Fanghua Lin: Revolution, Transitions, Partial Differential Equations

Fanghua Lin

Interview of Fanghua Lin by Y.K. Leong

Fanghua Lin (林芳华) is world-renowned for his important contributions to classical analysis and its applications to nonlinear partial differential equations.

Lin graduated from Zhejiang University, China in 1981 and obtained his PhD from University of Minnesota in 1985. He was an instructor at the Courant Institute of Mathematical Sciences, New York University from 1985 to 1988 before going to the University of Chicago as full professor in 1988. He returned to New York University in 1989 and was awarded the Silver Professorship in 2002 by the Courant Institute, where he continues to produce outstanding research in the Courant tradition of hard analysis applied to nonlinear partial differential equations.

His research output includes more than 160 research papers and three books of lecture notes. Even before completing his doctoral studies at Minnesota, he had already made a reputation for writing many research papers. Among his many contributions are his fundamental work on the Ginzburg-Landau equations with a small parameter and his deep results on harmonic maps and liquid crystals.

His honors and awards are numerous, notably the Alfred P. Sloan Research Fellowship, the Presidential Young Investigator Award, AMS Bôcher Prize and S.S. Chern Prize (China). He was elected to the American Academy of Arts and Sciences and has been invited as distinguished speaker at important scientific meetings and conferences in U.S., China and Japan and in major universities throughout the world. Besides supervising a number of doctoral students and post-doctoral fellows, he serves on the editorial boards of leading mathematical journals, such as *Communications in Pure and Applied Mathematics, Analyse Non Linéaire, IHP, SIAM Journal of Mathematical Analysis* and *Journal of Differential Geometry.*

Having established himself as a world leader in his field, Lin has not forgotten the moral obligation he has always felt towards China, his country of origin. For the past two decades or so, he has been actively engaged in promoting international scientific contacts and opportunities for the mathematical community in China. At a different level, Lin has a close association with NUS's Department of Mathematics and Department of Physics. He was co-chair of the organizing committee of the IMS program on *Bose-Einstein condensation and quantized vortices in superfluidity and superconductivity* held from 1 November to 31 December 2007. More recently, he also served as a co-chair of the organizing committee of the IMS program on *Mathematical Theory and Numerical Methods for Computational Materials Simulation and Design* (1 July – 31 August 2009) and of the program's Summer School (17 July – 19 June 2009).

He was interviewed by Y.K. Leong at IMS on behalf of *Imprints* on 10 December 2007. The following is an edited and enhanced version of the transcript of the interview in which he offered a glimpse of a student's life during the Cultural Revolution that swept through China during the 1970s and how he emerged from the throes of that period as one of the first batch of students to enter the reopened gates of Chinese universities and how he was sent as one of the pioneering group of students to do graduate studies in the US. In the interview, he exuded a passion for research and open-mindedness towards learning in mathematics and science.

Imprints: You went from Zhejiang University to University of Minnesota for your PhD. Why did you choose Minnesota and what was the topic of your PhD?

Fanghua Lin: The answer is very simple. I didn't choose Minnesota. My professor at Zhejiang University chose it for me. That has a lot to do with the history of the department. The mathematics department of Zhejiang University is historically very important in developing Chinese mathematics and has trained a lot of Chinese mathematicians. At the time I entered university, it was February 1978. The department had only a few professors. But, the professors made a decision to choose the top 10 students of the class of 77 and 78 and send them to US and Europe for their PhD and to get them to return to Zhejiang University. This was a rather strategic plan but as in many other schools in China, the plan was not carried out. Why would such an idea come up at Zhejing University at that time? It has to do with the history of the department. The early professors of the department, Professor Chen Jiangong and Su Buching, both got their PhD from Tohoku University, Japan around 1930. At that time, you don't see so many PhDs in China. With only two PhDs, it was already such a great department. So the professors thought then, if we have

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10 PhDs, we will, you know, be even better. Because of this decision, I was chosen among the ten to be sent to US to study partial differential equations. Going to Minnesota was also decided by the professors. The professor visiting Minnesota then was Dong Guangchang who was doing PDE, and the chair of the department then was Professor Guo Zuorui. It's very hard to say I really chose the topic of partial differential equations. When you get to US, you basically follow whatever you like to study. It turned out that my thesis topic was not on partial differential equations. It was more on a geometric variational problem and my PhD advisor was Robert Hardt.

I: That was in the late seventies?

L: I went to Minnesota in 1981. I entered Zhejiang University in 1978. I had basically three years of undergraduate study, the last half-year learning English mostly.

I: Was it on a national scholarship?

L: I got a teaching assistant fellowship from University of Minnesota, so I had to spend half the time working and half the time studying. It was kind of challenging but interesting.

I: That was after the Cultural Revolution?

L: Yes, I was in the first class (the 1977 class) that entered university after the Cultural Revolution, but the class entered university in the spring of 78.

I: Did the events of the Cultural Revolution affect your studies before you entered university?

L: Certainly, I never really studied at all in elementary school or for that matter in high school. Actually, thinking back, I liked it because it was completely free. There was no homework, no exams. There was no serious exam before I took the university entrance exam. The elementary school then was like a political camp.

I: How did you study your mathematics in high school?

L: I entered school at the third grade basically because the first two years was the start of the Cultural Revolution. We didn't really learn anything except Chairman Mao's quotations. At the fifth grade, we were starting to study the solving of equations. A lot of my classmate experienced difficulties with the problems, but I found them particularly easy. I found it started to get interesting because there was something else, not just something mechanical. My elementary school teacher thought that I did have some natural talent. I spent a year or two in elementary middle school (sixth and seventh grade) studying a lot of mathematics and physics too, basically by myself. I happened to meet a very good teacher. He was our physics teacher and he gave me the books published before the Cultural Revolution and some special books in mathematics. I read most of them and found them not too difficult to learn from. But then I really didn't spend too much time studying in high school because it was still during the Cultural Revolution and you didn't see the end of it. You didn't see much future then and therefore you didn't do much work – eventually you would become a farmer. But I enjoyed the free time however, never really followed any rules or studied anything systematically.

I: Maybe it's not that bad for creativity.

L: Yes, in some way. Because of that I was always interested in thinking about problems and trying to solve them by myself instead of reading them in the books. Of course, there are advantages and disadvantages.

I: Except for a short stint at the University of Chicago, you are essentially based at New York University. What is the attractive factor of NYU?

L: Many reasons. First, New York City is very unique. You feel at home there. Everybody feels at home there. It has so much to offer: music, art galleries, museums, movie theatres, restaurants. It's just fantastic. I'm a lazy person. I always want to get things very easily – live near to food, get to work easily and everything should be very accessible. The other thing is that the Courant Institute is one of the greatest institutes in the world. You feel very warm and you find a lot of colleagues, so educated, and from different cultural backgrounds. I feel the friendliness.

I: Did you feel any culture shock?

L: I didn't feel much of a culture shock. I'm always very open. When I was a graduate student at Minnesota, I interacted mostly with students from other countries, a couple from Hong Kong. Of course, the culture thing is a much deeper matter. As time passes, I find I'm still very Chinese – some things never change. However, you don't feel like a foreigner at the [Courant] Institute. People simply respect you if you do a good job. Faculty is very happy when you accomplish something. People congratulate you and so on. This is a particularly friendly place. You don't have to prove to your colleagues that you are good or so excellent, which sometimes happens in some other places. I like other places too, like Chicago. I love the University of Chicago very much. It's a very English society – gentlemen, treat you very nicely. It's great. The weather is very tough however, particularly at the time I was there. I enjoyed it very much and that is why later my family went back there again. I've been at Berkeley for half-a-year as a post-doctoral, Princeton for one sabbatical year and the Institute for Advanced Study for half-a-year as post-doc.

I: Have you gone back to China since then?

L: Yes, many times. My first return to China was in 1989 just before the Tiananmen Square incident. That was a

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cornerstone. That was my first visit to China after 8 years in the US. A lot of things were changing and it was very nice. After that I practically went back to China every year and spent a couple of months; in recent years, always two months or more. I go back in summer to give courses to graduate students and try to find post-docs and good students. At the beginning, none of us who became professors would return to China, we would settle in the US. It's hard to say whether it's good or bad. But I think, in general, I'm still very positive that we can still go back to serve the country in some way. China has changed drastically over the last ten years in particular. If it was 20 years ago, I would have gone back to China already. When I graduated in 1985, it was a very different time.

I: Your research spans both pure and applied mathematics. Were you already interested in applications to physics right from the beginning?

L: Physics is always a subject that I liked ever since high school. In the college admission test, I got my best score in physics. But I never really got seriously interested in physics. I always see myself as a mathematician. I am always interested in mathematical issues which may or may not be related to natural phenomena or science. After many years at the Courant Institute, my philosophy and point of view have been changing. To me, as your age grows, you realize that when you are young, whenever people tell you some problem or you see a problem, you just jump in and try to solve. But you gradually realize that there are simply too many problems, infinitely many problems, and "a man should know his limitations". You cannot solve all of them. And therefore, you have to be very selective. As age grows, your view changes and you spend more time selecting the problems. To me the type of problems is very important. At any point of time, there is only a small set of really interesting problems. When you look at the publications 50 years ago, you say, "Oh, why are the papers on such bizarre subjects?" You can be sure 50 years from now, people looking at our publications will say the same thing. What this means is that the problems that are interesting now may not be interesting in the future. So one has to choose a problem that is not just mathematically interesting but also relevant to what is going on in science. Science is always developing; it's not just imagination and creativity, it's driven by practical needs.

I: How do you select your problems you want to work on? Do you get them from journals or do you talk to people?

L: You choose a problem depending on your training, background and interest and by reading and talking to people – for me a lot of time reading non-mathematical articles in *Nature*, *Science* and so on. You know what is relevant and that is very important. Of course for some mathematicians, you can simply ignore what is going on in the world and do whatever you like. You know, science develops so drastically and is so diverse that if you don't pay attentions to the whole picture you are going to miss out quite a lot.

I: Courant Institute is mostly inclined to applied mathematics, am I right?

L: Yes, but we have faculty in both pure and applied mathematics, probably stronger in applied. But we have a very strong group in pure mathematics too. In the University of Chicago, I was regarded more as an applied mathematician, but at the Courant Institute I'm probably more on the pure side of mathematics.

I: Would it be fair to say that partial differential equations used in modeling physical phenomena are often based on simplified and ideal assumptions. As such do these equations actually reflect reality?

L: First of all, I would like to answer in a philosophical mode. Absolute truth or reality doesn't exist or is probably not so important to us. Even if it exists, when we try to understand natural phenomena, it's through our perception. So when we talk about reality or truth, it's always an approximation. If we know the absolute truth or reality, we probably understand the problem so well and therefore the problem is not worth studying. We model by using partial differential equations and other mathematical methods. A model is a model. Therefore you have to simplify and make certain fair assumptions. But between different models one can sometimes distinguish between good models and not so good models. So what is the distinction? First of all, we want simple models because we can understand simple things better. If the model is as complicated as the real problem, what is the use of the model? A good model should always capture the essence and characteristics of the issues you want to address. A minimum requirement of a good model is to be real enough. How much you want is a practical trade-off for your needs. Yes, partial differential equations always use simple models. The good thing is that most of the time when we understand these models, we also understand the general situation.

I: Would you consider modeling more of an art rather than a science?

L: It's a bit of both. You cannot forget the fundamental issues you want to capture or understand; this aspect is science. How you do it – you do it nicely and elegantly or you just do your very best – that is art, and it also depends on the technology available.

I: Is there any recipe for doing good modeling?

L: I'm not a specialist in modeling. I think it's like doing physics or mathematics. Unconsciously, people use some very basic principles.

I: Which type of problems is more tractable? Evolutionary problems (i.e. parabolic or hyperbolic type) with given initial values or elliptic boundary value problems?

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L: It's very hard to make a distinction or comparison between different types of problems and say one is easier or more difficult than the other. The problem can be extremely difficult for simple questions. The problem can be relatively simple even though it addresses a complicated system of equations. Really it is what you want to achieve. If you want to access a large space of phenomena, even for very complicated systems, it is easy to come by. If you want to understand very delicate, very detailed information of some specific issues, then you have to look into very detailed characteristics of the problem and it can become difficult to solve. I think the difficulty depends on the issues you ask or the final conclusion you want to draw from the problem. It is not the type of problem (stationary or evolutionary) posed that is easier or harder.

I: Are the difficulties merely technical?

L: Some problems technically could be very tough. Other problems you simply don't know how to approach. In that kind of situation, one has to be very original and have deep insight into the problem.

I: It seems there is a tendency to resort to computational methods when analytical solutions seem to be beyond reach. Is this a new paradigm in applied mathematics? Has this approach yielded new insights or breakthroughs?

L: In some way, I could say "yes". Historically, computation is an auxiliary tool. When we have difficulties understanding something, let's do some computation or we do some computations to verify. So therefore computation is always something supplementary to facilitate certain ideas and prove they work or do not work. With the development of science and technology, the situation is changing drastically, particularly over the last 10 or 20 years. The use of supercomputers in modeling is not only to understand certain issues and to do computations or numerical simulation; it is also becoming a preliminary kind of science. For example, in earlier times, we do a lot of experiments in materials. Then from the experiments we propose some empirical model according to our physical intuition and theory. From the equations there could be something new. Then you test the theory with the experimental data and then you modify and add in more parameters and so on. This is the classical way of doing things. But now it seems you don't go through the theoretical part so clearly at the preliminary stage. Some simply feed into the computer various parameters, effectively hundreds of experiments, at the same time. So you have much more data collected and how to handle these data and from these data get a reasonable mathematical model has become a sensitive issue in itself. The use of computers to model has become a necessity. It is not necessarily separated... I have this wonderful idea that I want to test using computers. No, you can also get the idea from the computational experiments.

I: So simulation will give rise to new ideas?

L: Right. Simulation itself will give insight into the problem as well as understanding of the problem. The computer can generate tons of data. Afterwards, you have to understand what these data are. You use statistical methods or other methods. With some models you can still do a testing. This process creates a lot of new mathematics. Some of the ideas that people have these days are from ancient times and they dropped out after Newton-Leibnitz's calculus worked so nicely and simply in ideal situations.

I: But Newton did not depend on simulation to come up with the calculus.

L: Mathematics does have a unique position in science. Sometimes it's indeed surprising that something that you somehow have purely from the imagination and logical deductions has to do with the real world. It may be because imagination is a part of the real world.

I: Is there any discovery of yours that you find intuitively surprising?

L: It's very hard to say. Sometimes when you prove something or create something you find it surprising. But after several years of deeper thinking and understanding, you realize that it's so natural. I find most of the things I did are indeed very natural. At a certain stage, something happens and one is surprised once in a while. For example, sometimes by looking at seemingly more complex problems, one can do much better. In the beginning it was a kind of surprise, but after years of thinking and understanding one realizes that it is very natural.

I: In your work on partial differential equations, do you put much emphasis on the beauty of the model rather than the technical details?

L: I'm personally much more interested in the ideas and methods that solve the problem. Sometimes there are certain technical computations you cannot avoid and you have to be able to handle such difficulties. Sometimes the technical things are the real things. But sometimes you are interested in the idea and the approach to the problem – which may be more beautiful and useful.

I: I believe some physicists believe that if a theory is beautiful, it must be correct.

L: In a certain way. If something is very simple, very beautiful, you say, "It's fantastic." Sometimes one can understand it from basic mathematics. But simple things could involve very deep and complicated mathematics. So you never know.

I: Is the Ginzburg-Landau equation completely solved in dimension 2?

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L: There are a lot of papers and books on the Ginzburg-Landau equation in 2-d and 3-d. In a certain sense we understand quite a lot about these equations and their solutions. In partial differential equations, it seems you have so many equations to work on. But the good thing is that only a few equations are really fundamental and interesting. These equations will appear now and will appear later again and again. So I won't say that we completely understand the Ginzburg-Landau equations. It depends on what kind of questions asked and what kind of issues you want to address. For example, the Laplace equation – people worked on it for 200 or 300 years, we practically understand every aspect of it, but once in a while people will tell you something new about the equation. Because the Ginzburg-Landau equation is one of the fundamental equations to model basic physical phenomena, it is a nonlinear partial differential equation and I think it will appear again and again. Even for 2-d, there are some issues we don't understand. So I won't say it's completely solved.

I: The Navier-Stokes equations are extremely difficult to solve. Is it due to the fact that it is difficult to formulate radically new concepts within the framework of classical physics?

L: They are probably one of the most fascinating partial differential systems I know of. I have personally spent some time thinking about them, but not really a lot because you get nowhere. We realize there are a lot of difficulties in understanding the issues but we don't know how to overcome these difficulties. Unlike a lot of mathematical questions, when you really understand the real difficulties of the problem you may try to find a way to overcome them. And sometimes you are lucky to solve them. For this one, looking at the difficulties from various views and angles, we understand it in a certain way but we don't know how to overcome them. Is it because of mathematically technical reasons or is it the formulation at the fundamental level? I really don't quite know. I won't be surprised maybe some day some people say, "This system is only one part of a grand physical system which may be solvable even though we don't understand this particular one." This may go back to the fundamental level of formulation of the problem. Maybe there is something missing from the very beginning. On the other hand, the Navier-Stokes equations are, from the mathematical point of view, already consistent and well-posed. In other words, it is a closed system and you don't need extra information from outside. But however, sometimes extra information from outside could lead to some fundamental or more radical ideas which may assist.

I: Is the existence part of the problem partly solved?

L: We understand existence under the so-called weak conditions, but we want a classical solution. People always tell you that it may not be important to real physics. But it is a very intriguing question, a simple mathematically formulated problem, and we simply do not know the answer. It's very mysterious.

I: But the physicists do not worry about the existence of solutions.

L: To the physicists, the physical system must exist but they may not talk about classical solutions. It's hard to say what one should really believe in. This is part of the difficulty.

I: The Navier-Stokes equations are classical and not quantum mechanical, isn't it?

L: Yes, there are many, many ways to derive that equation. Of course, from the mathematical point of view, you can forget whatever way you derive it.

I: Do the Navier-Stokes equations apply to all fluids?

L: Yes, but there are compressible or incompressible fluids, or visco-elastic fluids... One can derive similar equations in many real problems of physics.

I: Have they been extended or modified in some sense?

L: There are more complex forms of these equations and modified forms. But for the classical Navier-Stokes equations somehow the modified equations are couched in such a way that it is no longer interesting because the very difficulties of the original equations disappear. This is not the way to find a mathematical theory.

I: What are the chances of solving the equations in the next 30 years say?

L: It's very hard to make a prediction. Personally I don't like to make a prediction. But I would say it cannot be done in a relatively short period of time and may last a long time. For generations the best trained minds have attempted the problem and it has defied all attempts.

I: It seems that applied mathematicians are generally more gregarious in research in the sense that they collaborate more among themselves than pure mathematicians do. Why is that so?

L: Yes, I also tend to believe that applied mathematicians are more gregarious than pure ones. I'm not surprised. But you also see more and more pure mathematicians joining efforts together to solve problems. It depends very much on the nature of the problem. Traditionally the mathematician works individually. But as the problems become more multidisciplinary and complex, it is natural to have groups of people working together to attack a common problem. In applied mathematics, the problems are by nature across the fields – it's mathematics applied to other sciences – and come from different disciplines. So it's not surprising. It should be this way.

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I: What is your advice to the beginning graduate student who is interested in both pure mathematics and applied mathematics?

L: Just because you want to be cross-disciplinary, you try to understand a little bit from each field. That is, unfortunately, not the way. It is like that you are trying to understand the art better than they do. There is no reason you would do a better job than the experts from different fields; they can do much better than you do. Even if you do cross-disciplinary research, you have to be a specialist in one or two things – the insight, the ability to separate the problems. When you have that, you need to have an open mind, to learn things and get interested at the beginning. Even if you work in pure mathematics you shouldn't work alone in research. I may be more practical in a certain way but I'm interested in what is going on in pure mathematics and science and so on. If you have this kind of attitude and you specialize in one field, have an open mind in expanding your horizons to learn more things, you will do a very good job. People from different disciplines think in different ways. It's very interesting and good to know. Intellectually it's very satisfying. One would also find that there are many things in common.

I: Do you have any students?

L: At the moment I have four students – two are going to graduate, maybe next year. I have maybe 10 or 11 students who have already graduated. I also have some post-docs working with me.

I: Do you think that Chinese students are inclined more towards the applied side?

L: I don't think that way. They could be neither pure nor applied. The training in China generally seems to have certain disadvantages because students tend to focus and concentrate very early on one very special topic and remain so for most of the time. Once in a while you see a very top student but he is narrowly focused. If you are too focused very early on, then your ability will be very narrow also. If you don't expand your horizons and knowledge, then you lose your chance. Later on, when you see a problem you would say, "Oh, it's outside my field." Always take more topics courses. It shouldn't be that you take a course just to apply it to something. Of course, when you work on a problem, you will use whatever tools you have to solve it.

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