IPMU Interview
with David J. Gross

Interviewer: Hirosi Ooguri

Elementary particle physics was a goldmine in the 1960's but theorists were powerless

Ooguri: You have established the current paradigm of elementary particle physics by discovering asymptotic freedom, for which you were awarded the Nobel Prize, and you have also made great contributions to more prospective areas of particle physics such as string theory. In addition to your scientific achievements, over the past 10 years or so as the Director of KITP, you have transformed this place into the center of theoretical physics in the world. So, we have many things to learn from you, especially as we try to establish this new institute in Japan. It is an honor to talk to you today.
I would like to start out by asking when you became interested in science in general, in particular in such an esoteric subject as elementary particle physics. When did you decide you want to be a physicist?

Gross: Long before I knew what it really meant. I decided I wanted to be a theoretical physicist roughly at the age of 13 or 14.

Ooguri: That is pretty early. Not many people at that age know such a subject even exists.

Gross: I did not really know what it meant to be a theoretical physicist, but I was inspired mostly by reading popular science books, such as the ones by George Gamov. What excited me was that you could figure out how the real world works and solve the puzzles of the universe just using your mind. That seemed so exciting that I decided to become a theorist and try to calculate the properties of the world. I was very lucky since so many people are unsure exactly what they want to do until later in life.

Ooguri: Then, you went to Berkeley as a graduate student, which at that time was "the place" in particle physics.

Gross: There were some great theorists in Berkeley at that time, and it was certainly the

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center of the experimental particle physics. At that time, elementary particle physics was a true goldmine, a host of new particles were being discovered every month and it was not hard to discover new particles and new phenomena. It was a very exciting time experimentally, and experimentalists were the masters of the field. The theorists were pretty powerless.

Ooguri: But your enthusiasm towards theoretical physics was not diminished.

Gross: No, because there were so many problems. It was clear that almost everything was not understood, and the little understanding one had seemed ad hoc and paradoxical. It was exciting that constantly new things were discovered that changed the way people looked at elementary particle physics.

Ooguri: After graduating from Berkeley, you went to Harvard and then to Princeton, where you had great success with graduate students also.

Gross: Well, it is easy to have great success with graduate students when you are at a place which has many great students. Frank Wilczek (Nobel Prize winner) was my first graduate student, and I think Ed Witten (Fields medalist) must have been my third or fourth. I thought that was the norm. One of the interesting things about science and mathematics is that we still have a very old-fashioned way of teaching. We teach our students the same way a master artist will teach an apprentice by bringing him into his workshop and having him participate in his creation of works of art. Not all students are able to engage in research immediately, but the best students from a place like Princeton or other great research universities are certainly able to start doing science from an early stage.

Ooguri: When you moved from Berkeley to the East Coast, you also changed the direction of your research.

Gross: Berkeley was dominated by my advisor, Geoffrey Chew, who had this idea of the bootstrap – a theory without a theory. This was a very anti- "field theory" approach, which said that fields cannot be measured, are unphysical, and one should not construct a dynamical theory in terms of unobservable fields. Rather one should only postulate the general principles that constrain the S-matrix, which was observable. The hypothesis was that there was only one unique S-Matrix consistent with these general principles. I got quite tired of this approach even before I left Berkeley because you could not do very much with it. Moving to the East coast was good because there field theory was still tolerated.

Ooguri: But, even in the East, field theory was not yet the mainstream.

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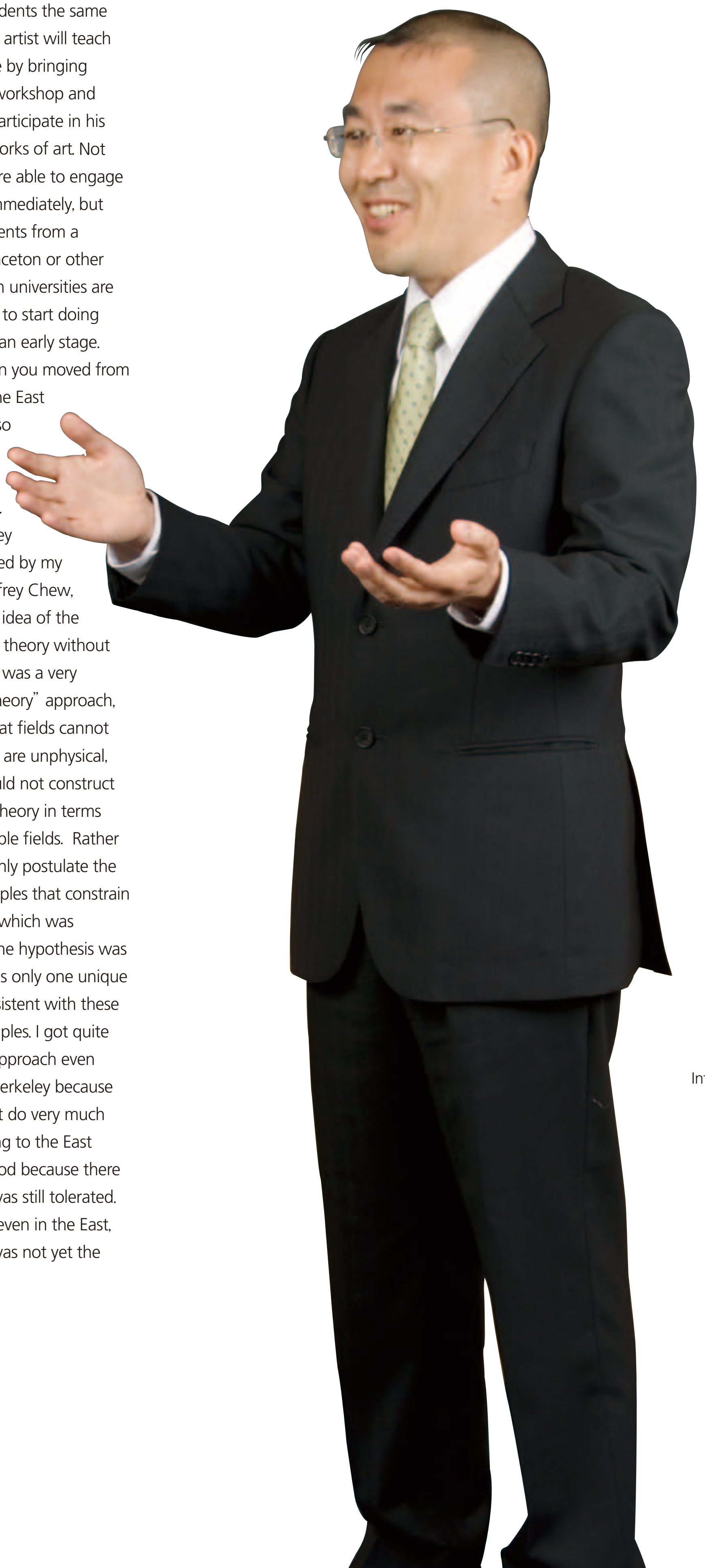

Planned to prove field theory useless, but discovered a theory that works

Gross: It was certainly not the mainstream, largely because of its impotence. It is essential for physicists to be able to calculate, to probe the limits of their theories and to make predictions that could falsify or confirm their ideas. Field theory at that time was quite insufficient for the strong interactions, since only perturbative techniques – Feynman diagrams – were available for calculation.

Ooguri: Then you discovered asymptotic freedom and changed the people's perception about the usefulness of quantum field theory as the language of elementary particle physics.

Gross: The phenomenon of asymptotic freedom was the answer to the search for a theory that could explain why the strong interactions seemed to behave as if they were free at short distances. It led to QCD, the theory of the strong force. But more generally, having a theory that was totally well behaved and under control in the ultraviolet gave enormous calculation ability and resolved a lot of the lingering doubts about quantum field due to its ultraviolet singularities. People's attitude totally changed, including my attitude, because I had been convinced that field theory was not going to provide the answer for a theory of the strong interactions. In fact, my original research plan was to prove that there was no quantum field theory that would describe asymptotic scaling.

Ooguri: So, you set out to prove that field theory was useless and then instead you discovered a theory that works.

Gross: There were really three parts of the program. The first was to show that you needed asymptotic freedom to get the observed scaling. The next thing was to prove that there were no asymptotically free field theories, which with Coleman I did, with the exception of non-Abelian gauge theories. The last part of the program, which I did with Frank Wilczek, was to look at non-Abelian gauge theories, which much to my surprise turned out to be asymptotically free. It was almost one, two, three, QCD. There was no choice. If you wanted to explain the scaling, you had to have a non-Abelian gauge theory.

Ooguri: After that, most of the community moved to quantum field theory.

Gross: Well, because you could calculate and furthermore, even better, the calculations worked. And then there were some spectacular experimental confirmations over the years. But for me the major problem was not so much continuing with calculating tests of asymptotic freedom or QCD, but rather understanding confinement, which turned out to be a lot harder.

Ooguri: Fast-forwarding to the mid 80's, you went back to string theory again but in a different context.

Gross: In 1968 when string theory was born, that was a period where I was just beginning to think about strong interactions and short distances and deep inelastic scattering. At that time I was convinced that what one really needed for to describe the strong interaction was something totally revolutionary. String theory was directly along that line of thought.

I got involved in string theory quite early, but I also realized that this was not going to explain hadrons. I was focused on trying to understand what was going on in short distances inside the proton. One of the nicest features of string theory was the soft interactions, but those gave rise to very strong falloff at large momentum, which was very different than what was seen in the experiment – exponential as opposed to power falloff. So, string theory was not a good place to try to understand the simple scaling behavior at short distances, and I stopped doing string theory at that point. But I continued to follow it even through its darkest days. It was always fascinating.

In 1983, I went off for a sabbatical to Paris and decided that this was a good time to get back and learn more about string theory.

Ooguri: Did you smell anything when you decided in 1983 that you want to get back and study string theory? That sounds like such a good timing, just a year before the superstring revolution.

I was ready when the superstring revolution happened in 1984

Gross: Well, to some extent, but not totally. Remember that during those years there was a lot of development of supersymmetric theory, which came out of string theory originally. Everyone in the field was interested in supersymmetric theories by that time. At Princeton, Ed Witten and I always had a continued interest in string theory. John Schwartz used to visit Princeton once or twice a year because his mother lived there, and he would always come and tell us what was happening or give a seminar.

So I was ready when the superstring revolution happened a year later, when the Green-Schwarz anomaly cancellation was discovered. Suddenly, a lot of interest was generated.

Ooguri: That led to your construction of the heterotic string theory.

Gross: Well, it was the unexpected answer to an obvious question – how to realize E8 x E8. And the answer was not so hard, once one realizes that one could treat right and left moving waves on the string differently. It was a very exciting time because suddenly all of these beautiful ideas fed together and you could have a reasonable unified gauge group and chiral matter.

Ooguri: I remember I was a graduate student when this happened. One of the things that impressed me was the fact that the families of elementary particles emerge very naturally from the geometric structure of a Calabi-Yau manifold.

Gross: Most of the previous explanations for the number of families, or for the hierarchies of Yukawa couplings, or for the chirality of fermionic matter, all of these were ad hoc or based on rather uninteresting symmetries. For the explanation to emerge from geometry was incredibly beautiful.

Ooguri: Since then, string theory not only made progress toward the unification of elementary particles, but its connections with many other areas of physics have also emerged. For example, right now at KITP, you are having the workshop connecting string theory to condensed matter physics. Condensed matter physicists attending the workshop seem very excited about this development.

Gross: String theory has turned out to be intimately related to quantum field theory, which is the language not just of particle physics but also of quantum many body theory, which is of great interest to condensed matter theorists. String theory is rich and big and contains a lot within it.

One of the great developments in the last few years has been the realization of that old dream of understanding the strong interactions in terms of string theory. So, the circle closes. But still my ultimate goal is the unification of the forces, namely the ultimate particle physics goal as well as the newer cosmology goal.

Ooguri: Some of the future experiments in cosmology and astrophysics may have direct relevance to the unification goal. For example, measurements of the polarization of CMB or gravitational waves from the inflation era could teach us about Planck scale physics.

Right problem and right people needed for successful interdisciplinary collaborations

Gross: We have so few handles on Planck scale physics that any conceivable way of learning about what happened at very short times or length scales must be followed up. The dreams of astrophysicists and cosmologists and of experimental particle physicists are really heroic. What I am impressed is just the unbelievable difficulty of doing these experiments or observations. We need such heroic efforts since it really is essential for our field to get some clues, not just from the beauty and power of our mathematical descriptions that we are perfecting, but also from nature itself. Your institute includes all of these attempts – experimental as well as theoretical.

Ooguri: Yes, it is inspiring to meet and talk to people who are working on experimental projects such as detection of dark matter in the universe. It gives us a focus on what theorists should be doing. Sometime, a new connection can arise. For example, we are planning a focus week where statisticians and astrophysicists will come together to try to develop new statistical methods. That can point toward a new interdisciplinary way for mathematicians and experimenters to collaborate.

Gross: I think one of the most important roles that an institute like the KITP or IPMU can play is to bring in together scientists from different communities to collaborate with each other on common problems. That is one thing that universities do not do very well. They create departments which are closed and largely ignore other departments. Some of that is for good reasons, because people have to concentrate and focus in order to push science forward. But institutes like ours have the ability and the responsibility to try to encourage interdisciplinary efforts.

Ooguri: KITP, especially under your leadership, became a role model of interdisciplinary collaborations. What do you think you did particularly right?

Gross: There are a few important points. One is to identify the right problem. You cannot force people to get interested in other people's problems. They have to be interested in the problem themselves.

You also need the right people. You need the willingness to explain to other scientists in ways that they can understand. All of that is possible if you have the right questions and the right atmosphere because in the end scientists are focused on the question, on the science. It is necessary to establish an atmosphere in which people are willing to ask stupid questions, explain obvious things, take the time and approach science as an exciting adventure together.

Ooguri: These are great lessons for us at IPMU.

Gross: I think you have started off very well. During the summer, I met some of your young postdocs, and I heard very good things about how your institute is going. That is the best review you can get. It is an excellent effort.

Ooguri: It is time for my last question. Do you have any message to our friends in Japan?

Gross: I am delighted with this new institute and everything I have seen so far. I am impressed with the fact that you have brought all these groups together, that range from mathematics to experimental cosmology. This is a remarkable collection of entities and remarkable collection of people involved.

I think it is really a very wise move of the Japanese Government to support this high quality science at the level of support that is really necessary to make something happen. The enthusiasm that you all have translates into a lot of work and effort. I have a feeling it will pay off, and it will contribute greatly to science. So, you are off to a great start.