IMPRINTS

Celebrating 20 years of Mathematical Synergy

NUS National University of Singapore | IMS Institute for Mathematical Sciences

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NEWSLETTER OF THE INSTITUTE FOR MATHEMATICAL SCIENCES, NATIONAL UNIVERSITY OF SINGAPORE

Summer Program at IMS: Research in Industry Projects for Students

ADRIAN RÖLLIN, *Deputy Director of IMS*

In 2019, IMS started a collaboration with the Institute for Pure and Applied Mathematics (IPAM) at UCLA to organise a Singapore-version of their summer program Research in Industrial Projects for Students (RIPS). IPAM has been running this program successfully for over 20 years with the help of various sponsors from industry and US government agencies. The idea of the program is to expose talented undergraduate students to real-world problems from industry and have them engaged full-time throughout the summer semester break. While the main focus of the program is educational, the problems posed by the sponsors are challenging, and any progress made by the teams is valuable to both students and sponsors.

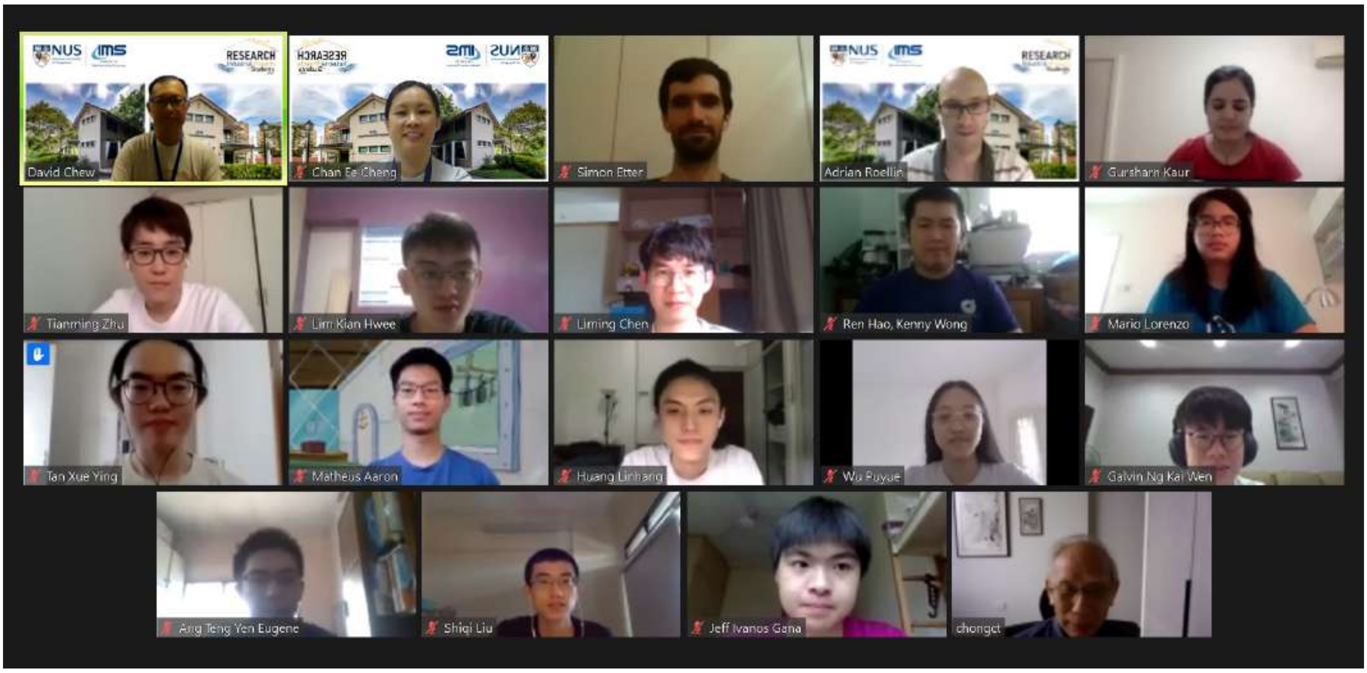
At IMS we adopted this program and ran it for the first time in 2019. Each team consisted of two NUS students, one regional student and one US student. IMS provided offices and accommodation to all students, so that interaction among the teams was not restricted to the workplace. This gave our local students the opportunity to learn how to work, live and communicate with people from other cultures and different backgrounds. Weekend trips to local places of interests guided by NUS students and common meals at the canteens helped reinforce interaction between NUS and overseas students and forge bonds. Each project team was guided by an academic mentor (a research fellow from NUS) and an industry mentor (an employee of the sponsoring company), and the whole program was supervised by our program director, Dr David Chew. We were fortunate to have Google, Grab, NVIDIA and the Saw Swee Hock School of Public Health on board as sponsors, and their projects were indeed challenging. Even more so were we pleased to see how enthusiastic students were and how much progress the teams made.

RIPS at IMS is part of a broader vision of NUS: We want to prepare our undergraduate students for the challenges of a modern workplace, where creativity, independence and interdisciplinary thinking are key. Students are assigned to teams and projects by the program organisers taking into consideration their academic background, grades, internship and other industry experiences, participation in competitions, the nature of the project and many other factors. By making the teams diverse, we force students to leave their comfort zone and face the reality of working in international teams with people who have different skills and different points of view. It also ensures that each team has a range of skills at their disposal to tackle the problems of the project — one student may be good at coding, another student may be good at analysing data, and yet another at writing reports.

While each team has an academic and an industry mentor, we explicitly instruct mentors not to solve problems for the students. A nudge here and there can get the teams on the right track, but the students are ultimately owners of their projects and responsible for coming up with a plan of what they think they can deliver after the nine weeks of the program. Hence, good communication with the sponsors and setting realistic expectations is an integral part of the RIPS experience.

In 2020, due to the pandemic, the program had to be held virtually. We were hopeful first that a physical RIPS could be held with NUS students, since overseas travel was suspended, but with the circuit-breaker and subsequent ongoing restrictions on movement and meeting around campus in place, we had to completely move online. Since working across different time zones would have been very difficult, we decided to proceed without the four US students, who originally planned to come to Singapore. Fortunately, they were allowed to participate in the respective program at IPAM. Despite the challenges a virtual program posed, we were amazed to see how engaged and passionate the students were and how quickly the teams were able to come up with creative solutions in their projects, which were this time proposed and sponsored by Google, the Ministry of Manpower and Singstat.

We would like to take the opportunity to express our thanks to all sponsors and industry mentors, whose support and enthusiasm was and is key to making this summer program possible. We thank our research fellows, who invest their time during the summer to guide the students, help them write reports and polish their presentations. Finally, we thank our program director, Dr David Chew, who oversees and manages all teams and, together with our IMS staff, ensures smooth operations of RIPS.



A screenshot of the RIPS 2020 team participating in the activities via video conferencing.

Research in Industrial Projects for Students (RIPS) 2021 – Singapore (31 May–30 July 2021)

The program is expected to commence from 31 May to 30 July next year. Please visit our website for more details.

NEWS

MANAGEMENT BOARD

The Institute would like to congratulate Professor Zuowei SHEN, our current Management Board member, who will be the Vice Provost (Graduate Education) from April 2020.

The Institute is pleased to welcome two new members to the Management Board, Professor Yeneng Sun and Professor Gregory S. Chirikjian.

Yeneng Sun

Professor Sun is Goh Keng Swee Professor at the Department of Economics and professor at the Department of Mathematics at NUS. He was the IMS Deputy Director from 2001 to 2004, the head of the Department of Economics from 2008 to 2012, and acting director and director of the Risk Management Institute from 2018 to 2020, where he is currently chair of the management board. He has been appointed as dean of the Faculty of Science of NUS in 2020.

Professor Sun obtained his PhD at the University of Illinois Urbana-Champaign. His research interests include mathematical economics, game theory, analysis and probability theory.

He received Singapore's National Science Award in 2000, and the NUS Outstanding University Researcher Award in 1998. He is Fellow of the Singapore National Academy of Science since 2014, and Economic Theory Fellow of the Society for the Advancement of Economic Theory since 2011.

He is a member on the editorial board of various journals, including Economic Theory, Economic Theory Bulletin, Annals of Finance and Journal of Logic and Analysis. He is a member of the Advisory Committee at the Institute of Economics (Academia Sinica) since 2017, of the Advisory Board of the Asia Pacific Mathematics Newsletter since 2010, and of the International Advisory Board of the Centre for Mathematical Research in Economics and Finance at the University of Manchester since 2017.

Professor Gregory Scott Chirikjian joined the Department of Mechanical Engineering at NUS as the head of department in 2019. Prior to joining NUS, he was a professor at Johns Hopkins University, where he served as chair of the Department of Mechanical Engineering from 2004 to 2007.

Gregory Scott Chirikjian

Professor Chirikjian obtained his PhD in applied mechanics from the California Institute of Technology in 1992. His research interests lie in robotics, applications of group theory in a variety of engineering disciplines, and the mechanics of biological macromolecules.

He is recipient of the 1993 NSF Young Investigator Award, the 1994 NSF Presidential Faculty Fellow award, the 1996 ASME Pi Tau Sigma Gold Medal, the 2014 ASME Mechanisms and Robotics Award and A.T. Yang Memorial Award, the 2019 ASME Machine Design Award. In 2008, he became a Fellow of the American Society of Mechanical Engineers, and in 2010, he became a Fellow of the Institute of Electrical and Electronics Engineers. He is the author of more than 200 journal and conference papers and primary author of three monographs.

Professor Chirikjian has been the editor-in-chief for the journal Robotica since 2005, and he has been an associate editor for other journals such as the ASME Journal of Mechanical Design, Mechanisms and Machine Theory, a guest editor for the IEEE Robotics and Automation Magazine. He was a program director for the National Robotics Initiative and the Robust Intelligence cluster in the CISE/IIS Division of the National Science Foundation from 2014 to 2015.

APPRECIATION

The Institute would also like to thank outgoing MB member Professor John Thong.

INTERVIEW

STEVEN NEIL EVANS: PROBABILIST FOR ALL SEASONS

Interview of Steven Neil Evans by Y.K. Leong

Steven Neil Evans has made important contributions to the theory of stochastic processes, notably coalescent processes and superprocesses and is well-known for his wide-ranging contributions to applications of probability and statistics in biology, chemistry, astrophysics and evolutionary linguistics[1].

Evans grew up in the orchards of the Australian rural town of Orange, 300 kilometres from Sydney. His undergraduate education at the University of Sydney was completely supported by scholarships and a stipend from his position as fulltime bursar of the Commonwealth Banking Corporation of Australia while he was an undergraduate. On completion of his BSc studies, he worked for a year for the bank before he was given further leave of absence to continue his PhD studies at Cambridge University, after which he worked for a year for the supporting bank. He left the bank and found his true calling in research as a postdoc at the University of Virginia while holding a tenure-track position as assistant professor at the University of California at Berkeley.

When he was barely in his thirties, Evans had shown promise in being awarded the Rollo Davidson Prize (for early career probabilists) and the Presidential Young Investigator Award of the National Science Foundation (NSF). This was followed by a series of prizes and distinguished lectures: G. de B. Robinson Prize (Canadian Mathematical Society), Astor Visiting Lecturer (Oxford University), Birnbaum Lecturer (University of Washington), Distinguished Visitor Lecture (National University of Singapore (NUS)), Hotelling Lecturer (University of North Carolina) and Medallion Lecturer (Institute of Mathematical Statistics). Rising quickly through the ranks, he became full professor in the department of statistics and held a joint appointment in the department of mathematics a few years later. In 2005 he was also appointed as a core faculty in the Center for Computational Biology and became a member of the Graduate Group in Computational and Genomic Biology and of the Graduate Group in Computational Science and Engineering. At the national level, he is a member of the NSF funded open collaboration project, Cyberinfrastructure for Phylogenetic Research (CIPRes), which enables researchers from 19 scientific bodies to access huge datasets of biomolecular sequences.

Evans' wide-ranging interests in applications of probability in general, and stochastic processes in particular, have resulted in the supervision of 19 doctoral students and joint work with numerous collaborators. This has produced more than 130 single and joint-author papers in areas[1] as diverse as random matrices associated with Coxeter groups and other aspects of probability on algebraic structures, measure-valued processes arising in population biology such as superprocesses, coalescent models

[1] Information taken from Evans' homepage

in biology, chemistry and astrophysics, phylogenetic invariants and inference regarding recombination, fitness landscapes, mutation-selection balance, stochastic PDE models of bacteria and yeast aging with applications to mortality modeling. More recently, his research applies metric geometry (such as the Gromov-Hausdorff metric) in the study of probability on tree statistics that occur in population genetics; in particular, the methods of phylogenetic inference in evolutionary biology to questions in historical linguistics, and the connections between matrix-valued orthogonal polynomials and queuing theory.

In addition, the breadth of his research work brought him to giving invited lectures at numerous major scientific meetings of the Bernoulli Society, American Statistical Association, Biometric Society, University of Southern California at Los Angeles, Cornell University, Simons Institute for the Theory of Computing, Clay Mathematics Institute, University of Colorado (Boulder), Stanford University, University of Utah (Salt Lake City), Mathematical Sciences Research Institute, Mathematical Biosciences Institute (Ohio State University), Centre de Recherches Mathematiques (Montreal), Fields Institute, Banff International Research Station, London Mathematical Society, Newton Institute (Cambridge, UK), Hewlett-Packard Laboratories (Bristol, U.K.), Manchester Institute for Mathematical Sciences, Warwick Mathematics Institute, International Centre for Mathematical Sciences (Edinburgh), Oberwolfach Mathematics Institute, Erwin Schroedinger Institute (Vienna), *Rheinisch Westfalische Technische Hochschule (RWTH) Aachen*, *École Polytechnique Fédérale de Lausanne* (EPL), *Weierstrass Institute* (Berlin), *Frankfurt University*, *German Mathematical Society*, *Center for Interdisciplinary Research* (Bielefeld), *Goethe-Universitaet* (Frankfurt), *Centre International de Rencontres Mathématiques* (Marseille-Luminy), *Institut Mittag-Leffler* (Stockholm), Australian Academy of Science, Australian Mathematical Society, New Zealand Mathematics Research Institute, International Congress of Mathematicians and Institute for Mathematical Sciences (National University of Singapore). He is an elected fellow and member of major scientific bodies such as the Alfred P. Sloan Foundation, US National Academy of Sciences, American Mathematical Society, Institute of Mathematical Statistics, University of California at Berkeley, Mathematical Sciences Institute (Australian National University). He has served as Associate Editor of *Stochastic Processes and their Applications*, *Annals of Probability and Probability Theory* and *Related Fields* and as core editor of *Probability Surveys*. He has also been active in the organising committees for the special year on stochastic analysis of Mathematical Sciences Research Institute (MSRI), the "Thirty Sixth Conference on Stochastic Processes and their Applications" at the University of Colorado, Boulder, "Recent Tends in

" I DON'T KNOW IF THERE'S A PHILOSOPHY OF RESEARCH THAT I HAVE, BUT IT'S A CHARACTERISTIC THAT I HAVE, THAT IS, A SORT OF INSATIABLE CURIOSITY FOR ALL SORTS OF DIFFERENT THINGS, YOU KNOW, ALWAYS TRYING TO READ AND LEARN ABOUT DIFFERENT THINGS WITHIN MATHEMATICS AND OUTSIDE MATHEMATICS. "

Stochastic Analysis Conference" of the Pacific Institute for the Mathematical Sciences, and the Workshop on "New Directions in Probabilistic Models of Evolution" of the Simons Institute for the Theory of Computing. He has served in an advisory role for the Institute of Mathematical Statistics, Banff International Research Station, and the Bernoulli-IMS World Congress in Probability and Statistics. He is currently on the advisory board of *Forschungsschwerpunkt Mathematisierung* (University of Bielefeld), Simons Institute and Samsung Science and Technology Foundation.

Evans is no stranger to the Institute for Mathematical Sciences (IMS) of the National University of Singapore (NUS). He had been invited to give a series of tutorial talks from 21–25 March 2011 on "Metagenomics and metrics on spaces of probability measures" within a sub-program of the Institute's program "Probability and Discrete Mathematics in Mathematical Biology" (21 March–1 April 2011). He was again at IMS from 29 July–12 August 2017 as IMS Distinguished Visitor for the program "Genealogies of Interacting Particle Systems" (17 July–18 August 2017) in which he gave the Ng Kong Beng Public Lecture "Some Mathematical Insights into Aging and Mortality" on 3 August 2017 and the Distinguished Visitor Lecture "Infinite bridges for Rémy's algorithm" on 7 August 2017. It was during his second visit to IMS that Y.K. Leong took the opportunity to interview Evans on 4 August 2017 on behalf of *Imprints*. The following is an edited and vetted version of the transcript of the interview in which he talked about his early interest in probability as a part work and part study employee of a bank in Australia, his formative years as a postdoc in the United States and his "insatiable curiosity" and thirst for knowledge beyond probability and mathematics.

Acknowledgement Y.K. Leong would like to thank Von Bing Yap of NUS's Department of Applied Statistics and Probability for preparing a raw draft of the transcript of the interview.

IMPRINTS I I notice that you were employed as a full-time bursar by the Commonwealth Banking Corporation when you were doing your BSc degree at the University of Sydney from 1979–1982. Was this a special arrangement?

STEVEN EVANS E The bursar program was a program with the Commonwealth Banking Corporation which gave employees who attended university during the year a stipend and worked for the bank in the long vacation. I don't remember how many students there were in the bursary program at that time. I was supported by that and my intention was that I would work for the bank after I graduate. This was great for me since I came from a family with very limited means. So that was the way I was able to get into the university.

I **You had to work for the bank during the long vacation?**

E Yes. I had to work for the bank while attending university. It was a wonderful deal. I was not given a full salary but a stipend that was certainly enough to live on. I don't know if they still have that program.

I **Did you have to sign a bond with the bank?**

E No, it was actually after they outlawed the bond thing in Australia. The intention was that you would work for the bank when you finished [your studies]. That's what I did. I worked for them for a year after my BSc, then they were very supportive of me doing a PhD. Well, they didn't pay me while I did my PhD but they were quite happy to keep my job open while I did that and I worked for them for a year after I got my PhD.

I **Why did you go to the University of Cambridge for your PhD after completing your BSc and was your graduate research interest motivated by your association with the bank?**

E Not really. You know, I had done work at the bank that was, you could say, research; it had a definite feel of that already. I had been excited about research and was motivated to do a PhD having done an Honours thesis as an undergraduate with Dr Neville Weber. You know, I just found it very exciting doing this Honours thesis on stochastic approximation algorithms. That was what convinced me that I wanted to do a PhD eventually. I decided to do that after working for a year with the bank.

I **Was your research interest connected with your job?**

E Not really, no. I wanted to do research but no, not even in finance.

I **During your early years as an assistant professor in statistics at the University of California at Berkeley (UCB), you were also an instructor in mathematics at the University of Virginia. This seems to be a rather unusual arrangement. Did you ever consider joining the mathematics rather than the statistics department?**

E In Berkeley I was in both departments, I have a joint position, a certain percentage of my appointment in statistics and the rest in mathematics. When I decided to apply for academic positions, I applied to both mathematics and statistics departments because probability tends to be done in both sorts of departments. I got a formal job offer from the University of Virginia and an informal one from the University of California at Berkeley that had to be approved by the higher administration there and it was taking a long time, and Virginia was pressing me more and more to say whether I wanted to take up this essentially two-year postdoctoral position there. So eventually I was able to come to an arrangement with Berkeley whereby they would let me go to Virginia for two years on the understanding that I come to Berkeley afterwards. So that was very fortunate for me because I was not forced to take up this postdoctoral position and give up the chance of a tenure-track position at Berkeley.

I **Could you have gone to Berkeley straight away without going to the Virginia position?**

E I could have but I would have to take the risk that even though they have given me an informal offer, the higher administration in the university may not approve the offer eventually. So, it was rather risky at that time. You know, in retrospect, in my experience, probably the higher administration would not have gone back on the department's decision. I guess it could have happened. And it turned out very well because going to Virginia was excellent.

I went there [Virginia] with the intention of working with a gentleman called James Taylor. So, I worked with him and two other probabilists, Loren Pitt and David Brydges. For me, that was very worthwhile and formative in my research.

I **You mentioned that it was some kind of postdoc.**

E Yes, it was called a Whyburn Research Instructorship. It was a postdoctoral position in memory of Professor Whyburn[2] , who had already passed away. He was one of the finest topologists.

I **It's quite hard to get a tenure track in Berkeley, isn't it?**

E Yeah. I realize, in retrospect, how lucky I was.

I **You have been at UCB from the beginning of your academic career for a period of 30 years. What is it at UCB that has managed to keep you rooted there scientifically and socially? How often have you been back to Australia for social and mathematical purposes?**

E Berkeley has wonderful colleagues and wonderful students. I have enjoyed both thoroughly. I can also be a little flippant and say "the weather". My wife, if we consider moving elsewhere, would certainly have refused moving to places where there is snow. Most good universities other than Berkeley and a few others, have snow. So that made it harder to move even if I had wanted to. As to going back to Australia, my parents both passed away in the 90s. So, I haven't been going back much since then. I still have a brother and sister whom I visit every so often. I don't really visit Australia much because I did my PhD in Cambridge and I don't have much strong academic ties to Australia. I have gone back for a few summers and worked with people once at the University of New South Wales and twice at the Australian National University, and I enjoyed it thoroughly. I'm able to travel much more in the northern summers to visit my collaborators in Europe, especially in Germany.

I **Where is your hometown?**

E My hometown in Australia is a place called Orange which is 300 kilometres miles west of Sydney. That's an area which grows mainly fruits like apples, pears and things like that. That is one of the things that I enjoy going back to see my brother and sister. They both live on farms.

I **That's a far cry from Berkeley, isn't it?**

E Yes, it's a very different atmosphere from Berkeley; that's for sure

I **Although you were closely associated with a major bank in Australia when you started your academic career, you did not pursue your research in financial mathematics or mathematical finance but instead you did much significant work in mathematical biology and phylogenetics. How did your interest in questions of a biological nature get started? Do you think that your early upbringing in rural Australia might have predisposed you to enquiring about nature?**

E I think that's a very interesting question. My parents were both interested in nature even though they have themselves a very limited education because they grew up in the Great Depression. They both had to leave school early and both grew up on orchards. They were very much interested in nature and they belonged to a local group called the Field Naturalist Society. They used to go on hikes (bush-walking) and do various things related to biology. I mean, I liked that sort of things as well and academically, the person who got me interested in biology was the colleague in the office next to mine. He is Australian and he's a gentleman called Terry Speed[3]. He had great enthusiasm for many types of problems in biology. He had very close connections with the biologists in Berkeley and people further afield like in the University of California San Francisco Medical School. He got me interested in these things and I just found his enthusiasm incredibly infectious. He ran a very interesting seminar series which I attended. It was through those seminars that I got interested in biology.

I **I think that recently they had some kind of celebration for him, some kind of festschrift.**

E Yes, I contributed something to the festschrift for him. That could have been for his 70th birthday, I think. I may be wrong on that because he has retired from Berkeley now. He's still very active in Australia at the Walter Eliza Hall Institute. But he doesn't come through Berkeley so much now and he doesn't have any more students in Berkeley. I see him when he passes through.

I **It seems there are a lot of Australians working in the US.**

E Yes. In Berkeley once we had three Australians in the statistics department - me, Terry Speed and Jim Pitman. Then we had another Australian, an assistant professor, Allan Sly; unfortunately, he went recently to Princeton. He's an absolutely brilliant young probabilist.

I **In the 1980s and 1990s, you and Edwin Perkins opened up the field of superprocesses which have now found applications in evolutionary biology. The term "superprocess" seems to embody an all-encompassing concept. Could you give us some examples of superprocesses?**

E These have become very general ideas. You start off with a probability model where you have particles somehow wandering around in some space and they interact with each other and do things like branch and split, and quite often it becomes more tractable when you study models like that where you consider an asymptotic situation where the number of particles in the population goes off to infinity and you feel that the [collection of] particles is a measure of mass distribution on the state space by giving each of them an

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[2] Gordon Thomas Whyburn (1904–1969)

[3] IMS Interview of Terry Speed at https://ims.nus.edu.sg/imprints/imprints-3-2004.pdf#page=16

equal mass and building a measure in that way. As you pass to the limit you end up with a situation where you have heuristically a mass of infinitesimal particles. So, what you see in the limit is a cloud of mass on the underlying state space that moves around in a stochastic way and because they move around in a continuous and stochastic way, you have all the tools of stochastic calculus or analysis to study such an object, and this becomes related to partial differential equations and tools like that. An example of an application like this is given in the distinguished lecture that I gave last night.

I was talking about a situation where we were modelling bacteria that, during their life time, accumulate extra damage to themselves, and how much damage they have affects their ability to split successfully so that they would die rather than split successfully. Also, they split at a slower rate if they have a lot of damage. So, what happens is that when they do split there is the existing damaged material and then they create a whole lot of new material. At the time of splitting there is going to be these daughter cells that will get a mixture of damaged material and new material. One can ask, "Is there some way that they use, sharing out, the damaged material and the new fresh material unequally among the two daughter cells, and if they do that, does that speed up the rate at which the population of the bacteria grows?" Is there an advantage to asymmetrically segregating the damaged material more in one of the daughter cells than in the other? Using the superprocess models we are able to get an indication that there is an advantage to doing that kind of asymmetric segregation between the two daughter cells. So, there should be some asymmetric segregation between the two daughter cells, but not too much, so that there is an optimum way somewhere in between complete equality and complete inequality between the cells.

I **This should be useful to the pharmaceutical industry. They found that some bacteria are getting resistant to antibiotics.**

E Right.

I **Have you consulted for the pharmaceutical industry?**

E I haven't been consulted for the pharmaceutical industry, but I have done a little bit of legal consulting, again alongside with Terry Speed. One of the things we were consulted for was the safety of things like silicone gel breast implants, the litigation being about whether these were causing women diseases. We were consulted for the manufacturers of these devices. I never actually got to testifying in that case and it was settled without the use of statistical arguments. I have done some financial consulting as well for the players in the financial industry in the San Francisco Bay area.

I **Among your results, are there any which you find surprising or counter-intuitive?**

E In the 1990s I did a lot of work on analogues of Gaussian measures, Gaussian probability distributions on vector spaces over local fields, things like the *p*-adic numbers and the *p*-series fields. What I just found there is that even though the vector spaces have a different structure from the euclidean spaces, there is perhaps nothing like that. The things I was interested in had a theory parallel to the ordinary Gaussian case. It surprised me; I don't know whether it was counter-intuitive but I had not gone into it with a strong intuition one way or the other. But I was surprised how much of the classical Gaussian theory had counterparts in this new world.

I **One of your more recent interests is linguistics. What kind of questions in linguistics is amenable to mathematical techniques?**

E Many things that have to do with structural questions about linguistics and structures about languages which I know essentially nothing about. You know, there is a big role for mathematics to play in various parts of linguistics. The area I got involved was again because of a collaborator, a lady called Tandy Warnow, who is now at the University of Illinois at Urbana-Champaign. At that time, she was at the University of Texas at Austin. She had already done work with a linguist at the University of Pennsylvania, called Donald Ringe. The sort of things that he is interested in is establishing the relationships between languages, trying to reconstruct the family tree of the Indo-European languages. Of course, much of that was already known without the need of mathematics – that there was a group of Germanic languages, a group of Italic languages, a group of Celtic languages. But the sort of evolutionary relationships between those you know – which ones came from a prominent ancestor, and which ones are more distantly related – are the sort of things where people hope to use the same sort of mathematical machinery that have been used in establishing evolutionary relationships between organisms and between species. You hope that you can carry across those modeling ideas that will work for languages. You don't have much data between languages as you have with DNA where you have millions of base pairs of DNA. It was something that we were quite successful – looking into the European languages (Donald Ringe's specific expertise), people applied these ideas to, for example, the native American languages in North and South America and Australian aboriginal languages.

I **Is there any connection between the Australian aboriginal languages and native American languages?**

E No, you know, the connection is, separately, for all the Australian aboriginal languages: almost all of them belong to one language family and people are trying to reconstruct the relationships between all of those languages in that language family.

I **How many languages do you speak?**

E How many languages do I speak? This is one of the problems of growing up in Australia. Just because you are very isolated, you don't see the merits of growing up and speaking different languages. I speak "a kind of okay" German as well as English; essentially monolingual, unfortunately, I think.

I **When you do the research do you need to study the languages?**

E No, I mean, that's the advantage of working with someone like Donald Rringe. He speaks many languages and knows all sorts of obscure languages, even ones that are not spoken now.

I **What about deciphering languages that are still not deciphered?**

E Yes, it's not something I have been involved with; presumably mathematics will play a role, that's for sure.

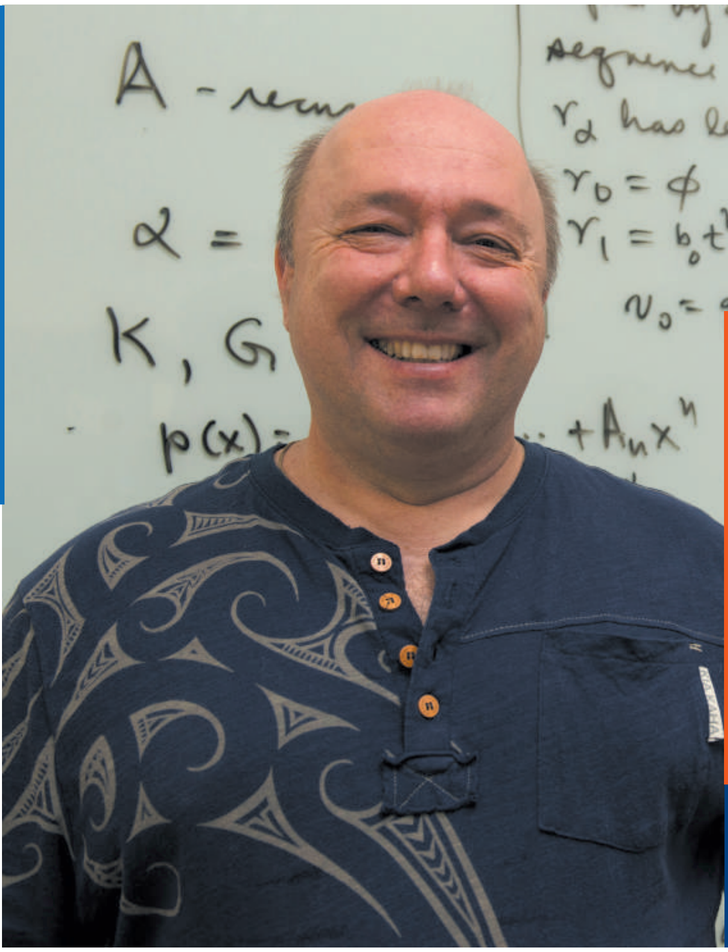
I **People who know you have described you as a selfless educator and a great collaborator. Do you have any philosophy of research that has contributed to this generosity of spirit and deed in a highly competitive academic environment?**

E That's very kind of people to say that and for you to pass that on. I don't know if there's a philosophy of research that I have, but it's a characteristic that I have, that is, a sort of insatiable curiosity for all sorts of different things, you know, always trying to read and learn about different things within mathematics and outside mathematics. So that's one way of satisfying that curiosity, just having good collaborators and good students helps a lot. You know, I have been late in collaborating with other researchers and students. So, I don't feel particularly generous in spirit. I feel that I am getting something from them more than they are getting from me. As to the highly competitive academic environment, I just feel that even though some place like Berkeley did maintain high standards, I still felt at that time in the early part of my career that there wasn't that expectation of a large number of publications. Maybe I should have felt more pressure at that time, maybe I should have been more aware of the pressure. I really didn't know about it. At that time I didn't feel I was under a huge amount of pressure to publish.

I **I read somewhere that students in Berkeley are very much left to themselves**

E Things used to be that way. It was certainly the case in the mathematics department. In the PhD program, for example, in the past, the university admitted many students and, coming from a good motivation that you want to give a lot of students a chance to get a PhD. The consequence was that you had only so many people who will be doing the advising. So, in the past, students didn't get that much mentoring, I think, and were left to themselves. Some students, of course, flourish with that while others floundered and found it difficult. There has been been a deliberate change to move away from that and to admit fewer students and to advise them more closely. And, of course, that means it is then tough because you take in only the very best students, and students who have a few blemishes on their academic records previously and who would have got into places like Berkeley and get a PhD, are now finding it more and more difficult to get in, which I think is something that we should now worry about because you can't always predict from someone's undergraduate record just how they are going to succeed in research, which is quite a different thing from doing well as an undergraduate in taking classes. You know, one just does not follow from the other, and you can't predict the result.

INTERVIEW



RODNEY GRAHAM DOWNEY:

THE ROAD TO PARAMETERIZED COMPLEXITY AND BEYOND

Interview of Rodney Graham Downey by Y.K. Leong

Rodney Graham Downey has made fundamental and far-reaching contributions to mathematical logic, effective algebra and theoretical computer science.

Of Scottish descent[4] Downey obtained his BSc from the University of Queensland in Australia and his PhD from Monash University. Subsequently, he taught briefly at the Chisholm Institute of Technology (now part of Monash), then National University of Singapore (NUS) and University of Illinois at Urbana-Champaign. He moved to Victoria University in Wellington in New Zealand in 1986 and was shortly promoted to Reader in 1991. He was given a personal chair in mathematics in 1995 and is reputed to be one of the top three computability theorists in the world.

Downey's research in the theory of computation, and complexity theory has led to deep contributions on the relationship between algebraic and descriptive complexity vs algorithmic complexity. Early in his career, he and his former colleague Michael Ralph Fellows did some ground-breaking and influential work in parameterized complexity. This was initially considered to be a peripheral area of classical and applied computability theory and complexity theory, and is now a flourishing area of computer science with regular meetings at *Schloss Dagstuhl* – Leibniz Center for Informatics. This center is considered to be the informatics (computer science) equivalent of the Oberwolfach Research Institute for Mathematics in Germany. Characteristic of his style of research, Downey moves from one field to another at regular intervals and has also done important work in effective algebra, reverse mathematics and algorithmic information theory. His current research interest is in model theory and computability theory.

His research output totals more than 280 (single or joint author) research papers in leading journals and conference proceedings. He has written 5 books: *Parameterized Complexity* (with Michael Fellows), *Algorithmic Randomness and Complexity* (with Denis Hirschfeldt), *Fundamentals of Parameterized Complexity* (with Michael Fellows), *Minimal Weak Truth Table Degrees and Computably Enumerable Turing Degrees* (with Keng Meng Ng and Reed Solomon), *and A Hierarchy of Turing Degrees: A Transfinite Hierarchy of Lowness Notions in the Computably Enumerable Degrees, Unifying Classes and Natural Definability* (with Noam Greenberg).

In addition, he has also co-authored and co-edited 14 books that are special issues of journal publications, or proceedings of workshops and conferences, and has served on the editorial boards of leading journals in logic and computer science, notably *Bulletin of Symbolic Logic, Journal of Symbolic Logic*, *Theory of Computing Systems*,

[4] Actually, of mixed descent. It also includes English, Irish, Welsh, Nordic, and Chinese.

Archive for Mathematical Logic and *Computability*. He currently edits 5 journals.

Downey's scientific contributions have been nationally recognized in New Zealand with numerous awards and honors, notably the Hamilton Award for Science, New Zealand Association of Scientists Research Medal for the best New Zealand based scientist under 40, Hector Medal (Royal Society of New Zealand), Fellowship of the Royal Society of New Zealand and the Rutherford Medal (the premier award of the New Zealand Royal Society worth $100,000).

The international recognition of Downey's work is clearly reflected by invitations to speak at the International Congress of Mathematicians, International Congress of Logic, Methodology and Philosophy of Science, and Association for Symbolic Logic (Gödel Lecture) and by his election as fellows of the following foreign scientific bodies: Institute of Combinatorics and its Applications, Association for Computing Machinery (becoming the second ACM Fellow in New Zealand), Isaac Newton Institute for Mathematical Sciences, American Mathematical Society, Australian Mathematical Society and Institute for Mathematical Sciences (NUS).

He has twice won the Shoenfield Prize (Association for Symbolic Logic) for articles on his work on randomness, and for his book on algorithmic randomness and complexity, Nerode Prize (European Association for Theoretical Computer Science) and Humboldt Research Prize Award (60,000 euros).

Though his passion in mathematical research is all-consuming, Downey has always been an avid sportsman. Besides having been, in his younger days, a volleyball state player, a rugby forward player at school, and a squash player of high standard, he is currently a keen tennis player while still maintaining his lifelong passion in surfing. Perhaps what is not so well-known is that he is heavily involved in Scottish country dancing in which he is a qualified teacher and choreographer with 5 books on dances.

Downey has a long association with the National University of Singapore (NUS), dating back to 1983-1985 when he took up his first permanent academic post, as a lecturer at NUS. However, he left NUS for a short visiting position at the University of Illinois at Urbana-Champaign and then moved to Victoria University of Wellington in New Zealand, which is geographically and culturally closest to his home country of Australia. In spite of the relative academic isolation of New Zealand during the pre-internet period, the move proved to be decisive – soon afterwards, he found in Michael Fellows a collaborator with whom he would make his first significant impact in complexity theory. From the beginning of his research career, he had already shown a restless streak in research collaboration. He would seek, on his own local as well as overseas grants to travel to the United States, especially to Cornell and Chicago. His gregarious nature soon established a routine of regular overseas visits to the United States, Europe and Singapore. Downey's attachment to Singapore is evident from the fact that he visits Singapore very often, practically every year (before the recent pandemic). Since 1993 he has been invited to NUS and NTU (Nanyang Technological University) as a visiting professor and as a member and fellow of the NUS's Institute for Mathematical Sciences (IMS) a number of times. He has collaborated with faculty members of NUS and NTU such as Chi Tat Chong, Frank Stephan, Yue Yang, Guohua Wu and Keng Meng Ng.

As a celebration of Downey's research contributions, his friends and collaborators within Singapore and without organised a workshop "Aspects of Computation" on parametric complexity, algorithmic randomness, classical computability theory and computable structures and reverse mathematics at IMS from 21 August – 15 September 2017. It was during his visit for this program that Y.K. Leong interviewed him on 12 September 2017. The following is an edited and enhanced version of the interview in which he traced the path he took from his early attraction to logic in high school in Queensland, Australia to the pinnacles of logic in the remote capital city of New Zealand. We also get a rare glimpse of the views of an unusually prolific mind on mathematical logic and theoretical computer science and of a not so glamorous side of mathematical research.

Acknowledgement. Y.K. Leong would like thank Von Ping Yap of the Department of Statistics and Applied Probability, National University of Singapore for preparing a raw draft of the transcript of the interview.

IMPRINTS I **In an autobiographical note, you have written that your interest in logic came in early during your high school years. Interest in logic at such an early age seems to be rather unusual for those who are gifted in mathematics. Most of them would be attracted to number theory, combinatorics, geometry and things of a more computationally concrete nature. In retrospect, do you see any indication in your early childhood of any tendency towards a logical or algorithmic attitude or approach in your thinking process?**

ROD G. DOWNEY D I think it's hard to recall my early childhood, to be honest. My parents were not well off and I know I went to 7 primary schools, so I guess I had "no fixed address" until grades 6 and 7, the last two years of primary school. I know at primary school, I was kind of turned on to math by a particular teacher I had there (a guy called Harry Seldon who, I remember, put up Pascal's triangle), going "Wow, look at that! That's really interesting." And I remember, at primary school I was reading the high school book on Euclidean geometry and I thought that was pretty interesting. I think the interest in logic would have come in secondary school because logic was offered as a subject at high school. And I just thought it was interesting; so I did what I wanted to do: Math I, Math II and Logic. I think I was the only person probably ever to do this combination of subjects. It was kind of fun really, you know, the problems of induction by Hume and modal logic and stuff.

When I went to university, I was planning to do chemistry, but when I got through first year chemistry, that was enough for me. I thought, you know, actually math looks a lot more interesting, especially when I was told I probably wouldn't be any good at it [mathematics] by my academic adviser. I had been pretty wild in my first year of university and had done almost no study.

But I don't know. Young mathematicians are often attracted to fundamental issues and there is no doubt that logic is concerned with fundamental issues, and in mathematics, it kind of feels good to study it [logic]. You feel like you're dealing with the fundamentals really.

I **But very few schools offer logic as a subject, isn't it?**

D Correct. It's no longer offered in Queensland, but back in those days they had [such] a curriculum. Funnily enough, logic was put in schools for those people who were not good at mathematics. If you're good, you could do like mechanics and things like that. We had a small number of options and "science" students did 6 subjects: Maths I, Maths II (mechanics), English, Chemistry, Physics and one other subject, usually "tech drawing" for male students and biology for female. A forgotten age. Logic was a maths alternative subject in the "humanities" or "commercial" streams and usually there was no crossover. The idea is that non-science students would not need, for example, calculus, but logic would somehow prepare you for reasoning in life. [Added December 2020: A bit of logic in our dealing with Covid-19 would have not gone astray.]

I **Many Australians would have chosen to go to England or the United States for their graduate studies in the 1970s and 1980s. Why did you choose to go to Monash University even if logic was done there?**

D Well, again, I think there were two reasons. Firstly, it was a different age. In some sense the reason I went to university at all was that Gough Whitlam had been voted into power in Australia, and universities had become free for all. I was very lucky and because of my family's modest means, I also got lots of financial assistance. But also I think there's a lot more opportunity to go overseas now than there was then (although this could be my ignorance in the 1970's). I applied for standard scholarships, like the Rhodes Scholarship and the Commonwealth Scholarship and things like that. I got interviews for the Shell Scholarship and the Rhodes Scholarship, but didn't get them. (Shell did make me a job offer, though!) I don't know why I applied, as I had no especial desire to have them. I just heard they were prestigious scholarships, but I had no particular reason [to apply for them]. And I think, more importantly, I was kind of naive in the ways of all things academic because, I mean, coming from a background where nobody in the family had ever gone beyond 10th grade, I had no idea why wouldn't Australia be just as good as anywhere else, you know? Then in my final year at university, I did get an interest in advanced mathematical logic (particularly computability related issues) studying things like the Eklof-Fisher work[5] on the decision procedures for abelian groups. So I kind of liked logic. The only people who did any computability theory anywhere in Australia were at Monash. So then I got a scholarship to go down to Monash to do a PhD. It's kind of funny that I've seen a lot of people who have got PhDs from the so-called power universities and who weren't actually that good in the long run. I speculate that perhaps this is because there's a vested interest in those universities to get people through PhDs. Of course, there are tremendous people that come through, but I'm just thinking about my own university [Victoria University of Wellington]. We have some people like that and we have three world-class mathematicians, Geoff Whittle, Robert Goldblatt and, I guess, myself (talking about "older members"; we also have a bunch of talented younger people. We definitely have a world class logic group.). Geoff Whittle's work was spoken about at a recent International Congress of Mathematicians – very, very difficult high-powered brilliant work in matroid theory. And Rob Goldblatt is extremely well known in logic and topos theory. Geoff Whittle got his PhD in Tasmania, and Rob Goldblatt got his PhD at Victoria University of Wellington. So he [Goldblatt] didn't even ever leave the university that he started from, which I don't think is really a good idea. None of these people did their PhD's at "power" universities. So I think somehow if you do your PhD away from the major centers, you may not get all of the ideas, but on the other hand, it's all your work and you really learn to work independently. And coming from that environment, you can still keep working as you have

[5] Paul C. Eklof and Edward R. Fisher, "The elementary theory of abelian groups", Annals of Mathematical Logic **4** (1972) 115 - 171

learned how to work independently, whereas I've seen some people who maybe can work at Harvard or somewhere, but when they're not in such rich environments it's much more difficult for them.

I **Was John Crossley your supervisor?**

D Yes. He was a supervisor in a kind of the British sense, which was rather hands-off in many ways. But he did have a lot of good visitors and an extremely good library, a personal library of preprints and things, which were important in those days. Because of that, I learned to do independent research. And Chris Ash was down at Monash, which was very lucky for me. Chris Ash was an extremely good logician and we spoke a lot. He committed suicide at a young age, sadly[6].

And there was another guy called John Stillwell. His PhD was actually in recursion theory but he later changed [his area] and he's now well-known for his expository works in the history of mathematics and in topology. I learned topology from him. I thought that was extremely interesting. I can't say that not doing my PhD in one of those major places hurt me in any mathematical sense, but it can hurt you in other senses. I'm now old enough to have been on more appointment committees than I ever want to be, but you see people go, "Oh, look at that person. They've got an MIT PhD." And I think, "Nay, so what?" I mean, let's see what they do in the years after their PhD, you know. I think we're too easily blinded by this vision of a hierarchy in our own minds, but this is still true around the world. I mean, it's very rare to go to an Ivy League school and find professors who weren't also educated at Ivy league schools. So "placement entropy" seems to operate, as well as an "old boy" (or at least "old person" network). I think I saw this with my eldest son [Carlton] too. He's did his PhD at Carnegie Mellon in machine learning, and it's an extremely good machine learning department that just has connections everywhere. This made his post-PhD path easier, I believe. So I guess that matters.

I **After your PhD, you spent a few short stints in Australia, United States and Singapore. You were, in fact, in Singapore for nearly three years before you moved permanently to Victoria University of Wellington in New Zealand after a year at the University of Illinois at Urbana-Champaign. It is understandable for you to leave Singapore, but to choose to move permanently to New Zealand rather than to the United States seems to defy logic. Is there any specific reason for this move?**

D Well, first and foremost, there's more to life than academics. The job situation was very tight in the eighties. All of the jobs had been filled up by people during the Sputnik years. And then as now, Australia didn't really employ logicians except in computer science. I was offered a job at NUS. I thought, "Why not?" And so I came over here and I found NUS a remarkably pleasant place to be in. I did lots of research there. I enjoyed working with [Chong] Chi Tat and Yang Yue and people like that. It was kind of fun. But there is more to life than the academics, you know. My wife and I really wanted to bring our family up in the kind of environment that we remembered being brought up in. And that meant Australia and perhaps New Zealand. We had been both to New Zealand. So we knew it was a reasonable place to go to. And when I went to New Zealand, it was going to be for four years, and then we'd go back to Australia. But here I am, still in New Zealand and now a citizen, because Wellington's a lovely place to be in. And you know, it's turned out to be a great department too.

I **Why not back to Australia?**

D Well, I didn't get offered a job, did I? It was pretty simple. By the time I could've got a job in Australia, I didn't want to move. Because we already had kids and I was already senior and the whole thought of moving, you know, ... I had opportunities later to possibly move to chairs and things like that but I didn't take them. We knew we didn't want to live in the US either. I have lots of friends who are Americans. I like visiting the place, but I don't think I'd like to live there. We wanted our kids to just go down to the beach and do those things that Australians and Kiwis do. It's just, you know, play rugby, cricket and all that kind of stuff.

I **You have a personal chair in New Zealand. They are quite generous to you in terms of being given the perfect liberty of doing almost anything you like, isn't it?**

D Oh, in terms of teaching, there's never been any expectation. The only thing I had to do when I got there as a junior person was that they needed someone to set up a discrete math program. And I basically set up the discrete math program at Victoria [University of Wellington]. And after that, when I was doing more advanced courses, I could teach whatever I like. And so I remember at one stage, I thought, "Well, it could be fun to learn some computer science." So I taught complexity theory for a few years and that's where probably parameterised complexity came from and meeting Mike Fellows, of course[7]. This academic freedom

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[6] Christopher John Ash (1945-95)

[7] This meeting and subsequent development of parameterized complexity is reported in "The birth and early years of parameterized complexity," *The Multivariate Algorithmic Revolution and Beyond*, Essays Dedicated to Michael R. Fellows on the Occasion of His 60th Birthday, Lecture Notes in Computer Science, Vol. 7370 (ed. Bodlaender, Downey, Fomin and Marx) Springer-Verlag LNCS 7370, 2012, 17-38.

was a kind of nice thing. Of course I always did my service teaching and I think that it is important that senior faculty are involved in, for example, first year teaching.

There were few constraints on me because I was always really productive and no one would ever have questioned that I wasn't working hard. And the other thing is, of course, in the early days, I always found that small trips to places and then working hard afterwards was definitely a way to be research productive. So I'd go and visit Richard Shore[8] at Cornell or Carl Jockusch at Urbana-Champaign or someone like that. I learned a lot from Richard, Carl, Ted Slaman[9] and others who changed my thinking about mathematics. And I'd work there for a while over a few weeks and then come back and look at the details. I don't necessarily find living in someone's pocket overly productive ... How often do you see people working with people in the same department over a sustained period? It's kind of rare actually. (I guess Hardy[10] and Littlewood[11] spring to mind.)

So the *only difficulty in the early days* was actually getting overseas because there was zero research money available. There was no granting agency (in New Zealand). So I used to beg these people. "Oh, look, if I can give you three talks here and three talks there, can you come up with, you know, a few hundred dollars to help pay for my airfare and things?" It all kind of worked out in the end. And now, of course, in New Zealand there's the Marsden Fund, which was a great initiative and funds blue sky research. Two good things happened to me: first, the internet came to be. I think the internet's made a big change for researchers away from the central places. If you go back probably a hundred years, any decent German university probably had more mathematicians than there were in the US, certainly at Harvard or something like that. Then there was a movement to the [United] States because of the War and things. So only then did US mathematics flower. And the internet came in in the early nineties and it was great because, instead of taking two months to write a letter to somebody and get a reply, it was kind of instantaneous. You can write a letter and you get an answer and you go, okay, and that kind of thing. So I could bug people really quickly rather than slowly. And the Marsden Fund has been very generous. I've been lucky enough to get lots of Marsden grants, and I've had great postdocs down through the years. I have had 22 of them, and many now occupy positions at leading Universities (like Chicago, Berkeley, etc), so I was lucky to have such brilliant people work with me when they were young.

I You once mentioned there was kind of a fighting spirit that spurred you to do well in your undergraduate studies. Do you think that this is indicative of if not characteristic of a kind of contrarian spirit that drives you in your early research work in areas that were not considered to be promising?

D I did see the questions beforehand and my wife said "absolutely yes" to this. [*Laughs*] My view in mathematics is kind of weird in the sense that I like to work on what I like to work on. And if something's interesting, then usually I find that if you follow your intuition, it probably will be interesting. And you know, I'm not one of these people that have, like, the reef fish effect with what's the current "research target" that the reef fish are following. This came, for example, with parameterized complexity. I mean, we used to get very discouraging reviews of that in the early days when people would go, "This seems to be a very small thing." (Even reviews saying, in effect, "if this is important why aren't people from e.g. Berkeley doing it?") And now there're millions of euros going into the research in Europe and things like that because it's turned out to be useful. People are using parameterized complexity methodology now in Australia for trying to solve deafness in Aboriginal children. So it's kind of shocking to me actually. I think you just got to follow your intuition rather than trends. I do what I think is interesting and also I get bored after about a decade; I want to do something else.

I Although one often refers to "logic and set theory" as if they were inseparable twins, there is no distinctive field that combines logic and set theory in the way that fields like algebraic geometry, differential geometry, topology, algebra, algebraic geometry, etc combine the language and tools of two different fields. Do you see any signs that logic and set theory are drifting further from each other, logic towards more applications like applicable disciplines like computer science, and set theory towards rarefied universes of its own, with its own questions?

D It's a very long question. Well, I'm not a set theorist and I don't actually know that much about set theory, to be honest. I mean, my understanding of pure set theory is that they have their own internal questions that have driven them like "What is the real value of the continuum?" But, of course, there are areas like descriptive set theory, which have been around since

[8] See "Interview with Richard A. Shore: Logic, Mathematics, Computer Science" *Imprints* Issue 30, July–December 2017,16-22
[9] See interview of Theodore Slaman and W. Hugh Woodin in *Creative Minds, Charmed Lives*, pp 265-273, World Scientific, 2010
[10] Godfrey Harold Hardy (1847-1947)
[11] John Edensor Littlewood (1885-1977)

Lebesgue[12] and Luzin[13] , and which are going through a tremendous development in the last few years with initiatives like Borel[14] reducibility where you're trying to understand the relative complexity of problems from analysis and things like that. This is a very flourishing area and it's actually interacting with the rest of logic and the rest of recursion theory.

This is an area that I'm working in myself currently too, in a kind of modest way, in a project trying to give a measure of complexity to discontinuous functions on the reals. So a function is continuous if and only if it's computable relative to an oracle. If I give you some kind of oracle, which tells you how to take rational balls to rational balls, which is a countable collection of things, so you can code them up with an oracle. Then if I know that oracle, then relative to that oracle, I actually have a computable function on the reals. The trouble is that it's more difficult to do this with discontinuous functions. So there's a nice problem: How do you extend a notion of complexity to discontinuous functions or how do you measure the complexity of a discontinuous function? And this is a really nice project because you're kind of climbing your way up through what's called the Baire hierarchy. This is some work with Linda Westrick and Adam Day. Amazingly enough, some of the fine reducibilities that we looked at turned out to correlate to things that people had studied classically, like there was a hierarchy called the Bourgain hierarchy, which we knew nothing about. And it turned out that viewing things computationally turned out to be identical to viewing things the way that people viewed them classically. The Bourgain hierarchy kind of classifies Baire functions. It classifies Baire class 1 functions according to how much they wiggle.

I So you're going back to the roots?

D Yeah. Logic does lots of those things. I remember talking to algebraists (who want to know what logic can do for them) and saying. "Well, we prove, for example, that the isomorphism problem for torsion-free abelian groups is what's called "analytic complete". Now, what does that mean? Well, people study invariants. They want to know what's the dimension of a vector space. There are the Ulm invariants for abelian groups and things like that. Well, logic tells you how not to do things. Proving this isomorphism problem is analytic complete essentially shows that there's no way of assigning invariants of torsion-free abelian groups. That is, no "invariants" can simplify the problem below to make it easier than the trivial invariants of "isomorphism type". So in other words, there can't be any invariants by showing that it's as hard as any other isomorphism problem, which is kind of a nice result. It's using logic to answer a question you might have asked, which is a classical question.

I It seems to me that in logic you sort of refer to the negative in a certain way. You cannot do this. You cannot do that.

D Well, there are lots of examples of that. But there're always positive aspects to negative answers; not that I suggest that my work is like Turing's, but we witness in Turing's work on the "*Entscheidungsproblem*" ["decision problem"], the key idea is a compiler ultimately. Look at compilers all around the world today.

I But he didn't use the word compiler at that time.

D It was a conceptual compiler. The idea that you can have a single interpreter which does the work rather than designing a machine for each computational purpose. But there are also lots of positive mathematical applications of logic, such as new proofs of various results using methods from logic. For example, work by Niel Lutz (Jack Lutz's son). They're using computable methods to analyse Hausdorff dimension. So what happened was, I think, there was a divergence in logic while it was being developed, particularly computability theory. But now there's a big movement towards pushing it back into the areas from whence it came, towards understanding the computational content of classical mathematics.

There is also a reason that logic is intertwined with computer science. People like Moshe Vardi[15] observe that it is the calculus of computing. But I was never drawn to proof theory.

I Set theory seems to be going the other way round.

D I don't feel competent really to comment. For descriptive set theory, that's certainly not true [that it's going the other way]. Descriptive set theory, which is a certain subarea of set theory, has never left analysis. It's always been intertwined with analysis. And I guess also, [Saharon] Shelah's work on classification (that's all about mathematics) that tries to answer questions like "When do theories have invariants?" in a different way. His view is that (this is my view of his view; you've got to read that monstrous book of his)[16] either a theory should

[12] Henri Léon Lebesgue (1875-1941)

[13] Nikolai Nikolaevich Luzin (1883–1950)

[14] Félix Édouard Justin Émile Borel (1871–1956)

[15] See "Interview with Moshe Vardi: Dare to Know" *Imprints* Issue 32, July-December 2018, 12-20

[16] Classification theory and the number of nonisomorphic models", Vol 92 of *Studies in Logic and the Foundations of Mathematics*. North Holland Publishing Co., Amsterdam, second edition, 1990

have a small number of models with a certain collection of invariants, and here they are, or they just have too many models to have invariants. Then they basically look like trees and there are just too many trees. So you can't hope to have invariants. This is called the Dichotomy Theorem.

I Other than the famous P = NP problem, what are some of the central problems of computability and complexity? Do you see any prospect of the P = NP problem being solved within the next 10 years? (There are actually two questions here.)

D Well, I think it's fair to say that most areas of mathematics have their own internally generated problems. And they gain importance when people can't solve them. Sometimes it's not even clear why they're important; it's just that people tried to solve them. Witness Fermat's last theorem. The fact that it's solved kind of damaged the research. I mean, it has developed Kummer's theory of ideals, all kinds of wonderful things, but now it's being solved, right? Of course, computability and complexity has had many, many of their own problems. I can name some of the things: Vaught's conjecture, Martin's conjecture, and these are internally generated problems, which give insight into the areas of their study. And most areas that I've kind of moved into or looked into are full of these things: the classification problem, the invariants problem. There're all kinds of problems like these. But are there other really profound things in logic which can transcend mathematics like the P versus NP question? Probably not. It's hard to say. I think that history decides these things.

Anyway, there are a lot of problems in complexity theory which seem just as hard as the P versus NP problem. For instance consider the P versus BPP problem, where BPP stands for "bounded probabilistic polynomial time". BPP are more or less those things which have probabilistic algorithms. And the famous example of something in that class was primality testing, and that was known to be BPP by using the Solovay-Strassen[17] algorithm, for example, which is a probabilistic algorithm with one-sided error. And it either says no, the thing is not prime, or yes, it may be prime with some error like 1/2. But if you then do it a thousand times independently, the probability that you have an error believing that it is prime, when it's not prime is like very, very low. However, this problem was famously proved to be in polynomial time (by a very bad algorithm in terms of running times). That is, a *BPP* problem was re-classified in to *P*. And now it is thought that everything in that class *BPP* is in polynomial time. When I was young, we thought BPP did not equal P, but now there's a belief that actually *BPP* = *P*: That is, everything that has these randomized algorithms can actually be de-randomized. We have no idea how to do prove this and it's kind of an important problem because, we do have *BPP* algorithms for lots of things.

Polynomial identity testing (PIT) is a good example. The problem is specified by being given a polynomial in a whole bunch of variables over a finite field. The question I want to answer is, "Is the polynomial uniformly zero?" For example take $z_3z_1z_2 - z_2z_3z_1$, which is always zero no matter what input I give it. There is a randomized algorithm for PIT but the algorithm is kind of dumb. You just put in random values from the field, assuming the field is large enough. And if you get zero, you go, "yes". If you don't get zero, you go, "no". "No" is definitely correct. If you say yes, then it is probably correct because the chance of guessing a zero is very low as zeroes for nontrivial polynomials are very sparse. And why is this an important problem? Because there are lots of things which can be transformed into PIT using an efficient reduction. Thus to solve our problem we transform it into a polynomial and solve polynomial identity testing. (Examples of this methodology can be found in my book *Fundamentals of Parameterized Complexity* with Fellows.) We have absolutely no clue how to derandomize PIT. So that's a nice problem.

There're lots of problems like this that we have that we're not even close to solving. And it would be hard to believe that all use the same trick that *P* versus *NP* does. But, on the other hand, you know, there's another one (my favorite): "Is W[1] = FPT?"[18]

But actually, if you want a really important problem [for practical computing], I say to myself, what is the goal of computer science? What is the goal of computability theory? And I think the answer is: to understand computation. And so I think that the biggest problem that I'd like to try and solve is the following. I don't know how many mathematicians out there would know that, in fact, Sat Solvers (solve the satisfiability problem for propositional logic, something which is a fundamental NP-complete problem) are pretty efficient in industrial problems. So what you do is – it's a very weird thing – you take an industrial problem and you use an efficient reduction to convert it into an instance of satisfiability. That's NP complete: satisfiability. And then you run it through a Sat Solver, something which has been algorithmically engineered to solve this. Now, we know that theoretically it won't work, but for lots of natural problems appearing in real life, it works very well indeed. You know, NASA uses it for their independent robots and all that kind of stuff. We have no idea why it

[17] Robert Martin Solovay, Volker Strassen
[18] W[1] = FPT, a central problem of parametrized computability

works well on such problems. I mean, we honestly don't know why. What is it about the topology (or something) of the "real world" which makes seemingly intractable problems tractable? If we could understand this it would revolutionize algorithm design.

I'm assuming P doesn't equal NP; of course, it would be revolutionary if P equals NP with an algorithm that is efficient. You know, modern banking security would be destroyed, and many computational tasks would become wildly more efficient. But I actually don't believe that $P=NP$ with an efficient algorithm. (If $P=NP$ via an algorithm which took polynomial time with an exponent or constant such as the number of atoms in the universe, it would not really be of much use.) Thus, if we could really understand what it is about the universe that enabled things that theoretically shouldn't work but does work most of the time, then that insight would enable us to more efficiently design things. This is a really hard problem, but I think it's the big challenge. Computing practice generating theoretical questions in discrete mathematics is an analogue to the fact that classical physics generated a lot of classical mathematics. Computing is a great source of wonderful problems in mathematics. Suppose a hundred years from now, we prove $P \neq NP$ is independent of the axioms of set theory, this problem of explaining practical tractability would be still a very important problem to solve. And I think that's a tremendously good problem. So, in other words, invent a theory of complexity that explains the actual behaviour of algorithms in real life. And we lack that completely. I mean, an average case doesn't work, a generic case doesn't work. Parameterized complexity explains things some of the time. There're all kinds of different approaches, but we don't have any approach in genuine mathematics. It could make a difference to mankind or something like that.

I **Could there be any sort of limitations in the human brain that actually prevents certain problems from being solved?**

D Almost certainly. But I think we won't know. I mean, if you take the "hard AI" view that we're just machines and therefore the incompleteness theorem must apply to us, then there are things that we won't be able to solve. The question also involves AI. I am aware that machine learning can solve hard problems like the game of Go better than humans. But also important problems like protein folding with far greater accuracy and we don't know how it does it! I wonder if we might get to the same state in mathematics.

I **Do you think we're hardwired for logic?**

D I would have thought so down through the years, but having seen recent events in politics, I'm not completely sure I'd agree with that. I thought the world was becoming more logical until recently. I mean, logic goes back a long way, but it was still an achievement. Throughout history, mathematicians regard Greek mathematics with awe. I mean, there was a good reason for that: it was an amazing achievement, really. Euclid's *Elements* is an amazing book. There is a reason that that horrible machine learning used on the net targets emotional responses rather than reason. We are emotional beings.

I **Is there such a thing as evolutionary logic?**

D Evolutionary logic ... what does that mean?

I **I mean, we talk about evolutionary psychology, evolutionary biology.**

D There are things called non-monotonic logic that studies the logics of things evolving with time. They use this in computer science quite a lot. There's something called linear logic, which is actually a kind of "capitalist" logic. You kind of only have a certain amount of things to spend. And every time you use something, you spend some of these. Non-monotonic logics are, generally speaking, a kind of logic of belief. You believe things and you'd make deductions, but then as things change, you update your beliefs and then maybe things you thought of before were no longer true. And, of course, that's very similar to the kind of logic that we use for the scientific method.

I **Some call it fuzzy logic.**

D Fuzzy logic, yeah. I remember one of my lecturers, when I was an undergraduate, said to me: "You know, fuzzy logic, that's an example of mathematical pornography." which I thought was hilarious at the time. And it turned out that fuzzy logic is actually used in these circuits in Japan, you know, these logic circuits that dry your washings and all that. They're using fuzzy logic. So who knows, I mean, there we go. Who knows what's important?

I **How do you choose the problems to work on? Do you prefer to work on hard problems?**

D Well, I just follow my intuition. If I look at something that looks good or doable, I'll give it a go. There are certain areas that I don't particularly want to work on because they're not to my taste. But sometimes you're driven by the need to find problems for students to do. That's about all, but usually I get bored after a while. Then I want to do something new. I like that initial learning when you get into a new area, when you kind of learn what's going on in that area. I like the challenge. So I had my parameterized complexity decade, and then I got into randomness. And now I'm more interested in model theory and computable analysis, also online algorithms, and who knows what. Maybe I've only got a few years left, I suppose.

I Which result of yours surprises even you yourself?

D Well, there are lots of things that I've worked on. The answers have surprised me. I mean, there are lots of things which I thought were true turned out to be false. Lots of things that [I thought] were false turned out to be true. I think the thing which really did surprise me was in parameterized complexity. The stuff we [Mike Fellows and I] began working on years ago really has turned out to be actually useful and we wouldn't have anticipated that. I know we didn't anticipate it in the early days because I have old correspondence from Mike on where we were going, "Oh, do we think this would ever be useful?" And it turned out to be quite useful, both in terms of the mathematics they're now using, for example, the techniques in low dimensional topology and providing algorithms much better than the currently known algorithms in, for example, computational biology. So those were great surprises to me. You know, my wife was surprised too. She said, "You did something useful." I said, "Well, it wasn't my fault, [*Laughs*] I didn't try."

I There is a persistent perception, that mathematics is a closet activity with mathematicians working mainly on their own as exemplified by the well-known successes of Andrew Wiles and Yitang Zhang. How much of this perception has changed during the past 10 years?

D I think you can't avoid the fact that mathematics is a social contract. It has its own society and each sub-area of mathematics has a different way of working. And I think there are certain areas of mathematics where people do tend to work a lot by themselves. You could say that with number theory. Yet look, Terry Tao has lots of co-workers and is notably, I think, one of the first to get a Fields medal for lots of joint work. I mean, previous Fields medal winners didn't have much joint work. I think the fact that communication is so easy now surely must encourage people. I know there's this stereotype of the mathematician that sits in his room and doesn't talk to people and indeed I've met such people. But, on the other hand, I've met lots of mathematicians that like working with other people. I love working with people. It's much more fun, and you have that sense of competition, especially with younger people and you also have this to-and-fro flow of ideas. Why wouldn't you want to work with other people? It's bizarre to me that people wouldn't want to work with people. I've had 20-odd postdocs now, and I stay in touch with all of them. I mean, try and keep working with them. So yes, I think it's now changed. Maybe not in some areas, but who knows? Of course, there are some areas of science where you've got to have Sherpas. I mean, you can't do big experimental data without lots of Sherpas to help you with your research and experiments. I think in mathematics, certainly in computability and complexity theory, the number of authors per paper seems to be rising. There is surely a reason that places like Google, Facebook, etc have teams of people working on their problems, as the sum of lots of very smart people is certainly greater than its parts.

I It's not so much with pure mathematics. Maybe in applied mathematics, there is more collaboration.

D Well, in applied mathematics, you've got to have people doing computations and things. As data gets more complex to understand, you need more skills being brought to bear. I'm sure there will always be pure mathematicians who are the lone geniuses that just want to do what they want to do. But I think it's much more fun to work with other people. You've got to find people who care about what you care about too, because there are only so many people in the world that care about Martin-Löf randomness, or other specific things you care about. You should get to know them.

I On a philosophical note, mathematics is something that is sort of drawing necessary conditions. So if you're very, very bright, or let's say you have an infinitely intelligent person, then mathematics is really easy, isn't it?

D Is mathematics easy? I wouldn't know about super bright people, but I don't think I've ever met a mathematician who was successful and who didn't really work hard. When I was at university in my early years, I was pretty lazy because I had this kind of belief that, well, you know, if you kind of knew the axioms and kind of knew how things worked, you could survive (i.e. pass exams). But once you get to doing research, forget it, right? You really have to work hard. I've met people that *pretend* not to work hard. Well, I think there was an old British thing that you're supposed to pretend that you were just occasionally coming back from the golf club or something like that. But of all the people I know whom I would regard as really good mathematicians, they work hard. There're no two ways about it. You know, it's like if you work hard, you get lucky. You've got to be pretty obsessive. If you want to be good at tennis, you've got to be pretty obsessive about practice. If you want to be good at mathematics, you've got to be pretty obsessive about the problems. And they've got to be in your head all the time. You've got to fill yourself with them.

> **"I THINK YOU CAN'T AVOID THE FACT THAT MATHEMATICS IS A SOCIAL CONTRACT. IT HAS ITS OWN SOCIETY AND EACH SUB-AREA OF MATHEMATICS HAS A DIFFERENT WAY OF WORKING."**

I This brings me to the next question. You have once written that mathematics is also a social activity and yet the breakthroughs are dependent very much on personal insights which occur only after an intense and almost obsessive preoccupation with a problem. What is your personal recipe for success in mathematics?

D My recipe is just as I would tell a PhD student, that if you really want to succeed in mathematics it's got to be something you can't do it from nine to five. It's got to be something you obsess about. One of the things that always annoy me is that, if I'm doing an administrative role, I wake up in the night thinking about how am I going to get some new course in or something like that. Whereas I should be waking up in the night thinking about a [research] problem that I've been working on. But I don't see how it's possible to achieve in mathematics without real work. I like working for short periods of time with people, but then I like going away and rolling it over in my head. And, you know, there are problems I've been working on for years that I still haven't solved and they come back to haunt me constantly. I don't know how other people work, but this is the way I've always worked: "Think really hard." If I do it too much, too long, I wake up [at night], I start feeling kind of unwell from indigestion and things. Like anything else, you've got to have little breaks. But if you're going to work on something, you really got to work at it. You got to immerse yourself in the problem. It's the only way to go, just like anything else. I mean, if you're going to do something, you might as well try and do it well. I guess I had a really great training from my parents who didn't really care about success but about effort. I mean, they had no idea what I was doing, of course. They didn't really care about anything I did as long as I gave it my best shot. If you succeed, great; if you don't, well, you can't look back and go, "Well, I didn't try." I find mathematics very, very personally rewarding when things work out. It's very personally frustrating when things don't work out. To a student or a young researcher, I say that the most difficult thing in mathematical research is coping with frustration and getting used to the fact that most of the time you lose. The opponent wins and the opponent is more powerful than you are. If you work on hard problems, you will fail most of the time. But, you know, sometimes you get a little insight and that's good.

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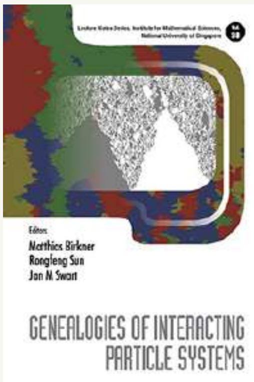

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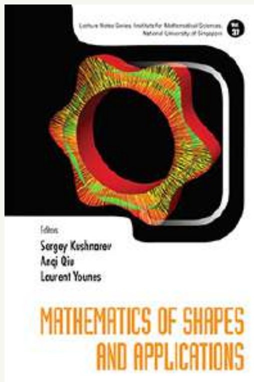

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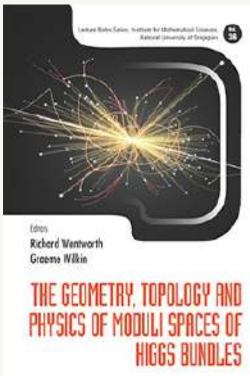

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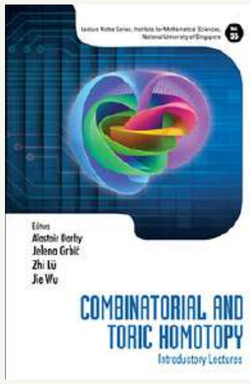

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