Round Table Talk :
Conversation with Takaaki Kajita

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Participated in Proton Decay Search Conducted by Kamiokande

Murayama: Congratulations on the Nobel Prize! I think you must be extremely busy.

Kajita: Yes, the number of e-mails I receive has significantly increased, for instance···

Murayama: You will feel things are tough for a while. Thank you very much for attending this round-table talk even though you are so busy. I'd like to start with a question — why did you decide to join the Koshiba group when you entered the Graduate School of Science at the University of Tokyo?

Kajita: As I wanted to participate in a particle physics experiment, I had a choice between the Fujii-Kamae group and the Koshiba group. Honestly speaking, I didn't have an idea about which to choose.

Murayama: Why did you want to participate in a particle physics experiment?

Kajita: As I was young, I hoped to do research in fundamental science such as elementary particle physics.

Murayama: Were you immediately involved in the construction of Kamiokande?

Kajita: I had not visited the construction site at all. It was just when the first batch of 20-inch PMTs (photomultipliers) were being produced.

Fukugita: Were you not involved with designing Kamiokande?

Kajita: No, not at all.

Murayama: Do you mean that when you entered graduate school, Kamiokande had been already designed and Professor Koshiba was leading its construction and you just joined it?

Kajita: Exactly.

Fukugita: When did you enter graduate school?

Kajita: It was April in 1981.

Murayama: At that time, was the Koshiba group still involved in an experiment at DESY?

Kajita: Yes.

Murayama: Who decided a graduate student would do which experiment?

Kajita: I have no idea. At the beginning of 1981, Katsushi Arisaka was writing his master thesis by conducting a Monte Carlo simulation study of the Kamiokande experiment. I had been encouraged by him to work on proton decay search before I realized it [laughs].

Murayama: Was the IMB experiment already running at that time?

Kajita: It was under construction.
Murayama: It is usually said that Prof. Koshiba worked on the development of 20-inch PMTs in order to compete with IMB. Was it really so?
Kajita: I heard that story only indirectly as you did.
Murayama: Then, the construction of Kamiokande started. I guess there were only a few members in the team at that time — Atsuto Suzuki, Arisaka, you, and…
Kajita: Prof. Teruhiro Suda of the ICRR (Institute for Cosmic Ray Research) and a few more.
Murayama: Yoji Totsuka was not there at that time?
Kajita: Prof. Totsuka returned from Germany in the spring, probably in May, of the year I entered graduate school. He was doing R&D of the OPAL experiment, but helped us because the Kamiokande team suffered from lack of manpower [laughs].
Murayama: At that time, was the Kamiokande project being advanced entirely by the Physics Department at the Hongo Campus?
Kajita: No, Prof. Suda of the ICRR was a member, and Prof. Jiro Arafune, who helped us with theoretical issues, was also at the ICRR. He then moved to the Tokyo Institute of Technology.
Murayama: When was Kamiokande completed?
Kajita: In July 1983.

Kamiokande Observed Neutrino Burst Soon after Being Upgraded

Murayama: From then, how had it been running till the supernova explosion in 1987?
Kajita: Prof. Koshiba was great. I don't remember the exact month, but in the fall of 1983, he already proposed upgrading Kamiokande so as to observe solar neutrinos. He also put forward the idea of Super-Kamiokande, because Kamiokande was too small for observing solar neutrinos. I think the upgrading of Kamiokande started some time in 1984.
Fukugita: Though Prof. Koshiba was aiming to make solar neutrino observations, the primary reason for him to have proposed Super-Kamiokande was his clear recognition that proton decay would never be discovered unless we had a larger detector than Kamiokande.
Murayama: Did he think so because of IMB? When was the first $p \to e\pi$ limit reported by IMB?
Kajita: It was in 1982.
Murayama: At that time, was there a prevailing atmosphere that Grand Unified Theories would not work?
Fukugita: We did not conclude so, but …
Yanagida: The prevailing atmosphere was that we needed a larger detector.
Fukugita: There were lots of arguments in favor of $p \to \nu K$.
Murayama: SUSY-GUT (Super-symmetric Grand Unified Theories).
Kajita: Yanagida-san, you pointed out the importance of νK. Was it around 1981?
Yanagida: Yes, it was pointed out by Weinberg and by Sakai-Yanagida. It may be that Prof. Koshiba envisioned νK around that time. Anyhow, he is very quick in identifying theoretical issues. In 1981 or in the first half of 1982, I was requested by him to give a seminar on the seesaw mechanism of neutrino mass because he was interested in it. At that time, he was very impressed with my talk, but other people weren't at all [laughs].
Murayama: He has a good sense.
Fukugita: His sense is extremely good.
Yanagida: Yes it is. His flair… How should I put it?
Murayama: So, the Kamiokande team realized that a larger detector was needed for a proton decay search and decided to switch Kamiokande to solar neutrino observation. For that, you constructed an anti-counter and lowered the threshold. Did you then start a new phase aiming to make observations of solar neutrinos and supernova explosions?
Kajita: We did not consciously aim to observe supernova explosions.
Fukugita: A supernova explosion happened by chance when the threshold was successfully lowered.
Murayama: And just a month before it was not possible to observe it because the radon level was too high. As Prof. Koshiba was retiring in a month, there was only a two-month window [laughs]. A supernova had exploded exactly 160,000 years before the middle of this window. Impossible!
Yanagida: Incredibly tiny probability.
Fukugita: Originally, the threshold was near 100 MeV, wasn't it?
Kajita: No, it was 30 MeV. Probably the analysis threshold was about 100 MeV, but it was for electrons.
Fukugita: Around that time, Rubakov pointed out that monopoles, if they existed, would catalyze proton decay.
Murayama: Yes, Callan-Rubakov.
Fukugita: Then, a 30 MeV muon neutrino would be emitted. So I told Prof. Totsuka many times to lower the threshold so as to detect it.
Murayama: That's interesting.
Fukugita: Yes, Prof. Totsuka said it was possible to lower the threshold down to there, and he worked hard with Atsuto Suzuki to achieve it. So, before lowering the threshold down to 6 MeV, there was an intermediate stage to detect 30 MeV neutrinos.
Yanagida: I didn't know that story. I didn't understand why they aimed

at monopole-catalyzed proton decay because I couldn't imagine that experimentalists knew Callan-Rubakov. Fukugita-san, you suggested···

Fukugita: So, they succeeded in lowering the threshold···

Murayama: But, it must have been very hard to further lower the threshold below 10 MeV.

Fukugita: It was really hard, in particular, to lower the radon level.

Murayama: What was the radon removing process?

Kajita: First of all, we didn't know about the existence of radon. We thought that the new hardware would allow us to lower the threshold down to 5–7 MeV as far as we took the accidental rate into account. But actually we found a trigger rate of more than 1,000 Hz. We started by wondering what the reason was.

Murayama: Do you mean that it was difficult to understand it?

Kajita: Prof. Atsuto Suzuki was great. As the trigger rate was 1,000 Hz, he stopped the pure water system. Then, the trigger rate rapidly dropped, and from its decay rate he found that radon might be the reason.

Murayama: How did you move forward with removing impurities from water?

Kajita: First of all, radon decays. So we had to prevent Rn from leaking into the Kamiokande detector. At first, we continuously supplied fresh mine water, that passed through filters, into Kamiokande because mine water was sufficiently clean.

Murayama: I see. At first, you were not using the water recirculation system.

Kajita: That's true. At the level of Kamiokande, we only needed to recirculate water while keeping its purity. But, as our pure water system was not very good, it was very difficult to do this.

Murayama: So, you managed to lower the threshold down to about 10 MeV only about a month before the supernova.

Fukugita: Probably it was lowered to that level around the end of December, because it was 6 to 7 MeV in January.

Kajita: I don't remember any more, but probably you are right.

Murayama: Then, a month or two after the threshold had been lowered, a neutrino burst suddenly happened. What did you feel at that time?

Kajita: Then, I was staying at CERN. So, I didn't know about it. As I was formally affiliated with the ICEPP at the University of Tokyo, it was my duty to help with the OPAL experiment from time to time.

Murayama: By chance, I heard Maurice Goldhaber's speech at a dinner party before his retirement. He first showed a slide of the Kamiokande's neutrino burst events, and then another slide of the IMB's events. He said, "The IMB events have higher energies. This means that IMB observed the burst slightly after Kamiokande. So, Koshiba got it, but that's life" [laughs]. What was the actual relation?

Fukugita: Kamiokande was asked by IMB about the burst timing. As IMB had a higher noise level, they searched for and found events at around the timing which they had been informed of.

Murayama: Without knowing Kamiokande's timing, IMB would not have been able to find the events?

Fukugita: I think it would have been difficult. It may be that they would eventually have found them, but not so quickly like that.

Murayama: It sounds like it was important.

Fukugita: Yes, it was extremely important information.

Deficit of the Muon Neutrino Flux

Murayama: After the supernova excitement, Kamiokande finally entered an era of solar neutrino observation. Were you involved with the solar neutrino analysis?

Kajita: No, I was not.

Murayama: Have you been working on atmospheric neutrinos since then?

Kajita: Yes.

Fukugita: Already around that time Kamiokande people were speaking about the atmospheric neutrino problem.

Kajita: No, in the spring of 1987 we never spoke about it to people outside the Kamiokande group.

Fukugita: I heard about a deficit in 1984 or 85, well before 1987.

Kajita: That must be a deficit of muon decays.

Fukugita: In 1984 or 85, I heard from Prof. Totsuka many times about a deficit of muon decays···, no, muon flux. A muon neutrino produces a muon, and an electron is produced from muon decay. When he was working hard on proton decay, he was already aware of a deficit of that rate.

Murayama: But, the absolute fluxes of atmospheric neutrinos were not very reliable at that time.

Kajita: At that time, we had roughly separated single Cherenkov rings identifying them as being due to muon neutrinos and electron neutrinos. But the problem was that number of electrons from muon decays was significantly less than expected, though the μ/e ratio looked OK.

Fukugita: In 1978, Frederic Reines et al. first reported a muon flux deficit from their experiment in South Africa. They observed only 60% of what they had expected…

Kajita: In their experiment, they used a detector that could count only penetrating muons. They reported the ratio of Monte Carlo prediction/Data was 1.6, but they did not explicitly mention the deficit.

Fukugita: That means 60% of what they had expected. So, the number became known then, but no one believed it would be a reliable flux. Later, IMB also reported a slight deficit earlier than Kamiokande.

Kajita: Actually, the submission date of our paper was a few days behind. Both Kamiokande and IMB reported data showing fewer muon-decay electrons than expected in single-ring events.

Yanagida: When did we suspect the possibility of neutrino oscillations?

Fukugita: Kamiokande's atmospheric neutrino paper[1] published in 1988 describes lots of details, and the possibility of neutrino oscillations is suggested at the end of the paper. But, I believed neutrino oscillations had been discovered when I read their paper[2] published in 1992.

Kajita: In our paper published in 1992, we somehow doubled the amount of data used and reported an allowed

region in the neutrino oscillation parameter space.

Fukugita: At that time, you reported the double ratio, the observed ν_μ/ν_e to the expected Monte Carlo value. It was not 2:1 as expected, but about 1.2:1. That means that the ν_μ flux was about 60% of the expected value. The flux uncertainties were cancelled out because you took the double ratio.

Murayama: But, calorimeter experiments like Frejus did not observe the ν_μ flux deficit at that time.

Fukugita: That's right. Both Nusex and Frejus reported no ν_μ flux deficit.

Murayama: They said something like "Particle identification conducted in a water Cherenkov detector may be wrong," because only water Cherenkov detectors observed the ν_μ flux deficit.

Fukugita: Yes. At that time, Prof. Totsuka said, "If it's wrong, we could lose our credibility." He was very nervous.

Believed the Correctness of Kamiokande's Particle ID Algorithm

Murayama: Kajita-san, did you first devise Kamiokande's particle identification algorithm?

Kajita: Yes, I devised an algorithm which, in the case of multi-ring events, identified particles by calculating electron-type and muon-type probabilities for each Cherenkov ring. When I applied it to the simplest case, namely, single-ring events, I found a significantly smaller number of muons than expected. It was around autumn in 1986. Then, I first realized something strange in atmospheric neutrinos.

Murayama: At that time, few people believed it was possible to reliably distinguish between clear muon single rings and fuzzy electron rings?

Kajita: I believed so, because cosmic-ray muons were identified as muons with more than 98% probability, but …

Murayama: What did the people around you think? I mean, as a community.

Kajita: I have no idea about that. Probably people were confused because soon after our paper, Frejus and Nusex published papers in which they reported no observation of a ν_μ deficit.

Murayama: Do you mean that there were various opinions even inside the Kamiokande group?

Kajita: Yes. Thanks to Professor Koshiba we could publish our paper in 1992, I think.

Murayama: Do you mean that he thought it was very interesting?

Kajita: Rather than that, he advised me, "You must write the next version of the paper, because the current paper remains unsatisfactory."

Fukugita: The paper published in 1992 was very important.

Yanagida: We published our neutrino oscillation paper[3] at a rather earlier time. I think it was because we had close contact with the Kamiokande group. We believed their results. Then, we thought if we could build a model···

Fukugita: We published it in 1993, about a half year after Kamiokande's publication in 1992. What most people did not believe was nearly maximum mixing. They thought "How do you explain it?".

Murayama: It was a prejudice.

Fukugita: Normally, we think the mixing angle (sin θ) is about the square root of the mass ratio. Then it cannot be that large. However, adopting Yanagida-san's seesaw mechanism, you can take the square root once again. The 4th root of a small number is nearly 1.

Yanagida: That is peculiar to the seesaw mechanism. The root of a root is 1.

Murayama: In 1991, I finished graduate school and went to Tohoku University. There, Yanagida-san immediately told me to see if atmospheric neutrino Monte Carlo was really reliable. So, I made a simulation to see if $\nu_\mu : \nu_e$ was really 2:1.

Yanagida: Did I tell you that?

Fukugita: Around that time, it was proposed that the $\nu_\mu : \nu_e$ ratio may shift from 2:1 if you take muon polarization into account. But, calculations showed that it produced only a small effect.

Murayama: I found that the ratio was really 2:1 even if I took every possible effect into account. When did you see the zenith-angle dependence?

Kajita: It was first reported in our paper published in 1994.[4]

Murayama: It took a very long time.

Kajita: Yes, it really took a long time from 1988 when we started. We had accumulated data for 6 years, and finally we wrote a paper. We felt, "It wouldn't help if we waited any longer."

Murayama: Because Kamiokande's fiducial mass was only 1,000 tons, it was difficult. Moreover, looking at the zenith-angle distribution, only the first bin was significantly high, but the other four bins could be reasonably fit to a flat distribution. So, I felt uneasy like, "Is there really zenith-angle dependence?".

Kajita: The double ratio compares the data and Monte Carlo of the μ/e ratio. Then, as the observed number of electron events was small, the double ratio didn't show a clear zenith-angle dependence. Instead, if you look at muons only, you can see an up/down asymmetry with about 99% probability. But, 99% probability is less than 3σ.

Murayama: Was it not possible to see upward-going muons in Kamiokande?

Kajita: We did publish it, but probably later in 1998.

Murayama: Oh, that late.

Kajita: Yes.

Murayama: IMB measured upward-going muons, and from the stop/through ratio they reported an excluded region in the neutrino oscillation parameter space.

Fukugita: It was around 1994.

Murayama: Looking at their paper now, they very clearly excluded···

Kajita: Doubly excluded the perfectly right region.

Murayama: What caused that? Was that due to background?

Kajita: I do not remember precisely, but I think it was caused by something going slightly wrong.

Murayama: In the meantime, construction of Super-Kamiokande started.

Kajita: It started in 1991. It was completed and the data-taking started on April 1, 1996.

Murayama: When did the main force of the IMB group join?

Kajita: I think it was around 1992.

Murayama: Oh, immediately after the construction had started. They joined Super-Kamiokande, because they could not fix the water leak of the IMB tank.

Kajita: Yes, it was due to their reason.

Murayama: Did the Japanese group accept them immediately?

Kajita: Basically, yes.

Obtained Decisive Evidence from Super-Kamiokande

Murayama: Having started data-taking in 1996, you very quickly accumulated the data. Soon the up/down asymmetry of muon neutrinos showed up. Then were you very excited?

Kajita: At that time, I was very glad at heart.

Murayama: Around the end of 1997, Hank Sobel visited Berkeley as a colloquium speaker. He showed data indicating a large up/down asymmetry. So, I said to him, "It looks more than 5σ. Why don't you announce it as a discovery?" He answered, "No, I cannot say my personal opinion, because this is the Collaboration's result" [laughs]. How did it converge to a level that allowed a real announcement to be made? You had been showing the data without concealing anything at all.

Kajita: Super-Kamiokande always presents the data when they are summarized. So, we did not conceal the data at all.

Murayama: What kind of discussion was there in the group before you finally announced the evidence at the International Conference in Takayama?

Kajita: I don't remember, but probably we had been waiting until it was confirmed that we could consistently explain everything such as upward-going muons.

Murayama: Then, you thought that only the zenith-angle dependence of the multi-GeV events was not sufficient. It was not until you obtained other pieces of supporting evidence··· At that conference, I was deeply moved. Originally, the conference had been scheduled to be held in Sudbury, Canada, but because of a delay in SNO's construction the venue was changed to Takayama at short notice. It was the right decision for Japan to have hosted it.

Fukugita: Around that time, neutrino oscillations were obvious to me. So, I wasn't very excited [laughs]. For me, Kamiokande's paper in 1992 was extremely important. I firmly recognized the reality of neutrino oscillations.

Murayama: But, for the first time in 1998 it was shown experimentally with more than 5σ that the Standard Model is not perfect. Then, after you had shown the data in 1998, what reaction to you did the people around or the entire community have? My impression was that everybody believed it soon.

Kajita: I had the impression that it was accepted more than expected.

Murayama: Do you mean you expected quite a bit of objections?

Kajita: I expected some, because I had continuously witnessed those things for 10 years [laughs].

Fukugita: It might have been different if there had not been a prehistory before 1998. Because of that, people thought "Atmospheric neutrinos may possibly oscillate with a large mixing angle." So, by looking at the decisive data presented in 1988 they felt at ease, like "Yes, that's it!"

Murayama: I also felt that it gradually converged. Though the Soudan experiment had a calorimeter detector, that group started to report a low double ratio. At KEK, a water tank was built to check the particle identification capability. It is true that the uneasy factors were gradually removed. But I remember around that time I heard many collider people saying things like "As a water Cherenkov detector is not very precise, it is unlikely that such a kind of thing will be clarified with it." So, I found some PhD thesis on Super-Kamiokande's website and read it thoroughly. This thesis carefully studied various systematic errors. Then, when I was staying at CERN I held a seminar. It was after the Takayama Conference. I mentioned what I understood about atmospheric neutrinos, such as to what extent various factors were reliable. Many experimentalists staying at CERN came to this seminar. They were mostly skeptical at that time.

Fukugita: I think John Bahcall was very skeptical even after the Takayama Conference. He asked me many times, "Is their experiment reliable?".

Future Direction of Neutrino Physics

Murayama: Well, what is future direction of neutrino physics?

Fukugita: Investigations of (neutrinoless) double beta decay.

Murayama: KamLAND-Zen is leading the world now.

Fukugita: The present effective mass limit from KamLAND-Zen is somewhere between 120 and 250 meV, considering rather large uncertainties in nuclear matrix elements. However, in the case of the normal neutrino mass hierarchy, the effective mass is 5 meV···

Murayama: Normal hierarchy is very difficult. The effective mass can be even zero.

Fukugita: The effective mass is zero if cancellation occurs due to opposite CP phases. Even if we exclude this possibility, the present limit is 50 times greater than 5 meV. That means a detector volume that is 2,500 times larger is needed.

Kajita: The factor 2,500 is for the case of no background. Background, if there is any, makes things worse.

Fukugita: Even for the inverted hierarchy, if we assume the effective mass to be 40 to 50 meV, it means an improvement by a factor of five, which in turn means a factor of 25 in volume. It would be difficult to make KamLAND bigger.

Murayama: There is an idea of suspending a balloon in Super-Kamiokande.

Fukugita: Even if you do it, it will be impossible to discover double beta decay during my lifetime if the normal hierarchy is the true one.

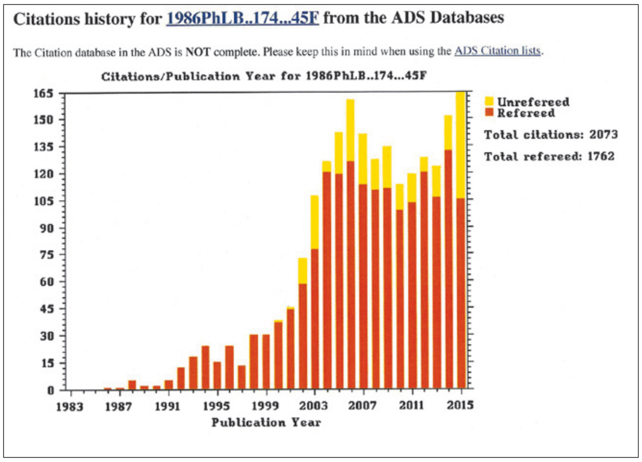
Murayama: Do you think it is possible to verify leptogenesis?

Fukugita: It's difficult. We were motivated by Rubakov et al.'s argument[5] that baryons created by normal baryogenesis will be completely washed out. At first, I suspected their argument was wrong, but accepted it in due course. So, I thought we should try to find a solution.

I have an interesting plot here. Yanagida-san and I wrote this paper[6] in 1986.

Murayama: You published it in 1986. It was well before the discovery of neutrino oscillations.

Fukugita: This plot shows the yearly number of citations of the leptogenesis paper. We published



it here, but at first it was very poorly accepted. In 1998, neutrino oscillations were accepted by the community, and thereafter the number of citations per year has increased. Recently, this paper has been having about 120 or 130 citations every year. These data are taken from ADS (Astrophysics Data System). SPIRES (database of particle physics literature) will give slightly higher citation numbers for this paper.

Murayama: Oh, I get it [laughs]. It's due to greater credibility since the discovery of neutrino oscillations.

Kajita: After the first few years of the 2000s, we started to extensively discuss how to explore CP violation in the neutrino sector. In relation to it, the importance of leptogenesis has been highlighted.

Yanagida: I am very happy with it. It means that theoretical physics has greatly developed thanks to experiments. This sort of thing does not happen so often. If double beta decay is discovered, it will be··· [laughs].

Fukugita: You can readily understand to what extent there is a delay in the increase in the number of citations. At first, we submitted our leptogenesis paper to *Physical Review Letters*, but it was rejected.

Murayama: Oh, was it?!

Fukugita: At that time, Yanagida-san was staying at DESY. So we submitted it to *Physics Letters* and it was accepted soon.

Yanagida: Yes, it was.

Fukugita: So, its publication was delayed by three or four months. As I said, at first it had a very poor reputation. So, a long-range view is really needed.

Yanagida: It's a good example of it. But, this paper would not have been widely noticed if there had not been neutrino oscillation results from experiments at Kamioka. I feel it is very impressive.

Sakharov's three conditions for creation of matter in the universe are baryon number violation, CP violation, and departure from thermal equilibrium. To come up with leptogenesis, we changed only one factor in Sakharov's framework, namely we changed the baryon number to B–L. Then we came up with the prediction that neutrinos have Majorana masses. I think this prediction corresponds to Sakharov's prediction of proton decay. Even if it is not possible to really check leptogenesis, non-zero neutrino mass has been experimentally verified.

Fukugita: Furthermore, it predicts a plausible neutrino mass. It is interesting that the average neutrino mass should be less than about 100 meV. For a heavier neutrino mass, it's not possible to create a baryon number.

Murayama: Yes, the baryon number is washed out.

Yanagida: It's very important.

Fukugita: It is a very important result of Buchmüller et al.'s calculation.[7] This 100 meV is comparable to every limit. The upper limit from double beta decay is 120 meV. Should double beta decay be discovered there, leptogenesis would get into difficulties. Also, the present limit for the sum of three neutrino masses from cosmology is less than 200 meV.

Murayama: Well, it is analysis-dependent.

Fukugita: Analysis is difficult. What I can trust is a limit of less than 600 meV for the sum of three neutrino masses, because it uses only CMB (cosmic microwave background). A limit of 200 meV uses BAO (baryon acoustic oscillations). That aside, 200 meV divided by 3 gives 60 meV. It is the present goal of double beta decay as well as cosmological constraint. So, the next goal is about 50 meV.

Yanagida: In that sense, leptogenesis will be seriously checked from now on, as to whether the upper limit of neutrino mass will be lowered from 100 meV or 50 meV. If neutrino mass turns out to be near these finite values, leptogenesis would be strongly constrained.

Fukugita: For now, everything indicates neutrino mass to be less than this upper limit. It is sort of like searching for a sunken ship in the Pacific Ocean.

Yanagida: We should recognize that. It is very important if the upper limit is really lowered.

Fukugita: Lowering this limit as much as possible is meaningful even if the result is not drastic. Of course, searching for double beta decay is the best way. But, if the normal hierarchy were true, it would be hopeless.

Kajita: How about if the inverted hierarchy is true?

Fukugita: Still difficult. As the target is 40 meV, you have to lower the limit by a factor of 5.

Kajita: My impression is that it may be possible to lower it by a factor of 5. It should be manageable one way or another during our lifetime.

What Will Be the Next Discovery in Neutrino Physics?

Murayama: What will be the next surprise, if there is any, in neutrino physics?

Yanagida, Kajita: Inverted hierarchy.

Murayama: How about sterile neutrinos?

Fukugita: I don't think they exist.

Yanagida: I don't believe in them.

Murayama: Will you be astonished if inverted hierarchy is proved true?

Yanagida: If so, it will be astonishing. In some meeting, probably at DESY, Ed Witten said, "Neutrinos' large mixing angle was a great surprise. We may have another surprise." He stopped without saying anything more. I wonder what the surprise is. Is it inverted hierarchy?

Fukugita: If inverted hierarchy should be true, it would be a surprise. It would be almost impossible to explain it.

Yanagida: It will be possible for me to put forward a far-fetched argument [laughs].

Fukugita: Yanagida-san is great. He can always put forward a far-fetched argument [laughs].

Yanagida: After all, double beta decay is important, isn't it?

Murayama: Double beta decay and cosmology.

Fukugita: In cosmology, the most reliable results are obtained by using only CMB. But, the limit comes from the fact that matter is nonrelativistic at the epoch of recombination. So, using only CMB, we cannot lower the limit too much. This is a difficult point for cosmology.

Murayama: Then, if we emphasize the surprise, our tentative goal should be showing a large neutrino mass and excluding leptogenesis [laughs].

Hoping for Coincident Supernova Observation by KAGRA and Super-K

Murayama: Kajita-san, what is your future plan with neutrino physics? I think you must be very busy with the KAGRA gravitational wave experiment for the moment.

Kajita: As the neutrino community want to see Hyper-Kamiokande achieved, I'd like to support it as much as I can.

Murayama: What is your view on the prospects of KAGRA?

Fukugita: What is the prospect of detecting gravitational waves?

Kajita: I think we have good possibilities.

Murayama: We can expect to conduct astronomy with it. Some supernova explosions may not be optically observed, but can be detected by gravitational waves.

Fukugita: I think supernova explosions would not emit gravitational waves too much. So, should they be detected by gravitational waves, clearly it would be a surprise.

Murayama: For instance, if the gravitational collapse of a massive star forms a black hole, no supernova explosion occurs. Then, it can be observed only by neutrinos and gravitational waves. It cannot be seen by telescopes. Such a scenario may be quite possible. It would be very interesting if KAGRA and Super-Kamiokande detected signals at the same time at Kamioka, but telescopes observe nothing.

Fukugita: If that happens, it's great. It would be much more interesting if signals were detected only in Japan [laughs].

Murayama: The third Nobel Prize from Kamioka may not be a dream.

Fukugita: You have to have another gravitational wave detector somewhere for coincident signal detection. With a gravitational detector, you have so many signals with more than 10σ significance.

Murayama: Coincidence is important.

Kajita: We expect coincidence with LIGO in the U.S. LIGO is already running.

Murayama: Well, time's up now. Kajita-san, good luck in your future. I am really happy with your Nobel Prize. It means a lot to me.

Kajita: Thank you very much.

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