INTERVIEWER: Today is September 6, 2011. I'm Chris Boebel. As part of the MIT150 Infinite History, we're talking with professor Samuel Ting. Professor Ting is the Thomas Dudley Cabot professor of physics at MIT. He holds BAs in both math and physics from the University of Michigan, as well as a PhD in physics from the University of Michigan. In 1976, Professor Ting was awarded the Nobel Prize in physics, shared with Burton Richter, for his discovery of the J particle. The discovery was made in 1974, while professor Ting was heading a research team at the Brookhaven National Laboratory.

Professor Ting is also the principal investigator for the Alpha Magnetic Spectrometer Project, a particle physics experiment module that was installed on the international space station earlier this year. Professor Ting has been on the faculty of MIT since 1969. Professor Ting, thanks very much for taking the time to talk to us today.

TING: My pleasure.

INTERVIEWER: So let's just begin at the beginning, as it were. Tell me a little bit about your childhood, how and where you grew up.

TING: I was born in the University of Michigan hospital, where both my parents were graduate students. My father studied civil engineering. My mother studied psychology. Right after I was born, the war between China and Japan broke out. The war has been going on for a long time. At that time, my parents, they were a patriotic type. They think they're Chinese. They want to go back to China.

So I was a few months old and nothing to say. So I went back to China with them. So from middle of 1936 to year 1945, I was basically a refugee traveling around China, and spent some time in Chongqing, which was the wartime capital of Chinese nationalists.

And then in 1945, we went to Nanjing with them. It was the final capital of the nationalist government. After that, in 1949, we went to Taiwan. From '49 to '56, I actually began my formal education in Taiwan. And in '56, on September 6, exactly today, I went back to the University of Michigan.

At the beginning, I really didn't speak any English. But since I was born in the University of Michigan hospital, I'm considered a citizen by birth and Michigan residence. So I got into the university. I don't know how I got in, because I speak hardly any English. At the first year, I studied mechanical engineering.

After the first year, I had a conversation with my adviser, Robert White, very good engineer at that time. He took a look at my grade and said, you are no engineer. The problem was, at that time, there were no computers, so for every engineer, you have to do drawings. So for object, you have to look from the top, look from the side, look from the end, and I was no good. I couldn't even draw a line with equal thickness. And Professor White said, maybe you should study math and physics. Why don't you take math and physics at the same time? Why don't you begin to take some courses in graduate school.

So in '56, when I first came in, I was 20 years old. I was the oldest in the class. And second semester, I began to take some graduate courses. And so I became the youngest. The university was really very nice to me. And the exempt me from taking English language, history, economics, allowed me to concentrate on math, concentrate on physics.

So in '59, I got the Bachelor's degree in mathematics and physics. And in '62, end of '62, got my PhD degree. So from entering university to leaving was relatively around six years. So that is, at that time, considered fast. Particularly, when I started, I didn't hardly speak any English. After that, I realized, gee, I spent six years only in Michigan, maybe I should go to Europe.

So I applied for a fellowship from the Ford Foundation. And so from that foundation, allowed me to go to the European organization for nuclear research in Geneva, Switzerland. I think I arrived March 31, '63. And so I was in CERN. And before I left for CERN, one of my advisers, Marty Perl, who was a graduate of Columbia University. At that time, Columbia's physics department was really the very best. He said, maybe I should recommend you to Columbia University to be an instructor.

So I went to CERN for a year, and then I went back to Columbia. And Columbia at that time had many, many, truly outstanding physicists. Particularly, I remember Professor Robbie. And from these people, I gradually learned how to do physics. At the University of Michigan, I finally graduated.

School somehow was somewhat easy for me. And I developed, besides school, an interest in University of Michigan football. So I was there six years, missed quite a few classes, never missed a single football game. So I'm very proud of this fact, that I never missed a single football game.

University of Michigan, as you know, if you're a student, and if you don't watch football, you are not with it. And so I was very grateful, still is very grateful to the university, because they really educated me and supported me. When I came to entering the university, I had a discussion with my father and my mother. And the discussion goes like this. I told my parents, I heard people in the United States go to school supported by themselves, so I don't need money from you. Just give me $100. So that's what they did. Fine, take $100.

And so I remember, September 6, I landed in, at that time, called Willow Run Airport, not current Metropolitan Airport. And I had a hamburger. It cost one dollar. This was only then I begin to realize what a serious mistake I made. But the university supported me with scholarship. And so that's how I started my little career.

INTERVIEWER: So before that all-important meeting with your adviser, when you decided not to become a mechanical engineer, did you have a sense before that time that you had an aptitude for math and physics, or was that college that you really began to understand that?

TING: Before I was 11, I was a refugee. So I would go to one school for a few months, go to another school for a few months. And at that time, during World War II, there were Japanese airplanes come to visit us. And so school is not stable. So school was not in the highest priority. And I also gradually developed an interest to watch the birds, to watch animals, but I had not much opportunity to go to school.

I went to school for the first time, in a serious way, when I went to Taiwan to attend the fifth grade. And then I realized, gee, probably it's important to take this matter seriously. In high school, I definitely was not the best student. I was interested in Chines history, in physics, math, and chemistry. The reason that I'm interested in Chinese history is at that time, I have a good memory. I would read things once and remember. So it was easy for me to get 100 in the score.

You're interested because you don't have to do too much work and you get a good grade. If you spend a lot of time, you still get the 0, then you cannot be interested. I did not know what I want to do. Because my father was an engineer, my parents' friend, who was the dean of engineering at the University of Michigan, G.G. Brown, said to me that I can, when I'm into Ann Arbor, said I can live in their house with them. And that's how I decided to study engineering.

INTERVIEWER: So your discovery of math and then physics came actually relatively late?

TING: Yeah.

INTERVIEWER: So would you say that it was something that you developed a love for very quickly? Tell me about that sort of process of discovering that you--

TING: Physics and math, I found it quite easy. Without much hard work, you can get good grades and enable you to go watch football on the side. So that was the only thing that was very easy for me.

INTERVIEWER: So I also wanted to ask you more about your parents. I mean, clearly it was a tumultuous time and place to grow up once you returned to China. Tell me about your parents and their influence on you.

TING: My father come from a very wealthy family in northern China, a place just across the Pacific from California called Shandong Province. And my grandfather, in 1920s, made a tour around China, and came back, told his wife, my grandma, and told my grandma that the Chinese society is going to collapse. The only way we're going to save ourselves is to have all our children have education.

So my father, and my father's father, my four aunts, all went to school. And they began to sell their land and to educate their children. This simple act enabled all of them, not being killed by the communists, by the difficult times.

My mother, her father studied in Japan, and came back to China and was one of the early designers of railroad in China. And he joined the revolution in 1911. Somebody betrayed him, so he was called and he was killed. My mother at that time was three years old, and my grandfather, my mother's mother, had sold everything they had to start a revolution.

And so now he was killed, and so my grandma become penniless. But she decided, I'm not going to stay in this little village. I'm going to take my daughter-- my mom, who was three years old-- to make sure she has education. So at 8:30, she left her village, and went to work for a missionary, and then went to school, and eventually to become a teacher in grammar school, brought up my mother. So I would say my mother and her mother are really quite determined people. They really have a great influence on me.

INTERVIEWER: This is a question that you could take in many directions, but having that childhood, having that background, how has that influenced your career, your life, the way you think about science, if that's the right way to ask the question?

TING: I think my father and my mother always, at home, mentioned to me the story of a great physicist, like Isaac Newton, James Clerk Maxwell, Michael Faraday. So ever since I was young, I've heard about their names and what they do. Even though I didn't go to school, I listened to them about the stories, and that really had some influence on me. And I also, since my mother's mother, who really had a very difficult life, but never gave up, always looked forward, perhaps had some indirect influence on me.

INTERVIEWER: So as a young PhD graduating from the University of Michigan, what were your interests and what was next for you at that time?

TING: I went to CERN and studied strong interaction. Strong interaction means interaction among nucleus, trying to understand nuclear forces. And after that, I went back to Columbia. While I was at Columbia, there was an experiment done by an accelerator in Cambridge called Cambridge Electron Accelerator, CEA, no longer exists now.

And they measured the size of the electron. One experiment done at CEA, another experiment done at Cornell, all shows electron has a size, has a measurable size. And this is a very important experiment, because according to modern quantum electrodynamics, electron should have no size, no measurable diameter.

So I was at Columbia. I decided, this is such an important experiment, I really should repeat it. I think it made me think, probably 1965 around November-- either '64 or '65, around November. I drove from Columbia University to Harvard to talk to the physicists about repeating the experiment with a method I'd developed. And the professors were very polite to me, said, well, you know, you've never done experiment like this. It took us 10 years to do these things. And you're welcome to join us, but we will not have a place for you to do the experiment. Besides, you have no financial support.

And I remember that day, because I talked to the professor at Harvard, and that day, there was a blackout. So I want to talk to him for a few minutes, then his face disappeared. He was in the dark. And so I went back to Columbia, and thought a little bit about it, and realized, this is such an important experiment, it really should be measured again. So I contact my friends in Europe at CERN, and they had been to Hamburg to build an identical accelerator, called DESY.

And they said, well, why don't you come talk to us? And that time, there were very few high level physicists in Germany because as a consequence of the war in '65. So I went there, the say, yeah, we will support you to do this experiment. So I went back to Columbia, told the faculty, and they said, look, you never done such an experiment. It takes, normally, five to 10 years, and you are an assistant professor. You have to teach.

So I tell them please give me a half-year leave. And they were very kind. They gave me a half-year leave. But the said, you're not going to finish this in a year. Maybe we should give you a year leave, but we guarantee you you're not going to finish it, because you have no experience with this. Fortunate for us, we finished in seven months, and showed the experiment at Cornell, the experiment done in Cambridge, all wrong. Your electron has no measurable size.

And because of that experiment, because somebody from no-- nobody knew who I am, never done anything before. And people began to notice me. Then after some time, I realized the light ray, light ray travels through space, it has no mass, no rest mass, but if the energy is high enough, then the energy and mass, e equal to mc squared, and so light ray must have an opportunity to change to a massive particle. This must happen.

So now I have a very high-energy light ray from the accelerator that must be able to produce massive particles. And these particles then will go back to the light ray, and goes through an electron processor. And that's a very rare process, but it must exist. And people have looked at before, and never found the result. And so I did a series of this experiment, and found, indeed, they do exist. The light ray changed to a massive particle, and go back to a light ray, and go through the electron.

And then MIT began to show interest in me. And so, I think it's '67, I came to visit MIT. The head of the physics department was Professor Weisskopf. And I had a conversation with him. And he said to me, we would like to hire you. But at MIT, an associate professor has no tenure. At that time, since I've done some experiments, some of them somewhat important in the world of physics, I had quite a few offers. All had tenure. MIT's the only one which did not carry tenure.

And at that time, I really didn't understand what tenure is. I said, that's fine, but I only have one condition. And professor Weisskopf asked, what is your condition? I said, I will come here without tenure, that's fine, but please allow me to work based on the development of physics, not working at the Cambridge Electron Accelerator, which is near Harvard Square. And they said, that's fine.

And with that condition, I came to MIT. After two months, I was in a faculty lunch. After the lunch, all the senior professors went to the room for tenure faculty meeting, and I was not invited, the first time I realized what tenure meant. And so I went to the head of department, I said look, you know, I have an interest in the university affairs. I'm very busy with experiment. But not allow me to listen to what's going is a bit excessive. And besides, my other offers are still pending. And Professor Weisskopf said, no, don't do anything. And two months later, MIT, to its great credit, gave me tenure.

So to get a tenure, at that time, was important to get a good offer from somewhere else. That's how I stayed at MIT. I never dreamed that I would stay most of my career at MIT. I thought, since I don't have tenure, I'll come here for a few years, then I will be gone.

INTERVIEWER: Good negotiations there.

TING: Hmm?

INTERVIEWER: Good negotiating. So I'm curious, I want to ask just a bit more about the experiment that you did and Hamburg, where you managed to do the experiment in seven months that they said would take much longer. To what would you attribute that success? I mean, was it a different way of thinking about the problem than other people were thinking about it, or pure hard work?

TING: Well, normally, when I do experiment, I spend a lot of time thinking through what could be the signal, what could be the background, what experimental check I will make. So if I'm given 100 hours to use the accelerator, I will spend more than 60 hours checking my instrument to make sure-- When you make a mistake, normally it's your instrument. Always it's your instrument. So I basically spent time checking the instrument. Also, I normally follow, in great detail, the electronics logic, the size of the detector, and the pressure-- all the technical details. I spent all the time following all the details.

When you make a mistake, it's normally in your instrument. And I make sure I understand it. If you know there's something wrong with your instrument, then you can correct it. You're going to make a mistake if the instrument is not working and you do not know.

INTERVIEWER: As you mentioned, you've spent most of your career at MIT. 1969, I guess, was the year you arrived officially.

TING: I don't remember. I thought it was-- Yeah, it could be.

INTERVIEWER: '69, but around that time. Tell me a little bit about what the environment at MIT was like then. And has it changed significantly?

TING: No. I found this is a great place because I was given a lot of support from the School of Science, from the physics department, and from the Laboratory for Nuclear Science. They really go out of their way to support. And also, the laboratories in Europe, in Germany, in Switzerland, and in Holland, in Italy, in Russia, they all support my work and collaborate with me.

The reason they collaborate with me, I think if you analyze it, is I've always managed to choose an interesting topic. And so physicists think this is an important topic. And second, every one of my experiments has produced some important report. So far, we have not yet made a mistake. And third, I always make sure the people who did the work receive proper recognition, be it a professor, be it a graduate student. In this way, people will work with you. If you're continually making a mistake, or take all the credit away, nobody will support you.

INTERVIEWER: You mentioned Professor Weisskopf at that time. Were there are other people at MIT at that time that had a significant influence, either on your career on your thinking, that are worth talking about?

TING: Well, I would say Herman Feshbach, the late head of the department of physics, Francis Low, the previous provost. They were all physicists. And Martin Deutsch, a very famous physicist. And they all had very important influence. Also, Steve Weinberg, who was, at that time, the head of MIT, and normally come back to MIT and talk physics with his people.

INTERVIEWER: Tell me a little bit about how you view the collaborative nature of your work. I mean, how important is it for you to be surrounded by people like that who are providing interesting questions or maybe interesting ideas, interesting answers?

TING: There are two kinds of experimental physicists, one to check people's theory, another tries to choose his own topic. Both are very important. But to prove somebody's theory, you don't learn much. It's when you destroy somebody's theory, you learn something new. So for a physicist, an experimental physicist, to choose the right topic, it's very, very important.

But in physics, public opinion, it's really not important, because the advancement of physics, you try to modify public opinion, to destroy common feeling. Then you can move ahead. And so, in most my experiments, at the beginning, there were a lot of objections. The one in Hamburg, I've already mentioned, the one which I eventually got a Nobel Prize in Brookhaven, and that was a very difficult experiment.

The experiment originally come from the fact, I was measuring light rays to change to massive particles, and then go back to light rays, goes through the electron-positron pair. It was very curious to me, why all those particles all has a mass of one billion electron volts, namely the mass of protons. I asked myself, why is this? Why is this like this?

People's explanation was, there are only three types of quarks in the universe. With three type of quarks, we can explain all the known phenomena. Now, to look to the fourth quark or fifth quark, it's very unpopular. The theory is that, no, look, three quarks can explain everything, you don't need four quarks. Experimentalists said, this is such a difficult experiment, no one can do such an experiment.

The sensitivity we eventually achieved was one signal to background, or one part in 10 billion. What does one part in 10 billion mean? Like during today, when it rains in Boston, there are 10 billion raindrops per second, and one of them is blue. You have to find that one. And it's somewhat difficult. So this experiment was basically or rejected by everyone, by Fermi National Laboratory, by CERN. Many theorists said it's totally rubbish, not needed. Experimentalists said, nobody can do this experiment.

Finally, Brookhaven National Laboratory somehow made a mistake, approved us. And it was approved in '72. And two years later, sure enough, we found a particle, which I called a J particle. This particle, once it's discovered, a subsequent family was discovered. And the subsequent family mostly was discovered by Professor Richter. It has a very unique property. That is, it has an unusually long lifetime. The lifetime is about 10,000 times longer than the rest of the particles.

What does this mean? Now, everybody on Earth lives about 100 years. If you find a family, a village somewhere, people live a million years, these people must be different, meaning they are a new type of matter, new type of material. And that's from the fourth quark. It it because of this that you break down the previous understanding. Nobody thought this exists. That's how Richter and I were awarded Nobel Prize .

Now of course, after you have the fourth one, the fifth one was found, the sixth one was found. Now people think, again, the sixth quark can explain everything. But if you don't look, you do not know.

INTERVIEWER: So you've answered this, at least somewhat, but I was going to ask, what transpired, what has developed since that discovery?

TING: Since that time, people found the fifth quark, found the sixth quark, and now people who are still continually looking for this, and looking whether quark has a size, has a dimension or not. And if you don't look, you will never know.

INTERVIEWER: So tell me about winning the Nobel Prize. Tell me about the experience.

TING: Yes, it was 1976, October 18. I was in my office in Geneva. Suddenly, the phone rang. It's a Swedish accent, from the secretary of the Royal Swedish Academy of Science, said, congratulations, you have won the Nobel Prize together with Professor Richter. And of course, I was very happy, because most the people wait for 10, 20, 30 years. In my time, it was a little bit less than two years. It's considered quite fast. So I was very happy.

And later on I learned MIT also was very happy, had a huge party to celebrate this. Of course, forgot to invite me, because I was in Geneva. And so, when I come back, months later, they had another party for me. And then I made sure that people who worked with me, who contributed to the experiment, are invited to the ceremony.

INTERVIEWER: You chose to give your acceptance speech in Mandarin, which I believe was the first time anyone had ever given a speech in that language. Can you talk about that decision and what it meant to you?

TING: When I was young I had very little education. And through my career, I was doing experiments. But as I mentioned, I know a little bit about Chinese history, before I was 20. And in the traditional Chinese education, people use their mind is considered superior. People use their hand is considered slightly less superior.

This concept has a tremendous influence on students in Asia. That's why many, many, want to do theory. But natural science, in physics, in chemistry, in biology, it's experimental science. A theory, however elegant, if it cannot be proved by experiment, it's useless. An experiment can destroy the theory. A theory can never destroy an experiment. It's only when experiment confronted a theory, you produce new theory. And this is how science advances.

So I thought I'd give a little short speech in Chinese aiming to the students in Asia, to mention natural science is really experimental science. This was not popular with the US embassy in Sweden. In 1976, the relation with China was somewhat tense. And so the ambassador actually come to talk to me. He said, well, you are American, and you should give this speech in English. I said, no. I have no political sympathy whatsoever with the Chinese government.

And Chinese language is a commonly used language. It's one of the oldest languages, and one quarter of the people on Earth speak it. And giving this speech in Chinese, it's a scientific thing. It had absolutely nothing to do with politics. So I decided to give it in Chinese anyway. That was the reason.

INTERVIEWER: What did it mean for you, personally, to win the Nobel prize? I mean, I guess I'm interested in how it changed things for you. And also, maybe more generally, what do prizes like that mean?

TING: If you read the will of Alfred Nobel, the prize is given to people who have done the work in the previous year which is most important. Previous year often means 30 or 20 years. In my time, I was 40 years old. And for experimentalists, this is not considered old. And so having received the Nobel Prize was really quite helpful to me, because it opens many doors for additional support. Indeed, it was very useful for me. If you give me a Nobel Prize now, it probably is not useful.

INTERVIEWER: And as you say, some people receive Nobel Prizes for work that was done many years ago.

TING: Most of them in their 60s, 70s, 80s.

INTERVIEWER: So what kinds of doors did it open? What were the next things that you were able to do?

TING: People suddenly increase their support to you, and you have more access to leading scientists. And I think that's really very important. You have chance to talk to other Nobel laureates, and access to the government. These are important things for continuing to do experiments.

INTERVIEWER: So what projects were you interested in taking on at that point in your career, having just won a Nobel Prize at a quite young age and still in the middle of a research career?

TING: It didn't change me. I just continued to do my experiments. It didn't change me. When you are 60s or 70s, often you do something else. And I, like many Nobel laureates, are often asked to sign petitions, give public opinion. I've never done that, because I view receiving a Nobel Prize is a recognition of a particular, restricted contribution I have made to science. Perhaps this contribution is important, but this does not mean then I am an expert in sociology, in politics, and public policy. And so I have never engaged in that. Basically, I still concentrate on what I'm doing.

INTERVIEWER: At that point in your career, in the late 1970s, what were you interested in working on, and what were, in fact, you working on?

TING: Two things. I continued to measure the size of the electron. It fascinated me how small the size of the electron. And then we did a very important experiment, and that is forces between an electron and a nucleus is transmitted by light rays. Forces between quarks, which are the smallest element, is transmitted by something called gluons. That's a theory. And we did an experiment in Hamburg, and did see a trace of the gluons. That is considered a important experiment.

And then we also have done some experiments to see how many types of electrons are there. We know there's the electron go through the electric wire, and there are the electrons from space. Its mass is 200 times heavier than ordinary electrons. And there are the electron inside the nucleus. Its mass is 4,000 times heavier than ordinary electron.

Besides these three, we've never found the fourth electron. It doesn't mean it doesn't exist. It just means the energy we have is not high enough. You will never know whether there's only three types of electrons or not until you do the measurement. So that another thing I was interested in is what is the origin of mass, why different particles have different mass, what mass comes from.

And in '94, the United States had wanted to build a very large accelerator called Superconducting Super Collider. And because costs overrun, and because of very serious mismanagement, and so the Congress, actually, President Clinton essentially stopped this project. And then I began to think, I've been doing experimenting accelerator all my career, maybe it's time to do something new, to do something I know nothing about.

And then I asked myself, if the universe come from a big bang, before the big bang, it is a vacuum. What is vacuum? Vacuum means nothing exists. So at the beginning, you have an electron, you must have antielectron, called positron. Indeed, positron exists. If you go to a hospital, PET scans is a positron. You have a proton, you have antiproton. So you have matter, you have antimatter. But if the universe come from a big bang, at the beginning, there must be equal amount of matter and antimatter. Otherwise, you would not have come from vacuum.

So now the universe is 14 billion years old. Where is the universe made up of antimatter? Early in my career, while I was still at Columbia, I did an experiment with Professor Lederman, another Nobel laureate. We found an antiproton, ant-neutron forms an antideuteron. Namely, antimatter nucleus does exist.

But I asked myself, is our universe made up of antimatter? Because antimatter, when you enter into the atmosphere, annihilates with matter, so you can never detect it on the ground. So you have to go to space. And because matter, antimatter has opposite charge, so you need a magnet. Inside a magnet, a positive go one way, a negative go another way.

Now, put a magnet in space, that's a small problem. You know a magnetic compass one end will go to north, another end go to south. But if you are not careful, if you put a magnet in space, the magnet will always rotate, and then the space shuttle will lose control. That's why, for many years, people could not put a magnet in space.

In '75, we figure out a way to design a magnet that doesn't rotate in space, a small little trick, but it took 40 years to figure it out. And so I remember, let me see, ninth of May in 1994, I went to see the administrator of NASA, Daniel Goldin. I brought my collaborators to talk to him. And normally, high officers in Washington doesn't see poor physicist. But Goldin is a very curious person.

So he heard I had this interest, and he called me. He said, you come see me. So I brought my collaborators. So we went there at 9 am and we talked till about 12 pm. It was supposed to be one hour, it lasted for three hours. He's a very, very intelligent person. He asked many, many technical questions. And he said, why don't you come back this afternoon? I want to talk to you more. So I showed up in the afternoon.

And he said two things. He said, you may have some experience in accelerators, but space is very hostile. You've never done an experiment in space. We're not going to trust you. And the second thing he said is, I have no money. Very clear message. But I'm interested in this experiment, and if can put it into the space station, it's ideal for the space station.

And he said, you know, who supports you? I said, well, the Department of Energy supports me, and the Europeans support me, and also MIT supports me. And he said, now, we have an agreement with the Department of Energy, with the NSF, with many agencies, if they make a request to NASA, NASA could choose to honor it. So have your project reviewed by peers in the Department of Energy.

So I went to the Department of Energy, and they were somewhat frightened. The said, look, do you realize what you're doing if you're going to space? A, it's very costly. And B, this is not a Department of Energy agenda. I mentioned to them, what you want to do is to choose the best scientists in the United States. Choose among the Nobel laureates, a member of the National Academy of Science, to have a review. See whether the experiment we propose is worthwhile.

And it is important, in fact, my only request, is you choose a first-class reviewer, because a first-class reviewer can look at the distance and have a feeling what the goals are. A third-class reviewer will perhaps think, gee, if you give the money to Sam Ting, what will happen to my project? And to the credit of DOE, they indeed choose first-class reviewer. In the review committee, there's James Cronin, a Nobel laureate, George Smoot, a Nobel laureate, and the rest are mostly members of the National Academy.

And they came to MIT in April. And my memory could fail me. I think it's around the 20th of April for two days, reviewed us, and then they said, this is a very important experiment. And so DOE decided to support us. In their approval letter, in the standard manner of reviews, this is a good experiment, we should support you, but our budget is limited, you need to find most of your funds from Europe.

Because the quality of the review, it was easy for my collaborators in Germany, in Switzerland, in France, in Italy, and actually, in Holland, Russia to quickly support us, so quickly formed a collaboration. And Mr. Goldin, the administrator, also, in the first conversation, said to me, I want you to fly this experiment on a space shuttle once to prove you know how to do this.

And so we were formally approved in September, in '97. No, I think '96. Two years later, we were sent to space.

INTERVIEWER: So we've been talking about the long history and development of the Alpha Magnetic Spectrometer. And I think we had just reached the point where AMS 1 flew on the shuttle in 1998. Tell me a little bit about the next phase, as it were, once you demonstrated that you could fly to space and put this experiment in orbit.

TING: The next phase is to install on the space station. And originally, we were approved for three years. And space station was going to have many experiments. In face, after us, on the station for three years, another experiment was already scheduled. So we thought we would use a superconducting magnet. The strength is much stronger than a permanent magnet.

To build a superconducting magnet at minus 273 degrees below 0 Centigrade is not a trivial job. It has many applications. Therefore, the Russians, the former Soviets, has tried many times without success. But just technical things. And this was pretty much solved by the Swiss. And the ETH Zurich, the equivalent of MIT in Switzerland, did most of the work.

But then there was an accident with the Columbia. And so the United States decided to reprogram its space project centered on send people to Moon and Mars.

So in October-- I forgot which day now-- early October, I had a conversation-- I always invite my senior collaborator to go with me, which then NASA administrator, the honorable Michael Griffin. I think the dean of science, professor Bob Silbey went with me. And the conversation lasts for one hour. I present the project.

And Mr. Griffin, I think, is a very good engineer. And you can feel he understood things. And at the end of the hour, he said, what do you want to from me? I said, I want to carry out the original project agreed between NASA and DOE and 16 countries. And they have put in most of the funds.

And at that time, we had spent already about $1.5 billion on this project. Mr. Griffin said, I have no shuttle, but I will continue to authorize the Johnson Space Center continues to help you through development. But bear in mind, we have no shuttle for you. So all of a sudden, the projects going forward mostly were getting nowhere.

So I looked with my collaborators whether we can use a Japanese rocket, Russian rocket, even a Chinese rocket. But it was just not practical, because it was designed for a space shuttle, and the bottom is just too big, the weight is too large.

Fortunately, a month later, suddenly I received an invitation from the Senate Committee on Commerce, Science, and Transportation. The chairman was the late Ted Stevens of Alaska. Invited me and three other Nobel laureates to have a hearing on the future of science in the United States. Three Nobel laureates were still actively doing research. And at that time, I was not interested.

I don't consider myself qualified to discuss the future of science in the United States. My wife, Susan, said, no, you should use this opportunity to explain what you're doing. So I said, fine. So I said yes. But then later on I learned, when you testify in the Commerce Committee, you're given five minutes.

So in five minutes, you cannot explain a $2 billion experiment. So I said, could I please have two large TVs? And they said, yes. And so I came. The first speaker had a prepared text. The second speaker. And I was the third one, I believe. I showed, in five minutes, 10 new graphs. But for whatever reason, during my presentation, many senators came. Kay Bailey Hutchison and a few others all came.

After my talk, there were many, many questions, because, for the first time, the senators realized the space station is $100 billion, and there's not a single science experiment. And this did not please them. So after the hearing, the chairman, Senator Stevens, said, now the meeting's over, everybody should leave. But Professor Ting, could you stay?

So I obviously stayed, and saw him walk over, shake my hand, said, you come to have lunch with me. I said, well, my wife is here. And we'll have also a senior collaborator, Michael Kappl, with me. And he said, bring them along too? So went to Senator Stevens' office, and there was a party. It turned out to be that it was his 82nd birthday.

And then he took us to lunch, asked many questions. Why have not done and fly such an important experiment? Who hasn't reviewed this experiment? Many, many very tough, very intelligent penetrating questions. And I said to him, Senator, if you're so interested, why don't you come visit us in Geneva? He said, OK, let me see what I can do.

So that was October. In January, the senior senator from Texas, Kay Bailey Hutchison, reminded me to visit her, explaining the AMS project to her. And she was really very concerned with such a heavy investment and no science at all. And during this meeting with her, I suddenly received a phone call from Senator Stevens' office.

So my wife and I went to Stevens' office. And Senator Stevens said, didn't you invite me to come to Geneva? I said, of course. He said, well, I'm coming. And I was foolish enough to say, which airline do you take? And I go to the airport. He said, no, I have my own Air Force jet. So he came with very important congressional staff.

A person I particularly remember is Jeff Bingaman, truly a capable person. And from then on, I met many senators, Senator Nelson and Senator Vitter, and many congressmen, Joe Barton, Ralph Hall, many, many people. People in the Congress and the Senate began to realize, there's something wrong, of why the money come from foreign countries, so United States, A, has an international obligation, because it's a signed agreement, and B, you cannot build a $100 billion space station without doing science.

And so in 2008, the House and Senate unanimously voted a resolution, I think it's HR 6063, request NASA to add a shuttle flight to deliver us to the space station. And so this was signed into law by President Bush in 2008. And when President Obama took office in January 20, 2009, on the 23rd of January, we were on the manifest to fly. That's how we managed, mainly because people realized, a carefully reviewed, carefully examined experiment, paid by international community, it's ideal for a space station, and it should be used, and United States should carry out its commitment. That's how we're up there.

INTERVIEWER: And was it the second-to-last shuttle flight that it was on?

TING: Yeah.

INTERVIEWER: Right in May of this year?

TING: Yeah, May 19.

INTERVIEWER: So tell me now that it's arrived and has been installed on the ISS, what's the timeline? What are the expectations? What's the--

TING: Yes, there's a small interlude. The experiment was originally designed with a superconducting magnet, very difficult to build, but we built it. And then we shipped it to a thermal vacuum tank. A thermal vacuum tank is, change the temperature from plus 60 degrees to minus 40 degrees, and totally a vacuum. So you assimilated space. We found this magnet that only carries 2,500 liters of this very cold helium. It can last approximately three years, as they originally designed.

Just as we finished the design, finished the test, there was an announcement. It said the space station will last for 20 years, and we will be on the last shuttle. That means we will be on the station for three years, and the rest of the 17 years, we'll be a museum piece. And by then, it was somewhat late. And that was 2010, April already. It was supposed to fly next year. We had arranged for a C-5 to pick us up from Amsterdam.

Just before we mount on a truck, there was this announcement that the space station will last for 20 years. And so I got together with my senior collaborators, and mentioned to them, this will not do. We're going to have to make a change. We're going to take the whole detector apart and make a change, install the permanent magnet, but install more detectors so the resolution is the same, 80 more detectors. And NASA was very concerned that nobody can take such a complicated detector, 300,000 channels. But before that, in 2008, 2007, I've took apart the detector, assembled them, took apart, assembled them two or three times, mainly to check whether there's something wrong. So I know how long it takes. So I made a decision, mostly by myself, to change the magnet.

So we changed the magnet from a superconducting magnet to a permanent magnet. The permanent magnet is the one we flew for the first time. And everything was done very quickly. because the universities, and the industries, aerospace industries in Italy, in Germany, were extremely supportive. And so we managed to change it.

And then, August 26 last year, we flew with the Air Force C-5 to Kennedy Space Center. So that's how we're now in space. We're going to be in space for the duration of the space station, which is planned to be about 20 years.

INTERVIEWER: So tell me a bit about your expectations for the experiment. I mean, so it's a very long duration experiment now. What do you hope and what do you expect to see?

TING: Since nobody has ever done such a sensitive, precise experiment, the most important thing is to make sure our instrument is correct, to understand the property of the instrument, to monitor the instrument. Every information we get is new information. To predict the future is very, very difficult, but the most important thing is not to make a mistake, because if you make a mistake, you're going to change the course of physics for many years.

Given the fact it took us 17 years to build this experiment at $2 billion, I doubt that in the 50 years, people will be foolish enough to repeat this experiment again. And so I always mention to my collaborators, the most important is to make sure the instrument is correct, because everything we measure, people have not done before, because the energy and the precision.

INTERVIEWER: So you spent much of your career negotiating very difficult waters that encompass not just science, an extremely precise and difficult science, but also politics, budgets, money, international collaborations. I was wondering if you would talk about that a bit, in terms of what it takes to do that successfully. And also, maybe look at the future of that very, very complex relationship between politics, money, and science. I mean, you know, you've mentioned the Superconducting Super Collider, I mean, clearly the cancellation of that had a huge impact on experimental physics. How do you do it successfully? And what are the pitfalls?

TING: I'm not qualified to give a general statement. I can only mention my own experience. The fact every one of our experiments has yielded a correct result, we have never made a mistake, we have always treated our collaborators correctly, give them high visibility, is the way I managed to have such a large collaboration together. I do not involve myself in personality disputes, in financial disputes.

Like in Germany, or France, or Italy, they agreed with me they would build this detector. An experiment has many detectors, so each country take a responsibility of the detector. They agreed with me on a specification and procedures of tests. And I sometimes help them for them to meet with the government, and then they request their own funding. They do their own thing.

Every three months, everybody gets together. We'll have a meeting. The meeting normally has about 200 or 300 people. The meeting is chaired by me. And the meeting sometimes lasts a whole week. And I want to make sure every person who has something to say says it, whether it's a professor, whether it's a laboratory director, or even a young graduate student. My only requirement is only one person talks. If the young student talks, everybody is quiet, to listen.

And normally, I listen very, very carefully. I probably remember what this person has said now, what he said last year, what he said three years ago. If there's an inconsistency, I will ask the question, why? And very often, there are conflicts. A detector, the French think should be built like this, the Italians think should be built like this. And there's only one space, and I have to choose one over the other.

And this decision is done by me after carefully listening to all the presentations. But these decisions is always on physics ground. You cannot say, well, these people have spent so much money, if I don't choose theirs, then they will lose their jobs, this and that. You choose it because you think that is the best for the experiment. If I have not understood it, I do not make a decision. I will come back in two or three months again. I will ask the experts.

People stay with me because, so far, the decision has been correct. You can't be making random, wrong decisions, people will walk away from you. But it's very important to listen to everyone, particularly the young people. And do not let any outside influence, financial or political influence disturb you, because it's a very serious matter. Once it's in space, there's no way for you to change it.

INTERVIEWER: Do you think it's getting more difficult to launch these kinds of very, very large, time-consuming, and expensive experimental projects? I mean, is it getting harder to navigate these waters that we've been discussing or not?

TING: I would not know how to answer this. When I was much younger, I was doing small experiments, nobody believed me, so I had difficulties. Now I have people tend to have some confidence that do reasonable work, and people support me. But then the cost is so much, you have to be involved in so many things. So I don't know which one's more difficult for me.

In general, almost all my experiment has been somewhat difficult. There's always people who object. You have to be somewhat tolerant of other people's mistakes.

INTERVIEWER: Looking out over the next 20 or 30 years-- I don't know, I'll let you choose the timeframe--

TING: How about 20?

INTERVIEWER: 20. What are the really, really exciting questions that you think people are going to be tackling in experimental physics? And how are they going to tackle them?

TING: I would not know. Physicists, when you ask them to predict future, it's normally you're getting nowhere. When Brookhaven National Laboratory was built in 1960s, the original purpose, the review by the National Academy was to study nuclear force. What was discovered was to have two kind of neutrino, the J particle, the violation of fundamental symmetry called CP violation.

When Stanford Linear Accelerator was built, it was to study the property of electricity. What was discovered was partons inside a quark-- you interviewed Professor Freedman-- and the psi particle, which is the other part of J particle, and the third family of electrons.

So when you build something new, you ask the best expert to see what you can do. But expert is based on existing knowledge. The advancement of physics is to advance beyond existing knowledge. So it's very difficult to predict the future.

INTERVIEWER: It sounds like, early in your career, when you had a lot of people were very skeptical about the kinds of experiments you wanted to do, having a certain number of mentors and people who actually were willing to take you seriously and take your ideas seriously was very important. And I was wondering if you could talk a little bit about mentorship in the sciences, both as someone who benefited from mentoring and also as someone who presumably has become a mentor since.

TING: I would say the two person who really had influence on me, one is Professor Rabi, a Nobel laureate, who mentioned to me, the most important thing for a experimental physicist is to choose the right topic, to do research. And the second person, whom I knew quite well, Richard Feynman. But before I do experiment, I normally go to see him. Explain to him, he listens quietly, and often asks me a simple question. He says, look, Sam, if you don't do this, what else are you going to do?

And just from this personal interaction, I learn many things. Another person I think who I listen carefully is professor Steve Weinberg, used to be at MIT, now at Texas, who really is a very deep physicist. So I would not now call this to be mentors or not, but I talk to them a lot.

INTERVIEWER: Do you consciously try to play a similar role in encouraging younger physicists? Or is it something that just happens?

TING: I do not know. When I came to MIT in the physics department, they were mostly a very different department, many very good physicists. Now many years has passed, there are many professors have worked with me before. And of particular interest is professor Bolek Wyslouch. Professor Wyslouch and I met in the cafeteria at CERN on a Saturday afternoon. I was drinking coffee. He came dressed in very shabby clothes, and said, I just escaped from Poland, I have no identification papers, I have nothing, but I want to study with you. Opening sentence.

And I said, fine, come to my office. We talked for three hours. I asked him to explain to me what he has studied, and I asked him to ask me questions, instead of me ask him questions. Afterwards, I wrote a letter to the physics department. I said, this person has escaped from Poland. That was during the Cold War time. And I interviewed him. I spent three hours with him. And I want him to be admitted, and I will pay all the costs from the budget I have. And the department admitted him, and he's now a full professor at MIT.

Another interesting case, it's a lady physicist, Dr. Marion White, who now has an important job in Argonne National Laboratory. She is always afraid of examination. She's very scared. As a consequence, her grades are very poor. Everybody said, no, she doesn't belong to MIT, She Should leave. And I talked to her, and I had a different opinion. I found she really is very familiar with physics.

So at my urging, the department kept her. And I took her on as my student. And the day after she finished her thesis, there's a final oral defense. And I talked to her. I said, look, Marion, you cannot mess up with this. To keep you calm, I will ask you a question first. How is the spinning of pi measured? Discovered?

So I posed this question. I told her to answer. I said, I will ask you this just to calm you down. And so, next morning at 10 am, there was this thesis defense. So I asked Marion, how was this discovered? She said, oh no, you asked me this and you told me this answer yesterday and I forgot. Everybody started laughing. I explained to the committee, she's really a very, very good physicist. And so they let her go, and now she has a very high position.

INTERVIEWER: So you mentioned that the department at MIT had changed quite a lot in the last 40 years. Do you mean just personnel or in terms of approach, ambition?

TING: Approach. When I first came to MIT, every professor carries our their own project. Now we work at CERN, the experiment has 2,000 people. Imagine 2,000 physicists working at one experiment. And so it tends to be people that tend to collaborate together. The quality has not changed. We have a very good physics department.

INTERVIEWER: What is it about MIT that has made it a conducive home for you? We mentioned some of your colleagues and how they've helped you, but--

TING: I would say they have always supported. And the whole project removed from the shuttle manifest, MIT continued to support me. And the fact MIT supported me, the Department of Energy supported me carries a strong signal to the europeans that this is an important thing to proceed. When people believe in you and people want to support you, they'll find a way to do it. If people don't believe in you, no matter what you do, it will not change their mind. Do you agree?

INTERVIEWER: I do. So I'm winding down. I had one question that I wanted to ask that's a rather vague question. You've spoken several times about the power of experimental physics and this relationship between experimentation and theory. And it reminds me of the mind and hand motto of MIT. I was wondering if you could speak a bit about another kind of balance which is similar and is related, between sort of practical application and pure knowledge, and experiments and efforts designed to push one or the other. Are they in conflict or are they mutually supportive?

TING: What you really want to ask is, what has fundamental science, pure science to do with daily life, with industry? 100 years ago, fundamental science is mechanics, thermodynamics. Now, using aircraft, you're seeing in aircraft in many industries. In the '30s, the frontier science was atomic physics. At that time, people would say, well, what are those things used for? Now it's used for IT, for your communication. All these things that you're using today were in the '30s considered frontier science. In the '40s, the frontier science is nuclear physics. Now it's used in your energy and in defense.

So from the fundamental research to applications, there's a time lag. The time lag could be 30 years, 40 years. But once it's used, it changes everybody's life. If you don't do frontier research, you're never going to push the envelope in technology.

INTERVIEWER: So as we wind down, I'd like to just ask in closing, is there anything that we haven't touched on that you think is extremely important to include in this record?

TING: Well, extremely important to me is my wife, Susan. She had a degree in psychology, PhD degree. She graduated from university at the age when I went to university as a freshman. And after we got married and she realized-- this is the second marriage-- and she realized her family is much more important than her career. So she does a lot of administration job for our group instead of researching psychology. And she really is extremely supportive, extremely important to me.

INTERVIEWER: And you have three children?

TING: Yes, three children, two from the first marriage, one from the second marriage. And all of them are good friends with each other. They all make sure nobody studies physics. You're never at home. We never see you. But they are all very close to my wife.

INTERVIEWER: Well, I want to thank you for spending a couple hours speaking with us. This has been really fascinating and wonderful. Thanks very much for coming in.

TING: OK, thank you.