IPMU Interview

with David Eisenbud

Interviewer: Toshitake Kohno

Kohno: Professor Eisenbud, it is a great pleasure for me to have this opportunity to talk with you about the Institute for the Physics and Mathematics of the Universe.

As you might have seen on our web page, one of the main issues for us at our institute is to create new research fields that go beyond traditional boundaries between disciplines; especially between mathematics and physics. It is therefore essential that mathematicians and physicists get together for discussion and work together. Could you tell us your viewpoints on the collaborations of mathematicians with physicists, astronomers, and scientists in other areas? What roles can mathematicians play in collaboration?

David Eisenbud is a professor of mathematics at the University of California, Berkeley, and was Director of the Mathematical Sciences Research Institute (MSRI) from 1997 to 2007. He was President of the American Mathematical Society (AMS) from 2003 to 2005. Eisenbud's research interests include commutative algebra, algebraic geometry, topology, and computational methods in these fields. He established the AMS Leonard Eisenbud Prize for Mathematics and Physics in 2006 in memory of his father, Leonard Eisenbud (1913-2004), an eminent mathematical physicist. The first prize was awarded to Hirosi Ooguri, Andrew Strominger, and Cumrun Vafa in January 2008.

Mathematics and physics provide each other with sustenance

Eisenbud: It's an interesting question. It's not so easy to organize collaborations, as I'm sure you know. The history is very interesting in mathematics and physics, and in the other sciences, too. Very many of the great problems of mathematics have come from applications. Mathematics is deeply enriched by its contact with the applications. Many other very important ideas in mathematics come from pure imagination. Somehow, they're just thought up by mathematicians because they are curious about mathematical things. The surprising thing, I think, is that both of these turn out to be equally applicable afterwards. So while there are these two very different sources, the way they look in applications is the same.

Riemannian geometry was in some way an applied and in some way a pure interest of Gauss and Riemann, and became the basis for relativity. The noncommutative algebra of infinite dimensional matrices became somehow the basis of quantum mechanics. These were completely unanticipated developments. I think that is a pattern which will continue in the future. The best guide we have to the future in this regard is the past. One can learn some lessons from this.

One is that it is very important for mathematics to be exposed to and interact with experimental and theoretical science. I think this kind of exposure is a wonderful thing the new Institute can bring about. That's where some of the problems that enrich mathematics will emerge. It's also very important to maintain the strongest possible purely disciplinary capability so that it can feed the interdisciplinary capability. You cannot have interdisciplinary science if you don't have disciplinary science. And I think that this university has such a strong tradition in mathematics, that it is well placed for that.

The current development of mathematics and physics is really very striking, because I think theoretical physics and mathematics are closer together today than they have been for 100 years. The development of string theory and the very intense work in quantum mechanics in our day is highly dependent on the tools that the mathematicians develop. Physicists are extraordinary and voracious consumers of these ideas. As soon as they hear anything, they quickly apply it, and it becomes high fashion in physics and very exciting.

In return, the mathematicians get problems which they cannot solve because physicists are liable to do things with their mathematics that mathematicians would never dream of doing. And the physicists are much better than we are in computing things. They make computations, and if the computations are successful they know that what they did must be fundamentally correct. In many cases this is enough for them, whereas for us it's not enough and we need to go on and develop the mathematics behind this. So I think it's a very fruitful time of interaction there.

How to decipher mountains of data

That's of course only one side of the interaction with physics today. There's another side to it, and the situation is common to the other sciences, as well. Biology has led the way here, but it's very much across the board. That is, we are now capable, with electronic instruments and computers, of producing much more data than we can handle.

Toshitake Kohno is a professor of mathematics at the University of Tokyo, and he is a principal investigator of IPMU.

I remember the physicist Robert H. Dicke coming to speak once at a colloquium many, many years ago. He talked about measurements of the oblateness of the sun. The sun is not perfectly round, and exactly how much it fails to be round is important if you think about the verifications of general relativity by the bending of light around the sun. So he was very interested in this question of the oblateness of the sun. He collected data—at that time, in those early days, it was still only a tiny, tiny fraction of what we do today—but the data sat in his laboratory. Each day there would be a pile of printouts. Who could read all these things?

This is widely recognized as one of the fundamental problems of experimental science today: our ability to produce interesting data is far greater than our ability to absorb and understand it. The mathematicians are the only ones, I think, who have the tools which will begin to be effective in this way, mainly through statistics and combinatorics and computer science. One sees this very much in the study of the genome and the matching algorithms we have developed there. This bleeds over into computer science. By the way, I think one should regard computer science, mathematics, and statistics one subject for this purpose. These are very important trends.

Chance favors only the prepared institute

Kohno: Research in statistics and experimental physics is also an important aspect of our institute.

You mentioned interdisciplinary research. Let's talk about this. As a former director of MSRI, you have organized a number of activities in various fields of mathematics. I was very impressed by a program of MSRI in the mid-eighties. Two very different programs, one in topology and the other in operator algebras, started in parallel and then were unified to create a new field of mathematics, the discovery of the Jones polynomial. In your opinion, what makes interdisciplinary research possible at the Institute level?

Eisenbud: I have to say luck plays a big role. Since you can't control luck, you have to control the other things around luck. The great biologist Louis Pasteur said: "Chance favors the prepared mind." In the same sense, chance favors the prepared institute.

One thing that MSRI does regularly is to bring programs together which are in some way related, in the hope that this will make interaction more likely. Of course if you have very smart people working on related things and one group has the chance to learn from the other, then this makes the luck possible. One way to inhibit interaction is only to have very technical, specialized discussions. Then one of the groups will have no chance of getting into the ideas of the other group. I think it is very important to have an organized forum in which each group is supposed to talk to the other group. It's quite hard for them to do, so unless you push hard they don't do it. But if they do it, then they are quite pleased afterwards, I think. So it's worthwhile.

There are various ways that one can do social engineering. Of course it's important simply that they meet each other, talk to each other, and know each other's names. This is difficult enough. I think it's also important to have series of elementary lectures by one group for the other group to make it possible for people to learn things they didn't know about already. Then there are new ideas, and it's exciting, and people talk to each other. "Oh yes! I have a tool that might fit your problem." And this then goes forward.

Approaches to string theory

Kohno: Recently there have been very close relationships, again, between geometry and physics; for example, mirror symmetry. Could you tell us about your prospect for future developments in synergy between mathematics and physics?

Eisenbud: I think the possibilities are very bright. In some ways, the mathematicians are still working on physics circa 1950. We have really understood the mathematics of the kind of quantum mechanics that people were doing before 1950, but the kind of quantum mechanics that was done in the second half of the 20th century is still very hard for mathematicians to understand, and I think for physicists to understand, too.

The most accurate predictions in all of physics, in some way, are those in quantum electrodynamics. They are made by summing the first few terms of a series which is known to be divergent. This is not a happy situation. Despite lots of work I think this remains a difficult problem. People in physics integrate over nonexistent spaces all the time perfectly happily, and I think the absorption and understanding of that material will be very important for mathematics, and ultimately for the progress of physics, as well.

On the other hand, the problems of string theory involve the deepest parts of mathematics. I think physicists have made very good use of many surprising tools and results from mathematics, and have often led the way. There's a great interchange between the two fields. I think this is a very happy time for mathematics and physics in that regard.

Kohno: Thank you very much for your valuable comments.