1 Introduction

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In recent years it has sometimes been difficult to distinguish between articles in quantum gravity journals and articles in philosophy journals. It is not uncommon for physics journals such as *Physical Review D*, *General Relativity and Gravitation* and others to contain discussion of philosophers such as Parmenides, Aristotle, Leibniz, and Reichenbach; meanwhile, *Philosophy of Science*, *British Journal for the Philosophy of Science* and others now contain papers on the emergence of spacetime, the problem of time in quantum gravity, the meaning of general covariance, etc. At various academic conferences on quantum gravity one often finds philosophers at physicists' gatherings and physicists at philosophers' gatherings. While we exaggerate a little, there is in recent years a definite trend of increased communication (even collaboration) between physicists working in quantum gravity and philosophers of science. What explains this trend?

Part of the reason for the connection between these two fields is no doubt negative: to date, there is no recognized experimental evidence of characteristically *quantum* gravitational effects. As a consequence, physicists building a theory of quantum gravity are left without direct guidance from empirical findings. In attempting to build such a theory almost from first principles it is not surprising that physicists should turn to theoretical issues overlapping those studied by philosophers.

But there is also a more positive reason for the connection between quantum gravity and philosophy: many of the issues arising in quantum gravity are genuinely philosophical in nature. Since quantum gravity forces us to challenge some of our deepest assumptions about the physical world, all the different approaches to the subject broach questions discussed by philosophers. How should we understand general relativity's general covariance – is it a significant physical principle, or is it merely a question about the language with which one writes an equation? What is the nature of time and change? Can there be a theory of the universe's boundary conditions? Must space and time be fundamental? And so on. Physicists thinking about these issues have noticed that philosophers have investigated each of them. (Philosophers have discussed the first question for roughly 20 years; the others for at least 2,500 years.) Not surprisingly, then, some physicists have turned to the work of classic and contemporary philosophers to see what they have been saying about time, space, motion, change and so on. Some philosophers, noticing this work, have responded by studying quantum gravity. They have diverse motives: some hope that their logical skills and acquaintance with such topics may serve the physicists in their quest for a theory of quantum gravity; others hope that work in the field may shed some light on these ancient questions, in the way that modern physics has greatly clarified other traditional areas of metaphysics, and still others think of quantum gravity as an intriguing 'case study' of scientific discovery in practice. In all these regards, it is interesting to note that Rovelli (1997) explicitly and positively draws a parallel between the current interaction between physics and philosophy and that which accompanied the scientific revolution, from Galileo to Newton.

This volume explores some of the areas that philosophers and physicists have in common with respect to quantum gravity. It brings together some of the leading thinkers in contemporary physics and philosophy of science to introduce and discuss philosophical issues in the foundations of quantum gravity. In the remainder of this introduction we aim to sketch an outline of the field, introducing the basic physical ideas to philosophers, and introducing philosophical background for physicists. We are especially concerned with the questions: Why should there be a quantum theory of gravity? What are the leading approaches? And what issues might constitute the overlap between quantum gravity and philosophy?

More specifically, the plan of the Introduction is as follows. Section 1.1 sets the stage for the volume by briefly considering why one might want a quantum theory of gravity in the first place. Section 1.2 is more substantive, for it tackles the question of whether the gravitational field *must* be quantized. One often hears the idea that it is actually inconsistent with known physics to have a world wherein the gravitational field exists unquantized. But is this right? Section 1.2.1 considers an interesting argument which claims that if the world exists in a half-quantized and half-unquantized form, then either superluminal signalling will be allowed or energy–momentum will not be conserved. Section 1.2.2 then takes up the idea of so-called 'semiclassical' quantum gravity. We show that the arguments for quantizing gravity are not conclusive, but that the alternative is not particularly promising either. We feel that it is important to address this issue so that readers will understand how one is led to consider the kind of theories – with their extraordinary conceptual difficulties – discussed in the book. However, those not interested in pursuing this issue immediately are invited to skip ahead to Section 1.3, which outlines (and hints at some conceptual problems with) the two main theories of quantum gravity, superstring theory and canonical quantum gravity. Finally, Section 1.4 turns to the question of what quantum gravity and philosophy have to say to each other. Here, we discuss in the context of the papers in the volume many of the issues where philosophers and physicists have interests that overlap in quantum gravity.

A word to the wise before we begin. Because this is a book concerned with the philosophical dimensions of quantum gravity, our contributors stress philosophical discussion over accounts of state-of-the-art technical developments in physics: especially, loop quantum gravity and M-theory are treated only in passing (see Rovelli 1998 and Witten 1997 for reviews, and Major 1999 for a very accesible formal introduction). Aside from sheer constraints on space, the reasons for this emphasis are two-fold: First, developments at the leading edge of the field occur very fast and do not always endure; and second, the central philosophical themes in the field can (to a large extent) be understood and motivated by consideration of the core parts of the theory that have survived subsequent developments. We have thus aimed to provide an introduction to the philosophy of quantum gravity that will retain its relevance as the field evolves: hopefully, as answers are worked out, the papers here will still raise the important questions and outline their possible solutions. But the reader should be aware that there will have been important advances in the physics that are not reflected in this volume: we invite them to learn here what issues are philosophically interesting about quantum gravity, and then discover for themselves how more recent developments in physics relate to those issues.

1.1 Why quantum gravity?

We should emphasize at the outset that currently there is no quantum theory of gravity in the sense that there is, say, a quantum theory of gauge fields. 'Quantum gravity' is merely a placeholder for whatever theory or theories eventually manage to bring together our theory of the very small, quantum mechanics, with our theory of the very large, general relativity. This absence of a theory might be thought to present something of an impediment to a book supposedly on its foundations. However, there do exist many more-or-less developed approaches to the task – especially superstring theory and canonical quantum gravity (see Section 1.3) – and the assumptions of these theories and the difficulties they share can be profitably studied from a variety of philosophical perspectives.

First, though, a few words about why we ought to expect there to be a theory of quantum gravity. Since we have no unequivocal experimental evidence conflicting with either general relativity or quantum mechanics, do we really need a quantum theory of gravitation? Why can't we just leave well enough alone, as some philosophical approaches to scientific theories seem to suggest?

It might be thought that 'instrumentalists' are able to ignore quantum gravity. Instrumentalism, as commonly understood, conceives of scientific theories merely as tools for prediction. Scientific theories, on this view, are not (or ought not to be) in the business of providing an accurate picture of reality in any deeper sense. Since there are currently no observations demanding a quantum gravitational theory, it might be thought that advocates of such a position would view the endeavour as empty and misguided speculation, perhaps of formal interest, but with no physical relevance.

However, while certain thinkers may indeed feel this way, we don't think that instrumentalists can safely ignore quantum gravity. It would be unwise for them to construe instrumentalism so narrowly as to make it unnecessary. The reason is that some of the approaches to the field may well be testable in the near future. The work that won first prize in the 1999 Gravity Research Foundation Essay Competition, for instance, sketches how both photons from distant astrophysical sources and laboratory experiments on neutral kaon decays may be sensitive to quantum gravitational effects (Ellis et al. 1999). And Kane (1997) explains how possible predictions of superstring theory – if only the theory was sufficiently tractable for them to be made – could be tested with currently available technologies. We will never observe the effects of gravitational interactions between an electron and a proton in a hydrogen atom (Feynman 1995, p. 11, calculates that such interaction would change the wave function phase by a tiny 43 arcseconds in $100T$, where T is the age of the universe!), but other effects may be directly or indirectly observable, perhaps given relatively small theoretical or experimental advances. Presumably, instrumentalists will want physics to be empirically adequate with respect to these phenomena. (We might also add the common observation that since one often doesn't know what is observable until a theory is constructed, even an instrumentalist should not restrict the scope of new theories to extant evidence.)

Another philosophical position, which we might dub the 'disunified physics' view might in this context claim that general relativity describes certain aspects of the world, quantum mechanics other distinct aspects, and that would be that. According to this view, physics (and indeed, science) need not offer a single universal theory encompassing all physical phenomena. We shall not debate the correctness of this view here, but we would like to point out that if physics aspires to provide a complete account of the world, as it traditionally has, then there must be a quantum theory of gravity. The simple reason is that general relativity and quantum mechanics cannot both be correct even in their domains of applicability.

First, general relativity and quantum mechanics cannot both be universal in scope, for the latter strictly predicts that all matter is quantum, and the former only describes the gravitational effects of classical matter: they cannot both take the whole (physical) world as their domain of applicability. But neither is the world split neatly into systems appropriately described by one and systems appropriately described by the other. For the majority of situations treated by physics, such as electrons or planets, one can indeed get by admirably using only one of these theories: for example, the gravitational effects of a hydrogen nucleus on an electron are negligible, as we noted above, and the quantum spreading of the wavepacket representing Mercury won't much affect its orbit. But in principle, the two theories govern the same systems: we cannot think of the world as divided in two, with matter fields governed by quantum mechanics evolving on a curved spacetime manifold, itself governed by general relativity. This is, of course, because general relativity, and in particular, the Einstein field equation

$$G_{\mu\nu} = 8\pi T_{\mu\nu}, \tag{1.1}$$

couples the matter–energy fields in the form of the stress–energy tensor, $T_{\mu\nu}$, with the spacetime geometry, in the form of the Einstein tensor, $G_{\mu\nu}$. Quantum fields carry energy and mass; therefore, if general relativity is true, quantum fields distort the curvature of spacetime and the curvature of spacetime affects the motion of the quantum fields. If these theories are to yield a complete account of physical phenomena, there will be no way to avoid those situations – involving very high energies – in which there are non-negligible interactions between the quantum and gravitational fields; yet we do not have a theory characterizing this interaction. Indeed, the influence of gravity on the quantum realm is an experimental fact: Peters et al. (1999) measured interference between entangled systems following different paths in the Earth's gravitational field to measure gravitational acceleration to three parts in 10^9. Further, we do not know whether new low energy, non-perturbative, phenomena might result from a full treatment of the connection between quantum matter and spacetime. In general, the fact that gravity and quantum matter are inseparable 'in principle' will have *in practice* consequences, and we are forced to consider how the theories connect.

One natural reaction is to correct this 'oversight' and extend quantum methods to the gravitational interaction in the way that they were applied to describe the electromagnetic and nuclear interactions of matter, yielding the tremendously successful 'standard model' of quantum field theory. One way to develop this approach is to say that the spacetime metric, $g_{\mu\nu}$, be broken into two parts, $\eta_{\mu\nu} + h_{\mu\nu}$, representing a flat background spacetime and a gravitational disturbance respectively; and that we look for a quantum field theory of $h_{\mu\nu}$ propagating in a flat spacetime described by $\eta_{\mu\nu}$. However, in contrast to the other known forces, it turns out that all unitary local quantum field theories for gravity are non-renormalizable. That is, the coupling strength parameter has the dimensions of a negative power of mass, and so standard arguments imply that the divergences that appear in perturbative calculations of physical quantities cannot be cancelled by rescaling a finite number of physical parameters: ultimately the theory depends on an infinite number of quantities that would need to be fixed empirically. More troubling is the strong suggestion from study of the 'renormalization group' that such non-renormalizable theories become pathological at short distances (e.g. Weinberg 1983) – perhaps not too surprising a result for a theory which attempts in some sense to 'quantize distance'.

Thus the approach that worked so well for the other forces of nature does not seem applicable to gravity. Some new strategy seems in order if we are to marry quantum theory and relativity. The different programmes – both the two main ones, canonical quantum gravity and superstring theory, and alternatives such as twistor theory, the holographic hypothesis, non-commutative geometry, topological quantum field theory, etc. – all explore different avenues of attack. What goes, of course, is the picture of gravity as just another quantum field on a flat classical spacetime – again, not too surprising if one considers that there is no proper distinction between gravity and spacetime in general relativity. But what is to be expected, if gravity will not fit neatly into our standard quantum picture of the world, is that developing quantum gravity will require technical and philosophical revolutions in our conceptions of space and time.

1.2 Must the gravitational field be quantized?

1.2.1 No-go theorems?

Although a theory of quantum gravity may be unavoidable, this does not automatically mean that we must *quantize* the classical gravitational field of general relativity. A theory is clearly needed to characterize systems subject to strong quantum and gravitational effects, but it does not follow that the correct thing to do is to take classical relativistic objects such as the Riemann tensor or metric field and quantize them: that is, make them operators subject to non-vanishing commutation relations. All that follows from Section 1.1 is that a new theory is needed – nothing about the nature of this new theory was assumed. Nevertheless, there are arguments in the literature to the effect that it is inconsistent to have quantized fields interact with non-quantized fields: the world cannot be half-quantized-and-half-classical. If correct, given the (apparent) necessity of quantizing matter fields, it would follow that we must also quantize the gravitational field. We would like to comment briefly on this type of argument, for we believe that they are interesting, even if they fall short of strict no-go theorems for any half-and-half theory of quantum gravity.

We are aware of two different arguments for the necessity of quantizing fields that interact with quantum matter. One is an argument (e.g. DeWitt 1962) based on a famous paper by Bohr and Rosenfeld (1933) that analysed a semiclassical theory of the electromagnetic field in which 'quantum disturbances' spread into the classical field. These papers argue that the quantization of a given system implies the quantization of any system to which it can be coupled, since the uncertainty relations of the quantized field 'infect' the coupled non-quantized field. Thus, since quantum matter fields interact with the gravitational field, these arguments, if correct, would prove that the gravitational field must also be quantized. We will not discuss this argument here, since Brown and Redhead (1981) contains a sound critique of the 'disturbance' view of the uncertainty principle underpinning these arguments.

Interestingly, Rosenfeld (1963) actually denied that the 1933 paper showed any inconsistency in semiclassical approaches. He felt that empirical evidence, not logic, forced us to quantize fields; in the absence of such evidence 'this temptation [to quantize] must be resisted' (1963, p. 354). Emphasizing this point, Rosenfeld ends his paper with the remark, 'Even the legendary Chicago machine cannot deliver sausages if it is not supplied with hogs' (1963, p. 356). This encapsulates the point of view we would like to defend here.

The second argument, which we will consider, is due to Eppley and Hannah (1977) (but see also Page and Geilker 1981 and Unruh 1984). The argument – modified in places by us – goes like this. Suppose that the gravitational field were relativistic (Lorentzian) and classical: not quantized, not subject to uncertainty relations, and not allowing gravitational states to superpose in a way that makes the classical field indeterminate. The contrast is exactly like that between a classical and quantum particle.[1] Let us also momentarily assume the standard interpretation of quantum mechanics, whereby a measurement interaction instantaneously collapses the wave function into an eigenstate of the relevant observable. (See, for example Aharonov and Albert 1981, for a discussion of the plausibility of this interpretation in the relativistic context.)

Now we ask how this classical field interacts with quantized matter, for the moment keeping all possibilities on the table. Eppley and Hannah (1977) see two (supposedly) exhaustive cases: gravitational interactions either collapse or do not collapse quantum states.

Take the first horn of the dilemma: suppose the gravitational field *does not* collapse the quantum state of a piece of matter with which it interacts. Then we can send superluminal signals, in violation of relativity, as conventionally understood. Eppley and Hannah (1977) (and Pearle and Squires 1996) suggest some simple ways in which this can be accomplished using a pair of entangled particles, but we will use a modification of Einstein's 'electron in a box' thought experiment. However, the key to these examples is the (seemingly unavoidable) claim that if a gravitational interaction does not collapse a quantum state, then the dynamics of the interaction depend on the state. In particular, the way a classical gravitational wave scatters off a quantum object would depend on the spatial wave function of the object, much as it would depend on a classical mass distribution. Thus, scattering experiments are at least sensitive to changes in the wave function, and at best will allow one to determine the form of the wave function – without collapsing it. It is not hard to see how this postulate, together with the usual interpretation of quantum measurements, allows superluminal signalling.

We start with a rectangular box containing a single electron (or perhaps a microscopic black hole), in a quantum state that makes it equally likely that the electron will be found in either half of the box. We then introduce a barrier between the two halves and separate them, leaving the electron in a superposition of states corresponding to being in the left box and being in the right box. If the probabilities of being in each box are equal, then the state of the particle will be:

$$\psi(x) = 1/\sqrt{2}(\psi_L(x) + \psi_R(x)), \tag{1.2}$$

where $\psi_L(x)$ and $\psi_R(x)$ are wave functions of identical shape but with supports inside the left and right boxes respectively.

Next we give the boxes to two friends Lefty and Righty, who carry them far apart (without ever looking in them of course). In Einstein's original version (in a letter discussed by Fine 1986, p. 35–39, which is a clarification of the EPR argument in its published form), when Lefty looked inside her box – and say found it empty – an element of reality was instantaneously present in Righty's box – the presence of the electron – even though the boxes were spacelike separated. Assuming the collapse postulate, when Lefty looks in her box a state transition,

$$1/\sqrt{2}(\psi_L(x) + \psi_R(x)) \longrightarrow \psi_R(x) \tag{1.3}$$

occurs. In the familiar way, either some kind of spooky non-local 'action' occurs or the electron was always in Righty's box and quantum mechanics is incomplete, since $\psi(x)$ is indeterminate between the boxes. Of course, this experiment does not allow signalling, for if Righty now looks in his box and sees the electron, he could just as well conclude that he was the first to look in the box, collapsing the superposition. And the long run statistics generated by repeated measurements that Righty observes will be 50 : 50, electron : empty, whatever Lefty does – they can only determine the correlation by examining the joint probability distribution, to which Righty, at his wing, does not have access.

In the present case the situation is far more dire, for Righty can use our non-collapsing gravitational field to 'see' what the wave function in his box is without collapsing it. We simply imagine that the right-hand box is equipped with apertures that allow gravitational waves in and out, and that Righty arranges a gravitational wave source at one of them and detectors at the others.[2]

Since the scattering depends on the form of the wave function in the box, any changes in the wave function will show up as changes in the scattering pattern registered by the detectors. Hence, when Lefty now looks in her box – and suppose this time she finds the electron – Righty's apparatus will register the collapse instantaneously; there will be no scattering source at all, and the waves will pass straight through Righty's box. That is, before Lefty looks, the electron wave function is $\psi(x)$ and Righty's gravity wave scatters off $\psi_R(x)$; after Lefty collapses the electron, its state is $\psi_L(x) + 0$ and so Righty's gravity wave has no scattering source. And since we make the usual assumption that the collapse is instantaneous, the effect of looking in the left box is registered on the right box superluminally. So, if Righty and Lefty have a prior agreement that if Lefty performs the measurement then she fancies a drink after work, otherwise she wants to go to the movies, then the apparatus provides Righty with information about Lefty's intentions at a spacelike separated location.[3]

It is crucial to understand that this experiment is *not* a variant of 'Wigner's friend'. One should absolutely not think that scattering the gravity wave off the electron wave function leads to an entangled state in which the gravity wave is in a quantum superposition, which is itself collapsed when measured by the detectors, producing a consequent collapse in the electron wave function. Of course, such things might occur in a theory of quantum gravity, but they cannot occur in the kind of theory that we are presently discussing: a theory with a *classical* gravitational field, which just means a theory in which there are no quantum superpositions of the gravitational field. There is in this theory no way of avoiding signalling by introducing quantum collapses of the gravitational field, since there is nothing to collapse.

It is also important to see how the argument depends on the interpretation of quantum mechanics. On the one hand it does not strictly require the standard interpretation of quantum mechanics, but can be made somewhat more general. In our example, the component of the wave function with support on Righty's box went from $\psi_R(x)$ to 0, which is a very sharp change. But the argument doesn't need a sharp change, it just needs a detectable change, to $\epsilon\psi_R(x)$, say. On the other hand, it is necessary for the argument that normal measurements can produce effects at spacelike separated regions. For then the gravitational waves provide an abnormal way of watching a wave function without collapsing it, to see when such effects occur. Thus, an interpretation of quantum mechanics that admits a dynamics which prevents superluminal propagation of any disturbance in the wave function will escape this argument. Any no-collapse theory whose wave function is governed at all times by a relativistic wave equation will be of this type.

The conclusion of this horn of the dilemma is then the following. If one adopts the standard interpretation of quantum mechanics, and one claims that the world is divided into classical (gravitational) and quantum (matter) parts, and one models quantum–classical interactions without collapse, then one must accept the possibility of superluminal signalling. And further, though practical difficulties may prevent one from ever building a useful signalling device, the usual understanding of relativity prohibits superluminal signalling, even in principle. Of course, this interpretation of relativity is a subtle matter in a number of ways, for instance concerning the possibility of Lorentz-invariant signalling (Maudlin 1994) and even the possibility of time travel (see, e.g. Earman 1995a). And of course, given the practical difficulty of performing such an experiment, we do not have definitive empirical grounds for ruling out such signalling. But since the kind of signalling described here could pick out a preferred foliation of spacetime – on which the collapse occurs – it does violate relativity in an important sense. Thus, someone who advocates a standard interpretation of quantum mechanics, a half-and-half view of the world and a no-collapse theory of classical–quantum interactions must deny relativity as commonly understood. They would need a very different theory that could accommodate the kind of superluminal signalling demonstrated, but that also approximates the causal structure of general relativity in all the extant experiments. (Note that this conclusion is in line with our earlier, more general, argument for the existence of a theory of quantum gravity; and note that the present argument really only demonstrates the need for a new theory – it does not show that quantizing the metric field is the only way to escape this problem.)

Of course, as mentioned, one *might* be able to avoid this horn of the dilemma by opting for a no-collapse interpretation of quantum mechanics, e.g. some version of Bohmian mechanics, or Everettian theories. We are not aware of any actual proposal for a half-and-half world that exploits this possibility (e.g. Bohmian quantum gravity – see below – aims to quantize the gravitational field). But the space may exist in the logical geography. In Bohm's theory, however measurements can have non-local effects on particle positions. Signalling could therefore occur if scattering at the gravitational field depended on the particle configuration and not only the wave function.

Let's turn to the other horn of the dilemma, where now we suppose that gravitational interactions can collapse quantum states of matter. Interestingly enough, there are a number of concrete suggestions that gravity should be thus implicated in the measurement problem, so it is perhaps not too surprising that attempts to close off this horn are, if anything, even less secure.

Eppley and Hannah's (1977) argument against a collapsing half-and-half theory is that it entails a violation of energy–momentum conservation. First, we assume that when our classical gravitational wave scatters off a quantum particle its wave function collapses, to a narrow Gaussian say. Second, we assume that the gravity wave scatters off the collapsed wave function as if there were a point particle localized at the collapse site. Then the argument is straightforward: take a quantum particle with sharp momentum but uncertain position, and scatter a gravity wave off it. The wave function collapses, producing a localized particle (whose position is determined by observing the scattered wave), but with uncertain momentum according to the uncertainty relations. Making the initial particle slow and measuring the scattered gravity wave with sufficient accuracy, one can pinpoint the final location sharply enough to ensure that the uncertainty in final momentum is far greater than the sharp value of the initial momentum. Eppley and Hannah conclude that we have a case of momentum non-conservation, at least on the grounds that a subsequent momentum measurement could lead to a far greater value than the initial momentum. (Or perhaps, if we envision performing the experiment on an ensemble of such particles, we have no reason to think that the momentum expectation value after will be the same as before.)

As with the first argument, the first thing that strikes one about this second argument is that it does not obviously depend on the fact that it is an interaction with the gravitational field that produces collapse. Identical reasoning could be applied to any sufficiently high resolution particle detector, given the standard collapse interpretation of measurement. Since this problem for the collapse interpretation is rather obvious, we should ask whether it has any standard response. It seems that it does: as long as the momentum associated with the measuring device is much greater than the uncertainty it produces, then we can sweep the problem under the rug. The non-conservation is just not relevant to the measurement undertaken. If this response works for generic measurements, then we can apply it in particular to gravitationally induced collapse, leaving Eppley and Hannah's argument inconclusive.

But how satisfactory is this response in the generic case? Just as satisfactory as the basic collapse interpretation: not terribly, we would say. Without rehearsing the familiar arguments, 'sweeping quantities under the rug' in this way seems troublingly *ad hoc*, pointing to some missing piece of the quantum puzzle: hidden variables perhaps or, as we shall consider here, a precise theory of collapse. Without some such addition to quantum mechanics it is hard to evaluate whether such momentum non-conservation should be taken seriously or not, but with a more detailed collapse theory it is possible to pose some determinate questions. Take, as an important example, the 'spontaneous localization' approaches of Ghirardi, Rimini, and Weber (1986) or, more particularly here, of Pearle and Squires (1996). In their models, energy is indeed not conserved in collapse, but with suitable tuning (essentially smearing matter over a fundamental scale), the effect can be made to shrink below anything that might have been detected to date.[4]

Whether such an answer to non-conservation is satisfactory depends on whether we must take the postulate of momentum conservation as a fundamental or experimental fact, which in turn depends on our reasons for holding the postulate. In quantum mechanics, the reasons are of course that the spacetime symmetries imply that the self-adjoint generators of temporal and spatial translations commute, $[\hat{H}, \hat{P}] = 0$, and the considerations that lead us to identify the generator of spatial transformations with momentum (cf., e.g. Jordan 1969). The conservation law, $d\langle\hat{P}\rangle/dt = 0$ then follows simply. But of course, implicit in the assumption that there is a self-adjoint generator for temporal translations, $\hat{H}$, is the assumption that the evolution operator, $\hat{U}(t) = e^{-i\hat{H}t/\hbar}$, is unitary. But in a collapse, it is exactly this assumption that breaks down: so what Eppley and Hannah in fact show is only that in a collapse our fundamental reasons for expecting momentum conservation fail. But if all that remains are our empirical reasons, then the spontaneous localization approaches are satisfactory on this issue, as are other collapse models that hide momentum non-conservation below the limits of observation. Thus, the incompleteness problem aside, sweeping momentum uncertainty under the rug need not do any harm.[5] In this respect, it is worth noting that if gravitational waves cause quantum jumps, then the effect must depend in some way on the strength of the waves. The evidence for this assertion is the terrestrial success of quantum mechanics despite the constant presence on Earth of gravity waves from deep space sources (and indeed from the motions of local objects). If collapse into states sharp in position