IMPRINTS

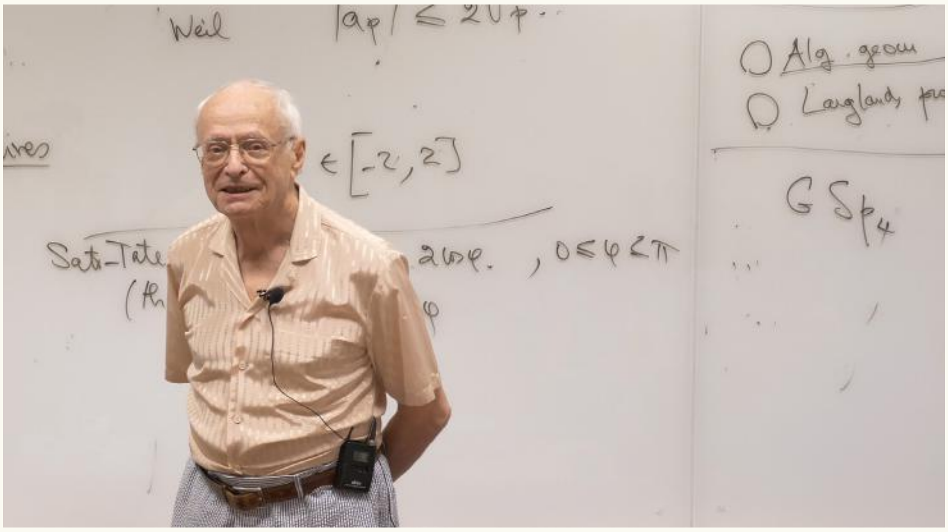
JULY – DECEMBER 2018 ISSUE 32

NUS National University of Singapore | IMS Institute for Mathematical Sciences

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

Jean-Pierre Serre: IMS Distinguished Visitor

BY CHI TAT CHONG



Jean-Pierre Serre

Professor Jean-Pierre Serre visited IMS as a distinguished visitor for the period 21 June to 1 July 2018. This was his fifth visit to Singapore. Serre first came to Singapore in February 1985 under the French Academic Exchange Program and delivered a public lecture organized by the Singapore Mathematical Society. In 1987 he returned to Singapore and gave a series of tutorial lectures at the Singapore Group Theory Conference (''Galois group extensions of *Q*'', ''Tits buildings'', and "Trees and group acting on trees"). He was at the IMS inaugural program in 2001 and delivered a lecture entitled "Codes, curves and Weil numbers".

This year, Serre gave the 2018 Oppenheim Lecture at the Department of Mathematics, National University of Singapore ("Number of points mod p as p tends to infinity") and a lecture at the Pan Asian Number Theory Conference held at the IMS ("Logarithmic capacity and equidistribution of algebraic integers"). At age 92, Serre was the most senior mathematician to have ever visited Singapore. Despite the advanced age, one of the world's most distinguished mathematicians displayed an abundance of energy and enthusiasm for mathematics.

WATCH HIS TWO TALKS ON OUR YOUTUBE CHANNEL

youtu.be/CoGMWDCmfUQ
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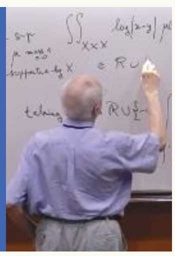


FEATURED

NEWS

OTHERS

Dynamic Models in Economics

From 4 – 22 June and 2 July – 3 August 2018, the Institute hosted a program on "Dynamic Models in Economics". The co-chairs of the program contributed this invited article to Imprints.

BY YI-CHUN CHEN AND YENENG SUN

Yi-Chun Chen and Yeneng Sun

In real life situations, most decisions are made in a dynamic context where multi-period decisions influence the final outcomes. The games of "chess" and "go" are simple examples. In a now classical paper, Zermelo (1913) showed that in any two-person zero-sum game of perfect information in which the players move alternatingly with finitely many choices, either one of the players has a winning strategy, or both players can individually ensure a draw. The more general class of dynamic games with complete or incomplete information, where the players may observe the history and move simultaneously in finite or infinite horizon, arises naturally in various areas of economics, political science, and biology. The associated notions of subgame-perfect/sequential/perfect Bayesian equilibrium are fundamental solution concepts in game theory. Besides the usual game-theoretic set-up with finitely many players, there is also the need to analyse games with a large number of competing participants, which leads to the study of the so-called large games or mean-field games. Those topics and related issues were discussed during the workshop on game theory, held in the week of June 4 – 8.

The second workshop, held in the week of July 9 – 13, focused on the theory of mechanism design which is, in certain sense, "reverse game theory". Its aim is to design particular games to achieve certain desirable social outcomes (e.g., efficiency) or to maximize the revenues. Standard examples include auction, trade, and public good provision. Much of the literature focuses on the static setting with only one-shot decision. In many problems of interest, the private information of agents and/or the set of allocations available change over time. As a result, the desirable class of mechanisms crucially depend on the dynamic feature of the environment. It should come with no surprise that the standard techniques for the static problems often cannot be directly applied to the dynamic setting where the evolution of private information plays an important role. Consequently, fundamental results such as efficient and revenue-maximizing mechanism design are significantly more difficult to establish in the dynamic setting than in the static setting. In order to address the difficulties, novel tools beyond those employed in the static contexts need to be developed. Towards this end, strong connections have been explored and established between mechanism design and mathematical methods such as linear programming, Markov decision processes, and stochastic games. The literature on the dynamic mechanism design is quickly growing in the last decade.

The workshop on matching search, search, and market design held in July 23 – 27 focused on the issues of matching and market design which receive considerable attention recently. Market design studies how to match people to other market participants or goods is an important problem in society. Prominent examples include (1) student placement in schools, (2) labor markets where workers and firms are matched, and (3) organ donation, in which patients are matched to potential donors. How can such matching be accomplished efficiently? What methods are beneficial to what groups? Starting from the seminal work of Gale and Shapley (1962), the economics of "matching and market design" answers these questions from abstract theory to practical design of markets. While the classical literature mainly focuses on static matching problems where agents' characteristics and preferences are fixed, many matching markets share the features that matching opportunities arrive over time, and matching is irreversible. As a result, the option of waiting for a better match is valuable. For these markets, it is crucial to understand the institutional rules that regulate the matching outcomes and the incentive to wait for a better match. These are the novel and important questions which the rapidly growing literature on dynamic matching market is trying to address. Economists and geneticists, among others, have implicitly or explicitly assumed the exact law of large numbers for independent random matching in a continuum population with continuous time. This result had been relied upon in large literatures within general equilibrium theory, game theory, monetary theory, labor economics, illiquid financial markets and biology without a proper foundation. A resolution of this problem was also presented at the workshop.

Besides the lectures given by six distinguished speakers, the Summer School of Econometric Society, held in June 15 – 19, offered a unique platform for promising graduate students from universities worldwide to present

Continued from page 2 ↘

their ongoing research projects. It consists of eighteen 30-minute talks presented by graduate students now studying at top universities including Harvard University, MIT, Princeton University, Northwestern University, University of California at Berkeley, New York University, Duke University, Toulouse School of Economics, University College of London.

The four workshops/summer school were well attended and contributed by more than 140 participants in the region and overseas. The programme including three 2-hour IMS distinguished lectures (by Darrell Duffie and Yuliy Sannikov of Stanford University, and Philip Reny of the University of Chicago), three 3-hour tutorials, eleven 1.5-hour mini-courses, one public lecture attracting more than 50 participants, and a total of 81 other research presentations of 30-45 minutes each. Each workshop was also attended by a number of local and international research teams making use of the event to discuss their ongoing projects and exchange new ideas. Researchers working on similar topics grasp the opportunity to solicit feedback and listen to the most recent progress in their areas. The discussion was highly engaging and focused during not only the presentations but also tea breaks, lunch, and dinner time.

Zermelo, Ernst (1913), *Über eine Anwendung der Mengenlehre auf die Theorie des Schachspiels,* Proc. Fifth Congress Mathematicians, (Cambridge 1912), Cambridge University Press 1913, 501-504

Gale, D. and Shapley, L. (1962), *College Admission and the Stability of Marriage*, American Mathematical Monthly, 69, 9-15.

IMS Distinguished Visitors

YULIY SANNIKOV

Yuliy Sannikov

Yuliy Sannikov is the Jack Steele Parker Professor of Economics at Stanford Graduate School of Business and the 2016 winner of the John Bates Clark medal, which is awarded by the American Economic Association to "that American economist under the age of forty who is adjudged to have made the most significant contribution to economic thought and knowledge". He also won in 2015 the Fischer Black Prize, which is an honor for a leading young finance scholar, analogous to the John Bates Clark Medal in economics and the Fields Medal in mathematics. By using stochastic calculus, Sannikov has significantly enriched the toolbox for studying dynamic games. As a result of his contributions, new areas of economics, game theory, and finance have become tractable for rigorous theoretical analysis.

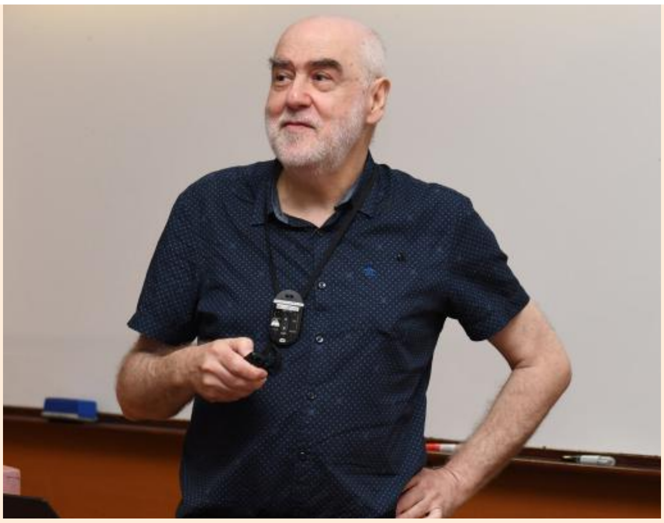
***Professor Sannikov** visited IMS for the program on Dynamic Models in Economics (4 – 22 June 2018 & 2 July – 3 August 2018). He gave two talks on 9 and 10 July 2018.*

Philip J. Reny

PHILIP J. RENY

Philip J. Reny is the Hugo F. Sonnenschein Distinguished Service Professor in Economics and the College at the University of Chicago and a member of the American Academy of Arts and Sciences (2015). His current research focuses on the existence of Nash equilibrium in discontinuous games, methodologies for analyzing rational behavior in extensive form games with infinite actions and types, and optimal mechanism design with multi-dimensional private information. Reny serves on the board of editors for the American Economic Journal: Microeconomics and was the head editor of Journal of Political Economy. He is also a Fellow of the Econometric Society (1996), the Society for the Advancement of Economic Theory (2012), and the Game Theory Society (2017).

***Professor Reny** visited IMS from 1 – 16 July 2018 for the program on Dynamic Models in Economics (4 – 22 June 2018 & 2 July – 3 August 2018). He gave two talks on 12 and 13 July 2018.*



Gunther Uhlmann

GUNTHER UHLMANN

Gunther Uhlmann's research concentrates on inverse problems and cloaking. He has done pioneer work on the Calderón of determining the conductivity of an object by making voltage and current measurements at the boundary. He has also pioneered the method of transformation optics to achieve invisibility. This leads to a proposal on how to build Harry Potter's cloak.

Professor Uhlmann received his PhD in 1976 from the Massachusetts Institute of Technology. He has been Walker Family Endowed Professor in Mathematics at the University of Washington since 2006, and is also Si-Yuan Professor at the Institute for Advanced Studies at the Hong Kong University of Science and Technology since 2014.

Professor Uhlmann is Fellow of the American Mathematical Society, named a Finland Distinguished Professor (2013), Rothschild Distinguished Visiting Fellow at the Isaac Newton Institute of Mathematical Sciences (2011) and Chair of Excellence (2012) of the Fondation Sciences Mathématiques de Paris. He is also Member of American Academy of Arts and Sciences and Foreign Member of the Finnish Academy of Sciences. In 2011, he was awarded the Bôcher Memorial Prize by the American Mathematical Society and the Kleinman Prize by the Society of Industrial and Applied Mathematics. In 2017, he received the Solomon Lefschetz Medal by the Mathematical Council of the Americas. He is on the editorial boards of many mathematical journals, including Inverse Problems and Imaging and Analysis and PDE.

***Professor Uhlmann** visited IMS from 4 – 18 August 2018 for the program on Theories and Numerics of Inverse Problems (6 – 17 August 2018 & 24 – 28 September 2018). He gave four hours of tutorial lectures and one talk on 13 August 2018.*

DARRELL DUFFIE

Darrell Duffie is the Dean Witter Distinguished Professor of Finance at the Stanford Graduate School of Business, and a Fellow of the American Academy of Arts and Sciences. He was an Independent Director of Moody's Corporation in 2008 – 2018. He is considered by many to be one of the most influential financial economists in the world today. He has developed the modern toolkit of term structure and credit modeling, which stands out for its immense practicality. He was President of the American Finance Association in 2009, Chair of the international Financial Stability Board's Market Participants Group on Reference Rate Reform in 2013 – 2017, and the Fisher-Shultz Lecturer at the World Congress of Econometric Society in 2015.

***Professor Duffie** visited IMS for the program on Dynamic Models in Economics (4 – 22 June 2018 & 2 July – 3 August 2018). He gave two talks on 24 and 25 July 2018.*

Darrell Duffie

IMS arranges visits to the Institute by distinguished scientists who are prominent leaders in their communities. The program started in 2015. This initiative aims to enhance the diversity of people participating in our research programs, and provide mentoring/ inspire junior researchers and graduate students. Each distinguished visitor spends at least two weeks in Singapore, and participate in a variety of activities, including lecturing about their own research, give public talks, meet with faculty, and interact with program participants.

Under this program, the Institute has enjoyed visits from a stellar array of distinguished scientists. The list of distinguished visitors may be found on our website.

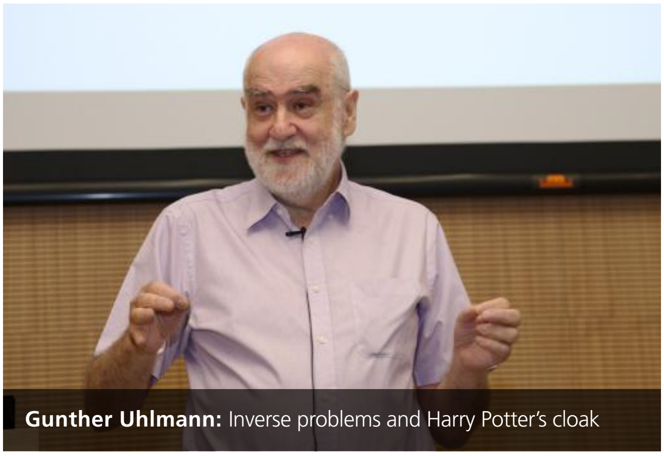
Ng Kong Beng Public Lecture Series

24 JULY 2018

Professor Fuhito Kojima gave a public lecture on "Introduction to Matching Theory and Market Design" at NUS on 24 July 2018. He started with the observation that some markets are not just driven by simple supply and demand of identical goods, but are more heterogenous in nature. The example that guided us through the lecture was that of doctors looking for jobs at hospitals — doctors have preferences at which hospitals they would like to work, and hospitals have preferences which types of doctors they want to employ. These markets are called *matching markets*, and one of the main problems is to device procedures to find matchings that are optimal in some sense. Professor Kojima introduced the so-called *deferred-acceptance algorithm*, which is an iterative procedure to find a matching based on doctors' preferences and the hospitals' decisions to hire or reject candidates. The key difference to the more widely used *immediate-acceptance algorithm* is that the decision of hiring is only tentative — in subsequent rounds, rejected candidates from previous rounds apply to their lower priority hospitals and are then treated the same way as

Fuhito Kojima: Introduction to market design

tentatively accepted candidates. This procedure is repeated until a stable matching is found. Professor Kojima gave many embellishments of this basic idea — what if a couple of doctors would like to work at hospitals in the same city? What if the government imposes restrictions over the total number of doctors in urban regions to ensure rural regions are being supplied with enough doctors? From the many questions asked, it was clear that the audience was captivated by this interesting topic and by the engaging delivery of the talk. A total of 65 people attended the lecture.



Gunther Uhlmann: Inverse problems and Harry Potter's cloak

16 AUGUST 2018

Professor Gunther Uhlmann gave a public lecture on "Inverse Problems and Harry Potter's Cloak" at NUS on 16 August 2018. He opened his presentation explaining the difference between direct and inverse problems in wave propagation. The former problem is concerned how waves, for example light waves, that hit a known object are deflected. The latter problem entails the question whether we can reconstruct an unknown object from how the waves are deflected. As Professor Uhlmann explains, using mathematics, it is indeed possible to reconstruct objects in some important applications, such as computer tomography, where X-rays are sent through tissue at various angles and the diffraction pattern is observed on the other side in order to reconstruct the internal 3D-shape of the tissue. Similar ideas are used in MRI and 3D ultrasound scans.

This raises the question whether we can design an object in such a way that we can control the deflection of the waves in such a way that the object itself *cannot* be detected – in other words becomes *invisible*. Besides fun applications like Harry Potter's invisibility cloak, there are serious applications considered by scientists, such as protective walls — shaped and arranged in an intricate pattern — to guide tsunami waves around a specific area, or neutralizing earthquake waves to protect buildings. As Professor Uhlmann explained to us, the higher the frequencies of the waves, the more difficult it is technically to manufacture the correct pattern that deflects the waves in the right way. High frequencies, such as those of light waves, are beyond current technology.

In a question from the audience, which consisted of 77 attendees, it was pointed out that if one was sitting inside an invisibility cloak and all light was deflected around the person, then that person could not see anything from the outside, and so would be blind! Indeed, replied Professor Uhlmann, but it is possible to design the patterns in such a way that a small fraction of the waves *do* reach the inside. These waves could then be amplified and make the person see.

28 NOVEMBER 2018

On 28 November 2018, Dr Yuval Peres from Microsoft Research gave a public lecture on "Visual Mathematics: From Fair Division and Graph Partitioning to Cellular Automata" at *AI Singapore,* NUS.

Dr Peres started with the problem of understanding large network by means of partitioning them into smaller communities. He visually illustrated the workings of the *evolving-sets algorithm,* which is a local partitioning algorithm, on the geometric random graphs.

He then proceeded to introduce the rotor-router and the Abelian sandpile models, which are deterministic models on the square lattice and lead to intricate visual "art", but it turns out that even simple properties like the shape of the covered area are very difficult to rigorously prove mathematically and require deep tools. Beyond their visual appeal, these models are interesting because they have surprising connections to many areas in mathematics, such as number theory, combinatorics, partial differential equations and others.

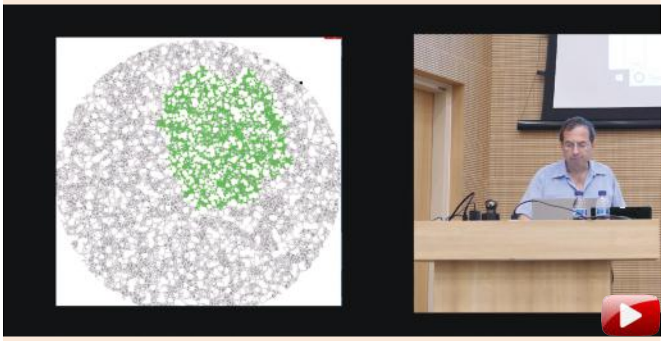
Peres then moved on to discuss another visually appealing area of mathematics, the problem of fair allocation of resources, in particular allocation of territories to centre points. This example was used to illustrate the cover of the *Notices of the American Mathematical Society* of the May 2017 issue. Related to the allocation problem is, surprisingly, the so-called *overhang problem*: Given a number of bricks stacked near the edge of a table, how far can they extend beyond the edge without toppling? These problems are connected through *potential theory,* a field of mathematics concerned with the study of harmonic functions.

Yuval Peres: Visual mathematics from fair division and graph partitioning to cellular automata

Watch videos of public lectures on our Youtube channel

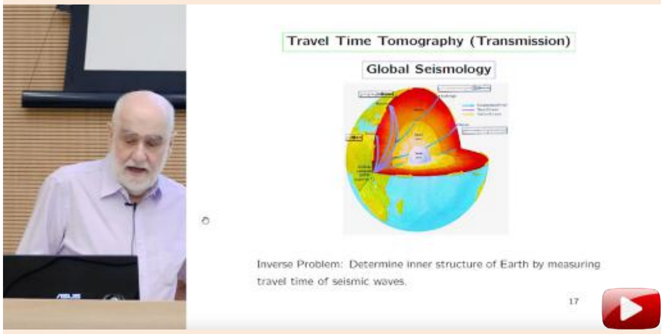
Visual Mathematics: From Fair Division and Graph Partitioning to Cellular Automata

Yuval Peres



Inverse Problems and Harry Potter's Cloak

Gunther Uhlmann



Introduction to Market Design

Fuhito Kojima

Dynamic Models in Economics

4 – 22 JUNE 2018 & 2 JULY – 3 AUGUST 2018

CO-CHAIRS:
Yi-Chun Chen |
National University of Singapore
Yeneng Sun |
National University of Singapore

Through the process of investigating new dynamic models, there is a systematic study of conditional expectation and conditional distribution of correspondences, a study on dynamic games with or without uncertainty, and independent random matching in more general settings with continuous time, and nonlinear matching probabilities and enduring partnerships.

There were 11 mini courses and six student sessions during the 6th Econometric Society Summer School 2018 (15 – 19 June), which was co-organized with the Econometric Society, and the Department of Economics at the National University of Singapore. The Summer School offered a unique platform for 18 graduate students to present their ongoing research projects.

Three workshops, covering aspects of Game Theory (15 – 19 June), Mechanism Design (9 – 13 July) and Matching, Search and Market Design (23 – 27 July) had a total of 69 talks. While the majority of the participants are economic theorists, tutorials (three hours per speaker) were conducted by domain experts Mingyi Huang (optimal control), Rakesh Vohra (operations research), and Fuhito Kojima whose research areas are situated at the boundary of economics, game theory, control theory and operations research.

There were a total of 143 participants which included close to 50 students.

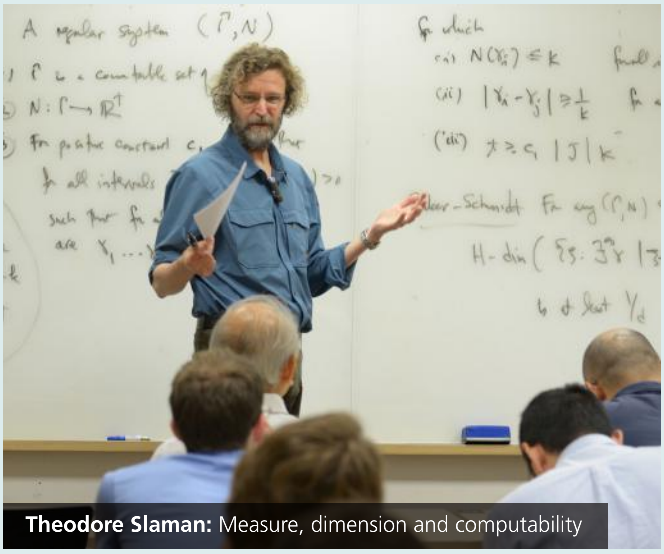
In-Koo Cho: Learning with model uncertainty

Georgy Artemov and Olivier Tercieux: Priorities in market design research

M. Ali Khan, Minyi Huang and Yeneng Sun: Equilibrium in a finite-action setting

Strategic certainty with almost perfect information

Mechanism design in a dynamic environment



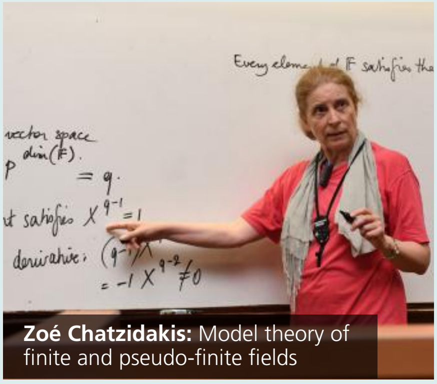
Theodore Slaman: Measure, dimension and computability

IMS Graduate Summer School in Logic

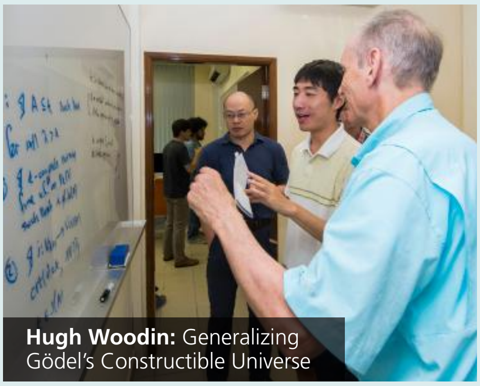
18 JUNE – 6 JULY 2018

Jointly organized with Department of Mathematics, NUS

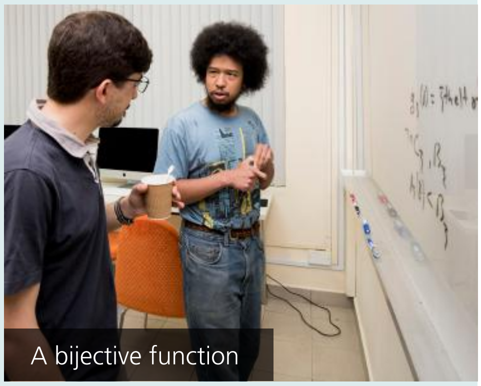
W. Hugh Woodin (Harvard University), Theodore A. Slaman (University of California, Berkeley) and Zoé Chatzidakis (Ecole Normale Supérieure) each gave 12.5 hours of lectures, covering topics on "Gödel's Constructible Universe", "Measure, dimension and computability" and "Model theory of finite and pseudo-finite fields". Five student participants presented their work. There were a total of 45 participants which included 31 students.



Zoé Chatzidakis: Model theory of finite and pseudo-finite fields



Hugh Woodin: Generalizing Gödel's Constructible Universe



A bijective function

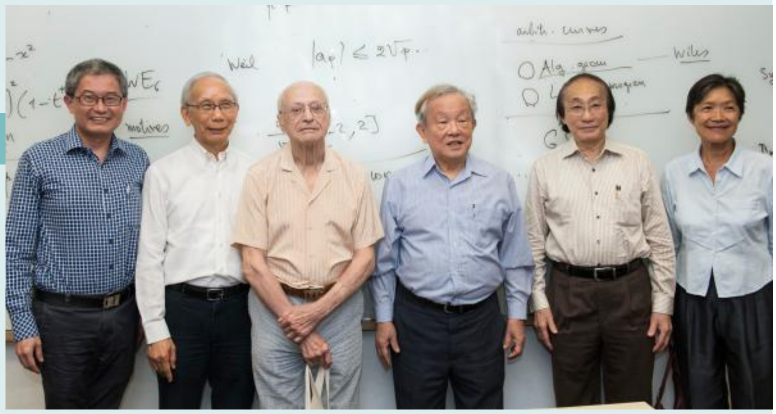
Definable sets in ordered structures

Oppenheim Lecture

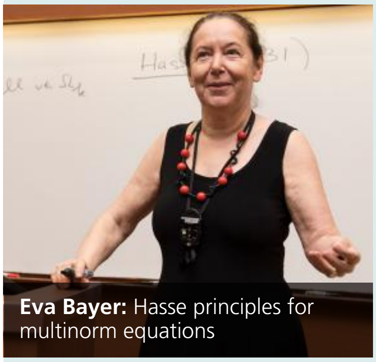
22 JUNE 2018

Jointly organized with Department of Mathematics, NUS

The fourth Oppenheim Lecture, jointly organized with the Department of Mathematics was delivered by Jean-Pierre Serre (Collège de France, France). A total of 140 people attended the lecture.



The fourth Oppenheim lecture: [From left] San Ling, Chi-Tat Chong, Jean-Pierre Serre, Tsu Ann Peng, Louis Chen and Kai Nah Cheng



Eva Bayer: Hasse principles for multinorm equations

Ming-Lun Hsieh and Wee Teck Gan: Densities and stability

2018 Pan Asia number theory conference in Singapore

Pan Asia Number Theory Conference 2018

25 – 29 JUNE 2018

ORGANIZING COMMITTEE:
Wee Teck Gan | National University of Singapore
Lei Zhang | National University of Singapore

The Pan Asia Number Theory Conference, which is held once every year, aims to highlight the vibrancy and diversity of number theoretic research in Asia. It featured 22 talks by young researchers coming from China, South Korea, Japan, Taiwan, Singapore and India, as well as speakers from the US and Europe. Highlights included a talk on the proof of ABC conjecture by Go Yamashita, and Jean-Pierre Serre sharing on Logarithmitic capacity and equidistribution of algebraic integers. There were a total of 44 participants.

Theories and Numerics of Inverse Problems

6 – 17 AUGUST 2018 & 24 – 28 SEPTEMBER 2018

CO-CHAIRS:
Xudong Chen | National University of Singapore
Zuowei Shen | National University of Singapore

Tutorials by Professor Gunther Uhlmann from 6 – 10 August 2018 introduced the mathematical foundations of electrical impedance tomography (EIT). The Calderon's problem considers how objects can be made invisible to electromagnetic and other kinds of waves. Lectures also concentrated on the topic of "transformation optics".

The 9th International Conference on Inverse Problems and Related Topics (ICIP), which ran from 13 – 17 August 2018, had a total of 12 keynote speakers and 12 mini-symposia. The Jiong-Wei Yang Young Researcher Award was presented to two scholars, Lauri Oksanen and Hai Zhang. Topics covered in this conference included inverse boundary value problems, inverse scattering problems, medical imaging, cloaking and invisibility, numerical methods etc. There were more than 90 participants.

A workshop on Qualitative and Quantitative Approaches to Inverse Scattering Problems (24 – 28 September) workshop provided a good opportunity for mathematicians and engineering scholars to exchange ideas. It is currently difficult to compare different reconstruction algorithms. Professor Jean-Charles Bolomey (Paris-Sud University, France) gave a though-provoking talk on "Qualitative versus quantitative approaches to microwave-based imaging techniques for medical applications" in this regard. There were 28 talks which coveredvarious aspects of inverse scattering, including theories, algorithms, modellings, and experiments, and end-user tests. There were over 100 program participants.

Jean-Charles Bolomey: Qualitative versus quantitative approaches to microwave-based imaging techniques

The 9th ICCP in Singapore


Bayesian computation using different statistical models

Bayesian Computation for High-Dimensional Statistical Models

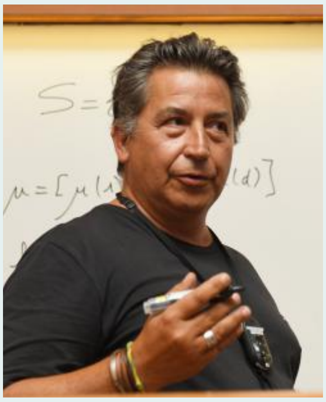
27 AUGUST – 21 SEPTEMBER 2018

ORGANIZING COMMITTEE:
Alexandros Beskos | University College of London
Hock Peng Chan | National University of Singapore
Dan Crisan | Imperial College London
Ajay Jasra | National University of Singapore
Kengo Kamatani | Osaka University
Kody Law | The University of Manchester
David Nott | National University of Singapore
Sumeetpal Singh | University of Cambridge

In recent years there has been an explosion of complex data-sets in areas as diverse as bioinformatics, ecology, epidemiology, finance and population genetics. In a wide variety of these applications, the mathematical models devised to accurately capture the dynamics and interactions of the data generating processes are very high dimensional and the only computationally feasible and accurate way to perform any kind of statistical inference is with Monte Carlo. Key areas of focus in this program included: (1) Markov chain/Sequential Monte Carlo (MCMC & SMC) methodology; (2) Theoretical Developments (TD) and (3) Bayesian uncertainty quantification (UQ) and multilevel Monte Carlo (MLMC).

The opening (27 – 31 August) and closing workshop (19 – 21 September) had a total of 47 talks. Research seminars were planned in between the two workshops, featuring 25 speakers. Highlights of the workshops included a talk by Professor Arnaud Doucet (University of Oxford) on new MCMC methods, Professor Pierre Del Moral (Université de Bordeaux and INRIA) on new SMC TD, Professor Christian Robert (University of Warwick and Université Paris-Dauphine) of new Bayesian techniques. Talks on piecewise deterministic MCMC by Christophe Andrieu (University of Bristol), George Deligiannidis (University of Oxford), Arnaud Doucet and Alexandre Thiery (NUS) were speculated to be the future of this field.

There were more than 90 participants and over 20 students.


Pierre Del Moral: Uniform estimates for particle filters


Alexandre Thiery: The bouncy particle sampler in practice


Approximate Bayesian Computation


Kody Law and Markus Eisenbach: Bayesian static parameter estimation

Upcoming Activities

String and M-Theory: The New Geometry of the 21st Century

10 – 14 DECEMBER 2018

CHAIR:
Meng-Chwan Tan | National University of Singapore

On the Langlands Program: Endoscopy and Beyond

17 DECEMBER 2018 – 18 JANUARY 2019

CO-CHAIRS:
Dihua Jiang | University of Minnesota
Lei Zhang | National University of Singapore

Statistical Methods for Developing Personalized Mobile Health Interventions

4 FEBRUARY – 1MARCH 2019

ORGANIZING COMMITTEE:
Bibhas Chakraborty | National University of Singapore
Ying Kuen Cheung | Columbia University
Eric Laber | NC State University
Jialiang Li | National University of Singapore
Susan A. Murphy | Harvard University
Ambuj Tewari | University of Michigan

Quantitative Finance

18 – 22 MARCH 2019 & 22 JULY – 31 AUGUST 2019

Jointly organized with Risk Management Institute, NUS

CO-CHAIRS:
Ying Chen | National University of Singapore
Min Dai | National University of Singapore
Steven Kou | Boston University

Equidistribution: Arithmetic, Computational and Probabilistic Aspects

29 APRIL – 17 MAY 2019

CHAIR:
Theodore Slaman | University of California, Berkeley

Research in Industrial Projects for Students (RIPS) 2019 – Singapore

17 JUNE – 9 AUGUST 2019

In collaboration with the Institute for Pure and Applied Mathematics (IPAM), IMS is organising an eight-week summer program for talented undergraduate students to work in international student teams on projects proposed by the industry.

This program provides students an opportunity to explore potential careers in mathematics, science and technology, while they also learn specifically about a company based in the region. Students will acquire the skills to do independent research, write a scientific report and publicly present their results. This will be invaluable to them as they continue with an academic or professional career in an industrial setting.

The program is open to students from NUS, students from universities from ASEAN countries and students who are U.S. citizens. Apply at **ims.nus.edu.sg/rips**

For full list of upcoming events, visit our webpage at ims.nus.edu.sg

MOSHE Y. VARDI:

SAPERE AUDE! (DARE TO KNOW!)

Interview of Moshe Y. Vardi by K.P. Choi

Moshe Ya'akov Vardi, a world-renowned computer scientist, made fundamental and deep contributions to the logical theory of databases, reasoning about knowledge, automata-theoretic approach to concurrent program verification, and finite model theory.

Vardi received his BSc in physics and computer science (summa cum laude) from Bar-Ilan University, MSc in computer science from Weizmann Institute of Science and PhD in computer science from Hebrew University of Jerusalem. He then took up a post-doctoral position in Stanford in 1981 after his PhD, and joined IBM Almaden Research Center as a research staff in 1985. Four years later, he managed the Department of Mathematics and Related Computer Science there. In 1993, he joined Rice University in 1993 as Noah Harding Professor in the Department of Computer Science, and was the chair of the department from 1994 to 2002. He is Karen Ostrum George Distinguished Service Professor in Computational Engineering and Director of the Ken Kennedy Institute for Information Technology at Rice University.

Vardi served as editor-in-chief of *Communications of the ACM* from 2008-2017. Under his leadership, *Communications*, the flagship magazine of the Association for Computing Machinery (ACM), is transformed to be the computing field's premier publication with almost 100,000 readerships. In recognition of his outstanding achievement, he was awarded the 2017-ACM Presidential Award for a second time, the first being in 2008.

In addition, Vardi has received numerous awards. Space consideration limits us to highlight only a few. As of 1 January 2019, he is promoted to University Professor, Rice University's highest academic title. He is the recipient of the ACM SIGACT Goedel Prize, ACM SIGMOD Codd Award, IEEE Computer Society Harry H. Goode Award, and EATCS Distinguished Achievements Award. He is a member of the US National Academy of Engineering, the National Academy of Science, and the American Academy of Arts and Sciences.

Vardi has authored/co-authored over 600 papers, and two books *Reasoning about Knowledge and Finite Model Theory and Its Applications.* He has given over 700 talks.

Vardi was in the Institute for Mathematical Sciences (IMS), National University of Singapore from 29 August to 3 September 2016 as an IMS Distinguished Visitor of the program Automata, Logic and Games (22 August – 25

September 2016). On behalf of *Imprints*, K.P. Choi took the opportunity to interview him on 2 September 2016. The following is an edited transcript of the interview in which he talked about his varied experiences in industry and academia and his thought-provoking views on humans, machines and the future of work.

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IMPRINTS I I noticed that you studied physics and computer science in your Bachelor of Science. Did you major in both?

MOSHE Y. VARDI V Computers were very new in 1971, so in high school there was no computer science, just lots of physics. So it was clear to me that I was going to major in physics, even though I had a teacher who tried to convince me to do mathematics. But I wanted to do physics. After I finished high school there was an advertisement in the newspaper for a two-week summer course in computer programming. I knew zero about computers but somehow it intrigued me. I went to my father and asked him for fifty dollars to pay for the course fee. He was happy to sponsor me and I went ahead and studied Fortran programming. I loved it. I found it so exciting that I almost wanted to give up physics. In the end, I did a major in physics and a minor in computer science in a bachelor's program.

Physics was considered the toughest subject and the coursework was very heavy. Typically, students had to do a major and a minor. Physics was considered as an extended major, and students majoring in physics were not required to have a minor. So I took this extended major plus a minor. So my freshman year was a killer year. And when I had to decide what to study in graduate school, I chose computer science as I loved it more.

I Physics still interests you?

V I think physics provides a very good education, because you have to do the mathematics, and it's kind of applied mathematics. I remember our TA who was making fun of mathematicians proving a solution exists and were happy about it. We have to solve differential equations: you know it's not enough to prove it exists. From physics, you kind of get an understanding of the world. You get a basic understanding of science and you learn chemistry and a little engineering.

I In your talk yesterday, you teased the physicists!

Z You can see. There is a joke "you can take the boy out of the physics, you cannot take the physics out of the boy".

I After your PhD, you had very interesting academia and industrial work experience. What led you to it?

V Yes. Well, there are some people who got a plan in life. I don't.

In Israel, it was expected of you to have some international experience. So if you did your PhD in Israel you were expected to do your postdoc overseas. I had a fellowship at Stanford and I went there for two years. I was planning to go back to Israel and to get an academic position. I wasn't quite ready to go back. The opportunity came when I was in IBM research. I was in the second year of my postdoc when I met my wife. So at that point it was not clear what would happen but it's ok to stay another year. After another year at IBM Research, I was not ready to make a decision, so I spent another year at Stanford. People made fun of me saying that I was going to get tenure as a postdoc. We decided to stay together, and as it was very difficult for her to move to Israel and adjust to living there, so I decided to stay in the United States and to find a job in the Bay area. IBM already knew me, so they were happy to hire me. I went there and eventually became a manager four years later. And things were going very well until business started going badly for IBM in the early 90s. What I learnt from this experience, which is a very important lesson, was that industrial research lab requires very successful business. Research doesn't make money and just burns money. So the major challenge for any industrial researcher is how to create value for the company. Mathematics and Computer Science are really relatively cheap disciplines. I need access to computers but that is relatively cheap as compared to experimental science. I don't know about it today, but when I was the chair of a computer science department, a starter package for an assistant professor was a hundred thousand US dollars. But half a million US dollars for a biologist is just enough for him to build a lab so he can start doing things to establish himself and bring more money to the lab later on. But cheap is not free, so in the end we were depending on the IBM business to fund us. When business at IBM was not going well, I decided to leave and ended up in academia. I received two offers in the middle of 1990s: one was from Rice University and another one from Bell Labs. In mid 1990s, Rice was a small university and Bell Labs was a fantastic place and had lots of people. I would have gone to Bell Labs but I just had a bad feeling about the industry and thought I should try academia. I went to Rice. Indeed, six years later, Bell Labs crushed and "blew" up. It was a right decision, and after that, I just loved academia.

I Looking back at your mixed experience in industry and in academia, what are the delights and challenges in different circumstances?

V Like what Friedrich Nietzsche said, "What doesn't kill you makes you stronger". When I was going through difficult times at IBM, I learnt from it. If you do research, you have to think about who is funding you. What are you doing for them? What value are they getting out of you? Many people don't think about these. So my IBM experience was very useful to me. In addition, being a manager at IBM gave me some managerial experience. Usually people in academia get very little experience in management or what it takes to run a department. At IBM, they sent you to management school; they tried to train you as a manager. Academia does a very poor job at this. So coming from industry, I had a bit more management experience; it contributed even to my academic life.

I We want to backtrack a bit and talk about your formative years. Who influenced you most in your growing-up years, and how?

V I grew up in a *kibbutz* which is a uniquely Israeli institution. The word itself literally means a collection—a collection of people. These people agree to live a collective life, very much influenced by socialism. The socialist idea in this community was "to each according to his needs, from each according to his ability". It was difficult to make everybody equal. I had a great experience growing up in this community but since young age I also had an affinity for technical topics. My parents got me a whole set of books called "*The Young Technician*". I would sit down and study how to build stuff, like a radio. I never built anything, but I would try to understand the circuits. I was theoretically minded, and thought as long as I understood the circuits my part was done. Why did I need to build it when I understood the circuits? Those years, I was just myself and read a lot of science – no high school or university. When I finished high school, at about age 16, I wanted to be a scientist. Somehow even though my parents had very little formal education in science, I was very interested in science from young. My father was a rabbi, a scholar in Jewish studies. Though he had genuine interest in science, he did not have a lot of exposure to it. My father would have wanted me to be a rabbinical scholar. I come from a line of rabbis, so I was like the black sheep in the family. He was a little disappointed in the beginning that I did not follow his footsteps, but kind of accepted the stuff I was doing.

I You have done very impactful research in a number of areas, for example multi-agent systems. What led you to this area?

V I would say much of my research is opportunistic. I have a general interest, and an idea comes up. For example, the topic which I described about yesterday, it was something I have been doing for the past five years. Someone gave a talk about an industrial approach. I asked him how he did that, and he said that they didn't have a good method. This was an interesting problem and later I followed it up. As I became more successful with it, I pursued it deeper and deeper and now it is my major activity. It was purely a coincidence. This is true for much of my research. In the early part of my career I worked only in database theory. I went to a conference, and to save money, I shared a room with Pierre Wolper, who was then a PhD student. He shared with me some of his ideas, and that led to my research direction in formal verification. I would say that most of my research was a "random walk on the Markov chain of science".

I It reminds me of your growing-up years, most of the time you picked up a topic at your own pace.

V Yes, my computer science education was only a minor; I had a lot to catch up later on. I took some classes in logic. I had to teach myself complexity theory. Once you become interested in the topics you are very motivated to go deeper.

I It led you to write the book "Reasoning about Knowledge"?

V I have to give credit to my colleagues. It came out in 1995, almost twenty years ago now. This book is now a classic. We're still selling hundreds of volumes per year and get hundreds of citations per year. It is now a basic reference book for multi-agent systems. Joseph Halpern got us started off looking into knowledge as a research topic. He encouraged us to write a book because no one knew anything about this topic; this area of computer science was very new. Writing a book is hard work. Writing a paper requires a shorter time span of work, but greater amount of new ideas, and usually less focused on how well it is written. We had to put in an enormous amount of effort and a lot of hours in writing the book. Every chapter was probably written three or four times, each time each author argued about how they wanted to do it differently and so the book was really an amazing undertaking.

I How long did it take?

V It took us ten years. We started very early. Actually the book motivated us to do a lot of research. We would start writing something, but we had to hold it as we didn't understand certain topics well enough. So questions kept coming up as we were writing the book. I joined IBM in 1985. We worked on the first paper "A Model for Knowledge" in 1985. Then Halpern said, "Let's write a book." In reality, when he wanted to write a book, there wasn't enough material for a book. The material developed as we were working on the book. We published our papers and the book finally came out in 1995. It was a long project.

I **Also because four of you were not at one location?**

V At first we were in one location. At that point, Yoram Moses was a PhD student of Halpern, but then he went to Israel. There was a lot of email communication. Halpern was very fed up with this long process, and decided to write the next book himself. He did and finished it much faster, but it is not as good as the first book, I suspect. For our joint book, four people came together and argued for ten years. That is what I love about computer science; it's very collaborative. People think together and argue what's the best way to do that. The book ended up very polished.

I **This book has a lot of impact in computer science, and I suppose it is very multidisciplinary?**

V The book has almost become a standard reference for anything that has to do with epistemic analysis of multi-agent systems. We get citations from Artificial Intelligence (AI), philosophy, economics and many different areas. Last fall I was in Israel and I had lunch with an economist, and I asked him what his interest was. He said he was thinking about the epistemic aspects of game theory. I said well I had written a book about it and I gave him a copy of the book. He said, "Wow!" This was the kind of thing he was thinking about.

I **Did you anticipate this when you wrote the book?**

V No. It was just fun to do. In 1995 there was a tool that preceded Google Scholar; it was called "CiteSeer". The man behind this idea is Lee Giles, and his idea was that all these papers were online and you could automate citation analysis. Before that, citation analysis was manual, published in the Science Citation Index. They would issue volume after volume, and every year there was a whole shelf of new volumes. If you wanted to find out who was citing whom, you would use a magnifying glass because the font was so tiny. Giles said, "Wow, I could automate the whole thing now." For the first time it became very easy to work with CiteSeer to find out which of your papers were cited. Today we look at Google Scholar every day, but these things were very difficult to do so at that time. For the first few months of CiteSeer I had a lot of fun. I would call my colleagues, "Ok, tell me your top five cited papers." People had no clue. They would try to tell you their top five papers according to how much they liked their papers. But it turns out that this is not correlated whatsoever to the number of citations. You could have a paper you liked very much but it is a deep paper, which is very difficult to read. Few people read it and thus it is not very well cited. Very often it is an easier paper that gets cited more often. It's a bit like TV shows or movies that you have all these studios and you thought by now they would know what makes good movies, which actually they don't know. Some movies are big success, some are just flops. *Star Trek* just celebrated its 50th anniversary. Now they have several TV shows and movies but then they barely decided to produce it. The same thing, when you write a paper it is very difficult to predict which paper would end up being successful. I think you just do what you like and you wait and see what happens. I was talking to my postdoc the other day, and he was trying to get me interested in a topic. I told him the topic was not promising enough. I may be wrong, but I make some judgment call because I cannot follow up everything that interests me. Time is my most scarce resource. I make the choice everyday what I will do and what I will not do. I told him if you were interested in this topic, go ahead and think about it. He argued and said, "This could be a big deal and I wanted to get you involved." I said I might regret it later, but right now, I had to make a bet that I think it's not going to be a big deal.

I **Do you have some kind of algorithm to decide whether you're interested in a topic? Do you see a common thread to all your research?**

V My taste changes somewhat all the time. I used to be more theoretical and now I am interested in more empirical issues, though I do like mathematical elegance. For example in Boolean Satisfiability (SAT), people who build this invest an enormous amount of engineering, which is not my style, even though I think these people do very good work. The ratio of result to effort is low because you have to put in so much effort to make progress. When people visit museums, most people have an intuitive sense of whether they like this piece or not. If you ask them they can invent reasons. But I suspect these reasons are posterior reasons rather than prior reasons. They don't come to the picture with a checklist. When asked why they like the picture, they can say that they like the combination of colors, the perspective, and the like, but their first reaction is likely to be emotional. In fact, I was thinking of giving a talk on how to do impactful research. The talk will not come up with rules, but simply take my five most cited papers and examine their contribution. I could not predict the impact then, but now we can observe some rules on what made them highly cited. I am not one hundred percent convinced you can articulate the rules that would be useful forward, but hindsight is wonderful.

I **Are there any intrinsic differences between human and artificial intelligence, or intelligent machines that people are building?**

V When working on the book "Reasoning about Knowledge", we talked to philosophers in the field of epistemology and to game theorists and economists, who think of games as a model of strategic interaction. Rational interaction seems to be a component of intelligence and logic seems a component of human reasoning. In fact, if you look at AI historically, people thought at first that thinking is a logical activity; good thinking should be logical; so logic was very dominant in

AI at the start. In the early 80s there was the Japanese Fifth Generation Project. The plan was to build this big expert system running on some kind of a logical machine. Everybody felt that the Japanese would take over computing. It did not quite happen. After the early days of AI, people got into pondering, starting in the mid-70s how common-sense reasoning can be done. Common-sense reasoning doesn't quite match logic reasoning. For example, logical reasoning is monotonic, if you give me more assumptions, I can infer more. In real life that is not quite the case. For example, if I tell you Tweetie is a bird, then you would assume that Tweetie can fly. Now I tell you Tweetie is a penguin; it's then a different story. I will have to withdraw my conclusion. In classical logic, you will never withdraw conclusion if you got more assumptions. People made an enormous effort to try and modify their logic, so it would be closer to capture human reasoning. But that effort at the end was a failure, as whatever logic you develop, you find that it does not describe how actually humans reason. And there is similar big failure (I would argue, not sure everyone agrees with me); it is in language. People thought in language there must be some kind of grammar. It was the idea of Chomsky's Universal Grammar. Linguists worked on developing a grammar to describe formally sentences in English. They worked for years on different formalisms, but nothing captures English fully. And so it is an object that does not quite conform to formulation. In the `80s another school of thought tried to base AI on probability and statistics. John McCarthy is considered to be the father of the logical approach, and Judea Pearl the father of the probabilistic approach. Today the achievements of machine learning are really based more on probability and statistics than on logic. And so people try to bring probabilistic techniques into logic. Logic brings you structure but probabilistic approaches do a better job in describing human reasoning. You can see a similar thing in game theory. You have this beautiful formulation of Nash equilibrium, but it does not quite describe human behavior. There are more refined notions of equilibrium, until it is not clear what you should do in strategic situations. Game Theory does not quite explain what is the right way to behave when you are in a game-theoretical situation. Psychologists debate whether there is general intelligence. Do we have general intelligence, or are we just portfolios of our abilities? Perhaps the brain consists of a portfolio of abilities that work very well together. There is a theory of multiple intelligences that says that you have a set of abilities: your quantitative ability, verbal ability, musical ability, and the like. There are people who are just very talented and have a fantastic sense of balance and this is also some kind of intelligence. Howard Gardner, a psychologist at Harvard, said there is no single intelligence but multiple intelligences. Now people turn to neuroscience to study intelligence after trying psychological approaches. We are still struggling to understand human intelligence.

Let us see if we can mechanize human tasks. We can read print, which is optical character recognition. But handwriting recognition is a more difficult task as there is a lot more variability. We used to have people at the post office who would read the addresses but now everything is mechanized. Most of the packages and letters are sorted automatically. In public spaces in Singapore there are active cameras everywhere and what do they do? They do facial recognition and this is human ability. We are very good in recognizing faces, in particular, among different ethnic groups, especially faces you are familiar with. We are building a portfolio of tasks. And so we are taking more tasks which humans can do, and we are solving them. There is a debate if this is enough. Is this the Holy Grail of intelligence? Do we need Artificial General Intelligence? I am not an expert but I am skeptical. I have not seen yet that we understand the concept well enough to build something like this. Human beings are very good in solving problems. People think hard and long and make amazing progress. The progress I see in AI is solving specific tasks. Sometimes they use general popular techniques to solve certain class of problems, for example, machine vision. So AI is engineered. Our understanding of human intelligence is evolving; we are still struggling to understand it. Sometimes people think we can learn from human intelligence on how to solve things in AI. But in AI, when we solve problems, we don't solve it by understanding how the brain does it. We sit down as problem solvers and we follow the mathematical and engineering techniques that we know, and come up with a solution. All systems of AI are actually engineered solutions.

I **Are there knowledge/insights/discoveries that are not a direct result of computation? What about creativity? Intuition? Hunches? Lateral think?**

V This is part of the debate. There is a limit to what intelligent machines can do. If there are rules, we can write expert systems, we can build a machine. What we discovered from machine learning is that you don't need to have rules if we can just observe enough data. For example, when I look at faces, I would say these are the same, these two are different. You ask what rules you are using. I say, "I don't know". These two look the same to me; these two look different to me.

This suggests a kind of neural system in the brain, and this system can be trained with enough data to do certain classification tasks. This is one idea we learn from the brain structure, the 1943 McCulloch & Pitts Neuron Model. And for many years people think this is how the brain works and we should be able to use it to build artificial intelligence systems. For seventy years, people keep trying it and it didn't work. Something happened five or six years ago. Machines are getting faster, and there were more data to learn from. You put everything together and suddenly deep learning exploded. Deep learning is really a neural network with seventy years of history. The field of machine learning has been completely transformed by deep learning. We took the basic idea of neuronal net from the brain, and we engineered it. Intuition is where we seem to have trained the neural network in the brain to recognize certain things and make certain decisions, but we have difficulties verbalizing the rules. You ask me what research problem I like; I say, "I only know it when I see it." Walking seems like a basic task: people naturally put one foot forward, but it is actually not easy. We try to build robots to walk on different terrain on two legs. There was a case where people built humanoids for rescue emergency; you saw them went to the door, opened the door and fell backwards. It was funny seeing the robots falling on top of one another. It takes kids about two years. Toddlers start crawling until they learn how to walk. Machine learning enables us to do the same thing. Let's learn how to do things. Intuition, to me, is the ability to accomplish cognitive tasks without a deeper and clearer understanding of how we do these tasks. AlphaGo, a game developed in February 2016, uses deep-learning techniques to develop intuition for the gamer. Now we can say machines have intuition. I would not say that machines have intuition exactly the same way that people have. That would be a bit of a stretch, because we don't fully understand human intuition. Machines are not very good in dealing with problems in novel situations. Humans are able to deal with an entirely new problem that they have never encountered before. In movies, someone was deserted on an island and they came up with creative solutions to situations they had never faced before. We don't know how they can do that.

Creativity is a very broad word that people use in different things. For example, people use it when they mechanize musical improvisation. When you improvise, there are rules about what you should be using. If you just do, it is just noise. For it to be accepted as music, there are some constraints. There are rules, but you can randomize within their constraints. This is analogous to having a logical formula, but you want a random solution. So you must randomize within constraints. Creative problem solving is different: it is coming up with new ideas, the Eureka moment. Some even say this is what God is telling them to do. We are far from developing a new computer or system which would be able to deal with all kinds of new and unexpected situations. This is what humans can do and machines cannot, to deal with unexpected situations in a creative way. I am a materialist. I don't believe in spirits or gods. We are just biological machines. One day we will figure out how humans can be creative.

I Can we really build biological machines?

V Who says we cannot build biological machines? Must our machines be built from silicon? The question is, are we smart enough to do so? I think it is just a matter of time. There is nothing inherent that we can do which a machine cannot, because I think we are all machines. The brain has an enormous number of connections. The chip is a two-dimensional object, now we are trying to build three-dimensional chips. The brain it is amazing because everything is connected to everything; we currently cannot build something like this today.

I **AlphaGo has beaten an 18-time world champion Lee Sedol. Do you think any human being will have a chance to beat AlphaGo? Does it take the fun out of chess playing now that the machine is becoming a better and better player?**

V Five years ago, people said it would take another hundred years. Now it does not seem that difficult anymore. In every tournament the computer has an advantage in the following way. If two chess players play against each other, they prepare months beforehand. The games of top-notch players are published. So before the actual game, each player would spend months studying what the style of the other player is, what his tricks are, and what kinds of attack he will make. When AlphaGo played against Lee Sedol, it knew how Lee Sedol played, because it studied all his games. Lee Sedol did not know how AlphaGo was going to play. Nevertheless, AlphaGo is a system. It plays in a particular way. Perhaps Lee Sedol can win a re-match?

In 1997, Deep Blue, developed by IBM, defeated Kasparov in Chess. Kasparov wanted a re-match, but IBM did not want a re-match. The reason was there was nothing for them to gain but a lot to lose. IBM was afraid that Kasparov could see how Deep Blue played; he could adapt and perhaps win in the re-match. Kasparov was so confident he would win in the first match that he did not put in the agreement a clause before the first match that he could request a rematch. Even if Kasparov won in the next year, we know that computers are getting faster, and so they can search deeper. It is now understood that machines will play better Chess than humans, because we know the right technique to mechanize this game. I have a brother who is five years younger than I. At (my) age fourteen he beat me in chess. I told him that chess was just a game for kids. "That's why you are better and that's why I am not going to play it anymore", I said. I convinced him that the reason he won is because chess is a game for kids. This is my way of dealing with the loss of face. We used to think that chess games had some to do with human intelligence; and now it's clear that machines will play better because in chess games there are very clear rules and ways to compute and to evaluate the moves. Today we should choose games that are harder to code and build machines to play these games.

No, it does not take the fun out of chess playing because what we're doing today is what I did with my brother. We don't compete against the machine anymore. It's a little bit like the Olympics. Someone who takes drugs cannot participate. Now there is an issue with people who have prostheses. Can they compete because their prostheses may make them better than the normal natural body? We would be "humanistic" in a negative sense: we would draw boundaries that only humans can participate in this activity.

I From the previous talks you gave, you seem to sound a little bleak about automation and robots. Why?

V From my background, both my parents went through the holocaust. Part of our history shows that bad things can happen. We call someone who is very optimistic very "pollyannaish", which comes from a novel by Eleanor H. Porter called "Pollyanna" who is very optimistic. I worry about the things that could happen, because we have this amazing technology today that is very ahead of us. It is doing more things than we thought could be done. So the question is what the impact will be. Nobody really knows. I read an enormous amount of literature coming out from the AI side and from labor economics. Everyone makes predictions but nobody knows what is really going to happen. So my view is that it could have an adverse impact on labor.

We know what happened in the past forty years even though we don't know why it happened. Economy is a very complex system and we are very bad in understanding complex systems. If you have a linear system then you know I move it, x, and I know the effect is going to be cx. A complex system is nonlinear, and we are very bad in predicting how nonlinear systems are going to behave. You have a lot of feedback relationships but we don't know what will happen. I become cynical about so many predictions that come up. Comparing different countries, United States is an unmanaged capitalistic economy as opposed to Singapore, which is a managed capitalistic economy. This government doesn't hesitate to intervene in a big way if it thinks that it is the right thing to do. In the United States it became part of their conventional wisdom to stay out of this, and so there is no industrial policy. Other countries, even Germany is still fine and employment is still very important. They negotiate frequently between the industries, government and the union what they should do. In US it doesn't happen; people thought that it would lead to the best outcome.

We have become a very technology-heavy economy, and very educated professional people do very well in this economy. So in my current circle people and things are going well. We had our ups and downs, economic crisis, but generally speaking, my son works for a tech company and he is getting paid very well. I am in a popular subject, computer science, considered an important topic in the university. From our point of view, things are going very well. But we have not paid attention to the working-class people, who have not benefited from the technology. Yes, they can buy an iPhone cheaply, but millions of them have lost their jobs and even if they find other jobs they may not have a good quality of jobs. We need jobs today that machines cannot do. Lots of jobs involve human contact, which machines cannot do, such as taking care of elderly people. Now, who is better in this kind of jobs – men or women? It turns out that new jobs are more feminine. Women are known to be better at jobs that require more human contact. This is nature or culture, it doesn't matter, but the reality today is that men do not want to do these jobs as they find them too feminine. For example, take care of someone who cannot wash himself. One of the really shocking statistics is what economists call labor-force participation rate, which is how many people are in the job market. This rate has been dropping for men for the past fifty years from ninety per cent to seventy per cent. Economists ask why these men are not working and not even in the labor force looking for a job. There was a lot of discussion on globalization. Globalization did not kill the manufacturing industry but made the global environment more competitive. Automation is the way to become more competitive. Even without globalization, this would happen. When someone automates the production process, other competitors would have to automate too. Automation would have happened anyway. Globalization makes it a very competitive environment that we need to survive. So globalization accelerates automation and causes a decline of labor unions. If the union gives you a hard time and if you are an industrialist, you will say, "I am having a hard time, I'm just going to move to China and so you better shut up."

Another factor is what we called the regulatory environment concerning corporate mergers. As part of the new liberal philosophy, more corporate mergers have been allowed. Corporations have more power than they used to have, because they are less regulated. So again it's the balance of power between the corporations and the unions. Finally, there is financialization, which enables unfettered growth of financial markets. This is often enabled by technology. Now you can manage money on a scale you could not have managed before. In 2008 we had the subprime crisis. In mortgages, you take a loan from the bank for thirty years. The banks would undertake a serious underwriting to make sure the person could repay the loan in the next thirty years. Someone with a clever idea said thirty years is a long time, why don't we package all these mortgages to create securities and sell them? This is called securitization; and then the bank does not have to keep the mortgage. The bank can take a whole bunch of loans, package them and sell the papers to investors which are pension funds in Norway. Now the banks love not holding the mortgage for thirty years. Technology enables behavior; in this case, it enables securitization with lower underwriting standards. You see, this is a very complex system. On one hand we see computers make it easier to do certain things, but computers also lower financial friction and the systems become less stable.

Many things happened in the past forty years, and automation is one of these factors. Well, I get alarmed in seeing a perfect storm coming to the drivers. Corporations like Ford say, "In 2020, we will produce self-driving cars because we don't expect human being to know how to drive." So the professional drivers are going to be hit by this tsunami. Do you know of any friend who drives for a living? Most likely not, your friends are most likely people who use technology. They like technology as they are not going to be replaced by technology in the next twenty five years. I talked about this topic four years ago, but then I stopped talking about it because people argued that technology is a good thing, and there is nothing to worry about. This has changed recently.

The thing that changes how people look at it can be seen in political events, like Brexit and Trump. Why did people vote for Trump? After looking at those who voted for him, people suddenly realized that the white working class in the United States had been neglected for many years. Why are they neglected? These people are typically Republicans. So the Democrats have to worry about their own constituencies which consist of more minorities. The Republicans have actually a less equitable ideology, and somehow these people thought that the Republicans are for them. Trump realized there was an issue here. The Republicans did not take care of the white working class people. Suddenly we see a lot of data coming out, how bad these people are going through. Technology plays a major factor in that.

I **Would you suggest putting a brake on automation?**

V I don't think you can stop technology. We have to manage the consequences of automation. Suppose NSF grants two hundred million dollars a year for AI research. Some people want ten per cent to go to Beneficial AI, research that clearly ensures AI is beneficial to all people. Facial recognition is a technology that is beneficial to some people; but also causes many other problems. Beneficial AI should be technology that is focused on benefiting society. This focus on Beneficial AI came about just in the last couple of years because people have many concerns about AI, from completely exaggerated evil taking over the world, terminator-type of stuff, to computer systems making more and more decisions in our lives.

For example, you met a banker for taking a loan. The banker may have known your father and that is how the decision is being made. But now we called lots of data, and we use machine learning. An algorithm will decide if you are high at default risk or not. What we discovered to be a problem is: what does it learn? Maybe it learns that people from a certain neighborhood are higher at default risk; it could also come to the conclusion that black people are terrorists. So you end up with having a black person who has the income to take up a loan, but will not get the loan because the system learned that black people are terrorists.

There is a new issue known as algorithmic fairness. If these algorithms are running our lives, we want to know they should behave in an ethical way! Algorithmic fairness is a very difficult issue. It is not easy to define clearly what it is. How do I even identify what specific features put you at a higher default risk? We tell people that if you are deciding to give someone a loan, you can look at their credit history. These are the only factors we allow you to consider. Other factors may be correlated with default risk but we do not allow you to take them into consideration. This is a research area that just came out in the past couple of years. So it's not just AI. As technology plays a bigger and bigger role in our lives, and we work with machines all the time, so you see heightened sensitivity to what is the power of these machines. Some of these are just paranoia; and some are justified. People are talking about ethics and computer science, which is not a common topic. For example, one of the courses in computer science curriculum is on the ethics of computing, and we have a very hard time to find someone to teach this course. Computer scientists say the philosophers should teach this course, but the philosophers will not know enough computer science. So it has always been a challenge. Last year I was invited to give a talk on ethics in an AI conference. There is a new sensitivity to ethical issues and societal impacts of technology.

"WE USED TO OCCUPY A SPECIAL PLACE IN OUR OWN MENTAL FRAME, NOW PEOPLE FEAR THAT WE MAY LOSE THAT QUALITY. WE MAY BECOME THE SECOND SMARTEST CLASS WHEN MACHINES ARE GOING TO OVERTAKE US."

I **Will robots rule us? Now that the world is so interconnected; some bad guys can write programs to rule us by robots. What do you think?**

V There is a book by Nick Bostrom on Superintelligence; and people like Stephen Hawking raised concern that AI is going to take over. To me, we are so far from machine intelligence that we won't seem to have intelligent conversations about it. I am not worried about AI taking over. I am more worried about the internet of things, devices. For example, you can buy a thermostat that is connected to the internet. I will not install such a device in my house because I do not think it is secure. My biggest worry is security, for example, connected vehicle security. I am not worried about some scientific fiction. I am worried about issues here and now. I am worried about graduates losing their jobs in five years, not in a hundred years. I am worried about the fact that we have not been able to have robust systems that are secure from intrusion. Anyone can be hacked. The Democratic National Committee was hacked. The NSA was hacked. We heard that the US Election could be hacked. These are the issues I worry about.

I Do you see US and China sending machines as negotiators, each bringing his set of constraints?

V A colleague of mine developed a program to play the game called Diplomacy. You negotiate treaties, you get certain rights. But you can break treaties as well. She said the program is superior to humans. I asked why? The program is not emotional. As humans, if you scold me, I become emotional and I scold you back even if it is not good for me to do so. Computers are unemotional. Breaking the treaty is a problem. The program just moves on to decide what the next best move is. It doesn't care about the past. It would be interesting if we can build machines to negotiate. I heard people ask, "Should we choose an algorithm to become the President of the United States?"

There has been an ongoing process for the past five hundred years. The Bible has the story on creation. Human beings are described as somewhere between God and the rest of the nature. They describe humans as created in God's image. We also behave as if we are separated from nature. It's very different to kill a chimpanzee, an intelligent animal, than a human being. We think of ourselves as special, above nature. We look at the past five hundred years, first of all Earth is not the center of the universe. It is actually an insignificant planet in an insignificant galaxy. Darwin said we were part of nature and came out from the animals. People have a hard time accepting that. The more we understand these animals it becomes more and more difficultto say how different we are. We say we use tools and no other animals use tools. We have language, which may be more sophisticated but animals have a way of communicating as well. We don't find any hard line, but still at the very least we are the smartest. We used to occupy a special place in our own mental frame, now people fear that we may lose that quality. We may become the second smartest class when machines are going to overtake us.

I Or we can be a dumb animal in control of a very intelligent machine?

V But people would argue if the intelligent machine is so intelligent it would break out of its control. We used to think that we are very special; but we are losing this specialness. We like to be special. We don't like to lose our specialness.

I You have done a lot of high impact work, and are very prolific. You also have rendered tremendous service to the university and to your profession. How do you manage to do so much, and so successfully? What is your attitude towards work?

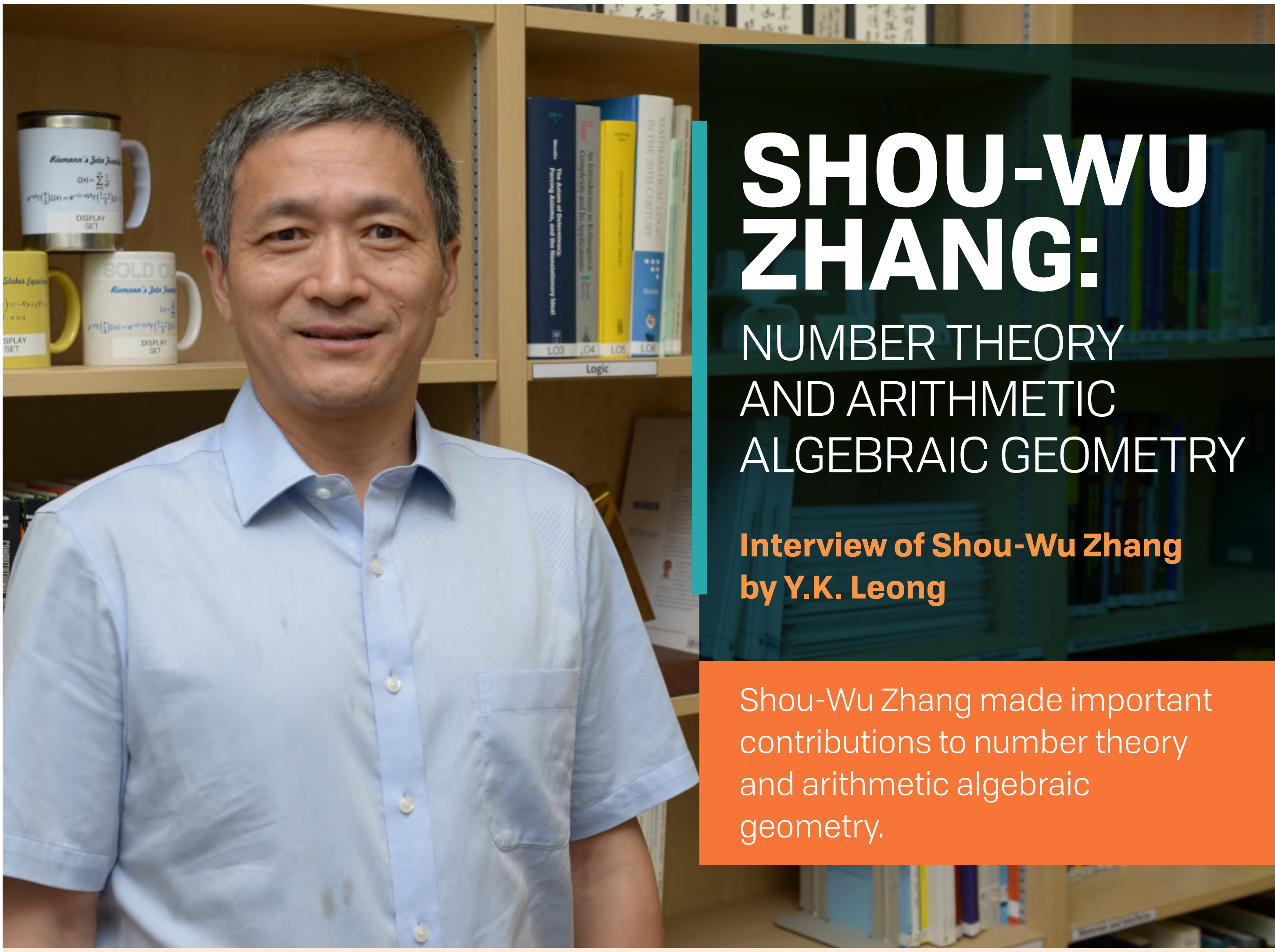
V I've been very busy. In academia you have people who just want to write papers and be left alone. Part of the period I spent in industry is a bit like an "activist". I was a manager in industry. I spent five years in the military in the Israel defense forces. I participated in two wars. There is a kind of the "doing" side of me. I still very much like the scientific world. I realize the scientific community exists because people are willing to make significant contributions. Generally, most people like to contribute to society.

I But you are able to perform these different duties so successfully.

V Some people say I am a bit abrupt. I am famous for writing very short emails. I try to be as efficient as much I can. I think of what I want to accomplish, and this comes from a part of my industrial and military background. So you have to take a high level goal and break it down into smaller steps to get things done. I've actually seen people having this goal but they don't know how to put it down into concrete steps.

I So maybe in closing I want to ask you the question that I learned from your talk. You quoted Nigel Cameron's question "Will a world without work be heaven or hell?" I am very interested in your take in this question.

V It's up to us. Some people believed in historical determinism, and Marx was famous for it. I don't believe history is the result of a preconceived direction. History is the result of many actions; and sometimes individuals can play a key role too. I can think of your first Prime Minister, Mr Lee Kuan Yew. In Israel, we had an understanding that your first Prime Minister was very influential in setting the history, but at the end of the day what makes Singapore successful is not just Lee Kuan Yew but all the people in Singapore work together to get things done. At the end of the day, it takes actions by many, many people but individuals can play a key influence in charting the direction of a group of people. It is very important to get good leadership. One of the reasons I talk about this topic is because it needs to be discussed. I don't have a solution for this; we need to discuss it collectively. We have been facing in the past with these kinds of sudden changes that force us to rethink completely. For example, during the US depression in the 1930s, the society realized that we are not comfortable to see people suffer. We believe people should be responsible for their actions and the consequences. But we are not willing to see a person starve and say it is his fault and we don't care. We care. Human beings have compassion. Until that point, US society was a very individualistic, yet people said they were not that willing to be that individualistic. So it was a huge change for the United States to get a social security system, and the country made major changes when it became clear that something had to be done. The British in the nineteenth century changed the laws forbidding children to work until they are of certain age; also people could not work more than a certain number of hours even if they said yes. We are going to face a huge challenge to how we structure our work life, and if it's heaven or hell, it's up to us.



SHOU-WU ZHANG: NUMBER THEORY AND ARITHMETIC ALGEBRAIC GEOMETRY

Interview of Shou-Wu Zhang by Y.K. Leong

Shou-Wu Zhang made important contributions to number theory and arithmetic algebraic geometry.

Zhang obtained his BSc from Zhongshan (Sun Yat-Sen) University, Guangzhou, MSc from the Chinese Academy of Sciences and PhD from Columbia University, US. After his PhD, he was at the Institute for Advanced Study for a year and Princeton University for four years. He moved to Columbia University and was there for 15 years before moving back to Princeton University where he is now.

In the 1970s, Suren Arakelov developed a geometric theory for the study of Diophantine equations. In the 1990s, Zhang contributed to Arakelov theory with a theory of positive line bundles, which was used by him and Emmanuel Ullmo to prove the Bogomolov conjecture [Fedor Bogomolov]. He further used his theory to generalize the Gross–Zagier theorem [Benedict Gross, Don Zagier] from elliptic curves over the rationals to modular abelian varieties of *GL*(2) type over totally real fields. He then used this result to prove the rank one Birch and Swinnerton-Dyer conjecture [Bryan Birch, Peter Swinnerton-Dyer] for modular abelian varieties of *GL*(2) type over totally real fields. His recent results are given in his book The *Gross-Zagier* formula on *Shimura Curves*, jointly written with his students Xinyi Yuan and Wei Zhang. He has also made important advances to the theory of arithmetic dynamics.

He was awarded the Morningside Gold Medal of Mathematics and fellowships of the Alfred P. Sloan Foundation, Simon Guggenheim Foundation, Clay Mathematics Institute, American Academy of Arts and American Mathematical Society.

He has served on the editorial boards of *Journal of Number Theory, Journal of American Mathematical Society, Journal of Algebraic Geometry,* and *International Journal of Number Theory,* and currently of *Acta Mathematica Sinica, Journal of Differential Geometry, Science in China, Pure and Applied Mathematics Quarterly, Algebra and Number Theory, National Science Review, Research in Number Theory* and *Forum of Mathematics.*

Zhang has given invited lectures throughout the world. He was an invited speaker at the International Congress of Mathematicians in Berlin in 1998. Each year, he goes to China to give lectures and mentor students at all levels. He is also actively involved in committees and projects of the Chinese Academy of Sciences and Tsinghua University. In 2015, he was invited to a workshop at Oxford University to discuss the work of Shinichi Mochizuki on a new theory (Inter-Universal Teichmüller Theory) related to the long-standing *abc* conjecture in number theory.

Zhang was at the Institute for Mathematical Sciences, National University of Singapore (NUS) for the program Higher Dimensional Algebraic Geometry, Holomorphic Dynamics and Their Interactions (3 – 28 January 2017). Under the Distinguished Lecture Series, he gave two lectures: (a) *Torsion points and preperiodic points: the Manin-Mumford conjecture and its dynamical analogue*, (b) *CM points and derivatives of L-functions: the Andre-Oort conjecture and Colmez conjecture*. He also gave a colloquium lecture at the Department of Mathematics, NUS on *Rational points on curves: the ABC conjecture and BSD conjecture*. On behalf of *Imprints*, Y.K. Leong interviewed him on 16 January 2017. The following is an edited and vetted transcript of the interview in which he tells us how from a somewhat faltering start in a provincial university in China, he seamlessly rose to the peak of his career in the United States. He also talks passionately about his life-long fascination with arithmetic algebraic geometry and his strong views on mathematical research in general and in China, in particular.

IMPRINTS I It seems that you were initially admitted into Sun Yat-Sen (Zhongshan) University to study chemistry. When and why did you switch to do mathematics instead?

SHOU-WU ZHANG Z I wished to do mathematics, maybe after two or three weeks [in the chemistry department]. The reason was that I was admitted into Zhongshan University chemistry department by mistake. It was not really a mistake made by them but myself because I failed [to do well] in my math entrance examination. Not completely failed. The total is 100 points and they have 20 bonus points and the bonus points can only be counted if your total point is 80 or above. I got 79 which is pretty bad for any top math department in China. On the other hand, my chemistry grade was very high, so I was admitted to the chemistry department. But I never really learned a lot of chemistry. When I was admitted into Zhongshan University I did some tricks to move to the math department. I pretended to be color blind, and thought that the doctor could not figure out what was going on. That was the way I got transferred to the math department.

I I believe it's quite hard to switch departments in Zhongshan University.

Z Yes. There is no regular way. So I told them I was color blind. In the chemistry department, if you are color blind you cannot do anything. So they moved me from chemistry to math department. They gave me a book to read; there were colors. "No, I could not see anything." Page one moved, maybe, twenty pages later. "No, I could not see anything." Then the nurse looked at me and said, "You are not color blind. You are really blind. There's no color there." *[Laughs]* Then I told her that I really wanted to transfer to the math department. She helped me and awarded me a certificate to say that I was color blind. I went to the school. They told me, "You cannot study chemistry. What do you want to study?" I said I wanted to study mathematics. They talked to the chairman of the mathematics department. They looked at what I had done before. He looked at my entrance exam results and agreed that I should have done better in mathematics. Then I was admitted to the math department.

I You had six years of university education (B.Sc. and M.Sc.) in China before going to Columbia University. How much influence did your university teachers have on your choice of graduate study?

Z When I was in college I transferred to the math department. Then I immediately had a good relationship with my linear algebra teacher. He let me read a book at graduate level and to report it in a seminar. In my second year, he had a visitor from the United States (Professor George Szeto from Bradley University in Illinois). He told me I should study algebraic geometry. I finished college in 3 years. Usually it takes 4 years but my teachers at Zhongshan University were kind to let me finish early. So I went to the Chinese Academy of Sciences to study with Professor Wang Yuan. He is in analytic number theory and has a very open mind. At that time [Gerd] Faltings' proof of the Mordell Conjecture (that big theorem) attracted me and this attraction lasted almost 30 years. It is considered to be one of the best theorems in the history of mathematics. At the Chinese Academy of Sciences I had already made up my mind to study arithmetic algebraic geometry. Professor Wang Yuan gave me his full support. I was very lucky that the professors at both Zhongshan University and the Chinese Academy of Sciences gave me full support in whatever I was doing.

I What was the topic of your masters?

Z It's in arithmetic algebraic geometry. Nobody there was doing arithmetic algebraic geometry. I remember I gave a talk for the oral masters exam at a blackboard, and Professor Wang Yuan said, "Well, we don't know what you are talking about. You are working very hard and everybody knows it. So I give you this degree for free but in future you must do something for real." *[Laughs]*

I Only numbers are real.

Z That's right. *[Laughs]* I was extremely lucky to be at the Chinese Academy of Sciences and Zhongshan University at that time. The Academy has more than 100 institutes. The Institute of Mathematics is one of them, has a graduate school and is located in Beijing. Many famous Chinese mathematicians came from there. Hua Luogeng [(1910-1985)] was a director of the mathematics institute in Beijing. At that time it was extremely hard to get in. In my time, there were about 150 students from all over the country [China] to compete for 12 spots and every university sent their best students to the Chinese Academy of Sciences. It's a very hard exam. I remember in my time the problems were from Pólya's book *"Problems and theorems in analysis"* [in two volumes I, II by George Pólya (1885-1987) and Gábor Szegő (1895-1985)].

I **If I understand it correctly, you obtained your PhD from Columbia University in 1991 but your PhD advisor Lucien Szpiro was head of research at CNRS in France until 1991. How was that possible?**

Z That was a very special situation because when I was in Columbia I was trying to get a professor to be my advisor. The first choice, of course, went to Princeton to try to study with Faltings. He rejected my idea; probably he thought I was not smart enough to study with him. Then I studied with Hevé Jacquet and Dorian Goldfeld. Finally I met one visiting professor who gave interesting lectures, that is, Lucien Szpiro. I knew that Faltings got his main idea from Szpiro's seminar in Paris. So I decided to study with him [Szpiro]. When he left I wrote a mail, not email, to him that I wanted to study with him. He mailed back one page with half a page description of what the problem was. So that was it. "This is the problem that you have to solve … blah, blah, blah." Half a page. Of course, I could not solve the problem. On the other hand, I was reading mathematical journals to see whatever I could solve. I wrote two papers and sent them to him. He felt very excited. In '89 I visited IHÉS for half-a-year and stayed there so that I could see him. During that time I got the main idea for my PhD thesis. The main idea was to reduce my thesis problem to a problem in differential geometry. So I wrote to Professor [Shing-Tung] Yau whom I have never met before. Yau told me that my problem was already solved by Gang Tian in his PhD thesis. So I used Tian's result to solve my thesis problem and that was enough for my PhD thesis.

I **They are quite flexible. Szpiro was not a faculty member of Columbia University and they allowed him to be your formal advisor?**

Z Yes, right. Szpiro and Columbia University math department have a very special relationship. He just comes whenever he wants to come. He has many friends in the department. It's a pretty flexible arrangement.

I **He moved to the US later, didn't he?**

Z He moved to the US in 2000 after I moved back to Columbia as a professor. He moved to CUNY [City University of New York] as a distinguished professor. Right now he is in CUNY but at that time he was mostly in Paris.

I **Was the problem he gave you anything to do with his [Szpiro] conjecture?**

Z It's not that conjecture but is, in some sense, in the same line. In fact, I proposed to myself six steps to prove the Szpiro conjecture. Now we know it's equivalent to the *abc* conjecture. I finally solved three steps. In 1996 my fellow student E. Ullmo (also a student of Szpiro) realized that what I had done were good enough for the Bogomolov conjecture.

I **His [Szpiro] conjecture is not solved?**

Z No. His conjecture is equivalent to the *abc* conjecture. It's just another way of looking at it [*abc* conjecture]. There is no idea up to now for how to solve it [*abc* conjecture].

I **After your PhD you were at Princeton from 1991-1996. Then you moved back to your alma mater at Columbia University to which you were attached for 17 years before recently moving again to Princeton. What attracted you to Columbia University for such a long period of time and what made you leave again?**

Z When I was still an assistant professor at Princeton, I was approached by Columbia. They offered me a tenure position. For family reasons, I wanted to settle down and go back to Columbia. Columbia, to me, is very much like home. I know so many teachers and friends [there], so I feel very comfortable. Also, they helped me a lot to settle down. The number theory group, after I moved there, hired a few important faculty. I felt very happy to live there for 14 years, until 2010. Then Princeton lost several faculty. [Andrew] Wiles left for Oxford, [Andrei] Okounkov moved to Columbia and [Rahul] Pandharipande moved to Zurich. Then I was approached by Princeton. "Are you willing to move back to Princeton to help out?" I thought maybe it was a good time to move back. It is a difficult decision since Columbia is a great place for working. It proved to be very successful for my research, to advise students. Both schools are great but have some difference. The difference is that Columbia has a smaller math department and bigger engineering school, each faculty has more working load in both undergraduate teaching and graduate supervising. Also it was a bit stressful for me to drive one hour every day between Manhattan and my home in New Jersey. Princeton has a bigger math department and smaller engineering school, so the faculty has less work load in both undergraduate teaching and graduate supervising. Also I can walk or bike between my office and my home. I moved to Princeton in 2011 but I resigned from Columbia in 2013. I was in Columbia for 15 years. I have been in Princeton for 6 years.

I **You were in both Columbia and Princeton for 2 years.**

Z It's a common thing in US. If you move from one place, you don't resign immediately, so that you can move back if you change your mind.

I **Some kind of joint appointment?**

Z No, you don't take two salaries. You are on leave from the old place.

I **What is the latest development on the Birch and Swinnerton-Dyer conjecture [BSD] (one of the seven Millenium Problems proposed by the Clay Mathematics Institute) and whose solution you have made some inroads into?**

Z We are still far away from completely solving the BSD. We have three big general progress. One is the Gross-Zagier formula (in 1983) and Kolyvagin's work (a few years later). Combined, they showed a weak form of the conjecture when the analytical rank is minimal, say 0 or 1. The second big progress is that recently Manjul Bhargava (a professor in Princeton) found that in probability the minimal algebraic rank happens pretty high, more than 60 percent. The third big progress is the converse theorem proved by [Christopher] Skinner and Wei Zhang which says that a minimal algebraic rank implies a minimal analytic rank. Combining all these together, one gets a pretty nice statement: the weak BSD holds for a majority of elliptic curves over the rationals. But, of course, for mathematicians, 60 percent means it could still be wrong. After all, these give us good evidence to support the conjecture. My own contribution is not to BSD directly but to the Gross-Zagier formula. It is a striking formula that makes a connection between some geometric objects — some points constructed using transcendental methods, and some analytic objects — some special values of *L*-functions. I feel excited if there is a number theory problem with a lot of geometry involved.

Oh, one thing I forgot to mention: the BSD conjecture also relies on Andrew Wiles' work. Wiles showed that many elliptic curve over the rationals are modular which was a basic assumption in all work in the BSD conjecture. In 1994 when I was in Paris, I heard about Wiles' proof of Fermat's Last Theorem. Then I decided to switch to Gross-Zagier. From 1994 to 2000, I wrote a few papers myself to answer some questions raised in Gross-Zagier's paper. Each paper contains a miraculous formula with a proof of about 100 pages. The proof is a term-by-term calculation, a mass attempt. I didn't really know how to give a simple reason why these formulae are true. So I spent something like 10 years in the 2000s trying to understand why the formula is true intrinsically. I'm an arithmetic geometer and know arithmetic geometry pretty well. The final solution actually needs a tremendous amount of knowledge from automorphic representation theory. Finally, with the collaboration of my two PhD students [Xinyi Yuan, Wei Zhang], in 2010 we finally figured out what was going on. We wrote a book *[The Gross-Zagier formula on Shimura Curves]* to prove a fully generalized Gross-Zagier formula in dimension one, so everything is as beautiful as we have dreamed. The proof is not simple but we know what is going on. One thing about the connection with Singapore is that a high dimensional generalization of Gross-Zagier formula has been conjectured in the framework called the Gan-Gross-Prasad conjecture (Wee-Teck Gan, he has played a tremendous role in how to generalize the formula). This is a very exciting and active new area of research in number theory and arithmetic geometry. My student Wei Zhang has developed a method for proving the conjecture in the high dimension case. He and his collaborators have many local results. But there are very few proven global results.

I **Some years ago, Shinichi Mochizuki came up with a new kind of mathematics which he calls "Inter-Universal Teichmüller Theory" (IUTT) and which he claims can be used to solve the *abc* conjecture (that arises from probably the most famous and oldest unsolved problem in number theory). I believe that you were one of the invited speakers in the workshop at Oxford University in December 2015 to unravel this new theory. What is the status of IUTT? What is your personal view of IUTT?**

Z This is a very interesting thing. I know Shinichi personally because when I was a junior faculty, he was a graduate student. Both of us were working with Faltings. I know him very well. No doubt he is one of the most brilliant mathematicians in his generation. I also knew he was working on this problem about 15 years ago in 2002 or 2003. In the very beginning, I followed his papers he sent to me. His work had some connection with my early work. I read it, a few hundred pages. It's pretty clear he had some new ideas but I was not convinced that his ideas can prove the *abc* conjecture. Then about 5 or 6 years ago, he claimed that he proved the whole thing. The whole mathematical community was very excited. Pretty everybody wanted to find out what was going on, and then in 2015, Oxford University wanted to form a workshop to see if there is something there. I was told that one of my papers on the *abc* conjecture for function fields had something to do with Mochizuki's new work. I was invited to give a lecture. I gave the first lecture. It was completely elementary and understandable. Faltings was there too and had a similar attitude as me, hoping that somebody could explain to us what was Mochizuki's proof. We were a little disappointed. Nobody could really explain what Mochizuki's ideas were. We had Skype with Mochizuki but that didn't help us very much.

I **There was another conference in Kyoto.**

Z Yeah, they had another conference in Kyoto. I didn't go. I knew somebody who went. I have talked to them. They said it was mathematically similar, a bit more, but, generally, people still did not know the general strategy of the proof. I mean, this is mathematics. The IUTT is not a simple 100 pages in length. It's a few thousand pages all put together. Without a general sketch of the picture, I think it's hard to convince people to look at the papers. In the mathematical world, before you jump into the pile of papers you need to have some idea at first. To be honest, I don't really understand his proof.

I **Mochizuki himself is not very articulate and does not seem to speak much about his theory in public.**

Z I don't know about that personally, but basically from my understanding he has said that the proof is in the pile of his papers. If you want to know the proof, you read it. If you want to talk about it, go to Kyoto and talk. That's my understanding. I don't know why the situation has come to that. It's pretty sad. I hope somebody can give us some idea. Anyway Mochizuki is a brilliant mathematician. That's for sure. He had some theorems that showed his brilliance.

I **Do you think a solution of the *abc* conjecture will be able to resolve the Goldbach conjecture?**

Z No, I don't think the *abc* conjecture can be used to solve the Goldbach conjecture.

I **The Goldbach conjecture also involves equations like $a + b = c$.**

Z The *abc* is really for the Diophantine equation over the integers and the Goldbach conjecture is about primes. For the Goldbach conjecture, we also have no idea what is going on. We can prove "one-plus-two" [a result of Chen Jingrun (1933-1996) related to the Goldbach conjecture] and the similar statement for the twin primes. We are talking about various tools in number theory: automorphic forms, sieve methods, algebraic geometry. For the Goldbach and the twin primes problems, more analytical methods are used because we want to understand how primes are distributed. For the *abc* conjecture we use algebraic geometry. Probably we may use automorphic forms in the future. So far, they are two different categories, but a solution of the *abc* conjecture can pull in some surprising results about the zeta function that might be interesting in the future. Right now, I don't see any connection between the Goldbach and *abc* conjectures.

I **Has the computer played any significant role in modern number theory?**

Z Yes, like the BSD conjecture. Before they formally proposed it, they actually verified it in the computer lab in 1965. But it has not played any significant role for most of the number theory problems.

I **I think the Fields Medalist Manjul Bhargava of Princeton uses the computer. Am I right?**

Z Yeah, he does, but I don't know whether it plays a fundamental role or not.

I **During the last 10 years, you have regularly visited Tsinghua University and the Chinese Academy of Sciences. What is your assessment of the state of development of mathematics in China? In particular, has the level of mathematical research in China caught up with that in Europe and US?**

Z The undergraduate math education in China is always very solid but the graduate level is a bit behind that of top places in the west. For example, graduate students in number theory in China were pretty much in the older tradition, and I would give courses, tutor and help them little by little. Then in the last 10 years, things changed and many of the best undergraduate students go to the west to study. Some of them have done very successfully. Most of the people still stay in US and Europe; a few people came back to China. Another interesting thing is that in summer, the activity in China is more than in any other place in the world because only China can organize so many activities in summer. Many mathematicians will visit China in the summer. I think the current situation in China is very encouraging. But the research level there is still far behind US and Europe.

> **"LEARNING ITSELF COULD BE A FUN AND EASY PROJECT, ESPECIALLY WHEN YOU HAVE A MOTIVATION OF SOLVING AN IMPORTANT PROBLEM. THAT HAS ALWAYS BEEN MY MATHEMATICAL LIFE. I MEAN, AFTER PHD, EVERY FEW YEARS I LEARN A NEW MATHEMATICS."**

I **How successful is the Chinese government in attracting talent back to China?**

Z I think they are pretty successful. One thing pretty unusual is that the Chinese government is willing to invest heavily to attract the best talent in the world to do mathematics and fundamental research. Beijing University has a new institute run by Gang Tian, Tsinghua University has a new institute run by [Shing-Tung] Yau, and the Chinese Academy of Sciences has a new institute, also run by Yau. Each year they bring in the best people to become their full faculty member: two, three, or even more. The institutes have quite a few very talented people there. The Chinese government provides them with houses, good salaries and good support staff. The strong undergraduate education also helps to attract these young people back to China as these young people need to have very strong undergraduates to be their graduate students. All together make stronger PhD programs which in turn attract more top mathematicians to work in China.

I **There is no tradition of pure research in China.**

Z Not much historically. There is some pure research in modern times, but it's more practical than US and Europe in some sense. People in China like to ask, "What is it used for?" This is why a lot of people in China study analysis because you can see applications immediately. It is used all over in PDE [partial differential equations] and applied math. And a lot of people think that geometry must be combined with physics. Recently i see that more top students want to study algebra and number theory, which is mostly based on beauty and curiosity.

I **Some of your doctoral students have made breakthroughs in number theory in their graduate work. How do you tell whether he or she has the potential to do good graduate research work in number theory?**

Z No, I don't have any idea about the potential of new students. But I think they are working hard (that's the typical Chinese way) to do mathematics like in anything else. My responsibility as a teacher is to give the student a problem that I feel is very interesting and very important. I have no idea how to solve it. I wish the student can solve it. At the same time the student can find his own problem. In the end, either he solves my problem or his own problem. Of course, that way I lose about 50 percent of new students. That's fine with me. The students are talented. If they don't study mathematics with me, they can study with somebody else, or do something other than mathematics.

I **What advice would you give to a beginning graduate student in number theory?**

Z I have two advice. First of all, you need to have a solid background in college mathematics. To solve a problem you have to get your hands dirty. You will use analysis, algebra, or everything you learned in college. The second thing is you need to be good in learning any kind of new mathematics. To start, you need to read one book on elementary number theory. If you like it and decide to go to the next step, you have to learn a great deal of algebra, geometry and analysis. Usually, people who do number theory are already good at everything, like Gauss, Euler, Fermat, etc. [Carl Friedrich Gauss (1777-1855), Leonhard Euler (1707-1783), Pierre de Fermat (1601-1665)].

I **It's tough having to study so many things in mathematics.**

Z Yes and no. Learning itself could be a fun and easy project, especially when you have a motivation of solving an important problem. That has always been my mathematical life. I mean, after PhD, every few years I learn a new mathematics. I use that to solve old problems.

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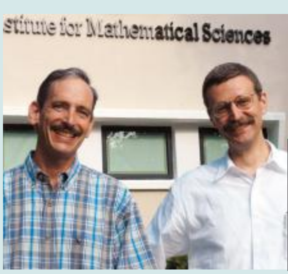

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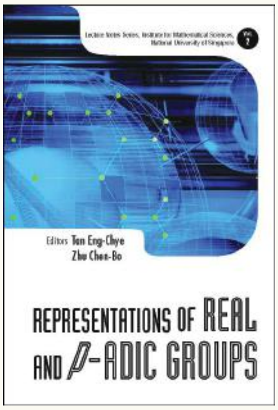

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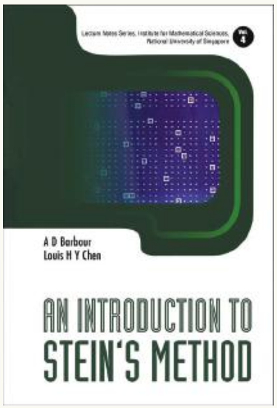

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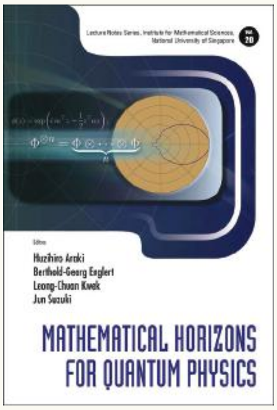

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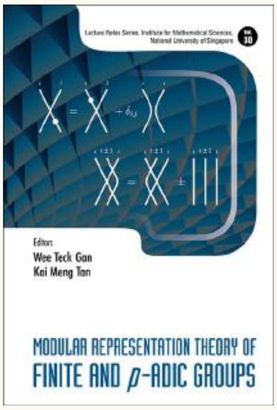

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This volume contains research works in the areas of modular representation theory of *p*-adic groups and finite groups and their related algebras. The aim of this volume is to provide a bridge — where interactions are rare between researchers from these two areas — by highlighting the latest developments, suggesting potential new research problems, and promoting new collaborations.

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