Interview
with Shing-Tung Yau

Interviewer: Shinobu Hosono

Found a Theorem Soon after Entering UC Berkeley Graduate School at Twenty

Hosono: It's my great pleasure to have this opportunity to have an interview with you today. Thank you very much for sparing your time. Needless to say, you are one of the greatest mathematicians in the world.

Yau: Thank you.

Hosono: At the same time, I can say that you are one of the most important persons in the universe because of Calabi–Yau manifolds in string theory.

Let me start by asking how you became interested in this special manifold. I brought a book you wrote, *The Shape of Inner Space*.* According to this book, it says that you were in the second year of the Chinese University of Hong Kong when you decided to go to the U.S. At that time, were you already interested in differential geometry or physics?

Yau: During that year in Hong Kong, I was much more interested in a subject called functional analysis. I spent a lot of time studying that there. I had some education in geometry but not that much, mostly in classical geometry studying surfaces, curves in three dimensions. I knew nothing about what a manifold means, so I had no modern knowledge about geometry, but I gradually learned later.

Hosono: How about physics?

Yau: I had reasonably good training in physics in the Chinese University of Hong Kong, but I had pretty poor training in physics in high school which I regret very much. I didn't obtain enough intuition that I should have learned when I was in high school. I always feel I am lacking in physics training, despite the fact that I did quite well in physics in the Chinese University of Hong Kong.

Hosono: Then, in 1969, when you were 20 years old, you

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* *The Shape of Inner Space—String Theory and the Geometry of the Universe's Hidden Dimensions*, Shing-Tung Yau and Steve Nadis, Basic Books, New York, 2010.

went to Berkeley and actually went into the graduate school there. Right?

Yau: Right.

Hosono: Soon after entering the graduate school, you found a theorem and it's quite amazing. Could you tell us a little bit about that?

Yau: I went to Berkeley and so I enrolled in many classes because I felt I didn't know many different areas of modern mathematics and I started to be interested in geometry. I learned quite a lot of things from different faculties in Berkeley and during the first semester I learned about manifolds, I learned Riemannian geometry, but not enough; it was just elementary.

Then, during the Christmas term, I didn't realize in America everybody went home, so I was basically left by myself and I spent most of my time in Berkeley in the library. We didn't have an office in those days. I went through all the books and journals and I found a journal which was quite readable for me, the journal was called the *Journal of Differential Geometry*. The second issue of the journal had some interesting papers written by a great mathematician called John Milnor and I found it fascinating reading his paper. The paper was about how curvature influences the fundamental group of the manifold. I learned what the fundamental group of the manifold means during my course in algebraic topology but I barely learned what curvature means in the geometry class. But then I found these two things can be linked together and I found it very interesting.

I studied the paper by Milnor. It was so well written, I could understand the whole thing and then he referred to some other

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paper, some older paper by a man called Alexandre Preissman. I looked at the paper and I decided I could understand it. Not only that I could understand it, but I could try to generalize some of the arguments to a more general case. I kept on doing it. Surprisingly to me, after a week or so, I was able to do something reasonably interesting (they later called it flat torus theorem) using some fundamental group of the manifold which depends on group theory. I happened to learn something about it when I was in the Chinese University of Hong Kong. I went through the library and looked up all these references by myself. It was a very interesting period of time, living alone, away from other people, and spending all my time on studying. That was good.

Hosono: Just half a year after you went to the U.S.?

Yau: Yes.

Hosono: That's quite amazing for us.

Yau: No, it was just exciting. So I found it interesting.

Encounterd the Calabi Conjecture; Not Believed It Could be True for a Long Time

Hosono: Good. After that, you encountered Calabi conjecture.

Yau: Right. In the first year, I spent a lot of time studying Riemannian geometry including this thing that I just mentioned, but I also studied complex manifolds and there were some seminars I went to and then I decided I would ask Shiing-Shen Chern to be my advisor; he was on leave the first year. When he came back, I said "I want to be your student." He agreed and I started to spend most of my time studying complex manifolds.

Then, I also spent my time in the library and I looked up some of the papers written by Eugenio Calabi. He mentioned this as the Calabi conjecture. This was fascinating to me because I was taking a course in general relativity and I looked at Einstein's field equation describing geometry in terms of Ricci tensors. I found it interesting because the Ricci tensor only represents part of the curvature, and yet in physics it means matter. So, I said "If there is no matter, I wonder whether there will be gravity." Well, this means in a Ricci curvature you can see whether there is still nontrivial gravity. This was very difficult to understand in just purely Riemannian geometry. Then, I looked at this paper by Calabi. He asked, even gave a way, to try to understand this problem in a special class of manifold called Kähler geometry. I found it fascinating because now I felt it would help me to understand Ricci curvature much better.

I was extremely excited about it because I studied Riemannian geometry at the beginning, but with Ricci curvature, I didn't know how to study it or what to do. But this gave me a way to understand it. After that, I wanted to understand this problem. But, at that time there were almost no examples, basically no examples of such manifold. Then, Calabi proposed that you can find a huge number of them by making use of algebraic geometry. Nobody believed that it could be true because it was just too good to be true. And perhaps I myself also didn't think that it could be true. I was struggling to try to see whether it was true or not for a long time.

Hosono: Do you mean at first you didn't believe it?

Yau: For quite a long time I didn't believe it. Many of my friends are very brilliant geometers, but none of them believed that it was true.

Hosono: None of them?

Yau: Right.

Hosono: That was the first year.

Yau: It was the first year when I learned about this problem. On the other hand, I felt it was such an essential, important question that it had to be solved one way or the other. If it was not true, I should find a counterexample. If it was true, it would be great. At that point, I really believed that it would be the greatest theorem to prove it is true, but I didn't believe it could be true.

Hosono: Eventually you came to the conclusion. I mean you completed the proof.

Yau: Oh, it took a long time.

Hosono: That means the problem was so difficult.

Developed Geometric Analysis, the Basic Idea to Understand the Calabi Conjecture

Yau: Yes. Actually, Calabi told me later, when he was trying to solve this problem, a famous mathematician, one of the greatest mathematicians in the 20th century, André Weil told Calabi that the major tools to understand, or to solve this problem were not there yet. It was premature to solve this problem because the tools were not there. Indeed, when I tried to solve the problem in a positive way (at the beginning I tried really hard to give a counterexample, and when I decided it was probably true) I needed to build up the fundamental tools to solve it. Nowadays everybody looks at it and says it's almost trivial, but before that, people were not even doing a differential equation on the manifold and people were just solving an equation in a domain. But I was working on my manifold to develop all the basic tools in order to solve it. That took quite a while. But I had some good friends like Shiu-Yuen Cheng and also Richard Schoen and Leon Simon, and we were working together. All these are very good friends. We started to understand what geometric analysis means on a manifold. At the same time, we developed a subject which is now called geometric analysis. This was the basic idea needed in order to understand the Calabi conjecture.

Hosono: I see. How many years did you spend on the problem?

Yau: To solve it?

Hosono: Yes. Originally you thought you would disprove it.

Yau: From 1970 to 1973, 1973 around September to November I thought it was wrong, and I tried to give a counterexample. Starting around November, I decided it must be right. I was struggling to try to give a counterexample. I announced a counterexample in a big conference in Stanford, 1973, and then it was found to be wrong. I felt very bad about it because I had made a big announcement and it turned out that it was wrong. I spent 2 weeks, basically day and night, without doing anything, just trying to give a counterexample. Every time I gave a counterexample, it failed in a very delicate manner, so I felt it cannot be that delicate unless God had fooled me; so it had to be right now. I changed my mind completely, and then I prepared everything to try to solve it. From the fall of 1973, I solved it after 3 years, in 1976. So, 3 years of preparation and doing estimate by estimate.

Hosono: I see. All the necessary stuff you prepared.

Yau: Studying and preparing the tools.

Hosono: I see. It's very interesting. In 1982, because of that theorem you were awarded the Fields Medal, at the same time with Alain Connes and William Thurston.

Yau: Yes.

Hosono: What is very interesting to us is that a historically big event in physics, a breakthrough in string theory, occurred soon after. What was the situation? I mean what was the communication between mathematicians and physicists around that time?

Calabi-Yau Manifolds Met String Theory in 1984

Yau: Actually, starting in 1973 in a big conference in Stanford, I met some physicists who gave a talk about general relativity. They posed some questions about gravity, which is called a positive mass conjecture. It turned out that it was an old problem starting from Einstein to prove that Einstein's equation is stable. That means the total energy of spacetime is actually positive with the assumption of what Einstein laid out; if the total mass were negative, the system would be unstable, and the whole universe would not hold together. It was a fundamental question to answer so as to make sure that that cannot happen. It was a beautiful question in geometry by itself. I worked on that until around 1977 to 1978, and we solved it. I solved it with my former student Schoen. We solved it together. I had known him from Berkeley. Because of that I had been in close contact with people studying general relativity. That was after the Calabi conjecture. In fact, in 1979, I went to Princeton where many people were interested in general relativity. Famous physicist Malcolm Perry and many others were there, and after 1 year I became a faculty in the Institute of Advanced Study and then I had postdocs, my first postdoc was Gary Horowitz. I invited him to come as a postdoc in general relativity. Then, in the same year, I also met Andrew Strominger. After a year, Ed Witten came and showed me how to give a different proof of the positive mass conjecture. All of them were there. I said I constructed this manifold which is now called the Calabi-Yau manifold. I said, "To me it's motivated by physics. You know, vacuum still has gravity. This must be useful for physics." But, at that time it had not matured enough in physics. So nobody believed that it was true. It's interesting.

In 1984, I was still in the Princeton institute, but I visited my wife; my wife worked in San Diego at that time. San Diego is beautiful, and I was in her office which looks out over the beautiful

blue ocean. I received a phone call from them; from Horowitz, Strominger, and Witten. They said "It's exciting. We are developing a new subject of quantum gravity; it's called string theory. This is great, but we need to know one thing for the vacuum solution — because we are building a model vacuum. What kind of manifold is it? A six-dimensional manifold meets all the conditions. Well, somehow you have mentioned something close to the truth but we are not so sure." They asked me whether I know how to do it, and I said "This is exactly what I told you before. That's exactly what I can do." So they were very pleased. In fact, Ed Witten wanted to know much more. So he flew from Princeton to talk with me for one day. We had a very good conversation for the whole day. Then, in the same year, 1984, there was a big conference on string theory at the Argonne Laboratory in Chicago. I went there and I met many more people who were very excited about the subject. I started to get much more interested in Calabi-Yau manifolds after that. Before that, we actually did not know many examples; on the other hand when the physicists joined, it became a big industry and I started to construct many more Calabi-Yau manifolds for them. At one point, I said that there are at least 10,000 of them; they were somewhat disappointed. At the beginning they thought there are only 3.

Hosono: Only 3?

Yau: Yes. Then, I told them there are many more. But, anyway after that we became much closer in developing the properties of these manifolds.

Mirror Symmetry of the Calabi-Yau Manifolds Discovered around the End of the 80s

Hosono: I see, it's interesting. So, in any case, activities for string theory had started. Around the end of the 80s, one of the big discoveries was the mirror symmetry of Calabi-Yau manifolds. Right?

Yau: Right.

Hosono: Mirror symmetry seems to be strange for mathematicians. What did you think about it?

Yau: Oh yes. Starting in 1984, we were interested in Calabi-Yau manifolds. We were exploring the construction; exploring some properties of them. Postdocs and all of us were talking together. We made some progress. Around 1988, I moved from San Diego to Harvard, and in 1988 there was a young guy called Brian Greene, who is now of course very popular. He became a postdoc. We talked about the Calabi-Yau manifold, we wrote some papers, and the study was going quite well. Suddenly, one day he came to my office. He said, "I think that each Calabi-Yau manifold has a mirror." I thought about it and I said, "That cannot be true."

Hosono: Oh, you said that cannot be true?

Yau: Yes. That was a mistake because most of the Calabi-Yau manifolds we constructed had a negative Euler number. So I said, "This is not symmetry because a mirror manifold means that the Euler number has a different sign, but there are more negative Euler numbers than positive Euler numbers." But, then I was wrong because I did a calculation just on a piece of paper and it's not so easy to do a large-scale calculation. Then, Philip Candelas and his co-authors did a large search based on a computer, and they found a diagram which is symmetrical.

Hosono: Yes, the famous diagram.

Yau: We started to have a good hint about what was going on, and then Brian Greene and Ronen Plesser, who was a student of Cumrun Vafa, developed the theory of mirror symmetry on a special class of manifold called the Gepner model. They were based on physical intuition and physical reasoning on symmetry. They actually proved "in a physical way" that a mirror for the quintic is good and verified some interesting examples, I mean, properties that are good. I was convinced that it looked very good, and what I'd said was wrong. But, the most amazing thing is the fact that Candelas, actually after 1 year of calculation, said that they got a really precise calculation of the mirror conjecture — starting with a conjecture, they did a lot of interesting calculations which was amazing to me.

Hosono: Calculations for the famous quintic?

Yau: For the quintic, yes. The instanton correction (to Yukawa couplings), which turned out to be beautifully done. I was extremely impressed by that.

Hosono: And then, soon after the work by Candelas et al., there was a development.

Berkeley Conference, a Big Turning Point for the Development

Yau: Right after that time, Isadore Singer asked me something without knowing about this mirror symmetry. He said that there would be some kind of special program on mathematical physics at the Mathematical Sciences Research Institute in Berkeley. He asked me to go there to organize something. I told him something suddenly occurred, namely the mirror symmetry calculation, and I thought it would be very good to have a conference on this. Both physicists and mathematicians should come together and communicate among themselves to see what should be done and what should not be done. We changed the original plan which was for another subject on mathematical physics mostly on gauge theory at that point. Singer was more interested in gauge theory at that point. We turned that into a mirror symmetry conference. That was the first conference that we had.

Hosono: I think after that conference many mathematicians changed their attitude.

Yau: That was a very dramatic conference because I decided something. After talks, which were rather formal, people did not communicate that much. So, one night, after dinner, I called physicists to come and algebraic geometers to come. We spent 2 hours discussing things. The most dramatic thing was that the calculation of the instanton number, calculation that Candelas and his group came up with, turned out to be different from the calculation given by two algebraic geometers in Norway. There was a big discrepancy. Then, there was a big debate because algebraic geometers thought that everything they did was so rigorous, every step was done right, and there could not be any mistake in their calculation. They started humiliating Candelas and others, saying your ideas cannot be true.

I remember very well that physicists were actually much more humble because of their normalizations in the Yukawa couplings and everything. I talked with Brian Greene and I talked with Candelas. We looked at all the possible normalizations and everything that seemed to be fixed, and we couldn't find any problem at the end

of that. We were very puzzled, wondering what's wrong. We thought something had to be fixed, but we didn't know how to fix it. The conference ended up with something puzzling. We all went home. After a couple of months, it was great because our two colleagues from Norway who were very honest sent us a letter saying that the program they used (they needed a computer program to do the calculation), the program they'd developed, had a gap, something wrong, and after fixing that they came up with exactly the number that Candelas had. This is not a simple quiz — because the number is a big number and they were exactly the same. Now, it became very convincing to our algebraic geometers friends that there was something in the physics of this calculation. Immediately, many algebraic geometers, especially David Morrison who was very critical of the calculation at the beginning…

Hosono: Was he critical?

Yau: Oh extremely critical. He said "You guys cannot be right," but after that he turned 180 degrees and he very faithfully started supporting this whole subject. And he has made a huge number of contributions since then. Especially, he started to work with Brian Greene who is a very good writer in the first place. After being my postdoc, I recommended Brian Greene to go to Cornell and he was in Cornell. Then, I think Morrison went to Cornell to work with him and started to understand what is going on. Brian Greene later went to Columbia. I helped him to get a job there and since then he has become very happy.

Hosono: Good. That workshop must be a very big turning point for the development.

Yau: Oh yes. After that both Greene and Morrison, and Candelas kept on doing very good works. And then, people started to work on it — many people started to look at the problem.

Hosono: After that people tried to understand what mirror symmetry is and now there are two major ways to understand mirror symmetry. One of them is your construction, named SYZ (Strominger-Yau-Zaslow) mirror construction. That's very attractive and seems to be very promising, but still very mysterious.

Yau: Right.

Hosono: What do you expect for future developments or what do we need to make developments?

Physical Intuition Helped Mathematicians Understand a Geometric Subject; Otherwise It Would Be Impossible

Yau: We were always interested in mirror symmetry; you came and Albrecht Klemm came in early 90s. The SYZ construction somehow was related to the brane theory which Joe Polchinski and all these people developed. I was talking with Eric Zaslow who was my postdoc. Then I was visiting Trieste actually, and in Trieste Ed Witten asked me, "Andy Strominger and (Katrin) Becker and (Melanie) Becker just came up with this supersymmetric cycles in a Calabi-Yau manifold. We are not sure that is the right thing, but could you give an opinion? What do you think?" He said, "Andy has been talking for quite a while about trying to get such a cycle, but this time seems to be interesting." He wrote down whatever – he would draw on a blackboard what it looks like. I said "This looks very good. I mean this is a minimal submanifold." I have been working on minimal submanifolds for a long, long time. These are special Lagrangian cycles. At that point, it was called supersymmetric cycles because we didn't know (and I should have known, but I forgot) some of this work was done independently by Blaine Lawson and Reese Harvey many years ago. But they had no idea what it meant for physics. In the brane theory developed by Strominger and Becker and Becker, they did not know that this happened before. So I suggested it looked very good, and once you should look in it.

Then, when I came home, Strominger came to visit. Harvard was thinking to make an offer to him. He did very important work on conifold transition of black holes at that time. He came to my office and we had a long discussion about what it is and we decided from the point of view of the brane theory there should be a mirror which is constructed using the brane duality. So we came up with this idea of the SYZ construction; the T-duality would be the right one. It was good in many ways because the time was just right and then brane theory developed. We felt that was the right thing.

I am very excited about it because it is geometric interpretation of something and I always like to see geometry and physics mixed together anyway. But the problem of course is that in the whole development there is always some quantum correction, which takes a lot of intuition to build on. This quantum correction has been always important and yet not understood and it keeps on giving some hints about what is simply true and what is to be done. But it's never precise mathematically. So we keep on developing some mathematics to understand it; each time we are going the right way, basically we see some very interesting mathematics that come up from that. Each time the mathematics comes out to be right and supports this conjecture. I think up till now there have been many, many accumulated supporting effects.

I must say I was very surprised and very happy — physical intuition helped us to understand a geometric subject which otherwise would be impossible to understand because many of the problems had someone to do with a singularity. The SYZ fibration has many singularities and up till now we still don't know how to deal with it. But somehow the quantum field theory basically says that although there is a singularity that should be fine, intuitively. The calculations always come out to show there is some way that we can overcome the problems. We are still very excited about it and now much more progress has been made on the homological mirror symmetries (HMS) proposed by Maxim Kontsevich. I think these two approaches will mix together and hopefully it will give very good intuition. A mathematical proof of some important statement will come up from the merging of these two approaches, I think. As I said, many interesting beautiful mathematics have emerged from this understanding; some of them were totally surprising to me when they came up and what's even more surprising is when we can actually prove that it's true.

Hosono: Yes, that's right. Somehow from the mathematics side mathematicians developed, I mean, for example, Harvey and Lawson developed a theory for special Lagrangian submanifolds and, on the other hand, physicists got the idea of brane or something like that. Then you connected those two ideas into the same thing.

Yau: That's very good, I think.

Hosono: This is actually the question that I would like to ask you. My question is whether this is a typical example of the relation between mathematics and physics. If you looked at those two subjects, there was no difference between the two for a long time in the history. But, in the 20th century somehow those two subjects went in different directions.

Yau: At the beginning, yes.

Hosono: But somehow the string theory suggests us something, though I don't know exactly what it should be. What do you think about the relation between mathematics and physics?

Need to Build Geometry That Can Understand Quantum Gravity in the 21st Century

Yau: I think it's fascinating. I mean it's always a subject that many great mathematicians tried to understand both sides and get ideas from both sides to make some advance. I think in the 21st century, we need to build geometry that can understand quantum gravity, something very big, which is governed by gravity, by Einstein's equation and something very small, which is governed by quantum mechanics. Of course, the major question then is something Einstein wanted to solve. But I think having just physicists is not enough, we need good geometers; and having just geometers is not good enough because we need extremely deep physical intuition. I think we are building the theory more and more on both sides, and hopefully at the end we can have a bridge.

But right now I think it is still not mature enough to build out right quantum geometry, because we don't understand many important questions like the Calabi-Yau manifold and many, many details. And on the physics side, also there are many things we still don't understand; I mean, black holes which create a lot of paradoxes and all that we still need to understand. I think maybe after 20 or 30 years we will understand much more; we will see a bridge in a clearly ordered view. I believe that will be the goal of many mathematicians and physicists put together. This will involve many subjects, I mean, algebraic geometry, analysis, representation theory, number theory from the geometry side, and from the physics side, of course, many ideas from quantum field theory and from statistical physics, from many, many different subjects. So it will be a merger of many, many subjects; not just one subject. It will involve many, many people. Not just one single person can understand all. I think this is a beautiful, important period of time in history.

Hosono: Yes, that's right. Finally, as you know, IPMU is an abbreviation of the Institute for the Physics and Mathematics of the Universe. Based on your experience, could you give us some words for the people doing research related to physics and mathematics?

Yau: I was there in the very beginning (in the opening symposium in 2008), when they were building up the center of the subject and I was very excited about it. I think it's very good and even essential and important for mathematicians, physicists, astronomers to come together, and listen to each other to develop theory. We need to have strong curiosity, and inspire each other. As for me, for example, I liked going to a physics department to listen to seminars. Although most of the seminars I couldn't understand, after 10 times I started to get something and that something could be very useful for my development in mathematics or even to physics eventually. I think people should be patient and not say "I cannot solve this problem today. So I am giving up." That's not right, because it's just like language. After you listen to some language for 1 year, you will know how to speak it. It's the same kind of problem. Going to a physics department we have to know the language and vice-versa, and mathematicians produce many things which are exciting for nature and vice-versa. I think we should understand each other. I hope people in the institute will do the same thing.

Hosono: Thank you very much. The words from you, a great mathematician who has experienced the great interplay between mathematics and physics in the last 50 years, will have a big influence on the people in the institute and for all of us. Thank you very much for this interview.

Yau: You are welcome. Thank you.