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Why Quantize Gravity (or Any Other Field for That Matter)?

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The quantum gravity program seeks a theory that handles quantum matter fields and gravity consistently. But is such a theory really required and must it involve quantizing the gravitational field? We give reasons for a positive answer to the first question, but dispute a widespread contention that it is inconsistent for the gravitational field to be classical while matter is quantum. In particular, we show how a popular argument (Eppley and Hannah 1997) falls short of a no-go theorem, and discuss possible counterexamples. Important issues in the foundations of physics are shown to bear crucially on all these considerations.

1. Introduction. Our theory of the very small, quantum mechanics, appears to be incompatible with our theory of the very large, general relativity. 'Quantum gravity' is the research program that seeks to develop a third theory that will consistently handle both quantum fields and gravitational phenomena. Currently there is no quantum theory of gravity in the sense that there is, say, a quantum theory of gauge fields, but there do exist many more-or-less developed approaches to the task, e.g., superstring theory and canonical quantum gravity.

Those brave or foolhardy enough to examine work in this field encounter a strange, sometimes fascinating, sometimes terrifying new world of physics. Here we find 1-dimensional objects tracing out worldsheets in

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10 dimensions, 3-geometries evolving (or not) with respect to a super-Schrödinger equation, and little, if any, contact with even potentially observable events. And that is just in conventional 'old' quantum gravity! In less established (though by no means fringe) theories we find spin foam networks, area and volume operators, topology change, and the idea that the world is a kind of hologram.

How should philosophers react to this zoo of speculative ideas? We believe quantum gravity offers philosophers of science several worthy projects (e.g., the essays in Callender and Huggett 2001). But one that seems particularly well-suited to the philosopher is to step back from these hypotheses and ask why we need a theory of quantum gravity in the first place. Must we follow such paths? We would like to ask two questions in this spirit: do we need a theory of quantum gravity, and if so, must we quantize the gravitational field? We will answer the first briefly, for we believe the affirmative answer is (or should be) relatively uncontroversial. Most of what follows is devoted to our answer of 'no' to the second question, for we need to argue against the widespread claim—dating back to Bohr and Rosenfeld (1933)—that it is inconsistent to have quantized fields interact with nonquantized fields. As we will see, it is surprising that philosophers have not really tackled this issue, for it involves many familiar issues in the foundations of physics.

2. Why Quantum Gravity? The first question to be addressed is 'why bother at all?' After all, we have no unequivocal experimental evidence conflicting with either general relativity or quantum mechanics: no observation is known to require a quantum theory of gravity for its explanation. It is of course the energy and size scales of the theory that make experiment so difficult. To give a feel for these, one might imagine determining the form of the gravitational field at the atomic scale by measuring the gravitational contribution to the ground state of the hydrogen atom. But Feynman (1995, 11) calculates that such an interaction would change the wavefunction phase by just 43 arcseconds after one hundred times the age of the universe! In these circumstances, can't we just leave well enough alone?

At least two philosophical positions might suggest such a stance, based on epistemological and metaphysical interpretations of the situation described. First, consider the instrumentalist who conceives of scientific theories merely as tools for predicting phenomena. Noting that we are ignorant of such phenomena for quantum gravity, she might view the endeavor to find a theory of quantum gravity as empty and misguided speculation, perhaps of formal interest, but with no physical relevance. However, instrumentalism spells the death of progressive science if it follows this train of thought. Science cannot just wait around for new phenomena, but progresses in part through theoretical advances that produce new phenomena. And quantum gravity is taking strides in this direction. For example, Ellis et al. (1999) explain how both photons from distant astrophysical sources and laboratory experiments on neutral kaon decays may be sensitive to quantum gravitational effects. Even an instrumentalist should accept that theoretical advances could make quantum gravitational phenomena accessible, quite possibly in the near future. So an instrumentalist cannot dismiss quantum gravity as an empty exercise.

Then again, one might argue that the present happy cohabitation of quantum mechanics and general relativity in the experimental domain reflects an underlying disunity of the realms: perhaps general relativity describes certain aspects of the world, quantum mechanics other distinct aspects, and that is that. According to this view, physics (and indeed, science) need not offer a single universal theory encompassing all physical phenomena. Now, whatever the general merits or flaws of this metaphysics, if physics aspires to provide a complete and consistent account of the world, as it traditionally has, then there must be a quantum theory of gravity, for the following reason.

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. But it might be that the world splits neatly into systems appropriately described by one and systems appropriately described by the other. In almost every situation treated by contemporary physics, from electrons to galaxies, one or the other theory serves admirably: For instance, the gravitational effects in a hydrogen atom are negligible, and the quantum spreading of the wavepacket representing the Milky Way won't much affect its motion. But it is an essential consequence of these theories that they govern the same systems. There is no division of the world into gravitational and quantum mechanical systems. This is, of course, because general relativity, and in particular, the Einstein field equation,

$$G_{\mu\nu} = 8\pi T_{\mu\nu}, \tag{1}$$

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 caused by 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, nonperturbative 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 can be, if not unified, at least made consistent with one another.

Given the enormous success of the quantum field program in treating the electromagnetic and nuclear interactions of matter, one obvious unification strategy is to attempt to treat the gravitational field using the same techniques. Presumably this would mean first breaking the spacetime metric, $g_{\mu\nu}$, into two parts, $\eta_{\mu\nu} + h_{\mu\nu}$, representing a flat background spacetime and a gravitational disturbance respectively. Then one would look for a quantum field theory of $h_{\mu\nu}$ propagating in Minkowski spacetime, like the other forces. Such a program goes against the grain of general relativity since it apparently relies on a non-dynamical background spacetime, but if successful it would show that a fully dynamical spacetime only existed in a classical limit. Unfortunately, no one has succeeded in formulating a theory in this way because the gravitational coupling is crucially different from those of the standard model: it is nonrenormalizable. Thus the approach that worked so well for the other forces of nature does not seem applicable to quantum gravity. So the array of programs—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 more radical lines of attack. What this suggests is that the philosophical consequences of quantum gravity may well go beyond those of quantum theory, to radically alter our conceptions of space and time. One can refer to Callender and Huggett 2001 for more detailed discussions of these matters; here we pursue the question of whether quantum gravity means *quantizing* gravity.

3. No-Go Theorems? We may need a theory to treat systems subject to strong quantum and gravitational effects, but that does not imply that one must take classical relativistic objects such as the Riemann tensor or metric field and quantize them: i.e., make them operators subject to non-vanishing commutation relations. Of course, such an approach is a natural one to adopt, but one could instead try to find a theory in which quantum matter fields interact with classical gravitational fields. However, there are several standard arguments purporting to show that such a 'half-and-half' theory cannot exist: that the world cannot be half-quantized-and-half-classical. If correct, and if we must quantize matter, it follows that we must also quantize the gravitational field. In this section we will examine these arguments—which bear strongly on foundational issues in quantum mechanics—and show that they fall short of strict no-go theorems. Though all major programs do involve quantization of the gravitational field, there is no logical requirement that they should.

The first kind of argument is based on a famous paper by Bohr and Rosenfeld (1933) that analyzed a semiclassical theory of electromagnetism in which 'quantum disturbances' spread into the classical field. To simplify, they are often taken to 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. In the present case the uncertainty in the position of a gravitating quantum body would, the argument goes, lead to quantum uncertainty in the gravitational field; ergo it must be quantized. Now, Brown and Redhead (1981) demolish the 'disturbance' view of the uncertainty principle that underpins these arguments, so we will not pursue them here. We would like to note, however, that Rosenfeld (1963) denied that the 1933 paper showed any inconsistency in semiclassical approaches. He felt that only empirical evidence, not logic, could force one to quantize fields; in the absence of such evidence "this temptation [to quantize] must be resisted" (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" (356). We concur.

The argument that we consider is due to Eppley and Hannah (1977) and reformulated by us, not by omitting important details of the physical processes involved, but by avoiding some extraneous complicating elements (see also Page and Geilker 1981 and Unruh 1984). Suppose first that the gravitational field was relativistic (that is, Lorentzian) and classical (that is, not quantized, not subject to uncertainty relations, and without superpositions of gravitational states that make the classical field indeterminate). As we shall note, it is the assumption that the field is classical that does the work in the argument, not that it is gravitational. Thus this argument, if correct, would show that no classical field can interact with quantum matter. Let us also for now assume the standard interpretation of quantum mechanics, whereby a measurement interaction instantaneously collapses the wavefunction into an eigenstate of the relevant observable. Now we ask how this classical field interacts with quantized matter, for the moment keeping all possibilities on the table. Eppley and Hannah see two (supposedly) exhaustive cases: gravitational interactions either collapse or do not collapse quantum states.

3.1. No-collapse Interaction. Take the second horn of the dilemma first: suppose the gravitational field *does not* collapse the quantum state of a piece of matter with which it interacts. Then—given our assumptions—

we can send superluminal signals, in violation of relativity, as canonically understood. There are any number of ways to achieve signaling (see Eppley and Hannah 1977; Squires and Pearle 1996) but they are all based on the idea that the outcome of the gravitational interaction depends on the wavefunction of the matter involved: in particular, the way a classical gravitational wave scatters off a quantum object depends on the spatial wavefunction of the object, much as it depends on a classical mass distribution. In this case one can observe the form of a wavefunction by observing how a gravitational wave scatters, without collapsing the wavefunction. And this—with important caveats—allows one to observe the effects of distant measurements on entangled systems.

Perhaps the simplest illustration of the situation relies on a version of Einstein's 'electron in a box' thought experiment. One first prepares a single electron (or perhaps a microscopic black hole) in a box and with a quantum state that makes it equally likely to be found in either half of the box. Then we 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:

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

where $\psi_L(x)$ and $\psi_R(x)$ are wavefunctions of identical shape but with supports inside the left and right boxes respectively. Next, two friends, Jill and Ben, take the boxes and, without looking in them, carry them far apart. In Einstein's version of the experiment, Jill now looks in her box—say finding it empty—simultaneously producing an 'element of reality'—an electron—in Ben's box. Assuming the collapse postulate, when Jill looks in her box a state transition

$$1/\sqrt{2}(\psi_L(x) + \psi_R(x)) \rightarrow \psi_R(x) \tag{3}$$

occurs. If quantum mechanics is complete then it seems that a spooky nonlocal action occurs. Of course, Jill and Ben cannot use this effect to signal, for Ben cannot tell by local observations on his box whether Jill has looked or not. In the single case, if he looks and sees an electron he cannot tell whether he is observing a state already collapsed by Jill's measurement of an empty box or whether he is collapsing the state himself. And even in the long run, Ben will see an electron half the time regardless of whether Jill looks in each case. But suppose that Ben could see in his box without collapsing the wavefunction, and observe its form rather than the presence or absence of an electron. Then he could register the local change produced by a nonlocal collapse and conclude that a distant measurement had taken place: Jill could signal to him. But this situation is precisely what a noncollapsing gravitational wave allows.

Suppose that Ben's box is equipped with apertures to allow gravitational waves in and out, that Ben arranges a gravitational wave source at one of them and detectors at the others, and that he observes the form of the scattered waves. There are three possible forms for the wavefunction in his box: $1/\sqrt{2}\psi_R(x)$ if Jill has not looked in her box; $\psi_R(x)$ if she has and found it empty; 0 if she has and found an electron. We assume that how the gravitational wave scatters depends on what is in Ben's box—indeed, if it interacts locally at all with the electron then the last case must produce a different result than the other two—so his detectors will register differently depending on what is in the box. So, without collapsing it himself, Ben can determine if and when Jill performs a measurement on her box; when she does, the wavefunction collapses, so the wavefunction in his box changes and he sees the detectors jump. And since we are assuming that any collapses occur instantaneously, Ben has this information faster than any signal can travel to him at finite velocity. So, if Jill and Ben have a prior agreement that if Jill performs the measurement then she fancies a drink after work, otherwise she wants to go to the movies, then the apparatus provides Ben with information about Jill's intentions at a spacelike separated location. (Aharonov and Vaidman (1993) claim that their similar 'protective observations' do not allow signaling in this fashion.)

Note that this example is *not* a variant of 'Wigner's friend'. One should absolutely not think that scattering the gravity wave off the electron wavefunction 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 wavefunction, or anything like that. One can imagine such theories, but we are considering half-and-half theories in which the gravitation field is *classical*, and such fields by definition do not have quantum superpositions but are always in definite configurations. One cannot hope to avoid signaling in such a theory by bringing in quantum collapses of the gravitational field, since there is nothing to collapse. Thus, as we noted earlier, the assumption of classicality is crucial. On the other hand, inspection of the argument reveals that nothing hangs on the field being gravitational, just that it not cause collapse and that scattering depend on the wavefunction.

The other crucial assumption is that of instantaneous collapse, since that is the mechanism by which Jill interacts with Ben; thus the argument depends on the interpretation of quantum mechanics. On the one hand, it can be generalized somewhat. In our model the collapse was completed instantaneously, but the arrangement doesn't need a sharp change to work, just an instantaneous change: as long as the wavefunction in Ben's box changes at all, faster than light can reach him, then he will detect a change occurring at a spacelike separated location. On the other hand, the argument won't work if measurement only produces disturbances in the wavefunction that propagate subluminally. And there are interpretations of just this kind. For example, in a no-collapse theory, all the effects of measurement on the wavefunction are encoded in the wave equation, and if this is properly relativistic, measurement effects cannot propagate superluminally. In particular, a relativistic Bohmian or Everettian theory could be of this type, and so could interact with a non-collapsing classical gravitational field without allowing signaling. In fact, most Bohmians interested in quantum gravity have not pursued half-and-half theories, preferring instead to find a Bohmian dynamics for the gravitational field, but nothing in this branch of the argument forces such an approach. (In Bohm's theory, measurements can have nonlocal effects on particle positions, so presumably signaling could occur if scattering of the gravitational field depended on the particle configuration and not just the wavefunction.)

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 signaling. But the usual interpretation of relativity prohibits superluminal signaling even in principle. Now, the connection between relativistic spacetime structure and a prohibition on superluminal signaling is a contentious matter (e.g., see Maudlin 1994, especially Chapter 4, for a discussion of Lorentz invariant signaling). But the kind of mechanism we have sketched represents the worst case scenario: it places no restrictions on the kinds or amounts of information that can be transmitted, or on the possible locations of the transmitters and receivers. In this case, avoiding causal paradoxes requires that the signals propagate along an *empirically determinable* preferred foliation of spacetime. Since Lorentzian spacetimes do not, in general, have such structure, this consequence would violate a fundamental aspect of relativity theory. (Known relativistic Bohmian theories also require a preferred foliation on which the 'quantum potential' propagates, but this situation is less drastic since familiar arguments show that the preference is not observable.) Of course, were one to perform some variant of our experiment it might turn out that there is superluminal signaling, and that a half-and-half theory is correct and relativity wrong: the effects are in practice so small that we have at present no direct evidence for or against them. However, 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 an important element of relativity.

3.2. Collapse Interaction. On the other horn of the dilemma we consider the possibility that a classical gravitational field interacts with quantum matter in a way that *does* induce quantum state reductions. Now, this possibility is rather interesting. For if one believes that quantum collapses occur as a matter of course in the interactions of quantum systems, and that some single mechanism must mediate all such collapses, then the fact that all matter interacts gravitationally with all other matter suggests the gravitational field as a candidate mechanism. We are hardly the first to notice this hint; Roger Penrose and others have long argued for theories along these lines. We shall have to explain how they avoid the argument that collapsing half-and-half theories are impossible, but these theories are counter-examples to Eppley and Hannah's 'no-go theorem'.

The argument against such theories is that they must violate energy-momentum conservation if they respect the Heisenberg uncertainty relations. Assume some particular form for the interaction: suppose, for instance, that when a classical gravitational wave scatters off a quantum particle, the particle state collapses to a narrow Gaussian; and that the wave itself scatters as if the Gaussian were a classical matter field. Then Eppley and Hannah's argument is straightforward: prepare a quantum particle in a sharp but low momentum state; scatter a gravitational wave off it as described; then measure the outgoing wave accurately to pinpoint the particle location; by the uncertainty relations the particle now has a highly indeterminate momentum. In principle, the initial particle momentum can be arbitrarily low and sharp. And in principle, the scattered wave, being classical, determines the exact form of the final wavefunction Gaussian, and so by the uncertainty relations fixes a finite minimum for the momentum uncertainty. So in principle, the system can be prepared to produce a finite difference between the initial momentum and the final momentum uncertainty.

(In fact, Eppley and Hannah make a stronger claim than this. They model the interaction so that the wave scatters off a point source. In this case measuring the outgoing wave can determine the location of the particle with arbitrary accuracy, and hence produce arbitrary uncertainty in the final momentum. They conclude that there can be an arbitrarily large difference between initial momentum and final momentum uncertainty. But in this model it seems that the final particle state depends, not just on the interaction, but how accurately one later decides to measure the scattered wave: that decision effects how sharp the position is. Though such mixing of dynamics and epistemology is sadly all too common, we can make little sense of it. Anyway, our weaker claim serves Eppley and Hannah's argument just as well—or in fact, badly—as their stronger one.)

Eppley and Hannah conclude that we have a case of momentum non-conservation, presumably on either of two grounds: that a subsequent momentum measurement can have an outcome finitely different from the initial momentum; or that in an ensemble of such experiments the momentum expectation value will not be a constant. One is again struck by the fact that this argument does not depend on the interaction being with a gravitational field. Any accurate position measuring device that causes collapses would do: apply it to a particle with a low but determinate momentum and obtain a particle with high indeterminacy in its momentum. But the standard collapse interpretation of quantum mechanics assumes that such devices do exist, so if this argument succeeds then it also rules out that interpretation. Since this problem is rather obvious, we should ask whether it has any standard response. And of course 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 nonconservation 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 the 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 specific addition to quantum mechanics it is hard to evaluate whether momentum nonconservation 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). 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.

But is it satisfactory to treat conservation as a purely empirical matter in this way? Eppley and Hannah's argument would still work if there were in principle grounds for the conservation postulate: collapsing half-and-half theories and spontaneous localization theories and the conventional collapse theory would be impossible in principle. Are there such grounds? In quantum mechanics, the theoretical justification for the postulate is, first, that the spacetime symmetries imply that the self-adjoint generators of temporal and spatial translations commute, $[\hat{H}, \hat{P}] = 0$, so that the expectation value of $\hat{P}$ is conserved. And second, one has reasons to identify the generator of spatial transformations with the observable for momentum (cf., e.g., Jordan 1969). But implicit in the assumption that there is a self-adjoint generator for temporal translations, $\hat{H}$, is the assumption that the evolution operator, $\hat{U}(t) = \exp(i/\hbar \cdot \hat{H}t)$, is unitary. But the very idea of a collapse interpretation is that this assumption fails during state reduction. Hence our fundamental reasons for expecting conservation do not apply in collapses, hence there is no in principle ground for conservation that applies to collapse theories in general. And it hardly counts against a collapse theory that it violates a principle of a no-collapse theory, but this is all that Eppley and Hannah can say. Thus there are neither empirical nor in principle grounds for ruling out collapse interactions. Consequently, we can escape this horn of the dilemma by simply denying the premise that momentum must be strictly conserved.

Indeed, there are sketches of quantum-gravitation interaction that do precisely this. The spontaneous localization model developed by Pearle and Squires (1996) includes collapses that are caused by the gravitational field of a collection of point sources punctuating independently in and out of existence. The rate at which collapse occurs depends on the mass of the sources and rate of source creation. By suitable tuning, one can ensure that, on the one hand, the amount of energy produced by collapse is undetectable by present instruments; and that on the other, responding to the first horn of our dilemma, the collapse rate is great enough to prevent Ben from observing Jill's actions from afar. Thus, the theory, though just a toy, is another counterexample to Eppley and Hannah's 'no go theorem'. Rosenfeld 1963 is right. Empirical considerations must create the necessity, if there is any, of quantizing the gravitational field.

4. The Semi-Classical Theory. We have argued that half-and-half theories are possible, contrary to often cited arguments, but that does not mean that the approach is very promising. For balance we should conclude by discussing the best developed specific suggestion for a half-and-half theory (due to Møller 1962 and Rosenfeld 1963) to illustrate why the approach is in fact not very promising. This theory, 'semi-classical quantum gravity' (though any half-and-half theory is in a sense semi-classical), postulates that the spacetime geometry couples to the expectation value of the stress-energy tensor:

$$G_{\mu\nu} = 8\pi\langle\hat{T}_{\mu\nu}\rangle_\Psi. \quad (4)$$

$G_{\mu\nu}$ is the *classical* Einstein tensor and $\langle\hat{T}_{\mu\nu}\rangle_\Psi$ is the expectation value for the stress-energy observable given that the *quantum* state of the matter fields is Ψ. This expression is just the Einstein field equation with the most obvious classical quantity that can be obtained from the quantum state of matter.

Now, (4) and the classical equation (1) are rather different in character. The latter is complete, giving the dynamics of both matter and spacetime, but the former only imposes a consistency requirement, since an expectation value underdetermines the quantum state. This means that a separate, Schrödinger dynamics for matter is also required and that one must seek 'self-consistent' solutions to the pair of equations. Finding a model should proceed by first picking a spacetime—say a Schwarzschild black hole—and solving the Schrödinger equation for the matter fields on the spacetime. But this field and spacetime will not satisfy the field equation, since we have not taken into account the effect of the matter field on the spacetime. So we use (1) to recalculate the spacetime. Now we have to solve a new Schrödinger equation for the matter fields on the new spacetime. But the new quantum state will not satisfy the field equation . . . and so on. One hopes that this process converges on a spacetime and matter field that satisfy both equations, but in the absence of such solutions it is not even clear that the equations are mutually consistent.

This lack of unity is one reason that physicists by-and-large do not take the semi-classical theory seriously as a 'fundamental' theory. Instead it is used as an heuristic guide to some suggestive results; in the case just described, 'Hawking radiation' is produced by quantum fields in a Schwarzschild spacetime, and if one could feed the 'back reaction' into the semi-classical equation one would expect to find the black hole radius decreasing as it evaporated. Unfortunately severe technical problems face even this example, and though research is active and understanding increasing, there are still no known interesting solutions with evolving spacetimes in four dimensions (Wald 1994).

Things look even worse if one believes that the dynamics of quantum matter involves collapses. (1) supplements the Schödinger equation, so one might hope that it somehow actually contains what we discussed in the previous section, namely a gravity mediated collapse dynamics. But this cannot be so. Turning around an argument of Unruh's (1984), it is impossible for (4) to contain a sharp collapse: the Einstein tensor is necessarily conserved, $G^{\nu}_{\mu;\nu} = 0$, but a collapse would lead to a discontinuity in the stress-energy expectation value, $\langle \hat{T}^{\nu}_{\mu} \rangle_{;\nu} \neq 0$. So the theory needs to involve the semi-classical field equation and a Schrödinger equation *and* a separate collapse dynamics. And if the collapses are discontinuous then Unruh's argument shows either that (4) is incorrect (as he suggests), or that the LHS is not a tensor field everywhere, but only on local patches of spacetime, with 'jumps' in the field outside the patches.

And surely these considerations are related to the so-called 'loss of information' problem (e.g., Belot, Earman, and Ruetsche 1999): as a black hole Hawking evaporates according to the semi-classical theory there is a transition from a pure to mixed state for the matter fields, reminiscent of measurement collapse. Such an evolution is impossible under unitary evolution but the system is also driven by the non-unitary field equation. But modeling the situation is very difficult because the field equation cannot actually drive a quantum collapse.

The prospects for the semi-classical theory *as a candidate fundamental theory* and for half-and-half theories in general are thus dim; while they are not impossible, if one weighs the insights they offer against the epicycles they require for their maintenance, they do not appear to be terribly progressive. And it is really this reason, and not Eppley and Hannah's arguments, that motivates most physicists to attempt to quantize gravity when they seek a theory of quantum gravity. We have no qualms with this kind of argument, so long as it is recognized that the need for such a theory is not one of logical or (yet) empirical necessity. We do however think that foundational problems in quantum mechanics may prove important in finding a theory of quantum gravity, so that care must be taken when considering what ordinary quantum theory might show about such a theory.

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