Interview

with Gerard 't Hooft

Interviewer: Shigeki Sugimoto

Physics Had Been Primary Interest since Very Early Age

Sugimoto: Thank you very much for letting me have this opportunity to talk to you today. There are a lot of things that I wanted to ask you.

't Hooft: Yes.

Sugimoto: First, I would like to ask you when and how you got interested in science.

't Hooft: I think that I was somewhat exceptional. When I was still very young, maybe even at nursery school, I really knew that I was going to be interested in the world of nature. I had much more difficulty in understanding humans than nature. I also found adding and subtracting much easier than learning to read and write. In Europe, in general, when you're 6 years old you learn how to read and write at least the basic stuff. Before that, I could not read or write but I could add and subtract, and I knew that I was interested in that.

Sugimoto: In mathematics?

't Hooft: In mathematics and physics. Physics was running in my family to some extent. My uncle was a theoretical physicist. He was well known in his field. My grand uncle was Frits Zernike, who also had a Nobel Prize in physics. I was very much inspired by that and from an early age on I knew I was interested in physics.

Sugimoto: I see. When did you decide to be a physicist?

't Hooft: Well, I don't know when I really decided to be a physical scientist but physics had been always my primary interest. Maybe 9, 10, or 11 years old, I knew that I was going to be a physicist.

Sugimoto: Then, you became a Ph.D. student of Veltman and soon after that you started to work on the renormalizability of Yang-Mills theories. Is that right?

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Started to Work on the Renormalizability of Yang-Mills Theories

't Hooft: Yes. Veltman was working on the problem of how to renormalize Yang-Mills theories and he had developed some very good, sound techniques which fascinated me. But he said, "This is very difficult" and "It may be better for you to work on something else." But my reaction was, "Well, I like the problem that you are working on very much. I want to understand more of that." From the beginning I said "I understand your difficulty, so I want to see what I can do about it."

Sugimoto: I heard that many people were skeptical about gauge theory at that time.

't Hooft: At that time, yes. It is a bit difficult to say how the history developed because now people are very much tempted to say that there was such a thing as the electroweak theory and the only question was how to renormalize it. But that is not how they looked at their problems at that time. I mean the majority of physicists did not want to think in terms of field theory. They wanted to replace it with something better, something where you don't have to renormalize away infinities. There were many electroweak theories. One approach was called the scattering matrix approach, another was called current algebra; there were all sorts of algebraic ideas about how to understand elementary particles, but field theory was not at all popular in those days.

Sugimoto: I see. What was the reason that Veltman and you believed in gauge theory?

't Hooft: Veltman was very pragmatic. He understood there was a basic problem in understanding the weak interactions. He learned about the experimental observations. Experimentally, a lot was already known about the weak interactions and about other properties of particles and forces. Also, their symmetry structure was very well understood since Gell-Mann's group theory became a well-known topic. It was understood how important

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group theory, Lie groups in particular, is for physics. That was clear but how to get the exact description of these particles was a big mystery. Now, Veltman even did not care pretty much about field theory although he liked the general formalism, but he just repeated what everybody else said that field theory is probably not going to be the answer. But in the meantime there was nothing better to do. He thought that field theory might not be the answer. But it was the thing I understood that I could do in principle, except we did not understand the details. We basically did not understand how to handle the renormalization effect of particles with the exception of the photon. The photon was understood; actually the best-understood particle.

Sugimoto: Right.

't Hooft: That was a vector particle, so why was it so difficult to renormalize other vector particles? The problem was in the masses of these particles. Veltman understood that the weak interactions are caused by vector particles with mass, and he tried to understand how to make a unitary renormalizable theory for such particles. He had discovered many of the problems that were there. He had his own approaches to the problems using gauge invariance and he was intrigued very much by the Yang-Mills paper. He said this should be somehow the way to do it.

Sugimoto: I see.

't Hooft: He had all the ingredients but there was still no answer about how to renormalize this theory properly. At that time this was thought as just one possible approach to physics and it was not expected that this was going to be *the* way to understand all the forces in nature.

Sugimoto: Were there many people trying to prove the renormalizability at that time?

't Hooft: No, there were not so many people who were studying renormalizability. There was Abdus Salam, there was Steven Weinberg, but they were asking more generic questions like "How in the world can these things hang together?" "How do we describe these forces?" "How can we understand what the next particle will be that is going to be discovered?" and so on. But, renormalization was not very fashionable.

Sugimoto: Did you think you would succeed when you started tackling this problem?

't Hooft: Well, as long as I hadn't been able to answer the main questions I didn't know whether I would succeed or not, but I was very ambitious and I knew for sure I wanted to get the best answers I could find. So I thought "If this problem can be solved at all I will try to solve it."

Sugimoto: I see, so how did you feel when you completed the proof of renormalizability?

't Hooft: I first thought, "I will need to convince people that this is the way to do things," because there was a sentiment against renormalization. So I realized that people were going to criticize whatever I had done and Veltman had the same response as well. "Maybe you have something interesting here, but people will ask this and that. Do you have your answers ready?" I realized that people were going to ask quite a lot of questions which I could not answer. This is a very mathematical problem. Mathematicians are very accurate and I was somewhat sloppy in the way of phrasing things. He said, "This is where you have to be more precise. Otherwise nobody will believe you."

Sugimoto: Were you excited about this?

't Hooft: Yes I was very excited because this was certainly the moment when I realized the importance of the Higgs mechanism. I didn't really call it that because I didn't know the papers by Higgs and Englert very well. I had heard that there were people thinking along these lines. So I accepted that I was not the first to write down these theories, but I did feel I was the first to understand how exactly the Higgs mechanism was solving the problem that Veltman had formulated.

Sugimoto: I see.

Found the Way to Cancel All the Anomalies

't Hooft: I realized, "Well, now I understand exactly how to do it and I have to fill in some details," but those were secondary details. The most important detail was the anomalies. It wasn't obvious that if you renormalize this diagram using this counter term, and that diagram using that counter term, if you combine the whole thing it will still be unitary; and indeed a counterexample was known. There were examples of theories where this would break down; that was the case when you have chiral symmetry; left and right particles are different.

Sugimoto: The chiral fermion.

't Hooft: The chiral fermion has anomalies in it. Those anomalies would be disastrous. Now not every theory has such anomalies, at least not that we knew. But still there was this danger; maybe there are more such anomalies. While formulating the rules to renormalize the theory, we have to prove that everything hangs together without any anomalies because if they were there, we could understand that renormalization would destroy unitarity, which would imply that it would not really work.

Sugimoto: Right.

't Hooft: I had some hopes that if there are anomalies, maybe you can find a way to rephrase the theory such that it still is unitary, but that hope quickly evaporated. No, you have to cancel out all the anomalies but how restrictive *is* that? Will there be *any* theory where all the anomalies cancel out? This was not known.

Sugimoto: I see. At that time, the anomaly cancellation of the standard model was not known?

't Hooft: No. Well, it was known that fermionic anomalies may be made to cancel. This was not certain, but I thought maybe the pure gauge part itself also has anomalies which we do not know how to cancel. It had to be proven that they cancel. The point is that the number of counter terms that can be used was not as large as the number of free parameters one has to renormalize. So, I realized there could be a clash, so that even though the theory looked renormalizable, things could go wrong if you try to work out all the details of all the diagrams; you will find that they are contradicting each other and then the theory will not be unitary. So this was still something that had to be proven. What was missing was a good way to regularize the theory and that was where basically I had the idea of varying the number of dimensions.

I first tried five dimensions, six dimensions, seven dimensions, and used these dimensions as regulators. In itself that nearly worked but it just didn't and then I discovered the correct answer or a much better answer by taking four plus or minus epsilon dimensions and letting epsilon tend to zero. That was the correct answer. It was amazingly so because you can ask "What does it mean, four plus or minus epsilon dimensions?" Physically it makes no sense, and mathematically it makes no sense. But I noticed where epsilon comes in the expressions it is just a number in a diagram which you can tune any way you like. You can choose a complex number if you want, and you can choose to let it go to zero.

Sugimoto: How long did it take for you to prove this renormalizability?

't Hooft: One of the questions is, how long did it take to convince myself? That was fairly quick, a year or so. Certainly after dimensional renormalization was introduced, I realized this is the answer and I don't need any further proof but that is not exactly the same thing as a mathematical proof. *To prove that it works correctly to all orders*: that was the main thing really. The way we phrase the problem is: prove that all final diagrams up to any number of loops can be renormalized using dimensional renormalization. That required some extra work but it was quickly sorted out that this indeed was the best way to do it.

Story of the Beta Function of Yang-Mills Theory

Sugimoto: I heard that you also knew that the beta function of Yang-Mills theory is negative before the work of Gross, Wilczek, and Politzer.

't Hooft: Yes, that is a somewhat strange story. Of course I was approaching the problem from the physical point of view. As a physicist, I wanted to understand how these fields work in practice. And then, it's very important to know how this system works at very short distances and how it works at very large distances. Very early, well before the dimensional renormalization and such, I asked myself what happens in the short-distance limit.

Sugimoto: Before dimensional renormalization?

't Hooft: Yes, because if the short-distance limit theory is sufficiently convergent, then all I need to do is to establish things at one or at most two loop levels. Everything else will become unimportant because if the theory is totally free that's all you need to know. So I did the calculation, I scaled, I had the final diagrams, and I could see how they scaled. I thought, "Well this is just fine." It has the right sign to be what are now called asymptotically free. I could clearly see that the sign implied what is now known as asymptotic freedom. So I couldn't understand why many people had such problems with it. There was an argument about Bjorken scaling, but I never quite understood what Bjorken meant when he talked about scaling. People said, "Bjorken scaling proves that field theories don't work." I could not understand why they said that because I thought, "Well when I scale the theory, it just works fine. I don't understand your problem." But what I did not realize is that nobody had yet calculated that beta function.

Sugimoto: Why didn't you publish this?

't Hooft: First of all, I thought there was a more urgent problem. The urgent problem was to understand why quarks are confined because this would be a theory for the strong interactions. The real problem of the strong interactions was the quarks. Why did they not come out as free particles? I thought we now had one half of the answer to this question, but the other half is what happens in the infinite distance region and that was much more difficult, of course. I think Veltman put me a little bit on the wrong track here in that he said, "Well as long as you don't understand why these quarks don't come out, you have nothing — it is not even worth publishing." That was a mistake. Of course, I should have. Yes, I believed in the theory but I hadn't understood that I was the only one who had calculated beta function correctly, and understood that it is negative and so the pure gauge theory would serve very, very well as a candidate for the strong interactions. Now we all know how this leads to the confinement of quarks, but that was just a big conjecture in those days.

Sugimoto: I see. Was it easy for you to accept QCD despite the fact that quarks and gluons were not discovered?

't Hooft: Oh, yes because I thought there's absolutely no reason for these quarks to come out as free particles because they have color and all physical states must be invariant under color. You can turn the question around. Why should there be free quarks? The answer is they are not there. I thought basically I understand that the theory doesn't have to have free quarks. But, the question is then what keeps those quarks together? How do you understand that such a theory will be unitary if you don't understand the physical states, the asymptotic states of the theory? That was basically answered by several pieces of insight. One was the fact that there are vortices and that the mechanism that keeps them stable was a dual opposite of monopole confinement, called the Meissner effect in superconductors. It's the dual Meissner effect, and the realization came slowly that you can understand everything with that.

The other thing was jet physics: that the asymptotic states are not free quarks but jets. Jets consist of hadronic particles, but they behave as a single quark coming out with high energy. Those quarks will then manifest themselves as jets and the gluons also as jets. So you have quark jets and gluon jets. And that was how unitarity could be understood to be restored; but those items were quite complex. They need not just hand waving but some more rigorous mathematical treatment.

Sugimoto: Do you think confinement in QCD is understood well enough these days?

Confinement in QCD: Acceptable as a Physicist, But Unsatisfactory as a Mathematician

't Hooft: I think the combination of these items to me as a physicist is quite acceptable and it explains everything. But as a mathematician, I would say, "Well the situation is not as good as it should be." QCD is not at the same level of accuracy as quantum electrodynamics and the confinement problem is part of that. It's now called the mass gap problem. That is, "Does a pure gauge theory, QCD, generate a mass gap?" The question immediately associated with that is, "Can we compute the mass gap?" "Can we understand what the lightest particles are in QCD?" The answer is that pions are basically the lightest particles of QCD, but can we prove this with mathematical accuracy and can we mathematically even define what a mass gap is? The question is if you know how to define the theory and know how to define the question, can we prove this property of the theory?

The strange thing is that the best procedure we have today is that we prove it numerically. We simulate this theory on a big computer by putting the thing on a lattice. We take the lattice as fine mazed as possible and then we see that the theory behaves exactly as all physicists expect. So there is no problem. They say "We can prove everything in mathematics; the first 10 decimal places obey this theorem, so indeed all numbers obey this theorem." That doesn't go with mathematics. Mathematicians will not accept this as a proof. Of course not. They shouldn't. It's still a problem in physics, but I think it's an academic problem. We don't need that problem to be solved to understand how QCD works, but we do understand that it needs to be solved from a mathematical point of view. The importance of a mathematical proof may well be that if you have proved this mathematically you also might find new alleys to do faster and more precise calculations. It won't be a waste of time to prove mathematically that the mass gap exists because then you can actually make accurate computations for everything.

Sugimoto: Do you think it will be proven someday?

't Hooft: I think it will be. I think what we need is some monk on an uninhabited island who sits in a monastery with his books and his computers and his laptop and his internet and he just works out the proof. There are hundreds of epsilons and deltas that you have to put in the right position and then I think you can prove it. I do believe that this is a property of our physical theory. We all believe it's true and therefore we all believe it can be proven, but it's going to be a very tough and very unrewarding work because after 20 years the monk would come out of his monastery and he says, "Look I have proven QCD to exist," and all physicists will stare at him and say "What's your problem? Why have you been doing all this work? We knew that QCD is a fine theory." So he will not be rewarded. Probably he might not get the Nobel Prize for it even though it's a very important mathematical question.

1/*N* Expansion and String Theory

Sugimoto: I see. Another thing that I wanted to ask you is about 1/*N* expansion. How did you come to the idea of expanding amplitudes with respect to 1/*N*?

't Hooft: At that time I was at CERN as a fellow and all these marvelous new ideas came along, and one question was exactly the question which I mentioned, "How to have good approximation techniques for QCD?" Is there a small parameter in QCD? Is there a parameter you can tune even if physically it might not be so small such that, if you tune it to be very small, calculations can be done accurately? If the parameter will be larger, the theory is slightly less accurate but certainly you have a systematic expansion. Of all parameters, of course, $1/N$ came along as a parameter and I asked myself now, "In what way does the large-N theory distinguish itself from arbitrary N theories? What does the limit N to infinity look like?" I knew that a certain simplification took place in the diagrams. I wanted to understand "What kind of simplification is that?" I found the answer to that. Unfortunately, the answer was that even in the N to infinity limit, the final diagrams can still be so complex that you cannot compute them exactly. We cannot do the large N expansion explicitly. The power expansion in that expansion constant generates all the planar diagrams. They are too complicated to solve. I searched very hard to see if there is any way to get some sort of internal equation to solve the large N diagrams for QCD, but that, up to this day, didn't work.

Of course, the question was extremely interesting because only the planar diagrams survived and they looked very much like the world sheet diagrams of a string theory. By that time we understood that there will be vortices that connect the quarks together. So this will be a perfect way to understand where these vortices come from, in principle.

Sugimoto: So, this $1/N$ expansion resembles the perturbative expansion of string theory.

't Hooft: Yes.

Sugimoto: Did you think that it can be used to formulate string theory?

't Hooft: That was certainly our hope, yes. I was hoping that this would also vindicate string theory. It would tell you why all the dual resonance models were so successful for the strong interactions. I wanted, in fact, string theory to solve my problem, which is, I want to understand the N to infinity limit of QCD. Maybe that's a theory that can be written down in a closed form. The point is that the $1/N$ to zero limit, or the N to infinity limit, is a limit where the mesons and the baryons do not interact. It is a free theory and for that reason, you might suspect it is exactly solvable. Free theory is basically trivial. All you need to know is the mass spectrum. I thought string theory should help me do this. Maybe the $1/N$ expansion is equivalent to a string theory. I hoped to see that happen. But though I tried many times, I couldn't identify any string theory that coincides with the $1/N$ limit of QCD.

Sugimoto: Do you think string theory is a promising candidate for quantum gravity or…?

't Hooft: Personally I think that it is a very good and interesting mathematical approach to quantum gravity, but not sufficient. I think physically there's got to be more. You have to make a distinction between the physical question and the mathematical question. Mathematically, string theory is a very interesting mathematical construction. It should be taken very seriously in trying to understand quantum gravity, but physically I think the ultimate underlying equations are not string theory. But I am in a minority here.

Sugimoto: You are the one who first proposed the idea of holography out of the consideration of black hole entropy. Later Maldacena and others refined this idea in the context of string theory. How do you think about this development?

How to Understand Physical Degree of Freedom of Quantum Gravity at Planck Scale

'**t Hooft:** They really took off in a direction which was never my intention. They are using duality which is not quite the same as holography. I find dualities interesting but they are not going to be an answer to our physical questions. They are going to be helpful. They are going to relate one problem to another problem. Holography is being used in the sense that certain different theories are equivalent. But that really never was my problem. The problem is how to understand the physical degrees of freedom of quantum gravity, and, in particular, at the Planck Scale. I am convinced that at the Planck Scale we only have bits and bytes of information. We don't have a continuum anymore in which things live. String theory is still suggesting that you have to think in terms of real numbers and continua, and I have reached a stage now that I don't believe anymore that the real numbers are going to be the fundamental variables of all the ultimate theory. I think the ultimate theory would just be based on bits and bytes basically. But to understand how it works is now the big problem. We don't understand that.

Now holography tells you that the number of degrees of freedom is actually even less than what fits in the bulk. It basically corresponds to what fits on the surface. Now the physical implementation, I think, is different from what you usually hear when people talk about string theory and holography and AdS/CFT and so on. I think the physical reason is in the fundamental origin of quantum mechanics itself; holography tells you that the degrees of freedom fit on a surface and not in a volume in a bulk of space-time. There must be a good reason for this. The reason I can find is called information loss. The point is that all information about a certain physical object in a volume of space is already to be found on its surface. You can think of taking a region of space and time. The region is bounded by a surface. If you look at all physical phenomena on that surface, you can actually reconstruct what happens inside. If you think a little bit you find that is not as strange as it sounds, because the gravitational field obeys Gauss's law, which really means that if you know the gravitational field on a surface, then you know exactly the amount of energy which is encapsulated by the surface. If you know the amount of energy, you have the Hamiltonian. In other words, strictly speaking Gauss's theorem for gravity will tell you that if you know the gravitational field accurately on the surface, whatever that surface is, you have got the Hamiltonian of the entire system, in principle. That means the inside is also understood.

That is of course a very strange situation, and that tells you quantum gravity is going to be a very crazy theory, unless you do it my way. My way is that you have to re-evaluate our understanding of quantum mechanics itself. And, if you replace quantum mechanics by a deterministic theory, then I can understand the holographic principle much better. Then, it tells me that actually the underlying quantum theory is not keeping all the inside information intact. Information dissipates away. Imagine a surface, and the information that has dissipated away through the surface. Then if you know the data on the surface, you have all the information you need to be able to predict how the inside of the thing will evolve in time. That's counterintuitive, but I have all sorts of ideas now about how this can come about in the ultimate theory of quantum mechanics.

The Best Theory Is a Theory That Explains Experimental Observations

Sugimoto: You have done so many ingenious works. Which one of your works do you like the best?

'**t Hooft:** Well, I think I am still very proud of what happened in the first few years of my career that I had the idea of renormalization of gauge theories, of dimensional renormalization, and the role played by the Higgs mechanism in renormalization. The magnetic monopole was a very fortunate moment, and so was the $1/N$ expansion, but also there are some very nice ideas about instantons and their role in explicit symmetry breaking of a theory. The standard model doesn't conserve baryon number even though it looks – if you look at Lagrangian – that baryon number must be conserved; but when you take the instanton effects into account, baryon number is not conserved. That's a very deep and beautiful observation that we made. Those are essential things, and so I think they are the best.

But also, in a different way, I am proud of what I did later on in gravity and quantum mechanics, although there are many that have to be proven. I would love to talk about gravity and quantum mechanics with anybody but I didn't have such great ideas there that solve the problem. I still see quantum gravity as a big problem that we don't understand. I want to make fundamental progress there. Of course, we have to realize physics is an experimental science in the very end. The best theory you can think of is a theory that proves or explains something that is being observed experimentally. What I would love to see is an explanation as to why physical constants have the values they have, why the proton electron mass ratio has the value it has, or anything of that sort, to understand where constants of nature come from. That hasn't happened yet. That's why I think that there is much more work to be done.

Advice to Young People Who Want to Be Scientists

Sugimoto: I see. Could you give some advice to young people who want to be scientists?

't Hooft: Science is still extremely interesting as an activity. When you are a scientist you will discover things, but usually you discover very small things. If you are hitting something big that's of course even a nicer experience. But you should realize that when you do some research on some topic, you want to know how that research relates to the ultimate questions that we are really interested in, like solving quantum gravity, finding the theory of everything, understanding this and that. We won't answer those questions overnight and the young students we see today may not find answers to such questions overnight. But they might contribute some further steps towards finding answers. You can only contribute if you understand what those big questions are. I think you should work on the big questions and if you are lucky and you are not afraid of asking difficult questions, then maybe you will find some interesting answers.

You have to be extremely critical. That's the other advice. In particular you have to be critical about your own results. You shouldn't be happy with what you have found or what you have understood so far. You should always ask more detailed questions, "Did I understand this?" "Did I understand that?" and "Why shouldn't the answer be formulated in a different way?" If you ask very critical questions to yourself, maybe you will find some new interesting answers.

Sugimoto: Okay, it's about time. I really enjoyed talking with you.

't Hooft: Thank you.

Sugimoto: Thank you very much.