IMPRINTS

JULY–DECEMBER 2024 ISSUE 44

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

Gift from GS Charity Foundation

The National University of Singapore, together with the Hong Kong University of Science and Technology, The Chinese University of Hong Kong, and The University of Hong Kong, have received a generous donation totaling HK$50 million from the GS Charity Foundation, the philanthropic arm of the Glorious Sun Group. This significant contribution will support academic research and talent development in pure mathematics across these institutions over the next five years.

Acknowledging the fundamental role and transformative potential of mathematics in driving technological innovation, this donation from the GS Charity Foundation empowers these leading universities to initiate and sustain efforts in talent cultivation, academic exchange, and international collaboration.

Funded by this gift, the Institute for Mathematical Sciences (IMS) will launch the Glorious Sun Postdoctoral Research Scheme and the Glorious Sun Visiting Scholar Scheme. Additionally, the gift will enable IMS to host academic conferences that foster knowledge exchange among scholars both locally and globally.

From left to right: Prof Zhang Xiang, President and Vice-Chancellor of HKU; Prof Rocky Tuan, Vice-Chancellor and President of CUHK; Dr Charles Yeung, Chairman of GS Charity Foundation and Glorious Sun Group; Prof Nancy Ip, President of HKUST; and Mr Clarence Ti, Deputy President (Administration) of NUS, signed an agreement to jointly support academic research and talent cultivation in pure mathematics over the next five years.

FEATURED

NEWS

OTHERS

COVER ARTICLE

Intertwining between Probability, Analysis, and Statistical Physics

From 5 to 15 August 2024, the Institute hosted a program on "Intertwining between Probability, Analysis and Statistical Physics". The organizers contributed this invited article to Imprints.

BY MICHAEL CHOI *(National University of Singapore)*

The recent workshop, "Intertwining between Probability, Analysis, and Statistical Physics," held during August 5–15, 2024, at the Institute for Mathematical Sciences (IMS), National University of Singapore (NUS), aimed to bring together researchers interested in the notion of intertwining relations. This interdisciplinary workshop on the concept of intertwining span across fields such as pure and applied mathematics, mathematical physics, and quantum mechanics. Organized by Michael Choi (NUS), Pierre Patie (Cornell), and Laurent Miclo (Toulouse School of Economics and CNRS), the event successfully fostered cross-disciplinary collaboration and enhanced engagement within the Singapore research community.

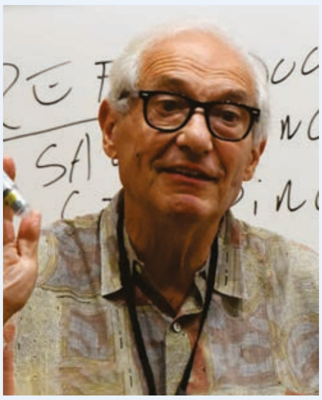
The first week of the workshop featured pedagogical mini-courses led by prominent experts, including Persi Diaconis (Stanford), Cristian Giardina (Università degli Studi di Modena e Reggio Emilia) and Laurent Miclo. Diaconis' mini-course is simultaneously an IMS Distinguished Lecture and NUS Department of Statistics and Data Science Distinguished Lecture, drawing wide participation from both NUS and the broader research community. In Diaconis' mini-course, the topics of Markov chain mixing times, duality, strong stationary times and cutoff phenomenon were introduced, followed by applications of these topics in intertwining. The mini-course concluded with a lecture on open problems. In Giardina's mini-course, a Lie-algebraic approach to duality of Markov processes is extensively discussed. Finally, Miclo's mini-course was devoted to set-valued intertwining of diffusion processes and functional inequalities. These mini-courses provided


Ali Zahra


Andrew Chee


Cristian Giardina


Filip Stojanovic


Persi Diaconis,
Distinguished Visitor


Sabine Jansen


Theo Assiotis

participants a solid foundation to engage with more specialized sessions in the following week.

In the second week, scientific sessions were dedicated to cutting-edge research presentations by leading scholars and young researchers. Talks showcased theoretical advances, such as Federico Sau's insights on Aldous's averaging processes and Rohan Sarkar's work on Markov semigroups on Carnot groups. These sessions were attended by graduate students and researchers from multiple departments within NUS. Discussions often continued in informal settings, such as group dinners and tea breaks, leading to productive exchanges and new connections among participants.

The event's interdisciplinary scope attracted researchers beyond pure mathematics, including those in physics and Bayesian statistics, emphasizing the workshop's broad relevance. For example, Diaconis connected with Wen Wei Ho (NUS Physics) on random walks on groups and their applications in quantum physics.

Social activities were also organized, including excursions to Gardens by the Bay and VivoCity, which allowed participants to explore Singapore's unique landscape and culture. The collaborative atmosphere led to noteworthy collaborations and exchanges. For example, Diaconis connected with NUS faculty on topics such as Stein's method, exponential random graph models, Markov chain Monte Carlo and probabilistic number theory, and Dario Spano (Warwick) discussed recent results in branching processes with Neil O'Connell (Dublin) and Matthias Winkel (Oxford), highlighting the workshop's impact in stimulating ongoing research dialogues.

In summary, this workshop provided a rich perspective on recent developments in intertwining and facilitated meaningful collaborations. With 43 participants from around the world, including prominent researchers and graduate students, the workshop underscored IMS active role in fostering international collaboration in mathematical sciences.

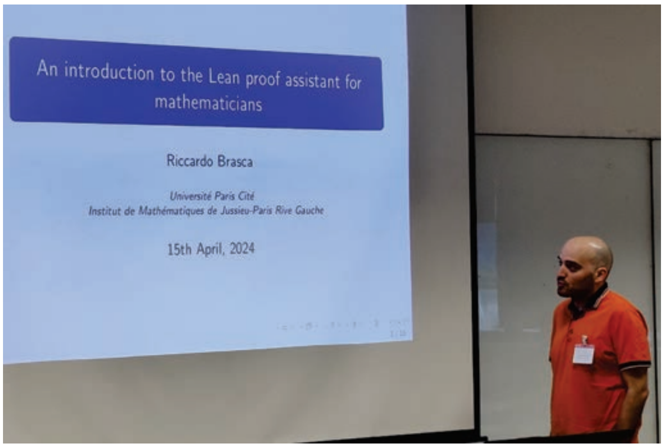
Workshop on Formal Proofs and Lean

15 April 2024–26 Apr 2024

CO-CHAIRS:

Huanchen Bao | National University of Singapore
Jiajun Ma | Xiamen University and Xiamen University Malaysia
Shanwen Wang | Peking University
Lei Zhang | National University of Singapore

For this workshop, there were a total of 56 participants (30 overseas, 26 local). There were 15 graduate students (three overseas, 12 local), which included ten PhD students and five Master's students.



Ricardo Brasso

Index Theory and Complex Geometry Part 2

29 Apr 2024–10 May 2024

CO-CHAIRS:

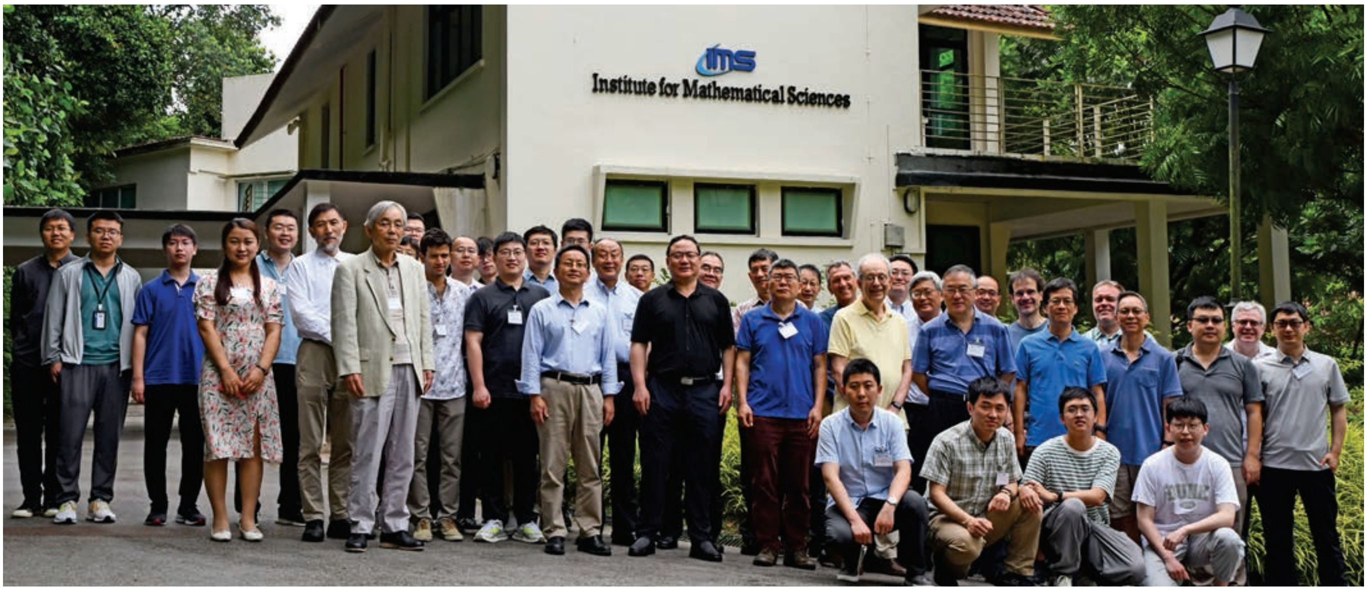
Tien Cuong Dinh | National University of Singapore
Fei Han | National University of Singapore
Xiaonan Ma | Université Paris Diderot - Paris 7
Shu Shen | Sorbonne Université
Mathai Varghese | University of Adelaide

The aim of the program is to deepen understanding and foster collaboration between Index Theory and Complex Geometry by exploring their shared connections, particularly in areas like geometric hypoelliptic Laplacians and the analytic localization technique. It seeks to leverage recent advancements in pluripotential theory, the Hörmander L2 method, and Bergman kernel studies to enhance cross-disciplinary insights and drive further developments in both fields.

A total of 38 talks were delivered during the two-week program, which was attended by 73 participants, including ten graduate students.

Jean-Michel Bismut, Distinguished Visitor

Ngaiming Mok



Statistical Machine Learning for High Dimensional Data

13 May 2024–31 May 2024

CO-CHAIRS:

Jialiang Li | National University of Singapore
Wei-Yin Loh | University of Wisconsin - Madison
Miaoyan Wang | University of Wisconsin - Madison

The workshop aimed to advance machine learning methodologies by addressing key challenges in data science, particularly in quantifying data structure complexities and linking them to suitable models. The workshop played an important role in bridging gaps by forging new connections between the fields of statistics, computer science, optimization, and domain sciences.

Jianqing Fan (Princeton University, USA) delivered a Distinguished Lecture Series in Statistics on "Inferences on Mixing Probabilities and Ranking in Mixed-Membership Models". His talk focused on making inferences about mixing probabilities and ranking within mixed-membership models.

The workshop had a total of 101 participants, including 27 students.

Miaoyan Wang

Andrew Barron

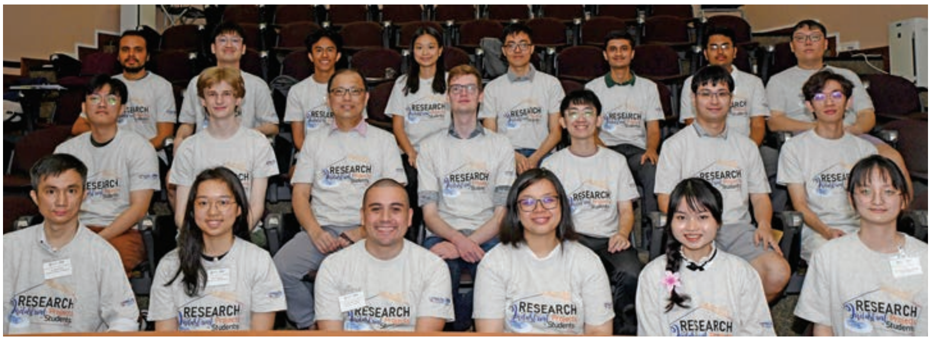
Delivered Distinguished Lecture, **Jianqing Fan**

Research in Industrial Projects for Students (RIPS) 2024 – Singapore

20 May 2024–19 July 2024

The Research in Industrial Projects for Students Program in Singapore (RIPS-SG) provides an opportunity for talented undergraduate students to work in international teams on a real-world research project proposed by sponsors. The student team, with support from their academic mentor and industry mentor, will research the problem and present their results, both orally and in writing, at the end of the program.

A total of 15 students (four from USA, three from Asia and eight from NUS) were selected for this program.



The sponsors of the research projects were MOH Office for Healthcare Transformation (MOHT), The Procter & Gamble (P&G) Singapore Innovation Center (SgIC), Cubist Systematic Strategies, and Qube Research & Technologies (QRT).

Computational Aspects of Thin Groups

03 Jun 2024–14 Jun 2024

CO-CHAIRS:

Bettina Eick | Technische Universität Braunschweig
Eamonn O'Brien | The University of Auckland
Alan W. Reid | Rice University
Ser Peow Tan | National University of Singapore

The focus of the two-week event was computational aspects of thin groups. The activity highlights the need for algorithms and procedures to study these groups. The event brought together a diverse group of mathematicians with backgrounds in group theory, number theory, geometry and topology to explore and report on recent advances.

The event was attended by 58 participants in total, including 13 graduate students.

Andrei Rapinchuk

Heiko Dietrich

Sang-hyun Kim

IMS-NTU joint workshop on Biomolecular Topology: Modelling and Data Analysis

24 Jun 2024–28 Jun 2024

CO-CHAIRS:

Jelena Grbic | University of Southampton
Wu Jie | Hebei Normal University
Xia Kelin | Nanyang Technological University

The program's primary aim is to establish the foundation of the new interdisciplinary subject Biomolecular Topology. The goal is to foster new research in biomolecular topology and promote transformative topological techniques by bringing together experts from geometric topology, algebraic topology, combinatorial topology, computational topology, and topological data analysis to tackle fundamental biological challenges.

The program had a total of 105 participants, comprising 80 from overseas and 25 local attendees. Among them were 20 graduate students (13 overseas and seven local), including 18 PhD students and two Master's students.

Christian Micheletti

John Z.H. Zhang

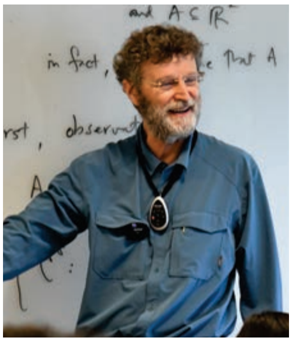
IMS Graduate Summer School in Logic 2024

20 Nov 2023–23 Nov 2023

LECTURERS:

Su Gao | Nankai University, China
Theodore A Slaman | The University of California at Berkeley, USA
W Hugh Woodin | Harvard University, USA

A total of 57 students attended the Summer School, which included 48 PhD students and nine Master's students.



Ted Slaman

Summer School in conjunction with SciCADE

08 Jul 2024–12 Jul 2024

CHAIR:

Weizhu Bao | National University of Singapore

In conjunction with SciCADE 2024, a summer school on "Scientific Computation and Differential Equations" took place from 8–12 July 2024. During the event, four distinguished researchers—Elena Celledoni (Norwegian University of Science and Technology), Qiang Du (Columbia University), Shi Jin (Shanghai Jiao Tong University), and Alexander Ostermann (University of Innsbruck)—delivered tutorial lectures on topics related to scientific computation and differential equations. The participants of the summer school included PhD students and junior researchers including postdocs.

There were a total number of 80 participants (61 overseas, 19 local). There were 58 graduate students (49 overseas, nine local), which included 57 PhD students and one Master's student.

Interpretable Inference via Principled BNP Approaches in Biomedical Research and Beyond

08 Jul 2024–02 Aug 2024

CO-CHAIRS:

Maria De Iorio | National University of Singapore

In week one (9–10 July), the program commenced an opening workshop featuring an introductory keynote lecture by Michele Guindani (University of California, Los Angeles) and 23 speakers covering recent theoretical developments and related applications.

In weeks two and three, a more open program was scheduled, with ample time for informal interaction, several tutorials and research talks. The lecturers of tutorials included Steven MacEachern (Ohio State University)) and Long Nguyen (University of Michigan) on foundations of BNP, and Yang Ni (University of Texas A & M) and Yanxu Xu (John Hopkins University) on BNP in biomedical research.

Additionally, there were ten full-length research talks to expose some current research frontiers.

During week four (30 July–2 August) a closing workshop was organized. The closing workshop was co-sponsored by the International Society for Bayesian Analysis (ISBA), section on nonparametric Bayesian inference (ISBA/BNP). With the co-sponsorship of ISBA/BNP Networking meeting we were able to generate renewed engagement, include additional speakers (not funded by the Program) and explore more dimensions of current BNP research.

A total of 73 people, including 12 were students attended the program.

Alejandro Jara

Fernando Quintana

Long Nguyen

Frontiers of Functional Data Analysis: Challenges and Opportunities in the Era of AI

19 Aug 2024– 13 Sep 2024

CO-CHAIRS:
Alexander Aue | University of California at Davis
Ying Chen | National University of Singapore
Zhenhua Lin | National University of Singapore
Qiwei Yao | London School of Economics

This one-month-long program provided an ideal platform for the local and international mathematicians, statisticians and data scientists to exchange ideas and promote development of FDA for the challenges it faces in the era of big data and AI. The program aimed to arouse local researchers' interest in FDA, especially via presentations and discussions that focused on connections between FDA and other domains such as machine learning. Professor James Stephen Marron (University of North Carolina) gave a talk under the Distinguished Lecture Series in Statistics.

Hannah Lai

James Stephen Marron

A total of two mini lectures and 18 talks were delivered during the four-week program. About 82 people, including 38 students, attended the workshop.

Workshop on Theta Correspondence

09 Sep 2024–13 Sep 2024

CHAIR:
Edmund Karasiewicz | National University of Singapore

A total of 25 participants (from Singapore, China, Taiwan, Japan, Australia and Israel) attended the workshop. On 10 Sep, there was 24 Hours of Theta, going round-the-clock, starting in Singapore, carrying on in the UK and finishing in the USA. On 11 Sept, the lecture by Binyong Sun also served as a colloquium lecture of the Department of Mathematics, with more than 70 attendees.

IMS-NTU joint workshop on Applied Geometry for Data Sciences Part I

30 Sep–04 Oct 2024

CO-CHAIRS:
Fei Han | National University of Singapore
Wilderich Tuschmann | Karlsruhe Institute of Technology
Zhenhua Lin | National University of Singapore, Singapore
Kelin Xia | Nanyang Technological University

This workshop aimed to combine the advanced geometric tools with data-driven learning models. It covered recent advancements from discrete geometry, computational geometry, geometric data analysis, to geometric deep learning.

Pietro Lio

Dingxuan Zhou

Pierre Alliez

Bohang Zhang

PAST ACTIVITIES

Interactions of Statistics and Geometry (ISAG) II

14 Oct 2024–25 Oct 2024

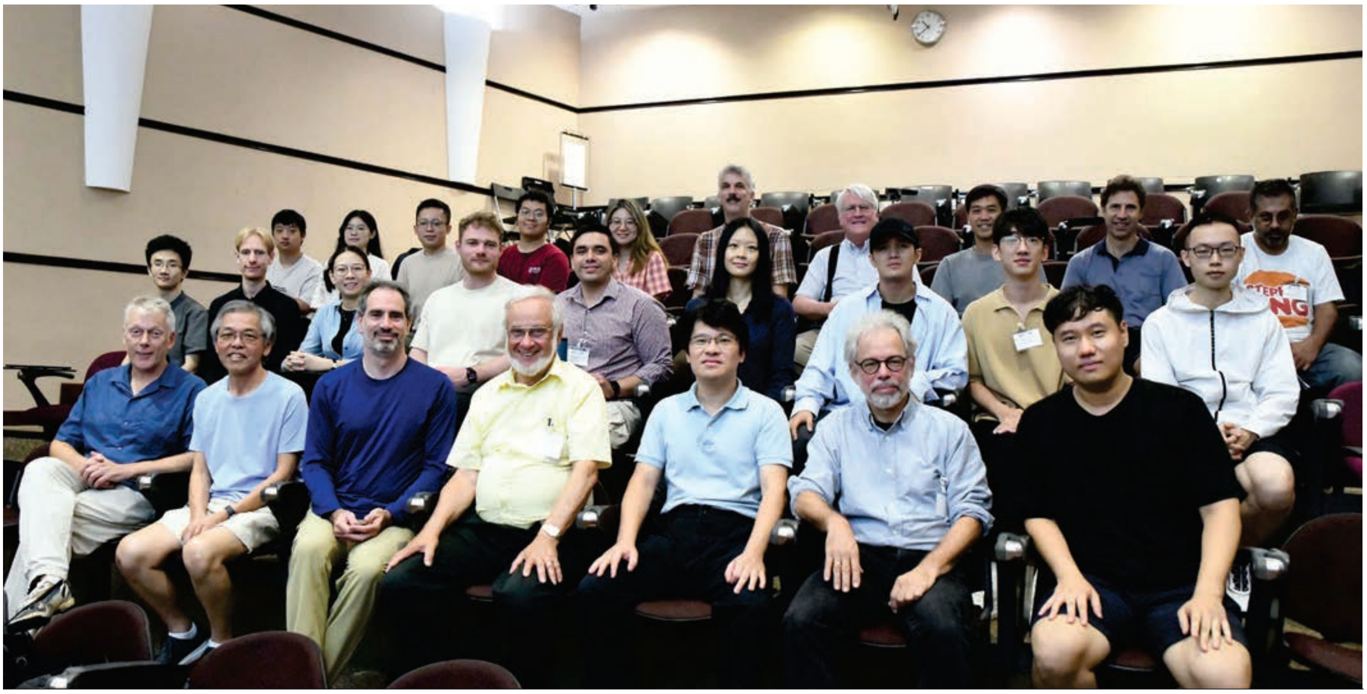
CO-CHAIRS:

Stephan Huckemann | Universität Göttingen
Ezra Miller | Duke University
Zhigang Yao | National University of Singapore

A total of four tutorials and 24 talks were delivered during the two-week program. About 46 people, including seven students, attended the workshop.

Armin Schwartzmann

Ezra Miller



Ng Kong Beng Public Lecture Series

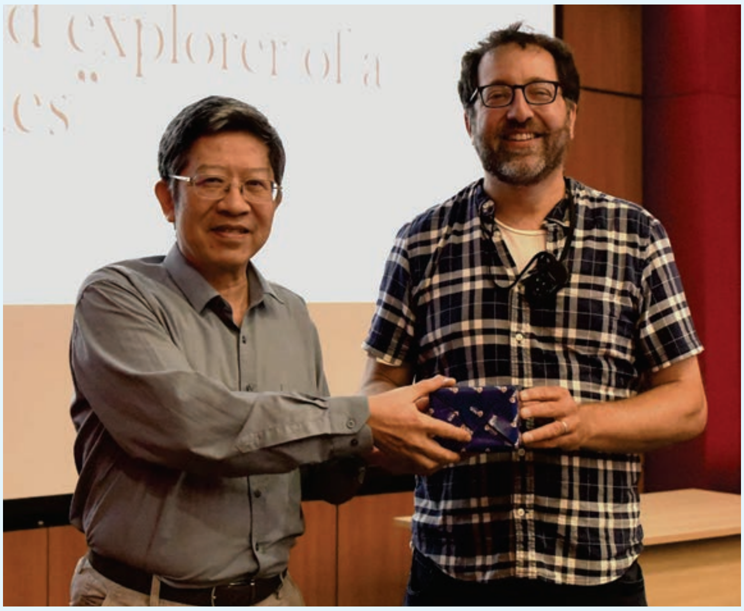
From mosquitoes to ChatGPT — the birth and strange life of the random walk

13 June 2024

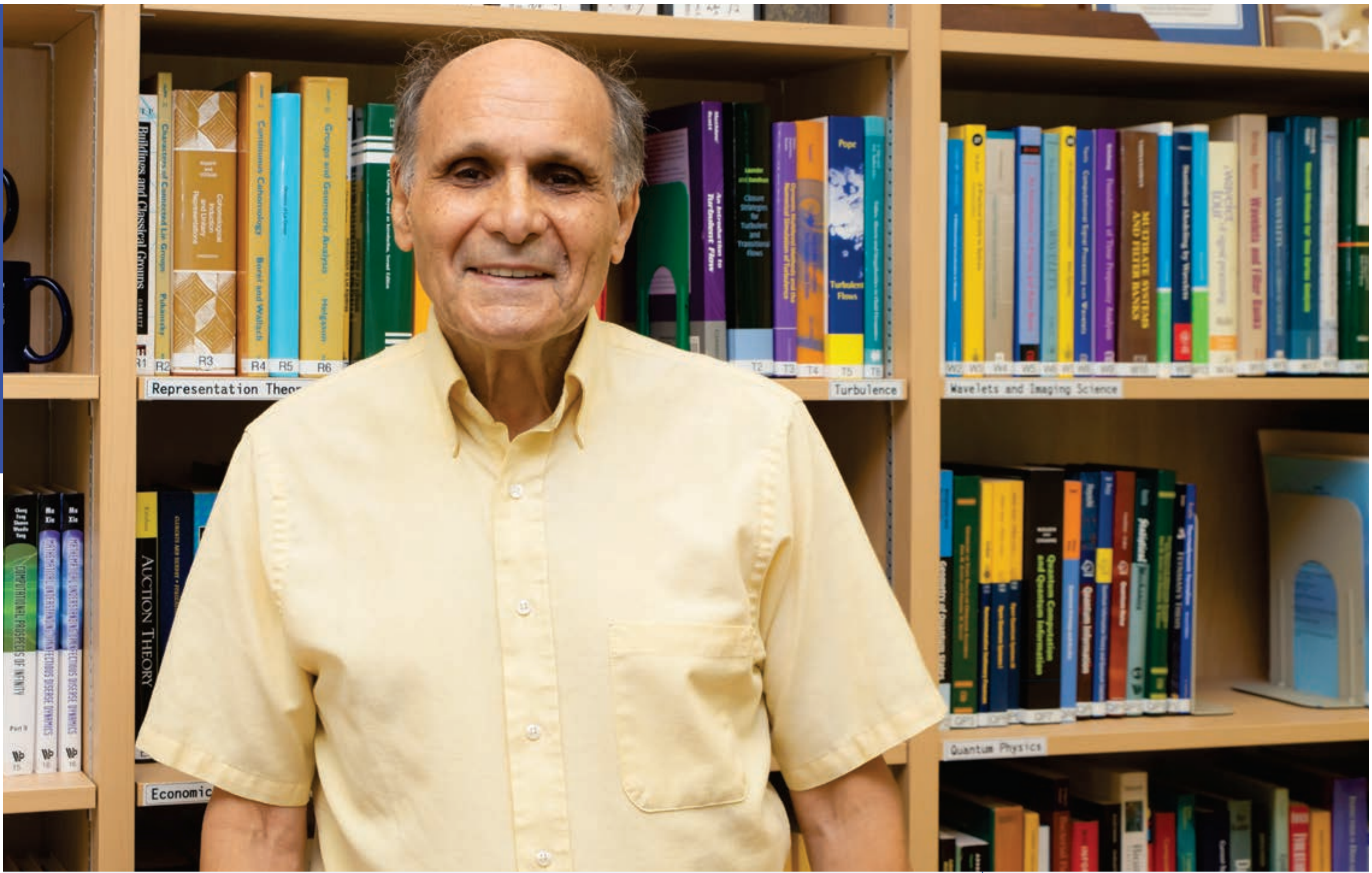
Jordan Ellenberg is the John D. MacArthur Professor of Mathematics at the University of Wisconsin at Madison, specializing in number theory and algebraic geometry, with related interests in algebraic topology, combinatorics, and data science.

In this public lecture, he discussed how, between 1905 and 1910, the concept of the random walk—now a significant topic in applied mathematics—was simultaneously and independently developed by various individuals across multiple countries for entirely different purposes, from mosquito control to physics to finance to winning a theological argument.

Jordan Ellenberg



Presenting Gift



MEL LEVY

DFT AND BEYOND

Interview of Mel Levy by Yukiang Leong

Mel Levy is Emeritus Professor at Tulane University. He obtained his PhD at Indiana University and postdoctoral education at Johns Hopkins University. He is one of the founders of the Density Functional Theory (DFT), and his works include the constrained-search formalism, the identification of the derivative discontinuity, the development of DFT perturbation theory, the discovery of many exact conditions on density functionals for the purposes of their approximations, especially those involving coordinate scaling, variational principles for excited states, and many more. He is a member of the International Academy of Quantum Molecular Science and Fellow of the American Physical Society.

From 1-15 September 2019, Levy visited the IMS as Distinguished Visitor for the programme on "Density Functionals for Many-Particle Systems: Mathematical Theory and Physical Applications of Effective Equations, which was partially supported by Julian Schwinger Foundation. He gave two Distinguished Visitor Lectures on 5 Jan 2019 titled "Coordinate Scaling Constraints in Density and Density-Matrix Functional Theories" and on 10 Jan 2019 titled "On the History, Variational Foundations, and Evolution of Time-Independent Density-Functional Theory". During his visit, Yukiang Leong took the opportunity to interview him on behalf of Imprints on 12 Jan 2019. The following is an edited and vetted transcript of the interview, in which he talked about his early education and his academic career, especially his deep involvement with DFT.

IMPRINTS I Could you tell us something about your early education and how you came to choose physics as a career?

MEL LEVY L I was born in Brooklyn, New York in 1941. I chose my career based on the fact that I was always very good in mathematics. I was also not very good in learning languages. When I would hear foreign languages, the words all came together and I could not understand too well. At a very young age, my family used to give me math puzzles that I could do in my head, and everybody was very surprised that I could do them. I loved math from the beginning, and so I wanted to do something mathematical.

I Was your father a mathematician?

L No, but he was very good in math, and he used to give me puzzles. My father was a victim of the Depression and he had to drop out of college to support the family. It was a very tough financial time. He was very good verbally as well, but he was unable to complete college, and he worked in the post office finally, and did other jobs. He worked very hard. He enjoyed giving me puzzles and taught me mathematical tricks. I used to solve them in my head, but then as I got a little older, I learned mathematics a little more formally, and I had more difficulty doing it in my head. I always had to write it down. At first, I could do it in my head, and later I had to write it down. Well, you are a mathematician, so you know about this as well. I knew I wanted to do something mathematical. Actually I started off in college learning both math and chemistry. I was a chemistry combined with math major. I liked chemistry, and physics too, but I wanted to use math to help understand the chemistry.

I What was the topic of your PhD thesis, and who was your thesis advisor?

L I got my PhD in chemical physics in the area of quantum chemistry. The purpose was to use the many-body Schrödinger equation to be able to make chemical predictions. I worked on what is known as strongly orthogonal geminals. These were electron pairs, functions within the full electron wave function and the objective was to build larger wave functions by transferring the pairs from smaller wave functions. That was the subject of my dissertation. It was in quantum chemistry, a branch of chemical physics. The title was "The transferability of strongly orthogonal geminals from water to hydrogen-peroxide". Hydrogen-peroxide is bigger than water. You took the pairs from water and you built hydrogen peroxide from it. But you still needed to do the oxygen-oxygen bond. That was the only thing that we had to calculate. My dissertation was at Indiana University. My research director was Professor Harrison Shull.

I Were you in the chemistry department, not the physics department?

L I was in the chemistry department, not at the physics department. But the degree was combined. I took courses in both departments. So it was a joint degree, chemical physics. Then, I did postdoctoral work with Professor Robert G Parr at Johns Hopkins University. He was also in the chemistry department there. He was a well-known quantum chemist. So this was my training.

I You were first at the University of North Carolina at Chapel Hill for only three years before you moved to Tulane University, where you have stayed ever since. Was there any particular reason for your move to Tulane, and what was it that made you remain there for such a long period of time?

L I would have liked to stay at the University of North Carolina at Chapel Hill, but the position they had for me was only a temporary position. It was understood that it was not tenure track and there was no permanent position available. I should say that it was a very, very difficult time. The job market was awful in the United States.

I Was that before the Sputnik era?

L Sputnik was in the middle 1950s. And then a lot of money went into science. What happened was, (this is my feeling, what I've observed, and I think I'm correct to some extent) when we successfully landed on the moon in 1969, the mission was accomplished. Although people were very excited in the few days after our landing on the moon, interest in science was lost by the government funding to a great extent. Because we already showed we could beat the Russians and get to the moon, the space project to get to the moon was too successful and too fast, so the funding was then decreased after all that funding increase after Sputnik. And then funding after the landing wasn't as much as far as I remember. Also another thing that occurred was that there was a lot of hiring of faculty right after Sputnik so that we would in the United States develop science. So when I started looking at the jobs 20 years later, all these faculty members were still there and not retiring. As a result, there were no positions for people like me and many other people. So the academic job market was very difficult. I was very fortunate to have this position at North Carolina, even though it was just for a couple of years, because it helped me get another position afterwards at Tulane. At Tulane in New Orleans, my position was only for one year at the start. It was another temporary position. But I worked very hard; I never worked so hard in my life. I taught many courses, I published papers, and it was a difficult time. My wife was supportive. We had our first child. And what happened was Tulane was trying to hire an experimentalist. At the last minute, the experimentalist did not take the job. So they offered it to me a permanent tenure track position, and I was therefore at Tulane for many years. That's where I started doing density functionals.

I Where did you do your postdoctoral?

L The postdoctoral was at John Hopkins with Robert G Parr. It was there that I learned about the Hohenberg-Kohn Theorem. And I learned about density functional theory (DFT) but I never did anything with it at the time. Do you know about density functionals? You know what it is?

I I know roughly.

L Okay, so you know the Hohenberg-Kohn Theorem. It was there that I knew the theorem existed. It was there that I learned that there was this functional of the electron density, that it exists in principle and that it simplifies the many-body problem, because you're just using the three-dimensional electron density rather than the 3n-dimensional complicated wave function. So I learned about it there, but I did nothing at the time. It was later, and it leads to the next question.

I At Tulane, was there anybody doing the DFT at the time when you joined the university?

L No, not when I was first there. When I went to Tulane, nobody was doing density functional theory at Tulane. Density functional theory was not well-known at all. There were very few people in the country [US] that did formal mathematical work with density functional theory. And that's what I was interested in. So when I went to Tulane, at that point, I studied for the first time the Hohenberg-Kohn Theorem. By that point also, I became very mathematical. When I started, I did calculations for my doctorate degree, but I was always more interested in the basic mathematics. Even though I was in a chemistry department, I knew that I wanted to only work on the mathematics of the density functional theory. So I studied the Hohenberg-Kohn paper for the first time, and I then realized that I could make a contribution. I studied the proof and it was very exciting to me, and I knew that I could do something with it. I got into it because I learned about it from Robert Parr at John Hopkins. I knew when I came to Tulane, I wanted to work on the Hohenberg-Kohn Theorem, and to work on the mathematical improvements.

I Was your doctoral thesis connected with it [Hohenburgh-Kohn Theorem]?

L Nothing to do with it. My doctoral thesis was on the wave function, the many-body wave function. It had nothing to do with density functional. I didn't even know about density functional theory at that point. My postdoctoral work had nothing to do with density functional theory, but I sensed that (since the density functional theory deals with the three-dimensional electron density, which is much simpler than the wave function) this was the future and I decided to [take a] gamble on it. So I came as a young faculty member gambling on it. Some older faculty members who were there longer, said I shouldn't do this work because it was a gamble. It may not go anywhere, but I sensed that something wonderful could happen and this could be a revolution. I decided to gamble. It was a gamble because I didn't have tenure. I could have been fired. I had a young family and I worked very hard, and my wife was supportive. But it did cause stress and it was a gamble.

I And the gamble paid off.

L The gamble paid off. No one was there when I arrived at the density functional theory [in Tulane]. There were very few people in the country [US] working in the area. It was a formalism. Quite by accident a year after I came there, John Perdew came to Tulane and he was in the physics department. I was in the chemistry department, and he was hired and it was a coincidence. Very few people were working in density functional theory.

I Was he working in density functional theory?

L He started it, yes. He did his post-doctoral work with David Langreth in density functional theory at Rutgers University, and he came to do density functional theory at Tulane. He didn't know I was there. I didn't know who he was when he arrived. And then we realized we were both working in the same area, density functional theory, and very few people in the country were working on it. It was a coincidence that we were. We had offices on the same floor. So that's how we started collaborating. But in your original question, it's interesting, you said John Perdew. What made you bring up John Perdew? I mean, where did you get this from?

I I was searching for information about the DFT, and his name came up. How did you get together to collaborate?

L I see. (I myself don't even have a website. I have to put up a website and things.) So we just started talking, and we went to the blackboard and we started collaborating. I will talk about that collaboration.

I I think your collaboration with John Perdew is one of the earliest on the subject, and you were the first to do the Hohenberg-Kohn Theorem extension?

L Yes, and this was very exciting for me. So, about three years after I came to Tulane, I realized that I could extend the basic [Hohenberg-Kohn] functional. Should I use the blackboard? I will show you what I did. [Writing in the blackboard, …] So they [Hohenberg-Kohn] showed that there exists a universal functional density. But their density was restricted. [Continuing to explain with the blackboard, …] So I extended the domain of definition to all densities and not just these special densities [non-degenerate ground state densities of some external potential]. I noticed this in late December 1978, and this is called the Levy constrained search. I would suggest looking it up in the web, at Levy or Levy-Lieb constrained search, Elliot Lieb at Princeton.

I How did you get to collaborate with Lieb?

L We never collaborated. I did it first. He did it later. Mine was in 79. Mostly, his papers were in 1983. People call it both: either Levy, or Levy-Lieb. So that [my paper] was in 1979, and it was published in the Proceedings of the National Academy of Sciences. That was an extremely important development. This proof (the Levy-Lieb proof) was used by Walter Kohn in his 1999 Nobel Lecture in the Reviews of Modern Physics. He uses this proof in the lecture, not the original Hohenberg-Kohn proof (in 1964). [After a long pause] I tell you what I did. [Continuing to explain, …] I was able to derive coordinate scaling properties of F, for the purpose of approximating F. [Continuing, …] And then we found out how the exact F would behave. This came in 1985 and it was in collaboration with John Perdew. This was one of our collaborations. And that was a very important work. I've been doing this coordinate scaling since, but this was with John Perdew. I've also started with earlier collaborations, dealing with fractional electron number in 1982 with Perdew and Robert G Parr. And it was called the PPLB (1982), Perdew, Parr, Levy, Balduz. You could just look that up on the web. Fractional electron numbers, that's also with John Perdew. So the first thing you do is: you need a formal definition and then you derive properties of this F that does this. [Continuing to explain, …] And then it keeps going. So what you want to do is to look at this definition and the objective is to get the rest of the terms here, in terms of just the density.

I Is it an infinite series?

L This is unknown. You see what I mean? It's not like a Taylor series. We don't know what this is. And it gets complicated, but you see what's happening. We have like a scaling series for the first part. This part here is the kinetic energy. [Continuing to explain, …] Now, let me tell you the relationship with John Perdew. He was very excited. So what I did, I derived a lot of the constraints to properties, and he derived properties too, of the functional that he put into his approximate functionals that are used all over the world. There are different functionals. So that's where he came in.

I Why was the DFT controversial in its early years of development? What was it that's led to its acceptance by the scientific community later on?

L I can tell you why. It was controversial mostly in chemistry. And the reason was because the chemists didn't understand it. You see, they didn't understand it.

I The physics is quite clear, isn't it?

L To physicists it was clear. It was most controversial in chemistry because they simply didn't understand it. They thought the kinetic energy was not treated properly in the theory. But in fact, the kinetic energy is treated very well in density functional theory, and better than in the traditional quantum chemistry way of doing things at the time. But the people in chemistry did not understand this. Also a lot of people [who] did calculations in physics as well as those in chemistry, were not careful enough to interpret the results in terms of the exact theory. You see what I mean? These are the two things.

I Why did you call it a theory rather than a method?

L Well, it's a theory because it's based on exact mathematical theorems.

I But in its applications isn't it a method?

L In its applications it is a method, of course. Quantum mechanics is a theory because it's based on the postulates, right? And we say, relativity is a theory, it's based on fundamental postulates, and density functional theory is called a theory because it arises from the fundamental postulates of quantum mechanics. And it is formally correct because quantum mechanics is correct. If it's simply a method, then it simply would be based on trial and error, but it's based on formal mathematical theorems that are very intimately tied to the basic foundations of quantum mechanics. So that's why I would say that.

I It is quite amazing that Walter Kohn, a theoretical physicist, was awarded a Nobel Prize in chemistry?

L Kohn was awarded the Nobel Prize in chemistry. The reason he won in chemistry is because of his work that established that there exists a functional of the density to be approximated. So he proved the first theorem with Hohenberg, Hohenberg and Kohn. And then there was later Kohn-Sham, which helped do the kinetic energy part. Afterwards people made approximations to the functional, and people like Perdew, Burke, Ernzerhof, Lee, Yang, Parr, Becke. These were very important approximations based on the definitions of the functional. And there was a revolution caused by what is known as the general gradient approximation. At first people just assumed the density was uniform and got functionals, then developed into the generalized gradient approximation for non-uniform densities. And then it got into chemistry programs and people were able to use it. So even though Kohn was a physicist, he won in chemistry because of his revolutionary applications. That's why he got it in chemistry

I Would you consider yourself a mathematician?

L Yes, well, I consider myself a mathematician the way Kohn was. I only do mathematics and I've only done mathematics now for 50 years. I use mathematics. I have to prove theorems. So in that sense, perhaps I am a mathematician, also a physicist as well. I've also taught in physics departments, but it's for others to judge whether I am, but I have a passion for mathematics.

I In the application of DFT to material science, have new materials been predicted or discovered by computer simulations?

L Has density functional theory helped discover new materials? There are examples of this. For instance, in 1994, boron nitride nanotubes were predicted to be stable and a semiconductor. First it was done through density functional theory. It was predicted and then it was proven to be true a year later.

I Theoretically proved?

L It was theoretically predicted. There are many, many examples. There were magnetic materials that were predicted and later proven to be true. And here's a very recent article, and it is with a theorist. I could send it to you if you give me your email address. What they did in this article was, through calculations using density functional theory, they were able to understand how the eyes of crustaceans work. I can show you the diagram in here. And you couldn't get at this experimentally. And they were able to do it through calculations.

I It is amazing, isn't it? And it was actually discovered later on.

L Yes, that's right. So the theory has been able to do that. It's been remarkably successful, but there are still open problems, what is known as strong correlations and so on. People still have to work on Van der Waals forces, and many other open problem. I have a list [of problems].

I Do you think that the quantum computer will render necessary to search further for new algorithms or methodologies?

L Quantum computer has a wonderful future. But there will always be a need for the new algorithms, methodologies and formalisms. It'll always have a need, I think. That's my feeling.

I In your long career of prolific scientific collaborations, what is the most memorable experience you have had?

L Well, I think the most memorable experiences to a certain extent, I've talked about. I think in density functional theory, it was the constrained search, the generalization of the Hohenberg-Kohn functional, and I'm so excited to see that it's so well-known today. So this is called the Levy constrained search or the Levy-Lieb constrained search. Working on a fractional electron number and the ionization potential theorem with Perdew, Parr and Balduz. Working on coordinate scaling to get the properties of the unknown functional that people have been using to make approximate functionals. So the coordinate scaling work that was started in 1985, and I did it with John Perdew. Then I did some work by myself in 1990 and so on. And then I collaborated with Andreas Görling, a postdoct in the mid 1990's. There's very exciting new work that I've done with a former student of mine, who's now at Iowa State. His name is Federico Zahariev, and we found a way to get coordinate scaling properties associated with individual spin densities by removing the spin from the wave functions in the definitions. So a spin density functional theory without spins in the wave functions, and this solved the problem we were working on for about 15 years and that I presented here. So that was also very exciting. And there were a number of other exciting moments.

I You're still very active in this field.

L Very active. I just submitted a paper. We first submitted it two years ago to Physical Review A. I'm 78, and I'm still very active and excited, and this is exciting work.

I I understand you need to go to the next lecture, and so let us end here. Thank you very much!

L Thank you.

Interview of Toshiyuki Kobayashi by Chee Whye Chin

TOSHIYUKI KOBAYASHI

ANALYSIS AND SYNTHESIS OF SYMMETRIES IN MATHEMATICS

Toshiyuki Kobayashi is renowned for his innovative contributions to the study of symmetry, spanning analysis, geometry, and algebra. He pioneered the general theory of restricting infinite-dimensional representations to non-compact subgroups, transforming the study of branching laws of unitary representations from a seemingly insurmountable problem to a fruitful one. He also developed a theory of discontinuous group actions beyond the traditional Riemannian setting, creating a new research area at the intersection of groups, geometry, and topology. Another key concept he introduced is 'visible actions' on complex manifolds, which has unified and extended multiplicity-one theorems for both finite and infinite-dimensional cases.

Kobayashi was already appointed as a permanent faculty member at The University of Tokyo (Tōdai) at the age of 24. He remained at Tōdai for 14 years before joining the Research Institute for Mathematical Sciences (RIMS) at Kyoto University in 2001. He returned to Tōdai in 2007 and has held a joint appointment as a principal investigator at the Kavli Institute for the Physics and Mathematics of the Universe (IPMU) since 2011. His prodigious research output consists of more than 110 journal articles and conference proceedings, many expository surveys, as well as three books on Lie theory. He has directed the studies of more than twenty Ph.D. students and postdocs. He has also served on the editorial board of numerous journals, and as a member in several international prize committees. He is the founder of the Takagi Lectures since 2006, and also has co-organized multiple international conferences.

Throughout his illustrious career, Kobayashi has received many awards and prizes, and has been invited to visit universities and institutions worldwide, including

Harvard, Yale, IAS at Princeton, IHES in France, and MPI in Germany. He has also given invited lectures at numerous major conferences, including the International Congress of Mathematicians (ICM). Three international conferences were held to honor his 60th birthday in Europe, Japan, and Africa. The first of these was in June 2022, at Université de Reims in France, with the theme *'Symmetry in Geometry and Analysis,'* where he received an honorary Doctorat Honoris Causa. From July 1-15, 2022, he visited the IMS as Distinguished Visitor for the programme *'Representations and Characters: Revisiting the Works of Harish-Chandra and André Weil'*, which was a satellite conference of the virtual ICM 2022 held at the National University of Singapore (NUS). He delivered three hours of tutorial lectures titled *"Proper Actions and Representation Theory"* on July 1, 2 and 4, 2022, and presented the Distinguished Visitor Lecture titled *"Harish-Chandra's Admissibility Theorem and Beyond"* on July 9. Chin CheeWhye interviewed him on July 13 2022 on behalf of the IMS newsletter Imprints during his visit. The following is an edited and vetted transcript of the interview, in which he talked about his mathematical background, his academic career, and his research ideas in representation theory.

IMPRINTS I You were raised in Japan and you went through the Japanese educational system. Can you tell us about your experience learning mathematics in Japan? Was there a particular parent or teacher who influenced you mathematically?

TOSHIYUKI KOBAYASHI K My family is not oriented towards mathematics, and never enrolled me in a cram school or hired a private instructor. They allowed me a lot of freedom, except for one strict rule: I had to get 10 to 11 hours of sleep every day. I didn't have a teacher who had a particular influence on me mathematically in elementary school. However, there was one thing that I benefited greatly from: I often had to teach my classmates.

I You were teaching, but you were still in elementary school?

K Yes, I was. The school system was quite relaxed back then. When I was around 8 or 9, my elementary school teacher wasn't very enthusiastic about teaching the regular subjects. Especially when it came to the more challenging parts of the lesson, such as explaining how to divide fractions, the teacher would often ask me to take over and would leave the classroom abruptly. Nowadays, it wouldn't be allowed, but at the time… I'm not sure how the other students felt about being taught by someone their own age, but it was an invaluable experience for me. I had to quickly figure out how to handle the situation. It was excellent training for me—I had to think on my feet.

I So you have learned to be very independent in the way that you look at things, at a very young age?

K Yes, looking back, having the rare responsibility of teaching in a classroom at around age 8 was indeed a challenging experience. At home, there was no pressure to study; I was free to pursue my own interests. I enjoyed playing sports, reading books I liked, and I even prepared for the junior high school entrance exams on my own without consulting anyone. Growing up in such a free environment was unusual, but I believe it helped me develop independent thinking from a very young age. However, knowing something and teaching it in an understandable way in a classroom are different.

I It's harder to explain and teach someone else than to just know something yourself.

K Yes. Sometimes I realized that my explanations weren't helping my classmates understand well. I often had to teach without preparation because the teacher would suddenly ask me to take over the class. In such cases, I would reflect on what I should have done differently and try to review and improve after I got home. I believe that this experience from when I was 8 or 9 has also been valuable in my current role, helping me when giving lectures at the university or explaining my theories to other experts.

I You went to Tōdai (The University of Tokyo) for your undergraduate studies, and also did your PhD there. Tell us about your university years. How did you get interested in mathematics?

K Actually, I did not go through a traditional PhD program. I have a PhD, but I didn't follow the conventional path.

I Oh, how is that so?

K At the time, there was a requirement that you had to be at least 27 years old to obtain a PhD. However, this age limit did not apply to teaching at the university, so I didn't…

I Oh, you were too young!

K Right after completing my master's degree, I was offered a permanent position at the University of Tokyo without having to enroll in the PhD program. However, due to the age limit, I had to wait for another 3 or 4 years to receive my PhD.

I That's interesting!

K But I think I started studying mathematics quite late. I didn't study much mathematics in high school. When I entered university at 18, there were still not many mathematics courses during the first two years. We had calculus and linear algebra only once a week. These two years focused on general education, with courses in philosophy, chemistry, physics, law, languages, and sports. In my free time outside of lectures, I started studying mathematics and physics seriously on my own, attempting to reconstruct proofs from math books without referring to the proofs in the books, if possible. In my third year, I joined the mathematics department. The mathematics department at the University of Tokyo had a long-standing tradition, though it has relaxed somewhat. During that year, all the fundamental subjects were taught in a condensed format over one year...

I In one year?!

K Yes, some fundamental subjects like complex analysis, Lebesgue integral, Galois theory, commutative algebra, manifold theory, topology, partial differential equations, and others were covered in just one year. In their senior year, students would choose a professor and begin presenting at weekly seminars based on specialized books or papers in a specific field. When I was a student, there were only a few courses available for senior undergraduates and graduate students alike, though there were a few very advanced lectures for graduate students and specialists. It seemed that professors expected students to have already learned everything by the time they reached their senior year. Of course, this wasn't actually the case for most students, but the atmosphere encouraged students to bridge the gaps by learning mostly on their own rather than through lectures. This freedom in the senior year was in sharp contrast to the junior year, which was very intensive with dense coursework. I think this was simply the tradition at that time.

I Is this still so today?

K No. A few years after I joined the faculty in our department, there was a significant change. We increased the graduate program enrollment by more than three times, which resulted in a significant easing of the education system, adopting a more American-style approach. We now teach at a slower pace and offer many specialized courses for fourth-year undergraduates and master's students.

I How did you decide that you want to become a mathematician?

K Throughout high school, I had many interests. I liked mathematics, but I was also passionate about other activities, including sports. At 18, I wasn't very committed to mathematics. However, when I started university, I wanted to focus on a specific area rather than spreading myself too thin across many interests. Since I wasn't very good at experimental subjects like chemistry, I thought of focusing on theoretical ones. I enjoyed mathematics and physics, and so I started to study those on my own. I gradually found myself becoming more deeply engaged in this field…

I Why did you choose representation theory in particular, among other areas of mathematics?

K At that time, senior undergraduates were expected to choose a direction of research, such as algebraic geometry or partial differential equations. However, I wasn't ready to commit to a specific subject, because I wanted to learn more. And in the theory of Lie groups and representations, I felt that an algebraic approach is possible, a geometric approach is possible, and an analytic approach is also possible. So, I thought this field would allow me to delay choosing a specific field of focus. It was a way…

I It allowed you to learn more!

K Yes! And I kept my options open while I explored the subject further.

I So, now you were in your fourth year as an undergraduate, and you wanted to learn more by going to representation theory. What happened subsequently? How did you do research and discover the problems that you wanted to work on?

K Okay. I focused on analysis during my senior year. I had some background in differential geometry, several complex variables, and Lie theory, which I had studied independently during my junior year in addition to my coursework. In my senior year, I had much more free time and thoroughly read the book by Gelfand and his collaborators.

I *"Generalized functions"?*

K Yes. I focused on the fifth volume of the book *Generalized Functions*. It took me a year to thoroughly read this volume and to study the contents of the previous four volumes. The volume was not always rigorously detailed and made bold use of ideas that could not always be fully justified by the mathematics available at the time of its writing. However, I was enthusiastic about the book, which was filled with Gelfand's unique insights and innovative ideas. I spent much of my senior year enjoying the process of trying to justify the arguments and developing further generalizations of Gelfand's work independently, incorporating my own ideas. The following year, my professor advised me that after studying analysis, I should explore algebra. Following his recommendation, I began studying a new area of algebraic representation theory, particularly the work led by Vogan, which was emerging at that time.

I That would be during your Master's program?

K Yes. This was during my first year of the Master's program. While learning the existing theories in mathematics, I also wanted to do something new, exploring problems that had never been studied successfully before. I learned that an engineer had posed a question to mathematicians about the shape of plasma. While it seemed to be plausible that it could be spherical, the question was whether this is indeed the case and why. This is a free-boundary problem in differential equations. Coincidentally, my seminar was canceled due to a university event, which gave me extra time to focus on this problem. At that time, I gave a seminar talk every week for three to four hours…

I As a student?!

K Yes, I was a student at that time. I needed a lot of time to prepare because my weekly seminar on algebraic representation theory was attended by experts and professors from various universities. However, that week I was completely free! I took the opportunity to explore the analytic question and discovered some interesting solutions.

I Wow!

K I tried to reformulate this question to explore whether one can recover the original shape from the zero set of the Fourier transform of the characteristic function. The original question is equivalent to a specific case of this formulation. I found this approach intriguing and studied it further. During that week, I discovered many interesting things. Afterward, however, I returned to the daily life in algebraic theory…

I So, it was done in one week?

K Yes, I was fortunate to prove something interesting that week. I revisited the problem and worked on it further during the summer break. This work, resulting in about 100 pages long, became my first published work. Although this paper wasn't related to representation theory — which was my primary focus at the time — I felt it was stimulating to take on challenges within that field.

So, at the end of my second year in the Master's program, I concentrated on representation theory. At that time, there had been significant progress in understanding irreducible representations of real reductive groups under some regularity assumptions of parameters, thanks to a powerful algebraic approach developed by Zuckerman, Vogan, Wallach, Beilinson, Bernstein, and others, which also addressed the unitarizability problem. However, I had an intuition that the unitary structure might be better understood through an analytic framework, so I began exploring singular unitary representations in the L^2-space on indefinite Stiefel manifolds. This work became my second paper; a part of the results was later published as a monograph in the Memoirs of the American Mathematical Society (AMS), and it was on this topic.

I And these two papers became your Master's thesis?

K Yes.

I You were appointed as a tenured assistant professor at Tōdai right after your Masters. How did that system work? I mean, I would imagine that most universities would require at least a PhD for the appointment?

K This was due to the age limit rule for obtaining PhD at that time.

I So, yours was already recognized as a very high standard Master's thesis, but because of the regulation, they couldn't…

K At that time, academic degrees were not heavily weighted in hiring decisions. In fact, some Master's theses from our department were comparable to, or even stronger than, PhD theses.

I I see. Very interesting!

K In a sense, I was working in two directions: one was in representation theory and the other was real analysis.

I Did you subsequently go back to the real analysis problem? Even though your main interest was still in representation theory…

K Occasionally, yes. I enjoy working on such problems and often revisit them. For example, if we have a large collection of low-resolution images, we might be able to reconstruct the shape. This idea is related to the previous problem and to integral geometry. It seems quite elementary and highlights the unity of mathematics. Another recent example is the (k,a)-deformation theory of the Fourier transform. These are the kinds of questions I enjoy.

I I see. You stayed at Tōdai for quite some time, and then subsequently you left Tōdai to join RIMS (Research Institute for Mathematical Sciences) in 2001. And then later on you returned to Tōdai in 2007. What prompted the moves?

K This is also tied to how I became a mathematician. I have never actively applied for a job myself; I was just invited to work somewhere and accepted those invitations. When I finished my Master's, the dean told me that I was appointed to work at Tōdai and that I did not need to enroll in the PhD program. I didn't have a choice in the matter. After working for about twelve years, RIMS in Kyoto invited me to join their institute, and this time, they did so in a more formal manner.

I But when RIMS asked you, didn't Tōdai object?

K Yes, Tōdai strongly objected to the move. From my perspective, I felt honored and excited to be invited to work at RIMS because I hold mathematicians there, such as Sato, Kashiwara, Kawai, Miwa, Jimbo, Saito, Mori, and others, in the highest regard. However, I was somewhat reluctant to move from Tokyo to Kyoto, so I initially declined their offer. If I recall correctly, RIMS invited me again about six months later. This time, every Wednesday afternoon, professors from RIMS would call me for two to three hours…

I They were quite persistent….

K For two or three months, every week they asked me, "Why are you reluctant to move to RIMS?" and how they could help with each of my concerns. In the end, I found it impossible to decline!

I Wow!

K And then I decided to move. Also, Kyoto and my birthplace Osaka are not very far apart, and so I thought this was also a good thing…

I But then what made you move back to Tōdai?

K Shortly after moving to Kyoto, Tōdai offered me a position to return. Rather than calling, they came to Kyoto in person to discuss it. They were very kind but applied considerable pressure. It was only natural that I couldn't agree to move back right away since I had just arrived. As I continued working at RIMS year by year, I grew to love the research environment and people there. I became reluctant to move back to Tokyo. However, Tōdai persisted in urging me to return, and RIMS wanted to keep me. I liked and valued both places highly, and it was a difficult decision. At RIMS, I had no teaching duties and could focus solely on my research. The secretaries are highly competent and supportive, even when inviting researchers from overseas, allowing professors to focus solely on mathematics. Additionally, Kyoto, with its cultural heritage and natural beauty — surrounded by mountains and steeped in tradition — was a place I deeply appreciated. On the other hand, people in Tokyo argued that you have a mission to contribute to educating the next generation, given the many talented students at Tōdai. Although it was a tough choice, I am not good at saying no, and eventually agreed to return to Tōdai after a fulfilling and beautiful six years at RIMS.

I Very interesting! In between, you have also visited many places worldwide. How do you find the mathematical environments at the foreign institutions and universities compared to Tokyo and RIMS?

K As a visitor, I am often impressed by the efficiency and support provided by the secretaries at institutes abroad. Their assistance allows me to concentrate on my research and enjoy fruitful discussions with my collaborators. Different environments overseas have shaped my thinking in various ways, and I've found great value in this variety — whether navigating harsh winters or engaging in lively discussions with researchers. I have many reflections on this, but let me begin by discussing my experiences teaching graduate courses.

For the topics of my graduate courses at Tōdai, I usually focus on some aspects derived from my own theories; however, in Japan, graduate students are often too polite to ask questions during lectures. Teaching in the United States, which included two terms at Harvard University (in 2001 and 2008) and one at Yale University more recently, presented a markedly different atmosphere. There, graduate students and postdocs actively posed a wide range of questions during lectures, sometimes about unresolved topics, which proved highly stimulating for both myself and the students.

Natural environments also influence my thinking. I spent one year at the Mittag-Leffler Institute in Sweden. Amidst the cold and dark winter, I had ample time for research. Additionally, the lively social interactions that brightened the dark nights, something I had never experienced in Japan, occasionally seemed to give me an extra boost of energy.

I Would you describe yourself more as an urban person — living in a big city like Tokyo? Or do you prefer more of nature — a rural environment? Which do you find more conducive for your research work?

K I think this depends on age, at least in my case. In my 20s, I considered myself an urban person. When I spent one year at the Institute for Advanced Study in Princeton, which had a wonderful natural setting, I occasionally missed the hustle and bustle of city life, so I would visit Manhattan every two weeks on Saturday afternoons to be around people. But now, after living in Kyoto and frequently staying at IHES, I feel quite comfortable in a semi-urban environment. So now, perhaps I'm not so much of a purely urban person anymore.

I Over the years you have had many PhD students and postdocs, some of whom are also joint authors with you. Can you share with us with your approach to supervising students? How do you groom them to become mathematicians on their own?

K I tailor my approach to supervising students according to their individual personalities and abilities. Students grow and change every year. Initially, I observe their presentations during my weekly seminars for senior undergraduates and master's students. I offer different guidance depending on the student. For graduate students, the first major hurdle is writing their master's thesis.

For exceptionally strong students, I encourage them to prioritize broad learning rather than immediately dive into writing papers. While they can certainly do that, I emphasize that exceptionally strong students should also consider long-term career prospects beyond their PhD. For generally capable students, I would suggest tackling concrete, unsolved problems in emerging fields during the second year of their Master's program so that they will be self-confident and motivated. This also helps them learn through problem-solving.

I typically recommend customizing these challenges based on their preferences, personalities, and backgrounds. I recommend exploring diverse research areas each year, such as geometric, purely analytic, and occasionally algebraic theory or combinatorics. Since my students share an office, they have the opportunity to teach each other. When research fields vary, younger students may end up teaching senior ones, which fosters more discussions among them. Diversity also helps prevent unproductive competition among students. This is how I have tried to foster a collaborative atmosphere in my research groups. While I provide substantial research advice, writing joint papers with my students is rare. Such collaborations only occur when I believe they will genuinely benefit the students.

I You want them to work out results on their own?

K Yes, yes, I encourage them at least try to. Students may not immediately grasp what I explain, but they might understand it the following year. I try to be patient and wait. The process involves both learning and research. When this combination is effective, students can significantly develop themselves.

I Since 2011, you have held a joint appointment at Kavli IPMU. And you continue to give graduate courses at Tōdai. You are also an editor for many journals and you have served as a member in numerous committees. How do you balance your time between doing research, supervising students, teaching courses, and academic service?

K I don't think I manage my time very well. For instance, I often spend nearly 10 hours preparing lectures, even for first-year undergraduate courses, to include explanations of original observations.

I But that must take up a lot of time!

K Yes, it does. However, I would be happy if I could present some original findings to students and then share that joy and excitement with them. In a way, I'm not very good at managing my time, and sometimes I spend too much time on various tasks. But I may work long hours every day, often seven days a week, because of the joy it brings me, so…

I You don't take a break over the weekend?

K Usually not; I just continue working. Of course, if I want a break, I take one freely. However, some tasks ease me in doing other activities. This is somewhat like how athletes do lighter exercises to cool down after intense training, which may aid in recovery. Some tasks are proportional to the time spent, but research in mathematics is different; it's not always directly proportional to time. For me, mental freshness, patience, and creative thinking often depend on the time of day. So, I try to organize my schedule to match different tasks with the best times of day. Some tasks are better handled in the early morning, while others are more suited for late at night…

I Let me see. Your research ideas — when you think about problems, do you tend to do that in the early morning?

K Not necessarily. However, I often use the quiet of the morning to rigorously develop my nascent ideas. Tackling problems that seem insurmountable is complex and nuanced, and requires focused, uninterrupted time…

I Uninterrupted time?

K Yes, in a sense. Continuous periods of time are crucial in mathematics research. For me, discoveries are not always made during these uninterrupted periods but often arise spontaneously after many months of sustained concentration. Nevertheless, long-term uninterrupted concentration is essential for achieving breakthroughs.

I You need the seed…

K Yes! I believe the seed must be somewhere, and it will be discovered and nurtured through sustained and uninterrupted creative efforts.

I Well, since we've talked about research and teaching, maybe we can talk a bit about representation theory, which is your area of research. What attracted you to it?

K I prefer challenging, emerging research fields where nobody knows the appropriate methods yet, rather than solving existing problems by refining sophisticated techniques. Needless to say, I am fully aware that even with an immensely extended time span, some ambitions may remain unattainable, and such pursuits carry a low probability of success. Nevertheless, I always strive to introduce novel concepts and methods based on a deeper understanding of the core principles, rather than merely relying on and developing existing methods. I believe in the unity of mathematics.

The 'theory of symmetry' serves as a meeting point for various branches of mathematics. I find that representation theory of real reductive groups has good potential for this. When aiming for the summit of a mountain, one can ascend by walking, driving, or even by helicopter. Similarly, there are various approaches to pursuing a beautiful theory of infinite-dimensional representations. Sometimes I employ differential equations, other times geometric concepts, and occasionally purely algebraic methods to reach my goals. Such diversity inspires me greatly, as I believe there is significant potential for discovering new methods.

I And you find that when you have a different method to prove the same results, you understand the whole landscape better?

K Ideally, yes, however, often I can't even see a single summit. I'm just forging ahead and climbing relentlessly! It's only when I'm fortunate enough to find a good path that I get a partial view of the landscape. This is already a very exciting moment. Understanding the whole landscape better would be great, but that typically comes much later, often several years after an initial breakthrough.

I Representation theory has a sort of reputation or notoriety as being very hard, requiring a lot of background and having a very steep learning curve. Some people may even say that one has to study the collected works of Harish-Chandra in order to get into the subject. Do you find this to be the case?

K No, not exactly. I understand that point. In practice, most researchers in representation theory focus on advancing and broadening the theory, building on their extensive knowledge and pushing its boundaries. While I do engage with this aspect quite a bit, I devote much more energy to exploring the possibility of opening up new areas that are deeply connected with symmetry and other branches of mathematics. If this symmetry is linearized, it relates to representation theory. I believe representation theory has the potential to encompass many profound and emerging areas that are not overly technical. Looking back to when Harish-Chandra created his theory, the field was still in its early stages. I imagine even Harish-Chandra himself didn't know exactly where his work would lead when he first started developing it.

I So there are areas in representation theory that you find are accessible without that big requirement of background?

K Yes, I believe that new concepts can be discovered from different perspectives, and I strive to uncover or create them. Often, new concepts in mathematics are first observed in highly symmetrical cases and then developed independently of symmetry. Representation theory remains a consistently fertile ground for emerging mathematical ideas.

I And all these tight connections appeal to you…

K Yes, this is one of the most fascinating aspects of the theory for me — how representation theory can drive advances in other branches of mathematics, and vice versa. While one can't study everything, one can still appreciate these connections. That's why I'm drawn to this topic.

I You gave your ICM talk on the branching problems for unitary representations, which was a summary of your trilogy of very influential papers on discrete decomposability. Can you outline the problem of the branching laws and the key challenges that one faces in this subject?

K Let me see … Consider the Greek philosophical concepts of *analysis and synthesis*. According to this philosophy, if one wants to understand something, then one decomposes it into the smallest pieces, such as molecules, atoms or elementary particles, and then tries to reconstruct. This is what we mean by *analysis and synthesis*. So, what are the smallest objects in symmetry? If these objects are linear, then the smallest ones are usually considered to be irreducible representations. However, things are not so simple. For example, if we think about the smallest element, then some might consider that the molecule is the smallest, while others might argue that the atom is the smallest. This variability illustrates that the concept of 'smallest' depends on your viewpoint. Thus, an irreducible representation is not necessarily the smallest if we change the viewpoint.

So, if we consider a specific symmetry — such as one described by a group action — a certain space might be considered the smallest from the perspective of this group. However, if we consider a different group, such as a subgroup, what was previously the smallest may no longer be so. The decomposition of this space may be complex and difficult to trace. Sometimes, however, we can gain some control. For example, white noise, which is unpleasant to hear, corresponds to a continuous spectrum, while clear and harmonious music corresponds to a discrete spectrum.

I I like the analogy!

K Thank you! Such symmetry of continuous groups is usually formulated in terms of Lie groups. Among Lie groups, the fundamental objects are often considered to be simple Lie groups, or more generally, reductive Lie groups. Irreducible representations of these reductive groups are considered fundamental, and they are typically infinite-dimensional.

A change of viewpoint is achieved by choosing a subgroup, leading to *symmetry-breaking*. From this new perspective, the original representation is no longer the smallest object and decomposes into yet another set of smallest objects, namely irreducible representations of the subgroup. This decomposition is known as *branching laws*. Finding explicit branching laws is very challenging. In the 1980s, the theory of symmetry-breaking or branching laws had not yet been fully developed, partly due to the difficulties of dealing with continuous spectra that could arise with infinite multiplicity.

However, I was fortunate to discover a remarkable phenomenon around 1987, where the decomposition avoided these continuous spectra, revealing a beautiful structure.

I Was it a specific example?

K Yes, it is a very specific example involving a six-dimensional non-compact Riemannian manifold with a three-dimensional complex structure and an indefinite Kähler metric. My method combines geometric ideas with representation theory. The result can be described in terms of the discrete spectra of two geometrically defined, commuting differential operators. This phenomenon was novel both from the perspective of global analysis and representation theory. I aimed to generalize it using differential equations rather than representation theory. I explored this intermittently over two to three years but was unable to find a fruitful direction.

I then decided to approach the generalization from the perspective of representation theory, specifically focusing on the branching problems of symmetry-breaking. This led to the three papers that revealed the discrete decomposability of branching laws: the first paper uses a geometric approach, particularly complex geometry; the second employs an analytic approach, specifically microlocal analysis; and the third explores the algebraic aspects of the theory.

I So, from the representation theory point of view, you have a characterization of the kinds of irreducible representations, the kinds of smallest objects of the big group that will give you only "beautiful music" (when restricted to the subgroup)?

K Yes, exactly. What I've discovered helps us avoid continuous spectra in symmetry-breaking, or, to use an analogy, it helps us achieve a state that is free from "white noise." This discovery led us to three main avenues of research.

Once we identify the smallest objects, we might typically stop, but further decomposition from a new angle can offer additional insights. So the first direction is to explore these smallest objects through further decomposition. You can think of this direction like understanding a substance by looking at its molecular structure. In representation theory, this approach is to study irreducible representations using branching laws. This method is especially simple when continuous spectra are not involved.

The second direction is to attempt to find the smallest objects by breaking down something we already understand well. An analogy would be listing atoms by breaking down molecules. In representation theory, many interesting families of irreducible representations are obtained by decomposing well-understood representations, such as the Segal-Shale-Weil representation.

The third direction is to understand the decomposition itself. This can be likened to determining the amino acid sequence of a protein or analyzing nucleic acid sequences. In representation theory, this type of decomposition for symmetry-breaking is known as branching laws, which has traditionally been considered intractable. However, if the decomposition does not involve continuous spectra, algebraic methods might help us find these branching laws more effectively.

Originally discovered as an unusual phenomenon in global analysis, its reinterpretation as a new instance of symmetry breaking opened up remarkably fertile research avenues.

I You described this as a sort of symmetry-breaking, which is really a notion imported from physics. And you also mentioned earlier that you studied mathematics and physics together in your undergraduate years. Do the ideas in physics influence you in the way that you discover the key ideas in research?

K Not really. While I am somewhat inspired by physics, I'm not an expert in it. Although I may be indirectly influenced by physics, I have not been successful in applying purely mathematical concepts to physical problems.

I But I think the use of the analogy in physics is very helpful to get a good understanding of the mathematical phenomena that we are trying to describe.

K Yes, I share that view.

I You also pioneered the field of discontinuous groups and their actions on homogeneous spaces. Can you tell us more about that?

K Yes. In '87, just after completing the two papers for my Master's thesis — one on real analysis and one on representation theory — I wanted to explore something new. I came across a paper by Calabi and Markus on Lorentzian geometry. Here is some background. The local-to-global theory has been one of the main topics in Riemannian geometry and has been extensively studied for many years. For example, the Bonnet-Myers theorem states that a complete Riemannian manifold with uniformly positive Ricci curvature is necessarily compact. However, in pseudo-Riemannian geometry, such as Lorentzian spacetime in relativity, very little is known about how local geometric structures influence the global nature of the manifold.

Calabi and Markus discovered a phenomenon that within the new context of Lorentzian geometry, any manifold with positive constant sectional curvature is non-compact. This contrasts with the classical Riemannian case, where such a manifold must be compact. Their result impressed me. Although I'm not good at reading others' proofs line by line, I attempted to give my own proof, as I typically do. Fortunately, I managed to prove the theorem, and my proof turned out to be much stronger and more general. I was new to this field and lacked a mentor, but I was excited and felt that I might do something on my own in this new and unexplored field. So I decided to delve into it further.

I Is "discontinuous groups" just another name for "discrete groups"?

K This is a very good question. In the traditional framework of Riemannian geometry, the terms "discrete groups" and "discontinuous groups" are used interchangeably, as there is no particular distinction between them; this is because any discrete group of isometries acts properly discontinuously.

Beyond this classical Riemannian case, the situation changes drastically. When the metric tensor is not positive definite, as in Lorentzian geometry, a discrete group of isometries does not necessarily act properly discontinuously. Therefore, I proposed using different terms to convey distinct meanings: 'discontinuous' describes a property of the action, while 'discrete' describes a property of the group. In this context, the notion of discontinuous groups for a space is much stronger than merely referring to a group as discrete.

The study of local-to-global properties can be approached in group-theoretic terms when the local geometric structure is modeled on homogeneous spaces of Lie groups. In this context, the concept of a discontinuous group plays a central role in understanding global properties. The Calabi-Markus phenomenon — a striking result proven in 1962 — states that there are no interesting discontinuous groups. Specifically, a discrete group of isometries acting on de Sitter space is necessarily a finite group. From this group-theoretical perspective, their result was initially considered a sort of 'no-go theorem', making it less enticing for further exploration in this context.

At the time I began exploring the new theory of discontinuous groups, there was little interest in this problem. However, with plenty of time on my hands, I decided to delve into it. I discovered a characterization of discontinuous groups that reveals many intriguing examples beyond the classical Riemannian framework and across various geometries. I found this quite fascinating, even though I felt somewhat isolated. Some years later, several mathematicians from abroad, including Yves Benoist, reached out to me and asked me questions about my work. I was no more alone in this new area!

I In 2003, you published, together with Ørsted, another trilogy of papers on minimal representations of $O(p,q)$. Tell us how you were led from branching laws and discontinuous groups to minimal representations.

K Our theory of minimal representations may appear unrelated to branching problems and discontinuous groups; however, at a personal level — and highlighting the unity of mathematics — these new areas served as the inspiration for our theory. The collaboration with Bent Ørsted, which began in 1990, emerged from the intersection of two entirely different fields: symmetry breaking and conformal geometry. We had many evolving ideas and didn't want to confine ourselves to a single goal. We simply enjoyed making various new discoveries each year. Aside from a brief announcement, we did not publish anything for over a decade.

Our first breakthrough was discovering a general framework for constructing canonical representations for every conformal manifold using the Yamabe operator. The next step was to analyze and understand these representations. Around that time, I was developing a general theory of symmetry-breaking, focusing on discretely decomposable restrictions. Although it began with a concrete example, the theory evolved in a highly abstract direction.

On the other hand, for any Riemannian manifold — or more generally, for any pseudo-Riemannian manifold — there are two natural types of symmetries: conformal transformations, which preserve angles, and isometric transformations, which preserve lengths. This geometric framework introduces a problem of symmetry breaking in function spaces, specifically from conformal groups to isometry groups. This provided an intriguing geometric test case for analyzing branching laws related to symmetry breaking. Conversely, the branching laws from conformal groups to isometry groups proved useful in exploring the conformal representations we had constructed. Specifically, we demonstrated that if a manifold is conformally flat, the resulting representation realized in the function space of this manifold is *minimal* — this term is from algebraic representation theory — loosely speaking, the smallest infinite-dimensional unitary representation of the conformal group, which is a simple Lie group of type *D*. This discovery opened the door to a geometric construction of the minimal representation of reductive groups, analogous to the classical Segal-Shale-Weil representation of the split simple Lie group of type *C*.

I The function space you're referring to here is the L^2 space of the geometry?

K Yes and no. Our original construction of conformal representations is realized in the solution space of the Yamabe operator, and it has a canonical Hilbert space structure given by a new conservative quantity associated with the Yamabe differential operator; this Hilbert structure is not derived from the L^2-norm associated with the geometry. However, this solution space is mapped into the distribution space via the Fourier transform. We discovered that the image is precisely the L^2 space of the characteristic variety, which generalizes the classical Schrödinger model of the Segal-Shale-Weil representation. In this way, we broadened the horizon of the minimal representation through these two geometric realizations.

Around that time, another approach to realizing minimal representations emerged, such as Chengbo Zhu's work from NUS, which employed the theta lift. This represented an encounter with different perspectives: our approach emphasized geometric aspects (angles and lengths), while the theta lift was a more representation-theoretic method.

This is how the joint work with Ørsted began. At that time, I was developing the theory of symmetry breaking in reductive groups in a rather abstract direction. In contrast, the analytical study of minimal representations is a young field that does not require an extensive background in representation theory. Therefore, I strive to provide proofs in the most elementary and concrete way possible, although I occasionally use advanced concepts from representation theory to explore promising directions. I do this not only for pedagogical reasons but also to facilitate connections with other fields beyond representation theory.

I I see that you like to explore the same question from many different angles….

K Yes! I enjoy delving into a new field from various perspectives, as deeply as possible. Minimal representations are particularly well-suited to this exploration. From a representation-theoretic viewpoint, minimal representations are merely a specific, finite subset of the unitary dual. However, what I find more interesting is how various mathematical disciplines intersect with the study of minimal representations in unexpected ways, as is illustrated by the classical theory of the Segal-Shale-Weil representation and our joint work with Bent Ørsted and other collaborators.

The shift in perspective — from groups to function spaces in geometry — allows us to see 'minimal' representations in a new light, as 'maximal' symmetries in function spaces. I propose 'analysis with maximal symmetries' as a new research area inspired by motifs emerging from minimal representations, which I believe has the potential to bridge diverse mathematical disciplines beyond representation theory. A recent example includes the development of the (k,a)-deformation theory of the Fourier transform.

I Are there other topics in representation theory that you would like to add on to? And can you tell us what you're working on currently?

K Some time ago, I began exploring a fundamental question: how and to what extent can representation theory be useful in global analysis? Unlike local analysis, global analysis often requires certain assumptions to develop a meaningful theory in the non-compact case. These assumptions might be formulated geometrically, group-theoretically, or in other ways. The group-theoretic approach, known as non-commutative harmonic analysis, seeks to extend classical analysis on Euclidean space $\mathbf{R}^n$ to global analysis by leveraging the representation theory of non-commutative Lie groups. Over several decades, there have been successful and profound theories developed in this direction, such as analysis on semisimple groups by Gelfand and Harish-Chandra and analysis on symmetric spaces by many researchers. However, progress in extending these theories further has been limited. Each approach relies on technical structural results about the spaces.

Rather than focusing on the challenges of generalizing these techniques, I am interested in the fundamental question: what symmetries of the space allow for a fruitful theory of global analysis? This is not yet fully formalized mathematically — though rigor is essential. I felt merely having the transitivity of the group action would not be sufficient.

I Is the issue about controlling the behavior at infinity?

K Control at infinity is part of the requirement; without it, easy counterexamples can arise in the non-compact setting. In group-theoretical terms, transitivity can offer control at infinity, but a different requirement arises. Let's consider some basic questions: Why does the Taylor expansion work? Why does the Fourier series expansion work? There are several reasons. One aspect is uniqueness of coefficients in the expansion. For example, in the Taylor expansion, we write $a_0+a_1x+a_2x^2+\ldots$, where we need only one coefficient for x^2, and only one coefficient a_3 for x^3. However, if we needed seven numbers for x^3 and eight numbers for x^4 for example, and if the coefficients were not unique, the expansion would be less useful. Thus, uniqueness of the coefficients is crucial for the effectiveness of the expansion.

Similarly, in representation theory, while the abstract framework ensures the existence of expansions in a general setting, it may not be sufficient for applications in global analysis due to possible multiplicities of irreducible representations. We can distinguish between different irreducible representations but distinguishing between multiple occurrences of the same irreducible representation, including potentially infinitely many, is fundamentally impossible without some additional structure. From my perspective, this limitation is crucial in determining the applicability of representation theory in global analysis.

I So, it's more like a multiplicity one question?

K Yes, that's essentially how I formulated the applicability of representation theory to global analysis in terms of multiplicities. The multiplicity-one case would be ideal, but it is too restrictive and thus excludes some natural spaces. Instead, I propose focusing on two conditions: one where multiplicities are uniformly bounded and another that is more relaxed, where multiplicities are individually finite.

I To characterize the situations when you have uniformly bounded multiplicities?

K Yes. I aimed to find geometric characterizations of these conditions for global analysis.

I So, you begin with this question in mind, and then you try to understand the situation better, and that leads you to discover theorems …

K In my research, I often collect many problematic situations or counterexamples while exploring ideas freely and ambitiously. This process, which can span several years, serves as a maturation period for developing new theories. Gathering these adverse scenarios often leads me to identify a good formulation to develop the general theory, especially when I am fortunate enough to find a particularly promising case.

Returning to the fundamental question of multiplicities, I had previously come across many extreme examples with infinite multiplicities before formulating the problem. These experiences guided me in pinpointing the right question and formulation, enabling me to focus on promising settings. Subsequently, I was able to characterize both finite and uniformly bounded cases in global analysis using the concepts of sphericity and real sphericity, respectively. This framework also applies to the related problem of symmetry breaking, opening up new avenues for exploring both global analysis and symmetry-breaking in greater detail.

Currently, my research is directed towards two areas with my collaborators: first, refining the study of symmetry-breaking within this framework by introducing the concepts of symmetry-breaking operators and holographic operators. Second, exploring global analysis beyond this framework from a completely new perspective. The recent theory of tempered homogeneous spaces, developed in collaboration with Benoist, aligns with this direction, particularly in cases involving infinite multiplicities.

There is yet another theme I am currently working on: spectral analysis on locally homogeneous spaces, extending beyond the traditional Riemannian setting. This has been a long-standing motif in my research. In my twenties, I encountered new geometry by discovering rich families of discontinuous groups beyond the Riemannian context. Just as one might want to play music if given a musical instrument, I wished to 'listen to the sound' of this new geometry through spectral analysis.

For a musical instrument, different pitches are produced by altering the length or thickness of a string, with a shorter string producing a higher pitch — an idea familiar from traditional Riemannian geometry. In our new geometry, there is also a concept of deformation, specifically explored through higher Teichmüller theory for discontinuous groups. However, I have discovered an intriguing phenomenon where some sounds remain stable under deformation. Together with my collaborator, Fanny Kassel, we are investigating this spectral theory using both geometric methods and representation theory, where we have found that the theory of symmetry-breaking for infinite-dimensional representations offers a novel approach to this geometric problem.

I Well, we should wrap up the interview; I wouldn't want to take up too much of your time. But before we end, what advice would you give to a beginning graduate student today who wants to work on representation theory?

K This is not an easy question to answer. Besides the three commonly recognized pillars of mathematics —algebra, analysis, and geometry — there is a fourth pillar: Lie theory, which describes the fundamental laws of symmetry. Lie theory integrates algebraic, analytic, and geometric methods, creating a unified field of study. Representation theory is a linearized aspect of this field.

In my opinion, many types of researchers can be successful in representation theory. You don't need to know everything; having a single strong area of expertise can greatly enhance your research, even if you're not familiar with existing techniques in representation theory. Some researchers excel in analysis, others are strong in algebra, some have a keen intuition in geometry, and others are drawn to very abstract mathematics or prefer concrete computations and combinatorics. All of these approaches contribute crucially to representation theory.

I believe it's not necessary to follow current trends or popular topics; what's more important is to bring your own strengths — such as your specialized skills, knowledge, or expertise — into the field of representation theory. While certain subjects may be very popular now, this can change in ten years. Instead of chasing trends, it is more rewarding to leverage your own areas of expertise to make original contributions to representation theory. I feel that representation theory can accommodate everyone's unique contributions in some way.

I Well, okay. Thank you very much for your time! I really appreciate it, and I think I've learned a lot from your stories. The way that you have done your mathematics is really inspiring. Thank you very much.

K Thank you!

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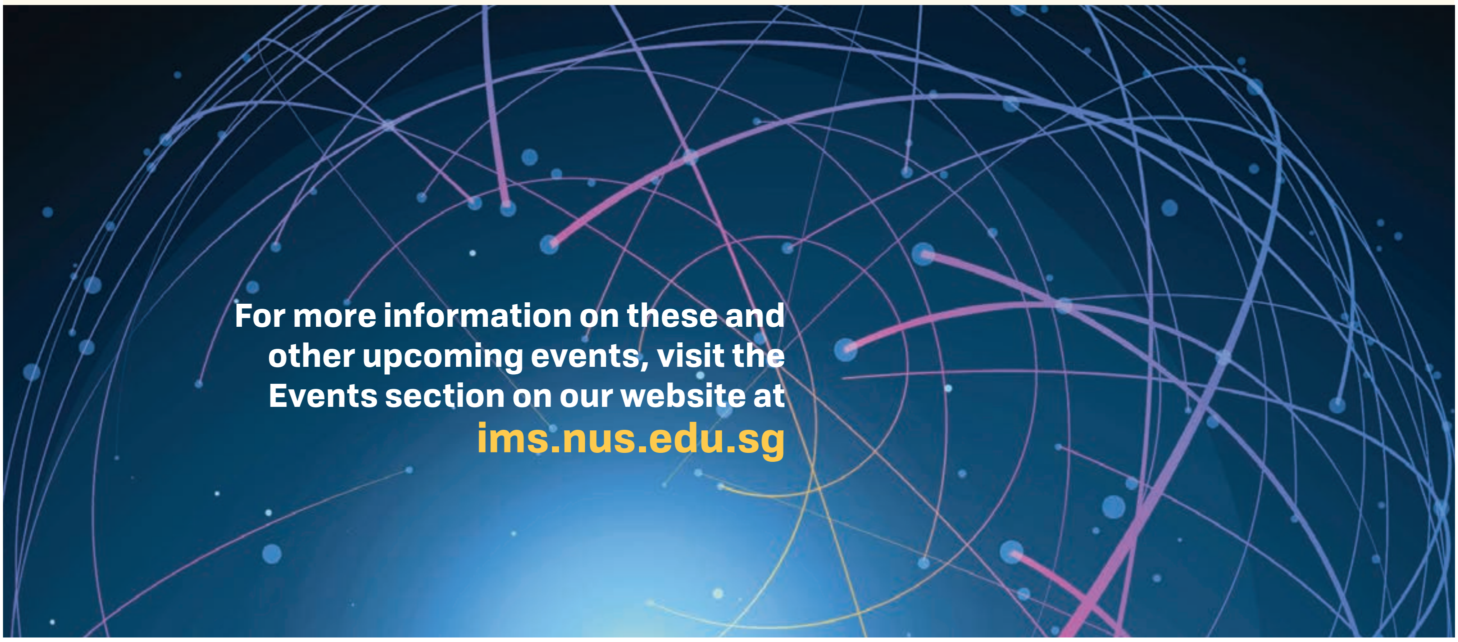

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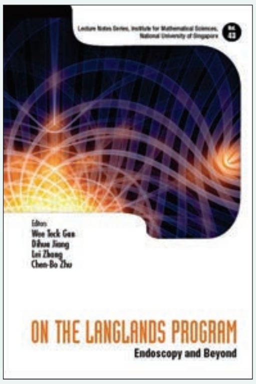

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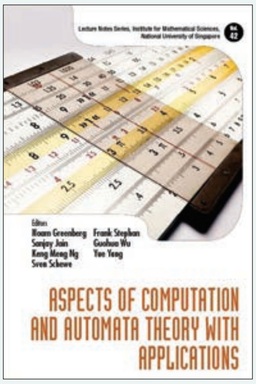

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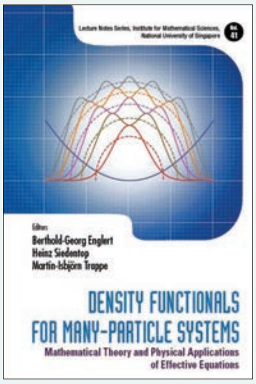

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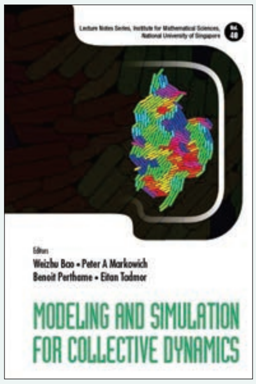

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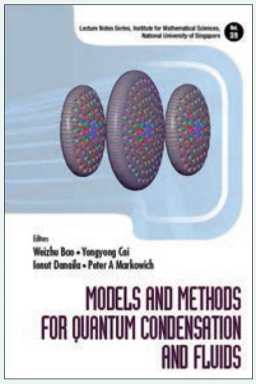

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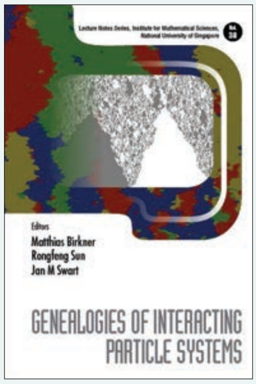

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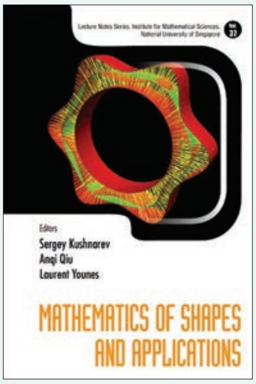

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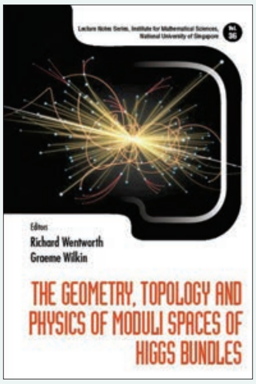

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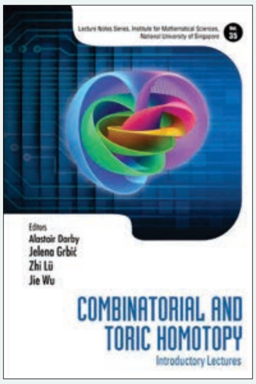

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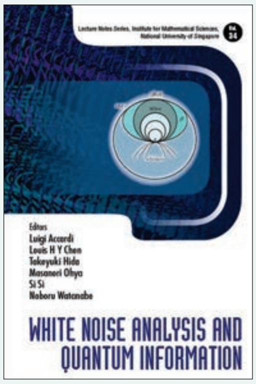

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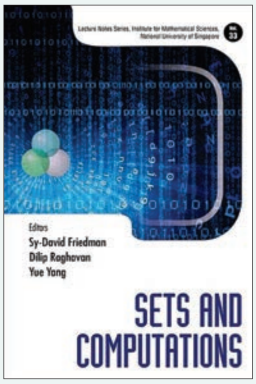

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