# In Conversation with Kenji Fukaya

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**Biographical Sketch.** Kenji Fukaya is a Japanese mathematician, currently professor at the Simons Center for Geometry and Physics, a research center attached to Stony Brook University, New York. He studied at the University of Tokyo, where he also obtained his PhD in mathematics in 1986 and only a year later, in 1987, he was promoted there to associate professor. He eventually became full professor at Kyoto University, where he stayed from 1994 to 2013. He then moved to the United States, where he became the second permanent faculty member in the history of the Simons Center.

Fukaya started his career working on Riemannian geometry, before moving to the area of Symplectic Geometry. Among other contributions, he focused on the study of Lagrangian submanifolds and on Floer homology theory. For his groundbreaking work, he was awarded several prizes, including the Japanese Mathematical Society's Geometry Prize as early as in 1989, the Japan Academy Prize in 2003 and the Asahi Prize in 2009. Kenji Fukaya visited the Yau Mathematical Sciences Center of Tsinghua University, in Beijing, in February-March 2023.

The first time we met Fukaya in person was at the inaugural meeting of the Tsinghua Open Problems Seminar (TOPS), where he presented easy-to-understand and open problems in geometry to Qiuzhen College students. He said that if someone could solve the problem, then it would definitively become a very nice thesis, and continued to say that he himself has worked on the problem for about 10 years without much progress. A week later he visited BIMSA, so we scheduled the interview at tea time. But then we happened to have lunch together and went on to have coffee. So instead of waiting till tea time, we decided to go for a walk in the park of the hotel resort BIMSA was located in and do the interview along the way. Fukaya is a very easy-going and humble person, who seems to do everything with a smile on his face. Many times during the interview he said that he cannot teach well, or that his talks are not good enough. But in fact his TOPS talks clearly show that he is definitively able to make complicated things look very easy. So easy that undergraduate students don't hesitate to ask questions.

**LH & LS**: *Professor Kenji Fukaya, it's a pleasure to have you here. How is your Chinese experience going? Are you liking China?*

**KF**: I am enjoying it. I am having very good experience so far.

**LS**: *What about food? Is there anything you tasted that you enjoyed?*

**KF**: I came from the US, so every food is good comparatively [laughs].

**LS**: *And about visiting the city? Did you have time for sightseeing? Have you seen anything of interest?*

**KF**: There are many interesting things to visit. I liked especially the Summer Palace and also the park near Tsinghua University.

**LS**: *Do you have any hobbies? Do you practice any sport?*

**KF**: I went skiing just before coming here. I usually ski with my daughter, but probably I am a bit too old to do the same things as my daughter [smiles]. I have had a small injury [points at the leg], but I am recovering now.

**LS**: *Did you go skiing in the US?*

**KF**: I have skied in Japan before traveling here. I have spent four days in Kyoto. I grew up in Yokohama, but my family is now based in Kyoto, where I have spent 15 years. I went skiing with my children for four days in Japan before coming here. And then there was a conference for Nakajima's 60th birthday, but I was there only for one day. It has been a busy month!

**LS**: *How many children do you have? How old are they?*

**KF**: Two, of 26 and 16.

**LH**: *What do they do? Are they mathematicians?*

**KF**: No no, they said they would never become scientists.

**LH**: *[laughs] I said the same thing to my parents when I was 16. It didn't turn out that way.*

**LS**: *We know you are visiting the Yau Mathematical Sciences Center (YMSC). Is there something that attracted your attention? What's the thing that you liked the most, there?*

**KF**: The YMSC is very much a new, modern place. It gives the feeling of a growing environment. I mean, many math departments in the world are shrinking, but here the YMSC is expanding.

**LS**: *Some unexpected pleasant surprise? Or was everything as you figured it?*

**KF**: It is very much how I expected it, actually.

**LS**: *You are Japanese, you were faculty in Japan. Then you received an offer from the Simons Center for Geometry and Physics, you accepted and moved there. This must have been quite a big change for you, because you were at home, in a sense, and decided to embrace a new adventure. Can you tell us what the tiebreaker was that made you decide to accept the offer and move to the US?*

**KF**: The important thing is that in Japan, when you get older, you are supposed to do more of an organizational kind of job. For example, you are requested to lead a group rather than do research yourself, or do other administrative jobs like becoming dean, and those kind of things, I feel I am not very good at them. Then you become a manager rather than being a player, and I want to stay a player. Something that I am sure of is, I would be useless for those managerial duties, or anything other than doing mathematics itself.

**LS**: *Surely there are other mathematicians facing a similar dilemma. They may have had a position and then received an offer from BIMSA and Yau Mathematical Sciences Center and decided to come, or are still in the process of deciding. Do you have any tip or suggestion for them?*

**KF**: Any situation is different. Simons Center is a good place for researchers, but for example for people born in the US it may not be an appealing place to live. There are not so many things to do.

**LH**: *So, are you happy with your decision of moving to US?*

**KF**: Sure, I am.

**LH**: *So, you have never regretted the choice, have you? Never thought "Oh my goodness why did I make that choice?"*

**KF**: The only issue is that my family recently decided to move back to Japan. They stayed with me in the US for about 10 years. My youngest daughter was at the beginning of primary school and now she is grown up, she is finishing middle school and decided to go back to Japan for the age of high school. In Japan high school students cannot live alone, so my wife was supposed to go back with her.

**LH**: *Are you commuting Japan-US?*

**KF**: Unfortunately we're living through a pandemic, so the travel was rather non-trivial. I went only once per year, not much. Before the pandemic I was going to Japan approximately every three months, but then it became almost impossible. At the beginning I was expecting both of my children to live the rest of their lives in the United States, but none of them decided to stay, eventually.

**LH**: *But their English must be great.*

**KF**: Better than mine, definitely! When I wrote a research paper recently, I had a doubt, I didn't know if I was supposed to use 'a' or 'the' in a sentence. So I went to ask to my younger daughter. She doesn't know any mathematics, but she could readily decide. I asked why, and she could not explain. She just felt what was more natural, without knowing why. That kind of things is what the referees always complain about when I submit my papers for peer-review.

**LH**: *I don't know how much you have seen about China, and how acquainted you are with the Chinese academic system. But if you were to compare the Chinese, the Japanese, and the US systems, what would you say are the advantages and disadvantages of each? What can they learn from each other?*

**KF**: China is a safe place to stay. I don't need to be worried about things being stolen, while in the US you have to pay attention all the time. Japan is more similar to China, it's not so dangerous.

**LH**: *... and what about the academic system?*

**KF**: I don't know about the academic system in China. But in the US, the way graduate student's education works, is that you give them papers to read or books to study and then they can ask you questions about that. One big difference with the Japanese system is that in Japan, usually, I asked my graduate students to read books or papers that I wanted to read myself but didn't have time to. That is not possible in the United States, because the graduate students would come and ask you questions, so I need to read the paper carefully before I assign that kind of task. For this reason, my students in the United States are usually doing research on something very close to what I am doing, while in Kyoto I was trying to push toward the opposite. Japan is a small country, so Tokyo and Kyoto are the sources of essentially all Japanese mathematicians. If every advisor follows the idea of educating the students to do the same they are doing, Japanese mathematics would become too narrow. Therefore, there is a general idea that students should be independent. Many of my students were doing research on topics which I am definitely not an expert about. In the US, instead, even the very good students usually do something close to what their advisor is doing, even if, of course, they can divert later. In Japan I didn't have too many chances to educate students on the things I was really working on, and this aspect is easier in the United States.

Another thing is about teaching. I feel that US students are not patient enough. When you are teaching something with a very detailed proof of, say, a theorem, they don't try to follow so much, usually. The same happens during the talks. In the US, the talks are supposed to be more like a propaganda for your own results. You have to explain something very easy to grasp. Japan is similar, but many times people in the audience will want to know some more delicate detail, which is at the heart of the main idea. However, the heart of the idea is often difficult to explain, as it requires to dive into the technical concepts and challenges. In those cases, what you are supposed to do in a talk in the US is to hide all of it, to swipe all those technical but crucial nodes under the rug. In Japan, on the converse, it is sometimes allowed to explain the heart of the ideas. It will usually look difficult to understand, but there will be people trying to understand. There are these people also in the US, but on average the number is much lower.

My impression is that China is somewhat closer to Japan. The other day we had a dinner and mentioned this same thing to some of the postdocs here at the Yau Mathematical Sciences Center and they expressed the opinion that they would very much like to do this kind of heavy seminars. In Japan some seminars can take up to four or five hours. I think that in the US nobody would remain until the end. There are exceptions, of course. Sullivan[1], for example, is organizing these kinds of seminars in the US, but he is the exception and possesses a very strong leadership. In more ordinary circumstances it would be impossible. In Japan one can do these long seminars and some student, maybe, will have strong enough will, insistence, and patience, to remain in this kind of endeavor. When I talked to S.-T. Yau and the group of postdocs here, they said they would like to do it. On the other hand, I must admit that, in the US, the speakers are more trained to make their talk understandable to a wider audience. That is also important, and I fear that I may not be enough well-trained in that direction. In the talks, I speak too directly what's in my mind, and maybe this is not always entertaining. To explain something difficult to everyone is important, but it is not always possible. The exposition should be calibrated depending on the audience. For example, you may have an audience composed, for the main part, of graduate students; if you talk about the most difficult passages of a proof, they will be lost, therefore you would need to be more accessible. I think people in the US is, on average, more trained to those presentations. Then, students of these people are themselves trained to do presentations in that form, whereas students in Japan, for example, are not usually trained in that way.

**LH**: *Do you consider you have personally improved your presentation skills in the US?*

**KF**: I cannot say for sure. In Japan I had to give public lectures sometimes, so I forced myself to train a bit those skills. But concerning lectures, I don't know. At the Simons Center we are not supposed to teach much. It is difficult, because in mathematics you often have to cheat a bit to make the lecture more understandable, and more easily digestible for those who do not master the technical machinery. But cheating is always a problem. Sometimes you may want to say that something is just a technicality and swipe it under the rug, but then after a while you find out that the technicality is actually the most important part of the problem. The method you develop to overcome that technical issue may turn out to be the most far-reaching part of the work. That happens all the time in mathematics.

Another thing I liked of moving to the United States is that I started to do a novel and completely different thing from what I used to. I have organized two special year programs at the Simons Center. They are meant as a tool to push some new direction, and for these types of activities the Simons Center is probably the best place. You can invite many people and promote a topic. That is something that I am very happy to do, when there are people to communicate with. At the Simons Center, many people are visiting every year, and this is nice.

**LH**: *How much of organization do you have to do for these events?*

[1] Dennis Sullivan, Fukaya's colleague at the Simons Center.

**KF**: The Simons Center has very good staff, whose job is to organize these activities. For example, to organize a conference, we just give them a list of speakers we want to invite, and they take care of everything. They are very capable and experienced, as there are conferences or workshops almost every week. Maybe in Japan, at RIMS[2] there are many conferences, but the administrative staff is supposed to work on completely different things. The organization of an event is not at all their job. Thus, the organizing committee has to do most of the job, including calling the hotels to make the reservations.

I believe that at BIMSA they have enough staff to take care of this kind of organization duties. Even if the volume of activities is not at that level right now, I was told that S.-T. Yau wants to host conferences in BIMSA more often. He wants to show that there is a flurry of activity, regardless of the cost.

**LH**: *That's right. Money is not going to be a problem at BIMSA when organizing scientific activities. This would be impossible in any other place.*

**KF**: What remains to do now is to set up more visible activities at BIMSA. Actually, the discussion at the Simons Center now is that there are already too many conferences. They want to decrease the number of conferences and concentrate more effort on long-term visits. I personally don't know which one is the best way. Conferences are good for mathematics, but for a mathematician it is also good to sit down in an office and just think, or discuss with a small number of people. This activity is equally valid, and sometimes even more important than a conference. Organizing those small-group activities is not easy, either.

**LH**: *Right. You need conferences to share the knowledge, but you also need to have time to think about your projects.*

**LH**: *Now about your job. Why and how did you decide to become a researcher? And, had it not happened, or something didn't work out, what was your plan B?*

**KF**: Becoming a researcher was an early decision. The choice for mathematics came around high school, probably. Honestly I don't know what else I could do. [laughs]

**LH**: *You never had a plan B, then?*

**KF**: I am interested in several other things, but there are not many other professions that I feel I can do. Teaching is something that might have worked, but, as I said, teaching to high school students is something completely different.

**LH**: *That's true.*

**KF**: So that is an option I had taken into account, but I am not sure how well I could do. As I said, to make a good and understandable presentation sometimes you have to cheat a bit, and calibrate it to the correct level. Those are the things I am still not very good at. I should have started training much earlier for that. That is a very different kind of skill.

Being a mathematician is different from other scientific fields. Imagine you are an experimental physicist, for example. Then, you spend so much money from the

[2] RIMS is the acronym for the Research Institute for Mathematical Sciences in Kyoto, Japan.

public sector, that you need to have good skills to explain why what you do is important for the taxpayers.

**LH**: *At the university I worked previously, there was the group that was involved in observing the gravitational waves. When you look at their advertisement videos, they are professionally made. They hired a film studio to make their advertisements. And I am talking about research advertisements. This is amazing: of course they got money!*

**KF**: Right. Besides, for these projects you need people, and the leader needs to be able to organize this community of researchers. Several people's job is a very small portion of the whole project. For example, if a group is building a spaceship, somebody will design the toilet for the spaceship. There must be the right motivation for those people, because their job is extremely important. That's another kind of skills than being a mathematician. Usually a mathematician works by himself, or in a very small group, and there is no need to motivate other people that much. Nevertheless, I believe that this cooperation is extremely important in some branches of science. But imagine if mathematicians were to do it, and split the job in very small parts. Probably most of the mathematicians would want to address the most important piece of the job. In the metaphor of the spaceship, everybody would like to design the engine, or to be the pilot; nobody would like to be the designer of the toilet. [laughs]. But every part of the design of the spaceship is important. So, how to organize people? How to motivate them to do those parts of the project? That is part of the skills an experimental physicist needs in their toolkit, for example. And that is the kind of skills I absolutely do not have.

This is a difference between manager and player, which is much greater in mathematics compared to other fields. There are people capable of doing both, of course. Like S.-T. Yau, for example. But they are very few, and it is very nontrivial to be a good researcher in mathematics and also be a good manager. The two are completely different. On the contrary, the definition of a good experimental physicist should include having the managerial abilities, or at least be able to motivate the researchers in your group to dedicate themselves to pursue one direction. That being said, for this reason probably mathematics is most suitable for me.

**LH**: *Then how did you decide your field of research? Why Floer homology?*

**KF**: Floer homology came much later. At the beginning, I have started as a Riemannian geometer. Geometry was a rather easy decision. One reason for becoming a geometer is that I am bit lazy. I am not good at finding a result by heavy calculations. Of course, there are always many calculations, but in some cases you can have a geometric intuition from the beginning, and to justify this geometric intuition you need a lot of lengthy calculations. That I can do. However, there are other people that can undertake long calculations without *a priori* any intuition. When you find curious results, then you start thinking.

I don't have much experience on that kind of approach, during my life. Therefore, I am probably more suited to be a geometer than, say, analyst or algebraist.

**LS**: *I understand. So you can think and visualize a solution, and then try to demonstrate it?*

**KF**: My main activity is just to walk around and think about a problem, and to visualize it, yes. In that sense, I am a lazy guy, I just want to walk around! Instead, to discover something by direct calculation, you cannot walk around. You have to sit down for many hours, and try the calculations.

**LH**: *So, do you always find your inspiration walking?*

**KF**: Not always walking, but never in front of the paper. It always happens outdoors, or in the bedroom, or in the shower. My wife thinks I don't work too much, because most of the times I am doing something that looks completely different. Only when I am doing longer calculations, or computer experiments, then everybody knows that I am working. But many geometers have their own and different ways of solving problems.

**LH**: *That's so true!*

**LH**: *What would be your recommendation for a student who hopes to become a researcher? What would you say to your student, for example? Or would you rather not advise to become a researcher at all?*

**KF**: I would suggest to become a researcher, but it depends on the situation.

Two years ago I visited a school, first started by Hironaka, where very talented students were educated. I met a middle school student who seemed very talented, who asked me more or less these same questions. I replied to him what Serre wrote. Serre first started discouraging his students as much as possible from becoming mathematicians, because the demand for research in math is not so strong. Then, after he discouraged enough, without changing the mind of the student, then he would start encouraging and guiding them.

Maybe many students will be motivated enough. However, I don't think it is a good idea to do it to too many students. It depends. With some students, you have to encourage them and explain them why it is so interesting. That's another difficulty.

**LH**: *Have you ever had the experience of one of your students, perhaps even a talented one, who eventually left academia because of the frustration towards the system? What did you tell to these people?*

**KF**: I don't have such experience too much. I think that, if someone decides to leave academia nowadays, it is very difficult to insist for them to stay. Maybe in China it might be easier, because there are a lot of job openings, but in other countries there is no guarantee of obtaining a permanent position. It has become harder now.

**LH**: *Do you feel somehow pity for one particular person that left?*

**KF**: I had a student that was supposed to begin a PhD degree. However, he eventually decided that he wished to earn more money and he went to work for a bank. It is very difficult to say anything to someone who makes a decision like that. After two or three years he came back to the university, to get a master's degree in probability theory. Afterwards he became a worker in finance. That's not a too bad story.

**LH**: *What type of math questions are you more interested in? How do you choose your research questions?*

**KF**: I have recently had a discussion along similar lines with colleagues working in theoretical physics, namely in string theory. In that area there are trends, and in string theory it is difficult to say what is good and what is not. I mean, in other areas of theoretical physics, what is good and what is bad is decided by the experiments. In string theory, however, there is no experimental check nowadays. Then, until they obtain an experimental confirmation, what is interesting and what is not remains hard to decide. Then, sometimes people are not strong enough, it is hard to be strong enough, to choose their research directions in their own way. So, there are always big trends, and many researchers just follow the trends. Only a few leading people actually decide the trends.

I consider this a problem of string theory, and I think mathematics should not follow this path. In mathematics, there are still trends. At a given point, several people are talking about similar kinds of problems. After some time, that problem may turn out to be not very important. However at that time people working in that direction look very passionate.

That I think is not good. In mathematics, people should always try to be independent. The greatest discoveries do not come from following a trend. Rather, they come from people who stay on their own way. What is good about mathematics is that, in the long run, you can prove that you were correct, because you eventually prove an important theorem. And the value of a theorem is easier to judge.

**LS**: *It probably is more objective.*

**KF**: This is the attitude I think mathematicians should keep, they should have their own way. Of course, it can be difficult, because there are many subjects in mathematics that are interesting. You may spend a lot of time working on something that you consider important, but it may be interesting or important only for you. Thus, at the same time, you have to be strong enough to maintain your will, but reasonable enough to always suspect yourself. To question yourself that something you are working on may not be interesting. That is a big part of being a good mathematician. You always have to struggle with this frustration. To understand that doing something that is your own, but is also interesting for the community. That is, possibly, the most difficult thing.

For example, to find a proof you are interested in, you may have to fight for ten years. In the meanwhile, you can prove intermediate stages, but to other people these results will look as small technical nonsense. You have to keep going, perhaps ten years, before you really find something valuable that other people cannot deny it. Unfortunately, nobody knows if that will happen eventually. It takes time, and for young people, whose job is unstable, that is harder.

The best thing a mathematician can do is to start studying something other people do not feel any interest in, and then after ten years find some interesting result. That is something I would really want to do. In fact, there are researchers that repeatedly do this kind of mathematics. On the other hand, there are many people who just follow the trends.

**LH**: *So you identify yourself as a geometer?*

**KF**: That is what I like to say [smiles]. I would say I am a geometer, yes.

**LH**: *What is the big picture? What is the next problem you are interested in? Or what do you want to learn?*

**KF**: For what concerns geometry, in the last ten or fifteen years, the homological mirror symmetry and the relation with string theory has given a big input. I feel that there is too much going into that direction. So I want to understand more about the relation between algebraic geometry and symplectic geometry itself, without any motivation from physics. Independent of any motivation from physics.

Saying that this relation between algebraic and symplectic geometry is interesting because of string theory is a bad excuse, in my opinion. To have a motivation from a physics problem is not bad, but to use physics as a justification somehow misses the point.

**LH**: *And how much do you understand of physics?*

**KF**: Not so much, honestly. Symplectic geometry is half-way between algebraic geometry and dynamics, in a sense. In algebraic geometry, everything is algebraic, everything can be written down, whereas Hamiltonian dynamics studies chaotic behavior. To understand this bridge is the biggest duty for the practitioners of the field. My sensation is that nowadays the effort is a bit too much on the algebraic geometry side of the bridge.

**LS**: *I would like to follow up on this point. I guess many people, especially from string theory, will probably associate your name with the Fukaya category. Can you tell us a bit about the inception of this idea? How did you start thinking about that problem? How did you come to the solution?*

**KF**: This is about gauge theory. There is a conjecture that relates three-manifolds with boundaries, with this gauge theory on it. In physics, after the work of many people, like Graeme Segal, there is a way to associate this setup with some category. I wanted to understand what this category should be. Then I started studying the space of flat connections and categories.

Afterwards, Kontsevich found that it is related to mirror symmetry, that is how the whole thing started. There is also work of Donaldson, which related a three-manifold with boundary to a category, and that category should be related to Lagrangian submanifolds on the space of flat connections. That's somehow how the idea was developed.

However, after about ten years, I left the gauge theory aside, and fully concentrated on the symplectic geometry. Then, after many years, around 2013, I came back again to gauge theory. Now the motivation is to apply this technology to its original problem.

**LS**: *You have already mentioned the homological mirror symmetry put forward by Kontsevich. What is your view on this program, from someone who stands on the symplectic side of the story?*

**KF**: Somebody told me that, when Kontsevich first introduced it, the homological mirror symmetry sounded like science fiction. It was 1996. Since then, there have been many important developments, so it is by now a kind of established technology. There are still a huge number of cases that still remain to be proved, but now the status of the subject has changed from science fiction to science. Now, something more dangerous or adventurous should come, and we will have to understand it. Something along these lines would be Hamiltonian dynamics, because it links algebraic geometry and symplectic geometry. Algebraic geometry has a long history, is more advanced. Therefore, to take something that we know in algebraic geometry and try to apply it to Hamiltonian dynamics could be the thing that I expect, which is not yet well-developed.

**LS**: *I agree. I have the sensation that, also for what concerns the input from physicists, they typically understand more of the algebraic geometry side. They know how to do explicit computations there. On the other hand, the symplectic geometry side remains somewhat more mysterious.*

**KF**: I think that symplectic geometry gives geometric intuition and motivation. However, for the explicit computations one resorts to the algebraic approach. Physicists, on the other hand, get their primary motivation directly from physics. Therefore, for the point of view of the physicists, symplectic geometry plays an overlapping role with physics. For this reason, what they need is the toolkit of the algebraic geometer, not really the ideas and insights of a symplectic geometer. They already have the motivation and the general picture in mind.

**LS**: *If I understand correctly, you feel that, in the near future, the symplectic geometry side of the homological mirror symmetry will still remain less accessible. Is it right?*

**KF**: That's right. I have to say, it is getting better and better. It is just more difficult. The main difference is that it is more novel. Anything that is known in algebraic geometry should be derived from the beginning in symplectic geometry, and it takes time. But in a sense, more novel means more interesting. I have personally always found the symplectic geometry side more fascinating than the algebraic geometry side. You may want to ask Kontsevich's opinion about it! My complaint is that he is doing a bit too much of the algebraic stuff, but he is the one that can deal with both sides. What Kontsevich says about the symplectic geometry side is usually more interesting than what he says about the algebraic geometry side, at least in my personal opinion. I mean, he will get more credit for important discoveries in symplectic geometry. In algebraic geometry he knows too much already [laughs].

**LS**: *You have been awarded several prizes, and you have made several crucial contributions to mathematics in general. What is, in your opinion, your major contribution to your field?*

**KF**: This one is a difficult question. Early in my career, I have spent ten years working on Riemannian geometry. I am still not so convinced if it was a good choice to move to some other subject. Riemannian geometry is extremely interesting.

In particular, the Gromov–Hausdorff distance, which has tremendously many applications. Not only the work of Perelman, but also that of Donaldson, for example, is very interesting and has a lot of activity and a vast number of applications.

I have done some research on the Gromov–Hausdorff distance at the beginning, but when I was working on that, the subject was not very popular. After I left that area, many people such as Perelman and Holding came and used these techniques, and since then more and more people are using the same techniques. It has now become a major trend among the Riemannian geometers.

**LH**: *Among your works, what is the one you are most proud of? For yourself, not for what others might appreciate most.*

**KF**: Precisely those kinds of contributions. Of course, Gromov discovered this notion, and did very important work on that, but people did not really follow. After about five years, I started to try to push it. I wanted it to become an important tool in Riemannian geometry. Now it is like that, nowadays nobody complains about using the Gromov–Hausdorff technology. Of course, many discoveries happened after I have left that topic.

**LS**: *Are you planning to explore other areas of mathematics? Is there something that you would like to learn which is different from what you have been working on?*

**KF**: Right now I am also interested in gauge theory. These theories are known to be related to D-modules, among other things. When I was young, when I was a student, D-modules and related topics were very active fields of research at University of Tokyo. I would like to work on something related to it.

On the other hand, I am a symplectic geometer but I have not done any important work on Hamiltonian dynamics, or chaotic dynamics. I would be very happy if I could figure out some results on that.

**LH**: *If you were now 20, what would you choose as your research field?*

**KF**: Oh [stops to think]. There are various new fields that are too dangerous, and I probably am too old to start working on them now. We learned that there are complicated systems, such as the biological systems, or even the economical systems, which are very difficult to model, and are not huge enough to be treated as statistical models. There is where I see something.

In biology, only relatively recently the practitioners are more onto the theoretical research. It means that, for a very long time, theoretical biology was not as developed as mathematics or theoretical physics. Then, it developed very quickly and firmly along the twentieth century. Now, they first need some language for their breakthroughs. When I was in Japan, I have discussed with some biologists, and they were interested in developing more the related mathematics. The role of mathematics could be to provide the language that theoretical biology needs. As it seems, however, the mathematics developed by now is not, unfortunately, suitable to describe the developments in that field. I believe that in the next thousand years, the most important mathematics will be the one providing the language for those subjects, like biology.

Right now, it looks like a too adventurous program. It is a long-term journey, and maybe it is a bit too late to start in your sixties. There are a few great mathematicians that studied these types of problems. Gromov is one of them. But probably, if you compare with their past work, this new strand has not been very successful.

To start when you are old is too late. You need to start in your twenties, when your first research job is related to that. You also need a completely different way of thinking. My impression is that Gromov tried to move into biology, and his way of thinking was more suitable for that area than many people. But it is still difficult. The problem is, for example, when you have a set with a large but finite number of objects. How to understand the structures in these large but finite numbers is a very difficult problem. To the date, there is no good theory in mathematics to address these situations. What Gromov mentioned to me once is that, if you start from scratch and try to build up the theory with logic, is very complicated. You will need relative proofs. It seems a natural idea, but I personally don't know how to do it in practice. And Gromov doesn't know either, maybe.

You know, in Riemannian geometry, you start with a manifold and you can calculate distances. The whole idea behind this Gromov–Hausdorff distance is that you can approximate by finitely many points and define a metric between them. You can then go on and compute a lot of things. These kinds of ideas could be related to the language needed for biology, to the problem of imposing a structure on a large but finite set. It is not too strong, you cannot compute everything, but there are still many things you can do. The great question is to understand what to do, and in order to understand what to do you will probably need more of a biologist background. That would likely be impossible for me. At some point I realized that staying closer to physics would be a better idea for me. I still have hope to understand physics, but not biology, it is too far. Younger people, nonetheless, can build their knowledge.

**LS**: *Let me ask you more about your interest in physics. Lynn and I have read an interview dating back to when you were still at Kyoto University. You said once that you were very interested in the role of homological algebra in quantum field theory. Have you followed up on that idea?*

**KF**: I believe it is already a common idea among physicists. The basic aspect of the theory of homological algebra is that you start with this huge algebra system. Then, the definitions of operators and the relations between them can be too complicated, and you need an input from geometric intuition to know what to do with that. In higher algebra and infinite category theory is precisely what happens. To try to write down a definition of infinite category, I think not even Grothendiek could do it. He spent ten years struggling with this idea of infinite category without succeeding. To overcome this obstacle, the intuition should come from geometry. Rather than laying down directly the definition of infinite category, you'd rather search for a geometric idea.

Homological algebra works in a similar way. You want to do geometry over algebra. That has always been my feeling. For instance, when I was writing the proof of a theorem I had on $A_\infty$ algebra, as usual of mine, I was walking around and look for a geometric intuition. I will not sit down and calculate. One can nevertheless figure out the result in that way, and the final formula will still be very complicated, filling a whole page. But it is based on a geometric picture, so that one can understand without paper and pencils.

In physics it goes along the same lines. Symmetry is an extremely important concept in physics, and a system with a symmetry works more or less like this homological algebra, in a sense. Quantum field theory studies systems that typically have symmetry constraints. On the other hand, all the methods of calculations in quantum field theory are based on certain perturbations. When you throw in the perturbation, the symmetry is lost, and you need some weaker notion of symmetry. In this approach, on one side you lose something, while on the other you are gaining something. As it appears, the mathematics of homological algebra seems related to this kind of reasoning, and to the notion of homotopy symmetry. In that respect, the two appear to be related from the very beginning.

Gauge theory is a prototypical example. Gauge symmetry is extremely important conceptually, but if you want to do any computation, you need to fix the gauge. Say, you fix the Coulomb gauge, which of course breaks gauge invariance at once. If you quantize a system in the Coulomb gauge, the way how this process eventually preserves gauge symmetry becomes rather non-trivial. The whole point is that, in order to do quantization, you must first fix the gauge. You then need to understand what is the underlying structure you are dealing with, which is not the usual notion of symmetry, and it is related to this homological algebra and higher algebraic structures.

It is also related to the famous moduli problem. You know, what Grothendiek tried to do with the infinity category was to have a moduli space, which is not necessarily nice. Then, for two spaces, you need more than just isomorphisms; you need to remember the way in which the two are isomorphic. But then you can go higher. Consider two spaces which have two different ways of being isomorphic. You also need to know the relation between the two isomorphisms. And so on, you can go ahead higher and higher. There is where the more geometric facet of the problem emerges, and geometry comes to the rescue.

**LS**: *Now I have a perhaps more conceptual question for you. What is your view on the interplay between physics and mathematics? There is a very active and powerful ongoing dialogue between the two disciplines, going both directions.*

**KF**: Yes, definitely. It started in the seventies, or late sixties, with the surge of the index theorems and their relations with gauge theories. Later, in the eighties and nineties, there has been a huge impact. After 2000s, the relation became a bit more technical and maybe difficult to understand. My impression is that my lack of understanding is my fault, because I worked on a lot papers related to physics in the nineties, but not in the 2000. You know, string theorists were very excited back in the days, but they found out that there were some difficulties that still persist. Since then, they moved closer to actual physics, and the relation between string theory and mathematics is not as preponderant as it was before.

If you ask me, what's the correct way to go, is rather mysterious. I would suggest that we mathematicians do not rely too much on string theory, because there is no way to know whether that theory is related to real-world physics or not. Ultimately there is input and influence from string theory, and that is good for the growth of mathematics. All the mathematics discovered under the impetus of string theory is real, it is an actual discovery. It is proved.

As an example, take supersymmetry. It was never discovered or measured in the reality. However, a lot of highly sophisticated mathematics that appears in these areas of theoretical physics relies on supersymmetry. That is a mystery, if you want.

**LH**: *Now, we are moving to our concluding questions. What is, in your opinion, the most exciting recent discovery, in any branch of mathematics?*

**KF**: Well, I don't know if it is correct or not, but the proof of the ABC conjecture should be a big deal. I cannot say because I have not carefully read the proof, I have just read the news and looked a bit.

**LH**: *Where do you think your field is heading? What is the future of the subject you foresee?*

**KF**: So, as I said earlier, mirror symmetry is a major trend in symplectic geometry. But that is somewhat at a corner point. People understands more and more of it, and it becomes less and less challenging and novel. The important question is what will the next big trend be. Something that I am trying to struggle with is the web of relations of symplectic geometry, with gauge theory, also with chaotic dynamics, or with D-modules, or the relation with manifold with exceptional holonomy. All of them appear to be grand ideas, and all of them are worth becoming the next trending topic. Which of them will turn out to be the most important, is still a mystery for now.

**LH**: *So, to answer the question you think of yourself as a symplectic geometer?*

**KF**: Yes, although as I said, I have tried to work also on other topics.

**LH**: *The last question for you. Your suggestion of an inspiring article or book for people to read.*

**KF**: In mathematics, right?

**LH**: *I was thinking about mathematics, yes.*

**KF**: To which audience should it be targeted?

**LH**: *As you prefer. Let's say graduate students. If you were to choose one book or article to recommend, what would you pick?*

**KF**: Let' see. I would probably recommend Gromov's green book.[3]

**LH**: *Sorry I am not in this field. Is that the h-principle book of which everyone says they don't understand?*

**KF**: No, it is not the book on the $h$-principle, it is another one. Of the the book on the $h$-principle, I have read only the first part very carefully, and I have to say I do understand it. But, as you say, the book on Gromov's $h$-principle is extremely challenging. One of my students tried to read it, because this $h$-principle is very important in symplectic geometry. It is instrumental in applications to dynamics. Gromov's book has not been studied by many people because it is hard, but there might be some hidden gem.

**LH & LS**: *Professor Kenji Fukaya, thank you so much for your time and the nice conversation. We wish you all the best and hope to see you again soon.*

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[3] Gromov's green book is the folklore name for [1].