The quest for quantum gravity

Gary K. Au

One of the greatest challenges facing theoretical physics lies in reconciling Einstein's classical theory of gravity – general relativity – with quantum field theory. Although both theories have been experimentally supported in their respective regimes, they seem mutually incompatible. This article summarizes the current status of the superstring approach to the problem, the status of the Ashtekar programme, and addresses the problem of time in quantum gravity. It contains interviews with Abhay Ashtekar, Chris Isham and Edward Witten.

'It is very important that we do not all follow the same fashion ... It's necessary to increase the amount of variety ... and the only way to do this is to implore you few guys to take a risk with your lives that you will not be heard of again, and go off in the wild blue yonder to see if you can figure it out.'

Richard Feynman (1965), Nobel prize in physics award address

The road to quantum gravity (QG) has had a long and winding history. Although we are unlikely to see the day of a direct test of quantum gravity, such a theory is needed to describe the early stages of the Big Bang. Around this time the universe would be about the size of the Planck volume. Quantum gravity effects, ignored in particle physics because of their weakness, would have a major influence on the subsequent evolution of the universe. Also a QG theory is desperately needed to make the standard model a consistent physical framework.

Why are we spending so much time on such a lofty goal? Richard Feynman commented on the aims of science: 'If you expect science to give you all the answers to the wonderful questions about what we are, where we are going, and what the meaning of the universe is, then I think you could easily become disillusioned and look for a mystic answer to these problems.'

'The way I think of what we're doing is, we're exploring – we're trying to find out as much as we can about the world. People say to me, "Are you looking for the ultimate laws of physics?" No, I'm not. If it turns out there is a simple, ultimate law which explains everything, so be it; that would be very nice to discover. If it turns out it's like an onion, with millions of layers, and we're sick and tired of looking at the layers, then that's the way it is. But whatever way it comes out, it's nature, and she's going to come out the way she is! Therefore, when we go to investigate it we shouldn't predecide what it is we're going to find, except to try and find out more.' [*No Ordinary Genius – The Illustrated Richard Feynman*, Christopher Sykes, W. W. Norton, New York, 1994, p. 251]

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The problem of time[1–5]

A key question which faces any approach to QG is the 'problem of time': how does a time variable, with its special properties in relation to the probability interpretation of quantum mechanics (QM), emerge? A related topic which has been attracting increasing attention, especially in the context of 'quantum cosmology' is the 'consistent-histories' approach to quantum theory. One of the most exciting features of the programme is the possibility of changing quantum theory itself in the context of the physics of the early Universe and the way in which it came into being.

The basic problem lies in the incompatible way in which the concept of time is treated in QM and general relativity (GR). One of the main researchers studying this deep problem is Chris Isham at Imperial College, London.

Chris Isham

In QM time evolution is described by the Schrödinger equation. However, the time parameter that comes into the equation is a background parameter. This is unlike other quantities which can be represented by Hermitian operators. So from the very beginning QM attaches a special significance to the idea of time. It is like the steady ticking of an external clock hand in the background, which we have no control over. Can we physically measure this quantity? Is it possible to reformulate QM so that the idea of a background time does not appear? According to Chris: 'It is possible to do this in a formal sort of way but it does not really get you far. The basic fact is that conventional quantum theory presupposes an external time whose ontological (see note 1) status is the same as it possesses in classical Newtonian physics. In relativistic quantum theories this is generalised to become a background Minkowskian spacetime – so it is the causal structure that is fixed. There are some intriguing mathematical theorems (going back to Pauli I believe) showing that you cannot construct quantum clock that would exactly measure this background time. In that sense, some people might want to consider "time" as part of the external classical realm that Bohr postulated was necessary to interpret the equations of quantum theory. I have always suspected that Bohr himself would have regarded the subject of "quantum gravity" as a non starter!'

Why does time deserve a special status in QM? Well, if you could raise it to the status of an operator T then its conjugate variable would be the energy H. These would need to satisfy the commutation relations $[T,H] = i\hbar$. Mathematically, it can be shown that it is impossible to have a pair of Hermitian operators satisfying these conditions, in which one of them has only positive eigenvalues. This relates back to Chris' remark about the problem of quantum clocks. In other words, we have the problem of a system with negative energy.

In QM, measurements of observables are taken at fixed times, a complete set of commuting operators (defining the maximum information you can obtain from a system) is required at fixed times, and the scalar product on the Hilbert space of states has to be conserved under time evolution of the Schrödinger equation. Time plays a foundational role in the technical and conceptual aspects of quantum theory. However, it is an idea grounded in the world of continuum mathematics. If it breaks down at the Planck scale, where some believe a discrete structure will emerge, what are we left with? Indeed, how much of QM will hold?

According to John Wheeler: 'The word "time" was not handed down from heaven as a gift from on high; the idea of time is a word invented by man, and if it has puzzlements connected with it, whose fault is it? It is our fault for having invented and used the word (see note 2). Taking this into mind, could we redefine time so that it reduces to the familiar clock ticking concept in some classical limit?'

Chris Isham is very sympathetic to this view: 'I would bet strongly that whatever the "final" theory of quantum gravity will be, we shall see our standard notion of time emerging only in some semiclassical sense.'

The role of time in relativity is very different to its role in QM. Relativity treats time at par with the other three spatial variables. Indeed, time and space are unified into a four dimensional spacetime manifold.

On this matter Chris explains: 'The notion of a single, four-dimensional, "spacetime" (rather than separated three-dimensional space plus one-dimensional time) first appears in special relativity (SR) because of Einstein's realization that any absolute split of 3+1 is observer-dependent. General relativity works with a curved version of the flat space–time of special relativity.'

'In both special and general relativity a "moment of time" corresponds to a single space-like hypersurface in the space–time. Time itself appears as the parameter that labels the elements of a one-parameter foliation (see note 3) of space-time by such surfaces. In the case of special relativity you restrict your attention to the case where the space-like surfaces are (i) hyperplanes, which are (ii) mapped into each other by actions of the Poincaré group. The main challenge in quantum theory is to show that all such admissible families give the same physical answers.'

'The situation in general relativity is more complex. One says that a hypersurface is space-like if the vectors tangent to each point of the surface are space-like. The idea here is that the Lorentzian metric on spacetime induces on each tangent space a copy of the Minkowski metric of special relativity, and it is with respect to this latter metric that the tangent vector has to be space-like. Put slightly less rigourously, a hypersurface is space-like if any "infinitesimal" transformation in the surface always points in a space-like direction.' (Figure 1)

The crucial point here is that the problems encountered when incorporating SR into QM can be overcome. When we try to add GR to the QM picture, the intricacies are much more difficult to handle.

With a *curved* spacetime equipped with a Lorentzian metric tensor, how many ways are there to foliate spacetime as a one parameter family of space-like surfaces? Chris continues: 'In some spacetimes there is a topological obstruction to performing *any* global foliations. But if such foliations are possible then there will be an uncountable number of them, each corresponding (in general relativity) to an allowed definition of time.' In other words, unlike in SR, we cannot define a unique time according to which we evolve a system. This is the main difference between the concept of time in special and in (classical) general relativity. It is an important aspect to the problem of time in QG. On the one hand, we have a Hamiltonian formulation of QM for privileged SR space-like surfaces. On the other we have general covariance in GR, which states that no one set of

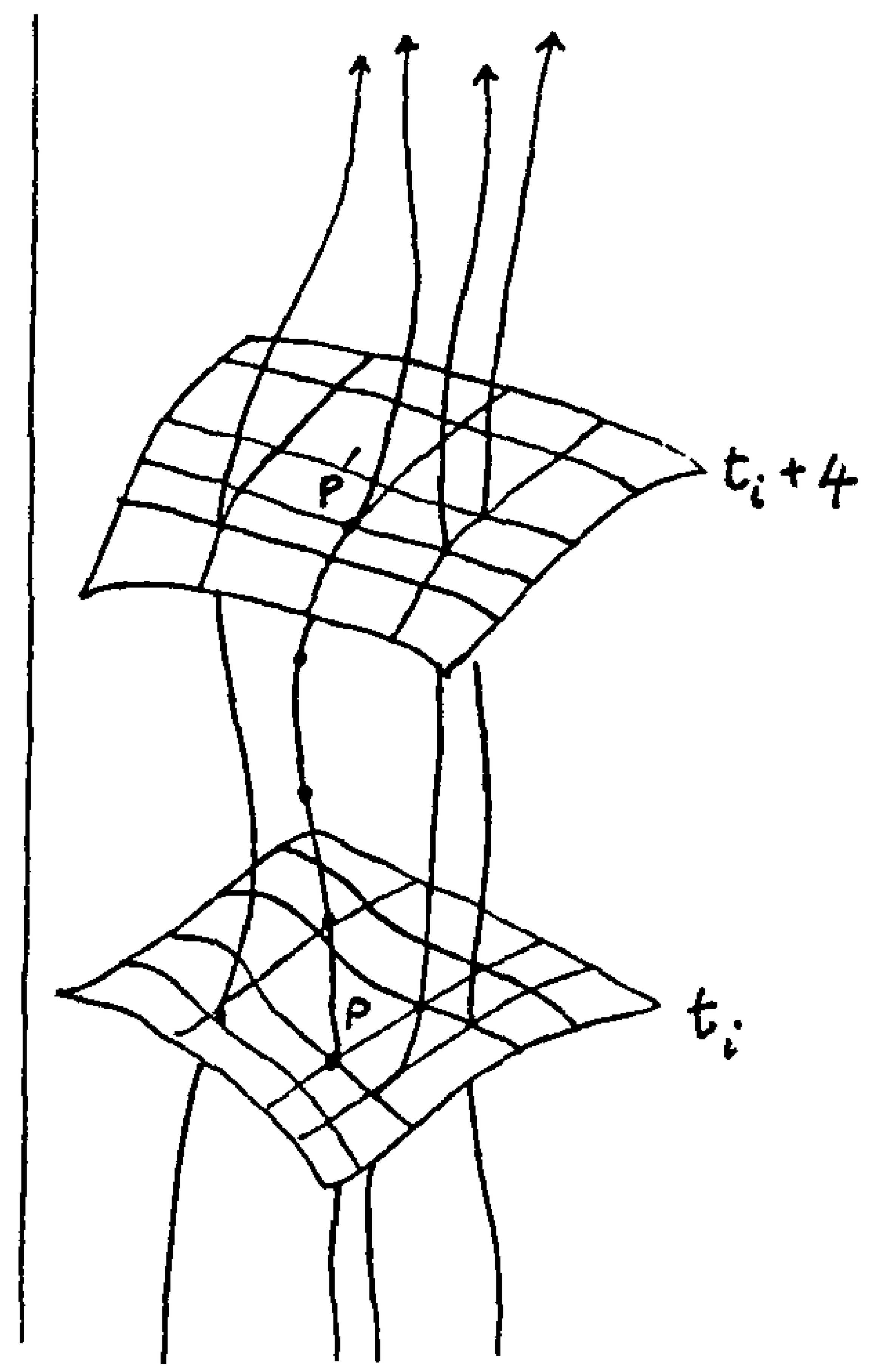


Figure 1. Foliating spacetime into a one-parameter family of space-like hypersurfaces. In Newtonian mechanics, a given moment of time has the same meaning for all observers Time is absolute. In SR, this notion is somewhat ambiguous because of the nonuniversality of simultaneity. However, once an inertial frame is specified, the notion becomes more precise Unless spacetime is flat, there are no global inertial frames in GR So 'a moment of time' in GR is ambiguous. However, a more general concept replaces it; the notion of 'a given space-like hypersurface'. It is possible to 'slice up' or foliate spacetime into a one-parameter family of 3D space-like surfaces, filling out all of spacetime At each event on a surface, there is a local Lorentz frame whose surface of simultaneity coincides locally with the hypersurface Lorentz frames at various events do not mesh to form a global inertial frame, but their simultaneity surfaces mesh to form the actual space-like hypersurface. This is a picture of two space-like hypersurfaces through events P and P' which occur at different times World lines of observers cross these surfaces orthogonally, and are labelled by (x_1, x_2, x_3), $x_0 = t_i + \tau$, where τ is the proper time measured along the worldline. Note that the third coordinate x_3 is suppressed in the diagram. Arbitrary coordinate grids defining x_1 and x_2 are drawn on the hypersurfaces. At the point of crossing a hypersurface, the proper time (defined by an observer's wristwatch) reads the same as other observers' watches Such a surface defines a notion of 'simultaneity'. Another surface of simultaneity through the event P may have a different slope or curvature (defining different local Lorentz frame). This second surface is defined by another set of observers with their own wristwatches, and their own proper time readings It is also possible to push the hypersurface ahead in time at different rates in different places, so long as the hypersurface stays spacelike This idea of a 'many-fingered' time is GR's dramatic new contribution to our understanding of time. See, for example, *Gravitation*, C. Misner, K. Thorne and J. Wheeler, W H Freeman and Co., 1973.

space-like surfaces has privilege as a reference system over others.

When we get to Planck scale physics, spacetime geometry could be subject to Heisenberg's uncertainty principle, varying about quantum-mechanically. In other words, the GR metric tensor $g_{\mu\nu}$ could have fluctuating components! How could one define a light cone and hence a space-like, light-like or time-like separation under such circumstances? Without such a definition, the idea of a space-like surface seems invalid. Perhaps the idea of a spacetime (a continuous manifold of points) does not have physical meaning in the QG regime, where the light cone could be smeared out.

To this Chris says: 'You cannot define a light cone, etc., except in some "background" sense, and this perhaps is the essence of the problem of time in QG: there is no fixed microcausal structure which can be used to construct a relativistic quantum theory in any of the standard ways.'

'However, this does not rule out a priori a continuous manifold: the ideas of causal structure and continuous manifold are in no way synonymous. You can easily have the latter without the former (although I myself am very sympathetic to the idea that the idea of a "continuum spacetime" is not something that will carry across to the "final" QG theory).'

One possibility at the Planck scale is for topology changes in spacetime geometry. Fixing a background topology and differential structure to that of Minkowski space, as is done in quantum field theory, seems to presuppose a lot at the Planck scale.

If we stick with a continuum formulation of QM based on complex numbers, could we be missing out on revealing some sort of a discrete structure at the Planck scale? One can see how a discrete theory could reduce to a continuum one in the large scale limit, but to shed light on a discrete theory while working from the perspective of a continuum one seems difficult to achieve.

Chris has felt this for a long time. One of the many reasons why he is so interested in the new decoherent-histories approach to quantum theory, and is developing his own quantum-logical version, is that the role of the continuum is isolated in a particularly efficient way. In his scheme it is just the space in which the decoherence functionals take their values. He says: 'It is sometimes suggested that near the Planck length a more "combinatorial" approach to physics may be appropriate, and I have some sympathy with this view. I think it would be much easier to think about such schemes within this new approach to quantum theory than in the old Hilbert space one.'

Is superstring theory able to handle this fundamental problem of time in its present perturbative formulation?

Chris replies: 'No. Although it is essential to emphasize that the "problem of time" is not a single problem (or even collection of problems) that applies willy-nilly to any attempt to construct a quantum theory of gravity. It is very approach-dependent and looks very different in different approaches to quantum gravity. For example, if space and time themselves do "emerge" in some nonperturbative way from superstring theory, then questions concerning the nature of time will look very different from those that arise naturally in, for example, the Wheeler–DeWitt approach to canonical quantum gravity. That is one of the many reasons why it is important to keep trying to find a new nonperturbative way of looking at superstring theory.'

The universe is modelled as a closed system. In its present formulation QM is inadequate for handling such models. After all, how does one define a time external to the universe? How successful have attempts been to define an internal time using, say, matter fields?

'They have been getting better. In a recent paper (see note 4) Karel Kuchar (who is the world expert on this type of thing) has come extremely close. Just how close depends precisely on what one decides one is trying to achieve. From one perspective one could say that he has succeeded in a technical sense, although he would be the first one to agree that his particular definition of time is not too compelling in any fundamental physical sense.'

Karel uses dust particles (see note 5) to fix points in space and identify moments in time. The dust acts like a reference fluid analogous to the old aether idea but with the significant difference that the aether was usually regarded as fixed, i.e. part of the background structure, whereas these dust particles do obey equations of motion. Hence, in a sense they are dynamic, as is the geometry of space–time.

One of the problems of working in QG is the lack of empirical data. One clue comes from the 1992 COBE results. These showed variations in the blackbody microwave background coming from different directions of the sky. These date back to 300,000 years after the Big Bang and are consistent with the inflationary scenario. The observations provide evidence for the idea of quantum fluctuations as the origin of the galaxies. Could they have any implication for the QG regime of 10^{-43} s after the Big Bang?

'Stephen Hawking has claimed that they support the ideas on the very early universe put forward by himself and Jim Hartle. However, the COBE data are currently being cited by a number of people in support of a variety of different views, and it is not clear yet what the outcome will be.'

Consistent histories

An approach which attempts to remove reference to external time in QM is that of consistent histories, pioneered by Griffiths, Omnès, Gell-Mann and Hartle. It generalizes QM via a sum over spacetime histories formulation. The motivation is to describe the QM of closed systems in the context of quantum cosmology. It hopes to shed light on the initial condition of the universe. The basic idea in this scheme is a QM history. The scheme, therefore, de-emphasizes the notion of a QM event at a single moment in time. It makes no reference to external observers, classical measurement apparatus, or wave function collapse (reduction of the state vector). However, it is possible to recover Bohr's formulation of QM under suitable conditions. Chris Isham is working on a version of this idea.

'Most work on this approach has been applied to standard quantum theory, not quantum gravity. To bring in quantum gravity, Hartle has discussed the possibility of using solutions of the Wheeler–DeWitt equation in the context of Euclidean path integrals. I myself am working on this general problem but from a completely different tack. I and my colleagues have been studying consistent histories using a new type of "quantum logic" of propositions about generalized spacetime structures. But the general idea in this formalism still holds: i.e. properties can only be ascribed to a "history" only when it is part of a consistent set. Typically, this requires deliberately loosing information by coarse-graining. One's expectation is that "time" is something of this sort: i.e. it is really only part of a semiclassical world that will "fade away" if one probes too closely.'

The scheme's main goal is to assign probabilities to families of histories in a closed system. A history is defined to be a sequence of QM events at a succession of times. QM events are tested for using projection operators which satisfy the properties of exhaustivity and mutual exclusiveness. Two types of projections are defined: fine-grained and coarse-grained ones.

The main problem in the formalism is interference. Interference between different histories forbids assignment of probabilities since probabilities have to satisfy certain axioms to be meaningful. They must be nonnegative, normalized, and they must sum to unity. Complex probability amplitudes can be assigned to histories, but a probability is defined as the modulus squared of an amplitude. This operation introduces cross terms which violate the probability axioms.

To handle this problem 'consistency conditions' are introduced. These determine sets of histories which negligibly interfere, and to which probabilities may be assigned. These conditions can be combined into a decoherence functional, which is the main calculational tool of the QM of history. It must satisfy the properties of hermiticity, positivity, normalization and the superposition principle. The functional is computed using standard quantum mechanics. Griffiths, Omnès, Hartle and Gell-Mann's contribution is the new probability interpretation. Here the notion of 'superposition' does not mean quite the same as it does in the standard way of superposing wave-functions. It is more a question that the decoherence functional must be additive with respect to a disjoint sum of histories.

In order to decouple interfering histories by coarse graining, we require the idea of decoherence. Consistent (decoherent) histories are then sets which obey the consistency conditions. Consistency is a property of families of histories, of which there may be many.

But what is the mechanism of decoherence? It has been postulated (see note 6) that the environment decoheres by acting like a sink, by draining off coherence – similar to a piece of blotting paper soaking up excess ink. But if the system is a closed QM universe, how do you define an environment? What other methods could there be to decohere closed systems?

To this problem Chris remarks: 'The mechanism for decoherence in a truly closed universe is, to my mind, still rather problematic. There have been attempts, for example, to identify part of the gravitational-field complex as the environment, and part as "really quantum" freedom in this environment. But I am doubtful whether this is really a universal notion. The examples I have seen have all been for very simplified models.'

In a closed QM universe, would it be possible to use black holes as decoherence devices? When matter goes into a black hole, the only properties of the hole which are observable via its distant electromagnetic and gravity fields are mass, charge and angular momentum. All other information is lost. Would this loss include 'interference' information between histories?

Unfortunately, Chris says: 'One could do that when studying quantum theory in a fixed background, since then one could consider a background containing black holes. However, this is not too meaningful a priori in a full theory of quantum gravity itself since the black holes will also have to be quantized in some sense.'

Jim Hartle made the important observation that one can talk about 'generalized histories' in QG in which the word 'history' need not presuppose a conventional type of time parameter. According to Chris: 'The basic entity in the scheme is a "possible universe". A simple, but informative example would be a Lorentzian geometry that is not globally hyperbolic. Such a spacetime does not admit a global foliation by space-like surfaces, and hence there is no global time parameter. Nevertheless, it is a viable classical possibility since time still makes sense locally (i.e. the local proper time along a world line). This was one of the original motivating examples suggested by Hartle.'

'However, my own interest in this scheme is when the basic entity is something more basic that does not necessarily include any manifest time-like features at all. This would be in line with the general idea that time appears only in some semiclassical limit. Of course, this leaves quite open what types of object one would actually use as "possible universes" since this depends entirely on the actual theory that is to be developed (the decoherent-histories approach is not a *theory per se*, it is more a *theoretical framework* in which new types of theory can be developed). For example, I am currently thinking about the possibility of constructing such a theory in which the basic entities are simply point-set topologies.'

'In theories of this generalized type, Hilbert spaces do not appear as basic mathematical structures in the same way as they do in normal quantum theory; indeed, in the scheme that I am developing, the basic mathematical structure is much closer to that of quantum logic (i.e. an algebraic approach) than it is to Hilbert space methodology. On the other hand, in any theory in which time could be shown to 'emerge' in some coarse-grained limit I would expect the usual Hilbert space structure to emerge too at the same level. However, as no one has yet written down an explicit example of this type, it is hard to be more specific!'

The Ashtekar programme[6–21]

People have approached the QG problem from many different directions, some more successful than others. Most attempts suffer from seemingly insurmountable problems. A short list of avenues includes higher-derivative Lagrangians, twisters, induced gravity, Kaluza-Klein theories, Euclidean quantum gravity, covariant perturbation theory, discrete gravity, nonlinear quantum mechanics, spin networks, asymptotic quantization, quantum cosmology, the decoherent histories just covered, quantum field theory in curved spacetime, superstrings, and canonical gravity. The Ashtekar programme belongs to the last category.

Abhay Ashtekar is at the Centre for Gravitational Physics and Geometry, Pennsylvania. Although his programme involves esoteric mathematics, some of its key features can be described qualitatively.

Abhay Ashtekar

An obvious question that people might ask about QG research is: Why are we spending so much time and effort trying to obtain a theory which is most probably untestable, and which will not have any practical application? Abhay doesn't quite agree with the assumptions here. He says: 'We are seeking a *physical* theory and, therefore, it will definitely have testable predictions. The initial tests will be probably indirect. For example, we have this great puzzle for over 50 years: quantum field theory provides incredibly accurate predictions in particle physics, yet it is, in a sense, only a set of calculational recipes, without a coherent, complete mathematical framework. Again, the recipes work incredibly well and should be taken seriously. But surely, they are incomplete in some essential way. The ultraviolet divergences arise largely because we assume that space–time is a continuum at all scales. If a quantum theory of gravity provides an alternate picture of space–time, it should lead to an alternate way of doing quantum field theories which is coherent and mathematically consistent. So, in a sense, the vast experimental data we have accumulated in particle physics could provide tests of the space-time models that come from quantum gravity.

'Furthermore, it is possible that quantum gravity will also lead to "practical applications". Recall that when Einstein discovered special relativity, most people thought that it was an esoteric theory which would have little impact, if any, on daily life. However, the theory then led to $E = mc^2$, which has had profound ramifications on the entire world order of this century. Quantum gravity will also change our understanding of space and time radically and may, therefore, have some really deep practical implications. I would hope that they would be more peaceful in nature!'

'But of course we do not have the faintest clue of such applications today and, as you indicate, it is beyond today's technology to test the quantum gravity effects *directly*. The primary motivation is really conceptual. Again, there is a similarity with special relativity. Just as the primary motivation for special relativity came from the incompatibility between Newtonian mechanics and Maxwell's electrodynamics, the two pillars of the 19th century physics, the primary motivation for quantum gravity comes from the tension between general relativity and quantum mechanics. There should be a deeper theory which unifies the principles of both in such a way that these two theories emerge in suitable approximations. That is the theory that workers in quantum gravity are trying to construct.'

Abhay's programme was inspired by the failure to find a renormalizable perturbative theory of quantum GR (QGR). What this is, and what would have happened if this failed programme had been successful?

He explains in depth: 'Perturbation theory assumes that answers to physical questions can be obtained by a method of successive approximations, generally expressed in terms of a power series in the coupling constant. We routinely use such approximations already in quantum mechanics. In quantum field theory, however, there is a key difference. Whereas in quantum mechanics, we know from general principles that the exact answer exists and is finite, and perturbative techniques are used only as a computational tool, in realistic quantum field theories in four dimensions, we do not have an assurance that there is an underlying well-defined theory; the perturbation series itself is being used to define the theory. To make matters worse, each term in the perturbative expansion diverges because one is allowing virtual processes of arbitrarily large momenta. In renormalizable theories, the infinities that arise are of a special nature; they can be absorbed into a finite number of parameters associated with the theory such as charges, masses and coupling constants. Once these parameters are renormalized by the appropriate infinite factors, all individual terms in the perturbative expansion become finite. The actual renormalized parameters can be determined by a finite number of experiments. Further experiments then provide tests of the theory. In a nonrenormalizable theory, this cannot be achieved and, therefore, the perturbation expansion has no predictive power.'

'Another way of stating the difference is that, in a renormalizable theory, predictions for phenomena at a given length scale are insensitive to what is happening at much smaller scales. So, for example, since QED is renormalizable, its predictions for processes at, say, a GeV scale are insensitive to all the goings on at the Planck scale (10^{19} GeV). In nonrenormalizable theories, this is not the case; the predictions for one length scale *are* sensitive to what is happening at much smaller, scales. It is thus harder to extract the physical content.'

'If perturbative general relativity had turned out to be renormalizable, on the one hand, life would have been simple. One could have calculated scattering cross-sections in gravity using familiar methods. On the conceptual side, however, at least some people, including me, would have been disappointed. It would have meant that most of physics is insensitive to the "true" small scale structure of space–time and hence it would have been harder to "probe" this structure. I have expressed my unease with the current status of quantum field theory. It is the nonrenormalizability of gravity that forces us to seek the true microstructure of space–time and holds clues for obtaining something which goes beyond (the calculational recipes of) quantum field theory.'

Perturbation theories assume that spacetime is a continuum at all scales. Could this be a reason why they fail for QG, where the short distance behaviour of spacetime might not be a continuum? An analogy has been made that spacetime is a 'foam' of sorts at Planck scales, subject to Heisenberg's uncertainty principle.

'That's right! The key infinity in perturbative treatments is the ultraviolet, one, where processes involve arbitrarily large energies. Surely, when the energy becomes bigger, gravity should become more and more important, and the space–time geometry, more and more nontrivial. Yet, in the perturbative treatments, one uses a fixed, continuum background space-time. Because space–time geometry is a dynamical entity in general relativity, it seems clear that in quantum gravity we cannot *presuppose* what the microstructure of geometry must be; we should let the theory itself tell us. Of course, one might have been lucky and made the right guess at the start. But the failure of perturbative methods tells us that this was not the case with the continuum hypothesis.'

Might quantum GR exist nonperturbatively, i.e. as an exact theory?

'Yes, it might. We do not have any clear evidence to the contrary. Incidentally, there do exist theories in three space–time dimensions which are nonrenormalizable but exactly soluble. Note that I am *not* saying that quantum GR must exist nonperturbatively. However, since there are qualitative reasons to expect that the nonperturbative theory will be very different from the perturbative one, the question is well worth investigating. More importantly, even if the answer turns out to be in the negative, the exercise will have been extremely worthwhile because it has already provided many new mathematical and conceptual tools to construct quantum field theories without a background space–time. Like many others, I firmly believe that, at a fundamental level, the final solution will have to be background-independent; space–time will emerge only as an approximation. And we have very little experience with quantum field theories which have an infinite number of degrees of freedom and which do not use a background space–time.'

Do you see the need for a fundamental revision of the present concepts on which the standard model is based? In other words, are we in need of a conceptual revolution to solve this problem? I once spoke to Bill Unruh, and he did not think QG could be obtained through 'tinkering with mathematics'. He said we needed to feed in new conceptual ideas.

According to Roger Penrose '...if there is to be a final theory, it could only be a scheme of a very different nature. Rather than being a physical theory in the ordinary sense, it would have to be a principle – a mathematical principle whose implementation might itself involve nonmechanical subtlety.' Is this your point of view as well?

'I believe that, at a fundamental level, the continuum picture has to go. This would be a profound change, in terms of both physics and mathematics since the picture is embedded so deeply in the conceptual fabric of physics. So, I agree with Bill that "tinkering" with mathematics alone will not suffice. However, I also feel that new mathematics *will* be needed. (I mean mathematics that was not previously used in physics.) This has happened with most big breakthroughs in physics. Newtonian physics needed calculus, general relativity needed differential geometry and quantum mechanics, Hilbert spaces and operator algebras. Indeed, without access to new mathematics, it would not have been possible to formulate new questions, let alone analyse them! Certainly, what Roger suggests is a possibility.'

Most approaches to the QG problem seem to take QM at face value, and try to modify GR in some way, or take GR as a weak-field limit. This is the case for superstring theory, where GR appears as a low-energy limit. The dimensional nature of the basic Planck units – the Planck length, time, and energy – lends credence to the idea that a QG theory could reproduce GR in regimes well away from the Planck scale. If QM is only an approximate theory itself, which fails to hold at Planck scales, then in the spirit of Roger Penrose's suggestion, QM will also have to undergo a revision. Abhay feels that this is in part why *all* attempts at quantum gravity that are being pursued are likely to be incomplete. 'However', he says, 'it is very difficult to provide concrete models of how QM should be revised. I think the best bet is to push all viable approaches to the end and if cracks develop, they will suggest how we should change QM.'

Loop variables gravity seems like a conservative approach compared to superstring theory. One major difference is that according to your programme, quantum gravity can be obtained without a unification of all forces. In the superstring theory, such a unification is vital for consistency. To be consistent, superstrings also need supersymmetry (a mathematical relationship between bosons and fermions) and higher spacetime dimensions (nine space and one time). The extra six spatial dimensions are supposed to be compactified into a very small region. Do you see these differences as complementary or conflicting issues? 'On the whole, I think the differences are complementary. String theory provides powerful constraints on allowable interactions. On the other hand, it has proved difficult to analyse string theory nonperturbatively, without assuming a background space–time. As I indicated above, our approach provides concepts and tools for a background independent approach. Over the last year or so, more concrete calculations have been proposed which may pull the two approaches closer.'

'I should also point out that we do not have a proof that quantum GR exists nonperturbatively. However, the tools required to analyse this issue finally exist. It is possible that we will find e.g. that supersymmetry or some specific couplings are necessary. This year is an exciting one for our programme because we have begun to analyse such issues.'

I spoke to Peter Bergmann (one of the founders of the geometrodynamical route to QG, and a coworker of Einstein) while I was an undergraduate and he was very skeptical of superstrings. Is it the same with you, considering most of the work in this field has been grounded in perturbation theory?

'I am perhaps more impressed than Peter by the mathematical successes of string theory. I agree that the use of a background space–time is a severe weakness. However, the very recent results due to Ashoke Sen (see note 7) and others on black holes within string theory are providing some glimpses of the possible relation between perturbative results and nontrivial geometries.'

Unlike the other forces, the gravitational force is nonlinear. In contrast, the electrical forces are linear because the total force due to a number of charges is just the sum of the forces due to each charge acting alone. This situation corresponds to the vector law of addition. Now in the case of gravity, we have a nonlinear combination of forces. This is because all forms of energy act as a source of gravity and inertia. This is true for the gravitational potential energy existing between two separated masses. Thus, the total gravitational force involved when two objects interact does not follow the usual linear vector addition law. Is this nonlinearity one of the root causes of all the problems in quantizing gravity?

'This profound nonlinearity is part of the difficulty. But this part is technical. The most important difficulty, in my view, is conceptual: it lies in the fact that gravity is encoded in the very geometry of space–time. To quantize gravity, one has to quantize geometry. One has to learn to do physics and mathematics in the absence of space–time.'

I guess one of the things the readers want to know is how everything reduces down to the picture of an apple falling from a tree. Newton's idea of a 'force' pulling the apple down is intuitive. In GR, this picture was replaced by the concept of the curvature of a four-dimensional spacetime continuum. The apple's mass curves the spacetime, and the Earth's mass curves the spacetime. This curvature tells how the masses should move. The two masses feel the combined curvature, and fall towards each other. In QFT, this 'attractive' picture is replaced by the concept of the exchange of particles called gravitons. The gravitons mediate the gravitational pull. Does the loop variables theory preserve this idea?

'Yes. Although in our approach gravitons do not exist at a fundamental level, in the low-energy approximation one can identify certain loop states with gravitons and then the picture is the one you indicate.'

'Incidentally, the loop variables also have a direct experimental significance. If you move an electron around a closed loop in a gravitational field, its spin undergoes a rotation. The basic loop variable just measures this rotation. Furthermore, if you know the rotation for all closed loops, you know the gravitational field completely.'

One of the problems with gravitons is that they seem to make sense only when the gravitational field of GR is linearized, i.e. split up into a flat space metric, and a perturbation. Gravitons are like ripples in a flat space–time background. Conventional particle theory tries to quantize these undulations, while leaving the background classical. When the background starts curving noticeably itself, what reference is there for these ripples? The problem seems to be that the arena and performers cannot be distinguished anymore.

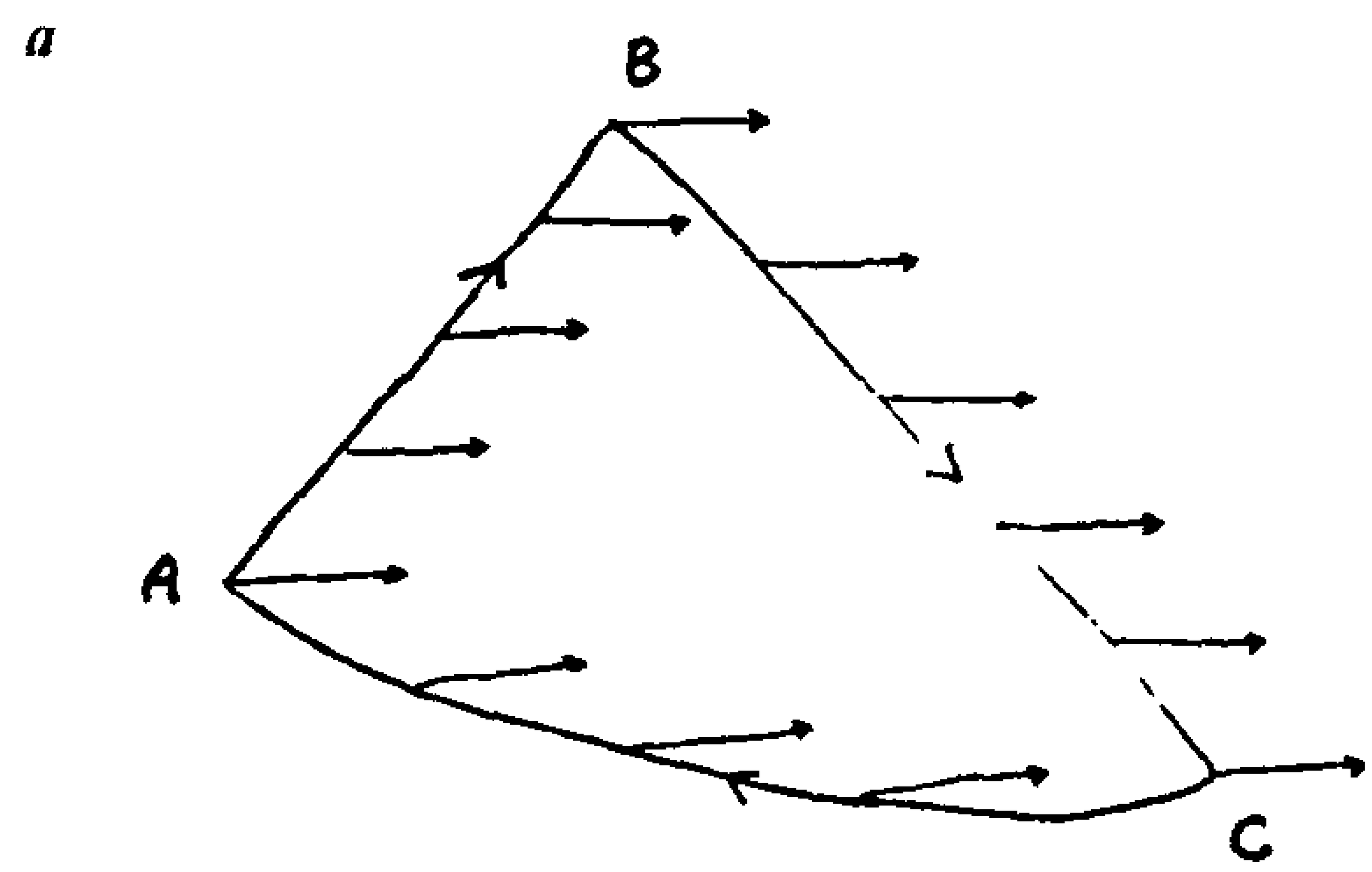
'Gravitons are indeed ripples on a space–time background. In the perturbative approach, one does quantize these undulations, leaving the background untouched. In our approach, the situation is different. Very roughly speaking, Minkowski space by itself is represented as a quantum loop state – which we call a weave – and certain nearby states – embroidered weaves – can be thought of as gravitons propagating in Minkowski space. However, as I indicated above, the picture works only for low energy gravitons. In this regime, one can distinguish the arena from the performers.'

Geometrodynamics

Abhay's programme takes conventional GR coupled to matter as the leaping point. It is based on the 'canonical' approach to QG, whereby the full four-dimensional spacetime is split up into an observer-dependent 3+1. The first step in any canonical approach is the Hamiltonian formulation of the theory. This was achieved in the early sixties by Dirac, Bergmann, Arnowitt, Deser, Misner and others. In their formulation, the basic variable is the metric on the three-dimensional space; it captures the intrinsic geometry of the hypersurface. The Hamiltonian framework was, therefore, baptized 'geometrodynamics' by Wheeler. Unfortunately, in this framework, the equations of the theory are complicated and, therefore, as far as the full theory is concerned, the quantization programme itself did not really take off.

In geometrodynamics the canonical variables are the intrinsic geometry and the extrinsic curvature of the 3D spatial hypersurface (which is embedded in a 4D Riemannian spacetime). The subject has close links with Gauss' theorem Egregium.

Abhay explains: 'The intrinsic geometry of a three-dimensional hypersurface is the geometry of space that we can "experience directly". It tells us, for example, how to measure lengths of curves lying in the surface. The extrinsic curvature, on the other hand, tells us how the surface is embedded in the four-dimensional space–time. To see the difference, let us go one dimension down and consider a cylinder embedded in a



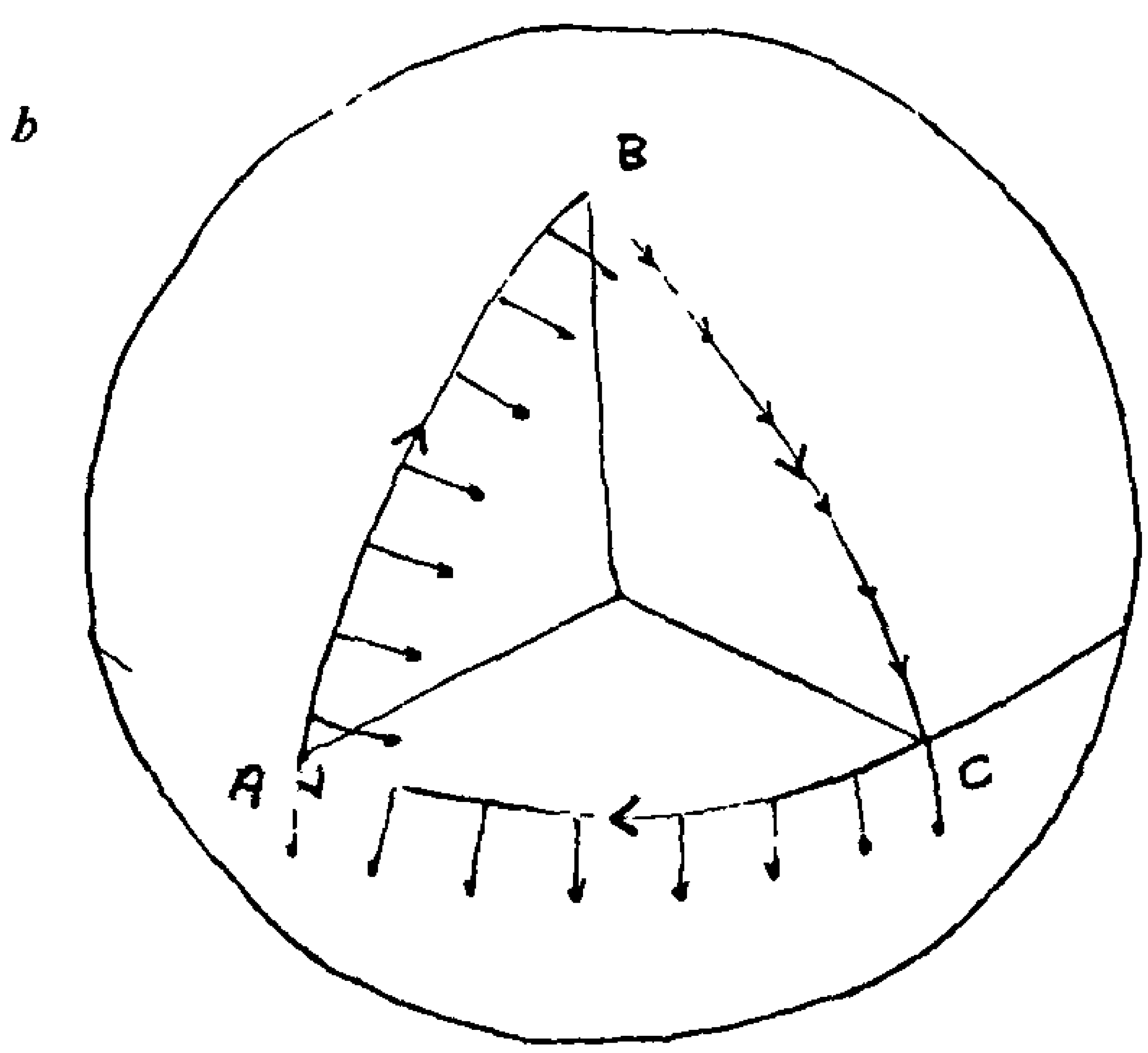


Figure 2. Extrinsic curvature. *a*, This is a parallel transport of a vector around a closed curve (ABC) The curve is drawn on a 2D sheet, embedded in 3D Euclidean space There is no rotation of the vector, meaning there is no extrinsic curvature. *b*, Parallel transport of a vector along geodesics on the surface of a sphere, embedded in 3D Euclidean space The vector is rotated 90°. The extrinsic curvature of a 3D spacelike hypersurface embedded in 4D spacetime is measured similarly. To obtain the curvature at a point on the surface, the closed curve (ABC) is pinched down infinitesimally.

three-dimensional Euclidean space. Since the cylinder can be obtained by rolling up a piece of paper, its intrinsic geometry is the same as that of the paper, i.e. a two-dimensional plane. The difference lies in how the paper and the cylinder sit in three-dimensional space. Technically, their extrinsic curvature is different.'

'Coming back to GR, the extrinsic curvature measures how the intrinsic geometry is changing in time. So, if we think of the intrinsic geometry as the configuration variable of general relativity, as one does in geometrodynamics, then extrinsic curvature can be thought of as the conjugate momentum. In quantum geometrodynamics, one would subject the two to the Heisenberg uncertainty relations.' (Figure 2)

By splitting spacetime into (3+1)D, are you treating the spatial and time coordinates on a different footing? Physically, this would correspond to choosing a special observer whose wristwatch keeps track of the distortions in the 3D spatial hypersurface. This method is very different to the 'covariant' schemes of quantization like the path integral approach.

'That is quite right. From aesthetic considerations, it would be nice to use, say, path integrals. However, no one has seen a way to make mathematical sense of path integrals in GR beyond certain simple minisuperspaces (which result when you freeze all but a finite number of degrees of freedom of the gravitational field and replace a quantum field theory problem by a quantum mechanics problem). The mathematical framework that my colleagues and I have developed over the last two years is, in principle, also applicable to the Euclidean path integral approach. However, there are two key difficulties. The first is conceptual. Even if one did have a complete Euclidean theory, it seems very difficult to extract physical predictions from it since, in quantum gravity, there is no analogue of the 'Wick rotation', which replaces the time coordinate t by it in Minkowskian quantum field theories, thereby enabling one to pass from the Euclidean to the Lorentzian regime (i.e. from Schwinger functions to the Wightman functions). The second difficulty is technical, associated with the specific form of Einstein's action.'

One of the main criticisms of the approach is that it breaks the general covariance of GR. Would the general covariance concept have any meaning if the continuum picture of spacetime is lost? One of the ideas brought up by your programme was the possibility of spacetime being combinatoric instead of geometric in nature, at the Planck scale.

'For technical clarity, I should first point out that, while canonical approaches do lack *manifest* covariance, they are, nonetheless covariant; for example, the full Poincaré group has a well-defined action on the phase space of electromagnetic fields in Minkowski space. With this side remark out of the way, let me answer your question. If anything resembling our approach is correct, then, in the quantum theory, there will be no such thing as a space–time geometry at a fundamental level. The picture, as you say, will be combinatorial and, therefore, the diffeomorphism group will not have a preferred role. Space–times will arise only semi-classically. In this approximation, one can ask if there is four-dimensional diffeomorphism invariance. The answer will be in the affirmative. However, it will be arrived at from the Hamiltonian picture; the four-dimensional covariance will not be manifested. This is a drawback of the approach but only from an aesthetic viewpoint.'

'Incidentally, for these reasons, I had first tried to use the covariant phase space for general relativity and avoid the 3+1 splitting. That approach faced technical problems and I now believe that, even if the technical problems could be resolved, it would not lead to an interesting theory. I will not be surprised, however, if there is another way to ensure manifest covariance in the semiclassical regime.'

By casting GR into Hamiltonian form, it is found that the theory is a dynamically constrained system. These restrictions go by the name of the Hamiltonian and diffeomorphism constraints. What physical significance do they have?

'*Constraints of this type signal the existence of a large* gauge group. In electrodynamics, for example, there is an analogous constraint, the Gauss law, which says that the electric fields should be divergence-free. It ensures that the classical (and quantum) theory is gauge-invariant. Higgs pointed out quite early in the game that the diffeomorphism constraint of general relativity is rather similar. It ensures that the spatial diffeomorphisms – *active coordinate transformations on the three*-dimensional hypersurface – do not change physics. This constraint is universal in the sense that its form and meaning is the same for all gravity theories in which there is no background metric.'

'The Hamiltonian constraint is more tricky. It is responsible for time evolution, i.e. dynamics. If you treat it in the same fashion as the diffeomorphism constraint, one is led to what people call "the frozen (time) formalism", in which nothing evolves and physical states are represented by entire space–times. There is nothing wrong with this picture. It is like doing Hamiltonian dynamics in terms of constants of motion; it is, therefore, inconvenient from technical and interpretational perspectives.'

I have heard that the constraint equations are (high-order) polynomials in terms of the basic canonical geo*metrodynamical variables – the three-metrics* and extrinsic curvatures. What effect does this have on trying to quantize the theory?

'It simplifies the technical problems of quantization enormously. In quantum field theories, what one might naively think of as operators are really operator valued distributions. Just as products of the Dirac delta distributions are ill-defined, products of these "operators" are also *ill-defined. This is the origin of the infinities.* So, if you have an expression which is non-polynomial in your basic operators, in general, there is very little chance that you would be able to regulate it and extract a meaningful operator. If the expression is a *low*-order polynomial, the chances of success are much greater. At various levels of rigour, this simplicity has been exploited by Rovelli, Smolin, Brügmann, Gambini, Pullin, Morales, Nicolai and Matschull to solve the quantum constraints. No such solutions exist in quantum geometrodynamics.'

'A simplification came in the mid-eighties when it was realized that there is another way of obtaining the Hamiltonian formulation where the basic variable is a connection which enables one to parallel transport chiral fermions. Thus, general relativity can also be regarded as connection dynamics. This formulation has two key advantages. The first is conceptual: general relativity is brought closer to gauge theories which govern all other basic forces of Nature. The second simplification is technical: equations of the theory become low order polynomials.'

The Ashtekar variables

At this stage, you came up with the idea of the Ashtekar variables. Where did the inspiration of these new variables come from? Was Amitabha Sen's work a major influence?

'For me, personally, a lot of the inspiration came from results on chiral solutions to the Einstein equations by Newman, Penrose and Plebanski in the mid-seventies. In a certain sense, this sector of Einstein's theory is completely integrable. Therefore, it seemed natural to try to base quantum theory on chiral variables. Sen's work was definitely an important influence up to a certain point. About the same time that Sen published his papers, Horowitz and I also had some results on positivity of the Hamiltonian of GR (for open universes) which used similar techniques. The two together suggested the direction that I finally took.'

What physical picture can you give us of them? You perform a canonical transformation of the old geometrodynamical variables, obtaining a connection and a triad as the new variables. A complex, group-valued, self-dual, spin connection is hard to visualize. In what way does a triad of orthonormal vectors on the three-slice take over the role of the extrinsic curvature?

'The connection enables us to parallel transport chiral fermions along closed loops and, therefore, has a direct physical meaning. Roughly speaking, the connection knows both about the (spatial derivatives) of the triad and the extrinsic curvature (the time derivative of the triad). If you think of the three-metric as the analogue of the position variable q in quantum mechanics and extrinsic curvature as the analogue of p, then the connection is analogous to the complex variable $z = q - ip$, which one often uses to construct the so-called coherent states in quantum mechanics. Clearly, if you know (q, p), you know (z, q) and vice versa. One can do quantum mechanics with (z, q) as basic variables, although it is somewhat unconventional. In quantum GR, we are led to connections and triads because these are the variables that simplify the equations.'

The actual canonical transformation is nonlinear. The result casts GR as a dynamical theory of connections. The phase space of GR is now seen to be embedded in that of Yang–Mills theory (a gauge theory). The constraints form surfaces which define restricted motions on this phase space. This hidden relation seems remarkable. Were you expecting such a relation, or did it just drop out of the working?

'I knew that if the triads and connections can be shown to be canonically conjugate, the embedding you refer to would be a consequence. But it was far from obvious to me that the Poisson brackets between triads and connections would be so simple. Also, because of some subtle differences between the connections that Sen was using and the ones I was led to use, it was not obvious to me that all of Einstein's equations would be low order polynomials in the triads and connections.'

The next step is the transition from classical GR to quantum GR. By using these Ashtekar variables, you are trying to quantize a theory of connections, instead of a geometrodynamic theory based on spatial three-metrics. In terms of connections, the constraint equations of GR bear a great resemblance to those of *non-Abelian* Yang–Mills theory.

'Progress came about because there already existed a rich machinery to deal with quantum gauge theories. We did have to make some important changes eventually because in Yang–Mills theories, one makes a heavy use of the background Minkowski metric and our framework had to exist without reference to any background structures. But a number of key ideas came from Yang–Mills theories.'

'Incidentally, because all other basic interactions can be formulated in terms of connections, the geometrodynamic formulation did create a distance between GR and other gauge theories. For example, Weinberg, in the introduction to his book (see note 8) emphasizes this point. However, both Einstein and Schrödinger had presented a formulation of general relativity with connections, rather than metrics as the basic variables. The reason the idea did not catch on, I believe, is that their equations were even more complicated than those of geometrodynamics. They used affine connections and simplifications occur with chiral ones. I became aware of this piece of history only recently.'

Loop variables

In QM we have operators and states. Operators act on states, and take them from one allowed state to another. The simplest example of a QM system may be the simple harmonic oscillator. There the raising and lowering operators take a particle subject to a constant restoring force, to different energy eigenstates. The canonical variables, position and momentum, are raised to the status of operators, and they have to satisfy commutation relations. What are the corresponding operators and quantum states in this approach?

'This is a somewhat technical point and I hope you will bear with me. In particle mechanics, we have the notion of configuration space – the space of positions, for example – and quantum states are functions on this space. In field theories, the situation is more complicated. States are now functions on a *quantum* configuration space which is a genuine enlargement of the classical configuration space and the measures which dictate the inner product are typically concentrated on the exotic, nonclassical configurations.'

'The precise domain space of quantum states is a well-defined completion of the space of gauge equivalence classes of connections. This space can be constructed without reference to a background metric and we have also developed integral and differential calculus on this space to define Hilbert spaces and operators. There is no background metric or connection anywhere. This calculus tells us that there is a well-defined "nonlinear duality" between connections and loops and, hence, one can also represent states as suitable functions of loops. The connection and the loop representations are "dual" to one another in the same sense as the position and the momentum representations in quantum mechanics are dual. In practice, issues related to the Planck regime are often more transparent in the loop representation while semiclassical questions are generally easier to analyse in the connection representation.'

'For technical reasons connected with diffeomorphism invariance, quantum states are represented by objects which are more general than functions in the connection representation, while in the loop representation, one can regard them just as functions of loops. In either case the basic quantum operators are associated with loops and strips, i.e. ribbons. Their commutator algebra is surprisingly simple; commutators can be expressed in terms of rerouting of loops and strips, and intersections of loops with strips, and strips with strips. It is a pretty, geometrical picture.'

In the loop representation, quantum states arise as 'functionals' of Wilson loops. A deep relation exists between the equivalence classes of loops and the theory of knot classes, invariants and polynomials. There is also a relation to Chern–Simons theory.

'This comes about because there is a specific "nonlinear duality" between loops and connections: Given a closed loop and a connection, one obtains a number, the value of the trace of the holonomy of the connection around the loop. It turns out that this duality implies that there is a 1–1 correspondence between certain functions on the loop space and measures on the quantum configuration space. Therefore, we can take these loop functions as quantum states. One can then formulate the diffeomorphism constraint rigourously and seek its solutions. Not surprisingly, in the loop representation, the solutions are (certain types of) knot invariants. Thus, after you have imposed the diffeomorphism constraint, you can say that the quantum configuration space is the space of knot classes and all knot invariants polynomials are the potential quantum states. At a heuristic level, one expects the Chern–Simons theory to define a diffeomorphism invariant measure on the space of connections and these ideas have, in fact, been exploited by Gambini and Pullin to obtain solutions to the Hamiltonian constraint. Whether such results can be made rigorous is an open question; one would have to wait and see if the current work by mathematicians in this domain leads to useful results.'

Are you feeding in what the microstructure of space–time should look like beforehand, or does it come out of the equations? I have heard the analogy of Planck space–time being like a mesh of mail armour, made up of loops weaved together.

'A key point in our approach is that we do not start out assuming what quantum geometry should be like but let the theory lead us to it. (Of course, the framework as a whole makes certain assumptions – there should be no background structure and the Wilson loop operators should be well-defined – but they are of a general nature). We construct operators that correspond to geometry and ask for states which, when probed with these operators, would approximate a classical geometry at scales much larger than the Planck scale. We then obtain specific states which have this property but which display a discrete structure of a definite type at the Planck scale. These are the states that look like three-dimensional chain mail. The gravitational field is excited only at the loops in the chains. However, on coarse graining, the states are indistinguishable from classical geometries. More importantly, we find that spectra of basic operators such as the area of a given two-dimensional surface are quantized in the units of the Planck area.'

'In the loop representation, all operators act by breaking, rerouting and gluing of loops. These operations will code the quantum gravity interactions. So, in the Planck regime, the familiar Feynman diagrams will be replaced by "topological" diagrams representing these operations. Feynman diagrams will arise only in a low-energy regime, where, as I indicated above, the concept of gravitons is meaningful.'

What was Profs Smolin and Rovelli's inspiration to come up with the loop representation? Has there been any collaboration with the Trias and Gambini group, which is working on gauge theories?

'As I recall, they were trying to solve the diffeomorphism constraint and realized that the solution is "obvious" in the loop representation. At that time, we did not have the mathematical machinery involving measures on the quantum configuration space and it was hard to see how to construct solutions to this constraint in the connection representation. Therefore, their realization was a real breakthrough. Curiously, none of us knew about the work by Gambini and his collaborators at that time. One of the students in our group, Brügmann, came upon their papers while applying the "loopy ideas" to Yang–Mills theory and told us about them. We then contacted Gambini and Trias. Since then, Gambini, Pullin and others have made some very significant contributions. We are all extremely happy with these synergetic developments.'

You can also bring in supersymmetric matter couplings.

'Yes, supersymmetry can be brought in. There is work in this direction by Jacobson, Matschull, Nicolai and others. The work by Nicolai and his colleagues on three-dimensional supergravity has been especially illuminating; it showed that supergravity does allow an infinite number of physical states rather than just, say, a single solution analogous to the Hartle–Hawking wave function.'

Current work in the programme

Let us talk about some of the current research problems in the field. In the language of connections, you have to complexify the phase space of GR, since you have mixed (both complex and real) variables. Now you have to restrict the phase space to real portions, since the physical observables must be real quantities, not complex ones. In other words, you must implement reality conditions in the quantum theory. Have these been dealt with satisfactorily?

'Several special cases had been treated satisfactorily over the last few years. These include certain Bianchi models and midi superspaces with a single Killing field. Over the past year, we have been able to treat the *general case*. In the harmonic oscillator case, the Segal–Bargmann transform enables us to pass from the Schrödinger representation, where the wave functions $\psi(q)$ are complex valued functions of q, to the holomorphic representation, where they are *holomorphic* functions $\psi(z)$ of z. Since we know that q and p are self-adjoint in the Schrödinger representation and since the Segal–Bargmann transform is unitary, you know automatically that the reality conditions also hold in the holomorphic representation; you do not have to check it explicitly. We have constructed a rigorous analogue of this transform for quantum gravity. So, in principle, the problem has now been solved. However, we still have to develop calculational tricks to make the transform useful in practice.'

One of the main problems has been to define an inner product (vital to make sense of probabilities) in the Hilbert space. What is the status of this search?

'This problem arises at two levels: kinematical and dynamical. One generally needs an inner product on the "kinematical" states prior to imposition of constraints to make sure that the constraint operators are well-defined and that there are no anomalies in the constraint algebra. This part is taken care of by the recent mathematical developments. Furthermore, as I indicated above, the diffeomorphism constraint has been regularized and solved. There are no anomalies. However, at the rigorous level, we still do not have a complete treatment of the Hamiltonian constraint. Very recently, the corresponding operator was constructed by Jerzy Lewandowski in Warsaw but we are yet to examine the question of anomalies. If there are none, we can construct a complete set of physical states. Fortunately, by now there are well-developed strategies to find the physically appropriate inner product and the question is if one of them will be easily implemented. However, it is quite possible that to ensure that there are no anomalies, we would have to bring in framed loops and quantum groups and/or supersymmetry. Mathematical machinery does exist to do this. And I would find it quite exciting if e.g. it is the dynamics of quantum gravity that ushers quantum groups into physics.'

'At a heuristic level, there has been a lot of progress on the issue of solutions to the Hamiltonian constraint and Rovelli and Smolin have introduced approximation techniques to find the inner product. These are useful guidelines for rigorous work. However, the problem of inner product on physical states is yet to be faced head-on.'

'Incidentally, if the programme is successful, we will have a quantum theory of gravity at a level of rigour that exceeds the current level in four-dimensional quantum field theories. This may seem like an overkill. However, since we have so little experience in nonperturbative QFTs, particularly in the absence of a background space–time, this level of care is needed both to ensure that we have not ignored some important subtleties and to convince the skeptics that a solution has really been found.'

How is the problem of time evolution handled? If there are only Hamiltonian constraints, and no Hamiltonian, how do you define a time variable, and evolve the quantum system?

'Two approaches have been followed, in both of which the notion of time is an approximate one. The first is some work I completed in 1989, where we saw that, in the connection representation, one can make a certain truncation of the theory in which the quantum Hamiltonian constraint can be written as the Schrödinger equation, where one of the connection components serves as time. This shows how the familiar evolution of QFT will arise in the low-energy regime. The second and more recent approach is due to Rovelli and Smolin. They couple gravity to a scalar field and use the scalar field as time. So, while we still do not know how to extract time and evolution in the Planck regime – and, like many of my colleagues, I feel this is not necessarily a meaningful thing to ask – in suitable approximations, time evolution has been recovered.'

For posterity, can I have your feelings as to where loop variables are heading?

'There are three parts to the answer. The first refers to quantum gravity, the second to other physical theories of connection, especially QCD, and the third to certain branches of mathematics.'

'Within gravity, as I indicated above, for the first time, we have a candidate for the quantum configuration space and the diffeomorphism constraint has been formulated without anomalies, and we know its general solution. If we can do the same for the Hamiltonian constraint – possibly by bringing in framed loops and quantum groups – one could say that quantum GR exists nonperturbatively. It is like saying, in QCD, that we have a representation in which the Hamiltonian is rigourously self-adjoint. This would be striking but not immediately solve problems of physical interest. The situation would be the same in QGR. However, that the theory exists, one could then look for reliable approximation methods and know that this is a meaningful programme. Many such methods are already being developed. I believe that it is crucial to continue to look for such methods.'

'Since the mathematical machinery that has been developed works for any theory of connections, it is tempting to apply it to QCD. We have already done so in two space–time dimensions and obtained several new results. The most notable among these are closed expressions for the Wilson loop Schwinger functions and a proof of equivalence of the Hamiltonian and Euclidean path integral methods. One might venture further and try to extend these methods to three and four space–time dimensions. That they will lead to mathematically interesting quantum field theories is clear. Whether these theories will be as useful in physics is an open issue. In any case, it is exciting that one and the same methods are being applied to theories of all basic forces of Nature.'

'On the mathematical side, there have also been some interesting new results. A few years ago, it was widely believed that spaces of gauge equivalent connections would not admit *any* nontrivial diffeomorphism-invariant measures. Several of us have found infinite families of such measures by now. The relation between knots and these diffeomorphism-invariant measures is equally exciting. Our methods also enable one to extend (sufficiently nice) invariants of ordinary knots to those of generalized knots, where loops are allowed to intersect and can have kinks and overlaps. This is of interest to knot and graph theorists. Such results arise because we are looking at the familiar structure from a new angle, with a new perspective. Such contributions, I believe, will continue.'

Superstrings[22-25]

Perhaps the most popularized of all the recent approaches to QG is superstrings.

Superstring theory proposes that the elementary constituents of matter are one-dimensional curves, rather than point particles. According to the theory, the quarks, leptons and gauge bosons of the standard model (SM) arise as excitation states of a truly fundamental entity – the superstring. These excitations come from rotational or vibrational degrees of freedom (rather like the harmonic modes in a violin string), or from internal degrees of freedom like supersymmetry (SUSY) and Lie group symmetries. If they exist, superstrings would be the smallest things in nature. They would be about 10^{-33} cm long, and reside in the weird world of Planck scale physics. Superstring excitations energies are about 10^{18} GeV, or 10^{-5} g. These particle masses are way too big to be detected by current accelerators, including the large hadron collider (LHC) being built in Europe. We are looking at detecting particles as massive as bacteria!

Superstrings can come in two varieties. One resembles a line segment with free ends. This type is called the open superstring. The other type is a loop with the topology of a circle – the closed superstring. Both versions reside in a ten-dimensional (9+1) spacetime.

Since we only see four (3+1) dimensions in real life, it is assumed that the six extra spatial dimensions are curled up, in a region as small as the Planck scale. The process of curling up is called compactification. There is no mechanism in the theory to tell how you curl the extra dimensions up, and this arbitrariness is a cause for concern. Theorists have explored Calabi–Yau spaces, group manifolds, and other spaces as candidates. The idea of compactification is reminiscent of Kaluza–Klein theory, which attempts to unify gravity with electromagnetism using an extended spacetime of 5 or (4+1) dimensions. This theory was extensively expanded in the 1970s to include the Yang–Mills theory, which gives for example, the type of non-Abelian gauge theory used in weak interactions (this involves a spacetime of dimension greater than 5; e.g. 12 for SU(3) internal symmetry). The mathematical techniques of these larger-dimensional spacetimes (including what is known as 'dimensional reduction' – the means whereby one gets back to the physical dimension 4) have been much used in discussions of supergravity and superstring theory. However, it is important to emphasize that these developments all work at the level of the *classical* field equations: the quantum theory used in quantising them is essentially standard.

String theories have an extraordinarily rich mathematical structure. Exploring their symmetries, described by various algebras, has been a major focus of work in the field. It has emerged that the same mathematical structures appear in many different fields. For example, as it moves through spacetime, the string traces out a world sheet or cylinder. Like the world lines (geodesics) of point particles, this world surface is constrained to be of extremal area. The symmetry properties of these surfaces are similar to those encountered in two-dimensional condensed-matter systems. There has been valuable cross-fertilization between these fields.

Superstring theory is also related to subjects like 2D conformal field theories, nonlinear sigma models, WZW models and other integrable models. It has associations with 2D supergravity, Lie superalgebras, and W-algebras, the latter being world-sheet symmetry algebras of bosonic string theories. Quantization leads to problems with 'anomalies' in these algebras, whereby classical symmetries are violated at the quantum level. Work is still being done on anomalies in W-algebras and also more generally in Lie algebras.

The vibrational spectrum of open superstrings includes a massless spin-1 gauge boson associated with a Yang–Mills group – SO(32) or $E_8 \times E_8$. These groups are needed for anomaly cancellations. For the closed superstring, a massless spin-2 particle arises. It has been identified as the graviton, the conjectured carrier of the quantum gravitational force.

GR is taken to be the large-distance or low-energy limit to the superstring. Strings seem like point particles at weak energy scales (10^{-16} cm) since it is many orders of magnitude less than the superstring scale (10^{-33} cm). A small extended object looks like a point from the perspective of lower energies. (Figure 3)

By examining string interactions, it is found that the existence of open strings implies closed strings. Closed strings alone can form a consistent system. It follows that every consistent string theory necessarily includes gravity, since closed strings have gravitons in their excitation spectrum.

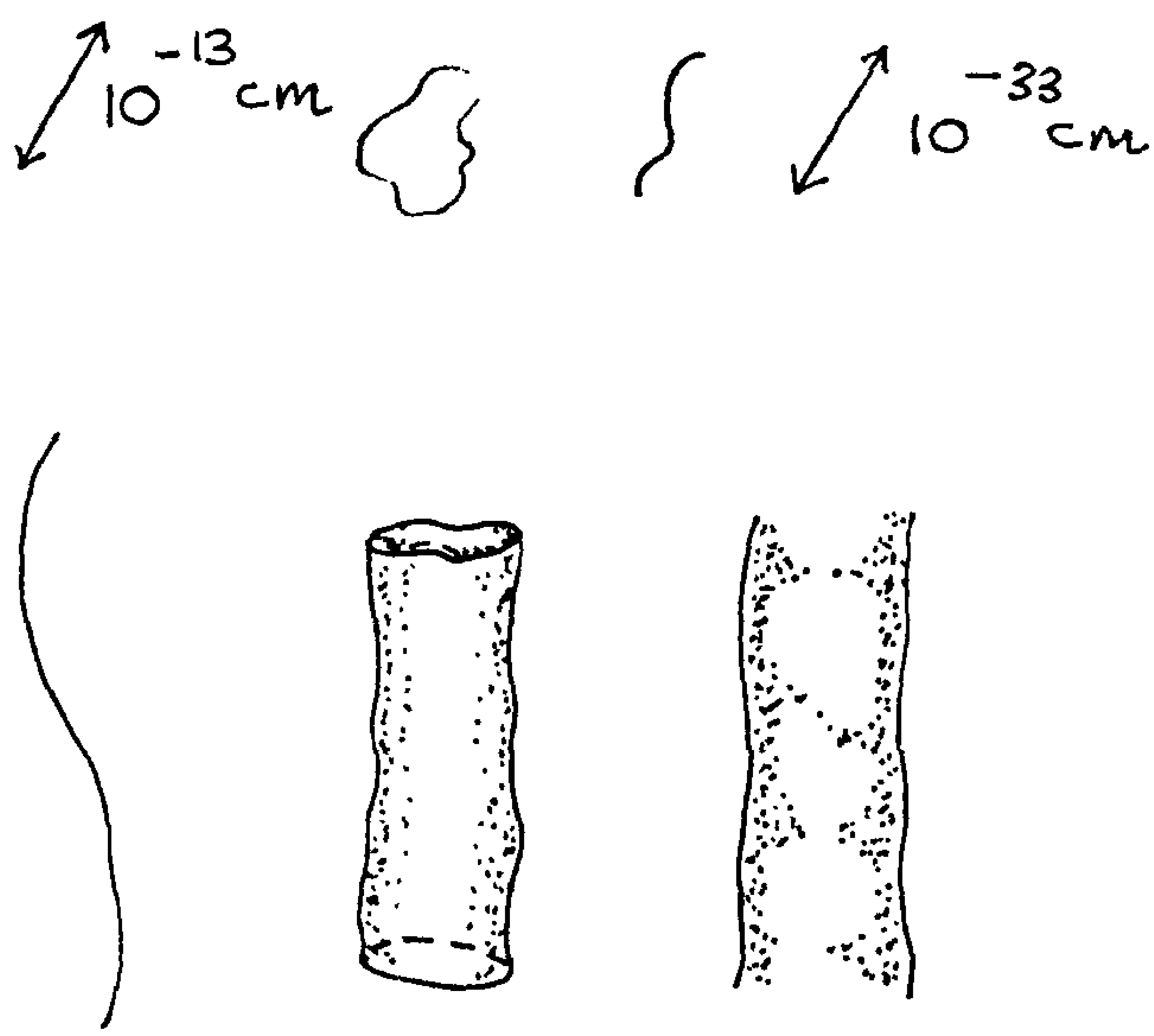


Figure 3. From particles to strings Point particle theories describe physics well at weak scale energies, but it is possible that at Planck scales, particles are actually tiny strings. World lines would then be replaced by world cylinders and sheets. See *Superstrings*, Vol. 1, M Green, J Schwarz and E. Witten, Cambridge University Press, 1987.

People have tried to generalize strings to higher-dimensional objects, like P-branes. However, it is very hard to make quantum theories of extended objects and get them to satisfy physical consistency constraints like causality and unitarity. In fact, the more extended you go, the harder it is. The success of string theory relies on conformal symmetry, which is a property solely of 2D world sheets.

The self-consistency of superstrings is heavily dependent on two experimentally undetected phenomena: supersymmetry and higher spacetime dimensions.

Ed Witten is at the Princeton Institute of Advanced Studies, New Jersey. He is a major contributor to superstring theory. He was once a history major at Brandeis, but had some science background. His interest in superstrings was sparked by a review article by John Schwarz. He says: 'Schwarz's review article made it easier to start learning about developments in superstring theory than it had been before – but my concentrating on reading it was, of course, a result of the fact that I was already very curious about superstrings.'

Edward Witten

Quantum field theory began when Planck tried to explain the emission spectrum of black body radiation. He postulated that energy was emitted discontinuously in discrete units, or quanta. Later, from a statistical analysis of the radiation field, Einstein suggested it was not just the emission and absorption of radiation which was quantized, but also the actual electromagnetic field itself – in the form of photons. This idea was confirmed experimentally by the Compton effect. Naturally, there were attempts to extend the quantum field idea to the classical gravity field, described by a metric tensor in Einstein's GR. The basic excitation of the quantum gravity field would be spin-2 radiation, which results from the symmetric nature of the metric tensor. How hard would it be to detect the gravitational analogue of the photon – the graviton – given the weakness of the gravitational interaction? Ed replies: 'Classical gravitational waves (for which there is indirect evidence from timing measurements of the binary pulsar) could be directly detected by laser interferometers – perhaps eventually giving also an important new window into some exotic phenomena in astrophysics. It is also possible that a very long wavelength cosmic background of gravitational waves could be detected by timing measurements involving space ships and pulsars. These experiments could indeed possibly verify directly the spin-2 nature of gravitational waves.'

'Unfortunately, there is no experiment in sight that could directly verify the quantized nature of the gravitational field.'

The gravity field is different from the other force fields since it plays a dual role. On the one hand, it defines the arena in which other fields interact. Yet, gravity itself is a participant in the interaction. Does QFT assume that spacetime is flat to all energy scales, i.e. has a Minkowskian spacetime signature even at Planck scales? 'QFT as we know it, without quantizing gravity, works on a fixed space–time manifold, which for most obvious purposes we can take to be Minkowski space. There is then a continuum space–time down to arbitrarily small distances.'

Gravity is highly nonlinear since the gravity field couples to mass–energy. The gravitational field gravitates. Is this a major conceptual subtlety when we try to quantize gravity? 'There are some conceptual issues in quantizing gravity, but it is not at all clear that they are decisive. What to me is the decisive difficulty is the problem of the infinities that arise when one tries to quantize the usual theory.' (It is instructive to contrast this point of view with Abhay's, stated earlier on.)

Renormalization, infinities and anomalies

QFT works with perturbation expansions around a Minkowskian background. Terms in these expansions are represented diagrammatically by Feynman diagrams. The Feynman diagram has an attractive interpretation as a representation of an actual physical process, and (though limited to perturbation theory) this picture has some validity.

In QED interactions between photons and electrically charged particles are linear. However, the situation is different with gravitons. Since energy gravitates, two gravitons can bend each others path. Gravitons can also emit and absorb other gravitons and couple to the other gauge bosons.

QFT introduces the concept of a particle's self-energy. Let us take the electron self-energy as an example. According to QFT, there is a difference between a bare electron and a physical electron. A bare electron is an unphysical entity because it has no radiation field around it. Such a radiation field is known to exist because of Heisenberg's uncertainty principle, which allows for the presence of a blanket of virtual photons. When a bare electron interacts with this virtual field, the bare electron is converted to a physical electron. This self-interaction affects the energy of the bare electron, and hence its mass. Quantities like charge are affected as well. In fact, all physical quantities are affected by the self-interaction.

When the Feynman integrals are evaluated for these self-energy terms, the integrals diverge to infinity. A similar phenomenon occurs when we calculate the photon self-energy in the form of vacuum polarization diagrams.

The first order terms (tree level) are finite, but the higher-order terms (the radiative corrections) involving closed loops diverge. Physically, the difficulties arise as a result of the point particle concept. Heisenberg's principle says that energy conservation can be violated locally such that the relation $\Delta E \cdot \Delta t > \hbar$ is obeyed. Since an electron is taken to be a point, emission and reabsorption of a virtual photon need take no time at all. So the virtual photon can have an infinite amount of energy. This contributes an infinite mass to the physical electron, since it must haul around the virtual photon blanket. Other properties of the electron get affected similarly.

Despite these shortcomings, QFT is one of the most successful theories we have. On this Ed says: 'Just because we write down a quantum field theory Lagrangian containing some fields does not mean that we know what the particles are going to be. QCD is an example where the particles (mesons and baryons) do not even appear in the Lagrangian (which contains quarks and gluons). Even if the particles are in natural one-to-one correspondence with the fields, as in QED, the masses of particles cannot be just read off from the constants in the Lagrangian. It is necessary to compute all kinds of quantum corrections. The fact that the nature and masses of the particles are, in large part, computed and not postulated, is a manifestation of the fact that the QFT description of nature is more thorough than its predecessors.'

To handle these divergences, QFT invokes the principle that energy cannot be measured, only differences in energy can be. A scheme called renormalization was developed by Feynman, Tomonaga and Schwinger back in 1947 to take care of this, for QED.

To handle divergent graphs, we firstly impose cut-offs on the integrals. This modifies the theory into another one. The cut-offs set some energy scale limit to the theory's predictions. Ed comments: 'The cutoff theory has physically unacceptable properties, so it is necessary to remove the cut-off and "renormalize". This is just as well, since the cut-off is arbitrary and if one did not have to remove it one would lose most of the predictive power. In good cases, like QED, after renormalization the parameters in the quantum theory are in one-to-one correspondence with the classical parameters; thus, in QED the only parameters are the charge and mass of the electron (the mass can be scaled to one by a convenient choice of units).'

The second step is to renormalize the theory by incorporating the blanket of virtual particles around the bare particle. This converts the bare particle into a physical one. In the process, relations between bare and physical quantities are introduced. These relations contain renormalization constants. Thirdly, we revert from the renormalized theory to the original QFT by taking the limit when the cut-offs go to infinity. This restores the original theory. The divergences reappear, but only in the renormalization constants relating physical and bare quantities. Like the bare quantities, the constants are not observable. The original integrals, written in terms of physical quantities, become well-defined and finite to all energy scales! So, in a sense we have shifted all the infinity problems aside. According to Ed: 'Renormalization is natural and necessary even if the effects are finite. So there is no issue of not having to renormalize. That would be so even if we were dealing with a fundamental theory valid to arbitrarily short distances.'

'In practice, we are always (except maybe in string theory) dealing with theories valid only down to very short distances, so there really is a cutoff – we just do not know what it is. Renormalization then extracts whatever can be learned without knowing what the cutoff is.'

It would seem that you have to apply this procedure to all the terms in the S-matrix expansion, for all the physical quantities you want to compute. Now the expansion is an infinite series of terms, which seems to require an infinite number of regularization and renormalization operations. Amazingly, QED has a gauge invariance which allows you to remove all the divergences from the measurable quantities to all orders of perturbation expansion, via just one type of renormalization.

Why cannot we compute physical quantities directly, instead of going via this roundabout? Feynman himself thought that renormalization was 'sweeping the problem under the carpet'. He thought that we should be able to formulate a theory such that we get zero ground state energy from the very start. What physics could we be missing here? One possibility, already mentioned by Abhay, is that the infinities ultimately arise from neglecting effects in Planck scale physics which would have a subtle effect on low energy physics.

When we try to fit gravity into QFT, the algorithm is to linearize the GR field by choosing a coordinate system in which the metric tensor can be expressed as

$g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu}$, where $\eta_{\mu\nu}$ is the Minkowski metric and $h_{\mu\nu}$ is a small perturbation such that $|h_{\mu\nu}| << 1$. This small perturbation, a tensor field, is subject to quantization on a Minkowski background which provides the causal structure. It is treated as the 'graviton' particle.

'Formally, there is no problem in constructing the perturbation expansion of the S= matrix along the above lines. The only real difficulty is that one meets ultraviolet divergences that cannot be renormalized away (without introducing new parameters absent in the classical theory).'

At the one-loop level pure QG's divergences can be renormalized away. However, the divergences arising at the two-loop level cannot be removed without robbing the theory of predictive yield.

On the associated problems of anomalies in QG he comments: 'The existence of a quantum theory is always delicate. Once one writes down a classical Lagrangian, one does not automatically get a quantum theory. Infinities and anomalies are two of the main ways to fail. Anomalies that affect gauge invariance are even worse than lack of renormalizability; they mean that the perturbation expansion of the quantum theory just does not make sense.' Essentially, the time rate of change of some quantity is zero in the classical theory and upon quantization this rate becomes nonzero, breaking some symmetry and, hence a conservation law.

Recent work in superstrings

How does extending the idea of particles to superstrings change things? An analogy would be helpful here. Let us take the decay of the free neutron, which is mediated by the weak force. We know that the old Fermi theory of weak decay is not renormalizable. However, the electroweak theory is. The electroweak theory replaces the point interaction of the old Fermi theory by inserting a massive W boson propagator, which mediates the decay. The theory is now renormalizable. Virtual supermassive superstring excitation states affect the renormalizability of superstring interactions by 'softening' or spreading out the interactions.

Ed says: 'Certainly, in going to string theory, everything becomes softened, and eventually finite. How best to understand it is not yet clear, but somehow the ultraviolet region is missing in string theory.'

Although the superstrings are extended objects, their interactions still take place at single spacetime points. However, this point depends on the Lorentz frame of the observer (the pants diagram phenomena) unlike the point particle case. There the spacetime interaction point is identical in all Lorentz observer frames. Thus there is less freedom in constructing string theories compared to the point particle case. This is why we have only a finite number of superstring theories instead of the infinite number of point field theories. (We have to choose the correct point particle QFT via phenomenology.) (Figure 4)

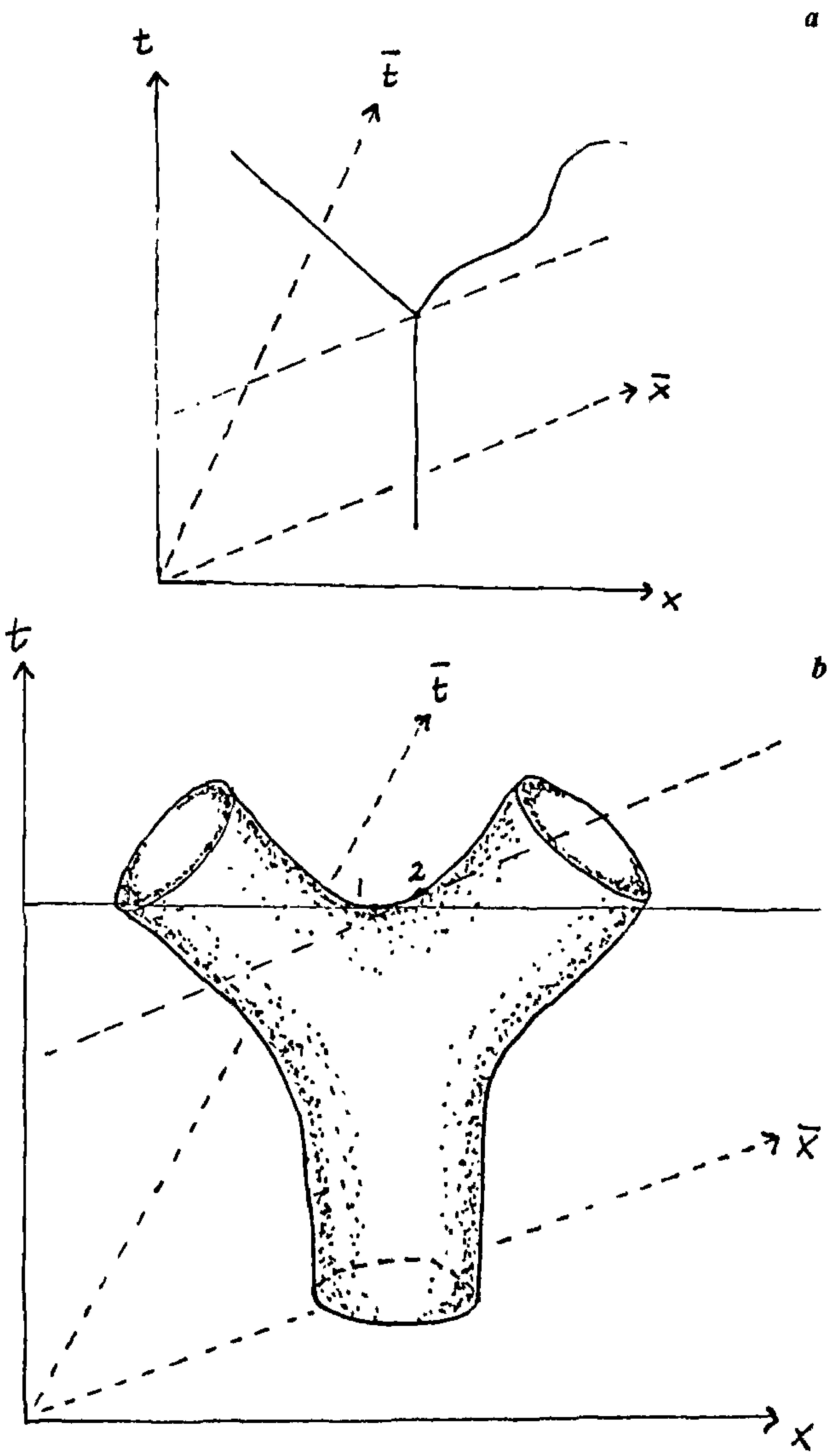


Figure 4. Contact interactions for particles and strings *a*, Interactions between particles occur at a single spacetime point, called the vertex in the Feyman diagram approach This point is Lorentz-invariant since it is the same in all reference frames. *b*,. The splitting of a closed string into two closed strings, otherwise known as the pants diagram The world surface is a cylinder, so the interaction region is no longer a point. One observer will see the interaction occur at point (1), and another will see it occur at (2) The contact point is no longer Lorentz-invariant Divergences in QFTs of point particles can arise when two sharp vertices get arbitrarily close to one another Renormalization is needed to remove the ultraviolet divergences by introducing ultraviolet momentum cutoffs, thereby 'fuzzying' out the vertices Since string diagrams are surfaces, they tend to accomplish this process automatically See *Quantum Field Theory of Point Particles and Strings*, B Hatfield, Addison-Wesley, 1992

According to one counting scheme, there are currently three d=10 superstring theories (type I, type II, heterotic) and although they have no adjustable dimensionless parameters, there are many mathematically consistent solutions which seem to satisfy the superstring equations of motion. Choosing physically representative solutions phenomenologically seems no different to inserting SM parameters by hand. The problem of finding physical solutions can only be solved if we know how compactification works. Doing this we can find the ground (vacuum) state of the theory. From there, we can work out the low-energy excitation spectrum, find the ratios of the excitation masses, and determine couplings constants from the topology of the compactified space.

Ed is optimistic about this arbitrariness: 'The attractive near-uniqueness that string theory appears to possess in principle is largely lost in practice because one finds a plethora of possible classical solutions. Hopefully, in time we will learn something new – perhaps connected with the question of why the cosmological constant vanishes – and the nature of the issue will change.'

On the problem of formulating a nonperturbative theory of superstrings, Ed says: 'My work with Nathan Seiberg (and related work he did by himself) led to some new results about nonperturbative behaviour of some quantum field theories, but not yet to new results on nonperturbative behaviour of string theory.'

String theory appears to admit a group of discrete field transformations, called S-dualities, as exact nonperturbative quantum symmetries. How could these symmetries help in formulating a nonperturbative QG theory of strings?

'I don't know yet, but I think it is a very exciting clue. S-dualities are symmetries that have no real analogue in our previous experience in physics (they do persist in some field theories – including some of those I studied with Seiberg – which are related to low energy limits of string theories).'

Particles trace out world lines, but strings trace out world sheets or cylinders, which can be treated as Riemann surfaces. When the Feynman diagrams are worked out for the closed string, it is found that they become finite order by order in the genus expansion.

Ed says: 'In string theory everything is finite – there is no need for renormalization.'

'Feynman diagrams are replaced by Riemann surfaces; these have much additional symmetry and structure, and this is related to some of the known wonders of the subject – there are surely also unknown wonders (S-duality is a hint of them), but since they are unknown I cannot tell you about them.'

On the matter of progress on the physical principles behind superstrings which should generalize Einstein's equivalence principle, he replies: 'The new physical principles haven't yet been found. It is hard to say how much progress there has been, since some of the things that have been learned may well turn out to be important clues.'

Superstrings are supposed to reside in ten spacetime dimensions, but we are really only speaking approximately. The phase space of the superstring consists of all the possible orientations and states the string can be in. What are the problems of formulating superstrings directly in four spacetime dimensions from the start? Are not superstrings supposed to generate spacetime, instead of just residing in it, since superstrings have gravitons in their excitation spectrum?

'It is true that in our present understanding ten spacetime dimensions is the classical solution of string theory with the maximum symmetry. Exactly how strings can be so smart as to generate space–time is still a mystery, but they do.'

'You can formulate strings "directly in four dimensions from the start" but then you do not have the degree of uniqueness that one has in ten dimensions. That is why the theory is often said to be ten-dimensional.'

There is no mechanism in superstring theory which tells us how the extra dimensions should compactify. Feynman said about superstrings: 'I don't like it that they're not calculating anything. I don't like it that they don't check their ideas. I don't like it that for anything that disagrees with experiment, they cook up an explanation – a fix-up to say "Well, it still might be true". For example, the [superstring] theory requires ten dimensions. Well, may be there's a way of wrapping up six of the dimensions. Yes, that's possible mathematically but why not seven? When they write their equation, the *equation* should decide how many of these things get wrapped up, *not* the desire to agree with experiment ....' What is your response to this remark?

'Some of the predictions are indeed checked. For instance, strings predict gravity while prestring physics makes quantum gravity impossible. This prediction has been tested – and the result is one of the main reasons for the interest in string theory.'

Are you familiar with the Ashtekar canonical gravity programme? If you are, do you think that the programme is founded on valid physical assumptions?

'Some of the work in that programme is interesting to me. I'd personally be surprised, however, if it turns out that conventional general relativity exists as a nonperturbative quantum field theory.'

Do you think the LHC will shed any light on low-energy superstring phenomenology? The detection of supersymmetry partners could be crucial. To explain the absence of SUSY partners at low energies we have introduced the idea of the spontaneous breaking of supersymmetry, analogous to the Higgs mechanism for electroweak symmetry breaking: one facet of the SM which is unconfirmed is this mysterious Higgs field which pervades all spacetime, which makes the electroweak theory work by giving masses to particles, and which we have no shred of evidence for.

'Apart from gravity, supersymmetry is one of the general predictions of string theory. Therefore, confirming it at the LHC would give a big boost to the whole effort. Moreover, if supersymmetry is observed, it won't be just that experimentalists will say "Yes, there is supersymmetry". They will observe a plethora of superpartner masses and couplings. There is a reasonable hope that in all that data there will be clues to how string theory should be done – or tests of theoretical ideas that may have emerged in the meantime.'

I asked Ed what the inspiration for his ideas was. Did he manipulate page-long equations in his head? Did he deal with concepts, and then phrase everything in maths later on? Where Einstein, Galileo and Newton were different from everyone else was they could ask really simple questions which had shattering consequences for existing physical theories. Was this Ed's modus operandi too?

To this he replied: 'I am afraid I don't work on the cosmic scale you are suggesting. Most of the time I am bogged down doing nothing, and whatever success I have has come by focusing on a very little bit of the total picture.'

Outlook

It is clear from the discussion that we are a long way off from even the foundations of a full theory of QG. Despite some famous claims, the end of theoretical physics is still not in sight. I asked Chris about possible lines of future investigation.

'I think that the superstring programme and the Ashtekar programme are currently the only two really viable approaches to trying to construct a full quantum theory of gravity, and I, therefore, support strongly work on both fronts!'

'Myself, I have long been bothered by the use of continuum ideas in both quantum theory and classical general relativity, whereas both programmes do take this for granted. So I would like to be able to get away from this, but the problem is to find a theoretical framework that does not seem to be contrived and artificial. One of the many reasons why the superstring and Ashtekar programmes *are* so important is that they do not have this contrived nature: within their own terms, each has quite compelling reasons for taking it seriously.'

'Similarly, I (like Roger Penrose) believe in my heart that quantum theory itself needs to be changed radically to reflect our changing conception of space and time. But, again, it is difficult to do this in a way that is believable to one other person than the author of the paper (Salam's definition of what it means to say that a theory is worth taking seriously!).'

Ed Witten gives this advice to those who want a try at superstrings: 'It is hard to give general advice to young researchers. But people who want to learn strings should definitely make sure that along the way they get a thorough knowledge of what is known in particle physics and how it can be (more or less) derived from strings.'

As to where superstrings are heading he has this to say: 'Lots of really nice things have been discovered, and that will continue. Sooner or later, probably far in the next century, the underlying unifying ideas will be discovered and then the physical landscape will be very different from what we can imagine today. If we are lucky and work hard, maybe we will live to see the day!'

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Notes

1 Ontology is the part of metaphysics concerned with the nature of existence.
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3. In SR, a foliation is a one-parameter family of three-dimensional *flat* surfaces (hyperplanes) which, taken all together, fill out the entire spacetime. In GR, these surfaces are curved
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Faint star counts and the Milky Way structure

Devendra Ojha

The Milky Way Galaxy offers a unique opportunity for testing theories of Galaxy formation and evolution. Here we discuss how large surveys, both photometric and astrometric, of galactic stars are the keystones in investigations into such fundamental problems as the merging history and future of the Galaxy. This work features a sample survey plan to produce probes of stellar populations in the Milky Way. Objectives of this work are to trace the fine structure of our Galaxy through the statistical study of the stellar distributions according to their luminosity, colours and proper motions. This involves two steps: first, acquiring a new photometric and astrometric sample survey in various galactic directions; and second analysing the data using a model of population synthesis and determining the properties of populations in the Galaxy and constraints on the scenario of formation and evolution.

THE field of galactic structure is currently extremely active, due mostly to improvements in observational capabilities and the realization that our Galaxy is perhaps the one best-suited to test theories of Galaxy formation. The concept of stellar populations has proved to be one of the most useful ideas of modern astronomy. Most simply, it is the idea that we can define a population as a set of stars that possess shared characteristics such as composition, age or kinematics, and that we can use the properties of various stellar populations to de-

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