An Interview with Tai-Ping Liu

Y K Leong

[Courtesy Institute for Mathematical Sciences]

The following is a reprint of an interview with Professor Tai-Ping Liu of the Institute of Mathematics, Academia Sinica, Taiwan, which was conducted on 2 December 2010 at the Institute for Mathematical Sciences (IMS) of the National University of Singapore during his visit for the IMS programme on *Hyperbolic Conservation Laws and Kinetic Equations: Theory, Computation and Applications* (November 1–December19, 2010).

We would like to thank the Institute for Mathematical Sciences, NUS for permission to reprint the interview. From the introduction to the interview published in Issue 22, June 2013 of the IMS newsletter *Imprints:*

"Tai-Ping Liu had his undergraduate education in the National Taiwan University at a time when its department of mathematics was in its formative stages. From there he went to Oregon State University for his MS degree and then to University of Michigan for his PhD. Immediately after that, he joined the University of Maryland, where from 1973–1988, he established for himself a niche in research on hyperbolic conservation laws and shock wave theory. He then spent 2 years at the Courant Institute for Mathematical Sciences of New York University before moving to Stanford University in 1990. From a distinguished career in applied mathematics, he returned to Taiwan in 2000 as a Distinguished Research Fellow at the Institute of Mathematics, Academia Sinica. Initially maintaining links with Stanford University, he soon took up a full-time position at Academia Sinica and retired from Stanford as emeritus professor.

"Since his return to Taiwan in 2000, Liu has focused his research interests on the study of microscopic phenomena; in particular, on the Boltzmann equation in the kinetic theory of gases. He was instrumental in forming a research group at the Institute of Mathematics to work on the quantitative aspects of the Boltzmann equation in a direction (via Green's function) different from the approach of the well-established French School. He began organising learning seminars on the Boltzmann equation for researchers, graduate students and postdocs. He and his co-workers started research communications with a group of physicists in Kyoto University led by Yoshio Sone and began the quantitative study of the Boltzmann equation.

"Liu's research output consists of more than 130 single-author and joint papers. Among his important contributions in shock wave theory for hyperbolic conservation laws are the introduction of the Liu entropy condition for the admissibility of weak solutions, the deterministic version of the Glimm scheme for the construction of solutions, and with Tong Yang, the Liu–Yang functional for the well-posedness theory. In recent years, Liu and Shih-Hsien Yu (of the Department of Mathematics, NUS) initiated the Green's function approach in finding quantitative pointwise estimates for the Boltzmann equation. This work has contributed to the mathematical understanding of physical aspects of the Boltzmann equation.

"Liu was elected an Academician of Academia Sinica. He is Honorary Professor of various universities and an elected member of the Academy of the Developing World, TWAS (The World Academy of Science). In 2009 he was awarded the *Cataldo e Angiola Agostinelli* International Prize by the august Accademia Nazionale dei Lincei of Italy. [He was elected fellow of the Society of Industrial and Applied Mathematics (SIAM) in 2014.]"

Imprints: When was your interest in mathematics first formed? Did the school environment in Taiwan play an important role in shaping your interest in mathematics?

Tai-Ping Liu: My interest may be a little bit unusual. It was not really the schools that shaped my interest in mathematics. My interest in mathematics was really initiated by my mother. My mother could not read or write but she is talented in mathematics. One time my elder brother was six (I was 4 at that time), went to school and then the neighbour from the village came back from the parents' meeting and said that the teacher told them that my brother could not count. My mother found that incredible. For her mathematics was so interesting. How could my brother not be able to count? Of course, my brother never learned [to count]; my mother was so busy with the farming. So she took time off from her farming chores and began to teach my brother and me how to count and how to read the clock. We learnt counting that afternoon but I don't think we got the clock precisely. We counted to 100 and then backward to one. A few days later, my mother told her neighbour in the village, "My Tai-ping can be an accountant." For her the only thing she knew about mathematics was about accountants. She is a farm woman but she is very talented. We went to market and she would begin to teach the farm women how to count the price of chicken — you develop a certain scheme by interpolation and so on, all this by herself. So she had a very genuine interest in mathematics. That really had a very deep impression on me. I was 4 at that time and my mother had that curiosity and interest in mathematics. That was how I got started.

I: Where in Taiwan was it?

L: It was in quite a poor village in Taoyuan. Taoyuan is where the [international] airport is in. That part of Taiwan was very poor because there was no irrigation. Anyway, all the farm women were very surprised that my mother was helping them to deal with the city people and with selling their chicken and vegetables.

I: Was your childhood spent mainly in the farm?

L: All the time until I went to college.

I: What about in school? Any particular teacher …

L: No one in particular. You probably know, like in Singapore, we had exams all the time. Everyone wanted to pass entrance examinations, from junior high to senior high, from senior high to university. I had difficult times in passing examinations. I did pass but that was not the fun part of my education.

I: You went from a BS degree in National Taiwan University to a PhD degree in University of Michigan. Tell us how you took this path.

L: In 1968 I went into military service, and in 1969 I went abroad. There was no graduate school for PhD in Taiwan, not even in Taipei, at that time. In fact, there was no department of mathematics until nineteen forty something. Since Japanese time [1885–1945] there were classes on mathematical teaching and elementary mathematics but there was no department of mathematics. The faculty in [National] Taiwan University, which had the best department in mathematics, had only 4 PhDs. That was pretty good; in the physics department they had even less. We had no choice if we wanted to pursue mathematical graduate study. My undergraduate record was poor; I couldn't get to a good school, so I went to Oregon State University. After one year I transferred to [University of] Michigan.

I: Were you on a scholarship?

L: It was a teaching assistantship. At that time in the late 1960s the economy in US was good, the student population was growing and they needed teaching assistants. Even though my English was very poor I had to teach a class and grade the final exam in English. I learned English in one semester or so and then I was okay.

I: Was your PhD research work crucial in shaping your subsequent research interests?

L: Yes, it is. When I was an undergraduate there was a professor, Wang Ju-Kwei. He would come to our dormitory to chat, and one day we were talking. I said I was reading Paul Cohen's booklet on the continuum hypotheses and I couldn't understand it. (I liked set theory. When I went to Oregon State I even had a conjecture, and someone from Cambridge [University] remarked that the conjecture was still open.) In any case, when I was an undergraduate, Professor Wang told me that "set theory will make you famous but nonlinear PDEs (partial differential equations) are difficult and important." These were two good adjectives, and so I started [to study] PDEs. In Michigan University Joel [Alan] Smoller was the one studying PDEs. But Courant Institute in New York University would be the stronghold of PDEs, not Michigan, at that time. But I wanted to study PDEs, and Joel Smoller was a good advisor. He gave me one problem on uniqueness and well-posedness. I could not solve it at that time. I came back to it some 25 years later and eventually solved it. The one that I solved in my thesis was about entropy conditions.

L: Where did you go to after your PhD?

I: I went to University of Maryland. I was very lucky. Joel Smoller was very helpful. There was Avron Douglis (he co-wrote the important Agmon-Douglis-Nirenberg paper). He was in Maryland and he read my application. With Joel Smoller's help I got the one and only one job in April when the hiring season was already over. I got a tenure track position. In1973 it was very difficult to find jobs, even for engineers. So to get a tenure track at Maryland was the envy of everyone. They have an institute called IPST [Institute for Physical Science and Technology], which is a good place for computation, finite element method and so on.

I: In your work on kinetic theory and shock wave theory, what is the guiding light — the physics or the mathematics?

L: At first I tried to solve any problem I could find and could solve. Those were very analytical. Eventually I try to find a problem and formulate something which is physically relevant. Now looking back, I think I'm more interested in physical phenomena.

I: Wouldn't that have something to do with physics?

L: I'm not very knowledgeable in physics but I want to understand the physical meaning behind the mathematical model, and so I did a couple of things in modelling, something called "nozzle flow" and so on. I'm interested in physical phenomena but I'm a pure mathematician. I prove theorems and I'm interested in the basic mathematical patterns and more of the solution properties.

I: After Maryland you went to Stanford, isn't it?

L: I was in Maryland for 15 years. And then I spent 2 years (1988–1990) in New York University, Courant Institute [of Mathematical Sciences]. Then in 1990 I moved to Stanford [University].

I: It is understandable that applied mathematicians are only interested in those nonlinear partial differential equations that are of relevance in nature and applications. Is there any work on nonlinear PDEs done from a more purely mathematical and general aspect without being physically motivated?

L: This is a very good question. It's very difficult to say what is applied mathematics; for example, the things that applied scientists find appealing are not the kind of mathematics that is basically using the existing tools to solve, justify their models, to prove existence and things like that. It's not like that. It is the mathematicians, motivated by the physical concern, who come up with some basic mathematical techniques or basic mathematical framework and mathematical idea. And that basic mathematical idea may help to solve this physical problem or may help to understand the general case or what not. So in a way the pure mathematics part comes after one takes into consideration what are the physical phenomena one is thinking about. This line between pure and applied [mathematics] is very blurred. One would suppose that if the mathematician proves existence and uniqueness theorems, the engineer would be somewhat interested in them because this will give him confidence in the model and because it works well. But it would be even more interesting if a mathematician motivated by the question comes up with a new formulation, a new technique and something which is fundamentally new in mathematics, and they will find it very nice because then it can tell them what are the things they could look into, what are the possible experiments.

I: Are there any cases where the pure mathematician looks at certain nonlinear PDEs which are not considered by the physicists.

L: Yes, yes. This happens a lot. A classical example in shock wave theory would be the following. The engineer would look at the gas dynamics, the set of Euler equations for the shock wave and do all the right computations and one can see them in the classical book of Courant and Friedrichs [Richard Courant (1888–1972), Kurt Otto Friedrichs (1901–1982)] on shock wave theory. However, mathematicians always find the questions very difficult. In spite of all the efforts by applied mathematicians of the first rank like Prandtl, G I Taylor, von Neumann, [Ludwig Prandtl (1875–1953), Geoffrey Ingram Taylor (1886–1975), John von Neumann (1903–1957)], mathematicians find it difficult to go on; instead they go back to simpler models, something like Burgers' equation [Johannes Martinus Burgers (1895–1981)]. People like Hopf [Eberhard Hopf (1902–1983)], Oleinik [Olga Arsenievna Oleinik (1925–2001)] and Peter Lax would try to do a more general theory for a certain class of PDEs which have a certain general pattern, entropy conditions, Riemann problem and things like that. After this general study, the next generation would go back again to the Euler equations for the compressible flow. So this took a very large effort by many very good mathematicians. After the general theory is done to some level, one would go back to the Euler equations and try to see if one can solve the problems that could not be solved during the classical period. This is the challenge for the new generation. There are some preliminary successes.

I: Are the Euler equations hyperbolic? I believe there is a lot of work done on elliptic equations, isn't it?

L: Yes, the elliptic PDE community is a much bigger community and has a very long and illustrious history.

I: Is it true that the elliptic case is easier to treat than the hyperbolic?

L: Yes, but if it is easier, then it goes deeper and eventually it is hard. Ordinary differential equations are easier than partial differential equations but it doesn't mean that ordinary differential equations are easy. It could be a deeper theory. You have KAM [Kolmogorov–Arnold–Moser] theory and so on. So you have a simpler situation but you go deeper.

I: Why are there more people interested in elliptic rather than hyperbolic PDEs?

L: Well, I think elliptic has more tools, and in mathematics there is also the habit that if you go into an area and you can do something then you tend to stay in that area. This is a kind of inertia or habit. People would say that hyperbolic [PDE] is a very exciting area, even more exciting are equations of mixed type, but in academia you want to be able to do something. So a lot of people stay in elliptic PDE. Of course, it is also very important.

I: For applications, which one comes up more often — the elliptic or the hyperbolic?

L: That is difficult to say. Both come up. Hyperbolic PDE, if you look at specific problems, can be reduced to elliptic PDE. If you look at waves of certain frequency or simplification of flow, then it is elliptic. In a way, elliptic PDE is more basic.

I: I'm always intrigued by these two terms hyperbolic and elliptic; they are geometric terms. Is there a geometry of PDEs?

L: It is a geometric term — hyperbola, right? I don't really know who first invented this term. The simplest hyperbolic PDE is $u_{tt} - c^2u_{xx} = 0$. If one simply pretends not to learn calculus very well, this is like $(1/t^2) - c^2(1/x^2) = 0$, or $x = \pm ct$. So this is a hyperbola. The elliptic PDE would be $u_{xx} + u_{yy} = 1$, say. Then it's like $x^2 + y^2 = 1$; this is like an ellipse.

I: Does this mean that the name has no real geometric significance?

L: Yes and no. One time I was in Paris and I began to just test around, and I said, "Hyperbolic is *yang* and elliptic *yin* (the Chinese term *yin-yang*)" but in English "elliptic" seems to mean "mellowed, smoother;" while "hyperbolic" means "wild". Mathematically this is true. Elliptic PDEs are always smooth and nonlinear hyperbolic PDEs result in shock waves. So maybe without knowing it, this "hyperbolic-elliptic" terminology makes sense.

I: How much has the computer helped in the "complete" solution of nonlinear PDEs in general, and the Boltzmann Equation in particular?

L: The computer is very, very important. In fact, the computer is one of the tools, which makes the general applied mathematics programme complete. In old times, the Chinese had the "*suan-pan*" (abacus). This was the computer at that time; great mathematicians even wrote treatises on this. With it you can do a lot more computations and so on. This can lead to other thinking in the hands of great mathematicians such as the Japanese Takakazu Seki [(1642–1708)] in advancing Japanese arithmetic (*ho-suan*) [*wasan*]. There is the algorithm for matrices and for manipulating systems of linear equations like the Gaussian elimination. The computer is definitely very important but it should be combined with traditional analysis or thinking.

Last time I was working on the Euler equations in multidimensional shock reflection and a former student of mine Volker Elling did some computation and the result was very surprising. The computer generated an interesting problem and we finally did something. The computer is an integral part [of research].

I: Is it the simulation part?

L: The computer is used for simulation. But in this particular instance, the computer was asked to analyse a certain problem. Sometimes the computer is used to find out what the solution looks like and whether the model gives the right phenomena and things like that. That is the use of computers in general but there are also situations where the computations lead to a different mathematical thinking. The most famous example is soliton theory.

I: Was it simulated by computer?

L: Yes, yes. They [Enrico Fermi (1901–1954), John R Pasta (1918–1984), Stanislaw Ulam (1909–1984) and Mary Tsingou] had this computation. The paper on the computer simulation was published in 1955 after Fermi's death. The physicist Fermi was so surprised by the result of the computation. But even in doing the computations you need to have a deep analytical thinking and Fermi had a deep analytical thinking. So when he saw this solitary phenomenon, he was surprised. Otherwise some people doing this computation would not be surprised by the result and it would not stimulate new thinking. So analytical thinking has to precede computation and new analytical thinking would come after the computation. You have to have that interaction.

I: In that case the computer has actually led to new theorems?

L: That's right, new theorems. In that case, solitary waves are really a revolution in mathematical sciences.

I: If it had not been for the computer, would solitary waves have been discovered?

L: That is hard to say.

I: I understand that the Boltzmann equation can be used to study galaxies but not the development of the cosmos in the early stages of the Big Bang. Can the equation be modified to incorporate quantum effects?

L: This is a hard question to answer. In one aspect there is a belief, in fact, there are models which incorporate quantum effects into the Boltzmann equation. However, it is more urgent to study the early stage of the Big Bang where quantum effects are very important. Afterwards, you have a lot of stars, galaxies and there are so many of them. Your question perhaps is: what happens in the time period between these two [stages]. That is a very difficult question because there are other effects, relativistic effects and so on. Let me turn this question around. Studying the development of the cosmos is a huge programme. This consists of all the physics because at the beginning it was so dense and at the end there are so many stars. You have to have fluid dynamics coming into play, kinetic theory, relativistic theory and what not. We are in a situation where there is explosion of human knowledge. Exactly how much we need to learn, what direction of research we should go into — that is a very serious question. Ideally we need to understand all the physics of the basic things. But our time and energy are finite. So we have to be selective in what we learn. You mention this cosmos — a hard — question. This is one example where perhaps we need to rethink what we need to teach our students, what our students need to learn. You are talking about quantum effects and the Boltzmann equation, and there are many things in between. It could be very exciting research in the future, but incorporating quantum effects into the Boltzmann equation by itself would not solve the equation; I don't think so. That's too much to hope for.

I: Will a complete solution of the Boltzmann equation lead to a clearer understanding, if not the solution, of the Navier–Stokes equations?

L: I guess the answer has to be yes, except for the fact that the Boltzmann equation is so much harder because it contains much more phenomena and has many more different scales. When we say the Boltzmann equation is much harder than the Navier–Stokes, people could misunderstand me because if you want to prove existence of the solution of the Boltzmann equation, it might be easier than the Navier–Stokes equations. But the point is not to construct the solution. The point is to understand the properties of the solution. In fact, your question is very good; it says "a clearer understanding". The Navier–Stokes equations represent a certain physical situation, and in that situation, the Navier–Stokes equations are good approximations of the Boltzmann equation. So the answer is yes, but a complete resolution of the Boltzmann equation is a difficult programme.

I: Is the Boltzmann equation more general than the Navier–Stokes equations?

L: Yes, the Boltzmann equation is more general.

I: Does that mean that if the Boltzmann equation is solved, then the Navier–Stokes equations will be solved?

L: The thing is what do we mean by "solved it"? To solve it, we have to have a good understanding of the solution on various scales. The Navier–Stokes equations are mainly at certain scales. In order to understand the solution of the Boltzmann equation in that sense is extremely difficult. Another thing is that the Boltzmann equation is more general than the Navier–Stokes equations around the boundary for example and there are very rich phenomena.

Shih-Hsien Yu here and I worked on the boundary and nonlinear waves for the Boltzmann equation. Around the year of 2000, Shih-Hsien and I tried to generalise the techniques developed for the conservation laws to the Boltzmann equation. We had some successes. However, we soon learned from the Kyoto School that the most interesting aspect of the kinetic theory is that it can model physical phenomena that the fluid dynamics equations such as the Navier–Stokes equations cannot. So we started from the basics, the construction of the Green's function, which allows us to gain quantitative understanding of the Boltzmann solutions. We are now able to study the bifurcation phenomena of the transonic condensation/evaporation, a problem of interest to the Kyoto School. Of course, within the whole scope of the kinetic theory, we have barely scratched the surface.

I: In that case, they should give the (US) one million-dollar prize to the Boltzmann equation rather than the Navier–Stokes equations.

L: I would agree with that. [*Laughs*] The Boltzmann equation is derived from more first principles in physics than the Navier–Stokes and is more basic.

I: The Boltzmann equation is much later than the Navier–Stokes equations.

L: Much later. The Navier–Stokes equations are phenomenological ones. The Boltzmann equation, thanks to the great genius of Boltzmann, is derived from very first principles.

I: In 2000 you went back to Taiwan (Institute of Mathematics, Academia Sinica) after 27 years of distinguished careers in the US. I believe you still have close links with Stanford University. How do you maintain the dual roles in two places that are geographically so far apart?

L: This question is very easy to answer. The answer is that now I am full-time in Taiwan. I'm professor emeritus at Stanford. I tried to have a dual role, as you put it, for a while (three or four years), but then I realised that it is not so much about giving your expertise to and teaching students. Doing research, I realised that the cultural and general attitude of people is really very important. There must be a reason why modern science was founded in the west and not in the east. For whatever reasons there was a different culture and different attitude. In order to have some effect, you need to be in one place most of the time and try to change the cultural attitude a little bit. For example, in traditional Chinese culture if you are dean or president or, in old times, if you were a *chu-jen (juren)*, *chin-shih (jinshi)* and *hanlin* and so on, then it is written on the front of your house and this is the most important thing. Never mind what exactly you have done; it's this title that is important. So it is not so much the curiosity driven kind of thinking. Like my mother, she never thought that she would be called the number one mathematician. Yet she has a natural interest and curiosity in mathematics and wanted to share her knowledge with other villagers.

I: If she had the education and opportunities, she might have become a mathematician.

L: She might have. She was talented. She could have been good in school and become a medical doctor or whatever. In spite of her lack of formal education, she had interested herself in mathematics. But the prevailing culture is not favourable to science from the point of view of traditional Chinese culture. This cultural thing and general attitude towards research demands a full time interaction and living together, and so I say I will retire from Stanford.

I: Do you think there is a difference in attitude towards learning between the east and west?

L: It is too big a question for me to answer. Even for myself, I feel that I am very much a product of the traditional Confucian system. We have taken some classics. When we look at some expert mathematicians, in a way we take them as the saints and therefore we want to solve some problems which they propose and their famous open problems. But that is much less so in the west, I think. In the west, young people, even though they may not know a lot, think they can be the equal of anyone. I think that's a different attitude.

I: If a student is interested and shows equally good potential in both pure and applied mathematics, what acid test would you give him or her to help in deciding whether to be an applied mathematician?

L: This is a very difficult question. For example, some of my friends are very good mathematicians and they were actually graduates or undergraduates from engineering school but they decided to go to graduate school in mathematics. It turns out that they are the purest kind of mathematicians; they are born to like mathematics in spite of the fact that as undergraduates they were engineers. Then, people like me, I was an undergraduate and graduate in mathematics but deep in my heart I like to understand physical phenomena. Also, these days there are a lot of possibilities — mathematical physics, mathematical chemistry, mathematical biology and so on. In general, I would say that the thesis advisor is very important because that is when you start your research. In general, I tell the student, "Never mind what you studied for your undergraduate or master's degree. When you go to a good school you take the courses, and the one who everybody says is a good mathematician and the one whose courses you can understand, that person is your advisor." Otherwise I don't have a more precise answer to your question.

I: Do you have many graduate students doing PhD under your supervision?

L: Not many. I have somewhere between 10 and 15 so far who had a PhD from me. I have some very good students and I think the credit should go to their parents and others. Some eventually very quickly became my teacher.

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