing something related to change-point problems but it's not what you would automatically expect.

I: How do you choose the model to use when you are doing change-point analysis?

S: I don't think the answer is any different from any other statistical analysis. One typically starts with the simplest possible model that seems to capture some of the conceptual features of the problem, and then starts adding complications, sometimes called "bells and whistles", to make the model more satisfactory in a quantitative sense, although there is always the desire to keep the things as simple as possible conceptually. There's a famous statement of Einstein to the effect that a theory should be as simple as possible but no simpler. It's the same thing in choosing a model.

I: Do you know whether change-point analysis has been applied to data in the social sciences or even in the historical studies of cultures or linguistics?

S: There is a simple answer to the question, which is "yes", but I can't very effectively describe these applications. There are some in economics and finance, which in fact was the origin of some of the early applications of change-point analysis. In finance, for example, my colleague at Stanford, T.L. Lai has developed quite sophisticated change-point models that can lead to different investment strategies from time to time. I also occasionally get sent a paper or am asked to comment on a paper in the social sciences that has a change-point aspect to it. I usually forget these pretty quickly, so I don't really feel comfortable trying to discuss them in detail. But, for example, I do recall some research concerned with learning theory that asked the question whether learning, say simple skills in elementary school, should be thought of as something that proceeds by occasional dramatic improvements, where testing would indicate that someone hasn't learned anything but then seems to learn overnight, or alternatively that tomorrow we will be a little better than we are today and the next day we will again be slightly better. The learning theorist was trying to build a theory suggesting that progress appears to be rather abrupt, which would be consistent with a change-point model.

I: In history, for example, there are events which are marked by changes which can be thought of as change-points.

S: Right, there's certainly some of that motivation for applications in economics. People ask whether certain policy issues actually lead to changes in behavior or changes in economic conditions or whether certain external shocks to the system lead to a dramatic change or lead effectively to no changes at all. Conceptually that kind of issue has been a part of some economic thinking.

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I: Is it possible to use change-point analysis to make history more quantitative?

S: I don't know. Historians try to use surveys and quantitative methods more and more. It would be interesting to know what kind of change-point models there might be. One problem that is an interesting conceptual application of change-points (and has an historical aspect to it) is the set of data examined by many people, which involves fatal accidents in British coal mines. For about 150 years, the British Coal Mining Board recorded accidents, and kept very clear records. Every accident that involved the deaths of more than 10 miners was recorded. During the period around 1890 there were royal commissions that studied the problem and made recommendations for how mining practices should be changed to make them safer. People naturally wanted to know whether this had an impact. Indeed, the rate of accidents dropped quite precipitously, or the average time between accidents increased quite sharply around 1891 - 1892, during the time that these activities took place. One presumes that this was a response to changes recommended by the commissions, which involve things like, if I recall, using a different kind of explosives, one that is less flammable, using water to wash down the interior of a mine, in particular, trying to get coal dust out of the atmosphere.

I: Is change-point analysis extensively used nowadays?

S: It is certainly widely used in the sense that you can find versions of change-point analysis in many, many different scientific contexts. Within those contexts, it's fairly specialized. For example, in drilling to find oil one wants to know something about the density of rocks through which one is drilling and in particular changes in density reflecting changes in the mineral composition of strata encountered during the drilling process. Change-point analysis of magnetic resonance image data is an approach to this problem that has a somewhat different flavor from most other applications I'm familiar with. Change-point analysis of DNA sequence data has recently become popular in some problems of molecular biology.

I: What about to evolutionary biology?

S: I guess there should be, but I've never looked at the data, and I don't know whether anybody has actually tried to formalize such a model. Certainly there is this ongoing debate about the hypothesis of Stephen Jay Gould of a punctuated equilibrium, that evolution doesn't proceed by small incremental changes as people more or less inferred from Darwin, but exists in a steady state without much in the way of changes and followed by a large number of changes occurring rather rapidly. I think this is a rather natural reaction to reflecting about the role of the environment in evolution, because we know that there are things like ice ages, meteors hitting the earth and volcanoes that have drastic impact on the environment leading to dramatic changes in, say, the average temperature of the earth and the seas. So it's natural to think that those changes, if they occur quickly, must lead to rapid changes in flora and fauna as well. But I don't know if anybody has actually tried to build a model and address the issues quantitatively. It would certainly be interesting, but it is also likely that the data are not sufficient, since this involves going a long way back in the history of the earth to find appropriate data. More modest questions of an evolutionary nature involve change-point analysis of DNA sequence data to identify, for example, places where mutations occur more frequently than the overall background rate.

I: Am I right to understand that the identification of a gene is a change-point problem in DNA analysis?

S: It certainly can be viewed that way. I would say it is helpful to view it that way, although most people involved in gene mapping, which is the area I'm primarily interested in now, do not share my view. I think they are missing something. With the advances made in technology that allow one to genotype markers closer and closer together, the change-point aspect of the problem will become more apparent. Historically, there were very few markers distributed across the genome. For the last ten years, in human genetics it has been customary to use on the order of 300 to 500 markers. Even at that level of resolution, the change-point aspect of the problem is not quite so apparent; but if the resolution should ever become what would be implied by having thousands of markers, which one can easily imagine, then the change-point viewpoint will increase in importance.

I: Is the problem of gene determination in the human genome completely solved?

S: No, it's one of those problems where progress seems very rapid, but then one realizes that there are still many more problems. With each step that we can take, we become more ambitious. Not so long ago one didn't try to map genes except for very simple diseases where there was one gene involved and the gene literally over-ruled almost anything in the environment to determine the phenotype of the individual. Now one is interested in what are referred to as complex diseases or quantitative traits that involve both the genotype, of possibly multiple genes, and the environment, which also may interact. These problems are much more difficult. As I said, at each stage when we think we can tackle more ambitious problems, we realize that the number of problems that appear to be solvable has actually grown and not shrunk.

I: What about the total number of genes in the human genome? Is that settled?

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S: I'm skeptical, but the answer commonly given is about 30,000. Only a few years ago, people were guessing 100,000. I would guess 30,000 is closer. But that ignores features that have only recently been recognized as important. One of these is what is referred to as "alternative splicing" so that a single gene, depending on how the pieces of the gene work together, can produce more than one protein. The mantra of molecular biology 50 years ago was "one gene, one protein". Since there are many proteins, one had the idea that there must be many genes. Now it appears that the number of human genes is much fewer but the number of proteins is still very large. So the basic problem doesn't change simply because we now say there are fewer genes. There is still a large number of functions that are incompletely understood.

I: Has any work with the computer led to theoretical insight in your research work?

S: I don't have a very good answer for that. I think that the computer is so much an intrinsic part of my research that it's hard to say what is an insight based on something I've done on the computer or some other kind of insight. It's very easy to say that the computer helps eliminate paths of research leading to dead ends and reinforces fruitful pathways. But working out detailed examples with paper and pencil is the more old-fashioned way to the same result. To some extent, I'm an old-fashioned person. What insights I've had come from piling up individual cases and trying to find the general pattern. I'm very envious of people who seem to get insights without compiling lots of special cases and who seem not to need to do the calculations until they already know what it is they want to calculate. In my case, most calculations are wasted. There's always a pile of papers on my desk. I cover them with scribbles and throw them away very quickly. The computer is helpful in saving some of those efforts in certain cases. Another very important consequence for statistical analysis is that the computer redefines what one means by a solution to a problem. There are still things computers can't do, but basically a problem is solved once it's reduced to something computers can do. Of course, even then, that is not a completely clear answer because what a computer can do in one person's hands is much more than what it can do in my hands. I have the good fortune to work with many good graduate students and younger colleagues, all of whom know computing better than I do. Often they will keep me from spending too much time in blind alleys by doing some computing for me.

I: Is there any software for the application of change-point analysis?

S: People do develop software for change-point analysis. I don't know of any commercial or large-scale programs largely because I don't use such programs on a day to day basis. I'm very poor at using other people's software, so when I want to do some computing I usually write primitive programs of my own. I have seen software that advertises the ability to do change-point analysis but I have never looked at it carefully to decide whether it is the right way or the way I would do it. Software development is a valuable activity, but it's not for someone of my primitive computing skills.

I: How often do you interact with clinicians and medical practitioners?

S: Here we have an issue of the definition of "interact". If interact means to sit down in an office face to face and have an in-depth discussion of a problem, the answer is "not very often", a couple of times a year. If it means to have a more superficial discussion trying to see whether we have common ground for deeper collaboration, then it's certainly much more often. Many of these discussions, I think, don't lead directly to that collaboration, but I find them very useful nevertheless in trying to formulate problems. Often my formulations are fairly theoretical, so I don't try to propose my research as an immediately practical solution; but I find these discussions a very useful conceptual bridge to finding an interesting research problem. If you broaden the definition more to mean reading articles in medical or genetics journals that don't themselves have completely satisfactory solutions to their statistical problems, I would say I spend a great deal of time doing that. That may be one of my primary sources of stimulation in finding problems. When I was much younger, I read the mathematics, statistics and probability literature to improve my techniques in solving problems that were already formulated. Now I depend on other people to tell me if there is an interesting new mathematical or statistical technique, and what I am really more interested in finding out is if there are new scientific problems that are to my taste, which is somewhat idiosyncratic. That may not be what people mean by interaction, but it's interaction at a distance, by the printed page, and I do that a great deal.

I: Do you interact through meetings or conferences?

S: Certainly. Each year I attend a few statistics meetings and a few genetics meetings. The main reason for going to the genetics meetings is to find out the way the science is going and to try to infer what are interesting statistical problems from what people are taking about. These can be problems that they realize they have not solved satisfactorily, or problems where I am not completely satisfied with the proposed solution. In either case I'm often stimulated to try to see what I can do.

I: I think you have touched on a related question: how do you choose the statistical problems you work on?

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S: I have certainly made a transition over the years in the sense that it is now rare that somebody says, "This is a beautiful mathematical problem you will be interested in", and I respond favorably. I'm much more inclined now to respond to the scientific description of a problem that I can see, or somebody will tell me if I don't see immediately, is related to a statistical problem that I might take an interest in. That was probably not the case when I was much younger. Anything that was related to what I was doing mathematically would automatically interest me. Occasionally I still work on problems solely because of their mathematical fascination, but much less so.

I: Has it happened that after attending a lecture or seminar a problem occurred to you and you wanted to solve it?

S: Yes. I don't think very quickly. I'd say, probably two out of three times when I come out of a seminar thinking that I have something to contribute to a problem that was discussed, it turns out I was wrong. Occasionally that can be a useful stimulus to further research. In many other cases, a seminar does not provide a problem that I work on immediately, but gets stored in the back of my mind in case a related idea turns out to be useful. In the world of mathematics people often admit they never understood somebody else's idea until they rediscover it for themselves. I think this is a real phenomenon. You listen to a seminar or hear a series of lectures on a subject without really internalizing it until a few years later when you circle back to this area by who knows what route, view it your own way, reconstruct what somebody was trying to tell you years earlier and for a while even think it's your own idea. Eventually you recognize that somebody else was there first. Maybe you can still make a contribution, or maybe you can't. Of course, one always hopes that one recognizes the situation before trying to publish a paper as one's own idea that was really something learned at a lecture a few years earlier.

I: Do you do direct consultation work?

S: I do a bit, but not much. There are a few people I work with who know the kind of problems I'm interested in and will be good enough not to come to me for routine assistance, but will come with a problem that interests me. This applies particularly to my colleagues at Stanford. Perhaps this is one of the main benefits of having moved there. A fairly large number of my colleagues in the statistics department are involved in many different problems throughout university, and they are kind enough to use me as a secondary consultant by suggesting problems that they know I would be interested in. If the problem originated outside the department, then I will often go directly to the source. This is exciting because the problems are often important, and it's much better for me than working as a real consultant for a living. Then you have to take problems for which there is a flow of income regardless of whether they are interesting or not.

I: Do you get people calling you up to ask whether you could solve this problem for them?

S: Occasionally, but not usually. I have been department chair from time to time and then it happens, not because the person knows anything about me or my interest, but simply because he finds my name somewhere in the directory or on the internet. Then I'm the first layer of contact and I play the role of trying to suggest colleagues who would be most suitable and most inclined to work on the problem insofar as I understand it. That has its own rewards but is quite different.

I: You were Associate Dean of Stanford's School of Humanities and Sciences from 1993 to 1996. What is your most memorable experience in that capacity?

S: I would say that the overall experience was quite memorable, but no single event. My role was to serve as an intermediary between the Dean of the School of Humanities and Sciences (which involves about 30 departments: humanities, social sciences, natural sciences) and the six natural science departments. The reward to me was to learn what was going on in the science departments. Part of the job that I did not particularly like was learning the enormous cost of doing modern laboratory science. I'm very thankful that I am not a laboratory person although I can also see the excitement of doing laboratory work, being closer to the scientific problems than a statistician can be, even for one doing genuine applied statistics. Lab scientists generate lots of data, and without them there wouldn't be any statistical data analysis. But modern science is an enormously expensive business and part of the job of the dean's office is to help allocate resources. You never can make people happy when you are allocating scarce resources. Learning why scientists want the resources and trying to prioritize competing requests is interesting and stimulating. It was fun trying to figure out what different people were doing, where the quality lay, what should be supported or what not. But you can never provide all the resources you want to, and you never learn as much about what is going on as you want to. You sometimes think that if you spend a few more hours, you would really make a better decision. But in the end you are forced by schedules and so forth to make decisions even when you don't understand things completely, and then you can make people upset. There are ups and downs. I'm happy to be back in my role as a scientist, which I find much more interesting.

I: Do you think statisticians are indispensable?

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Roger Howe: Exceptional Lie Group Theorist >>>

Roger Howe

Interview of Roger Howe by Y.K. Leong (matlyk@nus.edu.sg)

Roger Howe is well-known for his path-breaking work in the theory of Lie groups and representations and for his impact on mathematical education and pedagogy through his teaching, writings and active involvement in educational reforms. His research is also directed toward the applications of symmetry to harmonic analysis, group representations, automorphic forms and invariant theory.

He has a bachelor's degree from Harvard University and a doctorate from the University of California at Berkeley. He taught briefly at the State University of New York at Stony Brook and, since 1974, he has been at Yale University where he has served as director of graduate studies in the Department of Mathematics and as departmental chair. He has held positions at the Institute of Advanced Study in Princeton, University of Bonn, Ecole Normale des Jeunes Filles in Paris, Oxford University and Rutgers University, Institute for Advanced Studies at Hebrew University in Jerusalem, University of Sydney, University of New South Wales, University of Metz, University of Paris VII, University of Basel, Kyoto University, National University of Singapore, Hong Kong University of Science and Technology. He has

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S: I think they are very fortunate to have the opportunities to play as many roles as they do. They are dispensable if they abdicate their responsibilities to participate in the general scientific enterprise to the extent that scientists find it easier and more satisfactory to do their own statistical analysis. But it probably also works in favor of statisticians that they are very inexpensive. It may not make sense for first rate biomedical scientists to devote a substantial part of their time to thinking about statistics if there are helpful statisticians available. You can have a first grade mathematics and statistics departments with much smaller investment than a first-rate chemistry department.

I: The humanities and the sciences are under the same school at Stanford, but they seem to be incompatible.

S: There is a constant argument as to whether they should be broken up. In the United States, the Stanford arrangement is not unusual, but it is also not universal. One somewhat interesting feature of being an associate dean was to learn about different administrative structures in different universities, and which problems the structures help to solve and which ones they don't solve. For example, I was on a review committee once for the Department of Statistics at the University of Chicago. At that time I was just beginning and spent some time talking to the long-term dean of the School of Physical Sciences at Chicago, which has a quite different structure from our School of Humanities and Sciences. For example, their School of Biological Sciences includes medical faculty. At Stanford there are several "biology" departments, one in the School of Humanities and Sciences and several in the medical school. You would think that certain problems that arise at Stanford might have been solved by the different structure at Chicago. But it seems that while some problems are alleviated others are created, and still others exist with either administrative structure.

I: Were you able to bridge the gap between the scientists and the people in the humanities?

S: For most of my time in the dean's office, my main concentration was on the science departments. I didn't put in as much effort interacting with the humanities departments. For a short time I was put in charge of the philosophy department and the interdisciplinary program on ethics in society. I have occasionally thought that I am a "closet" philospher but fortunate that I don't have to earn my living that way, so I don't have to be rational, or consistent or possess other qualities we expect of philosophers. This was a very interesting experience even though I found it difficult to make informed judgments and came to rely a great deal on telephone conversations with faculty at other universities.

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been invited to lecture in many countries throughout the world. He is a member of the National Academy of Science (US), the American Academy of Arts and Sciences and the Connecticut Academy of Science and Engineering. He received the American Mathematical Association's Lester R. Ford Award for exposition. He is an exceptional research mathematician who also serves selflessly and tirelessly on national and international boards and committees for the advancement of mathematics and the improvement of mathematical teaching and education. Among others, he has been involved in the CBMS project on the mathematical education of teachers, AMS Review Group for revision of the NCTM Standards, NRC Mathematics Learning Study and AMS Committee on Education. He was on the Board of Directors of the Connecticut Academy for Education in Mathematics, Science and Technology and the Mathematical Sciences Education Board at the NRC. Recently, he received the 2006 American Mathematical Society Award for Distinguished Public Service.

In recognition of his distinguished scholarship and exceptional teaching, he became the first incumbent of the Frederick Phineas Rose Professorship in Mathematics, and he was recently appointed the William R. Kenan Jr. Professor of Mathematics at Yale. His influence on his students is well recognized. In particular, his influence is strongly felt in Singapore in his role as chair of the Scientific Advisory Board since the establishment of IMS in 2000. He has also bequeathed part of his mathematical legacy to the Department of Mathematics in NUS in the form of a strong research group centered round a number of his returned PhD students. In his honor and in appreciation of his numerous contributions, an international conference was organized at NUS from 6 to 11 January 2006 on the occasion of his 60th birthday. When he was here as the guest of honor of this conference and also for the annual visit of the advisory board, Y.K. Leong interviewed him on behalf of *Imprints* on 7 January 2006 at a café near Swissôtel The Stamford, Singapore. The cacophony of the surroundings and the cold from which he was then recovering did not dampen the passionate spirit with which he talked about mathematics. The following is an edited and enhanced version of the transcript of the interview.

Imprints: You did your B.A. in Harvard. What attracted you to Berkeley for your PhD?

Roger Howe: The main factor was that it was in California. I had spent my high school years in California and I still considered myself a Californian. Although I was in the east for 4 years, I really wanted to go back. Berkeley was the best-known place in California while I was there. In some sense, I was naïve. I didn't even think about Princeton. I didn't know that Princeton was the place you might want to go to. In some sense I should have stayed in Harvard because I had won the Putnam Competition and that included a fellowship to study at Harvard. I think that some of the faculty there were somewhat shocked that I decided to leave, but I was very intent on getting back to California at that time. A more substantial reason for going to Berkeley was that it had both a very large and very strong faculty so that you can study almost anything.

I: Was anybody there whom you particularly liked to work with?

H: I had already gotten interested in representation theory, in which I have done most of my work. George Mackey who was at Harvard (I did a reading course with him in my senior year) had a student there [at Berkeley], Calvin Moore. He ended up being my advisor.

I: You mentioned the Putnam Competition. When you went to Berkeley, did you have a scholarship or something?

H: Yes, they actually offered me a pretty nice fellowship. There were some special fellowships from the government intended to recruit students into scientific areas and I got one of those fellowships. It was called an NDEA (National Defense Education Act) Fellowship. The NDEA was motivated by a desire to keep ahead of the Soviet Union in science. It supported many graduate students who went on to productive careers in mathematics and science.

I: Why did you choose to pursue research in "pure" mathematics?

H: Well, actually I hesitated a lot. It bothered me that there seems not to be more emphasis on connections. To me the applications of mathematics are a very appealing part of the subject and it is very important to me that mathematics helps you understand the world, but eventually I ended up going into pure mathematics and I have been very happy working there.

I: No regrets?

H: Not really, no. Well, yes, some; I wish I had done some more applied kind of things but it hasn't worked out.

I: Do you consider yourself to be an analyst first and then an algebraist?

H: Well, this is going to sound kind of funny but I actually consider myself to be a geometer. I kind of think geometrically. I've never published any work specifically on geometry but I love the subject very much. Euclidean geometry was one of my favorite subjects in high school and I've continued to think about it. I'm working with a

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colleague on a textbook for geometry. I think in terms of pictures although my work doesn't seem to have much to do with geometry.

I: Not many mathematicians think geometrically, they think more symbolically.

H: I think topologists often think in terms of pictures. Algebraists and analysts probably have other means of figuring things out. For some topologists at least, pictures are very important.

I: Your work is connected to some kind of topology, isn't it?

H: Lie theory actually – this is an attractive aspect of it to me – it connects with all branches of mathematics. It is geometrical but it is also algebraic, it is also analytic. Some of the main examples of geometric figures of manifolds and constructions come from Lie theory. That's a very attractive part of the field to me. You can connect things.

I: Are you a theory builder or a problem solver?

H: I like problems. I often will work on problems but I have never published a paper that solved a specific problem. And I very much like making connections, sort of coordinating things and connecting things together. On the whole, I guess that I'm a theory builder.

I: To be a theory builder, one has to know a fair number of fields to see the connections.

H: It helps to know different subjects. I have to say that I'm quite surprised how useful many things I learned for no particular reason have turned out to be… you learn things that don't seem to have connections with one another, and later on, you do find that there is some way to relate them together. That's very satisfying.

I: Algebra, or for that matter analysis, has its origins in rather "concrete" problems but modern research in these areas seems to be getting more and more abstruse and esoteric. Do you think that this is a desirable trend?

H: I think there's a constant kind of dynamical dialectic between what seems to be very abstract and the more concrete things. A very dramatic example in recent years is Mandelbrot's exploitation of fractal geometry. The basic work that Mandelbrot has exploited or publicized was actually invented by mathematicians in the late 19th century and early 20th century, and they were for a long time considered to be extremely abstruse constructions that could never have anything to do with reality. These were prime examples of things that only mathematicians would ever think of. And then Mandelbrot came along and said, "Actually, clouds, coastlines and trees and many, many different shapes in nature share some of the qualities of these structures, and we can learn about nature by thinking about these seemingly very weird structures." There's always a pull between the abstract and the general and things that seem to be far from reality, on one hand, and very concrete things on the other. It's also the case that physics is very weird and physics had to go far beyond our basic intuition in order to uncover a more fundamental truth in nature. I think mathematics is like that, and of course, mathematics is a major tool in physics.

I: Did Mandelbrot discover those things independently or did he already know about them?

H: He was aware of the earlier constructions. He was the one who was able to see that there are things in nature that are like fractals.

I: It seems that the success of algebra is its ability to reduce problems to symbolic manipulation but that the ideas of analysis are more intuitive and their formulation often precede their justification. Do you agree with this view?

H: Of course, there are some famous examples of that, like the Dirichlet Principle which was used in the 19th century for a long time before it was justified. This may be an example of mathematical riches in a direction that we don't very well understand but when we come to understand them we can reduce them to more basic things. I also want to say that in Lie theory there is a very interesting dynamical interaction between the algebraic and the analytic. It turns out, for example, that a fairly important aspect of representations of Lie groups is that there are some natural functions associated with them called matrix coefficients. An important fact is that for many groups, matrix coefficients die off – they go to zero at infinity, and this has implications for ergodic theory and counting rational points on various varieties. The proof of that is a very interesting interaction between the algebraic structure of the group and the analytic structure of some vector space. Again and again in Lie theory, you find these things, which you think of as different, interacting in an interesting way.

I: In general, geometric intuition seems to be very nebulous and often initially the ideas do not have rigorous justification.

H: It's hard to pin down, yes, but then you can spend very profitable, maybe very long, periods trying to figure out what are the reasons why this thing is true and you learn a lot during that process.

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I: It's interesting what you said earlier on that you think in terms of pictures. That means that your intuition is basically a kind of geometric intuition.

H: I think it is.

I: It's quite rare, at least among algebraists.

H: It's hard for me to say. You can only know how you think. You can't know how other people think.

I: Could you briefly tell us some of the central problems in your area of research?

H: In representation theory in the strict sense, I guess the major fundamental problem still open is the classification of unitary representations. This has proved to be a very hard problem, Interestingly, the collection of all representations of a reasonable form has been known for 20 or more years but to figure which ones inside that set are unitary turns out to be a very hard problem. Then there are applications of representation theory to the theory of automorphic forms, and there, there are a huge number of problems which prove to be extremely challenging. A large part of it is what is called the "Langlands program" which has been challenging many mathematicians for several decades.

I: Has there been much progress in the Langlands program?

H: There has been very substantial progress but I'm not the best person to comment on it. In particular, Jim Arthur has established a rather general version of the trace formula and he has made applications of it. That's a good example where there is intuition and things are not proved, so there is a large web of conjectures. Only people who spend all their time thinking about them have a clear picture of what part is known and what part is conjecture. It's quite an amazing zoo.

I: How does it compare with the classification of the finite simple groups two or three decades ago? There were then a lot of things floating around.

H: That was a fairly well-defined problem. Of course, before the classification was achieved, people didn't know how far one would have to go. But the Langlands program is much more open-ended. I think that it includes problems that we, in fact, will never solve.

I: You mentioned the unitary representations. What is the significance of unitary representations?

H: Well, in physics, that is, quantum mechanics, representations arise because of symmetries of a physical system. And the representation should be unitary, because the states of the system come from vectors in a Hilbert space, and the inner product has a physical interpretation. In the theory of automorphic forms, again there is a natural inner product which was constructed in special cases before it was realized that representation theory was relevant. And of course, unitary representations have a nicer theory than more general ones, just as statisticians liked to use least squares approximation, because it is nice mathematically. However, not all representations useful in applications are unitary.

I: What are the prospects of settling this problem within the next 10 years?

H: Very good progress has been made. In fact, it is now understood that for any given group the problem of classifying the unitary representations comes down to a finite algorithm, but the question is: can it be made more specific to form some kind of global picture? Also, there are computational issues because for some of the exceptional Lie groups the computations that you have to do to carry out this algorithm may be very, very expensive. It's not a problem about which you know nothing. A lot is known. David Vogan and Dan Barbasch, in particular, have made a tremendous amount of progress, but it's not settled yet.

I: Has the computer been used?

H: Computers are being used. In fact, there is a group now working on setting up a website where you can go and input a given representation of a given group and it will compute for you whether that is unitary or not.

I: From your personal point of view, you would prefer something more conceptual?

H: More pictorial, yes. We need a more geometric picture of it.

I: What are some of the recent applications of your field to other areas?

H: The subject of matrix coefficients has applications to ergodic theory and counting of points on varieties (equidistribution of points in some larger space). That is one kind of applications. Of course, there is the ongoing application to to the Langlands program, the theory of automorphic forms, where there is a constant interplay between representation theory and a broader spectrum of number theory. Recently, S. Alesker has used group representations to settle some outstanding conjectures in geometric integration theory.

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I: What about to physics?

H: Well, this is what got people interested in representation theory at the start. Some representations of certain groups, you could say, are in some sense at the center of mathematics and at the center of the universe; in particular, a relatively simple kind of group called the Heisenberg group. Mathematicians call it the Heisenberg group because it is the group-theoretic embodiment of the Heisenberg canonical commutation relations in quantum mechanics. A tremendous amount of mathematics and physics relate to that group. Differential equations come in naturally and there are several absolutely fundamental mathematical structures which are connected with that group. There's the basic result of Hilbert's syzygy theorem in invariant theory or the linear algebra behind the Hodge decomposition of cohomology on Kaehler manifolds. So much is connected with this particular group that it's really amazing. Also, the quantum-mechanical hydrogen atom which is the basis of our understanding of chemistry is essentially a certain extremely distinguished group representation. So group representation theory in some sense is fundamental for our understanding of quantum mechanics. But it connects to many other things too. The odd thing is that these physical systems carry extremely special, very interesting representations of certain groups and the challenge is to find out what are the uses of more general representations. There have not been that many applications of general representation theory as we would like to have.

I: There are those super-Lie groups. Are they generalizations of the standard ones?

H: Yes, they are sort of combinations of several algebraic structures in one. Lie algebras are based on a product which is skew-symmetric – if you switch the order of two elements in a product, the product changes sign. There is another kind of algebra called the Jordan algebra which is commutative in the standard sense – you switch the order of two elements, the product doesn't change. Lie super-algebras are a combination of these two structures. They definitely have applications. They attracted interest and were classified when physicists became interested in so-called "supersymmetric" field theories. Actually, the Hilbert syzygy theorem involves a Lie super-algebra.

I: Is pure mathematics losing talented students to other more "glamorous" areas like mathematical finance or more applied areas like statistics and computer science?

H: This always happened to some degree. A talented person will have several areas to choose from and this has been going on a long time. Gauss had to choose between philology and mathematics. When he made some of his discoveries about the cyclotomic numbers and the construction of regular polygons, he decided that mathematics might be a better choice. Many people who could do mathematics can also do other things. Probably it's partly circumstance, what they get exposed to. I think it's also personality. For example, theoretical physicists and mathematicians tend to have very different personalities.

I: Talking about personality, there seems to be the observation that the personality of an algebraist is quite different from that of an analyst. Do you agree with that?

H: I would agree with that.

I: What do you think is the secret of your tremendous success in teaching mathematics at the university level?

H: Well, first, "tremendous success" are not words I would use. But I have worked hard to improve over the years, and it has been very rewarding to see students respond. Teaching is a complex art, and you can't sum up what you do in a few phrases. But the thing that I have worked on is to improve my communication with students. I spend a lot more time asking them questions, and less time just explaining. I sometimes say, that I used to try to show students why math is easy, and now I try to get them to see why it is hard.

I: As the Chair of the Institute's Scientific Advisory Board during the past five years, what is your deepest impression?

H: I have been extremely impressed by Louis Chen, the energy and devotion which he put into this institute. He has done a terrific job of soliciting proposals from the community in Singapore and trying to find ways in which IMS can help the mathematical community in Singapore. I think that without his leadership IMS would have been less successful.

I: I think you once mentioned the "critical mass" for active research …

H: This has been a problem and will be an on-going problem. Singapore needs expertise in mathematics and the IMS can help nurture that expertise and build it. If anybody can, Louis Chen will make that case to the Singapore government.

I: Of course, we will still need people like you to chart the direction.

H: It's really a pleasure for me to work with Louis and to help out the IMS. The whole Scientific Advisory Board has really worked extremely well together. I think we have done our best to be constructive and help make suggestions

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Keith Moffatt: Magnetohydrodynamic Attraction >>>

Keith Moffatt

Interview of Keith Moffatt by Y.K. Leong (matlyk@nus.edu.sg)

Keith Moffatt has, in a long and distinguished career, made important contributions to fluid mechanics in general and to magnetohydrodynamic turbulence in particular. His scientific achievements are matched by his organizational and administrative skills, which he devoted most recently to the Isaac Newton Institute for Mathematical Sciences at Cambridge.

Educated at Edinburgh University and Trinity College Cambridge, he first taught at Cambridge University and was Fellow of Trinity College from 1961. Except for a brief stint as Professor of Applied Mathematics at Bristol University (1977–1980), his career has been centered at Cambridge University, where he has been Professor (now Emeritus) of Mathematical Physics, Head of the Department of Applied Mathematics and Theoretical Physics (1983–1991), and Director of the Newton Institute (1996–2001).

He has been a visiting professor at the Ecole Polytechnique, Palaisseau,(1992–99), Blaise Pascal Professor at the Ecole Normale Superieure, Paris (2001–2003), and Leverhulme Emeritus Professor (2004–5). He has served as Editor of the Journal of Fluid Mechanics and as President of the International Union of Theoretical and Applied Mechanics (IUTAM). For his scientific achievements, he was awarded the Smiths Prize, Panetti-Ferrari Prize and Gold Medal, Euromech Prize for Fluid Mechanics, Senior Whitehead Prize of the London Mathematical Society and Hughes Medal of the Royal Society. He also received the following honors: Fellow of the Royal Society, Fellow of the Royal Society of Edinburgh, Member of Academia Europeae, Fellow of the American Physical Society, and Officier des Palmes Académiques. He was elected Foreign Member of the Royal Netherlands Academy of Arts and Sciences, Académie des Sciences, Paris, and Accademia Nazionale dei Lincei, Rome.

He has published well over 100 research papers and a research monograph *Magnetic Field Generation in Electrically Conducting Fluids* (CUP 1978). Although retired from the Newton Institute, he continues to engage in research and to serve the scientific community. In particular, he is a founding member of the Scientific Advisory Board (SAB), which has helped our Institute (IMS) to find its direction during the crucial first five years and establish itself on the international scene. When he was at the Institute during the annual visit of the SAB, Y.K. Leong interviewed him on behalf of *Imprints* on 6 January 2006. The following is an edited version of the transcript of the interview, brimming with reminiscences and good-humored chuckles, and capturing the excitement of discovery in an important and very relevant field of scientific activity.

Imprints: You already had a first-class honors degree in mathematical sciences from Edinburgh when you went to Cambridge to do a BA. Were the first two years in Cambridge decisive in your choice of research area for your PhD?

Keith Moffat: Yes, in fact my first year in Cambridge was decisive. In those days, it was still quite common for a graduate from a Scottish university to go to Oxford or Cambridge and take the BA. This was the tradition that I followed. I enjoyed fluid mechanics at Edinburgh University but I was also exposed to quantum mechanics, and I thought that my career would be in this subject – that was what attracted most graduate students in those days. It was related to nuclear research and everything else. In my first year in Cambridge, I attended more courses in quantum mechanics at graduate level, but realized in the course of the year that I didn't want to pursue research in that field. I yearned to go back to the fluid mechanics that I had enjoyed so much at Edinburgh. So after one year at Cambridge I took that

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which might make proposals stronger. It's been a pleasure working here.

I: Do you foresee continuing working for IMS for the next 5 years?

H: Well, that's up to Louis and what he wants to do. It might be good to have fresh people in to get new ideas.

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decision and went to see George Batchelor to talk about the possibilities in fluid mechanics.

I: That was in the mathematics department?

M: Yes, Batchelor was in the Mathematics Faculty, but he actually occupied an office in the old Cavendish Laboratory. He was quite close to physics as well. I remember very well my first meeting with him in his office absolutely crammed with books and papers. I had attended his course in fluid dynamics and I liked the subject, and turbulence was the natural area to go into. He was one of the world authorities at that time on the theoretical side. It was obvious that it was a very challenging subject, and it still is!

I: Was your interest in fluid turbulence largely due to the influence of George Batchelor?

M: Yes, he was the authority, and he had other very able people working with him in a strong research group – people like Ian Proudman and Philip Saffman; and G. I. Taylor still exerted a benign influence in the background. There was an atmosphere of great vitality in research in fluid mechanics. George put me on to a problem in turbulence. This was my second year in Cambridge when I started research, although I was still doing my BA.

I: Is it compulsory to do the BA in Cambridge?

M: Yes, I had to take the BA, although Batchelor took me on as a research student on the basis of my Edinburgh degree. He was an Australian and came from Melbourne University. He had a very open attitude (for Cambridge!). He regarded my degree from Edinburgh as quite adequate as a preliminary to research.

I: Did you do any experiments?

M: Not at that time. I was entirely on the theoretical side. I did some very simple experiments later in my career, but not on turbulence. One of the attractions in fluid mechanics is that you are concerned with phenomena that can be seen. You can easily visualize and that appeals to me. I like to do simple experiments. I like to watch, as we all do, the flow of water, for example, and the vortices that develop and the interactions of these vortices; it's fascinating. When an experiment can be easily done – a tabletop sort of experiment – then I will do it, often for demonstration purposes for students. It's interesting how often when you are preparing a demonstration for students, it raises more questions and leads to research problems.

I: As a child, were you already interested in observing physical phenomena?

M: I think most children are interested in what they see around them, they are curious about the behavior of mechanical things. It's one way to get children interested in science, trying to understand what we see around us. But, no, I think my real appreciation developed much later in life.

I: To be more specific, were you fascinated by the flow of water as a child?

M: Well, I always enjoyed water, I must say. Coming from Scotland, we were frequently on holiday either at the seaside or in the country where we have wonderful rivers and mountain streams. Yes, I would sit for hours watching the swirling flow.

I: Is magnetohydrodynamics mainly applied to astrophysics?

M: That is certainly one important field of application, but by no means the only one. Magnetohydrodynamics (MHD) has applications equally in geophysics, notably to the dynamo problem of generation of the Earth's magnetic field. Then there's the intensely practical problem of controlled thermonuclear fusion: the challenge is to contain a very hot ionized gas using a magnetic field. Many MHD problems arise in this context concerning existence, structure, and stability of magnetostatic equilibria. There has been huge activity in this area dating from the 1950s and 60s. Then there's the whole area of liquid metal MHD, relevant for example to processes of flow control in the continuous casting of steel, and other metals and alloys. And in the developing of new materials, there's a process called crucible-free casting: you have to contain a sample of liquid metal in extremely pure form and you can do this by using magnetic levitation. There are many important practical applications of this kind.

I: You mention magnetic levitation. Some trains work on that principle.

M: It's a similar principle. But there you are levitating a solid structure. To levitate a fluid with its infinity of degrees of freedom, there are delicate problems of stability.

I: Are there any other practical problems?

M: Well, there's a host of stirring and mixing problems using magnetic fields. If you use an alternating magnetic field, for example a field rotating at high frequency, you can generate rotational flow in a container, and by carefully crafting the field, you can generate quite complex flow fields. If you are interested in mixing, this is a valuable technique, which is more sophisticated than using a spoon! Again, there has been a lot of work in this area since the 1960s.

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I: Do magnetic fields have destructive effects, like radioactive particles?

M: No, they don't seem to have. On the contrary, the magnetic field of the earth protects us from very damaging radiation from outer space and it's very fortunate that we have a magnetic field serving as a protective blanket. I don't know what happens if the human body is subjected to an extremely strong magnetic field; it's not altogether known what the effects might be. It's better to avoid it. Experimental work in MHD can be dangerous not because of the strong magnetic fields that are used, but rather because you are dealing with very volatile substances. Even mercury is dangerous: it wasn't recognized in the fifties, but it's now well-known that the vapor from mercury is very poisonous, and so MHD laboratories using mercury have to be very carefully designed to meet health and safety regulations. Liquid sodium and potassium alloys are very high conductivity metals, which are used for experiments and are extremely dangerous, extremely inflammable. So you must avoid any possible leakage in an experiment.

I: Does every substance have a magnetic field?

M: Well, at the atomic level of microscopic fluctuations, yes. But in MHD one is dealing only with fluids that are good conductors of electricity, either liquid metals or hot ionized gases.

I: How much progress has been achieved in fluid turbulence, at least in MHD?

M: Progress in turbulence at the fundamental level is extremely slow. You sometimes take one step forward and two backwards! This applies even to the most fundamental theoretical development in turbulence, the theory of Kolmogorov (1941) which essentially boils down to inspired dimensional analysis. Even Kolmogorov recognized a fundamental flaw in his theory, and he published a revision (his updated thoughts) in 1962, some 20 years later. At that stage, he himself undermined his own theory! One of the 'firmest' foundations of turbulence from that point on became very shaky. This is typical of the history of the subject.

I: I think it was Feynman who said that turbulence was the major unsolved problem of classical physics.

M: I thought this went back to Einstein. You may be right, it may be Feynman. He was certainly concerned with turbulence in some of his writings. I think it is true to say that at the fundamental level, turbulence is still not fully understood. There are many approaches – mathematical, physical, engineering – and these are very different. You hope that there is some common ground at the center where real progress can be made. As regards MHD turbulence, the news is good; in fact, I think the greatest advance in understanding did come in magnetohydrodynamic turbulence and it came in the 60s. It came through what is now described as mean field electrodynamics where the turbulence is on small scales but you are concerned with evolution of the magnetic field on a much larger scale, so you have scale-separation, allowing you to average over the small scales and focus on what happens on the large scale. This works fairly well for MHD, and the application is very important both in astrophysics and geophysics.

I: Is it a statistical approach?

M: There is an averaging involved in it, so to that extent it is statistical, but it's a fairly rudimentary sort of statistics. You take care of non-linear effects through this averaging but there is great subtlety in the process. The great leap forward was in this area. I was lucky to be involved through recognizing the relevance of a quantity called helicity in turbulence: this is the correlation between velocity and vorticity. It relates to distinguishing between right-handedness and left-handedness. The physicist would describe it as a measure of the breaking of chiral symmetry, and it is an extremely important concept in MHD turbulence. This realization developed in the late 60s and gained acceptance through the 70s; that was the great breakthrough. So in this area at least, we can look back on the last 50 years and say "Yes, we have a big increase in understanding". But still now, when we look at pure turbulence, the undiluted problem with no magnetic effects, I don't think we have any such great increase of understanding.

I: You mentioned Kolmogorov's work on turbulence. Did Batchelor try to elucidate on his work?

M: Yes, this was his early work, just after the war, in 1946/7 when he came to Cambridge and worked under G.I. Taylor, although he was from the beginning very independent. He unearthed Kolmogorov's papers from the bowels of the Cambridge library, studied them very closely, gave his own lucid interpretation, and gave them very wide publicity. It was through Batchelor's work that the theory became widely known in the West.

I: Do you think that in the next decade or so there will be conceptual breakthroughs in turbulence or do you think that computers will play an even greater role in understanding turbulence?

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M: We must always remain hopeful! I think the answer is: "both". Important progress in turbulence now does come, no one can deny it, from advances in high-power computer simulations. We need these, but computer simulations alone do not lead to real understanding. They have to be coupled with theoretical and experimental work. You really need a three-fold interaction: computer simulation, theory and experiment. There will always be a place for careful theoretical analysis. Computer simulations often throw up new developments: for example, it was computer simulations that detected the prevalence of concentrated vortex filaments in turbulent flows. This immediately led to a new theoretical modeling and the search for an understanding of why these filaments are such a pervasive feature of turbulence. New theoretical insights then suggest new computer simulation experiments; and so on. We need both. As far as the next breakthrough is concerned, for the next 10 years (a good time-scale, I hope) there is one very big challenge and it relates to understanding the way that these concentrated vortices interact when they are non-parallel. In a fully 3-dimensional flow, they tangle with each other in a very complex manner. The big question is whether the associated solution of the Navier-Stokes equation remains smooth, regular for all time, or whether a singularity will typically develop within a finite time. This is an unsolved problem, which dates back to Leray in the 1930s. It is one of the seven millennium problems posed by the Clay Institute for which a prize of one million dollars is on offer. This calls for a mathematically rigorous solution, and that needs new theoretical ideas. The problem is that the computer can never demonstrate either a singularity in finite time or regularity for all time. At best, the computer can provide an indication of a trend, but then theory has to take over to establish that the trend is genuine and that it really does go to a singularity, or to regularity, or whatever. There is huge interest in this problem, which is central to turbulence because if it turns out that singularities of vorticity are a generic feature of incompressible flow, then there must be some means of resolving these singularities. My view is that compressibility must be taken into account on the very small scales at which such singularities occur. Compressibility effects are usually ignored and thrown out at an early stage in turbulence analysis.

I: Do the Navier–Stokes equations apply at the atomic level?

M: No. You do need to adopt a continuum approximation. Obviously that does break down when you get down to the level of fluctuations of density at the molecular level. Even so, the Navier–Stokes equations are still valid down to the level of microns, but not to the level of molecules or atoms.

I: One would have thought that at the continuum level it would be easy to solve the equations.

M: Yes, but it isn't. Within the continuum framework, the equations are nonlinear and dissipative. Also, the incompressible Navier–Stokes equations are non-local in character, because of the long-range influence of pressure. All these things conspire to make it very, very difficult.

I: Have the Navier–Stokes equations been modified?

M: Yes, they have been modified in a number of ways, depending on the context. For example, two-dimensional Navier–Stokes is relatively easy; but in three-dimensions, all hell breaks loose. You can creep towards 3 dimensions – two-and-a-half dimensions, for example, where you take into account some 3-dimensional effects but not all. That's generally where progress is made.

I: I may be simple-minded, but going beyond 3 dimensions may help.

M: Oh, going beyond 3 dimensions to 4? That is possible. There are other examples in physics where you go to $4 - \varepsilon$ dimensions, where ε is formally a small parameter; then having done the calculation, you boldly set ε equal to 1, and you are back to 3. Attempts of this kind have been made in turbulence but so far have had very limited success.

I: What about fractional dimensions?

M: That has a bearing. There was hope in the 70s that new ideas from chaos theory would help to crack the problem of turbulence, but I think that was fairly short-lived. Certainly particle paths are chaotic in turbulent flow, and ideas from chaos theory are relevant to mixing, but they don't solve the dynamical problem of turbulence.

I: Historically, there seems to be a British tradition in applied mathematics (classical physics) that can be traced to Maxwell, Stokes, Reynolds, Taylor and Batchelor. Do you consider yourself to be a successor of this tradition, and how much of it is being continued?

M: Well, it would be pretentious to claim to be a successor of the tradition established by these illustrious names, but I am certainly a beneficiary! I was greatly influenced by Batchelor, and I had a close relationship with him until he died in 2000. I knew G.I.Taylor well also at Trinity College, till his death in 1975, and I had frequent opportunities to talk with him informally in the college. Taylor told me he had attended a lecture of Lord Kelvin in 1904; this is another name I would add to your list – a very famous name in

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classical fluid mechanics (I would also add Lord Rayleigh). Kelvin knew Stokes very well and they had an incredible correspondence that extended from 1846 until the death of Stokes in 1903. Through G.I.Taylor, I have this remote link with Kelvin and Stokes! Three years ago, we commemorated the centenary of the death of Stokes in Cambridge and I immersed myself in his papers in fluid mechanics and gave a lecture on this subject. I do have a strong feeling for the achievements of Stokes and his relationship with Kelvin. Of course, Maxwell comes into the picture, and I feel an affinity with him too. (Like Maxwell, I was born and educated in Edinburgh.) He was, of course, a very, very great figure in science, increasingly regarded as being in the same league as Newton and Einstein. What a tragedy that he died so young! That was right there in Cambridge, where he was first Head of the Cavendish Laboratory and Fellow of Trinity College. As regards Reynolds, he was a Professor at Manchester, famous for his experimental observation of the transition to turbulence in a pipe and the fact that this apparently occurs at a critical value of a dimensionless parameter that later became known as the Reynolds number. So yes, I guess he's part of this great British tradition in fluid mechanics. I'm certainly happy to have been nurtured in this tradition.

I: What about its continuation?

M: I think it is still strong in Cambridge and the UK. We can't claim Kelvin at Cambridge because he spent his whole career as Professor of Natural Philosophy in Glasgow, although he had been a student at Cambridge and made frequent visits there, particularly to interact with Stokes. It is a peculiarly British tradition. The strength of fluid mechanics in the UK is a consequence of Stokes, Kelvin, Rayleigh and Taylor. Taylor didn't have that many students but his influence in the UK remains strong, particularly in my department (DAMTP) in Cambridge where a dominant theme is fluid mechanics and its many applications. I think this will continue but the nature of the investigations changes and, of course, the computer revolution plays an important part. You can't do research in fluid mechanics nowadays without being involved in computational work at the same time.

I: Are you still able to attract good students to do fluid mechanics?

M: Personally, no, because I reached the retirement age in Cambridge three years ago. I've had a very good research student from Poland these last three years (Michal Branicki) who has just completed his PhD, but it's not normal to take on new research students after retirement. It's possible but unusual. Most research students would wish to be with younger members of the faculty. There is a good continuing recruitment of research students into fluid mechanics in the department, certainly. Its applications are traditionally in the physical sciences and engineering, but the range of applications now embraces biological sciences, geophysics, and astrophysics as well; so it's very broad!

I: It used to be that the understanding of the term "applied mathematics" in the UK is different from that in the US. How much of this is it still so?

M: Well, I talked with Avner Friedman about it this morning. I agree with you that it used to be different, but the use of the term is now converging. Even within the UK, people would differ on what they mean by "applied mathematics". Even the distinction between pure and applied mathematics has been eroded, and quite rightly so. People don't like to use the term "pure mathematics" anymore, because some areas may be quite pure in one epoch and turn out to have important applications in the next. One of the functions of the Newton Institute is to surmount interdisciplinary barriers, particularly between pure and applied mathematics. It's one subject – mathematics and its diverse applications. For me, applied mathematics is mathematics applied to the physical and biological sciences. But some would extend the term to cover the social sciences also. Financial mathematics, for example, is that applied mathematics or isn't it? It's what you are practicing. The boundary between theoretical physics and pure mathematics has certainly been eroded. There is a very strong interplay between the two fields now and they are mutually beneficial.

I: The physicists do not seem to be very happy about that.

M: But there are some brilliant exponents like Michael Atiyah who started in pure mathematics but who gradually embraced theoretical physics. Maybe that's what the physicists are not too happy about, but it's a fact of life. Theoretical physics is a close partner of applied mathematics. My department back home is the "department of applied mathematics and theoretical physics", reflecting that these two disciplines are separate but related; the boundary is flexible!

I: I think that in the US applied mathematics is more about applications outside the physical sciences.

M: Perhaps you have in mind applications to economics and the like. That is possible, but Avner would dispute this, I think. Perhaps the interpretation of the term has changed in the United States. It is difficult, I agree with you. Someone in the States working in fluid mechanics would be more likely to be attached to a department of engineering than a department of mathematics. It is regarded as being more within the ambit of engineering. In Britain, we succeeded in keeping this kind of applied mathematics – fluid and solid

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mechanics – within faculties of mathematics. In this way, top mathematics students can be more easily attracted to the subject.

I: Did you have many graduate students during your career?

M: I did have a steady stream of graduate students and this is one very rewarding aspect of an academic career. I have been very fortunate to have had many good graduate students in my time. They are all good but some of them are absolute stars – people like Juri Toomre, Andrew Soward and Michael Proctor, who have done extremely well in their subsequent research careers. It is rewarding to work with graduate students, when mentoring develops progressively into collaboration. That is why it is so satisfying and it keeps one young. The new students coming in, of course, get younger and younger! The fresh excitement with every new graduate student is something very rewarding.

I: You succeeded Sir Michael Atiyah as the Director of the Isaac Newton Institute for Mathematical Sciences in Cambridge. What is your most satisfying accomplishment during those five years as Director?

M: I inherited from Sir Michael a very exciting development – the Newton Institute was 5 years old when I took over – the appointment as Director is for five years. The primary role of the Director is to maintain a high level of scientific visitor research programs and I think I did this during my 5 years, and this still continues. But as well as maintaining the high scientific level, the Director has the responsibility to maintain the financial health and viability of the Institute, and that is not easy. I was very much concerned with the financial health for the longer-term future of the Institute. I was in constant debate, and indeed argument, with our National Research Council (EPSRC) to maintain an adequate income level, and equally with Cambridge University and its Colleges for internal support. I was also constantly seeking to raise money from external private sources, and I was reasonably successful in this. During my time, we won one of the Queen's Anniversary Prizes for innovation at the Newton Institute, on behalf of Cambridge University. And to celebrate the millennium year 2000, we produced a series of 12 posters to demonstrate the enormously wide scope of applications of mathematics. They were reprinted by World Scientific in Singapore and have been widely distributed in Southeast Asia as well as in Europe. We have reproduced them in this little booklet. It gave a fair spectrum of the applications of mathematics. That was quite exciting and involved a lot of work during 1999 and 2000.

I: Were they distributed to the schools?

M: Yes. They were designed for display in the trains of the London Underground with the general title "Maths goes Underground". Each month a new set of posters appeared in the trains. After that they proved popular and there was great demand from schools. So we reprinted and distributed to all schools and universities in the country. They were all over the place.

I: Our own Institute for Mathematical Sciences is modeled partly after the Isaac Newton Institute. What do you think are the similarities and differences between these two institutes?

M: I was first aware that IMS was to be modeled to some extent on the Newton Institute when your Deputy Prime Minister, Dr Tony Tan, visited the Newton Institute in 1998. He came with Louis Chen, looked carefully at what we were doing, and we had a long discussion. This is how I became involved in IMS. There are similarities – the idea of holding programs and bringing in visitors from overseas, this is at the heart of the business of any visitor research institute: short-term programs of up to 6 months duration, with as many distinguished visitors as you can attract to come and engage in research, and interact with the local community. That is very much the spirit of the Newton Institute also. As regards the differences, the Newton Institute has a wider catchment area – the whole of Europe is at its doorstep. Many of the participants and many of the young postdocs and graduate students come from Europe. It's now very easy for Europeans to fly into Stansted Airport near Cambridge from anywhere in Europe. So there is a very large community there. It's not only Europe, of course. We have many visitors from the United States and from all over the world, but primarily you look to your local community. Of course, Singapore has a strong local community but it is relatively small. You have a wider Asian community. I think that IMS must regard itself as a beacon for that community, and extending to Australia. If you look at the globe, you can see that Singapore can be an attractor in a certain area. You have currently tremendously strong growth from China in particular.

I: But the local scientific level is lower …

M: Well, perhaps, but the ambition of IMS must be to raise that level to reach equality with the institutes in Europe and the United States. I think it's doing very well in that respect. The level of the programs here has been high. I think it is more difficult to maintain that level of activity here given the geographical isolation and the fact that you've got a smaller community in Singapore itself. The first 5 years is always easy. The second 5 years and the third 5 years – to maintain sustainability – will be more difficult. There is a danger of running out of steam, you know. There is quite a

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problem there of just keeping it going at the required level of intensity.

I: Has this to do with the "critical mass" needed?

M: Yes, it is a question of critical mass. There is also the question of diversification. "Mathematics and its applications" has to be interpreted in a broad sense. Any area of mathematics and its applications may be a potential area for a good concentrated program. The programs here tend to be of a shorter duration. It's difficult to get people to come and stay for more than a month or two. They'll come for a couple of weeks or one month for workshops, but to stay for a longer extended period is quite difficult. To maintain a research activity for up to 6 months is not easy. Two months seems to be workable and a good compromise. If IMS runs 4 or 5 programs in a year, each of two months' duration, I think that's excellent and can work very well.

I: Do you think we should be focused on certain topics rather than spread out over a large number of areas?

M: Well, despite what I just said, I do think it's good to focus here on topics that are most relevant to Singapore – local problems. It's interesting that environmental problems are emerging as one of the key areas. Environmental fluid mechanics is important in relation to problems of pollution, and problems relating to natural hazards. That tsunami was so close to Singapore that it must have been a matter of great concern here. Phenomena relating to extreme weather conditions are of ever-increasing concern, and these fall well within the scope of mathematical investigation. And then there is the whole vast field of biomedical science – another area in which Singapore can make great contributions. This is an area also where mathematics can play a vital underpinning role.

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