Interview with John Ellis

Interviewer: Hitoshi Murayama

"A" Higgs Boson or "the" Higgs Boson?

Murayama: Great to see you three times in a month. It was in Berkeley, the BrunoFest,[1] then the Nobel Symposium,[2] and now here.

Ellis: Yeah. It's great to be here at Kavli IPMU. It's been a really exciting time in particle physics recently. It's great to catch up on a few things. At Berkeley, obviously, the focus was on supersymmetry which is one of our best hopes in physics beyond the Standard Model. At the Nobel Symposium, the focus was on the Higgs boson.

Murayama: Right.

Ellis: We should be a little bit careful – on 'a' Higgs boson.

Murayama: Okay. That's right, according to the official statement...

Ellis: ...they discovered at the LHC.

Murayama: Yeah, so let us actually follow on that. Now that 'a' Higgs boson is discovered, what is the future for the field? What shall we be working on?

Ellis: I think one thing obviously we want to understand is whether this is just 'a' Higgs boson or whether it really is 'the' Higgs boson as predicted in the Standard Model. There're a number of theories that suggested that something, some sort of scalar particle might appear in the LHC but with properties rather different from the Standard Model Higgs boson; some composite models for example.

I think one can say that all those possibilities are ruled out, unless you adjust the parameters of your composite models to be rather similar to the Standard Model. I

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[1] BrunoFest 2013, Celebrating Bruno Zumino's 90th Birthday, Berkeley, May 2-4, 2013

[2] Nobel Symposium on LHC results, Krusenberg Herrgård, Sweden May 13-17, 2013

mentioned supersymmetry earlier. Well, supersymmetry is an example of a theory that says that there would not just be one Higgs boson but actually more than five of them in total and one of them would look very much like the Higgs boson of the Standard Model, but not exactly the same. I think that the differences between some characteristic supersymmetric predictions and the Standard Model are too small to have been seen in the data so far. So, we want to refine the measurements at the LHC, maybe other accelerators, and try to figure out how closely this resembles the Higgs boson of a Standard Model, or whether there's some difference.

Murayama: What are the hopes for supersymmetry being discovered at the LHC, in your mind?

Ellis: I think that some of my friends get a little bit discouraged but it's always coldest just before the dawn. The LHC is currently in a shutdown period. The energy is going to be increased by almost a factor of 2. The collision rate will go up as well. I think there're good prospects for discovering supersymmetry there. There are all sorts of reasons for thinking that supersymmetry might show up in the LHC energy range.

One of them, for example, is dark matter. There are many theories according to which dark matter is some sort of stable particle that was originally in equilibrium with all the regular particles in the universe. In such a theory, you would expect that dark matter particles themselves would weigh less than about a TeV, less than about a 1000 times the proton mass, and hence should be within the range of the LHC.

Supersymmetry is one example. There have been other examples as well. I think that in order to really probe this possibility, we need to go to the maximum LHC energy, and ramp up the collision rate, increase the luminosity, and see what we find.

Murayama: A lot of people are curious about this question. What if the LHC wouldn't find anything other than what looks like the Standard Model of Higgs boson? What are the paths for the field beyond that?

Ellis: I think in that case clearly one would like to build an accelerator which would study the Higgs boson in more detail than it's possible with the LHC. I should hasten to say that I think, in my opinion, it's premature to decide on this because we need to see what the LHC finds at maximum energy. But if indeed it didn't find anything at maximum energy, then clearly I think a lot of priority would go to building an accelerator that could study the Higgs boson in

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more detail than the LHC.

But remembering that the LHC has lots more capabilities for exploring a Higgs boson than have been done so far – as the energy goes up, also the production rate goes up, the collision rate goes up, and there's many other decay modes of the Higgs boson that would come within reach, and the ones that have been seen so far could be measured more accurately– one shouldn't forget that, in some sense, we already have a Higgs factory in the form of the LHC.

Murayama: Some people even argue that maybe the accelerator particle physics is coming towards the end. We have to think of something totally different to probe physics beyond the Standard Model, dark matter and other things. What is your take of this question?

Ellis: I think that that's premature. How many times have physicists said that the end of theoretical physics or experimental physics is within sight?

Murayama: Like even in the 19th century by Lord Kelvin.

Ellis: Right. One or two minor little details could be figured out which, of course, led to quantum mechanics and relativity.

Murayama: Exactly.

Ellis: I am sorry. I don't buy that argument. I actually think, in contrast, this is now a very exciting period because the LHC already, in its initial operation, has revealed a new particle that is completely different from anything else you've ever seen before. It's a boson, but it is the first boson without spin.

Murayama: Right.

Ellis: All the other bosons have a unit of spin. This Higgs boson is certainly unique in that respect, and that way it has all sorts of theoretical issues, theoretical problems. Also, the Higgs boson could be linked to some of the solutions of some of our cosmological problems. So, I think this is opening up an entirely new era in both theoretical and experimental particle physics.

Murayama: So, speaking of the theoretical side, I know that couple of young people may be sort of disappointed these days that there's no signal of new physics yet, and all the progress you really talked about are pointing to more experimental activities trying to measure the properties of this 'a' Higgs boson and something beyond. What are the theorists' roles at this stage in the development of the field?

Ellis: I think that previously people could have had reasonable doubts as to whether such a thing as an elementary scalar particle existed.

Murayama: Right, myself included.

Ellis: Of course when we say elementary, it's always provisional. It's something that looks like that down to some scale. Now, something has been discovered which looks like a point-like scalar particle, at the level that has been probed so far. I think this really focuses attention. So, many of these composite models, one could now put on one side. I think it just focuses much more clearly the question many of us have been worrying about, including you, the problem of naturalness. It also offers perhaps the prospect as I mentioned of the connections with cosmological issues, inflation for example, and it's a very challenging suggestion that actually cosmological inflation might be due to 'the' Higgs boson; I don't think that works.

Murayama: Right, that's very ambitious.

Ellis: It's very ambitious but, nevertheless, you know, one has to be ambitious in order to get anything done. I think that just in probing that outrageous suggestion, I think we may learn a lot more both about scalar boson physics and also about cosmology.

Murayama: You do think that theory has a role to play in here.

Ellis: It's certainly keeping me busy!

Flavor is one of the Big Issues in Particle Physics

Murayama: Oh yeah, that's good. That's very important. Another thing we're getting involved in now actually–based on your recommendation actually that we have to be involved in accelerator-based particle physics. We are actually taking part in Belle II, that's the forthcoming B-factory experiment. What is your view on this direction in accelerator-based particle physics?

Ellis: One of the big issues in particle physics is what we call the problem of flavor. Why it is that there're so many different types of quarks and leptons and why they mix in the ways that they do. The Belle II project is very definitely aimed at trying to understand that better. There's also work being done on that at the LHC. I think that Belle II and what can be done at the LHC complement each other very, very nicely.

There are some experimental measurements that you can make at the LHC that you can't make at Belle II and vice versa. I think it's great that Belle II is going ahead. It's worth mentioning that, perhaps, the second most important discovery at the LHC in the first phase of its operation was actually in the area of flavor physics and that is the rare decay of bottom strange mesons into $\mu^+\mu^-$, which is expected in the Standard Model. Evidence for that has now emerged from the LHC, which is quite conclusive. It's roughly in agreement with the Standard Model. I think that's something that we have to push on to see whether it really does agree at the level of theoretical accuracy that we can make a prediction or whether there's some discrepancy.

Murayama: Right. I was also very impressed by the B_s oscillation study by the LHCb. It's such beautiful data that clearly shows the oscillatory behavior of the B-mesons. That's fantastic.

Ellis: Right. I think that the decay of the B_s meson into $\mu^+\mu^-$ is a success, so far. Well, certainly a success for the experimentalists, but also so far a success for the Standard Model. But, there were a number of other puzzles. There may be matter-antimatter asymmetry in charm meson decays above the level which might be expected in the Standard Model, although both the theory and the experiment are unclear on that point. There're a number of other sort of anomalies in the physics of *b* quarks and charm quarks, which Belle II can address.

Murayama: One of the criticisms that had been in the community is that we have been making actually a huge stride in understanding forces, in symmetries, by going to energy frontier, but on the flavor side we actually have gained relatively little new grounds in our understanding because we still don't know the origin of the patterns of the quark/lepton mass and mixings. Do you think this would lead to a sort of a breakthrough down the line or what do you think is the future in the flavor physics era?

Ellis: I think it's certainly clear that our experimental friends are, in some sense, well ahead of us. First, I don't think we have good ideas about flavor. We got lots of ideas, but there's nothing which is very convincing. I think it's true in the quark sector. I think it's in a way even more true in the neutrino sector because neutrinos seem to mix in a way which is completely different from quarks. Well, you've got ideas about what might be going on but…

Murayama: Which you didn't like.

Ellis: Which I don't like because basically your model is to say there's no model.

Murayama: Right, exactly. Well, I am testing the hypothesis that some very peculiar type of model is really needed to understand the data. So far the data don't show any sign of that. That's how I am using this idea of random matrices.

Ellis: Yeah. I think that we really need more clues. You might have thought we got enough clues by now because lots of flavor mixing parameters have been measured. But, we feel it's apparently too stupid apart from you to come up with.

Murayama: Especially me, I guess…

Ellis: To come up with models for what's going on, I think the way to go is to look to see whether the paradigm for mixing that we have in the moment really works at the next level. I think this is where on the one hand LHCb, the LHC is taking this, and on the other hand Belle II will take this.

Murayama: One sort of a frontier on the side is the lepton flavor violation, which you worked quite extensively on in the past—like $\mu \to e\gamma$, $\mu \to e$ conversion. Do you think that it's also a fruitful direction to pursue?

Ellis: Yeah, well, I think that's in some sense the unexplored frontier in flavor physics. We've seen the flavor mixing amongst the quarks, we've seen flavor mixing amongst neutrinos. In fact, we've seen flavor mixing amongst different types of quarks– those with charge two-thirds and those with charge minus one-third– we've seen mixing and flavor effects everywhere except for the charged leptons.

Murayama: Right.

Ellis: The lepton, the μ, and the τ. It is clear to me that this is something that one has to push to the very limit of what one can. That's a place where Belle II could have something to say because SuperKEKB will produce, you know, enormous numbers of τ leptons, for example.

Murayama: Right.

Ellis: There's also possibilities that at J-PARC in a fixed target experiments of using muons, to offer muon flavor violation. Yeah, those are things that I am excited about.

Connection between Particle Physics and Cosmology

Murayama: Now, you also mentioned the connection between particle physics and cosmology. Clearly, dark matter is the prime topic of discussions in trying to connect these areas. How do these fields go together? Is dark matter the only thing? Is there anything else? What is the future of this intersection?

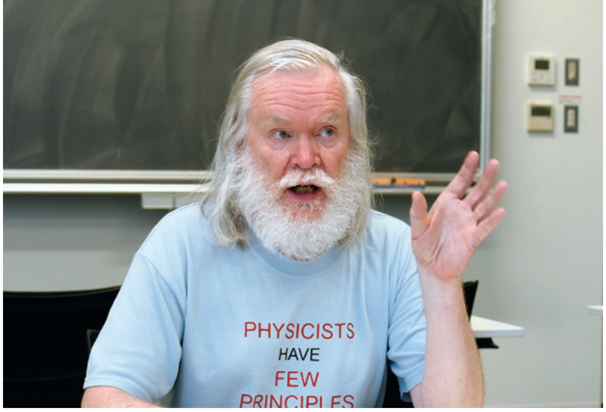
Ellis: I think that to really resolve the dark matter issue is going to involve a collaboration between accelerator physicists and non-accelerator physicists. I can well imagine that non-accelerator experiments might find some sort of signal for some sort of dark matter particle. But depending on its nature, I think that you'll need to study it in the laboratory. That's where the accelerator experiment comes in. Conversely, I can imagine that some accelerator experiment might observe events with an anomalous amount of missing energy.

Murayama: That'll be exciting.

Ellis: That will be exciting. It would be exciting anyway.

Murayama: Right.

Ellis: But if you really wanted to identify that as dark matter, then you'd need to pin down that this is a very long lived particle which is obviously not possible in an accelerator experiment.



Murayama: That's right.

Ellis: You know, 50 nanoseconds and it's gone right and you can't tell whether it lives for a 100 nanoseconds or 10 billion years.

Murayama: Right.

Ellis: You really need to correlate what you see in the accelerator experiment with what you see in the dark matter experiments. In the dark matter experiments, there're different types. One of them is looking directly for scattering in an underground laboratory. Then, there's looking for annihilations of these dark matter particles that are supposed to be flying around the galaxy. Maybe, some will get stuck inside the Sun or the Earth and they annihilate. They form particles that you can perhaps see. There's a whole range of different non-accelerator experiments. As I say, they really have to work in tandem with the accelerator ones.

Murayama: Speaking of that kind of signal, AMS has reported a tantalizing, actually beautiful piece of data, looking at the positron fraction in the cosmic rays. What is your take on this piece of data?

Ellis: I think it's really very difficult to think that it has a dark matter interpretation. You could get a spectrum that has that sort of shape in some dark matter annihilation models. The trouble is that the signal is just very, very big. A signal had been seen by previous experiments. The AMS experiment has done a fantastic measurement which really pins down the shape of the spectrum and tells you that some of those dark matter models are ruled out just on that ground. But, the magnitude is I think very difficult to understand.

If you take the very general arguments about how big the annihilation cross section could be, and if you take the conventional estimate of what the density of these dark matter particles is you cannot reproduce the data. I think the only way to do it would be to postulate that these dark matter particles are clumped, but clumped to an unbelievably big extent.

Murayama: Right, like packs of 100.

Ellis: A pack of 1,000, 10,000... So, I think that's really tough. I think that probably it's due to some sort of astrophysical source. I guess there're two schools of thought on that. One school of thought is that there may be some sort of a nearby astrophysical source that is pumping in additional positrons that are not taken into account in conventional models of cosmic rays. The other possibility is that those conventional models of cosmic rays are somehow inadequate and the number of positrons that you see is due to some interplay between how they diffuse out of the galaxy and how rapidly they lose their energy. It may be that one should tweak those parameters and in that way, one could perhaps get a spectrum that rises in the way that's seen. I think the jury is out on that.

Murayama: One thing that's not clear about this question is when or how we can actually settle these potential interpretations because relatively very little we know about the cosmic ray propagation, the origin of the positrons and pulsars and so on. What exactly can we do?

Ellis: One thing that clearly could be done looking at the positrons is to look for some sort of anisotropy. I think that if you had nearby sources then, at some level, you would expect the arrival directions of these positrons to remember that. As they wander around through the galaxy they forget from where they came from, but there should be some sort of statistical memory. AMS and other experiments so far don't see any sort of anisotropy.

Murayama: That's right.

Ellis: But that's something which should be pushed on. I think that's a potentially interesting signature. Another thing, obviously, is to look for other types of particles that might come from astrophysical sources or dark matter sources. One obvious thing is antiprotons. And in fact many models of the AMS signal–even if you forgot about the fact that the rate is very, very big–predict that there should also be antiprotons, which have not been seen.

Murayama: That's right.

Ellis: AMS will presumably sometime in the relatively near future come out with a new measurement of the antiproton spectrum. That may give us more information. Another possibility which some people have suggested is looking for anti-deuterons in the cosmic rays. There, the signal from dark matter annihilations, if there is one, might be easier to pick up from the background from normal cosmic rays. That's another thing which we'll be looking out for.

Murayama: Obviously, another big major puzzle in cosmology is dark energy. What do you think we might learn by studying dark energy?

Ellis: Difficult to tell. Clearly, there's the density of dark energy itself and then there's a question of how that density might have varied as a function of time. I think that we're getting–and you are getting through the activities here at Kavli IPMU—tools which could measure the dependence of that energy density over a large range of cosmological time, extending from the present day back to when the red shift was similar to one—maybe even more. It could be that the dark energy density is absolutely constant. Then that really would be cosmological constant that will be somehow—well, I was going to say the most boring possibility. From the experimental point of view, it's maybe boring but from a theoretical point of view, it's maybe the most challenging. I mean a number for that...

Murayama: Right, featureless number.

Ellis: How could you explain that? Right. I think many theorists like the idea that this dark energy density is actually varying slowly. Maybe eventually, it's going to relax away to zero and that's a very seductive idea. I think the ongoing experiments should tell us whether that really works or not.

How Can We Make Secure the Future of Kavli IPMU?

Murayama: Excellent. Now switching to gear to why you are here this time, and you come for the External Advisory Committee for Kavli IPMU, and you've actually been on this committee already from the very beginning, and you saw how this institute has got started and evolved and where we are today. What is your impression about this?

Ellis: There's a technical term for that which is gangbuster. I think that Kavli IPMU certainly established a very enviable brand. I think that people recognize that it's indeed a worldwide center of excellence which was your mission that was given to you by the Japanese government.

Murayama: That's right.

Ellis: I think that Kavli IPMU is driving many of the interesting theoretical and experimental developments, and not just here in Japan but also in international collaboration. I think that the Japanese government should be pretty happy. They got quite a lot of bang for their yen.

Murayama: Okay. That's great to hear. Of course, one thing we are still worried about is how we can make secure the future of this institute when the WPI funding may ramp down, possibly, in 4 years from now, maybe with a 5 year extension. What can we do actually to strengthen our position to the government and what should we be working on?

Ellis: What should you be working on? At the purely scientific level, I think that Kavli IPMU has a very good program of being in the most exciting developments in particle theory and also particle experiment, both in accelerators and non-accelerator experiments. In addition to the things

that are already happening, it's also getting involved in other things like we already mentioned, Belle II, improvements to Super-Kamiokande, and the dark energy experiments.

It's not clear to me at the moment what more you could do in terms of the science. But, there's another aspect of our work and of your work; I think that we need to convince governments and other funding authorities that fundamental science is not something that you can dip your toe in for 5 minutes and then go off and do something else.

Murayama: That's right.

Ellis: Fundamental science is something that has payoffs on the timescale of decades.

Murayama: Maybe even centuries.

Ellis: Well, maybe even centuries but I mean if you look at 20th century physics, so quantum mechanics, there is some anecdote out there on the internet that over 30% of the US economy at the beginning of the 21st century was based on 20th century physics.

Murayama: Interesting.

Ellis: I don't know whether it's true or not and I am sure you can get a big fight about it, but it is certainly true that all of electronics, lasers, etcetera, etcetera, you could make a big incredibly long list depends on 20th century physics and, in particular, quantum mechanics. Even relativity is now used in satellite navigational systems, GPS, and so on. Antimatter, a very abstruse discovery, postulated by theorists in the late 1920s, discovered in cosmic rays, and now is used routinely in medical diagnosis. Thousands of people every year have PET scans.

I think these examples show that advances in fundamental physics do benefit society in general and the economy but on a timescale of decades. I think it's the role of government to support that. I think it's unreasonable to expect an industrial company to do so. I think the Japanese industrial companies are maybe further sighted than European or American industrial companies. But, you can't expect them to be looking much further than a decade in terms of their R&D program.

You have to look to governments to take the longer view. The fact that the payoff takes a longer time also means that the research programs are not things that are done in 5 minutes or 5 months or 5 years. Right. They take longer.

Murayama: Right.

Ellis: I mean we're talking a lot about the LHC. The LHC was first conceived in 1984, and it's probably going to go on for at least one, maybe two more decades. All these other projects that we're mentioning, these also have long time scales. I think that the Japanese government like any other government has to come up with a mechanism for funding long-term forefront research in fundamental science. Physics is not unique. We're talking about physics but it's not the only example. Govenment has to understand that it's not something that you can leave. It's not like a quickie divorce. It's something that you're in for the long run.

Engaging with the Public Is an Essential Part of a Scientist's Work

Murayama: So, we have to communicate these benefits of basic research and, as you say, not just in physics, but in all areas as a long term benefit to the society and human kind which needs to be supported by the government. How effectively can we communicate these points to higher levels, politicians, governments, and the general public? How do we do that?

Ellis: I think one thing is the message, and we just discussed what I think the message is. But then I think that we have to get out the message. I think it's an absolutely essential part of a physicist's or a scientist's work to engage with the public. We can't just all sit in our ivory tower and expect yen to rain down. We have to explain what it is that we're doing, and we have to learn to explain it in terms that the person in the street can understand.

Sometimes it happens. I think that in Europe and at CERN, we've been very fortunate that the person in the street has noticed about the Higgs boson. I think we've got the ear of governments. They are basically sympathetic. But, I am sure that in Japan that's the case to some extent also.

Murayama: Yeah, absolutely.

Ellis: I know that you, personally, have been very much engaged in that. I think we just have to, perhaps, also convince our colleagues that they have to – some of them, maybe work harder on this.

Murayama: That's true. You spend a lot of time communicating the importance of science as sort of scientific ambassador to the general public. I'm sure that has a huge impact. Somebody told me that enrollment for young students in science, in mathematics in Europe, overall have been improving like 20% or so, partly thanks to this impact of CERN, showing up in media and being very visible.

Ellis: I think that there probably is an LHC effect. In fact, it was interesting that a few years ago, the UK science minister came to CERN, and he said that he thought that LHC startup could have an impact similar to the Apollo moon landings.

Murayama: Wow!

Ellis: Then I thought this is bullshit. But, now I think he was right. I think history has proven him to be more or less correct. I mean, I certainly know that in the UK enrollment in physics generally has gone up.

Murayama: Wonderful.

Ellis: The quality of the students has gone up. In fact, I was just talking with one of my colleagues at King's College London. He was marking exam scripts earlier on this week. I walked into his office. He said, "These kids are too good." Not only are their numbers but also that the quality is going up. Certainly at King's, we have decided to increase the threshold for students to come in to study physics.

Murayama: Good. Sounds like that your advice to us is basically the same, namely, "Do great science and get the word out."

Ellis: Yeah, but I think that you and I have to, like I said, convince our colleagues to participate in this.

Murayama: That's tricky actually.

Ellis: Yeah, you can't just rely on one or two "ambassadors" to do the job. I think it's particularly important that the young people get involved. The young people obviously have a lot of credibility with other young people. Well, after all it's people that we're trying to influence. I mean one of the things that–one of the deliverables, I think, that we have is an increased interest of young people in science, technology, engineering, and mathematics. Are you talking to a bunch of young people? That's one thing. But, I got a lot of gray hair. You're beginning to have some grays...

Murayama: Yes, I do.

Ellis: It'd be good if we could find some young people who don't have so much grey hair, who also are naturally very energetic to join in this effort.

Murayama: Right. That's something we should be working on. Great. Any sort of last message you would like to give to us?

Ellis: I don't think so. I think that Kavli IPMU is doing a great job. I think that you should have confidence in what you're doing. You, personally, certainly have confidence in what you're doing. I think that let's go out and communicate our excitement to the rest of the society.

Murayama: Okay, let's do that. Thank you, John.

Ellis: My pleasure.