

IPMU Interview with Makoto Kobayashi

Interviewer: Hiroaki Aihara

An Enjoyable Nobel Prize Award Ceremony

Aihara: Thank you for taking the time to speak to IPMU NEWS. We planned this interview well before the announcement of the 2008 Nobel Prize, but you actually won the Physics Prize before the interview. Since there has been a great deal of media coverage about that already, today I'd like to bring up the subject of research grants such as Grants-in-Aid, as you are a member of the Executive Board of the Japan Society for Promotion of Science (JSPS) and have been appointed as the Director of the JSPS Center for Scientific Systems. I'd

Makoto Kobayashi was awarded the 2008 Nobel Prize in Physics jointly with Hidetoshi Maskawa for "the discovery of the origin of the broken symmetry which predicts the existence of at least three families of quarks in nature," or, for the "Kobayashi-Maskawa theory" of CP violation. He has also received many other distinguished awards, in particular the 1985 Japan Academy Prize and the 2008 Order of Cultural Merit. He received a Doctorate from Nagoya University in 1972 and became a Research Associate at Kyoto University. Since 1979 he has been working at KEK. In December 2008, KEK awarded him with the title of "Honorary Professor Emeritus." He is now serving at JSPS as Executive Director, and from January 2009 also as the Director of the Research Center for Science Systems.

also like to touch upon your research accomplishments, which led to your Nobel Prize. But before going on to that topic, let me ask one question. Did you enjoy the Award Ceremony?

Kobayashi: I had a very tightly scheduled week in Stockholm. But it wasn't too formal, and overall I had good time.

Aihara: As a Nobel Laureate your opinions on science policy will become more and more influential. Could you tell us if you have any particular goals?

Kobayashi: I'm afraid that I'm not sufficiently prepared to answer your question yet. What recently happened to me was all too sudden, but this I can tell you: what I have been saying, whenever asked, is only what I have been usually thinking. I have been talking with many researchers and I feel my thinking isn't very different from theirs. So, I'd like to reflect their opinions.

Aihara: The program of Grants-in-Aid is often brought up in conversations between we researchers, since many of us are benefiting from it. Could you tell us what you

think, albeit preliminarily, about this program?

Kobayashi: Needless to say, Grants-in-Aid are now the largest research grants for basic scientists at universities and inter-university institutions. But we must examine whether or not the program is functioning as originally intended. Grants-in-Aid are taking the role of partially compensating the university base budgets for research, budgets that have been significantly cut in recent years. The fact that this grant is playing too big a role, I think, might reflect a sort of distortion. Let me explain in a more concrete way. A large number of relatively small-scale grants play an important role for many researchers. But people have to spend lots of energy on the application and review processes. So, I think it is better to guarantee a certain level of base budget for scientists at universities. It is important to expand the Grants-in-Aid program itself, but I also think that the preferred direction from a wider perspective is rather different.

Aihara: It is true that the University Operating Grants

that cover basic research in national universities are decreasing. This is national policy. In order to deal with this, we are encouraged to obtain competitive grants in the US style. In the University of Tokyo, as well, we are trying hard on this front under the leadership of the President. So, my question is, are there any discussions about this system inside JSPS, among the Program Directors and Program Officers (a post I once served in)?

Kobayashi: Larger schemes like that are decided on at higher levels than the JSPS. The day-to-day business of JSPS is more to review and select the proposals in a fair and impartial manner under the given scheme. I think we are doing well within this context and producing good results, so we are trusted by researchers. On the other hand, I have to admit that at JSPS we may not adequately discuss the big issues like what the best system for the

Hiroaki Aihara is Deputy Director and a principal investigator at IPMU. He is also a professor of physics at the School of Science, the University of Tokyo.



nation-wide science budget should be.

New breakthroughs appear when there seems nowhere left to turn

Aihara: Previously, it was possible to do research at our own free will, supported by the University Operating Grant etc., without worrying too much about evaluation. But, these days, the University Operating Grant has been reduced considerably. So now we depend rather heavily on Grants-in-Aid. However, there is a screening process for Grant-in-Aid applications. In the proposal, we have to specify a definite outcome within a certain time scale of say five years. Without a definite impact, the proposal most likely won't pass. This puts those working in the most basic fields such as mathematics, particle physics, and astronomy, in a difficult situation because research there doesn't tend to produce a useful outcome in a short time span, and some of them can't guarantee results.

Kobayashi: I know it's difficult to fill in an application form if you work in those fields.

Aihara: We will probably end up not being able to conduct high risk or "odd" research, for want of a better expression.

Kobayashi: That's a very serious problem. We don't know where breakthroughs will come from, and there is no guarantee that they will

come from a popular area which is currently drawing a lot of attention. I think the natural pattern is that something new, something which no one was paying attention to before appears when there is a bottleneck. It would be a serious mistake to nip things like this in the bud.

Aihara: This argument has a close connection with the Kobayashi-Maskawa theory. I'd like to ask if you believed in quarks in those days. Making a theoretical model will end up as just a game unless it reflects reality, won't it? This kind of game-playing, if you'll pardon the expression, used to be more tolerated in the past. But nowadays grossly unrealistic ideas tend to raise eyebrows, although some theoretical work is put up with to a degree. This sort of argument arises because research is supported by Grants-in-Aid, which come from the taxpayers' pockets. What do you think about this in conjunction with your own research accomplishments?

Tradition of the Sakata Group led us to think that quarks were realistic

Kobayashi: I'm not saying that theorists didn't need research grants in those days, but as I didn't apply for a Grant-in-Aid on this particular subject, I can't tie in my work with the problem of research grants. Let me talk about physics, instead. The quark model was taken rather realistically at the time we

wrote that paper. The other day, someone reminded me about this. We were talking about the atmosphere of the theory group at Nagoya University, both I and professor Maskawa being members there, compared with that of groups at other universities. We were thinking the quark model rather realistically, and this atmosphere led us to a relatively clear idea of applying field theory to the quarks. This was to some extent a difference between us and the other groups.

Aihara: Why did you see the quark model as a realistic proposition?

Kobayashi: It was more a way of thinking, a sort of tradition of the Sakata Group originating from the Sakata model.

Aihara: Did all members of the Sakata Group believe in quarks?

Kobayashi: I am not sure "believe" is adequate, but it is true that they were thinking over quarks.

Aihara: Was this before the deep inelastic scattering experiment?

Kobayashi: No, the experiment had already been done, as had the quark-parton model been put forward, but the existence of quarks was not yet widely accepted.

Aihara: I see, it was not until the charm quark was discovered.

Kobayashi: That's right. That discovery changed the atmosphere.

Aihara: People might have

been somewhat skeptical before that.

Kobayashi: At that point we were slightly different from other groups. We were working in an atmosphere that led us to see the quarks as a reality long before the charm discovery.

Aihara: Many people are working on superstring theory today. However, I think experimentalists see superstring theory as only a model because we can't prove it by experiments. But people actually working on this subject might be envisioning strings as realistic entities. Do you think your quarks case was similar?

No promised road in basic research, crucial thing is increasing chances

Kobayashi: Belief or disbelief isn't the point. When people concentrate on one subject, they accumulate knowledge and develop a certain perspective. Were a breakthrough to occur, they naturally will be the ones with a better chance.

Aihara: But we don't know when a breakthrough will appear.

Kobayashi: No, we don't know.

Aihara: That's a problem, isn't it?

Kobayashi: Of course we can only judge from the results afterwards. But the range of possibilities we have gives a depth to science as a whole.

Aihara: This argument should provide a good reason

for allocating more Grants-in-Aid on basic research.

Kobayashi: I agree. The significance of basic research and basic grants is how to add this depth to science.

Aihara: Do you mean how to increase our chances?

Kobayashi: Yes. There is no promised road leading to definite results. What's important is how to keep open as many options as possible.

Aihara: In a way, CP violation was discovered by chance. More than forty years have passed since the discovery of CP violation in K meson. It was great that you received the Nobel Prize by solving the mystery. But the problem might have remained unsolved. Also, we still don't know how to solve the problem of CP violation beyond the quark level.

Kobayashi: Oh, are you talking about the problem of the universe? It won't matter if the problem stays and people continue working on it.

Aihara: By the same token, experimentalists should tolerate superstring theories. We hope very much that you will encourage scientists to spend Grants-in-Aid for broadening possibilities.

Let me ask another question from a different point of view. You served as Director of the Institute of Particle and Nuclear Studies at KEK (High Energy Accelerator Research Organization). Big laboratories like KEK are mission-oriented.

In particular, the scale of high-energy physics experiments like those at LHC are very big, they need big budgets. Here comes the often brought-up problem of balance among various research fields.

What's your opinion on this issue?

Kobayashi: Future high-energy experiments will be on an even larger scale, probably by another order of magnitude, compared with the present ones. So they will present different problems. The problem we are facing now is how to deal with medium-scale projects requesting a few tens to one hundred million dollars. They are not top-down type projects but emerge from scientific necessities in various fields. Here we have very successful examples such as several projects at the National Observatory, KEK's B Factory, and Kamioka underground experiments. They produced excellent outcomes. We have to continue the process of picking up these projects, encouraging researchers and making their projects a reality with government support. I'm afraid this mechanism is somewhat confused and lacks transparency these days. One big problem I think is the new budget system following the corporatization of national universities and laboratories.

Aihara: That's an important point, I agree. We experimentalists often see projects that are reasonably well tested for feasibility,

yet can't be scaled up for improved sensitivity to reach meaningful result. So, there are many seeds for good proposals, but the present Grants-in-Aid hardly allows their realization.

Kobayashi: Some such projects don't fit into the scale of Grants-in-Aid. We need a mechanism for making a proper evaluation and selection of them.

Aihara: In other words, we need a system to deal with it. I guess many of us, not just in physics and life science but in all fields, feel that that system is missing. I hope you will speak up on our behalf on this issue.

Let me move to another subject. Five new research centers were established under the WPI program. They are all oriented to basic research, but they are different from the existing research institutes in that they are encouraged to bring in researchers from the outset. Their top-down funding is mostly for the purpose of hiring people. It is different from Grants-in-Aid. How do you see this program, or what do you hope to see from this program?

Kobayashi: It is a completely new attempt, and I'm expecting a great deal from it. We have free, extremely fast means of communication through the Internet these days, but I think it is somehow meaningful for scientists to get together under the same roof. Bringing people together

contributes to a sort of local accumulation. My view might be old fashioned, but my own experience suggests that it's very meaningful. In that sense, I expect good effects from organizations where bringing people together is emphasized.

Aihara: At IPMU, we have the research areas of mathematics, astronomy and particle physics. I used to think that mathematicians worked independently, but actually I often see that they get together and discuss things, something I noticed for the first time recently. A new IPMU building is under construction in the Kashiwa Campus, and it is our hope that we all mix and work together here. We are hoping this attempt will produce something new after a while. We are determined to develop and extract the maximum potential of this research organization.

Kobayashi: I wish you good luck.

Aihara: Thank you.