IPMU Interview

with Jerome I. Friedman

Interviewer: Kaoru Hagiwara

Interests changed from art to science at age 16

Hagiwara: Jerome, thank you very much for the inspiring lecture which we all enjoyed. Please let me start my interview with your biography. I learned that you were born in 1930, and that you were interested in art and music when you were very young.

Friedman: Yes, primarily in art. I did take violin lessons, but I had to make a choice between the violin and painting. It was during the Great Depression and my parents could not afford to give me lessons in both music and painting. I chose painting, and then, of course, things got so bad economically that I even had to stop lessons in painting. As a child, I would spend quite a bit of time drawing and painting, and I really thought I would possibly become an artist when I grew up. I went to a high school that had an art program that permitted me to draw and paint 3 hours a day.

Hagiwara: Three hours?

Friedman: I took very little mathematics, a course in algebra and one in geometry. I never had trigonometry. I had one course in physics that was so bad that all I could remember was that the teacher burnt his sleeve on a Bunsen burner. I was happily on my way to a career in art, and I was reasonably good in it. I won a national prize in art; and when I graduated from high school, I got a scholarship to the Museum School of the Art Institute of Chicago, which was the best art school in the area.

Hagiwara: I know the Art Museum, a landmark in Chicago.

Friedman: Yes. I was on my way to a career in art, but I decided not to accept the art scholarship and go to the University of Chicago instead. My interests started to change at the end of my third year in high school.

Hagiwara: Third year in high school is about 15 years of

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age or 16?

Friedman: Yes, about16 years. It happened as a result of a trip to the Museum of Science and Industry. You must know that Museum.

Hagiwara: Sure, of course, that's another landmark.

Friedman: I went there, and I bought a little book by Einstein. I had heard what a great man he was and had also heard about all the tremendously interesting things he had predicted about shrinking meter sticks and clocks that slowed down. I thought that this was really interesting and I really wanted to understand how it works. I spent the whole summer going through this book.

Hagiwara: I remember about the same age I also read a book written by Einstein and Infeld.

Friedman: This was a book very much like the book that you read. Supposedly if you knew algebra, which I did know, you could derive the Lorentz transformations, which I did. But I never really understood them, because the real issue in the derivation, which is a very deep issue, is why the velocity of light should be invariant in all inertial reference systems. As a teenager with no background, it didn't make any sense to me. I didn't understand it, and I decided that I would like to pursue that question and others like it. It was kind of audacious because I had an art scholarship and yet I decided to go to University of Chicago instead.

Hagiwara: I see, so you declined the scholarship from the art school and then decided to go to University of Chicago.

Friedman: Yes.

Hagiwara: Was it easy to enter the University?

Friedman: Yes, it was very easy. I was quite good academically. I got very good grades in all the courses I took. As a result of that, I got a full scholarship to the University of Chicago. I never paid a penny for my entire education.

Hagiwara: That's great.

Friedman: My parents had such economic problems that that was the only way I could go to the university.

Hagiwara: I see. When you went to University of Chicago, did you already know that Professor Fermi was there?

Friedman: Oh, yes.

Hagiwara: I see. He was very popular.

Friedman: Everybody in Chicago knew about Professor Fermi. I felt very lucky because I couldn't afford to go to a university anywhere else. First of all, I lived at *home*, and I took the streetcar to the University. I lived in the west side of Chicago. It was a long trip, but I went every day. For the first two years, I had the Great Books Program—a set of courses in which you read many of the great books of Western Civilization.

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Hagiwara: Western Civilization? That was what you first had about 2 years of liberal arts studies.

The days with Fermi at Chicago

Friedman: Yes, that's it. Then I went into the physics department, and initially that was very difficult for me. Fermi had the point of view that everybody who wanted to get into physics, could enter the Department with almost no restrictions. That's why I could get into the physics department, but I had to catch up in mathematics very rapidly. There were very difficult examinations and the flunk out rate was very high. At the beginning, I was struggling. I remember the first course I took; it was physics 105, 6, and 7. They covered in only 1 year what we used to cover in 2 years at MIT; and every Friday, there was a very difficult examination.

Hagiwara: Every Friday examination?

Friedman: Every Friday, and the average was about 20 to 30 out of a 100. There were times that I got a very low grade and I wondered whether I had made the right choice. About 125 people started in this course and about 35 people finished.

Hagiwara: Only 35.

Friedman: The reason most of them left was because they couldn't take it. It wasn't that they were forced to leave. You would take a test every Friday and you would get less than 50% of the test correct almost every time. These were all good students. They just couldn't take it.

Hagiwara: Did Fermi make those examinations?

Friedman: No. The individual who taught that course said in the first lecture, "most of you are not smart enough to be a physicist." What saved me was that if I got a bad grade on an exam, I said to myself, I don't know anything. What do I expect? I caught up, and I was able to pass all my exams. And so I was able to stay and continue my physics education. After 2 years they gave a long exam called the qualifying exam, and then another one called the basic exam after the third year, which was the Ph.D. qualifying exam, and that was a 3-day exam, 6 hours a day. About 50% of everybody who took each of those exams failed out. Basically one quarter remained, and it was really tough, but it was really enjoyable in the following sense: it was a very active place. Fermi was such a brilliant man, and the best physicists in the world would come and visit and then give talks. We heard the talks, and I saw some of the great physicists. Among those who visited were Pauli, Heisenberg, and Feynman, and a very young Gell-Mann taught at Chicago at the time. And of course, there was Fermi. Here is where I had a lot of audacity. I said to myself, "Look, maybe I am not the best student in the department, but I want to work with the best professor." So I went to see Fermi, and I asked him if I could work for him. I thought he would say, well, tell me how you did in this course and that course. Tell me how you did in the basic examination. He didn't ask me one thing. He just said yes, and I couldn't believe it. It was like I won the lottery. I worked for him until unfortunately he passed away during my thesis. He was a wonderful man, and it was just fabulous to see him think about physics problems, and do them in front of you and explain things. He was a brilliant man and a very kind and nice man. In addition, there were wonderful doctoral students in the Department. One of my fellow students was Tadao Fujii who was a good friend of mine. At this time, Koshiba was a young postdoc at Chicago, working in Marcel Shein's cosmic ray group. I often met him and would converse with him.

Hagiwara: Have you met Nambu?

Friedman: Yes, Nambu was a young assistant professor when I was a student. Even at that point, he had a very high reputation, and it just continued to go up. He is an incredibly gifted theorist, and I was so happy when he got the Nobel Prize because it was a wonderful recognition of the wonderful works he has done. I never had a course with Nambu, but I heard him give lectures, and I talked to him on a few occasions. He is very friendly, but very shy. You have to go up and approach him to talk to him because he is so shy. But I did, and he was always extremely friendly. Fermi had assembled an extraordinary faculty. Being a student in the department was an absolutely wonderful experience.

Hagiwara: Yes. I can imagine.

Friedman: I was so lucky. I was incredibly lucky.

Hagiwara: I have a next question. According to your biography, your research thesis work was on proton polarization in the emulsion experiment.

Friedman: Yes, that is correct.

Hagiwara: The subject was by using the polarization of the scattered proton, you wanted to learn if it is coming from elastic or inelastic scattering. Was it the subject which Fermi gave you or...?

Friedman: That's right. It turned out that in those days, it was noticed that if you scattered protons off a carbon target, they had a high degree of polarization.

Hagiwara: I see.

Friedman: One didn't know what the process was, whether you were actually breaking the carbon up, exciting carbon, or scattering elastically. Fermi wanted to know, but what I didn't know was that he already had made a calculation of this process. But he didn't tell me about it because he didn't want to influence my results. His calculation indicated that there was a very high degree of polarization in elastic scattering. He always had a great interest in the effects of spin. In fact, Maria Mayer got the idea of LS coupling, which was basically the key to unlocking the shell model, from Fermi. He thought that the LS couplings could produce this polarization. He did a calculation in which he put in a real and imaginary potential, an LS coupling, and he got a very high degree of polarization in elastic scattering. It turned out that when all results came in, they matched his calculation beautifully.

Hagiwara: Really.

Friedman: I did my own scanning of the exposed nuclear emulsions. I looked at 300 meters of track, 150 microns of field of view, and you can imagine what that was. My progress was very slow. When I was in the middle of my measurements, I got a troubling surprise. Segrè did the same experiment using counters, and the results confirmed Fermi's calculation. I felt that I had wasted the year and a half because I had been scooped. I went to see Fermi, and he was very kind about it. He said, "Don't be upset. You can also look at the inelastic scattering and make a comparison, and you can publish both sets of measurements," which I did. I was able to get a Ph.D. as a result of that, but unfortunately by the time I finished my thesis Fermi was dead. He died very tragically. He went to Europe one summer, the summer of '54, and developed a very rapidly advancing cancer of the stomach. When I saw him in the previous spring, he looked very robust. When he came back to Chicago in the fall, I saw him at a distance walking in the hall. I looked at him, and I could barely believe that was Fermi, because he looked so gaunt. I waved to him and he waved back to me, and then he went into his office. The next day, he went into Billings Hospital for exploratory surgery, and they found that the cancer was inoperable. They sent him home to die. But I want to give you an idea of what kind of man he was. When he was in the hospital, Herb Anderson, and Chandrasekhar, the great Indian theorist, went to see Fermi for the first time after the surgery. It was a very awkward situation, what do you say to a man who you are seeing for the first time after he has been given a death sentence, what do you say to him? They came in, and they were obviously at a loss for words, and Fermi noticed this. He said, "Chandra, tell me, when I die will I come back as an elephant?"

Hagiwara: Elephant?

Friedman: Yes. They all just broke out laughing. That broke the ice and then they had a wonderful conversation. He was obviously concerned about their feelings, and though he was on his death bed, he made a joke to get them to feel at ease. That's a real human being.

Hagiwara: I see, yes. It's a wonderful story of him. Thank you very much. Now, let me go ahead. After you got your Ph.D., you worked at Chicago and then you moved to MIT and…

Started working on electron scattering

Friedman: No, I went to Stanford first before MIT. I worked for 3 years with Hofstadter at the High Energy Physics Laboratory.

Hagiwara: Where you started working on electron scattering?

Friedman: Yes, I learned electron scattering there. It was a very fortunate move for me because I had been doing emulsions, and by the time I finished my thesis, it was a dying technique. I decided I didn't really want to go into bubble chambers because I had had enough of images. I had looked at images so many times I was tired of looking at them. I wanted to learn how to make measurements electronically.

Hagiwara: Starting from emulsion and then you moved up to counter…

Friedman: Yes, so I got a job with Hofstadter's group in electron scattering. There I did a number of things, but the experiment I remember the most, one that played a role in my later thinking was a measurement I made using a result of Drell and Schwartz. They did a calculation in which they showed that if you measure electro-disintegration of deuteron and you sum over all inelastic states, you could find out something about the exchange force of the deuteron. That struck me as an interesting result. I was really fascinated by the idea that by looking at all the inelastic states, you can learn something about the ground state.

Hagiwara: I see. So is that the first time you learned about inclusive measurement?

Friedman: Yes, and I learned how to do the radiative corrections from that, and found them to be quite complicated. In electron scattering, the radiative corrections can occur in two ways. The electron can emit a photon before scattering, in which case you are scattering at a lower energy than your beam energy, and you have to make a correction using a cross section you have never measured. Or the photon can be emitted after scattering, and that is relatively easy to correct for. But emission before scattering means that you have to learn something about lower energy cross sections, and every time you do that, you have to go to lower and lower energies.

Hagiwara: That is true, yes.

Friedman: It's a problem, and you have to learn how to do that. The other thing this measurement told me was that there might be something you can learn from the sum over states. When we decided to do inelastic inclusive measurements at SLAC, I already had done that with a deuteron, and I said to myself, I am not sure what we are going to learn about the proton, but it could be interesting. That electro-disintegration of the deuteron experiment turned out to be a very significant one for me. Now, was I successful in finding the exchange forces of the deuteron? The answer is no. I am going to tell you what happened. I did this experiment and evaluated the results, and I realized that there were some terms that Drell and Schwartz left out of their calculations. I went to see Sid Drell, and I talked him about it. He said, "Oh, yes, you really should check them…" and I sat down and spent 6 months calculating the effect of these so called gauge terms. I put the gauge terms into the calculations, and it turned out that they made the results inconclusive. I still published the result because I was able to show that the gauge terms are important and you had to put them in. I wasn't able to provide new information about the exchange force, but two things I did learn. I learned about radiative corrections and I learned about sum rules.

Hagiwara: Yes, I understand, because that's directly related to your very important work. I see. Then you went to MIT? The Cambridge Electron Accelerator (CEA)?

Friedman: Yes, the CEA. Henry Kendall and I were colleagues at that time and we led a group together. We wanted to do electron scattering from the proton at the CEA, but we weren't permitted to do that because somebody else was using the spectrometers for an extended period of time.

Hagiwara: I see. That is actually the reason why you went to SLAC.

Finding the magic to do research at SLAC, 3000 miles away, and yet teach at MIT

Friedman: Yes. We realized that if we wanted to study the proton, we had better go elsewhere. So Henry and I decided to try to go to SLAC and that was a difficult thing because how do you teach and do research 3000 miles away? That was an incredibly crazy idea that Henry and I had.

Hagiwara: How could you manage?

Friedman: There was a wonderful man who was Head of the physics department at MIT at the time. His name was Bill Buchner and I am eternally thankful to him. Henry and I, we went to see him, and we were perplexed as to what we could do. But we thought we should ask him what could be done to allow us to do research at SLAC. We thought the answer would certainly be no, that we better continue working at the CEA. We told him what research we wanted to do, and he said, "Simple, I know how to fix it." We were very surprised. What was this magic that could fix this problem? He said to Henry and myself, "You two will teach one course. Each of you will get a salary, and Henry can be away for 2 weeks and Jerry can teach and Jerry will go away for 2 weeks and Henry will teach," and that's what we did. For a number of years, I would be in California for 2 weeks and he would teach and then I came back to teach and he would go to California; and we built up a group there. In fact, in the United States, I think we were the first group that did research more than 1000 miles away from our University, establishing the first distant user group.

Hagiwara: Yes, that's really hard.

Friedman: It's true that Columbia would go to Brookhaven to do research, but that's relatively close. 3000 miles is a different thing.

Hagiwara: Yes, completely different.

Friedman: We set up a group. We moved postdocs and students to SLAC, and before we knew it, we were a presence there, and we played a role in building up the spectrometer complex, preparing experiments and carrying them out. It was really a good time.

Hagiwara: I have a question about your deep inelastic scattering experiment. You said that first you measured proton form factor, elastic scattering, and after finishing this, you could of course measure the form factor to really short distance, but that nothing was really interesting.

Friedman: We didn't really learn anything new from measuring the form factor, and we decided to abandon it. There was no point in trying to do it more precisely.

Hagiwara: You then proposed to do inelastic scattering.

Friedman: Yes.

Hagiwara: At that time, you said that absolutely nobody believed that you can find anything by doing this, but you should have had some picture probably or…

Friedman: I did. I can't speak about the motivation of Henry or Dick or the others. I can only speak of mine. My motivation came from the deuteron experiment.

Hagiwara: Yes.

Friedman: That's where it came from.

Hagiwara: You probably wanted to learn something about the force which makes up the proton.

Friedman: You could learn something because in analogy to the deuteron experiment the sum over exited states could provide information about the ground state.

Hagiwara: I see. It was really important.

Friedman: That was a lesson to me. I didn't know what we were going to find. We were certainly not looking for quarks, and Henry may have had a different motivation. I don't know what Henry's motivation was. He probably told me, but I can't remember anymore, and I don't remember what Dick said, but I can tell you what I remember about myself. But, the program committee wasn't very happy about our going in this direction. What we promised to do was to measure the inelastic excited states, namely the resonances. Of course, if you do that, you have to also measure the continuum because these resonances are sitting on the continuum.

Hagiwara: Excuse me, so the PAC approved your experiment because they thought that you can measure resonances.

Friedman: Yes. They weren't very happy about our just measuring the continuum.

Hagiwara: At summing up.

Friedman: Yes, summing up. We measured both. We measured the resonances, but we never published these results.

Hagiwara: Never published? That's a revenge.

Friedman: No. Because what happened was that the inelastic became so interesting and important, that we didn't take the time to publish the resonance results. I will tell you how we found out that the continuum spectra were so interesting. I went to a few theorists and asked them if they would give us some predictions of what we could expect in measuring the continuum. Nobody was interested. They thought it was garbage. After all, in the continuum you have all these processes, and how do you include them? Nobody would calculate it.

Hagiwara: Did you ask Bjorken?

Friedman: Bjorken did not calculate the cross-sections at all. He calculated sum-rules.

Hagiwara: I see.

Friedman: Now, Bjorken was possibly talking to Dick and to Henry, and I don't know exactly what Bjorken was doing, but I can tell you what I did. After having watched Fermi approximate complicated phenomena, I said to myself, why not just try to calculate a very crude model. I will tell you what my crude model was, and you'll see that it was actually not terribly unreasonable. If you have electron scattering, you have the virtual photon, right?

Hagiwara: Sure.

Friedman: If you have an inclusive process, it's the total cross-section for the virtual photon. So, the first thing that will come in is the total photo production cross-section at the energy of the photon. But there has to be a correction because the photon is virtual. If you look at the Feynman diagram, there is a vertex correction, which is a function of Q^2. You don't know what that function of Q^2 is, but there's one thing you do know, it can't be a function of only Q^2 because Q^2 has units. You have to have something else. You have to have Q^2 times a distance squared so that it is a unit-less number. This means you have to put a distance in it.

Hagiwara: Is that the proton size?

Friedman: That's exactly right.

Hagiwara: You knew it from your form factor experiment.

Friedman: Yes. That contains the proton size. In fact, the most efficient thing is to just put in the proton form factor, because that's the best reflection of proton size. Calculating the inclusive cross section from the proton form factor along with the total photo production cross-section provided a template for what we could expect. So we then could simulate what we would measure, by putting in the radiative corrections. We started measuring, and at one point, the measurements were a factor 5 bigger than this calculation.

Hagiwara: Yes, when your Q^2 is not too large.

Found point-like objects in the proton

Friedman: We thought, well, it's okay. A factor of 5 is not surprising for such a rough calculation. Then we observed a factor of 10, then a factor of 100, then a factor of 1000, then a factor of 10,000, and we knew there was some new physics here.

Hagiwara: Is that the detector which you showed us in your lecture? With its rail you gradually start from the low angle to the large angle?

Friedman: Yes, that's right. I didn't show you a distribution because it was supposed to be a popular lecture. But if you look at the structure function as a function Q^2, it's flat. It has a little turnover because it has a kinematic limit. To me that suggested point-like structures immediately. If there is a finite size, it has to come in and the structure function begins to fall with increasing four momentum transfer. If you look at inelastic electron scattering from a carbon nucleus it has a spectrum like that of the proton. You get excited states and then you get a broad continuum. If you look at the momentum transfer distribution in the range of the maximum of the carbon continuum, you get the form factor of the average nucleon. It's called quasi-elastic scattering. Basically, in the same way you can talk about quasi-elastic scattering off some tiny objects in the proton.

Hagiwara: You measure electron scattering off carbon atom nuclei and the size of the carbon nuclei matters at very low momentum transfer. Beyond it, you start measuring the proton…

Friedman: That's right, exactly. Because it is no longer coherent scattering.

Hagiwara: You already knew that.

Friedman: That's right and it becomes incoherent scattering. Once you get into the incoherent range, you are looking at the form factor of the nucleon, and of course, you have both the neutron and proton. So you are looking at the average form factor for the nucleon. It became quite clear, and we were already getting the idea of point-like objects in the proton. At about the same time, Henry was looking at BJ scaling, and it appeared that scaling was working. I don't think we really understood what scaling meant at that time. In retrospect, it is clear that scaling implies point like behavior, but at that time we didn't understand it. I am sure it was in BJ's mind, but I don't remember that he ever discussed it with us.

Hagiwara: If there were no other scales…

Friedman: Yes, right, exactly.

Hagiwara: …and then that should depend only on a ratio of kinematical scales.

Friedman: Exactly. If there is no size for Q^2, it has to be a ratio; but at that time I didn't understand that. By1968, some of us had the point of view that there was possibly point-like structure in the proton, and the Vienna Conference was coming up. I was going to be the representative for the group at that conference.

Hagiwara: Vienna?

Friedman: Yes. Before I left, I had to talk to the group to find out what they would permit me to say. Henry and I and few others, wanted to mention the idea that there could be point-like structure in the proton. The rest of the group objected, saying that such an interpretation was too bizarre. The group took a vote, and we lost.

Hagiwara: You lost!

Friedman: When I gave my paper, I never mentioned the possibility of point like structure. I just showed the plot of the Q^2 dependence. Panofsky gave a plenary talk on our sessions. In the plenary talk, he said about two sentences about our experimental results indicating the possibility of point-like structures within the proton. He said nothing more and nobody paid much attention to it. It was just like dropping a stone in water, you get few ripples, and then nothing. It was so far from people's thinking because first of all, it implied a field theory. In those days, field theory was thought not to be valid for the strong interaction. Nuclear democracy was the prevailing theory, and nobody wanted to talk about the possibility of point-like structure. But we kept on working on that possibility. Then, we used the Callan-Gross sum rule and found that if there were constituents in the nucleon, their spin was one half. We then used another sum rule which when combined with neutrino-nucleon inelastic scattering demonstrated that these constituents have the fractional charges of quarks. That was it. It took a number of years, but it was a very exciting time and I had a great time doing it. That's a long answer to your short question.

Hagiwara: Thank you very much. That was a very, very fascinating story. I entered graduate school after all your works were completed.

Friedman: Where did you get your graduate school education?

Hagiwara: I went to Tokyo Metropolitan University, and it was in 1974 when J/ψ particle was discovered.

Friedman: Yes.

Hagiwara: Even in those days, at least in my small university, field theories were not at all popular.

Friedman: They weren't popular at all.

Hagiwara: Actually, my Professor (Tetsuro Kobayashi) was doing a Regge phenomenology for pion-nucleon scattering physics, but after this J/ψ discovery, he told me that I should not really study what he is doing, but instead his recommendation was that I should read all the preprints coming from SLAC.

Friedman: He was a wise man.

Hagiwara: Yes, indeed. That's how I started my graduate studies. I was just reading and reading and reading. In those days, I read many articles written by Bjorken, Feynman, Glashow… I really learned from those physicists.

Friedman: Things were changing so rapidly. The whole standard model was coming into play at about that time.

Hagiwara: Yes, I know that was all based on your work and many theorists' work. Let me see what else I should ask you. I have already asked the most important questions. Actually, I prepared two last questions.

Friedman: Sure.

Hagiwara: They may not be so kind.

Friedman: That's okay.

The whole high energy physics is now global

Hagiwara: My first question

is this. When I was studying in the 70's, most high energy physics results were obtained in the United States, and it was in '82, when I was in Madison as a postdoc, the W and Z were discovered at CERN. Now it seems almost everything at highest energy is coming from Europe. Do you have any picture for the future of high energy physics in the United States? What is your opinion?

Friedman: I think in general the whole high energy physics has changed, and it's now global, and every facility will have to be multinational. I think CERN is a good example of what it is, and it's hard to know what will exist in a country in 10 or 15 years. I would say that in the near future it will be difficult to build any new facility because of what's going on in terms of the economic meltdown and deficits. When the countries come out of it, and they will come out of it because we have been there before, it's not clear where the next big facility will be. But wherever it is, it is going to be multinational. The Super B-factory you are going to have will be a national project because it is still of a magnitude that can be paid for by a single nation although I am sure you would like to get international participation, which I am sure will happen.

Hagiwara: Yes, of course.

Friedman: But if you start getting into an ILC or CLIC or something like that, we are going into TeV region and we are talking about multi-billions of dollars. It's going to be multinational and where it goes will be less important than having it in some place and having international participation. I think whether a nation will be in the field will depend on whether it wants to join multinational efforts. Even though the US will not have a large accelerator after the Tevatron shuts down, we have a large number of physicists working at CERN, well over a 1,000, which is pretty substantial.

Hagiwara: Yes, so that's an extended version of your commuting between Massachusetts and California.

Friedman: But things have gotten a lot better since those days because now we can communicate much easier. Now we can have real meetings at a distance, which we couldn't have in those days; and that is happening all the time. You are going to move bits. You are not going to move people, and people participating in an experiment will be participating from home.

Hagiwara: The location of the lab doesn't matter.

Friedman: Yes, it's great. It's changing, and as time goes on, it's going to change even more.

Hagiwara: I can imagine. Do you have any comments about Japanese High Energy Physics? We have been kind of concentrating on efforts towards flavor physics. Japanese community has been quite successful in flavour physics experiments, such as B-factory and neutrino experiments.

Friedman: Those are very important areas, and Japan has always had a very high level of physicists. I have been interacting with Japanese physicists since my very early days as a student because, as I said, I went to school with Tadao Fujii and knew Nambu and Koshiba at Chicago.

Hagiwara: Yes, you told us.

Friedman: Tadao was a classmate of mine. He was a terrific fellow and a very capable physicist. Look at what Koshiba has done. Nambu is one of the greatest theorists of our time. Japan has a great array of physicists who have done extremely important things. The B-factory and neutrino programs have been a great success, and I think Super-B-factory and the programs at J-PARC will also be great successes. You do things in a careful, thoughtful manner; and you are willing to try difficult things and you make them succeed.

Hagiwara: Yes, I think so. I was amazed also.

Message to the political leaders, both in Japan and in US

Friedman: The Belle and Kamiokande programs were very impressive, and got beautiful results. I think Japan has done extremely well, and it will continue to do well as long as it keeps on investing in research. Even though particle physics does not have known applications, the spin-offs of its technologies will provide the nation with economic benefits. The training of your students in the new technologies utilized in particle physics will give rise to innovations that will be enormously beneficial to the economy, and that's the message you have to convey to your political leaders. We have to do the same thing in the US. We have the same problem. As budgets get tight and problems develop, there is a tendency to want to spend money primarily on projects that can provide immediate economic benefits. But if you stop spending money on basic research, you are going to lose a lot. That's the message you have to get across. If your government keeps giving good support to your science, you will continue to do wonderful things.

Hagiwara: Okay, I understand. This is my last question. If you have anything…

Friedman: No, I think I have said enough.

Hagiwara: Okay, well, then, thank you very much.

Friedman: Oh, you're very welcome. It was a very interesting interview because you made me think about things I haven't thought about for some time. Thank you.