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In your opinion, what are the main puzzles in high-energy physics at the moment?

The main puzzle is to understand the ultraviolet sector of the world and that includes the ultraviolet sector of quantum field theory and also the ultraviolet sector of gravity, since gravity is non-renormalisable.

Does this mean that you need to find a theory that describes quantum gravity?

Not necessarily. According to our current understanding, only non-Abelian theories with fermions seem to have a well-defined ultraviolet completion. The scalar sector of quantum field theories has a Landau pole in general, which is also the case in the standard model. Assuming the theory to be supersymmetric can facilitate finding a completion but we don't know if supersymmetry is a symmetry of nature or not.

Your recent research has been focused on quantum gravity. Why is it so hard to establish a theory of quantum gravity?

Well ...because gravity is non-renormalisable and we don't have a firmly established framework to think about such theories as quantum field theories. It is possible to tackle gravity as a non-perturbative quantum field theory but we don't have many examples of non-perturbative quantum field theories to guide us, which makes it difficult.

Do you think that string theory solves the problem of quantum gravity?

String theory is an attempt to describe a theory of everything but the only problem with it is that, so far, it has made very little contact with the real world. From this point of view, it is hard to call it a *theory of quantum gravity*. The best understood part of string theory is the case in which you take a string moving in 10-dimensional flat spacetime and quantise it. In this situation, you observe that certain vibrations of the string correspond to spin-two particles that you refer to as gravitons. But how do you get from that description to our world? That is unclear, hence you cannot claim that string theory explains why we seemingly live in a four-dimensional world in which we have a theory of gravity (laughs).

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I have nothing against string theory but it is completely unclear whether it has anything to do with the real world or not.

Do you think that, even though it might not have anything to do with real world, it can be used to solve other problems of physics?

It's a very well-known strategy to use a given theory to study other problems than those the theory was meant for. For instance, the theory of epicycles was used to understand the kinematic motion of the planets around the sun besides the motion of anything around the earth, which was its original purpose, and, in fact, the theory of epicycles gives you a perfect valid kinematical description of such systems. From this point of view, string theory can also work as a tool and I have nothing against that.

(laughs) Ok, so you don't believe that it's the right theory, if I can put it this way?

But . . . what do you mean by "It's the right theory" (laughs)?

You could have the feeling that string theory, even though it is not clear right now how to make contact with experiment, is the right theory . . .

I see little experimental evidence that it is the right theory, in fact, I see none and that is the problem. This is the reason why someone has to try to think in alternative ways. However, I'm not saying that string theory couldn't provide, in the end, some explanation of the world but I must say that it seems that no one is interested in pursuing this direction within string theory anymore. Perhaps only a few string theorists work on trying to use string theory to describe the world, as in starting with some action and showing that the world is a consequence of string theory. Most string theorists have branched off and started using string theory like a theory of epicycles, that is, as a tool to describe the kinematics of something else.

I assume that you don't like too much the anthropic principle, which can be used to pick the right vacua of string theory [1]?

Anthropic reasoning doesn't have anything to do with string theory. It was around long before string theory and it's well documented. In principle, you can never falsify it so can you call it *science*? It's not my cup of tea. Clearly, some people cannot figure out how the world is made up and, since they believe that they're so clever, it must be that the world cannot be explained by ordinary science. I'm more modest and I still believe that we have a chance.

One of the criticisms that people often make to string theory is that it is not background-independent while, for example, loop quantum gravity [2, 3] tries to construct a background-independent theory. Do you think that loop quantum gravity is a good candidate for a theory of quantum gravity?

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As far as I know, loop quantum gravity still hasn't made contact with the macroscopic world. From this point of view, it is a little bit of a strange theory, whose proponents claim to know everything at the Planck scale but nothing at the macroscopic scale (laughs). It's up to those pursuing loop quantum gravity to provide a proof that it is a theory of gravity. I haven't fully seen it yet but maybe I'm not too well informed.

Do you think string theory can be formulated in a background-independent way?

The way that string theory is usually formulated assumes the existence of a background metric. From a field theory point of view, you could try to formulate a fully fledged string field theory, which would be background-independent. However, very few people work on that at the moment so it might take some time before it's formulated in that way. However, once that is accomplished, it should be background-independent. Any field theory theory is in principle background-independent since you integrate over all fields contained within the theory.

But why is it important to have a background-independent theory?

I don't understand why you ask this question. When you talk about a *background* you mean the *metric*, right? But string theory is supposed to describe the geometry, which is supposed to evolve dynamically. I mean, look at our real world out here! If you start with string theory in 10-dimensional space, how do you end up in this world right now? Clearly, there has to be a dynamical process in which spin-two 10-dimensional objects condense and have expectation values that describe a completely different universe than the one you originally started with. It doesn't make too much sense to have a theory that depends on the very structures that you originally wanted to describe. Thus, it doesn't make sense to me to have a background-dependent theory. Such a theory would be so ugly that I couldn't imagine it would be the right theory.

Do you think that it is actually possible to have a unified theory of everything or that you have to use different theories to describe different aspects of the world?

The latter possibility makes no sense to me. Of course, it's acceptable that something is best described in one language than in another language but the overall description should be unified in only one theory. You can maybe use different mathematical representations of the same theory to approach different problems but that's a different story.

Do you think that we will be able to see any quantum gravity effects in the near future, for instance, at the LHC?

No. I think that there have been ridiculous claims made by certain people that this could be possible.

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So if you cannot really test such theories, what are they useful for?

Well . . . it's not really ruled out that they can't be tested but I think that any test has to come from cosmological data in one way or another. It is hard to test it at any other scale, in particular by scattering particles. Such cumulative processes could in the end add up to something significant but it would require some very detailed calculations to show that it would be indeed the case. Such scenarios have been supported by some people in a not so far past but I think that it is still preliminary. At the moment, it seems that the best hope of constructing and testing a consistent theory is to apply the theory to our current universe. As such, I do not see such theory as providing us with multiple predictions for the future in such a way that we can live long enough to verify them (laughs).

Since it is hard to test these theories, would it be reasonable for someone to stand up and say, "This is not really science but religion" because it's a matter of belief in a specific theory of quantum gravity?

Clearly, science might come to a limit in which this will be the case because in order to construct an accurate theory of the universe you need to conduct experiments. However, you don't really have the possibility of making many experiments since you just have this one universe. From this point of view, you might have to establish different kinds of standards for what should be called *science* and *scientific predictions*. At the moment, I don't consider this to be a problem. On the other hand, I must say that you will probably always be able to wave your hands in the air and use something like the anthropic principle to explain the workings of the universe. I tend not to refer to that approach as *science*.

Can you tell me what is the story behind the approach to quantum gravity that you pursue? How did it begin?

The approach that I pursue was developed by myself and collaborators, as well as other groups, in the mid-1980s in order to provide a non-perturbative regularisation of Polyakov's string theory [4–11] and it has been extremely successful in describing two-dimensional quantum gravity and non-critical string theory. In the beginning of the 1990s, it became clear that string theory was not going anywhere and it also became unclear whether it would be able to make any predictions. At that point, one had to think whether or not there could be other descriptions of quantum gravity. The generalisation of our approach to higher dimensions, which is related to what is referred to as *asymptotic safety* [12], was possible and could potentially lead to an alternative theory. Thus, we began pursuing it.

What is asymptotic safety?

Asymptotic safety is a kind of generalisation of Wilsonian standard renormalisation group philosophy according to which a quantum theory needs not to be renormalisable in the ordinary sense [12]. By a theory being *renormalisable* one usually means that it has a Gaussian

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fixed point and hence a small coupling constant but, in principle, one could have a non-trivial fixed point for which this coupling constant is not small. The Wilsonian approach was exactly developed to deal with such situations, for example, the three-dimensional Wilson-Fischer fixed point governs all behaviour of second-order phase transitions in the real world. However, this is an infrared fixed point, contrary to what is needed in gravity, which is an ultraviolet fixed point. Nevertheless, it is possible that it could also apply to ultraviolet fixed points, although it is not as natural as infrared fixed points from the Wilsonian point of view. Taking this as the starting point, it is then natural to have the right lattice formulation to study these fixed points and that's what we have developed.

Okay, but what kind of theory is this? A theory of the universe, a theory of particles, matter, gravity and interactions?

Well . . . it's a theory of quantum geometry and it's a theory of the universe, for sure.

You called this theory *causal dynamical triangulations* (CDT) [13, 14] . . .

CDT is the end result of various attempts of finding a theory that seems to have an interesting phase structure and in which you can properly define a continuum limit. The word *causal* in CDT is due to the fact that we have incorporated a kind of global time in the approach. In more detail, the word *causal* originates from the fact that we triangulate the universe assuming the existence of a proper time which, when cut into small pieces, gives rise to a connected space.

But what are these triangulations or building blocks (if they can be called that) that you assemble together?

They are just what makes up the lattice and have no physical meaning. It's like in any quantum mechanics course you have had when you were an undergraduate. When you perform the path integral you're chopping up your time interval into ϵ pieces. Here it is exactly the same but you have to be a little bit smarter because you are now dealing with geometries and not only with time. However, in the end, you're just chopping up these geometries.

But I thought that these triangulations, which you refer to as *four-simplices*, need to obey certain rules when you assemble them together . . .

Suppose that you have some manifold with a certain topology which you keep fixed, say $\mathbb{R} \times \Sigma$. As in any attempt to quantise gravity canonically, you assume that you have the real line $\mathbb{R}$, which is the topology of the time part of the manifold, and you have a spatial manifold Σ, which is a space with a certain topology. When you glue the building blocks together to create the corresponding geometry you have to do it in such a way that respects this topology that you keep fixed. This requirement implies certain rules for how to assemble the four-simplices.

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You have said a few times that the manifold has to be fixed but shouldn't it be dynamical in a full theory of quantum gravity?

Not necessarily. Our approach consists in assuming vacuum canonical quantum gravity where the manifold structure is fixed. Of course, you can have any geometry on that manifold. This is exactly like when you solve Einstein equations for which, usually, the manifold is fixed and you try to find the geometry.

But the fact that you have this time foliation in $\mathbb{R} \times \Sigma$, doesn't it make a background-dependent theory? Clearly, you don't have time and space on an equal footing . . .

But time and space are not on an equal footing in any theory of relativity or in any Lorentzian formulation. You always assume to have a certain signature. Even in string theory a certain metric signature is assumed, which should actually be determined dynamically but this is never discussed within string theory. Why is time being inserted in the theory? Similarly, the starting point of our approach also introduces time.

Shouldn't time emerge? Shouldn't the Lorentzian signature emerge from a fundamental theory?

Well . . . now you are demanding more than what you demand from string theory. This is not how we developed our approach. We assume that we have a Lorentzian signature as the starting point.

Is CDT diffeomorphism-invariant?

It is, of course, diffeomorphism-invariant. Diffeomorphism invariance has nothing to do with this discussion. At any point in the process, we are always dealing with geometries. I'm not sure if you understand this difference so let me explain. Our approach only deals with geometries, that is, we never use coordinates to describe the system. Diffeomorphism invariance is something that you really don't want to deal with. It is forced upon you in the usual description in which you use coordinates but in the end you want the description to be independent of the coordinates. The diffeomorphism class of all coordinate transformations yields the geometry-invariant so if you work with the geometry from the get-go, everything is diffeomorphism-invariant. The path integral that defines the theory only sums over geometries, which means that there is no need for gauge fixing of any sort.

You are summing over all geometries but at some point you evolve your system and you get a single de Sitter universe [15]. Why do you get just a single geometry in the end?

If you look at our world and assume that it is described by a quantum theory then you are summing over all geometries and nevertheless you have only one geometry here right now. So what is this geometry? It is the expectation value of the sum over all geometries and

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around that value there are small quantum fluctuations. This is what we mean when we claim that we have a de Sitter universe. When we look at the theory non-perturbatively, the expectation value of the sum over all of these geometries is, seemingly, a de Sitter universe [15].

And this universe also turns out to be four-dimensional? Is the spacetime dimension a parameter that you put in?

We put in four-dimensional building blocks.

So could it happen that you would get more or less dimensions than four in the end?

That depends on what you mean by *dimension*. Look, you're putting nothing in except the requirement that the sum is over all possible four geometries. Of course, these are just configurations in a path integral, which is how you implement the theory, and none of these configurations are physical, in the same way that the path of a particle is not physical in quantum mechanics. It doesn't make any sense in a quantum theory to talk about the path in the same way that it does not make sense to talk about the geometry, as it is not a quantum object. In other words, you have to define physical observables and those are the ones that you can talk about. In the case of quantum mechanics, one of the observables is the expectation value of the sum over all paths satisfying some boundary conditions and, analogously, the sum over all geometries in the case of the universe. After you sum over all geometries you can ask, "Is there a sensible average geometry around which there are well defined quantum fluctuations?" and "Are these quantum fluctuations determined in terms of the coupling constants in the theory?" That leads you to a Euclidean de Sitter universe [15], since we actually have to rotate everything to Euclidean space in order to perform computer simulations.

Can you then rotate back?

In principle you can rotate back but in practice *rotating back* is somewhat of an unclear statement. We can rotate back in a certain way but that requires a bit of a technical explanation.

But if you could not express your results in Lorentzian signature, would you still claim that the theory is sensible?

It's still sensible. It's exactly the same situation that you have in ordinary quantum field theory when you perform computer simulations. All these computer simulations are done in Euclidean quantum field theory. We then need to do some work to understand what these results correspond to in Lorentzian quantum field theory. For example, there are general axioms stating that correlation functions in the Euclidean sector can be rotated back to certain correlations functions in Minkowski space. In our setup, the issue has to do with what can be rotated back in the case of quantum gravity. That's a complicated discussion but in principle observables can be rotated back to the Lorentzian context.

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So do you take as a prediction of the theory that the world we live in is a de Sitter universe?

We would like to make that claim. We have a fixed cosmological constant and . . .

Does the theory have anything to say about the value of the cosmological constant?

No, that's a free parameter in the theory.

So couldn't there be a more fundamental theory that fixes the value of the cosmological constant?

Okay, so now you're asking if there is a more fundamental theory that fixes the value of the electric charge and so on, right? That's not our theory. Our theory is exactly like standard quantum field theory so it doesn't fix any of the renormalised coupling constants that enter it. Ordinary quantum field theory doesn't fix the mass, the charge, etc.

So would you say that CDT is a kind of formalism in which you come up with a model and experimentally adjust the constants?

That's what it is all about.

Does the starting point contain any matter?

In principle, you could introduce any kind of matter in it but you first have to define some observables. Since the analysis of CDT is performed numerically by means of Monte Carlo simulations, you have to first understand what are the well-defined physical observables that the simulations should measure. In this case, we are looking at a universe and if you put it in into a computer it corresponds to a 10^4 lattice. So, clearly, we are talking about something which is slightly larger than the Planck scale. In fact, we can calculate the Planck scale in terms of lattice units which yields a result of the order of 20 Planck lengths.

That's the total size of the universe?

That's the size of the universe we can fit into a computer.

Is that big or small?

What's your own verdict?

I think it's quite small (laughs) . . .

Yeah . . . so before you are carried away and start studying how matter propagates in CDT, you should think about what it means to have matter coupled to a universe which is of the size of 20 Planck lengths.

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Would you like to simulate a universe with a much bigger size?

It would be nice to simulate a much bigger universe but there’s a limit to what you can do with this numerical approach because it has to fit into a computer and the simulation has to run in a finite amount of time.

Can you do anything besides numerical simulations?

Not in four dimensions at the moment.

Okay, so the claim is that according to the numerical simulations, a universe of such a small size is de Sitter?

Even though it is so small, the end result of the simulation is surprisingly well described by the assumptions of isotropy and homogeneity. What you actually observe is a universe which fits these symmetry assumptions that you started with, namely, isotropy and homogeneity, which is basically de Sitter spacetime [13, 14].

Would I get a de Sitter space if the universe was just one Planck length in size?

There is an interplay between the coupling constants and the size of the universe in the following way. For a universe of a given size there are quantum fluctuations around it and we encounter exactly the same phenomenon as in quantum mechanics, that is, if you are looking at sizes which are comparable to the coupling constants in units of $\hbar$, then quantum fluctuations dominate, making it senseless to claim anything at all. However, if the size is large compared to the coupling constants, then it makes sense to talk about expectation values around which there are small quantum fluctuations. In this latter case, what we observe in the simulations are such expectation values as a function of the volume of the universe until we run out of computer memory and time.

But now going back to the issue of the dimensionality of the universe . . . I have read in one of your papers that the universe is four-dimensional at large distances and two-dimensional at small distances [16]. How can that be?

In a quantum universe you do not have a standard quantity to which you can call *dimension*. You have to define what you mean by *dimension*. If you choose a nice four-dimensional metric and expand around it, then of course your space is four-dimensional. But if you have a quantum theory where you are summing over all geometries, even if these geometries are kind of four-dimensional, the result will not be necessarily four-dimensional.

Let’s imagine the following thought experiment. Suppose that you did not have an action, such that every geometry is equally represented in the path integral. Then, if you sum over all these geometries, will you obtain something which is four-dimensional? The answer depends entirely on the entropy (number of geometries of the same type) of a certain type of geometry. Let’s assume that there are many geometries which look like a very thin string and let’s assume that when you make the string longer and thinner the number of such

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geometries increases. If that's the case, in the end, the effective one-dimensional structures will dominate the path integral. Even if all the geometries you started out with were kind of four-dimensional, it might be that if you introduce a length cutoff the geometry that dominates the path integral will be small one-dimensional strings. In this situation, the other higher dimensional structures are *behind the cutoff*.

In other words, I'm reiterating what string theory likes us to believe about its six extra dimensions, that is, that such extra dimensions are small. In principle, in any theory where you sum over geometries, this kind of situation may well happen and it does happen [17]. If you start out with four-dimensional building blocks and glue them together, in the end, when you take the scaling limit where the size of the individual blocks shrinks to zero, there is no a priori reason to expect that the end product has anything to do with the four-dimensional world.

But you did get a four-dimensional world at large scales, right?

Yes . . . but that's an indication that something is right about the approach that we are taking. Describing a four-dimensional world is a major challenge for a background-independent theory.

But what about the claim that the universe is two-dimensional at small scales [16]?

The structure at small and large distances of a typical geometry, which is generated by some action, does not necessarily agree. At large distances it may effectively look like a four-dimensional world and at small distances it could look completely different, with some fractal structure and so on. At small distances you can encounter ultraviolet divergences which would lead to wild quantum fluctuations so you cannot guide yourself using classical intuition. The way to proceed is to define *dimensionality* in a specific way. For instance, you can define the spectral dimension, measured by studying a non-dynamical diffusion process in these geometries. In this context, you can study the dimensionality of the geometry as a function of diffusion time. This allows you to verify that at large distances you have an effective four-dimensional geometry and at small distances you have a lower dimensionality. I do not claim that it is exactly two-dimensional at small distances because it is a purely numerical study but it is around two dimensions, plus or minus something. This indicates that there is a gradual change in the dimensionality of the geometry from large to small distances.

This kind of phenomenon one could also observe in Hořava-Lifshitz gravity [18, 19], right?

Yes, but in any theory where you have an ultraviolet fixed point you will observe such an effect. What does it mean to have an ultraviolet fixed point? It means that the dimensionless coupling constant approaches a constant value but since in four dimensions the gravitational coupling constant has dimension two, this implies that as you approach the fixed point two dimensions will sort of disappear in such a way that you effectively start with four

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dimensions and end with two dimensions. From this point of view, it is a good sign that one does observe it. It is not a proof that this ultraviolet fixed point exists but it is not in contradiction with it. In fact, people who are studying renormalisation group flows with such an ultraviolet fixed point do observe the same effect [20]. So, it's not only in Petr Hořava's gravity (Hořava-Lifshitz gravity) that you observe it [18, 19], as it is a generic effect.

Do these two approaches – CDT and Hořava-Lifshitz gravity – have more in common than just this flow from two to four dimensions?

I think they could be quite related.

How could they be related?

Since we treat time and space differently in CDT, our regularised theory could potentially be creating an asymmetry between space and time. When approaching these non-perturbative fixed points, we don't know exactly what to expect. We have explicitly put in an asymmetry between space and time, in the sense that we have a time foliation, so it is possible that the underlying theory will develop dynamically such an asymmetry. At first, we did not think that this could be the case since there are very few fixed points and we expected to find isotropy. This is what usually happens in an ordinary lattice theory, where you have no problem in recovering the continuum limit in the end, which is rotationally invariant. However, this is not necessarily the case.

From this point of view, I think that there can be similarities between the two approaches. In fact, in a certain sense, some of our motivations are quite similar to those that led to Hořava-Lifshitz gravity. In particular, we were interested in a lattice formulation that is as close as possible to canonical quantisation. If you do have a canonically quantised theory, then you expect the theory to be a unitary theory. Within the context of CDT, there is a type of transfer matrix [21], and the existence of a transfer matrix usually implies the existence of unitary time evolution. To devise a unitary theory, which is also ultraviolet finite, was one of the main motivations of Hořava. He concluded that one should add some higher derivative terms in the spatial directions but in the time direction one keeps a second-order time evolution in such a way as to have a unitary theory.

You mentioned earlier that certain geometries have different entropies associated with it . . .

That's a trivial statement. Suppose you have an action and this action only depends on the geometry, for example, Einstein's action depends on the integral of the curvature. There are many geometries which have the same on-shell value of the action. In the path integral, what enters is the number of geometries for which the action has a specific value. So in this way, the entropy – the number of such geometries – of a specific class of geometries enters in the path integral. This entropy of configurations for a given value of the action can play an important role in determining what is the dominant configuration in the path integral.

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In one of your papers you state that CDT is an entropic theory of quantum gravity [22]. Is it in any way related to Erik Verlinde's idea of gravity as an entropic force [23]?

No, I would say it is anti-related to it. First of all, this is a quantum theory or, to be more precise, an attempt at formulating a quantum theory since we don't know if the continuum theory exists, while Verlinde's theory is some kind of classical theory. When we discuss the notion of *entropy* in our formalism it is really genuine *entropy*, in the sense of the number of configurations. In fact, in our formalism – and I think this is quite beautiful – this actually singles out, in some way, gravity compared to all other forces.

The partition function of quantum gravity is basically the generating function of the number of geometries. This can be made very precise [13, 14]. In this way, the theory of quantum gravity is entirely given by the counting of geometries. In fact, you can use these piecewise linear manifolds (or four simplicies) to decompose Einstein's action in such a way that you make apparent that you are just counting different four-dimensional triangulations. Therefore, if you could count them, exactly you would have solved quantum gravity completely. However, it is difficult to count them, unfortunately, except in two dimensions. This is the reason why non-critical string theory could be solved analytically, that is, in this context you can count the number of geometries explicitly and analytically.

Do you expect that you will be able to use CDT for other things like computing scattering amplitudes at high energies?

No. That's completely uninteresting I think. That's not what CDT is made for.

Why do you say it is not interesting?

I don't know what you precisely mean by *computing scattering amplitudes*. Usually we use quantum field theory to compute scattering amplitudes at high energies where you have an S-matrix and so on. What exactly do you have in mind? I don't know why you would like to include gravitons in this setup. I still have to remind you that the size of the numerically simulated universe is 20 Planck lengths and, in addition, in order to have an S-matrix you need to have flat spacetime, asymptotic states, etc. In summary, CDT is not the right framework for this.

At some point you write down the wave function of the universe [13, 14]. What does it mean to have a wave function of the universe? Is there a multiverse at play in the context of CDT?

That's a good question. There is no multiverse in our approach. We are not doing anything else than in any kind of mini superspace attempt to quantise gravity. You can always use the Wheeler-DeWitt equations, or the Hamiltonian if you have one, to state that the ground state is the wave function of the universe. The way people define the wave function and apply it to a macroscopic system like the universe is, of course, unclear and we do not improve such issues.

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The wave function of the universe is defined in Euclidean space by fixing the geometry at the boundary. Then, you use whatever quantum theory you have to sum over all possible geometries and this sum is what you call the *wave function of the universe*. This, we can clearly calculate and that's what our formalism does. The information that the wave function has is the following. If you ask some question in this universe then you can use the wave function to say that, with a certain probability, this or that observable will take a certain expectation value. The wave function contains this information because, in some sense, it accounts for the probability associated to a given spatial surface compared to another spatial surface.

The true meaning of the wave function of the universe is complicated and no one fully understands it. Many people have tried to shed some light on this matter. In fact, there's a huge literature about mini superspace (which are basically just quantum mechanical models) calculations involving this wave function.

I read in one of your papers that the final picture that emerged from your simulation is that of a universe created by tunnelling from nothing [24]. What does *tunnelling from nothing* mean?

This is a standard process in quantum cosmology. For example, people like Stephen Hawking and Vilenkin have discussed it in detail [25, 26]. Given the wave function of the universe, which you usually calculate in Euclidean signature, you can rotate it back to Lorentzian signature and obtain a wave function in Minkowski space. This Lorentzian wave function typically looks like a potential that starts out with zero four-dimensional volume but also contains a barrier which, once you cross it, will roll down and the four volume will expand. In this sense, there's a kind of tunnelling from zero four volume to a real and finite four volume – this is what *tunnelling from nothing* means.

What the precise interpretation of this is, is difficult since one is dealing with the universe. In particular, Vilenkin has been arguing that the interpretation is that in the beginning the universe has zero volume and quantum mechanically it can tunnel to a situation with a finite macroscopic volume [26]. In simple models you can even calculate the amplitude for such process. Our model is not in disagreement with this.

Is CDT at a point of delivering experimental predictions?

Of course not. CDT is a theory of a quantum universe for which not much is yet known and, additionally, without incorporating the effect of matter in detail it is hard to make any predictions. The problem of dealing with matter, from the point of view of cosmology, is that it is difficult to understand the real time evolution of the system, given that we primarily work with Euclidean time. This is the usual issue that one encounters in ordinary quantum field theory, namely, issues of rotating from Euclidean to Minkowski signature and issues of obtaining the real time evolution. These are of increased difficulty when working with only numerical data. So, unfortunately, you have to think carefully about these problems if you want to make intelligent statements about the coupling to matter. At the moment, I don't have too many intelligent things to say about it.

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Do you expect that at some point you will be able to study black hole formation for example?

Black hole formation is usually associated with matter and you have to remember that, in a universe that has the size of 20 Planck lengths, black holes are not the most relevant objects to discuss. However, in principle it should be possible to study them. In this case, we do indeed face the problem of rotation from Euclidean to Lorentzian signature. Black holes hardly exist in Euclidean signature and they are usually associated with topology change. At the moment, we have kept the topology fixed but of course you could have chosen a topology that corresponds to a black hole. However, if you want to study a dynamical process, it should be understood how to do the rotation properly and our main emphasis at the moment is to understand whether there is a continuous theory or not. CDT is a lattice theory and before worrying about all kinds of subtleties we want to understand if, at all, there is a continuum theory.

Why is it difficult to understand whether there is such a limit when taking the lattice spacing to zero?

Maybe the best analogy is that of a spin system on a lattice, in which the spins can be up or down. In this case you ask the analogous question "Is there an underlying continuum field theory associated with such a spin system?" The answer is not obvious at all. Clearly, you have to find a region where there are long-range correlations between these spins otherwise there is no notion of continuum. Additionally, you want to remove the dependence on the lattice spacing because you introduced the lattice to regularise the theory. As such, some of the parameters in the theory need to be large compared to the lattice spacing because otherwise you do not recover continuum physics. This usually implies that you need to find a sort of phase transition where you observe long-range correlations. Once you found that phase transition, you have to study the limit corresponding to a continuum field theory. In summary, there are a number of steps to be taken in order to find this limit, even in an ordinary kind of system, and it is not easy to establish its existence, specially if you do not know in advance that there must be one.

How do you plan on trying to establish this limit?

In order to find the limit one has to identify transition lines in the phase diagram, where the geometry undergoes a transition. However, first of all, you have to understand how to think about these phase transitions in a coordinate-independent way, which is something that people don't usually think about because they are not forced to think about it.

Suppose that you have a scalar field $\phi(x)$. Then you can talk about the correlation between $\phi(x)$ and $\phi(y)$. If you have a theory involving quantum gravity, it makes no sense to talk about the correlator between $\phi(x)$ and $\phi(y)$ because x and y are just coordinates and the theory sums over all metrics. In this context, there is no fixed notion of distance between x and y and hence the correlation length as a function of $x - y$ does not have meaning. This implies that one needs to define invariants in a different way.

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Conceptually, the study of invariants is very interesting but there isn't such a great deal of literature on how to formulate these invariants in a non-perturbative quantum gravity setting. In fact, it is interesting to know that Albert Einstein was kind of disgusted with the name *theory of relativity*, which was given to the theory that he developed. He thought it was a completely misleading denomination and he actually wanted to call it *theory of invariants* because what defines the theory are quantities which are invariant under coordinate transformations. Thus, in a sense, it's stupid to refer to Einstein's theory as *theory of relativity* because it's not about what is relative but about what's invariant. In the context of CDT, you are forced to think about not only how to define objects which are invariant under coordinate transformations but also, since you are integrating over all geometries, how to define objects which are invariant under geometry.

In order to summarise this conversation, what do you think are the lessons about quantum gravity that came out from CDT and what do you think were the main advances in the search for quantum gravity in the past, say, 30 years? Do you think we've reached some understanding of it?

No, I don't think so. I think that there are no lessons in the sense that we still don't have a theory that we could call a *theory of quantum gravity*. CDT is a formalism that is trying to define quantum gravity non-perturbatively but we haven't proven that there is a continuum limit yet. As with all other theories of quantum gravity, little has been achieved because we still don't know what is the right theory. In my opinion, we still don't have a natural quantisation procedure. We know, from quantum field theory, that this is not going to be easy to show because it cannot be an ordinary Gaussian fixed point.

However, I would like to say that while working with CDT, it became clear that statements like *geometry is emergent*, or *geometry has to go away at the Planck scale* and so on are naturally encoded in any attempt of defining the theory non-perturbatively via pure path integral formulations. These desirable properties are there because, first of all, from the very beginning you don't have anything. You are summing over all geometries but these by themselves are not observables in the theory like the path of a particle is not an observable, even though you still sum over it in the path integral of the particle. In the same way, geometry is not an observable in quantum gravity so the best you can hope for is the emergence of a kind of classical limit where the quantum fluctuations are small. If you approach smaller and smaller distances then you expect that quantum fluctuations are so large that it makes no sense to talk about geometry. In this sense, geometry kind of resolves itself, which naturally happens at the Planck scale where the quantum fluctuations are large no matter what kind of theory you imagine to be dealing with.

All these words/statements have been used within the context of string theory and I think that they have some validity no matter what framework you are using. What is nice about trying to follow our approach is that there are a lot of concrete questions such as "What are the observables?" These are the sort of problems that are not dealt with too much within string theory. When you look at the details of string theory, you can feel uneasy because it only makes sense on-shell and you have an S-matrix which you don't

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know exactly where it lives, etc. Usually string theory replies to difficult questions with the answer “The beauty of the theory will clarify this for us.” Well that might be (laughs) but at least CDT provides a very concrete framework in which you can address these questions.

I don’t think that we have a theory of quantum gravity and I don’t think that we have learnt too much about it. Honestly, in a way it’s difficult to understand if finding the fixed points of the theory will give you the final answer. In a certain way, I hope not (laughs) but I think we have an obligation to look at it and since so many people in the world are working on something else, I think that some of us should work on this direction, which also has its own beauty.

One often hears that string theory *eats* everything like quantum field theory, fluid mechanics, etc. Do you think that at some point string theory will incorporate CDT?

No, I don’t think that string theory *eats* everything. I think that this idea that string theory *eats* everything started with some drawings by Dijkgraaf at one of the lattice conferences and in a way I think it is not really what has happened. Instead, what has happened is the following. People who pursued theoretical physics in the 1970s, in a theoretical avant-garde sense like those working on QCD, were running out of things to explore. There was a lack of experimental evidence for guiding how to proceed in the field theory direction and icons like Alexander Polyakov, and later Edward Witten, told us that we should think about string theory. As such, in the 1980s string theory was a very promising candidate for the theory of everything, so it was an extremely exciting research direction that motivated many people to work on it and be part of a big discovery. However, today we still have QCD and we still have grand unified theories but we don’t know how to progress from a field theoretical basis. Hence, string theory hasn’t *eaten* anything because the theoretical development in field theory basically stopped there.

Loop quantum gravity is also a counter-example to the statement that string theory has *eaten* everything. In fact, loop quantum gravity has become a larger community, regardless of that being a good or a bad thing. In the context of quantum field theory, there is also a pretty stable community in terms of size that studies lattice gauge theories. This community hasn’t been *eaten* by string theory either.

What can I say about the future of string theory? I don’t know. I have no strong verdict about it but it is true that there is now a huge mass of people working on it. This fact was also one of the motivations behind the book of Lee Smolin [27], though I think that the book also expressed Lee’s own frustrations, which I can kind of understand to some extent. It is a real danger if string theory becomes a self-sustained community decoupled from real physics, so to speak. In a way, this problem is inherent not only to string theory but to theoretical high-energy physics as whole. If theoretical physics doesn’t produce anything interesting in the future, it will be cut away because there are many other fields which will want to grab the funding away.

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Do you think that the lack of contact with the real world is one of the reasons why the number of jobs in theoretical high-energy physics is decreasing?

I think that it will be difficult to continue to argue that so many people should be working on something that is not necessarily leading anywhere. It might be interesting to pursue these directions for the sake of discovering new mathematical structures or exploring weird physical theories but that would lead to a much harder fight for funding and, in the end, would make it difficult for us to defend our position.

Historically, there have always been many more experimental physicists than theoreticians. In particular, there have almost always been more solid state physicists than high-energy physicists. High-energy physicists have had an amazing success in the twentieth century in creating and developing general relativity and quantum mechanics as well as formulating the standard model. However, after these developments, the success of high-energy physicists has stopped. In the last 30 years there have been no new important discoveries, nothing in fact, and if you wait 30 more years and there are still no new discoveries related to the real world then I think that our community will shrink in a significant way. Perhaps if that happens it is only fair (laughs) since if we have nothing to offer then our existence is useless from that point of view.

There will still be room for theoretically minded people but perhaps only a small number, which I don't think will stop progress. If there is a possibility for making progress some bright people will still be able to do it. Most progress, anyway, is made by a few bright people and not necessarily the ones we think about as being the brightest. In a large community, as high-energy physics is now – and here I'm getting a little bit in line with Lee Smolin though I don't agree with many other things that he claims – there is a heated environment of global discussion and fashionable topics, which might actually not be beneficial for what you might call *deep thinking* about which direction to pursue and how to make progress. Whether *deep thinking* leads to any progress is a complicated discussion because you can find examples where progress is made by people developing random ideas and trying out everything they can as well as examples where it is made by *deep thinkers*. Nevertheless, it is true that the milestones in our field were set by only a few people and some of them have basically existed in a kind of vacuum bubble (laughs). Albert Einstein and Paul Dirac, for example, were almost in their own world, so to speak, in order to make progress. Other people interacted a lot with the physics community. Luckily there are no fixed rules for how progress takes place.

The LHC could perhaps change all of this but it is not clear at the present time. People working in our field, at the moment, will be able to defend their motivations for one more generation but afterwards society will be feeling fatigued, due to our inability to deliver, and other fields will take the funding (laughs). Nuclear physics, for example, dominated the funding in the 1950s, 1960s and even in the 1970s it was quite important theoretically but then it died out. At that time, you could still claim that the study of very special elements enriched with neutrons was important for understanding certain experiments but this line of research became more esoteric as time passed, placing nuclear physicists in a position that could no longer be defended.

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Did this happen because nuclear physicists solved all the relevant problems they were meant to solve?

To some extent yes but also what they were claiming to be new problems didn't really *click* with the rest of the world. In the same way, it might not *click* with the rest of the world to discuss different aspects of Calabi-Yau compactifications and so on (laughs). There are many people now working on Calabi-Yau compactifications and they can defend its importance for some time but in the end I think that they will run into similar problems as the nuclear physicists did.

However, if supersymmetry is found LHC it will change everything. If that would be the case, I would basically give up studying my own theory of quantum gravity because extremely strong methods that deal with supersymmetry are naturally incorporated in string theory. It is possible to study supersymmetric quantum field theories just by themselves but it all originated from string theory. It's natural to implement supersymmetry in string theory, though one can discuss how natural it is (laughs), and in a way, it is needed to make sense of string theory while it is not, strictly speaking, needed to make sense of other theories.

I am a bit skeptical about supersymmetry being discovered at the LHC but it is a possibility which, at least for some years, one can still wait with excitement to see if it actually happens. Personally, I hope that it will happen since it would be a fantastic prediction of theoretical physics, as one is led to it by various arguments. It's surely a mathematical possibility though not all mathematical possibilities are realised in nature unless you are an extreme believer in the anthropic principle (laughs). We can only hope that the experimental physicists know what they are doing (laughs).

You mentioned earlier that no important discovery was made in the last 30 years. Is there something you consider a breakthrough in theoretical physics in the past 30 years?

I haven't seen any.

As simple as that (laughs) ?

Yeah, I don't think there has been any.

So you think that after the standard model was formulated not much has happened (laughs)?

Well . . . if you think about contributions related to the real world there has been nothing. Come on! It's obvious.

That's a bit pessimistic, no (laughs)?

I don't think you can call string theory a massive scientific breakthrough of any kind because it's not necessarily related to the world. So in terms of physics . . . nothing. Come on! It's completely trivial.

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Why did you choose to do physics? Why not fishing?

I don’t like fishing. Physics and astronomy have always been fascinating to me. I think that many of us entered this field because we were interested in fundamental questions about the universe. I’m very privileged that I am just sitting here and studying the universe using a computer. When I was 13 or 14 years old, I was reading popular books about Einstein’s relativity and I was deeply frustrated with them because I couldn’t really understand what they were trying to convey. The books were trying to convey important concepts (like curved space, time travelling, etc.) without explaining precisely what they meant (laughs). All this was, of course, deeply fascinating and I think many people have been dragged into theoretical physics due to the excitement provided by the science fiction version of it. I’m definitely one of them (laughs).

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

I’m obviously biased but I think that theoretical physics is here to stay as a fundamental pillar in our society to what concerns the understanding of nature. No matter whether or not we make any progress beyond what has been accomplished so far, theoretical physics is here to stay. It is an exact way of describing the world and the way you teach physics to others is via theoretical physics. It is the fastest way to pass this information to others. I personally view it as one of the *unbelievable* achievements of mankind, in particular, the fact that we are able to describe nature in abstract mathematical terms and in a very precise way. Take quantum mechanics as an example. I view quantum mechanics as mankind’s greatest intellectual achievement. It is so far from any intuition and still it is unbelievably precise, in fact, so precise that we have not seen any glimpse of its failure, though it might fail at some scale. Any experiment performed so far has confirmed quantum mechanics. In summary, I think this kind of knowledge will always have to pass from generation to generation even if we don’t make further progress.

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