IMPRINTS
DECEMBER 2004
NUS National University of Singapore
INSTITUTE FOR MATHEMATICAL SCIENCES

Inaugural Mathematics Camp >>>

On 8 June 2004, within the limited and comparatively spartan grounds of the Institute for Mathematical Sciences, modern mathematics opened its doors a little and offered a glimpse of some of its trade secrets to a group of 30 bright-eyed, eager-to-learn students from Raffles Junior College and Victoria Junior College. It was both an initiative and an experiment by the Institute to venture into an area where "angels fear to tread" – the promotion of mathematical education at the school level.


William Abikoff: Deluge after lecture

The Institute is coming up with new plans to organize in each year several educational projects, called "Math Camps" for want of a better name, that it hopes will attract curious, and perhaps scientifically and mathematically uncommitted, but potentially creative minds biding their time in the schools and junior colleges.

As Director Louis Chen said in his welcome address at the inaugural Math Camp, its purpose is "to provide enrichment rather than acceleration in mathematical learning". There are no tests, no ranking, no pressure to perform, but there are plenty of problems – old and new, solved and unsolved – that will introduce the uninitiated mind into the realm of mathematical research. There is no fixed syllabus, but there is an abundance of knowledge – classical and modern – that is rarely glimpsed at except by the initiated. At the national level, the objective of the Math Camps is "to promote the long-term health of mathematical education in Singapore by broadening and deepening students' understanding of mathematics", and at the vocational level, "to encourage talented students to pursue careers in the mathematical sciences."

For a start, the program of the inaugural camp was a modest less than one day affair. It started at 9.00 am with a short word of welcome by the Director, followed by a 40-minute talk on "Symmetry in geometry and physics" given by William Abikoff of University of Connecticut. For our young audience, it opened up a totally new realm of mathematical ideas both intuitive and precise. Significantly, the genesis and development of the central ideas of symmetry were due to a then much-misunderstood and under-estimated twenty-something genius, by the name of Evariste Galois, caught up in the political maelstrom of France of about 150 years ago.

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From the Editor >>>

If I remember correctly, the only time when there were more female undergraduates than male undergraduates in the Faculty of Science of the University of Singapore and its successor (NUS) was during a short period in the early seventies during the initial implementation of national service. But it did not take long before the boys (or rather men) came back to establish their traditional majority. At the mathematics honors level in NUS, figures for the last three semesters give a relatively "balanced" ratio of males to females of about 1.7. Even in the United States, gender-related disparity in the sciences is evident. One would have thought that such differences and inequalities would have been insignificant in the US. Yet in 2000, an NSF study showed that although the number of women in science and technology has increased since the 1960s, the number of women moving into the academic community remains low. More precisely, women made up only 19.5% of the science and engineering faculty in 4-year colleges and universities in the US.

The role and contribution of women in the scientific professions obviously require a long process in time, education and cultivation of mindset to be raised to a significant level. Historical reasons aside, there seem to be socioeconomic and even "sociopsychological" obstacles that have been identified, at least in the US. Why is it that "balancing work with family" is hardly mentioned as an obstacle for men scientists? One obvious reason that comes to mind is that men do not need to go through pregnancy and its aftermath in order to raise a family. Changing mindset, as it has been constantly pointed out, is not just changing the mindset of the affected group but changing the mindset of the whole population to believe in the equality of potential and talent in the sciences among men and women. Without this conviction, how could genuine encouragement be offered and how could women cross the threshold of acceptance in a male-dominated community?

There is one quality which is gender-blind, and that is passion for research. The message from the masters, male or female, for reaching the stars is consistent: there must be talent, an inspiring teacher, a conducive (not to be equated with physically comfortable) learning environment plus the most important ingredient – a passion for what one does. What one does is perhaps not so crucial – after all, every domain, as an old saying goes, has its masters. The abundance of opportunities at every stage of a student's educational path makes the final outcome even less predictable. And the path to one's niche domain is often non-deterministic and often serendipitous – no matter how much one believes in the exercise of optimizing choice in a free market economy. The paths of the masters are lighted up by lots of passion and a little bit of luck.

Y.K. Leong

People in the News >>>

Baby Isaac

The Institute's IT Manager, Sunn Aung Naing, is the proud father of a baby boy born on 30 August 2004.

Yeneng Sun, who served as the Institute's Deputy Director from 23 July 2001 to 31 July 2004, relinquished his Institute position to take his sabbatical leave at the University of Illinois at Urbana-Champaign and Stanford University in the academic year 2004/05. He is succeeded by Denny Leung from the Department of Mathematics.

San Yee Yeoh, an administrative officer of the Institute, left the Institute on 19 August 2004. Her duties have since been taken over by Cindy Tok who joined the Institute on 26 July 2004.

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Open book, inquiring minds

The "tea break" (or "coffee break", depending on one's taste) that followed in the Institute lounge was not the conventional cup of tea that students might have expected. They soon learned what "coffee breaks" often mean to active mathematicians in the leading centers of learning and research all over the world – brain-storming and more brain-storming. (It is perhaps not an exaggeration to say that many a coffee break should be called a "coffee breakthrough".) Students gathered in groups around four visitors to the

Programs & Activities >>>

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Institute's program on "Geometric Partial Differential Equations" – Abikoff, Zindine Djadli (University of Cergy-Pontoise, France), Andrea Malchiodi (Institute for Advanced Study, Italy) and Frank Pacard (Université Paris XII, France). It was indeed a close encounter of the most cerebral and challenging kind for those young minds from the junior colleges. Even the Director himself contributed his share of knowledge and encouragement.

After a buffet lunch at the Institute, it was again time to expand students' mental horizons with a talk by Frank Pacard on some geometric problems that were posed by the Greek mathematicians of antiquity more than two thousand years ago but that could only be solved relatively recently by modern mathematical ideas. It was a talk with interactive participation from the audience.

The break that followed again opened up more informal discussions and interactions between students and mentors. There were more new ideas to savor, more problems to tease the mind and more tricks glimpsed. Some seemed inconsequential, but the overall mathematical power was clearly demonstrated. By the time the camp was supposed to wrap up at 3.30 pm, there was unwillingness among some students to take leave – their mental curiosity and appetite have only been barely whetted. To quote some student feedback given afterwards, it was "enriching", "an amazing experience" and the "informal training sessions were brilliant" and "promoted curiosity and asking questions".

Fishing for bright ideas

Pointers from Frank Pacard

Past Programs in Brief

Final Part (August 2004) of the Program Mathematics and Computation in Imaging Science and Information Processing

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

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

This program was held in two parts: the first from July to December 2003 (see preceding issues of *Imprints*), and the second part in August 2004, which comprised two workshops, an international conference and a poster session. The concluding part continued to provide a fertile ground for exchange of ideas and collaboration (new and old) among local scientists and foreign visitors. Foreign participants came from United States, Norway, New Zealand, Denmark, France, Germany, China, Canada, Israel, Hong Kong, Taiwan, Brunei and Malaysia.

A workshop on "Functional and Harmonic Analyses of Wavelets and Frames" was held from 4 to 7 August 2004 and attended by 37 participants, of whom, 17 came from overseas. The tutorial speakers were Palle Jorgensen (University of Iowa) and David Larson (Texas A&M University).

An international conference on "Wavelet Theory and Computation: New Directions and Challenges" was held from 10 to 13 August 2004. It presented state-of-the-art reports by experts and leaders in the field on recent developments in the ever-expanding applications of wavelets to computation, imaging science and information processing. It also attracted 83 participants, of whom, 52 came from overseas.

A Joint Workshop on Data Representation was held from 16 to 20 August 2004 jointly with the Center for Wavelets, Approximation and Information Processing (CWAIP) and the Center of Ideal Data Representation (IDR), the latter comprising top U.S. universities such as Princeton University, Stanford University, University of Wisconsin-Madison and others. It dealt with the two themes: (i) subdivisions in computer graphics, (ii) wavelets in statistical data analysis. The tutorial speakers were Emmanuel Candes (California Institute of Technology) and Denis Zorin (New York University). A total of 44 scientists and researchers participated in the workshop.

A public lecture on *"The romance of hidden components"* was given by David Donoho (Stanford University) and attended by 105 people.

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Some feedback from visitors:

"I really enjoyed my trip in Singapore and was very impressed with the organization of the workshop and of my stay. I had numerous great conversations with many participants. The thing that made it special is that I rarely felt "so well taken care of." So I would like to thank the staff of the IMS for truly letting the participants feel so welcome and making everything possible so that we can concentrate on sharing our ideas. Even though Singapore is on the other side of the world, I will be happy to take any opportunity to visit the IMS again, and participate in your exciting programs. You have created quite a unique place."

"Excellent, I really enjoyed the bright offices; computer facilities were set up in advance and were working perfectly. I was able to really work. Excellent atmosphere around the institute!"

Image enhancers

Imaging: A never-ending story

Amos Ron: Championing Caplets

What's that blurred image?

David Donoho: Romancing hidden variables

Emmanuel Candes: Harmonising statistical estimation

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

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

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

Activities:

a) Seminars and Tutorials

Details at http://www.ims.nus.edu.sg/Programs/pdes/activities.htm#semtut

b) Workshop (28 May to 3 June 2004)

Details at http://www.ims.nus.edu.sg/Programs/pdes/activities.htm#wk

This program focused on important mathematical aspects of geometric analysis and nonlinear partial differential equations arising from geometric questions, especially those related to the scalar curvature, Q-curvature and Sigma curvature problems. New methods and techniques for such equations were presented and applied to geometric and topological problems. The program attracted more than 30 international mathematicians, most of them from Princeton University, Paris University XII, Australian National University, University of Tokyo and Beijing University.

The activities consisted of seminars, tutorials and a workshop. Tutorial lectures were conducted by Alice Chang (Princeton University), Thomas Branson (University of Iowa), Neil Trudinger (Australian National University) and Frank Pacard (Université de Paris XII) on the scalar curvature equation, Q-curvature equation and its recent development, fully nonlinear partial differential equations and its geometric application, and conformal invariant operators and their updated progress. The workshop was held from 28 May to 3 June and attended by 28 participants. Reports on their recent work were given by 17 participants.

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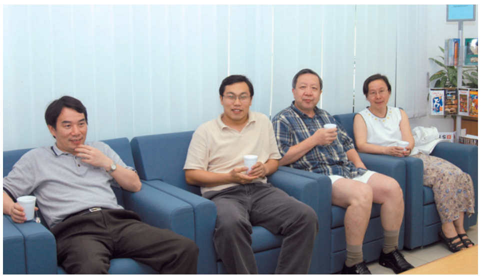
Some feedback from our visitors:
"The tutorial lectures which I have attended are all excellent! I have asked to have the lecture notes to bring back to Princeton for students to read!"

"I had discussions with a large number of colleagues during my visit – some local and some visiting members of IMS. We had discussions during and after the lectures, at offices, during lunch, tea hours and other informal meetings! I think we have all benefited a lot from these professional contacts."


2-in-1 coffee mix: Analysis + geometry


More brain storms


Weiyue Ding: Softly flows the Schroedinger

Current Program

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

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

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

This program consists of seminars, tutorials and workshops on the following sub-themes:
(a) Computation of turbulence I (13 – 15 July)
(b) Computation of turbulence II (3 – 5 August)
(c) Turbulence at a free surface (27 – 28 October)
(d) Transition and turbulence control (8 – 10 December)
(e) Developments in Navier-Stokes equations and turbulence research (13 – 16 December).

Tutorial lectures were given by Timothy Craft (University of Manchester Institute of Science and Technology), Hector Iacovides (University of Manchester Institute of Science and Technology) and Pierre Sagaut (LMM - University of Paris VI (Pierre et Marie Curie)/CNRS).

Next Program

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

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

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

This is an interdisciplinary program for researchers in materials science, physics, applied mathematics and computational science. The following have agreed to conduct tutorials: Qiang Du (Penn State University), Qi Wang (Florida State University), Chun Liu (Penn State University) and Robert Pego (Carnegie Mellon University).

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

Programs & Activities in the Pipeline

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

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

Co-chairs:
Artur Ekert, *University of Cambridge*
Choo Hiap Oh, *National University of Singapore*
Kok Khoo Phua, *South East Asia Theoretical Physics Association and National University of Singapore*

Jointly organized with Department of Physics. The tentative list of invited speakers comprises Yakir Aharonov* (Israel); Hans Briegel (Innsbruuck); Mo-lin Ge (Nankai); Daniel

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Greenberger (CCNY); Gerald Milburn (Queensland); C.P. Soo (NCKU) and Reinhard Werner (Braunschweig).
(Subject to further confirmation)*

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

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

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

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

Workshop on Data Analysis and Data Mining in Proteomics (9 - 12 May 2005)

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

Co-chairs:
Maxey C.M. Chung, *National University of Singapore*
Newman S.K. Sze, *Genome Institute of Singapore*

Confirmed Overseas Speakers
- Vineet Bafna (University of California, San Diego)
- Jacques Colinge (GeneProt Inc., Switzerland)
- David Creasy (Matrix Science, UK)
- Paul Eilers (Leiden University, Netherlands)
- Athula Herath (Nestle Research Center)
- Neil Kelleher (University of Illinois at Urbana-Champaign)
- Andrew Keller (Institute for Systems Biology, USA)
- Bin Ma (University of Western Ontario)
- Peter Roepstorff (University of Southern Denmark)
- Rovshan G. Sadygov (Thermo Electron Corporation)
- Richard Simpson (Ludwig Institute for Cancer Research, Australia)
- Marc Wilkins (Proteome Systems Ltd., Australia)

With the completion of the genome sequences of many species, their proteomes can be inferred from the sequence analysis. Together with the availability of high throughput mass spectrometry based protein identification technology to study protein expression, post-translational modification and interaction in global scale, enormous amount of data have been generated everyday. The analysis and understanding of these data is a huge challenge at this moment. In this workshop we wish to examine and discuss some of the problems existing in data analysis and data mining in proteomics through a series of seminars and discussion papers given by eminent international and local speakers.

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

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

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

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

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

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

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

Asian Mathematical Conference 2005 (20 – 23 July 2005)

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

Chair (International Scientific Committee):
Kenji Ueno, *Kyoto University*
Chair (Steering Committee and Organizing Committee):
Eng Chye Tan, *National University of Singapore*

Mathematical Modeling of Infectious Diseases: Dynamics and Control (15 August – 9 October, 2005)

Website: to be announced

Chair:
Bryan T. Grenfell, *University of Cambridge, UK*
Co-chairs:
Stefan Ma, *Ministry of Health, Singapore*
Yingcun Xia, *National University of Singapore*

The impact of infectious diseases on human and animal is enormous, both in terms of suffering and in terms of social and economic consequences. Mathematical modeling is an essential tool in studying a diverse range of such diseases. Effective prevention and control of the diseases need collaborations between mathematicians, statisticians, epidemiologists, biologists and medical scientists. This program focuses on dialogue and bridging the gaps between those researchers.

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Activities:
(a) New development of the SEIR models for the transmission of infectious diseases (August 15-21, 2005)
(b) Influenza-like diseases (August 22-28, 2005)
(c) Immunity, vaccination, and other control strategies (September 5-11, 2005)
(d) Molecular analysis of infectious diseases (September 12-18, 2005)
(e) Clinical and public health applications of mathematical modeling (September 26-October 2, 2005)

Semi-definite Programming and its Applications (15 December 2005 – 31 January 2006)

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

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

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

Highlights of Other Activities

IMS Math Camp (8 June 2004)

IMS organized its inaugural Math Camp for 30 students from Raffles Junior College and Victoria Junior College. Short talks and informal discussions were conducted by Institute visitors William Abikoff (University of Connecticut, USA), Zindine Djadli (University of Cergy–Pontoise, France), Andrea Malchiodi (Institute for Advanced Study, Italy) and Frank Pacard (Université Paris XII, France)

Workshop on Mathematical Logic and Its Applications (17 - 18 June 2004)

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

The workshop was organized in conjunction with the visits of Peter Loeb (University of Illinois at Urbana- Champaign), Ted Slaman and Hugh Woodin (both of University of California at Berkeley) to IMS and Department of Mathematics. Professor Loeb is well known for his work in nonstandard analysis and its applications, while Professors Slaman and Woodin have made fundamental contributions to recursion theory and set theory. In addition to the three visitors from the US, the speakers at the workshop included Qi Feng (Institute of Mathematics, Chinese Academy of Sciences and NUS), Ansheng Li (Institute of Software, Chinese Academy of Sciences), Guohua Wu (Victoria University of Wellington), as well as Chi Tat Chong, Yeneng Sun and Yue Yang from NUS.

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

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

Jointly organized with Institute of High Performance Computing (IHPC), Faculty of Engineering and Faculty of Science, the conference provided a multidisciplinary forum for scientists and engineers who actively use computers in their research. It was held at the Grand Copthorne Waterfront Hotel.

The 6th International Chinese Statistical Association (ICSA) International Conference (21 – 23 July 2004)

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

Jointly organized with the Department of Statistics and Applied Probability, the conference was attended by about 200 participants from 20 countries. The plenary speakers were David Siegmund (Stanford University), Kung-Yee Liang (National Health Research Institute, Taiwan and Johns Hopkins University) and Jianqing Fan (Princeton University).


Logic minders


Qi Feng: Of mice and logic


Hugh Woodin: Playing real games of logic

Mathematical Conversations

Alice Chang : Analyst in Conformal Land >>>

Alice Chang

Interview of Alice Chang by Y.K. Leong

Sun-Yung Alice Chang is well-known for her many important contributions to real harmonic analysis, geometric analysis, nonlinear partial differential equations and applications of analysis to problems in differential geometry. In 1995 the American Mathematical Society awarded her the Ruth Lyttle Satter Prize in Mathematics (awarded every two years to a woman for outstanding research in mathematics) for her deep contributions to the study of partial differential equations on Riemannian manifolds.

Born in Xian, China, she grew up in Taiwan and had her undergraduate education at the Taiwan National University in Taipei and her PhD at the University of California at Berkeley. She has taught at the State University of New York at Buffalo, University of Maryland and University of California at Los Angeles before moving, in 1998, to Princeton University where she is a full professor.

She has given invited addresses at major mathematics meetings and conferences, including a 45-minute talk at the International Congress of Mathematicians (ICM) at Berkeley in 1986, a one-hour plenary talk at the ICM at Beijing in 2002 and an AMS Colloquium talk in 2004. In 2001 she gave the Emmy Noether Lecture of the U.S.-based Association for Women in Mathematics. She has served as editor of several leading mathematical journals and was Vice-President of the American Mathematical Society from 1989 to 1991. In 1988, she received the Outstanding Woman of Science Award from University of California at Los Angeles. Her life and work is a fine example of what women are capable of achieving in mathematics and has set an inspiring role model for women pursuing careers in the scientific field. Her husband Paul Yang is also her long-term collaborator in mathematical research, and they have a son and a daughter.

The Editor of *Imprints* interviewed Alice Chang at the Institute on 12 June 2004 during her visit to give invited lectures at the program on "Geometric Partial Differential Equations". The following is a vetted account of the interview in which she talked about her school years, her fascination with and devotion to mathematical research, and her views about the need to encourage women of talent in mathematics.

Imprints: Was mathematics your first career choice when you were at university?

Alice Chang: Yes, it was. In Taiwan there is an entrance examination for college, but I was one of the small percentages of students who did not need to take the entrance exam. I was one of the "pao-song" (literally, "guarantee send" in Chinese), the people in each high school who can choose which college to attend without taking the entrance exam. The positions are allocated according to class standing. I was ranked first in my high school. I had my college education in Taiwan National University in Taipei. In high school, I liked both Chinese literature and mathematics. In college, I decided to major in mathematics.

I: You started as an analyst but you are now interested in problems about geometry. Do you consider yourself to be an analyst first and then a geometer, or the other way around?

C: I consider myself to be an analyst first and a geometer second because of my background and the way I think about mathematics. So basically I am an analyst and now I am working on problems which are very geometric in nature. Fortunately, I have other co-authors who are more of a geometer than an analyst. So we cooperate with each other. I always think of myself as an analyst.

I: There have been many recent developments at the interface between geometry and partial differential equations. When and how did the interaction start?

C: It has a long history. The interaction between geometry and partial differential equations (or between geometry and analysis in general) is most natural. I would say it started even in the nineteenth century. Geometers like Poincaré already used the analytic approach to study problems in geometry in the late 1890s. There are also the geometers of the previous generation, like S. S. Chern, Atiyah and Singer, who laid the foundations for approaching problems in geometry using analytic methods. There has also been pioneering work by contemporary people like Nirenberg, Uhlenbeck, Schoen and Yau.

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I: Partial differential equations are analytic in nature.

C: Yes. You first study the problem in the plane domain and then you study the problem in Euclidean 3-space and it is most natural to study it in higher dimensional space. There the curvature and the geometry come in.

I: It seems that geometry is becoming very analytical in nature. Is there any intrinsic geometry that is involved or is it just a matter of using the language of geometry? Does it involve any geometric intuition per se?

C: I wouldn't put it that way. I think that analysis is a tool in studying geometric problems. Geometry provides concrete examples for studying some problems at the interface between geometry and analysis. The geometric objects are concrete examples for problems in analysis. For example, you want to know what happens on the sphere – that is a concrete model. You have the analysis which is abstract analysis – convergence, weak convergence. To apply the abstract theory you need concrete examples. Geometric objects provide those examples. Of course, it is intrinsic geometry that is involved. And geometric intuition plays an important role in the approach. Analysis is sometimes a tool.

I: Geometric intuition is not something that everyone has. Is that so?

C: That is true, but on the other hand, I think that everybody has some type of geometric intuition. A problem would not be natural without the geometric intuition.

I: There are some famous mathematicians who are able to look at geometric problems and see the results even before they can prove them. This must have involved a lot of geometric intuition.

C: Yeah, yeah. If you are talking about 2-dimensional problems, maybe some people have more intuition than other people. It could also be a way of training, from their background, from the way they see things. It is true that some people have more intuition than others.

I: Is it possible to develop such geometric intuition?

C: I think so. For example, a lot of analysis problems need a lot of intuition. It's not just geometric problems that need intuition.

I: But analysis is more axiomatic.

C: Yes, it is more structured and more systematic. You are trying to derive formulas to solve a problem. However in the direction of approach to the problem - in most times, you also need intuition. You need to have some picture in your mind in both geometry and analysis. Maybe more so in geometry.

I: Have you applied your mathematical ideas to problems in physics or other scientific areas?

C: I hope to do so in the future. At the moment, no. Some of the problems I am working on are related to problems in mathematical physics. Sometimes I do read the literature in mathematical physics and see the interaction between the problems I'm working on and developments in mathematical physics. But so far, I have not applied my results to problems in that direction.

I: With the advent of computers of increasing power, and since computers operate in an essentially discrete domain, do you think that it will be necessary in the future to "discretize" geometry in order to make full use of the power of the computer?

C: I think it's the other way around. Let me explain. I'm saying that geometry has always been developed from approximation. This concept of a discrete approach to geometry has been there at the beginning. People took that approach not because of computers. For example, even in the old times people already think of the circle in terms of approximations of polygons as the number of sides gets larger and larger. The discrete approach to geometry was there before the computer. But now using computers, it's easier to take large data sets and test them.

I: Has there been much influence of the computer on the theoretical development of geometry? Is there such a thing as "computational geometry"?

C: Of course, there has been influence of computers on the theoretical development of geometry. For example, you can now construct minimal surfaces using computational methods to generate approximations and more examples. So it has a lot of influence on many areas of mathematics. But on the other hand, this computational method will never replace abstract thinking or imagination. You first need to have the idea of something that happens, and then you use the computer to test the intuition. I always think of the computer as a tool and it cannot replace the abstract thinking and intuition.

I: What are your favorite pastimes when you are not doing research?

C: I like to take walks and ride my bicycle. I like to read novels and I enjoy classical music. When I was young, I played a little bit on the piano, but not now.

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I: Mathematics has traditionally been a male-dominated activity. From your experience, did you encounter extra obstacles in your mathematical career? How do you think we could encourage more female students in the university to take up mathematics?

C: It is true that mathematics has traditionally been a male-dominated profession especially at the research level. But on the other hand, I think this is due to reasons which are more – how do I describe it – social issues because women used not to have time to devote to any career. This profession could be a good profession for women. It requires a lot of thinking and you have to be very calm and patient and willing to think through things. I don't think that it should be a male-dominated profession. It's only for historical reasons. From my own experience, I think the main obstacle was that when my children were young, I felt I did not have enough time to do the work I liked to do. This is probably true for any career woman in any profession, not just for mathematics. I think this is a profession quite suitable for women in the long term. Mathematics could be done at any time. You can choose the subject you want to do research in.

I think there should be more women faculty to serve as role models. It's very hard for a woman to think that this is a possible career if the faculty in a department are all men. You do need role models. Also, I think you should encourage women. Let it be known that this profession is suitable for women. This is a problem faced by many departments in the US and around the world. It is very difficult to increase the women faculty. There are no graduate students who are women. How do you increase your women faculty? You have to reach certain standards. But this should be gradually changed with more and more women getting into graduate school and then there would be more and more women in the pipeline for assistant professors. They should be encouraged. It's a complicated issue. First, one has to understand that the intellectual abilities of men and women are the same. There must be confidence to encourage women to get into the profession.

I: What advice would you give to a beginning male or female graduate student in mathematics?

C: First, you have to be really interested in mathematics to be a graduate student in mathematics. It's a long-term commitment. You have to think that this is not temporary – you get your PhD and then … You have to be devoted and really like the subject to be a graduate student in mathematics. The other thing is that, I feel, nowadays there is a lot of information (the world is changing very fast) through the web and conferences and so on. Maybe it's more difficult for young people to really quiet down and think through a subject more deeply - the fundamentals, the basic background. I think young people should not be pushed by fashion and should not be forced to be very quick and have a lot of publications. They should instead think more quietly and think through the foundations of the subject.

I: Of course, mathematics has a lot of competition from more lucrative subjects like economics, computer science, financial mathematics.

C: The competition is always there, of course. First, you must have a real love for the subject, so you really want to understand something. So you are willing to devote your time and take the long-term approach.

I: Also, mathematics is a very demanding subject.

C: I think if you want to do well in any subject, it is demanding. If you want to be a good musician, a good painter, a good economist or to know your subject well, it takes a lot of devotion.

I: Can you tell us something about the latest work you are working on?

C: I'm working in a field called conformal geometry. Recently we are trying to use fully nonlinear partial differential equations to study patterns in geometry; in particular, the latest project I'm involved in is to classify a certain type of 4-manifolds up to diffeomorphism using the analytic approach.

I: Do you think collaboration is very important for mathematical research?

C: It is. You learn much faster by talking to other people. For me, I'm an analyst and, in my project, it's important for me to talk to geometers. In this case, fortunately for me, one of my main collaborators is my husband, Paul Yang, who is a geometer. I think it's very important to collaborate and talk to other people.

I: Papers written by single authors and papers written with other authors are often given different weights. What is your view on this?

C: For papers written by yourself and papers written with others, the weights should be a little different. If you have to do everything by yourself, the weight should be heavier because the speed will be slower. If you have collaborators, you could have more papers. When you work alone, you may have only one paper. If you have collaborators, you may have two or three. Even if you count it as one-half, it's still fair. However, the quality of a person's papers is more important than their quantity.

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David Donoho: Sparse Data, Beautiful Mine >>>

David Donoho

Excerpts of interview of David Donoho by Y.K. Leong

(Full interview at website: http://www.ims.nus.edu.sg/imprints/interview_donoho.htm)

David Donoho is world-renowned for many important contributions to statistics and its applications to image and signal processing, in particular to the retrieval of essential information from "sparse" data. He is reputed to be the most highly-cited mathematician for work done in the last decade (1994–2004) — a reflection of the impact of his work on engineering and the physical and medical sciences.

He has received numerous honors and awards, notably the Presidential Young Investigator Award and the Presidents' Award (of the Committee of Presidents of Statistical Societies). He is a member of the National Academy of Sciences, USA, and the American Academy of Arts and Sciences. He has been invited to give prestigious lectures of scientific bodies, such as the Wald Lecture and the Bernoulli Lecture, and at the International Congress of Mathematicians. He has served on the committees of professional scientific bodies and on the editorial boards of leading journals on probability, statistics and mathematics.

The Editor of *Imprints* interviewed him at the Department of Mathematics on 26 August 2004 when he was a guest of the Department of Mathematics and the Department of Statistics and Applied Probability from 11 August to 5 September 2004 and an invited speaker at the Institute's program on image and signal processing. The following is an enhanced and vetted account of excerpts of the interview. It reveals little-known facets of his early scientific apprenticeship in the primeval and almost unreal world of computer programming and data analysis of the seventies. He talks passionately about the trilogy of attraction and fascination with computing, statistics and mathematics and about the many statistical challenges and opportunities arising from the exponential growth in data collected in all branches of human knowledge.

(**Acknowledgement.** *Imprints* acknowledges the efforts of Ms Tombropoulos of Stanford University in the preparation of the final version of the interview.)

Imprints: How did you come to be interested in probability and statistics?

David Donoho: When I left for college, my father suggested I get a part-time job where I'd learn computer programming. The employment office at Princeton sent me to the Statistics Department. I went to work for a professor (Larry Mayer) doing statistical data analysis of household energy use; this taught me to use statistical computer programs. Through this I gradually became very interested in computers and also in data analysis. At the same time, I was taking mathematics courses. I saw that a career in statistics would let me do mathematics and use computers to analyze data. By the end of my first year I was hooked; I remember that I started pulling all-nighters hanging out at the computer center already by spring term.

I: Why would you need to stay up all night?

D: In those days, computing was much more difficult than today. It was a major effort to translate your program into a physical form (punch cards) acceptable to the computer. It was a long wait for the computer to process your work, and then often it would just spit out something like "IEHK6040 Job Control Language Operand Agreement Error". You had to be very persistent to get things done. Sometimes it would just take all night.

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I: Knowledge is now so broad that it is quite natural for research to become more like a collective activity.

C: But I also want to say that in the evaluation system, we should leave room for people to work alone. Some of the fundamental work requires deep thinking and a long-term commitment. Some people may want to work on their own project rather than in collaboration with others and be obliged to listen to other people's opinions and be influenced by them. In the mathematical community, we should leave room for people who want to do work in their own way. Mathematical research is not just a scientific approach; the nature of mathematics is sometimes close to that of art. Some people want individual character and an individual way of working things out. They should be appreciated too. There should be room for single research and collaborative research.

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I: What was the attraction?

D: Today it's all the rage for young people to do "Extreme Sports"; triathlon, bungee, and so on. The point is the sheer exhilaration of taking on a daunting challenge and prevailing. Computing in those days was a kind of extreme intellectual sport. To get over all the physical and intellectual hurdles was really an achievement. By comparison, computing today is like jogging, or maybe just a brisk walk around the block.

I: What came next?

D: Computing improved very rapidly. The next year, the Statistics Department got a PDP-11 minicomputer — one of the first Unix computers outside of Bell Labs. Don McNeil and Peter Bloomfield gave me a job: to build up all the statistical software that was needed. I had to program linear regression and graphics, and I also had to provide graphical displays from exploratory data analysis. I had to learn C (a totally new language in those days) and even assembly language programming. I had to learn all the basic algorithms for statistics and numerical analysis, and implement and test things. At times I practically lived in the computer room almost 24 hours a day. I remember very clearly the room — named after Princeton statistician Sam Wilks. There was an oil painting of Wilks, the minicomputer, Wilks' personal books, the old-fashioned teletype terminals, the fancier pin writer terminals, the tape drives, the clack-clack, buzz-buzz sounds everything made. I remember all the strange things that came with staying up late at night working on the computer in this place. Sometimes the computer users held "afternoon tea" at 3 am in the Math lounge!

I: Tell us about the computing tools you were developing.

D: The computer package was called ISP (Interactive Statistical Package). In addition to regression and data manipulation, it had all the basic tools of Exploratory Data Analysis. John Tukey's book Exploratory Data Analysis was just being published during my Junior year in college. Later, the software was used at hundreds of universities, especially after people at UC Berkeley took it over, revamped it, renamed it as BLISS and worked on it as their daily bread. Gradually, this got displaced by the S and R languages.

I: What were your statistical interests?

D: In those days, robust statistics — being able to cope with small fractions of really bad data — was a big deal. For my Junior Paper and Senior Thesis, I immersed myself in Annals of Statistics papers on robust statistics. Many researchers were interested in knowing the "right" score function to use in a robust (M)-estimator. For example, my Senior Thesis adviser John Tukey had proposed the "biweight" score function which I had programmed into the ISP software. I studied the notions of minimax optimality due to Peter Huber; you play a game against nature where you pick the score function, and Nature decides how to contaminate the data with outliers. I formulated and solved the problem of unimodal contamination. Years later Jim Berger (now at Duke) came across an equivalent problem from a different viewpoint (Bayes Decision Theory) and published an equivalent solution.

I: I believe you had some industrial experience after college. Can you tell us something about that?

D: I lived at home in Houston and worked for the research labs of Western Geophysical on problems in signal processing for oil exploration. It was after the second oil shock of the 1970s; there was tremendous interest in the oil business in finding new oil and developing new imaging and signal processing methods. I was assigned to work on what seemed (for the time) massive imaging problems. They had to fit a linear model with thousands of unknowns and tens of thousands of observations. This was huge for 1978; they used the largest mainframe computers of the day, filling up rooms the size of basketball stadiums run by hundreds of people in white coats. Computer jobs had to run for weeks to produce a single image.

The key point was that the data were actually of very bad quality, with many outliers; since I knew robust estimation inside out, I showed the geophysicists how to do robust regression. They were very eager; it took only days for a senior researcher to take it on himself. The result, immediately, was a much clearer picture of the subsurface. Western right away wanted to make this into a product and send me to conferences to speak about it. They sent me to London for an extended stay and I made presentations to the chief geophysicist at ARAMCO and developed further ideas about image and signal processing for geophysical signals. My first scientific paper came out of that work; it studied the problem of "blind deconvolution" where a signal has been blurred, but you don't know in what way it has been blurred, and you want to sharpen it up. That's a problem of real interest today. I worked on that in early 1979.

I: Was that after your PhD?

D: No, just the Bachelor's degree. I wrote a paper on blind deconvolution which I finished in early 1980, and it turned out that three papers on this appeared about the same time — one in automatic control, one in astronomy and my paper. About ten years later, such techniques began to be used heavily in digital communications (e.g. mobile phones). A big review paper in Proc. IEEE referred to my paper prominently, in the second paragraph.

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I: How did you feel about that?

D: I felt lucky to come to the problem as a statistician, which gave me a broader view. I noticed connections between deconvolution and robust statistics, and saw that the key point was small departures from the Gaussian distribution. In robust statistics you viewed departures from a Gaussian error term as annoyances you want to protect yourself from. In signal processing, it was exactly upside down: you could view departures from Gaussian in the signal term as gifts from heaven, allowing you to recover the signal, against all odds! To explain this, I used things I had learned from the literature of robustness in the Annals of Statistics. My paper could actually be read by people in digital communications ten years later with some profit; I felt that if I had been working in a more narrowly defined subject matter, my papers would have aged more quickly.

I: How did you get your PhD?

D: I went back to graduate school at Harvard, and after finishing my paper on blind deconvolution I worked on robust estimation with high-dimensional data. I became obsessed with the idea that robustness was hard in high dimensions. Ricardo Maronna had shown that existing robust methods (like affine-equivariant M-estimates) could break down under fairly light contamination; I showed new approaches would be coordinate-free and avoid breakdown even with 49 per cent of the data completely corrupt. Werner Stahel did related work in Zürich at the same time.

It was beginning to dawn on people in those days that we should all be thinking about high-dimensional data. In the mid 1970s, Jerry Friedman and John Tukey had made a movie of PRIM-9, a system for looking at 9-dimensional particle physics data using 3D computer graphics. I remember the thrill I got when I saw that movie as a Junior in college (the Statistics Department at Princeton showed it to students to attract enrollment in the statistics major). My advisor at Harvard, Peter Huber, had been bitten by the bug and had gotten a very fancy-for-the-day computer — the Evans and Sutherland Picture System 2 — which could be used to look at three-dimensional objects from different angles and was shared with the Chemistry Department at Harvard. It had been developed for architecture and was being used by chemists to look at molecules. We used it to look at high-dimensional statistical data and we made movies to display our results — point clouds of statistical data, spinning around so you could sense their 3D structure. I gave talks at conferences and the high point of my talk was to show a movie of computer results; for presentation techniques, that was about twenty years ahead of the curve. I got to be a movie producer and screen writer!

At the same time I had to write statistical software for the VAX minicomputer that hosted the picture system; I remember working late into the night surrounded by chemistry grad students and postdocs. It seems that chemists were just not as weirdly interesting as mathematicians late at night. A lot of other magical things were not so magical either. The air conditioning was always mighty cold. The operating system was not UNIX, etc.

I: How did your industrial experience affect your later work?

D: In a typical academic career you get an advisor, get introduced to a problem and a specific field, become an expert, develop linearly. My industrial experience added a whole set of other interests to my academic portfolio. I'll give you an example. One thing that I learned in industrial research is that sparsity of a signal is a very important element in doing any kind of analysis of that signal. If you look at seismic signals, they are, in some sense, sparse. The reflectivity is zero a lot of the time with non-zero values relatively rare. And this sparsity was a fundamental constraint. I saw that seismic researchers were using sparsity to do surprising things that didn't seem possible. They were solving problems with too many unknowns and too few equations — somehow using sparsity to do that. Empirically, they were successful, but linear algebra would say this is hopeless. Over the years, that paradox really stuck with me. I felt that science itself involves too many unknowns and not enough equations and that often scientists are solving those equations by adding sparsity as an extra element. This somehow rules out the need to consider all the variables at once. In the last twenty years, I returned again and again to this theme of solving under-determined systems that seem horribly posed, and yet are actually not if you think beyond linear algebra, and use sparsity.

As a result, I worked frequently in applied math and information theory in addition to statistics. I have two careers, and this goes back to having worked in oil exploration.

I: Can you give some examples?

D: In seismics they have band-limited signals — that do not have frequencies outside a certain range — but they want to recover wide-band signals with all the frequencies that were not originally observed. It sounds impossible, and you can cook up counterexamples where it really is hopeless. But for signals that are sparse (most samples are zero and a few are non-zero) people were having good empirical success in seismology, and I worked with Ben Logan to prove that the problem is solvable. If you exploit sparsity, even though you only have band-limited information, you can recover a broad-band signal. Later, I considered the problem of representing a Signal which was made up as a linear combination of elements coming from

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more than one basis, say sinusoids and wavelets. It sounds impossible, since the underlying system of equations has n equations and $2n$ unknowns, so there can be no unique answer. With Xiaoming Huo, I showed that this problem could be uniquely solved if the signal was made up of any sufficiently sparse combination from the two bases, simply by singling out the linear combination having the smallest ℓ^1-norm.

I: Although we've talked in this interview about computing and robust statistics, we haven't talked much about theoretical statistics. Yet, you have worked in this area extensively. How did you get interested in that field?

D: My theoretical immersion started early, as my undergraduate thesis solved a problem in robust statistics basically calculus of variations. I published a few theoretical papers as a graduate student, and even when they concerned "practical topics" like signal processing, they were ultimately based on things I'd learned from the Annals of Statistics, the main Soviet probability journals, etc. Also, Persi Diaconis visited Harvard one year while I was there; he made it easy to believe that theory was where the fun was!

I was lucky enough to win a postdoctoral fellowship at MSRI, the mathematics institute in Berkeley. The other young visitors included Iain Johnstone, who had just joined the faculty at Stanford, and Lucien Birgé, now a professor at the University of Paris. Both were interested in decision theory — Iain, the exact finite sample "Charles Stein" kind, and Lucien, the asymptotic "Lucien LeCam" kind. I hadn't had much deep exposure to either, and my interest in such subjects really picked up. The long-run role of those interests in my career has been enormous.

My first academic job was in the Statistics Department at UC Berkeley. When I arrived there, I was equally interested in computing, data analysis and statistical theory. My career could in principle have gone in any one of several directions. I was immediately given the explicit advice "don't get swallowed up by the computer". A certain faculty member had been spending lots of time revamping the statistical software ISP that I had developed as an undergraduate. Some faculty told me directly that I probably would go back to my computing roots and get "swallowed up" in the same way. Another faculty member gave me the advice that if I wanted tenure, I should publish ten papers in the Annals of Statistics. So the message was clear: do theoretical statistics!

Peter Bickel and Lucien LeCam were very kind and patient in speaking to me about their own work and interests. These personal qualities supported me in doing what could have been very isolating work. Bickel and LeCam were also patient in listening to me explain the results of my own work, as were David Blackwell and Rudy Beran. There was a steady stream of visitors giving very interesting talks at Berkeley, with most talks emphasizing theory. It was said in those days that attending statistics seminars at Berkeley could be painful, because seminar speakers would often want to present the most challenging, abstract, and technical achievements of their life to date — meaning that some seminars would seem impenetrable. But I found them mind-expanding.

I: What's the attraction of theoretical statistics?

D: On the one hand, it's about exploring the boundary of what can be learned and what can never be learned from a given amount of measured data. On the other hand, it's about taking what scientists and engineers are inventing, and making loud claims about, and subjecting those claims to scrutiny. I sometimes feel that if we didn't have theoretical statistics, science would degenerate into a crass business of people claiming they can do the impossible from their datasets, without any fear of critical scrutiny. Finally, some ideas in theoretical statistics are just beautiful ideas, very intellectually rewarding, I think of Wald's decision theory itself, of Huber's minimax robustness theory, of Stein's insight on shrinkage in high dimensions, of LeCam's equivalence of experiments theory. You have to make a decision in Life about what ideas you want to spend your time with. These ideas wear well as constant companions.

I: According to the Institute for Scientific Information most-cited website, "incites.com/top/2004/third04-math.html" you are the most highly-cited mathematician for work in the period 1994–2004, with 23 highly-cited papers and well over 1500 citations to your work. Do you think that citation counts are important? How can statisticians increase their citation counts?

D: I'd like to emphasize that many of those papers are joint with my co-author, Iain Johnstone of Stanford. In fact he's number two in that list, close behind me. Statisticians do very well compared to mathematicians in citation counts. Among the top 10 most-cited mathematical scientists currently, all of them are statisticians. There's a clear reason: statisticians do things used by many people; in contrast, few people outside of mathematics can directly cite cutting-edge work in mathematics. Consider Wiles' proof of Fermat's Last Theorem. It's a brilliant achievement of the human mind but not directly useful outside of math. It gets a lot of popular attention, but not very many citations in the scientific literature. Statisticians explicitly design tools that are useful for scientists and engineers, everywhere, every day. So citation counts for statisticians follow from the nature of our discipline.

A very specific publishing discipline can enhance citation

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counts: Reproducible Research. You use the Internet to publish the data and computer programs that generate your results. I learned this discipline from the seismologist Jon Claerbout. This increases your citation counts, for a very simple reason. When researchers developing new methods look for ways to show off their new methods they'll naturally want to make comparisons with previous approaches. By publishing your data and methods, you make it easy for later researchers to compare with you, and then they cite you.

The important thing: do the reproducible research; don't worry about citations. My website has a paper on reproducible research giving the philosophy in more detail.

I: You have written that statistics is an "invisible" profession. Could you elaborate on that?

D: Many people don't even recognise that statistics exists as a discipline in academia. They are surprised when they hear that one can be a "Professor of Statistics" . Statisticians, in general, don't do public relations. I think we're all too busy. There are not enough statisticians to go around. The world is flooded with data; scientists, engineers and doctors all wanting to analyze their data. Outside every statistician's office in the world, there is a line of people waiting to get in to get some help with their data. Since we are completely over-subscribed, no one is out there advertising the existence of our profession. It is a sort of secret.

I: How do you select the problems that you work on?

D: This is the problem of life, isn't it? Some problems are urgent because many people are interested in them; I like to do those once in a while because of the challenge. I often look at articles in Science and Nature. When people write articles that make a big splash, I try to understand what they did and I either criticize it or build on it. So that's one angle. Another angle is to study some fundamental area of mathematics where a breakthrough just occurred, and to trace out implications in the real world.

I: In your 2004 American Statistical Association President's Invited Address, you spoke about missed opportunities for statistics. Could you elaborate?

D: Many fun problems in computer science could be attacked by statisticians, but statisticians don't even know about these problems, partly because they are already "fully booked" . Today statisticians are immersed in genomics; but there are many, many other interesting problems that are equally urgent. Go to a conference like NIPS on neural information processing. There is work on analyzing catalogs of images and sounds, problems of all sorts in signal array processing that come up in electrical engineering. There are so many interesting datasets, so many interesting problems, so many great opportunities!

I: You have worked with wavelets. How is that related to statistics?

D: Wavelet theory is a fascinating branch of applied mathematics — harmonic analysis, numerical analysis, approximation theory all come together. Studying wavelet theory you learn about representing problems, about representing signals, about representing noise.

This background is useful in statistical theory. In non-parametric estimation, everything depends on your assumptions about some unknown regression function or unknown density function. Coming merely from a background in statistics, you don't have tools to think deeply about your assumptions and how they should be represented. By learning what wavelets are all about, you suddenly understand a lot of things that were mysterious in non-parametric estimation. A simple example: often nonlinear estimators dramatically outperform linear estimators in nonparametric estimation and regression, even in problems where everything seems linear and convex and banal. Once you understand wavelets it's very easy to understand this phenomenon and extend it in many directions.

There's a wide collection of signals and stochastic processes where modeling by wavelets is appropriate — any time you have impulsive events or long memory. Many non-Gaussian stochastic models are very important in applications — remember that the Gaussian is a myth. In certain application areas such as Internet traffic, if you come with only a Gaussian stochastic process background or only a Poisson process background, you just cannot analyze the data perceptively. So knowing about wavelets widens your scope quite a bit.

The wavelet transform broadens your mind in the following way. If all you know is the Fourier transform (which every statistician has to learn in the guise of the characteristic function) then you have in your mind only a very poor collection of transforms. As soon as you have wavelet transform, you suddenly realize that there are not just two — i.e., not only Fourier and Wavelets — there are many, many transforms. The right one can depend on the data you are studying.

Finally, for many kinds of signals, the wavelet representation is sparse. That gives an impetus to the statistician to study high-dimensional parameter vectors where the vector is sparse, with relatively few big entries. Iain Johnstone and I were very inspired by this viewpoint, and it has influenced all my later work.

I: What do you think will be the forces shaping the future

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development in statistics?

D: Statistics is a data-driven discipline; each time someone invents a new kind of data there is always an infinite supply of new questions. Genomics is an example: microarrays came along and there were enough new questions to keep all statisticians busy. There are many new kinds of data. For example, we are now entering a world of ubiquitous sensors where there are sensors on your body, sensors in space, and everywhere sensors are talking to each other. Because of this sensor network, there will be many new questions. Another example: all sorts of data come out of analysis of blood chemistry. In proteomics, they subject blood samples to high-resolution mass spectrometry and get very finely-resolved spectra that reveal all the chemical constituents present in the blood. They hope to detect diseases early and forecast about your health. All the time we see new data sources creating enormous volumes of data with completely different structures from anything we have seen before. Basically, we need statisticians to cope with this onslaught of new data types. Each new one is going to cause a revolution in our field because you have so many new questions arising from each new data type.

I: You mentioned revolutions. Do you think there will be some conceptual revolution that will change the direction?

D: Over the last twenty years there was a shift away from an intellectual attitude, where you think very carefully before you do something, to a computational, experimental attitude where you quickly do something with the computer. At some point this will run its course and statisticians won't be able to really do much of value simply by running to the computer. Then there will be a whole bunch of new questions which arise out of dealing with these new data structures; they'll ask "what can we learn from graph theory?" or "what can we learn from theoretical computer science?" We'll go back to a much deeper level of thought in order to make the next step. I think that's coming soon.

I: In that case, do you think there is need to relook at the way statisticians are getting their undergraduate training in order to meet the challenges you just mentioned?

D: They should be good in mathematics and computers, and really care about analyzing data. In some parts of the world, statisticians are just trained at math and they aren't interested in science. In some other parts, they learn a lot about data but are not well-trained in math. In most parts of the world, they don't get enough computer background to really push the field. It's a three-legged stool — you need all three. That's really demanding for an undergraduate education, but I just don't see any other way.

On the one hand, things are much easier these days. We used to have to work really very hard to get the computer to do anything. For even the most routine analysis, I had to write a short computer program, get it into the computer, wait for the results, and if I made one tiny mistake, I had to start all over again. It's much easier these days. On the other hand, in economically advanced countries like Singapore, in Europe and the United States, kids have so many possible entertainments that few will choose to really use their minds. It is very unlikely that more than a small number are going to look at a field and say, "Oh, this is so inspiring, I want to know everything about it." Kids will pursue social life and many other diversions. Plus, they'll be "cool" and sophisticated and materialistic. Finally, in a comfortable society parents may be a little afraid if their kids are too intense about study and consider it unhealthy.

Every once in a while at Stanford, I see a kid with that "look" in the eyes. I know they still exist. We get some of them in the graduate program. I'm very fortunate to have had some great students who have gone on to become very distinguished scientists in their own right. I know that more great young minds are out there. That's for sure.

Carl de Boor: On Wings of Splines >>>

Interview of Carl de Boor by Y.K. Leong

Carl de Boor made fundamental contributions to the theory of splines and numerous applications of splines that range from highly efficient and reliable numerical algorithms to complete software packages. Some of these applications are in computer-aided design and manufacturing (of cars and airplanes, in particular), production of typesets in printing, automated cartography, computer graphics (movie animation, for example) and signal and image processing.

He has given numerous invited talks at scientific meetings throughout the world. He has served as editor of leading mathematics journals. He has received numerous honors and awards, among them the Humboldt Research Prize and John von Neumann Prize. He is a member of the U.S. National Academy of Sciences, the National Academy of Engineering, the Academia Leopoldina (Deutsche Akademie der Naturforscher) and the Polish Academy of Sciences, and a fellow of the American Academy of Arts and Sciences. He was Professor of Mathematics in the Departments of Mathematics and Computer Science at University of Wisconsin-Madison from 1972 until his planned retirement in 2003. He remains there as Emeritus Professor and continues to be active in research as a member of the Wavelet IDR Center.

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Carl de Boor

He was interviewed by the Editor of *Imprints* on 16 August 2004 when he was at the Institute to give a plenary talk at the international conference on wavelet theory and its applications. The following is a vetted and enhanced version of the transcript of excerpts of the interview which gives a frank account of the serendipitous path from Hamburg to Harvard, followed by first contact with splines (in a research laboratory in Michigan) that soon took off into an exciting world of path-breaking discoveries and immediate applications.

Imprints: Your early university education was at Hamburg and your PhD was from Michigan. Could you tell us something about the way you went from Hamburg to Michigan?

Carl de Boor: It's definitely a story of accidents. I moved to Hamburg in 1955, and I met there (as the result of a further sequence of accidents) an American girl to whom I got engaged in '58. Her father was a professor of political science at Harvard, and he arranged for me to come over to Harvard for a year, in '59. My future father-in-law had known the Birkhoffs, both the older and the younger, well and had proposed that Garrett Birkhoff might give me a research assistantship and had mentioned that I had worked as an "assistant" to Collatz who was at that time a main figure in numerical mathematics, at least in Germany. Now, I had indeed worked as a teaching assistant for Collatz and even done some calculations for him. But Birkhoff misunderstood "assistant" and thought that I had been an "Assistant". In the German system, an "Assistant" is someone close to a PhD. So, Birkhoff was very happy to give me this job. But it became clear very quickly that I was not at all qualified for it. Birkhoff was a very kind person. He did not kick me out, but he gave me a good problem, and I have never worked so hard in my life, trying to produce at least something. During that year, I decided that American university life was freer than German university life at that time. So I decided to stay, and I got married. However, I couldn't support my wife on this research assistantship. Birkhoff was then consultant to General Motors Research in Warren, Michigan. He persuaded them to give me a job. So I worked at General Motors Research and even ended up writing some papers there. But my colleagues with PhDs were much freer in the choice of problems and were much better paid, of course. So I decided I should get a PhD too, and the nearest good university was the University of Michigan at Ann Arbor.

I: Was Garrett Birkhoff at any time interested in numerical mathematics?

dB: Birkhoff was, in fact, interested in many aspects of mathematics. He was trained as an algebraist. He did various things in algebra, universal algebra, and he wrote a book on lattice theory – he practically invented lattice theory. He was also interested in applied mathematics and numerical mathematics. He wrote, for example, a book on the numerical solution of elliptic PDEs (with R. E. Lynch). Maybe his interest in numerical mathematics started in the war years – as it did for many mathematicians then.

I: He gave you some problems to work on?

dB: Yes. He was at that time working on problems in fluids. He had written a book (with Zarantonello) on "Jets, Wakes and Cavities". He was looking at specific problems. I had to work out two-dimensional flows over or under an obstacle, like flow under a sluice gate. I produced some numerical results which ran counter to the perceived wisdom at the time.

I: Was your PhD connected with that work?

dB: Not at all. My PhD was totally different. When I came to General Motors, they had just started to use computers in order to represent car surfaces mathematically. The notion was that once you had a mathematical description of a car surface, then you could use computers to generate cutting paths for numerically controlled milling machines to cut the dice needed for forming or stamping that surface in sheet metal. Computers had just become powerful enough for this to be feasible. When I came to General Motors, they had started using splines to represent curves and surfaces. My thesis was something that came to me as I was thinking about improving on what I found there.

I: Who was your PhD advisor?

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dB: Robert Bartels. He was the only numerical analyst at the University of Michigan at that time. He was also running the Computing Center there. But, in a way, I learned perhaps more from Birkhoff because of the many interactions with him at General Motors, and from Bob Lynch, a Birkhoff student there, and from John Rice - I wrote papers with each of them there. My thesis, the topic and the writing, did not have any real input from anybody else. But I'm sure that Collatz also had some influence on me, because I had learned from him when I was in Hamburg. I did not have an advisor in the sense that I went to him every week and he would say, "That's okay" and "What's the next step?"

I: Was your PhD work crucial in shaping your future interests?

dB: Yes and no, in the following way. My thesis has to do with the use of splines in solving ordinary differential equations, and it was really looking at what is now called the projection method. You project the equation onto a finite-dimensional space and in this way get a finite-dimensional problem which you then solve. What was new in this thesis was that I pushed this point of view of projection. Second, I used not the standard functions that people used to use (people like Galerkin or Ritz used polynomials) but I used splines, especially B-splines. Unfortunately for me, I finished the thesis in 1966, and in that very year there appeared an English translation of a book by Kantorovich and Akilov which also dealt with projection methods, and in 1966 there also appeared the paper by Schoenberg and Curry concerning B-splines. So I felt scooped, and I did not publish my thesis. But it shaped my thinking because in the thesis I realized that B-splines (these are splines of minimal support) really are the right tools for understanding and working with splines. Of course, this was understood by other people before that - Schoenberg, who invented them, understood that. It became clear to me then and it colored what I did for the next twenty years.

I: What does the "B" stand for?

dB: "B" stands for "basic" or "basis". Schoenberg called them "B-splines". If you take any space of piecewise polynomials with a certain number of continuous derivatives across junction points, any such space has a basis consisting of these basic splines (splines of minimal support).

I: How much was your theoretical work motivated by problems in other disciplines in science and technology?

dB: I like to have what I do used by others. I like that very much. But what really turns me on is when in this mess, this complicated situation, I can see something simple, that it all actually comes down to something very simple. Initially a problem might have come to me because I was interested in some applications or because I like to look at problems with some applications. But once I get intrigued by it, it doesn't matter any more where it came from although I'm very pleased that people use what I do.

I: Do you seek out problems in other fields?

dB: I never have been a person to look around for problems. There are always more problems than I can do. You listen to a talk and there is a problem. I don't actively go and talk to physicists and say, "Please give me a problem." No, but I do listen more carefully to a problem that I see has some uses. If it has no use, it has to be very intriguing.

I: Did the computer play any significant role in your discovery of theorems or proofs?

dB: Well, first of all, without computers, there would be no full spline theory today. Spline theory really developed in the sixties because only then could the computer make use of it. There was then some pressure to understand better these piecewise polynomials. So, in that sense, most of what I have done has been motivated and used by computers in a central way. But these days, I also use a computer simply because it is a wonderful tool. I work on very practical things, like representing functions or solving functional equations. For anything that I wish to prove or try to understand, the computer readily provides examples. It's an integral part of my research work. I travel with a laptop and use it all the time.

I: Is there any particular discovery or result of yours that gave you the greatest satisfaction in your research career?

dB: I have had this wonderful feeling of sudden insight only a few times in my life, but I remember every one of these moments. I can taste them even now. For example, finding the dual functionals for B-splines, realizing that the recurrence relations for B-splines, which I had come across earlier, could actually be used for the stable evaluation of splines, seeing the final step in a proof that Allan Pinkus and I made up for conjectures of Bernstein and Erdös, seeing the mathematical reason for the superconvergence numerically observed by Blair Swartz, seeing the Courant hat function as the shadow of a cube, i.e., as a box spline, etc. You suddenly see, and every time I think about these moments of insight, I'm pleased all over again.

I: Do you know whether any of your discoveries have been directly used in industry?

dB: I have to smile at that question. Three weeks ago, I was at a meeting, the annual meeting of SIAM (Society of Industrial and Applied Mathematics) in Oregon, and one of

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the invited speakers, Thomas Grandine, gave a talk entitled "One day in the life of splines at Boeing". In this talk, he made the point that the B-spline recurrence relations that I mentioned were used at Boeing, by his estimate, five hundred million times a day.

The Fortran programs I wrote at General Motors in the early days were still in use there in the late eighties, as I discovered when I was back there as a consultant then, and may still be in use there buried in some big code today.

I: Could you briefly explain to a non-specialist the difference between approximation theory and numerical analysis?

dB: I know I mentioned these two terms in my CV. Maybe I can explain them along these lines. What I really do is work with piecewise polynomial functions or splines. You can use these functions to represent information, say to represent some function, curve or surface. To the extent that you then worry about how well you can approximate a particular function, or class of functions, by those splines, you are doing approximation theory. But how you approximate a function depends also on what you know about the function. If the function is given to you only implicitly, as the solution of a differential or integral equation, then you are solving functional equations numerically. When you develop and analyse those numerical procedures, you are doing numerical analysis.

I: I believe that approximation theory has a rather long history.

dB: Both have a long history. You might say that both started with Newton, with polynomial interpolation. Approximation theory proper maybe started in the 19th century,with Chebyshev, with his characterization of a best uniform approximation. Then there is Weierstrass who showed that any continuous function can be approximated arbitrarily well by polynomials. But it is Bernstein, in the early 20th century, who really developed the theory, characterizing the rate at which a function can be approximated by polynomials. On the other hand, numerical analysts think of Gauss as an early contributor (think of Gauss elimination, least-squares, and Gauss quadrature) and, by the beginning of the 20th century, people were finding numerical solutions to partial differential equations in systematic ways.

I: Numerical analysis started even before computers came in?

dB: Yes, definitely. Scientists have to find solutions to the models that they make of the world. They have no choice but to compute and they had to be very clever in this when they could only use pencil and paper.

I: How has applied mathematics changed since the early years of your research career?

dB: I don't know that much about applied mathematics. Some people have a global vision, they see their field in some more general context. I'm very much of an "opportunist". I see something interesting, I go for it. I don't have long-range plans. I don't worry about what's going to happen ten years down the road. I follow what I am intrigued by. So, how has applied mathematics changed? The computer for sure has totally changed it. Before, you had to worry very much about formulating models in such a way that a good approximate solution could be hoped for. These days you are much freer to formulate a model. You can have a very complicated model and still hope to compute good approximate solutions.

I: Do you think that the computer has, in some sense, not encouraged conceptual development?

dB: Well, it is true that even some pure mathematicians these days behave more like physicists in the sense that they can explore problems experimentally, by computations. Certainly, numerical analysts now work on complicated problems without being able to prove that the methods they are using are appropriate or effective. They have to come to terms with the fact that they may not be able to prove their results in a rigorous sense.

I: Do you consider this to be a positive development?

dB: Very much so. The more freedom there is to find out something, the better off we are. Of course, we are mathematicians, so ultimately, we do try very hard to prove that what we see experimentally is actually so.

I: Mathematicians also like to create theories. If you have a lot of information being churned out by the computer, …

dB: … then mathematicians are all the happier. I think mathematicians are always trying to make order out of chaos, trying to see what is really going on and what makes it go. With the computer generating all this experimental evidence, I think mathematicians are in their element. I think having the computer is very enriching.

I: Do you have any predictions or expectations of the directions in which approximation theory and numerical mathematics will be moving in the next ten years?

dB: I might guess that approximation theory will concentrate on the efficient representation of information, but I really have no idea, nor do I feel badly about that. As an example, in 1985, certainly nobody in my area would have predicted

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the onset and influence of wavelets. There were, at that point, some experts who knew about them, who knew a lot about them. There were even people in numerical methods who knew about the idea of multiresolution. Still, when wavelets hit approximation theory and numerical mathematics, it was a real surprise. So, for all I know, another such fundamental change is just around the corner.

I: What sort of advice would you give to a graduate student in applied mathematics who wishes to get started in research?

dB: First, get a good teacher. If a student does not know enough to choose a good teacher, there is no hope. Also, it doesn't matter so much what the student does or chooses to do - the teacher is there to help - but the student must feel passionate about it, must really want to do it. And then the rest, assuming that the student has talent, will not be a problem.

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