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SPECIAL INTERVIEW

by Chee Whye CHIN

AN INTERVIEW WITH CHONG CHI TAT

CHONG Chi Tat is University Professor at National University of Singapore (NUS) and former director of the Institute for Mathematical Sciences (IMS). His mathematical research in the area of logic has resulted in important contributions to recursion theory and reverse mathematics. A central problem that he has worked on concerns the exact proof-theoretic strength of the infinite form of Ramsey's theorem. He and his collaborators Theodore SLAMAN and YANG Yue introduced the novel idea of looking at nonstandard models of Peano arithmetic for the study of such questions, and through a decade-long collaborative work (*J. Amer. Math. Soc.* 27 (2014)), they constructed a nonstandard model of first-order arithmetic with carefully chosen properties to show that Ramsey's theorem for pairs (RT^2_2) is separated from Stable Ramsey's theorem for pairs (SRT^2_2), thereby resolving important questions first raised by CHOLAK, JOCKUSCH and SLAMAN in 2001.

Chi Tat obtained his BSc (with distinction) from Iowa State University and his PhD from Yale University. He joined the University of Singapore (predecessor of NUS today) as a lecturer in 1974 and became full professor in 1989. He was conferred the title of University Professor in 2004. Throughout his long and illustrious career in NUS, he has served in many leadership positions, beginning with being appointed as a Vice Dean of the

Faculty of Science in 1985 and then concurrently in 1993 as the Head of the Department of Information Systems and Computer Science (predecessor of the School of Computing today). He took up senior leadership appointments in the university's administration first in 1996 as Deputy Vice-Chancellor, then continuing as Provost and Deputy President in 2000 following the restructuring of the university. He returned to the Department of Mathematics in 2004 and subsequently served as its Head for two terms. He was the Director of the IMS from early 2013 through June 2023, and retired in late 2024.

Chi Tat's transformative work as Director of the IMS has already been detailed in the July-December 2023 Imprints article "Leaving a Legacy: Professor Chi Tat Chong's Tenure at IMS" by Alexandre THIERY. His numerous contributions to NUS, the Faculty of Science and the Department of Mathematics are equally far-sighted, long-lasting and deeply influential; it will not do them justice to simply list them here one by one. Instead, Imprints took the opportunity of Chi Tat's successful completion of directorship at the IMS to engage him in a 3-hour long interview on 11 December 2023, to hear directly from him his reflections on 50 years of multi-faceted work. The following is an edited and vetted transcript of the interview with him by CHIN Chee Whye on behalf of Imprints, together with SHEN Zuowei and YANG Yue.

IMPRINTS I Let's start chronologically from your younger days. Can you tell us how you got interested in mathematics? In your growing up years, were there mentors or teachers or parents who influenced you?

CHONG C When I was young, my ambition was to be a car designer. I was really interested in cars, but when I reached my early teens, I realized that in Singapore, to be a car designer was just something that's not workable. Anyway, I cannot say that mathematics already caught my full interest at that time. As happened to many of us, we were doing well in mathematics in school, and some of us would just challenge one another with problems. That's what I did, but that was about it. My interest was still very much in car design. But then I was also interested in several other things: Chinese calligraphy — I was spending quite a bit of time on that; Chinese painting, history, and so on. I was even involved in writing some Kungfu-fighting novels during the time I was in high school; two or three of us fellow students would be writing, and then we would show it to one another. Those were the days.

My parents actually started to send the children overseas as soon as our high school days were over. So, as I was finishing my O-levels, my parents thought, well, maybe I should go overseas. My brother at that time was studying at Iowa State. So it was a natural thing to send me there so that he would take care of me, because I was only around 16 at that time. I have several siblings, and one of them was a chemist — she retired. And she said, "Chit Tat seems to have done well in chemistry (which I did), so maybe he should be a chemist!" But my mother said, "No, he's too much of a dreamer; he cannot be a chemist!" Anyway, I went to Iowa State.

I suppose what actually changed my life was the very first semester that I was there. I had a roommate, an American. To me, he was extremely gifted and extremely well informed. He was also a freshman, as was I, and we talked a lot about mathematics. He was actually the one who introduced me to the world of mathematics — all the possibilities! He had read a lot. Although he was a normal 18- year old freshman, but to me he seemed to know everything in mathematics, because I knew nothing, obviously. And he also read a lot about philosophy. He told me about Bertrand Russell, Alfred North Whitehead, the philosophy of mathematics, the paradoxes in mathematics, and things like that. And then, in the second semester as a freshman, he convinced me to enroll in a course called Math 301. It was actually a junior-level course on point-set topology. Math 301, 302, 303 formed a one-year sequence in the quarter system at Iowa State. And that sort of changed my life, because what happened was that the course on topology was taught by a young assistant professor using the Moore method. I don't know whether you're familiar with that? Robert Lee Moore — he was a professor at the University of Texas; the Moore method became quite famous in those days. The lecturer did not lecture. On day one of our course Math 301, the lecturer came into the class and gave each of the 10 or 15 of us a handout with just a few sheets of paper, and on it was printed Definition 1, Definition 2, Definition 3, Axiom 1, Axiom 2, Axiom 3, and so on, then Theorem 1, Theorem 2, Theorem 3, and so forth, all the way to, I think, theorem 33. Then he said, "Okay, go back and look at this and try to prove the theorems yourself. Honour system. You're not supposed talk to anybody, nor consult any book. Think on your own. Base on the axioms." He called it linear point-set theory. The first few axioms were straightforward: if x_1 is less than x_2 and x_2 is less than x_3, then x_1 is less than x_3 — the usual transitivity property, and so on. And then we started proving something about order. By theorem 10 or 12, we were dealing with the notion of density. At the end of the course, we were supposed to have done something close to the very basic part of real analysis, but no mention about real analysis, just abstract point-sets. And our job was — the following week, we would come back to the class and take turns to present the solutions on the board, and then the class would critique. The professor would say, "Why this? Why that? This is wrong! Go back! Next!" And that happened throughout the semester. Many of us were very brave at the beginning, but by the end of the first semester, of the 10 or 15 of us who started, I think only about 5 were left. My roommate was obviously the star. I think I was doing okay, clearly ranked number two or something like that, above the rest of the students. So, I got really, really excited, and I said to myself, "Oh, this is really what mathematics is all about, and it sounded very challenging, so I should do this Math 302." Eventually, when we finished Math 303, there were only three students left.

So, under the influence of my roommate, I followed him. He introduced me to a basic course in mathematical logic. At that time, it was taught by someone from the department of philosophy who was a student of William Van Orman Quine. Quine was a professor of philosophy at Harvard, a student of Whitehead. In the philosophy of mathematics, Quine is a big name, a legend. Anyway, this young assistant professor in philosophy was a student of Quine. He knew very little about mathematical logic, so he was struggling with the course while he was teaching it. My roommate and I were trying to… I wouldn't say help, but we tried to, you know, complete his explanation of the proof of Gödel's incompleteness theorem, and so on. By the third year of my undergraduate study, I was majoring in mathematics and minoring in philosophy. But I also spent some time in physics. Eventually, I decided that physics was too difficult for me.

I Was it because of the lab experiments?

C No, because I don't understand it! Mathematics seems to me to be something well-structured and logical. One can visualize a geometric object, for example, and try to understand it mathematically, or start with a set of axioms that are intuitively clear, and see them as a description of something geometric, and so on. But physics deals with the physical world, and studies things that to me are hard to grasp… Electrons or Newton's law of gravitation. Somehow from these objects or concepts we come up with equations that describe phenomena in the universe. To me such a connection is mysterious, even if there is experimental data to support. Anyway, I suppose that's a weak part of my brain.

SHEN S But your serious course in topology was maybe also leading you to logic, because the way it was taught was very logical. Usually people don't teach it this way via logical deduction from the axioms…

C Right. I think it did influence my way of thinking about mathematics. And I ended up spending quite a bit of time on philosophy, including writing a minor thesis on the philosophy of mathematics for my Bachelor's degree.

S But to this day, you're still actively involved in the philosophy department!

C Well, I was given a courtesy position there. But I no longer have an office in the department, because I seldom showed up.

I Do you remember the name of this roommate of yours?

C He wanted to be known by his middle name, James. What happened was rather sad. He was big, maybe six feet tall, but a small boy in the sense that he was, shall I say, pure — totally absorbed in his own world: mathematics, philosophy and music. He came from a small town in Iowa, and you know, Iowa is a farm state. I think he's the elder of two children, and his father was a farmer. They didn't know that they had a gifted kid. And I think, psychologically, it was very hard for him to fit into the system. After one year, he suddenly told me one day that he had to move out to another dorm in another part of the campus. I asked him why. He said he's been suffering from nervous breakdown, depression. He's been seeing a doctor and said he would not want to cause trouble to me. So, I did not have a roommate for I think another year until a different person moved in. Later on, although we still attended courses together, we did not get to see each other daily.

He's an interesting guy. For example, he played the first movement of Beethoven's Moonlight sonata beautifully. He also introduced me to classical music. Before that I was interested in the Beatles, although I had two sisters who studied piano at the Juilliard School. Anyway, he introduced me to classical music, and he had a very strange, unusual way of listening to music. Every morning, he would wake up to the Jupiter movement of the Planets by Gustav Holst. Every night before going to bed, he had to listen to the very last section of Romeo and Juliet by Tchaikovsky, a sad and tragic ending. And he did this throughout the year. We went to concerts here and there, on campus, and that's how I got to know and love classical music.

I How did he play them? On a gramophone, I guess?

C Gramophone. He had a one-box player. In those days, it was very common. You put the LP on the platter and then the tonearm on the LP, and the music starts playing. Anyway, when he and I graduated in the same year, I moved east, and he went west to Berkeley. Initially I didn't know where he went, because he just disappeared when the exams were over, and we were already not staying at the same place. And so we didn't communicate with each other for a while. But a year after I went to graduate school, I suddenly received a letter from him. He told me that he was a graduate student in Berkeley, doing a PhD in Set Theory, working under Robert Solovay. And he was really very excited. He said he would send me notes of Solovay's lectures soon, and indeed he did. A week later, I received a stack of cyclostyled lecture notes that he took of Solovay's course. He said, "I didn't know that you're also doing logic; we should communicate more often." We did this for a few more months. Then suddenly one day he said he had a problem again with his mental stability. In another letter, he said, "I have a girlfriend now; I'm very excited; I'm gonna meet her now; I'll write again later." And that was the last I heard from him. I wrote to him, but he never replied. I suspect –– well, okay, this is not confirmed — something happened. Maybe the love affair did not work out. Perhaps something very unfortunate happened. Anyway, I suppose there is a record of him in the Berkeley math department register.

YANG Y This was in the early 1970's?

C He and I graduated in 1969. I went east, and he went to Berkeley in '69. We started writing letters to each other in 1970-71. Anyway, that's how I got into mathematics.

S Maybe he's still around somewhere?

C I don't know. I hope so. If he is, he has probably given up on mathematics and moved on to doing something else. But it's hard for me to imagine him doing anything but mathematics.

Y So, at that time, Iowa State was quite liberal; you could take many courses. What was the system like? Did you have to declare a major?

C Oh yeah, the usual undergraduate thing. You declare a major in the second year. Like most universities, they have broad-based requirements; every undergraduate in science had to take mathematics, of course, physics, biology, and chemistry. Chemistry, I thought I could endure, but biology… I actually disliked biology. But luckily at Iowa State, they have a programme called the honours programme. If you are enrolled in the honours programme, you can design your own undergraduate curriculum, in consultation with a professor, a mentor. So, I enrolled in the honours programme, but before that, I had to talk to the professor. I said I would like to be in the honours programme, and he asked me why. I said, "I like mathematics, I think I also like physics, I can take chemistry, but I cannot stand biology." He said, "That's not a good reason. You can spend more time doing mathematics, but you must take at least one course in biology." So, I had no choice, and one summer, I enrolled in the summer school at Iowa State to take a biology course, Botany 101, and that was the last biology course I took in my life. Somehow, I managed to skip chemistry altogether. Oh, and I also took courses in the social sciences like American government, economics, sociology, even psychology. I also took a few courses in computer science — including Fortran programming and one on compilers. This turned out to be useful as one summer at Yale I worked as a research assistant for a chemistry professor writing computer programmes for him.

I Those were the broad-based university requirements?

C Yeah. It's interesting to take courses about nations and societies. For instance, learning something about the political system in America — of course, America in those days was something that everybody looked up to. It was the shining city on the hill. The US government system was touted by the professor as the model for the rest of the world. It was the time of expansion of the Vietnam war. There were student demonstrations on campus against the war, but no one in the class seemed troubled by it to take issue with the claim.

I Was it during your undergraduate years that you decided to become a mathematician, to make mathematics as a career for yourself?

C Close to that. When I was graduating, mathematics seemed to be the thing that I could do better than others, so I applied to graduate schools to do a PhD.

S Throughout your whole career, you have taken on many special administrative positions. But no matter which position you took on, you were always persistently doing mathematical research. When I first came to NUS, my office was next door to yours. You were the Provost of the university at that time, but you still came during the weekends to do research, and also at the end of each year, you took annual leave to work back at the department. It is beyond mere interest! What drives you?

C Well, I don't know. When I was applying to graduate schools, as usual, I looked at the brochures of graduate schools — they called it catalog in those days. I told my honours programme mentor that I would like to apply to graduate school. He said okay, and he suggested several places I should apply to: Berkeley and other places. But somehow, I decided not to apply to Berkeley, to his disappointment or displeasure. Anyway, I looked at the catalogues of Harvard, Princeton, Yale, Stanford, MIT and I thought I would apply to all of these places. I was rejected by Harvard pretty early; I think they thought that I was not good enough. And Princeton followed suit. But then soon after that, I got admitted by Yale. Now, what attracted me to Yale was that in its departmental brochure, the very first statement, it said that the aim of the mathematics programme at Yale was to train students who intend to make mathematical research their life work. And I thought, wow, that sounds very attractive, something that maybe I should pursue.

So, when Yale's offer came, and I already got rejected by Harvard and Princeton, I called up my brother who was then doing his Ph.D. in applied mathematics at Brown. I said that I got this Yale offer, but I was still waiting to hear from MIT and Stanford. My brother said, "Accept Yale! Why do you want to go to the west? The west is far away." At the time I had a sister whose family was in Pennsylvania as my brother-in-law was an economics professor at Penn State. Anyway, I waited maybe for another week, and there was still no news from Stanford or MIT, so I accepted Yale's offer. But then a few days later, I got accepted by both of them. It was too late to change my mind, so I went to Yale.

And that's why — to answer your question — that statement I read in Yale's brochure sort of left an indelible mark on me. Some people are attracted to university administration work because they see it as an opportunity for a career change. I saw it as a challenge and one that offered me a chance to do things that hopefully would make a positive difference in the end. Retiring from NUS as a career administrator was never my plan and so I did not give up doing mathematics. Put it this way: doing both was a hard struggle. At times administration took priority and mathematics had to take a back seat and suffered. But I just had to keep trying.

S Did you see yourself as setting a role model for the younger people, like me?

C You don't need a role model. You're doing great!

S I was influenced by you! I also try to never give up on my research, whatever other duties I'm involved in. That's important. We are here in the university because of mathematics.

C And it doesn't have to be mathematics; it is the same with any scholarly pursuit, something that you really want to explore or discover. Some people enjoy feeling important. But then, when one's no longer in a position of power, there may be a sense of loss, of being ignored. In Chinese they call it 打回原形. One has to be prepared for that. But if you believe that there are other things at least as important or worthwhile, for example doing mathematics, then you can return to it with energy and enthusiasm. Some people's attitude to you will change when you are no longer in a position of power or influence, but you should still do the things that you enjoy. In other words, the meaning of life for you is not measured by how much power or influence you wield. The value of one's existence should be beyond such things.

S That's important. I think it applies to the university too. The university exists because of education and research, not because of administration.

C Precisely. Administration is actually there to make things happen, to facilitate education and research. The tail should not wag the dog! Administration should be at the service of the core functions of the university. But, of course, to make this happen, a university does need capable leadership in the administration, as is true of any organization.

I Was it during graduate school that you decided to specialize in logic? Or did you already want very much to become a logician when you applied to Yale?

C No, when I went to Yale, I was interested in doing topology, I think because I was influenced by the unusual course I took at Iowa State. It was just point-set topology; nothing great about that, but the experience really struck me. What I didn't realize was that point-set topology was no longer fashionable. At Iowa State, I think nobody at the time was doing algebraic or differential topology. I took some graduate courses but they were in analysis and algebra. But soon after I arrived in Yale, I discovered that it's a different kind of topology. The first course I took there carried the title "algebraic topology", and we were using this book by Edwin Spanier of the same title. It started with category theory, then functors, exact sequences, homotopy, homology, cohomology… It was a culture shock to me. I saw that this was a different kind of topology than I had come to know. So, I became hesitant.

Yale had an interesting system, which is now no longer in place. Every first-year graduate student in the math department had to take four courses: algebra, analysis, geometry/topology, and logic. Everybody — regardless of the intended area of specialization. Now, that is absolutely unusual! I don't think you see that in other places. Why was logic there? I believe it was because of the influence of Abraham Robinson. Yale hired Robinson from UCLA in 1967. Robinson was a very influential figure. His EQ was extremely high, and with his persuasive power he convinced the department that every graduate student must also do a course in logic. So all of us had to pass the qualifying exam on these four subjects.

Soon after his arrival at Yale, Robinson assembled a group of young logicians. In particular, the year that I went to Yale, the department hired Manuel Lerman, a recursion theorist, who was a student of Anil Nerode at Cornell. After his PhD, Lerman went to MIT as a Moore instructor, and when he was there, he solved a very important problem on initial segments of the Turing degrees, and published the results in an Annals paper. After MIT, he came to Yale as an assistant professor. Robinson also got Gerald Sacks of MIT to be a visiting professor at during my second year of graduate study. The purpose was to attract Sacks to move to Yale. There was a very lively group of students in logic. So I thought, since I was not going to do topology, and I was about to take my qualifying exam, perhaps if I pass, I would go for logic. And that's what happened.

When I had passed my qualifying exam, I went to Robinson and said I wanted to be his student. He said okay, and I was his student for almost a semester, and then I switched. The reason was that Robinson was the only senior faculty member in logic at the time (the department hired Angus Macintyre as a full professor after I had graduated), and he had many students — I think it was easily five or eight. Every time when a student wanted to see him, an appointment would have to be made. And when I went to his office at the time of the appointment, I would see two or three students ahead of me, standing there waiting, taking their turns. This happened many times. Robinson had very little time for me. We would talk for about 15-20 minutes, and then he would say, "Okay, come back next week." Maybe that was his style; I don't know. I thought I was not learning much from him. Most of the stuff I had to learn by myself or from talking to fellow graduate students. After one semester, I thought I would not go very far with this, so I approached Lerman who had no student. He asked, "How much time have you got at Yale?" I said, "I have just passed my qualifying exam, so about three more years." In those days as it was a four-year programme. So Lerman said okay, and I became his student.

I Within the area of logic, why did you go into recursion theory? Was it because of Lerman?

C Originally, I had wanted to do model theory, which was Robinson's expertise. But then, since I switched, I became a recursion theorist. And what happened was, at that time, Gerald Sacks was developing his α-recursion theory — recursion theory on admissible ordinals, a theory that extends the classical theory of Kleene and Post, tracing back to Church and Turing in the 1930's. And Lerman was keen to learn this new thing from Sacks, who was visiting Yale. He would usually pull me along on some walks with Sacks around the campus, and I would just tag along listening to the conversation. And I got excited because Sacks had this programme of looking at recursion theory from a generalized point of view. He started life in "classical" recursion theory — recursion theory on the natural numbers, and he dived deeply into the study of Turing degree theory. He published, I think, two or three very influential papers in the *Annals*, between 1961 and 1963, and in 1966 he published the Princeton monograph *Degrees of Unsolvability*, perhaps the culmination of his work on the subject. Then he decided that recursion theory needed an overhaul, and started to look at it from a generalized point of view, in the context of Ronald Jensen's fine structure theory of Gödel's constructible universe *L*. He told me in an interview with him in 2013 that he had frequents discussions with Georg Kreisel, a philosopher mathematician at Stanford. According to Sacks, Kreisel held the opinion that recursion theory needed a good foundation; looking only at the natural numbers was too restrictive and would not capture the essence of computation; one had to take a step back to review the essence of the concept of computation (he wrote a 60 page long paper with the title "Some reasons for generalizing recursion theory"). I think Gerald took that view seriously. In the mid to late 1960s, Jensen, who was then at Berkeley, developed his fine structure theory of Gödel's *L*. It's a deep and beautiful theory, hugely influential and led to what one calls inner model theory in set theory today. And Sacks saw that recursion theory could be framed in the setting of the fine structure of Gödel's *L*. He was developing it at the time when he was visiting Yale, and Lerman wanted to learn that from him, so he pulled me along, and then my thesis topic dealt with some problems in this area. That's what happened.

I And it seems that you never got out from this discussion since then!

C In a sense I never left it. But it led to something unexpected many years later. But let me first digress. During the early days of the development, Sacks trained several students, including some of his best, in this new theory. They included Steve Simpson, who went to Yale as a Gibbs instructor in 1971 after his PhD and from whom I learned a lot about fine structure theory and its applications in recursion theory. Sacks had three students at MIT who graduated between 1971 and 1973 that he called the Golden Age of MIT logic: The first was Steve Simpson, who together with Harvey Friedman (student of Sacks who graduated in 1968) developed the subject of reverse mathematics. Then there was Richard Shore (advisor of our colleagues YANG Yue and GOH Jun Le), and Leo Harrington. In the mid-70's Ted Slaman went to Harvard and became Sacks' student (Sacks was then holding a joint appoint at Harvard and MIT). All of them wrote theses in what Sacks later called "higher recursion theory", a term Sacks used to refer to the generalized theory, to the annoyance of some recursion theorists. In fact, in 1990 he published his last book and it carried the title "*Higher Recursion Theory*". Anyway, all of these outstanding students subsequently made major contributions to logic.

There were several very challenging problems posed by Sacks in α-recursion theory. The most prominent one is the minimal $\aleph_\omega^L$ degree problem. The question is: does such a degree exist? Many tried this problem and failed, including me. In 2018, the last time that I saw Sacks, I asked him about the minimal $\aleph_\omega^L$ degree problem, that is when α is $\aleph_\omega^L$ (the ω-th infinite cardinal in Gödel's constructible universe *L*). Sacks said, "I don't think that I will see a solution in my lifetime," and he turned out to be right. Sacks died in 2019. The problem is by now I think almost 60 years old, and nobody has any idea how to solve it. So people began to drift away from higher recursion theory and moved on to other things. That was the time when reverse mathematics started to develop, and some spectacular results on the definability of the structure of Turing degrees over the natural numbers were achieved by Simpson, Shore and Slaman-Woodin.

Once I was chatting with Slaman, and I commented that interest in α-recursion theory had waned. He said, "Well, not all is lost". Many of the ideas and techniques in admissible recursion theory have been applied in other areas, which is true. One example I can cite in particular, something that I am very pleased with, is the work that Yang Yue, Slaman and I did on the proof-theoretic strength of Ramsey's theorem for pairs — more specifically, whether it is strictly stronger than what is called a stable Ramsey's theorem for pairs. It was a major problem in the reverse mathematics of combinatorial principles. The solution of that problem makes use of ideas from α-recursion theory.

What happened is an interesting story, to me anyway.

The problem regarding Ramsey's theorem for pairs was posed in 2001 in a paper by Cholak, Jockusch and Slaman. There were several open problems in that paper, but this was considered to be a major one. The question was whether you can separate the so-called Ramsey theorem for pairs from the stable Ramsey theorem for pairs. The conjecture was that this was true and one could produce a model to separate the so-called SRT_2^2, Ramsey theorem for pairs, from the stable Ramsey theorem for pairs, SRT_2^2. This would be a model of SRT_2^2, the weaker system, and not SRT_2^2. Now, a natural approach was to come up with a model of SRT_2^2in which every set is what is called low — in recursion theory language, it means that it is a set whose degree is below that of the halting problem, and whose Turing jump is the halting problem. It was known by a theorem of Jockusch that if you have a model with that property satisfied, then you can separate Ramsey's theorem for pairs from the so-called stable Ramsey's theorem for pairs. But then, there was a theorem by Downey, Hirschfeldt, Lempp and Solomon which showed that such a model does not exist in the natural numbers. And to them, that was it! It meant that this approach was dead and gone.

I looked at that paper, and I thought, "Well, what about the non-standard models?" Nobody had looked at the problem from this angle. The reason is that by around 2000, most recursion theorists worked on problems defined over the standard natural numbers. Few were interested in other models of computation. Since I had spent some time studying recursion theory on nonstandard models, it occurred to me that things could be different there.

Now Shore had shown in 1972 that at level $\aleph_\omega^L$ every set below the halting set in Turing degree is low. At the time I thought it was an intriguing result. Then Slaman and his student Mytilinaios showed that this phenomenon could take place as well in some nonstandard models of a certain fragment of Peano arithmetic. Later Yang Yue and I analyzed such models and used them to characterize infinite injury priority constructions. These took place in the late 1980's and the 90's. The overall impression formed was that there is a close parallel between recursion theory on admissible ordinals and on fragments of Peano arithmetic.

Shore's 1972 paper on $\aleph_\omega^L$ suggests that if you have a model of SRT_2^2 in which every set is low in the Turing degree, then well, you're done. The question is: how do you find this nonstandard model? I started thinking about this, and gave a talk in 2005 at the IMS workshop "Computational Prospects of Infinity" on using nonstandard models to study this problem. The idea was somewhat vague, and it was not clear how it would work.

In 2006 I visited Slaman in Berkeley, and I had a chat with him. I said, "Suppose we look at it this way; what do you think?" Slaman said, "Hmm, it's a thought." To me, it meant that it's something worth looking into. I came back to Singapore and started to work a bit harder. Then Slaman came for the logic summer school at the IMS, and Yang Yue, Slaman and I got together and started working on this problem earnestly over the next several summer schools when Slaman was here, and online. The proof was finally done in late 2011; Slaman gave a talk on the result at the 2012 Oberwolfach meeting on computability theory.

I This appeared in the 2014 *JAMS* paper…

C Yes. When I look back, on hindsight, I think that the reason that people could not solve this problem for a long time was because they were always looking at the standard natural number system, but reverse mathematics by definition is a model-theoretic approach to proof theory, meaning that you have to consider potentially all possible models. If you confine yourself to standard models, you're not taking full advantage of the rich resources available elsewhere.

I Tell us about your working relationship with Gerald Sacks. You mentioned earlier that he and Lerman and yourself started these walks, which initiated you into recursion theory. But over the next 50 years or so in your career, how did he influence you? More generally, what was the impact of his work on logic and foundation?

C Gerald Sacks was a towering figure in logic. When I was a student, I felt intimidated by him (Lerman told me later that Gerald enjoyed seeing students being intimidated by him). To me, he seemed arrogant. And although Lerman took me along for these walks, I would say it was more of me following them. When we were doing the walks, he would mostly talk to Lerman, and if I asked him a question, he would say, "Yes," and then he would turn to Lerman and give his reply. In 1970, I was 20 years old, and he was maybe 36 or 37, not that old but already a big time MIT professor.

In 1980, I spent a year at MIT for my sabbatical leave. That was my very first sabbatical leave after I came back to Singapore. Sacks was my official host. Initially the way he addressed me varied from day to day. Sometimes he would call me "Mr. Chong". Other times he would use "Professor Chong". And then one day he would just call me "CT", a name for me which many logicians later followed using.

Not long after I settled down in Cambridge, Massachusetts, I found out that Sacks was very hospitable. He was divorced and lived in an apartment in Cambridge. Every Saturday night he held a party at his apartment, and he would invite all his graduate students and logic faculty at Harvard and MIT and me to the party. Ice cream, cakes and wine or beer formed the spread. After the party there would be midnight movie at a local theater, which I always skipped.

Although during the sabbatical I worked mostly with Sy Friedman (brother of Harvey Friedman and also a student of Sacks) who was then an assistant professor at MIT, I had mathematical interactions with Sacks fairly regularly, including taking part in a "private seminar" he arranged in his office at MIT for a small group of people to study Jensen's "mice theory" for inner models of set theory. And when I was about to end my sabbatical, I told him about my plan to write a book on α-recursion theory. He was very supportive and suggested that I submit the manuscript to the Harvard/MIT subseries of the Springer Lecture Notes series, of which he was an editor. The monograph was published in 1984. I became comfortable interacting with him towards the end of my sabbatical. Indeed in 1996 during the Asian Logic Conference in Beijing, every morning he would ask me to join him for a walk after breakfast, before the first talk. This time it was just the two of us as Lerman was not there…

Sacks was influential in two ways. First was his mathematical work. The research direction in recursion theory followed and extended along the paths he created, lasting several decades. We have a much better picture of the Turing degree structure today, thanks to his pioneering work and those that followed. The Sacks Density Theorem continues to be regarded as a crown jewel of infinite injury priority constructions in the subject. The second was the group of outstanding students mentioned earlier that he produced. The Association for Symbolic Logic honors him with the award of an annual Sacks Prize for the best PhD thesis in logic.

I Did Sacks' conjecture drive a lot of research activities?

C No, unfortunately, because it requires some new ideas altogether and no one knew how to proceed. People felt that there were other challenging and perhaps more tractable problems, and so they moved on. But from time to time some would return to the conjecture. For example, in the 1990's there was a claim by a Japanese logician about a solution, but the proof turned out to have a gap.

S I assume his group of students contributed to the developing of new areas, bringing in innovative directions and fresh ideas. A group of young students holds great potential for generating interesting mathematical insights

C Yes, a number of big results or developments in recursion theory or logic in general bear the names of Sacks' students. The ones that come quickly to mind are the Paris-Harrington mathematical incompleteness theorem, Simpson's theorem characterizing the complexity of the theory of the Turing degrees, the Slaman-Woodin coding theorem, Shore's theorem on the homogeneity conjecture, the theory of reverse mathematics of Friedman-Simpson, and the works of Slaman-Woodin and Slaman-Shore that led to the definability of the Turing degree *0′* etc

I Speaking of applications of logic to other areas… Logic is certainly less well-known compared to more traditional or classical areas in mathematics like algebra, geometry or number theory. How do you see the position of logic in the whole landscape of mathematics?

C I think the current state is that the link between model theory and what is called "mainstream mathematics" is strong. It all started with Robinson's work in the 1960's and continues to this day. During this period there were some spectacular results proved using model-theoretic methods. The best example, to me, is Ehud Hrushovski's proof in 1996 of the geometric Mordell-Lang conjecture in all characteristics.

There are people who use techniques in model theory to study problems in their own area. For example, I was talking to Mok Ngai Ming, who is certainly not a logician, but he, Pila and Tsimerman proved the Ax-Schanuel conjecture for Shimura varieties using o-minimal theory in model theory. And you know, at the recent International Congress of Basic Science (ICBS) in Beijing, they received a best paper award for this important piece of work. Ngai Ming told me that to him o-minimal theory is a very useful black box. I remember that Zhang Shouwu, in a lecture he gave at IMS in 2018, also said the same thing.

But there is always a desire to see, for example, an algebraic proof for an algebraic theorem. I remember — this was years ago — when Jean-Pierre Serre was visiting Singapore in 1985, and I was talking to him. At that time, there was a paper by Jan Denef that came out in the *Inventiones*, on the rationality of the Poincaré series. I asked Serre, "What do you think? This is a paper that made use of model theory to solve a problem in number theory." He said, "Yes, it's a nice result, but I believe that there's a purely algebraic proof!"

So, if you ask me, I think model theory, that part of logic, has done well in terms of making connections with other areas. But recursion theory has not. I think it has to do with the nature of the subject. Fundamentally, recursion theory is concerned with the notion of computation, of what is computable and what is not (hence more people call the subject computability theory than recursion theory these days). This concern about computability or effectivity is certainly shared by the computer science community, but not by the general mathematical community, unless one is interested in computation. But the question of what is computable has always been of great interest in mathematics. For example, we have the word problems in group theory, and also Hilbert's 10th problem on solutions of Diophantine equations over rings and fields…

Another important problem has to do with the Mordell-Weil theorem, even just for the case of elliptic curves. Well, given an elliptic curve over Q, you want to know its rank. Whether this rank function is computable or not, is still unknown. This is actually a natural recursion theory problem, because we are talking about a function whose input is the code of an elliptic curve, a finite set of integers, and whose output is the rank of the curve. It becomes a function on the set of natural numbers. Now, recursion theory studies the complexity of functions or sets — given a set, or a function, what is its complexity? At the lowest level is whether it is computable or recursive. And if it is not computable, what is the level of complexity? Can you classify that? So, this very basic problem regarding the Mordell-Weil rank can be understood or viewed as a problem in recursion theory. But it's not something that can be done in the abstract. You will clearly need number theory to do that. From the recursion theoretic point of view, if the rank function is not computable, it is not the end of the story. You will want to know its Turing degree. Knowing it will shed light on the long-standing general question about Turing degree of "natural examples" in mathematics.

I should also add that some of Slaman's recent works are nice applications of recursion theory to Diophantine approximations in analytic number theory and geometric measure theory.

S As an applied mathematician, I believe that our work — whether as individuals or as a community — requires active engagement with colleagues in engineering, computer science, and physics. These interactions not only help drive progress in those fields but also inspire new directions in mathematics itself. At the same time, applied mathematicians must look toward pure mathematics, which offers the tools and conceptual frameworks essential for deeper development.

I see a similar dynamic in the relationship between logic and pure mathematics. Since logic forms the foundation of pure math, logicians — whether as a field or as individual researchers — also benefit from engaging with other areas of pure mathematics. Such exchanges can help advance those areas and, just as importantly, provide fresh ideas and motivations for logic itself. In the end, everything is interconnected.

C I think you are right there. There are exceptions, perhaps because someone has a really original and brilliant idea. But in general, cross fertilization is always good. If a discipline always looks only internally, it will have a rather restricted view. There are always lots of possibilities to generate new problems by just looking inwards within an area, but in many cases such activities will be of limited consequence. You get more papers, but if that is the objective then there is not much more to say. I remember reading Freeman Dyson in an article that appeared in the *Bulletin of the AMS*, saying that for many years, mathematics inspired physics and was inspired by physics. But then mathematics turned to look inwards, which he thought was regrettable. This is both good and bad, but I think maybe more of the bad than good if that is all that one does. I don't mean necessarily between mathematics and physics, but between what you are expert in and what is happening out there, because then you are not getting ideas anymore from the outside, and after a while it could become an echo chamber.

S Interdisciplinary research is widely encouraged today — and rightly so. People are naturally drawn to it. Yet, if we reflect carefully, bridging distinct fields is profoundly challenging. Take your own field, for instance: even for logicians, connecting meaningfully with other areas of pure mathematics is no simple task. True impact comes from building genuine bridges between disciplines, and that is no easy feat. Still, it's precisely the kind of effort we should continue to encourage.

C That's true. But sometimes people are worried about publishing, and also getting tenure…

S Yeah, because sometimes, in a discipline, you have a fixed way to publish papers…

C You made a very interesting point. In 1990, I was at MSRI for its logic year programme. At that time, there was a paper by Blum, Shub and Smale that had just appeared in the *Bulletin of the AMS*, on what was later called the BSS computation model. The authors wrote that their aim was to bring the continuous part of mathematics into logic. By that I suppose they meant models of computation. As you know, the Turing machine is based on 0's and 1's, hence on the natural numbers and therefore discrete. In that paper, they produced a new model of computation and used it to study some problems, including complex dynamical systems. They showed, for example, that except for the trivial case of the unit circle, all Julia sets of complex quadratic maps are not computable in the sense of the BSS model. I was very intrigued by that, so I started to read some books and papers on complex dynamics. In 1990, when I was in Berkeley, Smale was teaching an undergraduate course on dynamical systems. I attended his course and talked to him to learn more about BSS. Towards the end of my stay, I gave a talk at the MSRI logic seminar on some results concerning the notion of Turing degrees in the context of BSS, using Julia sets of quadratic maps as examples. After the talk a prominent logician came up to me and said, "C.T., nice talk, interesting results, but it's not recursion theory!"

If a discipline always looks only internally, it will have a rather restricted view.

In other words, the talk did not fit into the established notion or framework of the subject. You see, in recursion theory, the natural numbers are taken as given, but not the real or complex numbers. When I say given, I mean computable, something that you can calculate. A set of natural numbers is an object that in general cannot be computed. So, taking a real number as given, like what the BSS model does, is not considered kosher.

S You've raised an important question. Across the mathematical community, there's often a natural tendency to define the boundaries of the field — what is and isn't considered mathematics. Historically, this has led to areas like statistics and computer science, which both originated within mathematics, gradually developing into separate disciplines. While this evolution reflects the growth and specialization of knowledge, it also highlights an opportunity: to reconsider how we draw these boundaries and how we might benefit from a more inclusive perspective.

I think we should aim to be more inclusive. Blurring boundaries between areas doesn't dilute mathematics — it enriches it. For example, model theory seems especially dynamic to outsiders, in part because model theorists actively reach out to and collaborate with other branches of mathematics. That openness benefits not only model theory but the broader mathematical community. Inclusivity, in the end, strengthens us all.

C Yes, perhaps the case of model theory can be traced back to Robinson. We talked about him. He started off life as an engineer in Israel. He became a professor at the Hebrew University, and he wrote a book on aerodynamics — absolutely classical applied math. Siu Yum Tong who was on the Yale faculty during that time told me this story. Once he was having a conversation with Robinson, and Siu asked, "You started life as an engineer, that's really applied and practical, and you wrote a book on aerodynamics. But now you're a logician, purest of the pure –– foundations of mathematics. How did it happen? Why was there such a drastic change?" And then Robinson said, "Ah no, I have not changed my career at all. To me, logic is applied mathematics, and model theory is applied logic. I look at model theory and then apply it to other areas of mathematics." And that is true, because Robinson's model theory was the beginning of applied model theory. I remember in those days it was popular to call the Robinson school as "eastern model theory" as opposed to "western model theory" led by Robert Vaught of Berkeley. Vaught was primarily focused on pure model theory and not in its applications. But for Robinson, a model has to be something concrete: a number field, a differentially close field, or whatever.

I So far, there's only one Fields medalist in logic, Paul Cohen. Even Cohen himself worked across disciplines. He started off as an analyst, and it was almost by chance that he worked in logic.

C Cohen was not a logician by training. According to Nerode, Cohen was always looking for important problems to solve. Nerode and Cohen were both students of Saunders MacLane at Chicago. Cohen had won some AMS award in analysis, and he was talking to Nerode and said, "What is there left to do in logic?" And Nerode, according to him, challenged Cohen, "Well, there's this continuum hypothesis." And he didn't expect Cohen to solve it!

I think one reason that there has been only one Fields medal awarded to logic is because the work of a logician is seldom understood by those outside the field. At least two very prominent mathematicians (members of the US National Academy of Sciences and all that) have told me that they have tried and failed to understand the work of Hugh Woodin in set theory. And then of course there is very stiff competition. So even the proof of the Mordell-Lang conjecture, a result which is certainly better appreciated by non-logicians, did not win Hrushovski a Fields medal. I was told that Tsimerman, who maybe considered a model theorist/number theorist, might have a shot at it. But who knows…

I Speaking of careers, we know that in many universities, the math department will typically only hire one logician, if any at all. What are your views on how we can educate more logicians within mathematics? How do you think the education of logicians will be done in future?

C That's hard to say. First of all, I think the fact that the Mathematics Department (here in NUS) hired one logician in the beginning was an accident. I joined the department in 1974, and it was by accident. The department was not looking for a logician. But I was hired and turned out to be one! And then after I joined the department, I guess I got on very good terms with the senior members in the department like Peng Tsu-An and Malcolm Wicks.

Malcolm Wicks was a student of John Shepherdson at the University of Bristol. His thesis was on combinatorial group theory, but Shepherdson was a logician who did some work in set theory, so Malcolm became very interested in logic and set theory. He was already here in the department when I joined, and I think he was surprised that there was someone in logic. Anyway, Peng Tsu-An became the Head of the Department, and so that was how we got to hire a few more logicians. Rod Downey came in 1983 and later moved to New Zealand. Joseph Mourad, who was a student of Slaman, joined us in 1989, stayed for three years and then left. Feng Qi came in 1991 and returned to China in the late 90's. Yang Yue joined in 1992. Frank Stephan and Dilip Raghavan came only after the year 2000.

I certainly don't think there's a need to have too many logicians in the department. I was reading our interview of Serre the other day just to refresh my memory, and I remember asking him a question, "How do you train and encourage young people to be mathematicians?" And he said, "First of all, there's no need to have too many mathematicians!" So, I guess as a corollary, there's no need to have too many logicians. But I think you do want to have strong logicians on the faculty, colleagues who can and do interact mathematically with those outside the logic.

Coming back to the training of logicians, I think it is the same as training students who will later specialize in other areas. They should have sufficient breadth in the basic graduate courses before they dig deep into logic. Even in logic they should learn something about recent developments in recursion theory, model theory and set theory. Of course, logic just like any other mathematical discipline, has gone through significant developments over the past few decades, and so students have to be properly guided for this.

S Beyond logicians, we are also teaching general students the logical thinking skills. This is not only for mathematics student but for every student in the university. How do we do that? Traditionally, we do it through mathematics education, but now, the curriculum seems to be much more broad-based. How do we enforce the logical thinking skill? It's very important because this ability to reason with logic is needed no matter what you do. It is one of the most important way of thinking about things. How do we train our general students to be logical thinkers?

C Ideally, every student should take at least one rigorous course in mathematics. But this is not realistic. So maybe a course that teaches basic logical reasoning would help.

I Actually, this reminds me of something that we typically encounter at open house duties. Parents will bring their high school kids, and typically the question they ask is: if I do a major in mathematics, what kind of job will I get when I graduate?

C We always say that the training of logical thinking is basic, which is true. But it's not something that parents see immediately. They don't see the connection. That's a problem.

S Also, it is not like we can show them everything by teaching one course. It requires the training from a series of courses to lead you to a logical way of thinking…

C That's the thing. I was talking to a chief technology officer with a startup robotics company. We were talking about Singapore's ambition to train some 15000 AI professionals. I said, "Maybe some of these trained professionals can join your company." He said, "Only if they learn some mathematics and programming." The question is, when we say to train AI experts, what is the level of training? The kind of training we provide will determine the level of technical skills they will receive, and then the kind of jobs they can take on.

I It seems to me that parents harbor the mindset that they send their kids to the university to prepare just for their first job, rather than to educate and prepare themselves for life. It is seldom appreciated that mathematics is a very transferrable skill. The exact knowledge content may not be relevant to what you do in future, but the ability to think rationally and to reason logically is transferrable. It applies everywhere.

S The key is that when we give our students all the transferable capabilities, we also need to remind them that they have to keep an open mind. They have to try to see the connection between mathematics and other fields.

I But if they hold a very shortsighted, short-term perspective, and look at spending three, four years in the university just to maximize their chance to get their first job, then their decisions will be very myopic.

S University education is now more on training abilities, not just a particular skill. Particular skills can become outdated anyway.

C Yes. And also, since we're talking about administration, I must say that when I look back, I do feel that my mathematical training did help in my understanding of administrative matters, analyzing them, and maybe even it helped me in making decisions. It helps us think more rationally, more objectively. Zuowei will know, definitely.

S Yes, especially during your time, when NUS went through a major transformation. You essentially built an entirely new system. As a mathematician and logician, you truly have a talent for building everything from the empty set!

C Well, I think it helped in our decision making.

I You mentioned about philosophy earlier, and I understand that you are also very interested in the philosophy of mathematics. How do you see its relevance to mathematics? How did it apply to you in particular?

C It makes me more troubled, makes me question myself more and makes me feel even more uncertain, I guess. The basic question in the philosophy of mathematics is: How do we know what is true? What is mathematical truth? How do we find out about truth in mathematics? And the problem, of course, is that now we know that this is an unattainable task. And so, what makes me like logic, and I guess at the same time feel disappointed about logic, is that it makes you see that there are things that cannot be done, and there's no way of just going around it. I'll put it this way: it makes me accept the fact that life is not perfect, and there are so many things that one can never reach and understand, no matter how clever you are.

I But don't you find it in a way somewhat comforting to know that logic and more generally mathematics is practically the only subject or discipline that proves its own limitation, that establishes its own incapability? I mean, in many other areas, one would try to claim as much as possible, to have a theory of everything, to answer all the relevant questions as much as possible. But at least in logic, we establish our own limitations as to what is doable, what is understandable.

C Yes, but does it make us feel more comfortable? I don't know.

S For me, every field — whether mathematics, physics, or chemistry — relies on foundational assumptions or starting points. These assumptions serve as our axioms. The critical question is: what do we choose as our axioms? Mathematicians strive to work with the minimal set of axioms possible. In other disciplines, foundational assumptions may be set at a higher level, but when contradictions arise, researchers often revisit these basics to resolve inconsistencies. By definition, an axiom is something we accept without proof — it becomes a matter of belief. The distinction between beliefs and axioms often reflects one's underlying philosophy; when belief becomes absolute and is accepted without question, it shapes how we approach problems. As mathematicians, we aim to select the smallest possible set of axioms and to explore their consequences as far as we can. However, we must accept that there are limits: certain truths cannot be proven and must simply be assumed.

C Yeah. If you push this thing to the limit, you reach the point where you start asking about things like the origin of the universe, is there a god, and all that stuff. And then it becomes very close to religion.

I Well, let's talk about your career in NUS. You mentioned that you returned to NUS in 1974…

C Actually, I graduated in 1973, and I came back in the summer of that year because my passport was expiring. In early '73, I wrote to the embassy in Washington for a passport extension, saying that I was graduating and that I was thinking of applying for positions in America. But they said "no". They checked and saw that I had not done my national service, because I was away when national service was introduced in Singapore, and so there was no passport extension for me: "You go back to do your duty". So, before I left America, I thought, "Well, if I'm going back I might as well look for a job in Singapore." I think it was in May or June 1973 when I wrote to the University of Singapore as it was called in those days. I said, "I'm getting a PhD within a few weeks, and I wonder if there's a position available." And I got a reply from an Assistant Registrar of the University, a young junior officer from the Registrar's Office. In those days, there was no HR or Personnel Office. Everything came under the umbrella of the Registrar's Office.

Anyway, she wrote back saying, "When you come back, write to us again, and we'll see." The reason that I wrote to the university and asked about a position was that I was hoping they would say, "Yes, why don't you come for an interview?" And they would pay for my air ticket. But I only found out later that in those days the university did not fly people in for interviews. So, I bought an air ticket and came back to Singapore. Upon arrival at the airport, an immigration officer looked at my passport and handed me a copy of the Notice of Enlistment. I was to report to the Central Manpower Base within a week, which I did.

Then I went through the medical exam, and almost failed it because the vision test showed that I was at the borderline. Anyway, I passed it and within a week or two, I received a letter informing me that I was to report to the Central Manpower Base in December 1973 for full-time national service that would last two and a half years.

Meanwhile I wrote to the university again to inform them that I was back. They arranged an interview for me soon after that. In those days, interviews were always chaired by the Vice-Chancellor, the equivalent of the President of the university today. He was a physiologist and a cabinet minister. It was held in a big courtroom. There was a big panel of about 15 people. The Chair was seated in the centre, flanked by Heads of Department, Deans and so on. I was at the other end, a singleton, and I didn't know anybody. So, he asked me the first question, "Dr Chong, you're not doing anything useless like topology, are you?" I thought, "Oh, it was lucky that I didn't write my thesis in topology!" I replied, "No, no, I don't do topology." He said, "Are you interested in applications?" I said, "Yes, I'm interested in applications," and I described my work in recursion theory which formed the foundations of computer science. He asked, "What about OR (operations research)?" I said, "Yes, I'm willing to think about that." At that time, I thought, if I could get a position in the university, I'd be happy to try something different. He said, "Are you willing to teach engineering mathematics?" I said, "Yes, of course, I don't mind." Little did I realize that for the first six years of my appointment in the university, I would be teaching exclusively engineering mathematics!

Anyway, the interview was over and I waited outside for about 15 minutes, and then the young Assistant Registrar came out and said, "We will make you an offer." I said, "Oh, I forgot to mention that I have been called up to do full-time national service." She said, "You send me those letters, Notice of Enlistment and all the relevant documents; we'll see what we can do about it." I did that, and then I think about a week or two later I got a phone call from her saying that the university had made an arrangement with the Ministry of Defence. I would do my basic military training which would take three months to complete, and then join the university after that. The condition was that I must serve the university for eight years; if I left any time before that, I would have to go back to do the rest of my full-time national service. I thought this was a good deal and said, "No problem!" Holding an academic position was to me a dream job!

S At the time when you were hired, were you already tenured?

C No. In those days, tenure was given after six years, like today, basically. With a few exceptions, everyone would be granted tenure. In those days, I think just a recommendation from the Head of Department with support from the Dean would do it!

S No committee needed, right?

C No committee. But for promotion from senior lectureship to associate professorship, or from associate professorship to professorship, there was a panel chaired by the Vice-Chancellor to interview the candidate.

Y When you returned from national service, was it already called the National University of Singapore?

C No. NUS was formed only in 1980.

I So, it was the University of Singapore that you joined?

C Yes, I received my tenure during the transition from the University of Singapore to NUS.

I Can you describe the state of the math department and mathematics research at that time?

C In those days, there was already some research being done, as a matter of fact. Not because the university required it, but I think mathematicians were just somehow doing it by nature. One good thing about math is that you do not need millions of dollars of funding to buy equipment for research. You just do it! That's the advantage. The department had about 18 faculty members when I joined.

I That few?

C Yes, 18 people housed in an old house. The house used to be residence for British officers, with servants' quarter.

S At the Bukit Timah campus, right?

C Yes. It was a nice house, but I was told that it was haunted, because during the second world war, there were some Japanese officers staying there, and they were torturing prisoners of war in that house... Anyway, I would say that of the 18 of us, maybe 8 or 9 were active in research in various ways. So, it was good, but there was no research funding, and there was no fund for traveling. My very first travel outside of Singapore after I joined the university was in 1978 when I attended the ICM in Helsinki, and that was because I got a travel grant for young mathematicians from the IMU.

Back then, it was very difficult to communicate with mathematicians. For example, a letter to and from Singapore to America would take at least a week. If I wrote to someone in America, it would take a week or more to reach him; assuming that he replies within a week, I would get the reply maybe three weeks after I sent out my letter. It was very hard to have meaningful mathematical communications that way, but that was the way it was. And then, two years after Helsinki, six years after I joined the university, I took my sabbatical at MIT in 1980. Spending one year to interact mathematically with people is just different from attending a congress, or through writing letters.

In 1980, the University of Singapore and Nanyang University merged to form NUS. Nanyang University had a vigorous programme in mathematics, in terms of its mathematical activities. Teh Hoon Heng (郑奋兴) was then the head at Nanyang, and he had a very good relationship with the Chinese business community. The Lee Foundation gave Nanyang University a large grant to set up a mathematical center. They even used the money to publish a journal called NanDa Mathematica, now defunct.

Following the merger, the Lee Foundation's center, which was called the Lee Kong Chien Center for Mathematical Research, became part of NUS. The money came under the jurisdiction of the department at NUS. Then Peng Tsu-An became the Head of Department in 1982, and he made good use of the money. Every year the department organized an international conference in a specific area. But even before that, in 1981, when I came back to Singapore from my sabbatical leave, there was already a conference funded by the Japan Society for the Promotion of Science. It was a big mathematical conference with 10 senior Japanese mathematicians from Japan, including Nagata, Iwahori, Fujita and others. After Peng Tsu-An became Head, he started the annual conference series. That was also the first time that Serre came to Singapore, in 1985. Peng was very interested in raising the image of the NUS math department. There was a conference in group theory, a conference in logic, a conference in analysis etc. representing research interests of the department. The annual conference series continued for many years into the 1990s.

I So, Peng Tzu-An was Head till…

C 1996, a long, long time, maybe 13 years. He hired many people during his time, Zuowei and Yang Yue among them. That was the time when the department had more than a hundred faculty members.

S Statistics was part of us…

C Statistics, yes. And then later a separate department, called the Department of Statistics and Applied Probability, was formed. What happened was that in the old days the mathematical statisticians were housed in the Department of Mathematics, and there was the Department of Economics and Statistics in the Faculty of Arts and Social Sciences. I always thought that for the statistics discipline to grow and flourish at NUS, it had to be an independent department. When I became a Deputy Vice-Chancellor in 1996, I took a close look at major statistics departments in universities overseas and talked to some prominent statisticians to learn about the setting up of such a department. Thereafter the Department of Statistics and Applied Probability was established, bringing everybody together under one roof. We had to put in some efforts for the transition but in the end, it proved to be the right move. It is now the Department of Statistics and Data Science.

I What about education? How was the students' curriculum?

C I would say, not much changed between the time I joined the university till maybe the 1990s. It was a three-plus-one programme: in three years, you get a bachelor's degree, and then in one more year a selected group of students would get an honors degree. The only difference was that the university introduced what was called a direct honors programme. That was under Vice-Chancellor Lim Pin. It was for the especially talented students to get an honors degree in three years. But you know, in those days, if you do mathematics, you do only mathematics; at most you do one physics course, but you don't have other requirements to meet. One could say that students of those days learned more mathematics than students of today. And there was no such thing as watering down; everybody took the same exam, and if you failed, too bad, you repeat the year! Of course, we later introduced the broad base curriculum, modular system, and so on.

I But that was much later, in 2005 or 2006?

C Actually no. As early as in the mid-1990s, the planning had begun for the modular system. I was then a Vice Dean of the Faculty of Science, and Lim Pin appointed me, and I think one from Engineering, one from Arts and Social Sciences, to do a study tour on the modular system. I think that was not long after I became the Head of the Department of Information Systems and Computer Science (ISCS). We went to several places in America and in the UK to learn about what was known as the broad base education system. We went to Berkeley, and the first question they asked was, "If it ain't broke, why fix it? What's the problem with your system?" We said, "No, we want to learn…" And they said, "You're doing fine, why do you want to change?" But we wanted to change the system to one that would better prepare our graduates for Singapore's future economy, and not change for change's sake. Anyway, the whole process took time to complete.

S Before that, we essentially followed the UK system. Then we've converted to be essentially more like the American system.

C Yes, not only in education, but also the academic structure in general. This took place roughly between 1998/99 and 2002. I was involved in the conversion of academic titles from the basically UK system to the US system. I was then Deputy Vice-Chancellor, and I made that suggestion to the Vice-Chancellor, but it was not for the university alone to decide. We had to get the government's approval.

I Was it because the university was part of the government back then? A government agency?

C Yes. NUS at that time was not an autonomous body like it is today. Also, changing the academic titles at NUS could have implications on the other institutions of higher learning in Singapore, so it made sense to seek approval. The proposal was to change lecturer to assistant professor, and senior lecturer to associate professor. At the time, we followed the Australian system: lecturer, senior lecturer, associate professor, and full professor. Back then, full professors were a precious few. Most academic staff would retire as an associate professor or below.

Anyway, I presented to Lim Pin the proposal to switch to the American system: lump the four ranks into three: assistant professor, associate professor, and full professor. We received approval from the government on the second attempt. And we began the process of changing the nomenclature around '98, '99. It turned out to be an arduous process, because we had to go through the whole list of individuals. Lecturers becoming assistant professors was fine. The hard part was the group of senior lecturers.

S Associate professors remained in their positions, while senior lecturers who were above the superscale bar were automatically converted to associate professors.

C Some associate professors in the old system were unhappy. They thought, "It took me a long time to become an associate professor, but with this new thing, these people become associate professors overnight?"

S In the past, promotion to associate professor required an interview with a university-level committee. With the changes introduced, I was automatically converted from senior lecturer to associate professor because I had already crossed the superscale bar. Some associate professors from the old system pointed out that I had not undergone the rigorous committee interview process they had experienced.

C Yes, that's what happened. But after some pain, this conversion finally went through. Maybe this was the easy part. The hard part was to introduce and implement the promotion and tenure (P&T) system.

S Because now, a promotion from assistant professor to associate professor is a tenured promotion, so it has to be implemented through the committee?

C Yes, I remember this well. During the Christmas period of 2000, I visited UCLA, Berkeley and Stanford, and I talked to the provosts or senior administrators of those three institutions to understand their P&T system. I remember vividly that I even got the form that Stanford used for their P&T evaluation. When I came back to Singapore, I wrote a draft of the P&T system for NUS based on what Berkeley and Stanford were using; one can still see the basic format and structure in the current form, though of course, the details of evaluation have been refined. And I remember, after the draft was written, I made the proposal to Shih Choon Fong, who was then the NUS President. He supported it. Then we had a town hall meeting in the Engineering Faculty with all faculty members, to introduce the new P&T system. I said to the audience, "The highlight of the system is that from now on, when it comes to promotion and tenure, it will not be decided by just one or two persons, but I counted maybe around 29." And I added, "It's better than the current system. A lot more rigorous and fair. It may take a longer time, but it's certainly more transparent and objective, with international review and so on." Then someone stood up and said, "Wonderful system! I support it and like it very much! But can you implement it after I've got my tenure and promotion?" Anyway, that's what happened.

I Before this, you were serving as a Vice Dean, and then you took over as the Head of the Computer Science Department, right?

C It was concurrent. I was a Vice Dean, and then one day I was asked whether I would take up the headship of the ISCS Department as we called it — Information Systems and Computer Science. Bernard Tan, who was the Dean of Science, told me that they were looking for a Head. In those days, there was no international search for such positions; you just do an internal search. He said I was the most suitable person to do that, because they could not find one suitable in the computer science department, and my work was in logic, which was close enough.

S At that time, the computer science department was still part of the Faculty of Science.

C Yes, it became the School of Computing in 1998. The field of information technology was developing very quickly in Singapore and it was becoming clear that it would play a very significant role in the Singapore economy. I remember writing a paper on the setting up of a school or faculty within NUS dedicated to IT, with the vision that in due course it would develop into a center of excellence in computer science education and research. The paper was presented to the Vice-Chancellor and the proposal was accepted.

So, I agreed and took up the appointment. But it turned out to be a huge challenge when I started, because I realized that I was treated as an outsider, almost an alien — a person that the university had imposed on the department. There were some very stinking letters… Email was already available in those days, and people were circulating emails badmouthing me. In fact, a senior member of the department sent an email to me and copied it to everybody — an open letter — saying how unqualified I was. First, my training was not in computer science. Second, a Head of the Computer Science Department not only had to be good in academic work, but also had to have very good links with the IT industry, and I had neither of these.

I held the job for three years. I started in '93, and in '96, the Vice-Chancellor appointed me as a Deputy Vice-Chancellor. But I should say that by 1996, many people in the department had become very friendly to me, even till today, for example Ooi Beng Chin who is an outstanding computer scientist. He told me some years later that he was very upset with my appointment as Head in the beginning. I suppose people realized after some time that I went in to do a job, trying my best for the department.

S Beng Chin, who served as Dean, is known for being selective in his interactions, yet he has always treated you kindly. Notably, he was elected as a foreign member of the Chinese Academy (CCA) this year.

C He's very proud of it. But let me back track. I must first say relied a lot on Tay Yong Chiang when it came to administering ISCS. Yong Chiang was my student in the math department. Later he went to Harvard for a PhD in computer science. When he came back, he joined the Department of Mathematics, although he also taught courses in ISCS. He works in database theory, and wrote a very good thesis. When I was appointed as the Head of the ISCS, I consulted him. I said, "I know little about computer science, so you should be my advisor." He said, "I can give you advice privately, but I will not be appointed to any position." He didn't want to be the face. He provided me with very good ideas and suggestions on developing the department. Not long after starting my appointment, I went to visit the computer science departments at MIT and Stanford — where quite a number of Singaporeans and Asians were doing their PhDs — with the intention of hiring some of them. And Yong Chiang helped me set up the meetings. He knew the people, and wrote to them. Through him, I went to these places and interviewed quite a number of people. At MIT, I think I hired three of them who later joined NUS, and two of them were Singaporeans. One of them was Sung Kah Kay — I think you have never met him because he was killed in a tragic plane crash in 1998. It was very unfortunate. Kah Kay was a Singaporean, a very smart guy. As a young kid, he dabbled in computers. He went to MIT as an undergraduate, I think at a very young age, and continued to do his PhD there, and after he got his PhD he worked for a company in Princeton on computer science research. When I went to MIT, Yong Chiang helped me contact him. He went up to Cambridge to meet me. After the interview, he decided to return to Singapore to join NUS. His parents had also wanted him to come back. Kah Kay was a very promising young faculty member, but unfortunately, he could not realize his full potential. In 1998, while on his way to a conference in LA, the Singapore Airlines flight he took crashed at Taipei airport during takeoff in a thunderstorm! He and his wife were among those who did not survive the tragic crash, and Kah Kay was only in his early thirties! I think it was a great loss to the department. We later set up the Sung Kah Kay Assistant Professorship in his name, in memory of him…

Besides hiring, I also set up an external visiting committee scheme for ISCS. Before that, the University only had a system of external examiners, whose job was to come every year to look at our exam papers, make comments and so on; the focus was on undergraduate education. After consulting Yong Chiang, I invited two renowned computer scientists, Jeff Ullman of Stanford (who won the Turing Award in 2020) and H.T. Kung (Kung Hsiang Tong) of Harvard, to visit the department as members of the Visiting Committee. They were asked to assess the current state of ISCS, hold meetings with faculty members and students, and make recommendations for future development. I remember a key recommendation they made was that our computer science research should shift its emphasis from pure paper publication to systems work, because it was the modern trend and that's where the department could deepen its links with the industry. These recommendations made a great impact on the direction of research in ISCS. Some of the best works coming out from the department since then, for example those of Beng Chin, were on systems. The visiting committee scheme, to my mind, was quite successful. So later I implemented it throughout NUS, replacing the external examiner scheme.

Yong Chiang also helped set up contact with Charles Leiserson of MIT. In that particular year, Leiserson was planning to take a sabbatical leave, going abroad for a year with his family. Yong Chiang told me about that, so I wrote to Leiserson, who said it would be an attractive thing for him to come to Singapore, but he was actually thinking of going to ETH in Zürich, which would be, I suppose, scientifically more rewarding. But after some back and forth, he said, "Well, maybe let me come to Singapore and take a look first." He came for a short visit, and upon his return to the US, he said, "I'm still waiting for Zürich, but if you can make me an offer within a week, I'll take it." I talked to the Dean, Bernard Tan, who supported my proposal, and he called Vice-Chancellor Lim Pin who gave his approval. So, I emailed Leiserson and said, "Here's our offer." Within a day, he said okay and accepted it. Later I asked him how he came to the decision. He said it was because Zürich seemed to be taking a long time, but Singapore seemed to be very efficient. He taught a course at ISCS when he was here, and interacted with faculty and students. Following that sabbatical leave, he came back again and again. There was a year when he came with Tom Leighton, who held a joint appointment at MIT mathematics and computer science departments, and a co-founder of the high-tech company Akamai.

Overall, I would say that the ISCS Department became very international and even more so today. I think I learned a lot spending three years there, and I would say that I did my part for the department. In 2004, when I was leaving the Provost's Office, I had lunch with the senior member who sent the open letter, and he said, "Okay, I think you have done alright."

You see, administration, if you look at it, is not so difficult. What does one need to be a good administrator? First of all, you have to learn the ropes and all that. Secondly, you have to be willing to listen. Thirdly, you have to see how your role models — other successful departments — how they do it, and see to what extent you are able to follow or adopt. You just have to always keep in mind that your personal interests and preferences should not influence your decision. Then I think you'll do fine. Especially so for me as an outsider to the computer science department. But there were a few things that, fortunately, helped. First, even though logic is not directly a part of computer science, it is somewhat related. Second, I got a good advisor in the person of Tay Yong Chiang. And third, there were good successful departments that served as role models.

S I always tell people that administration is not a hard science. If you approach it with empathy, are willing to put in the effort, and take the time to talk to and listen to others, most problems can usually be resolved.

Occasionally, some issues may require a bit more thoughtful consideration.

C Yes, that's how it works. And also, while it is true that I had no links with the IT industry, within a relatively short time it was no longer an issue. The way the game is played is that once you're in the position of influence, people will naturally come to you! Today you may be a nobody, but let's say tomorrow you become leader of a company or organization somehow; people will come to you. Even if you knew no one before that, suddenly you will have many new friends! It's like that. So, I became the Head of ISCS and very soon I got calls from industry. Within a month, I would say that I knew as many industry people as any ISCS Head would need to know.

S On this point, many people don't realize that when they suddenly become important, it is not because they did something, but because of their position. They don't realize that. Once you leave the position, you're not important anymore.

C Absolutely. Once you leave the position, no one comes to you anymore. They go to the next guy!

I They are friends with the position, not with the person!

C So, this is something to always keep in mind. It's in the nature of things that one should be prepared for and accept.

S In any role one takes on — especially in positions of power or influence — it is important to focus on building something that will benefit the university or the state in the long term. While many people can manage the day-to-day operations, making a real difference means looking beyond immediate needs and working towards a lasting, positive impact.

C Indeed. I recall that in 1981, not long after Lim Pin became the Vice-Chancellor, Louis Chen and I wrote to him with a set of suggestions on what NUS could or should do to advance to the rank of a world-class university. This was in response to the vision for NUS which he articulated soon after his appointment. Louis and I were excited by his vision and saw it as the dawn of a new era for higher education in Singapore. We drew on our past experiences and what we knew about world-class universities to propose some steps NUS could take to make progress towards that. I remember, a week or two after that, the Principal Assistant to the Vice-Chancellor called us up for a meeting. He said the VC (as we then called Lim Pin) had read our letter and thought it contained some good ideas. The University would look into the proposals and explore the ideas. Perhaps Louis and I were somewhat naive in those days and did not appreciate the amount of work required for the proposed changes. But it is nice to see that some of the things suggested in the letter did become reality over time. wo that I can think of immediately are the P&T system and a more flexible education system.

I Well, as I was walking up to the IMS earlier this morning, I saw on the plaque that the auditorium was opened by you as the Provost in 2003. This was 20 years ago! Setting up of the IMS was also one of the things that you oversaw. What were the key challenges and opportunities that you saw at that time for the IMS?

C Well, in the speech I gave at the IMS when it celebrated its 21st anniversary, I sketched the history of the IMS, about how it evolved. Peng Tsu-An, Louis Chen, John Berrick, myself and quite a number of others, I would say, had always been interested in having a mathematical institute located within NUS. And of course, the visit to Berkeley for the 1986 ICM, and the conversation with S.S.(Shiing-Shen) Chern at the party he hosted in his house strengthened our belief that the University should have a mathematical institute.

I Was MSRI the model for the IMS?

C Yes, that was our idea. Chern strongly supported this. But when we wrote to the university soon after the visit to Berkeley, I think the response was lukewarm, because… well, the idea of a mathematical institute was just not on the radar screen of the university. And I think it would not even have happened if not for this opportunity that in the year 1998–99, the Ministry of Education (MOE) was looking at large proposals or initiatives that it could fund, projects which would have an impact and contribute to the new knowledge-based Singapore economy in the 21st century. The Department of Mathematics submitted a proposal on setting up a mathematical institute to the MOE. I was Deputy Vice-Chancellor and represented the University to consider all proposals. Louis Chen made an excellent presentation at the MOE meeting and the Ministry approved the proposal. This was exciting because until then I did not expect the field of mathematics to be recognized as capable of making significant contributions to the Singapore economy. But it happened, and so we got an institute.

I So, the initial funding money came from MOE?

C We were given $5 million for five years, which was modest compared to what mathematical institutes at other countries were getting, but still it was a good start. And it was the first time in Singapore that we had something officially dedicated to mathematics, other than for the purpose of education…

S Also, it was probably the first time in NUS, maybe even in the whole of Singapore, that we have an institute which does not have producing papers as its aim, but to serve as a platform for the interdisciplinary work. That's the cultural part.

C I think on that point, we still have to continue to convince people. Different people come to assume positions of influence, and they have to understand this.

S This kind of institute is really about the mathematics culture. Those in other disciplines may not understand that mathematicians need to get together and exchange ideas. Some discussions may not look important at first, but subsequently become important. But these things are hard to quantify. That's exactly why it is a part of the culture. What we want to build is not something that can be measured using some index.

So, Chi Tat, I'm thinking — compared to the time when you came in the 70s, mathematics in NUS and in Singapore has progressed a lot. You are one of the representatives of your generation who worked hard to build up mathematics in NUS and more generally in Singapore. The IMS is also part of this buildup. What was the vision of your generation at that time that has led to our mathematics today? And what is your view of the future? Some of the ambitions and ideas you have had when you were young have been realized, becoming a part of the department now. Of course, while we are getting better in terms of research, we still need to work hard and do even better than before. What can you tell the younger generation today about what we need to do? How can we learn from your experience, and what do we need to do further to make it better?

C You know, I was saying that when I was the Head of ISCS, I got Charles Leiserson from MIT to spend one year here for his sabbatical and he came back several times after that. There was an occasion when I was having a conversation with him about the computer science department in Singapore and that at MIT, and Charles said, "You know, one difference I can easily tell between the MIT EECS department and the one in NUS is that the people at MIT, young or established, are not afraid to think about doing big things, even if they eventually do not succeed. Because they dream that they can do big things, they aim towards that. But at NUS," he said, "many just want to publish papers!"

So now to your question. I think in the early 70s, when we looked to the future, our dreams were more modest, obviously. So, a mathematical institute,

> **It was probably the first time in NUS, maybe even in the whole of Singapore, that we have an institute which serves as a platform for the interdisciplinary work.**

interactions with visitors, research funding, and so on. These were our primary concerns, because in those days, they were not available to us. If we compare today to the 1970s, we have gone a long way. But if we look to the future, what would we like to see?

Can we get many of our people to dream big and work towards realizing them? To do that, we must have the right people. And I think we have made great progress in that direction since the 1970s. Just look at the invitations to speak at ICM and ICAIM that our math department colleagues have received. But I think it is fair to say that we are not yet a great department. How much further can we go? Every step forward from here gets harder. At this point, it is easier to fall back than to advance. I remember some years back I was talking to the Chair of Berkeley. I said, "Berkeley's math department is doing great. What are your worries?" He replied, "We have to ensure that we always do things right, because once you miss a step, the road to recovery is not guaranteed." Will we become a great department? I don't know, but we should give it a try. Just bear in mind that the road to mediocrity is not necessarily marked with warning signs…

S The institute must be a part of our math community. We are here to work together with the math department.

C Yes, and the department must take advantage of this. We are lucky that the IMS, modelled after the MSRI, is not in competition with the department. We have seen examples where the institute and the department fight against each other. They spend more time doing this than collaborating to advance science and mathematics. Luckily this is absent here, and so that hurdle is not there. But I think we should make use of the opportunity.

I You spoke about having the right people in the department — people who dream big and work towards realizing them…

C People with dreams and ideals and just want to do something — if we can get a group of people like that, and they work together, then we are fine. But for that, you have to have a balance of people, between the individuals aspiring for achievements and those willing to work as a team for the greater good. For my generation during my time, we were lucky that there was a group of us who had a little bit of both, meaning keen on research and interested in doing something for mathematics in Singapore.

You know, we have not talked about the Singapore Mathematical Society (SMS), but four of us — Louis Chen, Leong Yu Kiang, Ng How Ngee and myself — got very involved in the SMS activities. The SMS was publishing a research journal (the *Bulletin of the Singapore Mathematical Society)* which was not a very good journal and is now defunct. A new journal, the *Mathematical Medley*, was launched not long after I joined. This was a publication for students and the mathematically inclined. The SMS also ran an annual mathematical competition for high school students, organized public lectures periodically, and conducted refresher courses for high school math teachers. We even organized some small mathematical conferences… nothing big, because we did not have much funding. But the four of us (young people) were doing all of these.

Back then, things were more hierarchical. When I joined the department, I was the most junior, obviously. First, I was assigned to teach engineering mathematics, which I promised I would do. But it was not trivial to teach engineering mathematics, because engineering classes were held at the Prince Edward campus, near Shenton Way today, not at the Bukit Timah campus, so I had to drive there several times a week. The second thing was that, although the department had only 18 people, there were not enough offices. I was squatting in someone's office for a while. One day the Head of Department said to me, "Come, let me show you your office." We got into his car, and drove for about 10 minutes before arriving at the place. And he said, "This is your office." I was going to be the only person there. I said, "Why me?" He said, "Oh, you're the most junior!" But as if by a twist of fate, just as I was ready to move into that office, the colleague (I've never met him) whose office I was temporarily using, and who was on medical leave due to cancer, had passed away. So his office became mine.

Okay, so in the SMS, the people who did most of the work were the secretary, assistant secretary, treasurer, the editors etc., positions held by the young people. The four of us were involved in all of these things. For example, in publications, Yu Kiang and I took turns to write articles for *the Mathematical Medley* on the history of mathematics, featuring a mathematical figure in every issue. We did this for about six or seven years, besides our own research and teaching. For one issue, I would write about Gauss, for another issue, I would write about Euler, Riemann or Poincaré. Nowadays you can search information on the internet, but back then searching for information was not easy. Worse than that, personal computers were not available — only typewriters. The department and the SMS had no funds to pay for a typist, and I was the one to type up every issue of the *Mathematical Medley*. We had a big typewriter in the department that could type Greek alphabets and the usual letters. It's big! It was basically a typewriter with two keyboards. On one keyboard, you get the usual A, B, C, D; on the other you get α, β, γ. The typewriter worked like this. When I wanted to type an α, I would lift up the lever, pull the carriage return to the right side where the Greek keyboard was, push down the lever to lock it in, look for the α key, type it, and then lift the lever, push the carriage return to the left side of the typewriter, push it down and lock it in, and then type a sentence, and continue this way. Of course, if there was a typo or if I wanted to edit a sentence, then I would have to start all over with a fresh sheet of paper to be inserted into the machine...

You have to have a balance of people, between the individuals aspiring for achievements and those willing to work as a team for the greater good.

S That's really a full-time job!

C And in those days, there was no admin support. The four of us had to run the annual SMS competition. We set questions, wrote to the schools to invite them to participate, organized the logistics. We also went to different places to buy gifts for the winners, got the trophy engraved with names of winners, wrote to the Ministry of Education to invite a big shot to come to give away prizes, arranged catering service for the prize-giving ceremony, and contacted the press for them to cover the event and interview the winners. We did all of that, surprisingly with energy and enthusiasm because we believed that we were doing something meaningful.

S You have to type the letters yourself?

C Yeah. The department had only one secretary, Harry Song. His desk was upstairs and he took orders only from the Head. Most of the offices had no phone, for example my office. If there's a call for me, it would go to Harry Song upstairs, and he would press the buzzer, my room would buzz, and then I would be given about 30 seconds to rush upstairs to pick up the phone. That was the environment in which we had to operate.

S Do you have old copies of the *Medley* kept somewhere?

C We threw them away, but maybe some digitized version is in the department. By the way, I also translated S.S. Chern's autobiography. He wrote it in Chinese, 《学算四十年》. I wrote to him asking permission to translate it into English, and he granted it. So, I translated that into English, and again, I typed it and then published it in the *Medley.* In 1978, when I attended the ICM, I was also the secretary of the SMS, so I attended the IMU general assembly meeting. The Singapore delegation had only one person. The American delegation had five. By chance, I was seated across the table from S.S. Chern, and Chern said, "Oh, you're the one who translated the article!" So that's how, during the conversation, I got to invite him to visit Singapore, which he accepted. In the following year, he came to Singapore, and I organized a public lecture for him. I contacted the Straits Times and the 星洲日报 to interview him. That gave his visit a big publicity. When he gave his public lecture that evening, more than 400 people turned up. The lecture theatre could only seat 400 people and it was full with only standing room. All of that was done with no secretarial support.

S Was Chern a very good speaker?

C He's okay, not a particularly great speaker. I taped his lecture with a recorder. I asked him, "Do you want a copy of your lecture?" He said, "I never listened to my own talk. It is awful. I don't want to listen to it." Unfortunately, I think I have lost the tape.

By the way, I think it would be great if we can somehow find that big typewriter. I think it's a museum piece. They don't make these things anymore.

S If you can find that, we can put it in our lounge!

I Let's talk about the time after you have stepped down from the Provost office, when you took over as Head of the Math Department in 2006. I remember there was this dinner that you got some of us to attend and to hear from us the various concerns and issues about the department. And I remember quite clearly that you articulated your two main areas of focus for your subsequent years as the Head of Department. One was the graduate programme, the other one was the special programme for mathematics (SPM). Can you share with us how you came to the realization that those two aspects of the department's activities were the most important to develop at that time?

C I think the graduate programme was just beginning at that time, and there was a lot of things to be improved. I was thinking that it needed a good structure. For example, I don't quite remember whether there was a qualifying exam for PhD candidacy, but we needed a framework to do that. Also, I noticed that the quality of our graduate students was not even. And so I thought it was a good opportunity to strengthen the graduate programme. But this was a chicken-and-egg situation. If you want a very good graduate programme, you need very good students; but if you don't have a good graduate programme, you don't get good graduate students! How do you jump-start and make things happen? And you have to make things happen sort of in parallel, maybe gradually. You cannot expect that one day you are here, the next day you are at the level of Princeton. I mean, that's unrealistic. So, how do we bring the graduate programme quality up, maybe over time, but eventually reach a level of "world class"? That to me was a challenge, but something that was worth doing. Furthermore, a research-intensive university that NUS aspired to be ought to have a solid graduate programme, and invariably every university with high research reputation has a strong graduate programme in mathematics. And there we were, at a point with opportunity and promise, and let's work towards that. So that's the graduate programme thing.

For the undergraduate special programme, I guess it goes back to my undergraduate days. I always remember this topology course Math 301, and I thought, "Maybe we can try something like this here." The Moore method is not realistic in the context of NUS as our system is more rigid. Obviously, the Moore method of instruction would cover less material as it emphasizes student self-discovery; so maybe time-wise, it is not a good idea. But could we at least create a space, a platform, to allow students who want to learn "real" or "hard" mathematics, to have a chance to do so? I also thought that this would also be the place where we could train our future colleagues. This programme would provide them with a solid mathematical foundation and prepare them well for a top graduate programme. Some people think that instead of a special undergraduate programme, we can simply get the talented student to take graduate courses. I think the two are not the same. First, more does not necessarily mean better. Without a solid foundation, learning more stuff need not prepare you to be a better mathematician. And second, the availability of relevant graduate courses is also a consideration. And I also believe that it is important to have a segment of the department staff to be local or trained locally at the undergraduate level. The SPM, I thought, would be sort of a standalone entity, separate from the rest of the curriculum, a place where eager and enthusiastic colleagues can immerse themselves in teaching eager and enthusiastic students. That's what I wanted to see.

I don't know about other places, but when I was a graduate student, I noticed that Yale had a programme in the mathematics department; they called it "Early Concentration". This was for students who were really keen in mathematics and who wanted to do a PhD later on. They concentrated in mathematics early, and then they could take advanced courses within that programme. I thought this was something that could be easily adapted to our local context. Of course, in those days, I was not aware enough than I am today — if you look at how leading universities in China today are training their students in mathematics with special programmes, it is shocking! If Singapore does not do something, I think we are going to be left far behind pretty soon! So, the SPM I think is a place where NUS could, or try to, compete.

I Looking back, how would you evaluate the programme? How successful do you think we have been? And how can we build on where we are today and go further?

C Well, it's both early and not too early. Not too early in the sense that we have seen some of the SPM graduates complete their PhDs from top departments. That is very reassuring, and the programme is a success measured in those terms. But this is just an early stage. It will be interesting and useful to see how many will succeed as professional mathematicians. But this will take another 10 years to find out. If we have maybe ten percent of them do, then we are okay. We should bear in mind that there are many factors that make a successful mathematician. SPM's role is to provide a good mathematical foundation.

I should also say that it is not just the people who go on to do PhDs but also the people who graduated from the programme but took up a different profession. How are they doing? If they are not doing anything different or better than their counterparts in the standard curriculum, then one can ask the question if there was a need to have the SPM? Could you not just have special private individuals training for the very few who really want to do graduate school, when the rest makes no difference in the end anyway? In other words, is the SPM cost effective? I think it would be good to do a study to find out where the former students of SPM are today, and how they are doing. Not the recent graduates, but let's say those from 10 years ago. I think we can at least gauge the non-PhDs more easily.

I Well, in 2 more years, we would have had the SPM for 20 years already. It started in your time, 2006. In fact, a few of our SPM graduates like (Goh) Jun Le and (Tran Chieu) Minh are now back with us in the department. So, in some sense, we have built a pipeline and have started to receive the fruits of the first few years of the efforts.

C Yes, this is a very good start.

I I guess one of the developments that we have had over the past 10-15 years is the influx of postdocs. In 2006 when you took over, there weren't that many — in fact, I don't remember there were any postdocs around at that time. Nowadays, we have a steady flow of postdocs joining us.

C When I was Head, the department allocated some funds to hire postdocs, but the funding was limited; I think we had only one or two. Then we set up the MQF programme — Masters for Quantitative Finance. The idea was to use the income to support mathematical research: hire postdocs, support fifth year graduate students, support graduate students to attend conferences, and so on. Over the years, the reserves from the MQF have gone up exponentially, and the department is now able to support more postdocs. The only problem now of course is that we don't have enough space! MQF was set up during my time, I don't quite remember which year…

S We started the MQF in 2009, but I remember I went to this university committee meeting in 2008 to defend our programme. That was just after the financial crisis in the US, and people were asking, "This Black-Scholes model doesn't work very well, so why are you still doing that?"

C Yes. Funds do take time to accumulate, and now the department is in a fairly comfortable position, and is making good use of the income.

I Before we wrap up the conversation, what advice would you give to a young colleague joining the department today?

C That's a big question! For young people, I think, work hard, and take full advantage of the opportunities, and think big. But I know… in the face of P&T (promotion and tenure), maybe only thinking big is not a good idea. If someone says, "I want to prove the Riemann Hypothesis," you cannot say, "Go ahead!" That person may lose his job! So, you have to do it with moderation. But luckily, I think we have gone past the culture of publication just for the sake of publication. Especially for colleagues who have gotten tenure, I think they can really aim for something. In recent years, many of our senior colleagues, even some junior colleagues, have done very well, and they can be role models for the young people, who can now see what is possible. So, to the young people on what they should or should not do, perhaps look at the successful cases in the department and see how they can emulate.

I What about for senior colleagues who are taking up administration and the leadership positions?

C My experience is that the hardest part of being in the leadership position is to make the right decisions. Sometimes making the right decisions means that you will make some people unhappy; do you do it or do you not do it? I remember there was a time when I got more involved in central administration, and I felt troubled from time to time when it came to making decisions, because I knew that if I did this, it would affect somebody or some groups, and if I did that, it would affect other groups. There were times when I became sort of indecisive. Then one day, I thought, this was all nonsense! In the long run, if you look at what is good for the university, and going one way will be good for the university, then so be it, bear the cost. You may lose some friends along the way. You may lose your admin position. But does it really matter? Unless you see staying in the administration as a matter of highest priority, that your future depends on this — if you think that way, then of course your decisions will be colored, dictated by other considerations. But it will be useful to ask: If you're a Provost, how long can you stay as a Provost? People may want you to go! The term of an admin position is fixed. When you leave the position, I think what you want to know is, do people say, "Finally he's gone," or do they say, "Too bad he has to go"? This is always something to keep in mind. I remember the phrase "without fear or favour" which my old friend Leong Yu Kiang taught me years ago, when we were young and very idealistic. To practice it in decision making requires constant reminder to oneself, and perhaps some courage, as one would soon find out. It was challenging but one should always try…

I Well, we've been talking for 3 hours now! Your insights and your thoughts have been very valuable, and we have all benefited and learned a lot from having this conversation with you. Thank you again for sharing with us all your experiences.

C Well, you know, I have done interviews with mathematicians quite a number of times, but this is the first time that I am at the other side of the table. So, it's a different experience. Thank you all for the opportunity. It is an honor.

For more information about our institute, visit our webpage at
ims.nus.edu.sg

Recent Applications of Model Theory

16 Jun–11 Jul 2025

From 16 June to 11 July 2025, the Institute hosted a program on "Recent Applications of Model Theory". The organizers contributed to this invited article.

BY TRAN CHIEU MINH *(National University of Singapore)*

The program spanned four weeks and was organized into two major thematic blocks: (i) Model Theory and Combinatorics / Valued Fields (16–27 June) and (ii) Model Theory in Complex Geometry / Differential Algebra (30 June – 11 July). Each block began with a tutorial segment (first week) to equip participants with the requisite background, followed by a workshop week where research talks, problem sessions, and collaborative interactions dominated.

A long-term visitor to the program was François Loeser (Professor Emeritus at Sorbonne University and Honorary Senior Fellow of Institut Universitaire de France), well known for his work on motivic integration and its connections to singularity theory, arithmetic geometry, and definability in non-Archimedean settings. His participation added continuity and depth to the program, and his lectures and informal conversations helped clarify how classical techniques continue to shape current developments in model theory and valued fields.

We were also fortunate to have Jinhe Ye (Nanjing University) serve as a long-term visitor and active organizer. His research bridges model theory, algebraic geometry, and non-Archimedean geometry, and he played an important role in supporting junior participants and helping build connections between local researchers and the broader international community. His efforts significantly contributed to the collaborative character of the program.

Together, Loeser and Ye helped sustain momentum across the four weeks and encouraged engagement across career stages, which proved essential to the program's scientific productivity.

A joint colloquium lecture by Lou van den Dries (University of Illinois Urbana-Champaign) offered an accessible overview of work with Aschenbrenner and van der Hoeven on the model theory of transseries and on Hardy fields. The lecture highlighted how these structures support a unified model-theoretic framework and demonstrated the relevance of these ideas to the study of differential equations, including unexpected historical links back to Sturm and Liouville. Many participants commented on the value of the talk in making advanced topics broadly understandable.

Throughout both thematic blocks, discussion and problem sessions complemented the formal presentations, often continuing beyond scheduled times. Participants formed small groups around specific questions, creating opportunities for exchange between fields that do not often meet directly—for example, between combinatorics and arithmetic geometry or between model theory and analysis.

One of the most meaningful outcomes of the program was the way it enabled conversations across different areas of mathematics—such as number theory, algebraic geometry, combinatorics, dynamics, and mathematical physics—while maintaining a clear focus on model theory. The tutorials and survey talks were designed to be accessible to researchers from various backgrounds, and many attendees noted that this helped them understand perspectives outside their primary specialties.

These interactions were especially valuable for early-career researchers, who were able to observe firsthand how methods and questions move between disciplines. Several participants reported that the discussions sparked new research directions and potential collaborations that might not have arisen in a more narrowly focused setting.

We hope that the program contributed not only to current progress in model theory, but also to strengthening the connections that enable new developments in adjacent areas. We are grateful to everyone who participated and helped create an atmosphere that supported open exchange and sustained scientific engagement.

Francois Loser, Distinguished Visitor

Lou van den Dries, Colloquium Speaker

IMS Graduate Summer School in Logic 2025

30 Jun–18 Jul 2025

Three lecturers delivered a series of lectures during this year's edition of the summer school. They were:

Anand Pillay | University of Notre Dame, USA
Theodore Slaman | University of California Berkeley, USA
Hugh Woodin | Harvard University, USA

Efficient Sampling Algorithms for Complex Models

14–25 Jul 2025

The workshop convened a diverse group of both international and local researchers for two intensive weeks of research talks and activities. It was structured around several core themes: sampling problems in application and data science settings, convergence of various sampling algorithms, acceleration of sampling algorithms and distribution optimization. The workshop successfully met its objectives by showcasing the new development in the fields, from both local and international speakers. Students have learnt from the lectures and interacted with senior researchers.

Singapore-Hong Kong Glorious Sun Symposium on Representation Theory

28 Jul–01 Aug 2025

This workshop is funded by the gift from the GS Charity Foundation.

This symposium is organized by the Institute for Mathematical Sciences and the Department of Mathematics at NUS, in collaboration with the University of Hong Kong (HKU), the Chinese University of Hong Kong (CUHK) and the Hong Kong University of Science and Technology (HKUST).

Representation theory, with its diverse applications in numerous other fields of mathematics such as harmonic analysis, number theory, and algebraic geometry, is as lively today as it was a century ago. The ideas and methods of representation theory are pervasive in much of the modern mathematics.

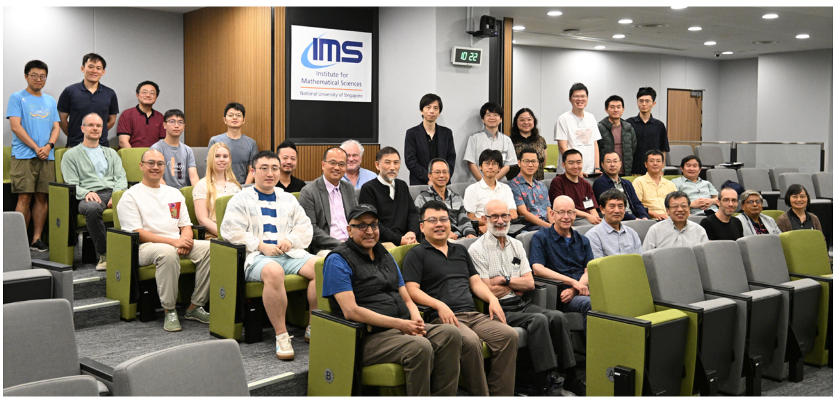
The aim of the symposium is to examine important recent developments on the structure, geometry, and representations of Lie groups, algebraic groups, and their various generalizations. A second aim is to promote and sustain efforts in talent cultivation, knowledge exchange, and international collaboration, among scholars/institutions in the Asia-Pacific region and beyond.

The highlight of the symposium is that it brought together a good section of the representation theory community from Singapore and Hong Kong, including many PhD students/young researchers. The event has promoted talent cultivation, knowledge exchange, and international collaboration, among scholars/institutions in the Asia-Pacific region and beyond. About half of the participants of the symposium were graduates and postdocs, and there were lots of interaction between them and with senior participants.

The 4th Australia-China-Japan-Singapore-US Index Theory Conference–Analysis and Geometry on Manifolds

4–8 Aug 2025

This conference belongs to the series "The Australia-China-Japan-Singapore-U.S. Index Theory Conference". The previous conferences in this series were held in China (2019), Japan (2023), Australia (2024). In this 4th conference, the focused topics are on index theory and geometry/analysis on manifolds. The five-day workshop comprised 27 invited talks along with informal discussion and collaboration and networking opportunities.

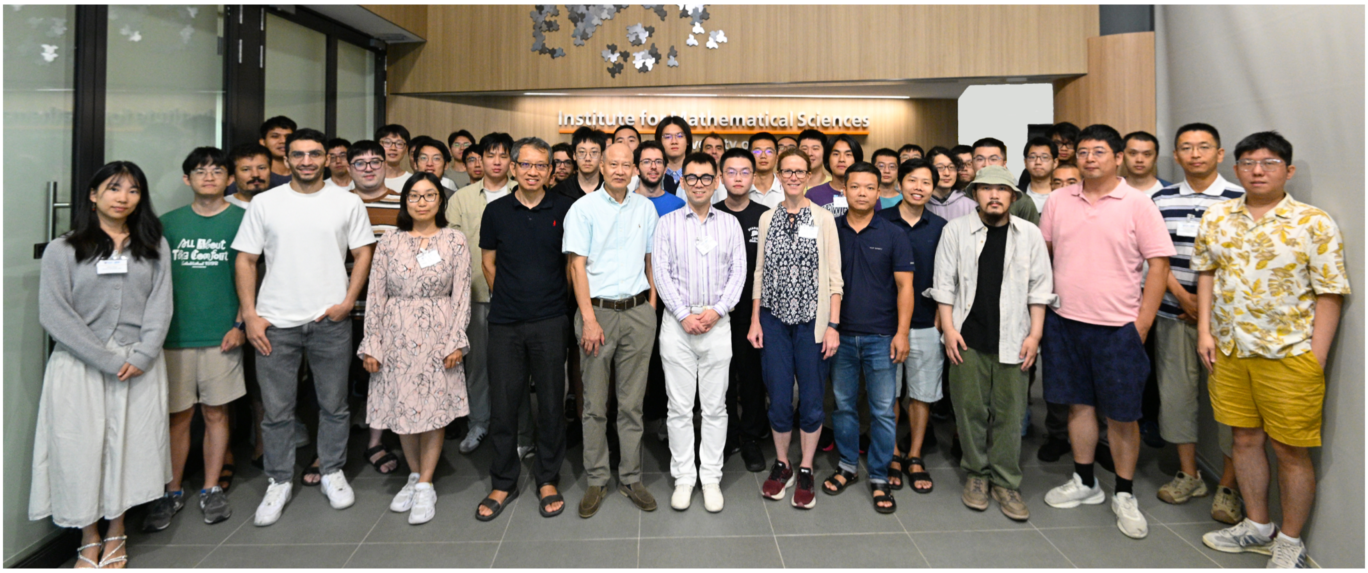


Arithmetic Dynamics and Diophantine Geometry

25–29 Aug 2025

This workshop is funded by the gift from the GS Charity Foundation.

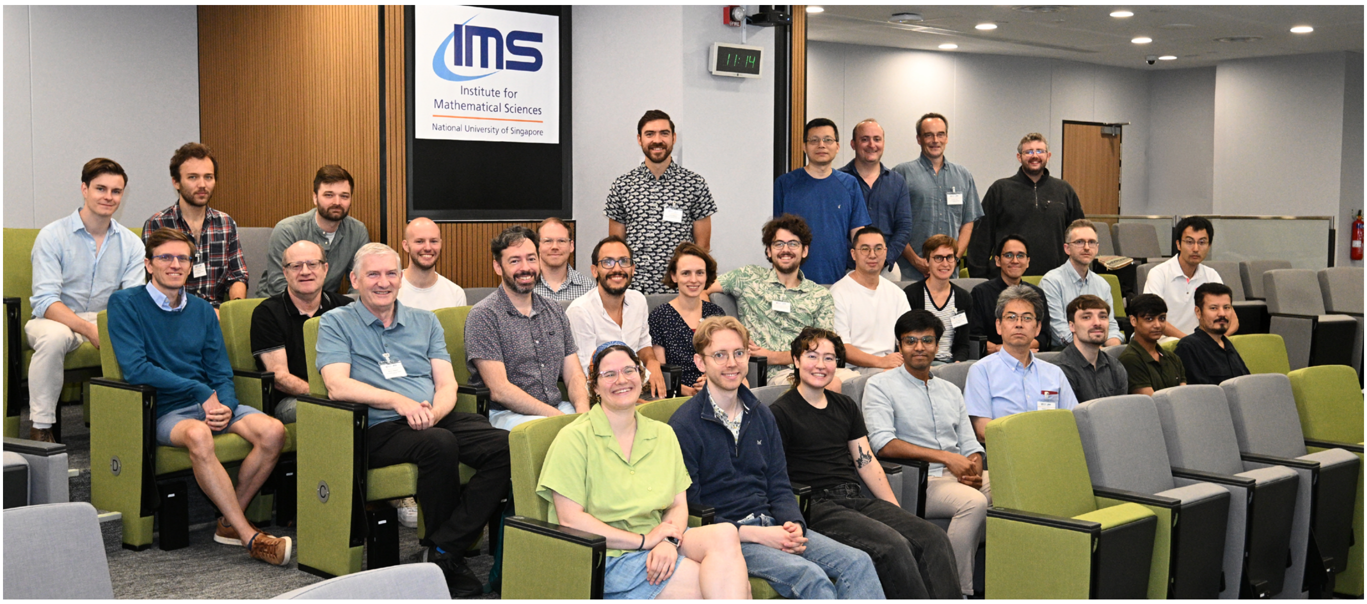
This is a preparatory workshop comprising three mini-courses and seven research talks with the aim of introducing local participants and early-career researchers to current developments in the field and prepare them for a three-week program in 2027. There were also six lightning talks presented by the PhD students.



Mathematical Methods for the General Relativistic Two-body Problem

11–15 Aug 2025

This week-long IMS workshop provides a chance for researchers from relevant communities to work together on solving the remaining challenges in the modelling and interpretation of gravitational waves from asymmetric binary systems. The workshop was structured along three main themes: theory, computation, and science. Each theme was addressed through a combination of invited talks and discussion sessions. The first of the invited talks in each theme will be a keynote talk that reviews the history and current status of research under that theme. Each subsequent talk will target a key challenge under each theme, and will be delivered by a leading expert on that specific topic. The discussion sessions will then expand upon the various topics covered by the invited talks, and will be chaired by relevant experts who will initiate and guide discourse among all the attendees of the workshop.



IMS Young Mathematical Scientists Forum — Statistics and Data Science

24–28 Nov 2025

The aim of this forum is to bring together a community of early-career scientists in the areas of statistics and data science from around the globe. This event will provide a platform for them to network, engage in enriching discussions, showcase their research, and exchange ideas with each other and faculty members from NUS.

There are 28 speakers for this workshop.

Upcoming Activities

Relative Langlands Program
29 DEC 2025–16 JAN 2026

Statistical Mechanics and Singular SPDEs
4–22 MAY 2026

Conformal Field Theories: Randomness and Geometry
18–29 MAY 2026

Quantitative Finance
1–19 JUNE 2026

Annual Summer School on Mathematical Aspects of Data Science
22 JUN–01 JULY 2026

Modern Challenges in Data Decentralization: Federated Learning, Differential Privacy and Communication Constraints
06–17 JUL 2026

Innovations and Challenges in Extremal Combinatorics
27 JUL–07 AUG 2026

Mathematical Foundations & Methodologies for Artificial Intelligence and Data-Driven Scientific Computing
11 AUG–30 SEP 2026

Interacting Particle Systems and Their Applications
05–16 OCT 2026

Mathematics and Artificial Intelligence for Weather and Climate
02–13 NOV 2026

FIM-IMS Joint Workshop on Mathematics of Data Science
16–27 NOV 2026

IMS Young Mathematical Scientists Forum — Statistics and Data Science 2026
23–27 NOV 2026

Pan Asia Number Theory Conference 2026
30 NOV–04 DEC 2026

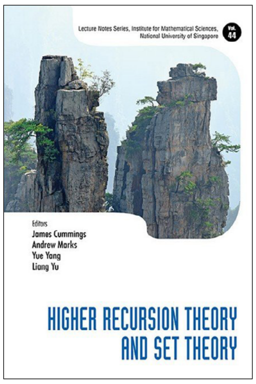
Optimization over Matrices: From Data Science to Quantum Computing
07–18 DEC 2026

For more information on these and other upcoming events, visit https://ims.nus.edu.sg/events/

Lecture Notes Series

VOLUME 44

Higher Recursion Theory and Set Theory



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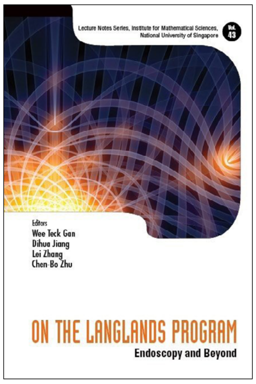

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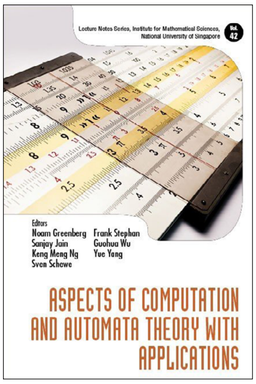

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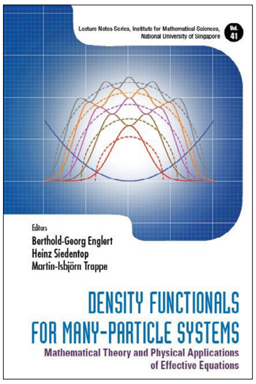

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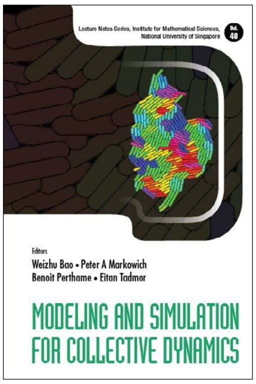

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