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In your opinion, what are the main problems in theoretical physics at the moment?

Understanding the foundations of quantum mechanics and the problem of finding a theory of quantum gravity are the key main problems. Also, of special importance, is the whole conundrum of questions such as why we see the laws of nature that we do and why these specific initial conditions that originated the universe that we are in.

And why do we need a theory of quantum gravity?

We need it in order to have a unified picture of the world and therefore a unified understanding of the world. There are actual experiments in the domain where Planck's constant, Newton's constant and the speed of light are all relevant. At the present moment, we can really do experiments at the Planck scale and therefore we need to be able to predict what the outcome is. That is why we need a theory of quantum gravity.

What kind of experiments are you referring to?

Astrophysicists have been conducting several experiments in this domain. For example, one can measure whether there is a time delay in the propagation of light, or in other words (though not the most exact but good enough), one can measure if there is a dependence of the speed of light on the energy of incoming light rays due to quantum gravity effects. These effects would be of the order of energy over the Planck energy and hence potentially observable, for instance, in observations of gamma ray bursts at FGST. Also, the question of whether Lorentz invariance is broken or not is observable at that order, in fact, at six or seven orders of magnitude pass that order in a number of different experiments which test for effects of birefringence. So we are definitely in the era where quantum gravity is testable and we need to have theories that are able to make predictions. Simultaneously, we have the opportunity of constraining ideas and theories of quantum gravity by experiment. Now, whether the "quantum theory of gravity" is the quantization of a classical theory of gravity or is arrived at through some process that is deeper I don't know. I'm very open to the possibility that it is not the quantization of a classical theory of gravity.

Why is it so difficult to quantize gravity?

It's not difficult. The situation we are in is that there is not one candidate but there are half a dozen instead. Some of them are more developed than others and the two that are more developed are loop quantum gravity (LQG) and string theory. LQG, certainly, is a consistent theory, which is the quantization of general relativity. It has quantum dynamics, quantum kinematics and we understand it plus we can do calculations with it. In string theory, there is, to a certain approximation, a unification of all forces. The difficult part is not combining general relativity and quantum mechanics.

The real difficulty is in finding a way of selecting the right way of doing it. The fact that it was not enough just to find a theory of quantum gravity is very interesting. One would have not thought that one could get half way in a half a dozen different ways. I think that's the truth. There are other approaches, which are not as well developed, that look promising such as causal dynamical triangulations.

Why did you say that string theory is only valid up to a certain approximation?

Because it's only defined in perturbation theory and it's only demonstrated to be consistent in finite perturbation theory to genus two. Finiteness has not been proven to all orders and related is the problem that there is no non-perturbative formulation of the theory in general. There is the AdS/CFT correspondence but that's a conjecture. We can talk more about it but it's a longer discussion. However, I should say that I think that there is an AdS/CFT correspondence but I think that it can be argued for on very general grounds without having to recur to supersymmetry or string theory. This is not to say that the specific form of the AdS/CFT correspondence conjectured by Maldacena could not be a specific instance of this more general point of view.

What is the problem with string theory being only defined perturbatively? The standard model is only defined perturbatively and it is very useful. . .

I don't want the focus of this interview to be a critique of string theory if you don't mind.

Sure, I just wanted to hear your opinion about it because there are also many criticisms of LQG so it would only be fair. . .

Let's spend our time on LQG then. My view of string theory is very public and I have worded what I have to say very carefully. People can read what I wrote, which is not totally negative. Very quickly, my view of string theory is that there are some things that I'm very interested about in it and there are some things that I believe have not yet been demonstrated convincingly. I have put it all out on paper so we don't have to go through it again. Also, LQG has been developing much faster in the last few years and not so much has changed since five years ago with regards to my critique to string theory, that is, not so much has changed since I've published "The Trouble With Physics" [1]. A lot of things have happened with LQG so I'd rather spend a lot of time on that.

Okay, let's go there then. What is loop quantum gravity?

There is a broader and a narrower LQG. The broader LQG is a method to quantize diffeomorphism invariant gauge theories. It can be applied to any diffeomorphism invariant gauge theory and has been applied to a wide variety of them including lattice general relativity in 3+1 dimensions, general relativity and supergravity in higher dimensions, various theories in 2+1 dimensions and 1+1 dimensions. So this is the broad sense: it's a precise method of quantization, that is, you give me a diffeomorphism invariant gauge theory in any dimension, with dimension two or greater, and we give you a Hilbert space of states, which is gauge invariant and spatial diffeomorphism invariant, and a procedure to construct the dynamics through a path integral.

You said spatial diffeomorphism invariant, not spacetime, right?

Yes, that's the kinematics. So, that's the broad meaning. The specific meaning is a particular instance of this, in which it is applied to general relativity in 3+1 dimensions, coupled to matter fields, coupled to gauge fields, fermions, chiral fermions or scalar fields, and that is quite a bit developed by now. The quantum kinematics has been understood rigorously for a long time but in 2002 there was a uniqueness proof, namely, that the Hilbert space that we use for the quantum kinematics is unique when subjected to some natural assumptions, that is, there is a unique spatial diffeomorphism invariant space of states in which theories can represent the Wilson loop operator and the area operator. In the last few years, there has been a convergence of results to a unique dynamics, which is called the new vertex. Before, there were other forms of the dynamics that were investigated and found fault with for different reasons. The so-called new vertex was invented independently by four different groups and later found to be equivalent. As far as we know, it has only good properties, namely, there is a continuum limit, which is classical general relativity.

You mean a continuum limit or a semi-classical limit?

They are different in their result about both.

Okay, so you are saying that there is a continuum semi-classical limit that gives general relativity?

We had a seminar group meeting a week ago by Claudio Perini about his work with Elena Magliaro with a demonstration of exactly that [2, 3]. Another work that has shown this is the one by Laurent Freidel and Florian Conrady published about two years ago [4, 5].

But this last work by Freidel and Conrady is not a continuum limit, right?

Right, that's more like a kind of semi-classical limit. The one of last week by Claudio and Elena is a continuum limit because they take a limit in which the Immirzi parameter goes to zero, which means that the area quanta goes to zero, the size of the energy quanta goes to zero, so that the spectrum of the area invariant becomes continuous. Then, in that limit, they were able to use a semi-classical expansion in order to compute corrections in powers of the Immirzi parameter, for small Immirzi parameter. So they were able to derive general relativity at the leading order in that expansion and corrections which include...

What kind of corrections?

Parity breaking corrections, for example. The most interesting thing is that it had been kind of conjectured for a long time that the Immirzi parameter would be a sort of measure of the quantum corrections that break parity and chirality []. For example, Claudio and Elena have a computation of the graviton propagator, which to leading order is what it should be with the right tensor structure, but there are corrections proportional to the square of the Immirzi parameter which break chirality.

Okay, but I thought that one of the big claims of LQG is that it can explain the Bekenstein-Hawking formula for black hole entropy under the condition that the Immirzi parameter had to be fixed. So, how does this reconcile with an expansion in the Immirzi parameter?

That's a very good question, so let me highlight some of the results in black hole entropy. One can consider a spacetime to the exterior of a black hole horizon and model it as a dynamical system with the horizon represented by boundary conditions. So, first of all, there is a consistent classical version of general relativity where the horizon is treated as the boundary and there are dynamics on the boundary and degrees of freedom on the boundary. One would like to be able to do much better but this is what one can do.

What do you mean?

Ideally, one would like some sort of quantum model of a black hole that lets you go inside and so forth but what's done in the papers that you are referring to, where strong claims are made, is that the exterior of the black hole horizon is modelled as a dynamical system with degrees of freedom on the horizon as a kind of boundary space of degrees of freedom. When one quantizes the system employing the boundary conditions, one gets a Hilbert space of states, which has a sector with just degrees of freedom on the horizon. Furthermore, one discovers, first of all, that it encodes the classical degrees of freedom of the horizon - the ways that the horizon can oscillate or shake - in a way that seems to be correct and, secondly, that the Hilbert space is finite dimensional and the logarythm of the dimension of the Hilbert space is proportional to the area of the horizon. Now, there are two ways to do this, one of them was developed by Ashtekar, Baez, Corichi and Krasnov [6], in which the $SU(2)$ local gauge invariance of the connections is broken to a $U(1)$ on the horizon and, in such formulations, you have to fix the Immirzi parameter to get the one fourth in the Bekenstein-Hawking formula. By the way, there is still some controversy, at least in my mind, about what the precise value of the Immirzi parameter needs to be but its something of order 1.

The other way to do this is actually the way I originally introduced the idea of entropy on the horizon in the cosmological case which doesn't break the $SU(2)$ to a $U(1)$ on the horizon [7]. The key idea there, which was taken up in other works, is that it's a Chern-Simmons theory describing the degrees of freedom on the horizon. Recently, the group in Marseille has been applying that method to the black hole horizon and they find that the Immirzi parameter does not need to be fixed, that is, the results are independent of it. This is the work of Alejandro Perez and Daniele Pranzetti [8]. So the relationship with the Immirzi parameter is no longer necessary and I like that because it was a problem that always made me nervous, namely, why should it only agree with semi-classical results for one value of the Immirzi parameter? What about theories with other values of the Immirzi parameter? Why shouldn't those have a good semi-classical approximation? So, this is the story of $SU(2)$ Chern-Simmons black holes and is new so there are still somethings to be understood there.

There are some other results about black holes that I think are interesting. One of the most interesting is the result related to corrections to the Hawking spectrum, that is, it turns out that there is a hyperfine structure to the Hawking spectrum because the area is quantized in a way which is not uniform. These ideas have been developed by Mohammad Ansari [9], who was a student here, and by a group in Valencia in Spain [10]. So, if somebody could see a primordial black hole evaporating, which is unlikely at the moment, there would be a way to check if these results are correct.

Okay, we derailed a little bit so going back to some more general questions... what are the fundamental degrees of freedom in LQG?

LQG is a very conservative research program so, as I mentioned, it is a quantization method for classical theories. So, the fundamental degrees of freedom are the degrees of freedom of whatever classical theory you are quantizing. If you take classical general relativity, for example, its degrees of freedom are represented by the Ashtekar connection or some generalization of it, or the canonical coordinates and the conjugate momentum related to the area of surfaces.

Is there a good reason for expecting that you can take general relativity, quantize it and obtain the real world at the Planck scale? Why not supergravity, for example?

Well, as I said earlier, you can apply the same method to supergravity and it has been done so. There is some recent research program by Thomas Thiemann and collaborators to apply to eleven dimensional supergravity [11], which is at the heart of the whole string world, and give a quantization of eleven dimensional supergravity. So, as I said, in a broader sense, it is a methodology. However, one can be ambitious is two different directions. For instance, one can look for a way in which the fermions and gauge fields of the standard model might emerge from structures in quantum general relativity. There was a research program related to that in which braid excitations - excitations which look like braids - were found to propagate in a way that is characterized by some quantum numbers and there were some speculations that one would be able to get matter out of quantum topology, which is an old idea. I was part of this research program together with Fotini Markopoulou and Sundance Bilson-Thompson [12].

Was this successful?

Not yet. I think it's interesting but it's certainly not yet successful.

I guess this would be some sort of unification, right?

Yes, if it would be successful but I don't expect it to be successful. In any case, I think it's worth investigating since there are these excitations which have braids and more complicated topologies and we want to understand their role in the theory. Another direction is that there is a natural way to unify general relativity and gauge fields in a non-trivial way by extending the dynamics in LQG either in the Ashtekar form of the constraints which was the program of Peter Peldan [13] or in the Plebanski action which is the more recent way that I developed in response to Garrett Lisi's work. Garrett Lisi had a conjecture related to using a certain gauge group E_8 to unify all the degrees of freedom [14] - an idea that he took from a very old paper by MacDowell and Mansouri [15]. I was then able to make a more elegant version of this idea, working together with him and Simon Speziale, not specifically for E_8 but for unifying gravity with any gauge group [16]. I think that it is an encouraging work and worth investigating.

Okay. I want to return back to the issue of a semi-classical continuum limit because I am slightly confused since I hear contradictory statements from different people and different papers which lead me to think that there was no such limit...

Let me just respond to that. First of all, we have been saying that. In LQG, we have, with a few exceptions, never exaggerated our claims. We have always been upfront with what the issues are, what the open problems are...

I understand but I did notice in several papers many years ago that people were saying that there was a semi-classical limit if you introduce a cosmological constant [17, 18]. If I'm not mistaken, this is referred to as the Kodama state [19, 20], right?

Let me come back to the Kodama state later. In any case, in spite of the interest in the Kodama state, most of us have been saying that the important issue was to have a well-defined semi-classical limit and semi-classical approximation. The recent results using the new vertex are good results and people are working on this so better results are coming, in fact, I should also say that I have been very impressed with this work. My work in the last years has not been focused on LQG but on a variety of things, such as quantum gravity phenomenology and some exploratory ideas like unification that we have just talked about. I have not been working in mainstream LQG which has consisted of the exploration and development of spin foam models. So, I had to be impressed by the recent results.

Now, since you mentioned the Kodama state, let me separate out two issues. First of all is the Kodama state a semi-classical state? Yes, at the level of a WKB wave function it's a semiclassical state associated with deSitter space. There are a number of things that can be derived from it, for example, one can couple the theory to matter and derive quantum field theory for the matter in deSitter space using the Wheeler-DeWitt equation, etc [17]. It functions just fine as a semi-classical state. Secondly, is it an exact state of quantum gravity? The answer is that there is a particular ordering and regularization of the Hamiltonian constraints which makes it so. There was understandably a lot of excitement about this a long time ago. However, is it a physical state, that is, is it normalisable under the correct inner product? And, do the choices that you have to make in the Hamilton constraints to make it a physical state have the right regularization order in the Hamiltonian constraint? This is not clear and I'm not optimistic.

Okay. There is a claim in the literature that using the spectral triple approach to LQG one can find a well-defined semi-classical limit [21]. Is this something useful?

I must say that I have not understood their work properly and I feel bad about it. Their work is in the realm of mathematical physics, which is not where my tool box is. We've invited them here twice and they have given talks but I still don't understand it properly. If you wanted an evaluation of that work you should ask some more mathematically-minded people.

In LQG is general covariance actually there at the quantum level? Is this clear?

I think it's not. Let me give you my view and not everybody's view. My view is that the approach to dynamics to the Hamiltonian constraint does not yield a successful dynamical theory that has a good low energy limit and massless gravitons in it. There is a lot of disagreements about this but my understanding is that after Thiemann made rigorous some ideas that many of us have been playing with, regarding the construction of the Hamiltonian constraint, and formulated the Thiemann constraint in closed form, it was realised pretty quickly that we did not have a low energy approximation with massless gravitons. This was clear to me and pretty soon the interest in dynamics shifted a lot to spin foam models. There is some hand waving arguments stating that because of the kind of discreteness that is present in LQG you shouldn't expect there to be an infinitesimal generator of time translations because there is not an infinitesimal generator of spatial diffeomorphisms. There is only finite unitary operators representing spatial diffeomorphism invariance. There are various versions of this argument, for instance, Fotini Markopoulou had some, Carlo had some and I had some.

So, starting sort of in 1995, roughly, the interest in spin foam model began to develop. It took a long time from then till now to find out what is the right form of the spin foam amplitude so that new results could be derived from it and it's embarrassing that it took so long. In order to demonstrate that there is really a strong form of spacetime diffeomorphism invariance requires some results a lot stronger than the existence of a semi-classical limit where you find the semi-classical Einstein equations dominating the path integral. Issues of spacetime diffeormorphism and how they should be recovered or not at the quantum level is something that, for example, Bianca Dittrich has been very focused on [22]. I don't know everyone you are talking to but if you talk to people in the LQG community you will find a range of people from highly enthusiastic and optimistic to concerned about technical issues. Probably Carlo Rovelli is on one extreme and if you listen to his talk at the Madrid meeting, which I think is online, he says something like "here is the theory that we have, we don't know everything about it but it is well defined and has this and that property.". I've tended to be on the more critical side. My role in the LQG community is not that different than my role with respect to the string theory community, it's just that LQG is a small community and hence it is less publicly manifest that I tended to be more critical and more conservative, especially in the last 15 years or so. So, I've been kind of won over but say Bianca is much more critical then Carlo is about technical issues regarding the recovery of diffeomorphism invariance and if you talk to Laurent he'll probably take a position somewhat in the middle.

One of the predictions of LQG is that the spacetime is quantized. There is an area operator and there is a minimum area. If this is a prediction, how can you probe it? How can you check that this is there?

By measuring some really small areas (Laughs), like black hole entropy, which is a consequence of the discreteness of the area. The corrections to the semi-classical Hawking radiation that I mentioned would be another way. One important question that we don't know the answer in 3+1 dimensions is whether, as a consequence of the discreteness of LQG, Lorentz invariance is either broken or deformed. I think that there is good theoretical evidence in the new spin foam models that there is a relativity of inertial frames and that there is no breaking of the relativity of inertial frames in the definition of the path integral. Whether, resulting from it, there is a phenomenology or not, that is, whether the excitations around the low energy limit transform naively as in special relativity or there is a deformation of the Poincaré group, it's not settled. I hope for the second possibility and I've had some papers making very rough semi-classical arguments in this direction but it's far from being shown. In 2+1 we know it's true that the low energy description of 2+1 gravity coupled to matter is that the matter moves in a non-commuttative geometry, where the Poincaré algebra is quantum deformed. This has been understood precisely partly due to the work of Laurent and Etera Livine [23] and probably a bunch of other people. So this could potentially have phenomenological implications.

Do you find any evidence of a fractal structure at small scales?

I don't think so. There is a story where speculations about spacetime being fractal-like were put forward - a work that Louis Crane and I published in the early 80s inspired in some of the ideas of LQG [24, 25] - but that is not within LQG. The idea that spacetime should become scale invariant at short distances has been around for a while. It is an interesting idea and ultimately leads one to consider a fractal structure. In LQG, I don't think that you would find a fractal structure generically. There are some interesting papers from different approaches to quantum gravity that suggest that the scaling dimension seen by matter fields propagating through quantum spacetime might become less than two at short distances. This is found in the work of Ambjrn and company in causal dynamical triangulations [26] and it's also argued for in the asymptotic safety scenario [27]. Leonardo Modesto, who is a postdoc here, and some collaborators have had some papers arguing that this could be happening in LQG [28, 29] but I don't think that those are strong results.

Is there any evidence for discreteness coming from other approaches to quantum gravity?

But the most important question is: do we have any evidence coming from experiment? And the answer is no. This is important because experiments like Fermi in which one can look for parity breaking effects in the propagation of light are there. The effects might be of the order energy over Planck energy which is the most natural prediction if Lorentz invariance is broken [30]. There are strong constraints by now at the Planck scale, the strongest were published a couple of years ago and were using data from gamma ray bursts [31, 32]. So we have good evidence that test discreteness, if it would show up as the breaking of Lorentz invariance. Similarly, there is good evidence that the GZK cutoff is really there in high energy cosmic rays experiments which was another prediction of the breaking of Lorentz invariance []. So, for me experimental evidence is the most important thing.

What about theoretical evidence for spacetime discreteness from other approaches?

The causal set approach assumes this from the beginning, so they don't derive it if. They claim that they have a form of discreteness which also preserves relativity of inertial frames [33].

And what about string theory?

The string vacua that people understand, say, around ten dimensional spacetime transform under the standard Poincaré algebra naively without any breaking of Lorentz invariance. There is theoretical evidence that in certain kinds of string scattering you can start to see that the longitudinal momentum is quantized. There is actually some papers of Lenny Susskind and others [], which exhibit this beautifully.

In your book "Three Roads to Quantum Gravity" [34]... you seemed pretty optimistic that all the different approaches could be combined and give one single theory. Do you still hold this opinion?

That belief had several parts which I don't hold anymore. First of all, there is no background independent formulation of string theory. Whether the tools and mathematical structures in LQG could be used to formulate a background independent form of string theory it's an interesting direction.

But is this even possible?

Yeah, I worked out some of it until I got sick of it [35, 36]. Thomas Thiemann and collaborators aimed for a first principle quantization of eleven dimensional supergravity which using LQG methods, which is one way to go about that [11]. Another place to try to make a unification is using matrix models, which are a common language. Recently, one form of spin foam models, called group field theory [37], was introduced, which is a kind of natural extension of matrix models. The furthest that people got in formulating M-theory was by using some matrix models. So, there is a kind of common language and about 10 years ago I was spending a lot of time on it while I was writing that book. A few years after, I was spending a lot of time working on a classic matrix model that I call cubic matrix model [38] or Chern-Simmons matrix models which were aimed at expressing that unification. I think there is some interesting ideas and some interesting structures there but nothing conclusive.

Was there any explicit connection found? For example, Dijkgraaf, Gukov, Neitzke and Vafa found a connection some years ago between topological string theory and the topological sector of LQG [39]. Was any thing as concrete as that found using these other ideas?

Not a strong one, it was more at the level of "this might be the case". What I do believe is that the intellectual roots of string theory and of LQG are common, they are based in the ideas of Polyakov, MacDowell and Wilson, namely, that the degrees of freedom of quantum gauge theories can be understood as extended objects with some dynamics. I understand that string theory is the exploration of that idea in background dependent terms, in which these extended objects are living in a classical background spacetime, whereas LQG is an exploration of those ideas in a background independent way because it is the natural quantization of diffeomorphism invariant gauge theories - hence background independent gauge theory. So I think there is a common root, and it's my guess that if there will ever be a background independent form of string theory, it will look like or be reminiscent of LQG as we have understood it so far. There are several obstructions, the first being that I'm not convinced that there will ever be a background independent formulation of string theory. I'm not satisfied by the AdS/CFT correspondences as a background independent formulation because it is, in fact, highly dependent on boundary conditions.

Is it a problem that it is dependent on boundary conditions?

Yes, it is a problem because, first of all, the world is not AdS. But anyway, it's not what I would call a background independent formulation. It is something remarkable but it is not what I would call a background independent formulation.

Can you explain why not?

Because background independence means no boundaries and no asymptotic structure. Background independence goes back to the idea of Leibniz that everything is relational and that there are no absolute structures that you have to specify that give properties to degrees of freedom without themselves being dynamical. Anything that figures in the definition of a dynamical degree of freedom must itself be dynamical. That was the idea, that's the tradition of Liebniz, Mach, Einstein, etc, and I think it's the most important idea in the search for quantum gravity.

Summarising, because there is this boundary condition there you think it's not a complete background independence. Is that it?

Yes. Also, as I mentioned earlier, many of the results of AdS/CFT can be explained by properties of general relativity without asymptotic boundary conditions or symmetries. There is this new formulation called shape dynamics of Henrique Gomes, Sean Gryp, Tim Koslowski and Flavio Mercati [40] where they derived, from a reformulation of general relativity, a deSitter/CFT correspondence without any assumption of supersymmetry or string theory. Nevertheless, I would also love to see the development of a real background independent form of string theory. I'm very disappointed that as far as I know nobody is working on that problem anymore. I continue to think that string theory is an interesting hypothesis even though the landscape issue for me is a big issue but...

Why is this a big issue for you?

Mainly, my problems with the landscape are related to the third type of questions that I mentioned as main questions that theoretical physics faces. These are the sort of cosmological questions of why these laws of nature and not others, what are the initial conditions, etc. So let's come back to this once we are done talking about quantum gravity.

Okay.

Something that I also would love to see, and I've said this for years and years, is LQG applied to extended systems with extended symmetries such that you could compare results about black holes, about supersymmetry and so on. String theory results about black holes are all about extremal or near-extremal black holes, which means supersymmetric black holes. There are results in $N = 1$ supergravity but they are not strong enough, so this is one reason why I'm very supportive of Thiemman's research direction.

You mentioned this new correspondence related to shape dynamics. Would you be able to recover all AdS/CFT results with this approach?

That's too strong of a claim but I suspect that many of the results will turn out to have nothing to do with supersymmetry or string theory. The correspondence is more general. Then, there would be particular cases, for instance, if you impose supersymmetry you would get stronger results. The idea of an AdS/CFT correspondence seems to me to be quite general and to hold independently of string theory and supersymmetry.

But in this new approach you would still have a quantum theory living in AdS or not?

That's a very important question. I don't know the answer.

Do you expect that supersymmetry will be found at LHC?

I'm very neutral. It doesn't bother me that it hasn't been seen so far and it doesn't destroy anything about my world view if supersymmetry is not there. I don't think that it's particularly elegant. If it had played a role in unification of particle physics and solved the hierarchy problem, I think it's fair to say that we would already have seen it experimentally. I'm not an expert but that's my understanding. It was a reasonable hypothesis as far as the hierarchy problem is concerned but I think that technicolor is a much more elegant hypothesis. I'm much more interested in the idea that the Higgs is a composite field, which is a result of some dynamical chiral symmetry breaking. However, if there is any $N = 1$ supersymmetry in nature, it won't disturb me.

Why do you put so high on your list of demands background independence?

Because I think that's the lesson of general relativity and because I think that Leibniz' principle of sufficient reason has been over the history of physics the most important and most beneficial productive guiding principle. I think that the triumph of general relativity, namely, that spacetime geometry is dynamical and relational is a triumph of that view and I don't want to go back from it, I want to go forward from it.

Okay. Is spacetime somehow emergent in LQG?

Yes.

It is?

Sure.

Even time?

That's a very, very interesting issue. My own view for other reasons, which has changed over the last years, is that I strongly believe that time is not emergent and that there is a fundamental time irrespective of LQG or any other approach to quantum gravity. I have a variety of reasons for this but the most strong has to do with the problem of understanding why we have these laws of nature rather than others. I have a view which is very close to the view of Charles Sanders Peirce - the American pragmatist-, who said in 1893 that, and I could probably quote it because I often quote it in writing, the problem of why these are the laws demands explanations basically restates the principle of sufficient reason. It's not rational to simply say that these are the laws of nature but not justify why those are the laws and not others. Then he said that the only possible rational or scientific - scientific is my interjection - way of accounting for law, is if it they are the result of evolution. I take that to mean evolution in time and so, if laws result from an evolution in time, then time is older than law. Therefore, it can't be that time emerges from some fixed law. I started to develop this idea in my work on cosmological natural selection which is where I introduce the idea of the landscape and I have been spending a lot of time on it. At the moment, I'm writing two books about that idea. One of them goes much deeper that the "The Life of the Cosmos" [41] and it is about the reality of time [42]. It's basically the case for time not being emergent and time being real. The other is a book with philosopher Roberto Mangabeira Unger which is going to be a professional philosophy book about why laws have to evolve and how to deal with various issues that come up when you claim that the laws evolve [43].

But I always thought that all the results from loop quantum cosmology pointed towards the direction that there is no time and that time emerges at some point...

Yes, because there is this idea that the Wheeler-DeWitt equation is timeless and within some models you can realize that idea. Loop quantum cosmology has simple models and that idea can be realized. However, I don't believe that that idea can be realized in a full quantum field theory with infinite number degrees of freedom. By the way, one of the reasons why I think that shape dynamics is important is that it shows that general relativity is equivalent to a theory with real time.

Going back to the same question... is time emergent or not in LQG?

In LQG, to the extent that it is a conservative realization of quantum mechanics, if you study it in the cosmological context, in which there is no boundary, then, if that succeeds, time will be emergent. All the dynamical results of LQG like the results about the graviton propagator and some of the results of the semi-classical limits, are best defined in the situation with a boundary, as they are not cosmological theories. So, it would not surprise me if the project of quantum cosmology consisting of deriving the world from the wave function of the universe fails at the full quantum field theory level but that would not contradict any of the successful results about loop quantum gravity so far.

Now, could you explain a bit your idea about cosmological natural selection?

Let me just focus on the aspects of it relevant to the context of this interview. A very important thing that I have to say is that the landscape issue was on the table since a paper from Andy Strominger in 1986 [44]. It didn't come about in 2003 with Susskind [45]. Andy Strominger got me worried because he found a vast realm of string vacua beyond Calabi-Yau manifolds. I began to worry about how the correct version of string theory would be picked out and I found some inspiration in population biology and how natural selection works. So, I took the idea that the space of parameters of theories in string theory were analogous to fitness landscapes in biology and then that's where the idea of cosmological natural selection came from. I understood, because I understood how biology works, that if you want to get predictions or want to be able to test this idea with access to only one element of the population then that element has to be a typical member of the population and not an atypical member of the population. Therefore, the anthropic principle would never work. The anthropic principle was already on the table back then and it was obvious to me that it would never work. Eternal inflation was also on the table back then since Linde was already talking about the production of universes and it was also obvious that it wouldn't work. Nothing that has happened since has changed my mind about that.

The point that I was interested in was finding out what was necessary in order to have a cosmological scenario where the population of universes is somehow generated dynamically on a landscape that you could make predictions from. The two things that I understood right away from biology were that the distribution would have to be highly out of equilibrium as well as time dependent and that our universe would have to be typical and not atypical such that there would have to be a mechanism that drove every universe to be bio friendly (or all almost every universe to be bio friendly). If you could set up a scenario which was like that then you could make predictions. The cosmological natural selection scenario [46] demonstrates that there are scenarios that follow that criteria which are predictive because it makes real predictions.

What kind of predictions?

For example that the upper mass of a neutron star has to be less than 1.6 solar masses, which may have been recently ruled up by evidence from a two solar masses pulsar. Maybe that's the case, I have not spent sufficient time in communication with astronomers and the astrophysicists to understand how reliable that measurement is but I'm looking forward to possibility of writing a paper saying that it's true.

In this scenario, do the constants of nature change with time?

At the time I came up with this idea, I started out working with the landscape of string theory and I got such flack for when I would give talks and say that we are on the string theory landscape and are evolving. Also, I realized that like in biology, there is a space of phenotypes and a space of genotypes. The space of phenotypes is analogous to the primary space of the standard model - it's the things that are actually measurable in an experiment. The string landscape is the space of genotypes and there is going to be some complicated relation between them. If I wanted to get real predictions, I would have to work directly on the space of phenotypes. So I would have to work like Mendel and Darwin and not like evolutionary geneticists do today.

I see. But even if the model is right or not, do you expect that it's going to be possible to find the theory of everything that exactly predicts which vacua we live in and which are the precise values or the constants of nature?

I hope so but only by taking very seriously what Charles Sanders Peirce wrote that I quoted, that is, the only way of explaining what the laws of nature are is if it's the result of evolution.

Do you consider the scenario of eternal inflation being one such scenario?

It doesn't satisfy the criteria that I mentioned, that our universe is a typical member of the population. Let me mention also another criteria, namely, that the dynamical mechanism that selects the population of universes has to be highly sensitive to low energy physics. We have to be able to explain why the neutron and proton mass are what they are, why the Fermi constant is what it is, why the electron mass is what it is, etc. Therefore, the distribution of the population of universes has to be very sensitive towards low energy parameters. It can't be, therefore, that the population is generated by very high energy dynamics like what happens in eternal inflation. This is not new, it was obvious in 1986, 1987 and nothing has changed since then. There are no predictions coming from the dynamics of eternal inflation except for a very week one, namely, that the spatial curvature must be negative, which is not very falsifiable because it could be arbitrary small. So, 25 years later there is nothing which is any different than it was in 1986 when I started looking at it. There is a lot more people interested in it and there is a lot more intelligence being thrown at what seems to me to be a non-solvable problem. I must also say that I don't believe that the cosmological natural selection scenario is the only cosmological scenario that satisfies all the criteria.

In your opinion, what is the biggest breakthrough what has been the biggest breakthrough in theoretical physics in the past 30 years?

So I'm very convinced by Paul Steinhardt's arguments about the weakness of the claims about inflation but I think inflation as an idea, even if it's not true, it's a great success. I hope it's not true because I would prefer that the explanation for the CMB sky comes from quantum gravity instead, which inflation, if true, would not let that happen since it dilutes the quantum gravity effects. Nevertheless, it's a proper scientific idea, it was developed and it led to expectations (though not precise predictions) and there is evidence for those generic expectations. There is a huge interpretational issue which Paul made clear in his comments in the conference today[1] but even if it's wrong it has been a very successful idea. Everybody involved in it should be given a lot of credit for it.

I also think that the idea that quantum gauge theories are to be understood through studying non-local excitations like loops or strings or branes, which is common to LQG and string theory, is the most influential idea in the theoretical side of theoretical physics. It's also deeply influential in condensed matter physics, in fact, it came from condensed matter physics and it continues to be there, for example, in models and ideas about topological quantum computing.

Concretely about LQG and string theory or other ideas about quantum gravity it is too soon to make any statement. Both ideas are quite old, LQG is now 25 years old while string theory is 40 years old and both are worked on by relatively large communities of scientists. My sense is that things are kind of evening out, that is, the size of string meetings has fallen to something like 257 attendees and LQG meanings have had over 180 attendees in the last couple of years but the fact that there is a large number of people working on an idea doesn't make the idea right.

Do you think that your book "The Trouble with Physics" increased or decreased the distance between these two communities?

I'm not the right person to ask about that.

Why not?

Well, because I cannot be objective about it since I was the author of the book and, secondly, I have many doubts about wether or not I was the wrong person to be the author of that book. The most interesting feedback I got about that book was not from physicists. I did get a lot of positive feedback from physicists, from faculty members all over physics, saying that I nailed something but most of them misunderstood something just like most of the people in the string world who thought that it was a hostile attack misunderstood it and most of them didn't read it. More broadly, I got a lot of feedback from people in the academic world, like in the world of economics and fine arts and so forth. A lot of people got in touch with me to say that they have the same issues in their community whether its biology, neuroscience, artificial intelligence, computer science, linguistics, etc. I have a pile of email and letters where people explicate the whole art or history of their subjects.

The book was principally about the role of controversy and disagreement in high profile research, wherever it is found, and string theory was intended as a case study because it was the case that I knew. Outside of the physics world, I think the book was understood. Inside the physics world I don't want to say that it was completely misunderstood but a lot of people misunderstood it. I also have piles of email from physicists saying that I was so balanced and that I caught the plus and minuses of different things right. I was not very interested in the controversy as it developed on blogs or things like that. It was not very constructive and it was not very reflective of the issues that I was interested in. I kind of jogged out of it, I mean, I haven't appear much on blogs in the last five years. As far as my research goes, I've written several papers on string theory and since then I mostly worked on quantum gravity phenomenology. There was never an issue in our community here at Perimeter Institute. All the string theory faculty and many of the postdocs had copies of the book and we discussed it. Some of them didn't want to discuss it. In any case, it was never an issue here. There were a few friends in the string world who were angry with me and I regretted that that happened. I felt bad about that

[1] This interview took place at the time during the conference Challenges for Early Universe Cosmology at Perimeter Institute.

because I had not intended to anger anybody. I think that I was very careful, I was not ad hominem and I only discussed the ideas and work that I thought were important.

Since 1986 that we started doing LQG and I regret that there is a separate string community and a separate LQG community. I never understood the reason for it because for me they were different ways of exploring the same idea even if I'm more pessimistic now about realizing a joint or unification of them. In my mind they were always the same thing. The idea of expressing gauge degrees of freedom of a gravity theory in terms of Wilson loops was something that I was already working on with Louis Crane before LQG, as a way to try to realise a background independent version of string theory. Then, I tried to apply to general relativity some of the technical things which I've understood with Louis about facing quantum gravity and diffeomorphism invariance using Wilson loops. So, for me it was the same thing and, furthermore, the ideas that went into LQG were the ideas that I brought in from the work of Polyakov, MacDowell and Wilson, which the high-energy community understood and the gravity community had to learn. I came from the high-energy community so the fact that LQG was categorized as an approach originating in the general relativity community and outside of the high-energy community, which is where Carlo and I were trained, always seems to be bizarre to me.

I have just two more questions and that's it. Why have you chosen to do physics why not something else?

Well, there's a story but I've told that story in "The Life of the Cosmos" [41] so let me just refer to that. When I was 17, I was a high school dropout. I wanted to be an architect and I was studying differential geometry. I educated myself on differential geometry because I needed it to do structural calculations on ideas about structures in architecture I was playing with, which were generalized geodesic domes - extending geodesic domes to arbitrary curved surfaces. So I started reading about general relativity because it was in the differential geometry book that I was reading and then I read an essay by Einstein and his autobiographical notes, which appealed to me enormously as an adolescent. This idea that there is a true and beautiful, impersonal, eternal world of beauty and truth out there and one can aspire to become part of it was very appealing to me. That romantic idea made me want to be a physicist. I don't believe that anymore. That's not the notion of truth I have and I'm not a Platonist anymore.

I'm very interested in physics and it's been kind of a mission for me to try to do my part to extend the knowledge of the world but it's a wonderful community to be part of. It is sort of the opposite of what Einstein wrote about, that is, in my case, many of my close friends are in physics and I have the warmest and most meaningful interactions and personal relations inside the community of physics so I love it and I feel very humble to be part of this community.

What do you think is the role of the theoretical physicist in modern society?

The role of a scientist is to make discoveries about nature. I think that human beings find themselves from the earliest times living in several different worlds. We live in a social world, we live in a world of imagination, we live in a mystical world and we live in a natural world. Science is the development of many centuries of disciplines about how to gain knowledge about the natural world and negotiate the natural world. Politics is the development of many centuries of how to negotiate the social world, art is the development of how to negotiate the imaginative world, etc. So I think science is deeply embedded in human culture and our job is to make discoveries about nature.

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