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Is Quantum Gravity Necessary? *

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17.1 Introduction

Quantum gravity presents something of a unique puzzle for the philosophy of science. For in a very real sense, there is no such thing as quantum gravity. Despite near unanimous agreement among physicists that a quantum theory of gravitation is needed to reconcile the contradictions between general relativity and quantum mechanics, there are no pressing empirical issues that require this resolution—the regime in which one would expect to observe a conflict between the claims of general relativity and quantum mechanics is at the Planck scale. Thus the question naturally arises "Why quantize gravity?" Are there other issues that compel us to seek a quantum theory of gravity?

The standard response is intimately connected with a desire for theoretical unification. Quantum field theory successfully describes the physical world on small length scales at low "particle" density. General relativity is a successful theory of large length scales where individual features of particular objects are swamped by their mass-energy properties. It is natural to seek a unified theory that captures these successful features and yet is somehow a "fundamental" theory of both regimes. But why should the resulting theory involve a quantized gravitational field? There is clearly *something* wrong with the general relativistic treatment of matter fields as classical. Very well. Let us stipulate that an acceptable theory of gravitation will take due note of the quantum nature of the fields to which it couples. Now what? Are we thus compelled to treat the gravitational field itself quantum mechanically?

There are a number of arguments urging the necessity of a full quantum gravity—i.e., a theory of gravity that treats the metric itself as a quantum field. There are, as well, a number of proposals for how we should go about producing this theory. I will not here be concerned to articulate the panoply of attempts to quantize gravitation theory (nor to elaborate the many prob-

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lems attendant to that effort). I will instead present a partial catalogue and evaluation of the various reasons for constructing such a theory. These reasons group themselves naturally into three reasonably distinct classes. There are what I will call problems of experiment, problems of theory and problems of meta-theory respectively. In the first class are included experiments either actually performed or detailed thought experiments. In the second class are those problems of a theoretical nature that appear to derail efforts to avoid quantization in the absence of experimental evidence. The last class will then contain, in particular, implicit as well as explicit philosophical motivations for quantizing gravity. It is this last class, I will argue, that is really responsible for the conviction (quite widespread in the physics community) that gravitation is necessarily a quantum mechanical phenomena.

I will begin with an account of the typical problems of experiment that are offered as definitively settling the question of gravitation in favor of quantization. My account, perhaps, will not be exhaustive, but I believe it captures the flavor of the reasons of "experimental" physics and shows their inadequacy. I follow this account with some remarks about the theoretical difficulties of producing a realistic non-quantized gravitation theory that takes due notice of the quantum mechanical nature of the matter producing the gravitational field. These difficulties are not trivial, and, more to the point, I cannot resolve them. But they are no worse, at least on their face, than many of the difficulties facing those who would construct a fully quantized theory of gravity.

If I am right about these first two points, then meta-theoretical commitments of some kind are at the root of efforts to quantize the gravitational field. Focusing on just one type of commitment—theoretical unification—I argue that (as is often the case with very general principles) it fails to entail the conclusion it is used to justify. I conclude from this that the real justification for quantizing gravity has yet to be articulated.

The theory on which I will focus, in what follows, is the semi-classical theory of gravitation. Of the possible approaches to avoiding the quantization of gravity, this theory has been the most studied (albeit only as an approximation technique). Moreover it instantiates Rosenfeld's (1963) suggestion that a realistic theory of gravity could be one where the (classical) Einstein tensor is proportional to the expectation value of the (quantum) stress-energy operator.

In this theory, one constructs a quantum field theory on a curved spacetime and then allows the stress energy tensor to couple to the Einstein tensor via the semi-classical Einstein equation:

$$G_{\mu\nu} = k\langle T_{\mu\nu}\rangle.$$

For details and a particular construction of $\langle T_{\mu\nu}\rangle$ see (Wald 1999).

There are other approaches to a non-quantized gravity but I will not mention them here. It should be kept in mind, however, that even if the semiclassical proposal is inadequate, the case against treating the gravitational field classically is not, thereby, decided.

Problems of experiment

In 1975 a thought experiment was conducted (if that is indeed what one does with thought experiments) by Eppley and Hannah (1977) purporting to show that the gravitational field must be quantized. They assume the validity of semi-classical gravity, and use a gravity wave to measure the position and momentum of a macro-scopic body such that $\Delta p_x \Delta x < \hbar$, thus violating the Heisenberg uncertainty principle. The key idea is that a classical wave may have arbitrarily low momentum and, simultaneously, arbitrarily short wavelength. This observation already conflicts with the de Broglie formula relating momentum to wavelength $\lambda = h/p$. But the whole point to taking seriously the semi-classical theory is to avoid directly applying quantum mechanics to the gravitational field. In order to find a conflict with quantum mechanics, it is necessary to couple such a low wavelength/low momentum gravitational wave to a quantum system. The wave may then be used to localize a particle within one wavelength while introducing vanishingly small uncertainty into the particle's momentum. Eppley and Hannah's experiment does just this.

Their experiment is, however, unrealistic in a number of ways. I cannot discuss this here, and perhaps the defects could be remedied in a different version of the experiment. Even so, their case that gravity must be quantized would still not be made. And this for two reasons: it may be that the uncertainty relations *can* be violated. They haven't really been tested in this way. Second, there are empirically adequate interpretations of quantum mechanics for which these relations are epistemological and not a fundamental feature of the world. Thus the thought experiment cannot be considered definitive.[1]

At the second Oxford symposium on quantum gravity another empirical problem for semiclassical gravity was articulated. Professor Kibble (1981) there argued for the viability of semiclassical gravity. He proposed a thought experiment that, strangely, now has come to be interpreted as having a significance diametrically opposed to the one he offered for it. His experiment consisted of a Stern–Gerlach magnet that first separated the spin up and spin down components of a particle's wave function and then passed these components by particle detectors. Detection of the particle released a heavy mass to one side or the other of the device according to the component of spin possessed by the detected particle. The whole device was included in a black box to prevent outside observation. Kibble pointed out that the standard story of quantum measurement (assuming the Einstein tensor proportional to the expectation value of the stress-energy tensor) implies that, on measurement of the spin of the particle, there would be a physically unrealistic jump in the gravitational field. The lesson he drew from his thought experiment is that the semiclassical theory of gravity would require a new theory of quantum measurement. But, he continued, we already knew that quantum measurement theory is a mess.

Page and Geilker (1981), on the other hand, regard Kibble's result as damning but not definitive evidence against a semiclassical gravitation theory. For them, its only defect is one it shared with Eppley and Hannah's experiment—it hadn't been performed. They propose to remedy this lack of experimental evidence against semiclassical gravity by performing a concrete test of the theory. Their test is essentially a classical test of the gravitational response of a torsion balance to the presence of macroscopic masses. The quantum feature is entirely captured by the method of choosing the locations of these masses. The choice is determined by what amounts to a quantum random number generator; depending on the value of some quantum variable, the masses will be sent either to the left or the right of the balance. Page and Geilker find, not surprisingly, that the balance responds only to the presence of mass and not the expectation value of where the mass *will* go.

Why should anyone have expected a different result? How does this count against the semiclassical theory? Apparently anyone using Everett's relative-state formulation of quantum mechanics would expect the torsion balance to remain fixed. On this formulation, the wave-function never collapses. Instead it "branches" out into new worlds with the distribution of worlds governed by the standard measurement probabilities of quantum mechanics. Since under this interpretation the wave function never collapses, Page and Geilker claim that the semiclassical Einstein tensor responds to the presence of matter in all branches of the universe. For example, taking $G_{ab} = k\langle\psi|T_{AB}|\psi\rangle$ as the semiclassical Einstein equation and $|\psi\rangle = c_1|\phi_1\rangle + c_2|\phi_2\rangle$ then, even if measurement shows that, after some interaction, $|\psi\rangle$ in our branch appears to have collapsed to $|\phi_2\rangle$, we still have

$$G_{ab} = k(c_1^* c_1\langle\phi_1|T_{ab}|\phi_1\rangle + c_2^* c_2\langle\phi_2|T_{ab}|\phi_2\rangle).$$

Page and Geilker perform a quantum experiment that they plausibly assume affects only a small subspace of the total wave-function of the universe and so casts $|\psi\rangle$ into a state like $(\frac{1}{\sqrt{2}}|\phi_1\rangle + \frac{1}{\sqrt{2}}|\phi_2\rangle) \otimes |\psi_{everythingelse}\rangle$. They set their masses according to the result of their experiment and assume, again plausibly, that Page and Geilker in the other branch do the same. Since their experiment shows that the balance responds only to the matter in our own branch of the multi-verse, they conclude that this version of semiclassical gravity, with their particular interpretation of quantum mechanics, is empirically inadequate.

In 1981, when their experiment was performed, they offered an argument that was supposed to show that only an Everett style interpretation is compatible with semiclassical gravity. Their argument assumes first that the only choices for an interpretation of quantum mechanics are (instantaneous Copenhagen style) collapse and Everett. They then claim that semiclassical gravity is incompatible with collapse. To this end they consider a superposition state $|\psi\rangle = \sum_i c_i|\phi_i\rangle$ and, in the Heisenberg picture, calculate the covariant derivative:

$$\langle\psi|T_{ab}|\psi\rangle_{;b} = \sum_{ij}(c_i^* c_j)_{;b}\langle\phi_i|T_{ab}|\phi_j\rangle \neq 0 \equiv G_{ab;b}.$$

For example, if $|\psi\rangle$ is a superposition of eigenstates of T, the expectation value for the energy may change during measurement. That is to say, if

$|\psi\rangle = c_1|\phi_1\rangle + c_2|\phi_2\rangle$ where the $|\phi\rangle$ are eigenstates of T and our experiment is a measurement of T, then after the (instantaneous) measurement the $c_i s$ have changed discontinuously and it would seem miraculous if the total change had derivative 0. Then the semiclassical Einstein equation becomes inconsistent. $G_{;b}$ is identically 0 but $T_{;b}$ need not be. This argument is not entirely convincing since I can see no reason to suppose the wave function of the universe to ever be in other than an eigenstate of the stress-energy tensor (in much the same way that for conservative systems, the total state is an eigenstate of the hamiltonian). But I won't make an argument to that effect here. I will point out that Wald (1994, 78–89) has constructed a prescription for measurement in semiclassical gravity which can be given a collapse interpretation and which also satisfies $\langle T_{ab}\rangle_{;b} = 0$. So already there is trouble with Page and Geilker's interpretation of the significance of their experiment. I won't pursue this here, but instead I will question another of Page and Geilker's assumptions.

Because they assume that the only possible interpretations of quantum mechanics are Cøpenhagen and Everett's relative state formulation, they conclude that if semiclassical gravity fails for Cøpenhagen and the relative state formulation, it fails for quantum mechanics generally. Once again, what we are really up against is the quantum measurement problem. Because they use the results of a quantum mechanical measurement to set their device (a measurement well separated from their device), by the time it is set the quantum decision is already made. So *only* a no-collapse interpretation can be used to draw conclusions about their experiment. For only on that view does the expectation value of the wave function continue to reflect the entire state of the "multi-verse." But there are many other interpretations of quantum mechanics for which the expectation value is updated as our knowledge of the wave function is updated. And for other no-collapse models the quantum state is exhaustive in the way it is classically—all values of all observables are definite all the time. This is true of the quantum logic interpretation for example. So for someone using a quantum logic interpretation of quantum mechanics, it would not make a great deal of sense to equate the Einstein tensor with the expectation value of the stress-tensor. In such a case we would not take the expectation value seriously, of course, so we would have to modify the interpretation of semiclassical gravity slightly. The natural seeming approach in that interpretation would be to equate the Einstein tensor directly to the stress-tensor in the way it is done in classical general relativity. Naturally how one would accomplish this is not obvious, but I only intend here to make it clear that, even if the semiclassical approach as outlined does fail (and at least experimentally (thought and otherwise) we don't have good evidence that it does), this failure does not constitute strong evidence that gravity is itself quantum-mechanical.

I won't go any further into this here, but I do wish to emphasize that only a peculiar[2] reading of quantum mechanics is incompatible with Page and Geilker's result. It is certainly interesting that some versions of some interpretations are incompatible with semiclassical gravity. But it is *only* interesting. Other no collapse models—such as the de Broglie–Bohm theory, and modal interpretations—as well as continuous-collapse models are apparently unaffected by these results.

Here we have an interesting footnote to debates about the proper interpretations of quantum mechanics. I have no solution to the quantum measurement problem. Nor do I find wholly satisfying any of the extant proposals for solving it. Yet I cannot worry overmuch about a proposal for a gravitation theory merely because it fails to solve the problem, or rather because it undermines one or two of the proposed solutions.

As far as I know, this is the extent of experimental evidence for quantum gravity. I think it falls somewhat short of showing the inadequacy of any semiclassical gravitation theory, including the standard, naive $G_{ab} = \langle T_{ab} \rangle$ prescription I've been considering. What is most interesting though is precisely the paucity of the evidence. There are not many experiments and those there are have not been looked at very carefully. That the problems with the experiments (and their interpretation) are so obvious and yet entirely unremarked shows, I think, that the experiments hold very little interest for researchers either in quantum mechanics, general relativity or quantum gravity. What this indicates is that, for most, the issue is not to be decided on the basis of experimental investigation. Given the deep and persistent scrutiny applied to experiments that merely confirm predictions of well established theories (the discovery of the top quark for instance), this lack of attention indicates that the conviction that gravity is quantized derives from another source.

Let me now turn, very briefly, to theoretical problems.

17.2 Problems of Theory

Mathematical physicists have identified a number of problems with the formulation of a viable semi-classical gravitation theory. These are outlined in a number of places. The following list is compiled from (Wald 1994) and (Butterfield and Isham 2001).

- The expectation value $\langle T_{\mu\nu} \rangle$ needs to be regularised to avoid divergences. Wald has done this, but there remains an ambiguity in its definition. Since his regularisation procedure is not scale invariant, there is a problem determining two conserved local curvature terms. The presence of a natural length scale for the theory would resolve this ambiguity, but it is not clear how to determine this scale.
- Some solutions of the semi-classical Einstein equations are unstable. Small changes in initial conditions produce dramatically different solutions. Some solutions have runaway behavior. Thus we need a way to distinguish physically acceptable solutions from those that are not.
- There is trouble with choosing the quantum state. "In addition," observe Butterfield and Isham, "if $|\psi_1\rangle$ and $|\psi_2\rangle$ are associated with a pair of solutions γ_1 and γ_2 to [$G_{\mu\nu} = k\langle T_{\mu\nu} \rangle$], there is no obvious connection between

γ_1 and γ_2 and any solution associated with a linear combination of $|\psi_1\rangle$ and $|\psi_2\rangle$. Thus the quantum sector of the theory has curious non-linear features, and these generate many new problems of both a technical and a conceptual nature."

These are serious problems and not mere chimeras to be banished in the bright light of philosophical reflection. But neither are they so profound to have alone derailed a dedicated research program. In the last few decades, the quantum gravity community has faced extraordinary challenges—many of which resulted in unqualified defeats for the quantum gravity program. For example the non-renormalizability of quantum gravity cast serious doubt on the proposition that the tools developed for quantum field theory could be of any use in quantizing gravity. The sheer number of programs that have flourished and then, in turn, withered in the field of quantum gravity indicates the diversity and severity of the problems to be overcome before a full theory of quantum gravity may be harvested. These problems, any one of which, perhaps, is as severe as all those facing a non-quantized gravity program, have served rather to energize than to daunt the quantum gravity community. So since neither empirical evidence nor theoretical issues suffice to make the case, why then is the conviction that gravity must be quantized so pervasive?

17.3 Problems of Metatheory

While a dedicated research program could have withstood the various conundrums outlined above, the truth of the matter is that no real research program ever sprang up. Despite Leon Rosenfeld's urging that physicists take seriously the possibility of coupling classical to quantum field, few ever did. Indeed, I know of no-one, since Wald's axiomatization of QFT in CST, who has seriously proposed trying to treat such a theory as fundamental. Why not? It is here that we encounter meta-theoretical positions.

Some, no doubt, are convinced by the arguments mentioned above. For example, Wald (1999), when asked what is wrong with the semi-classical Einstein equation, repeated essentially Page and Geilker's many-worlds objection. But others in the quantum gravity community seem motivated by more abstract concerns. Hawking, Salam, Davies and many others have advocated quantum gravitation as an essential part of a unified physics. Others advocate a unificationist position without articulating it explicitly. For example, Carlo Rovelli (2001) maintains that "we have learned from GR that spacetime is a dynamical field among the others, obeying dynamical equations, and having independent degrees of freedom. A gravitational wave is extremely similar to an electromagnetic wave. We have learned from QM that every dynamical object has quantum properties, which can be captured by appropriately formulating its dynamical theory within the general scheme of QM.

"*Therefore*, spacetime itself must exhibit quantum properties. Its properties, including the metrical properties it defines, must be represented in quantum mechanical terms. Notice that the strength of this "therefore" derives from the confidence we have in the two theories, QM and GR." It seems clear that Rovelli is using some kind of thesis about the unity of nature to extend our evidence of quantized fields to cover the case of the gravitational field. While not a complete sampling, I will take it that an important meta-theoretical impetus for quantizing gravity follows from notions of unification.

One hears a great deal about the unity of science but is rarely sure what is meant by the phrase. Oppenheim and Putnam (1958) faced, some years ago, the same quandary. Wishing to elevate the unity of science to a provisional regulative principle for the theory of science, they found it necessary first to specify its connotation. They began by enumerating three concepts of unity: 1. "Unity of language" where all the terms of science may be defined using those of one discipline; 2. "Unity of Laws" where all the laws of science can be reduced to those of one discipline; 3. "Unity of Science in the strongest sense" where the internal structure of the distinguished discipline is unified. Their treatment of the subject is not entirely relevant to my present concern. Theirs was a vision of a hierarchical structure whose various levels could be seen as reducible to the level below until, at the base, would be found a single discipline capable of supporting the entire edifice.

Considerable effort, by philosophers of science, has been expended, depending on what view of science is being promulgated, trying either to undercut or to bolster this conception. But one part of Oppenheim and Putnam's definition plays no part in their analysis. This is the idea that unity means that the single sciences must be internally unified. Clearly though it is this latter conception of unification that is at issue in discussing the unification of GR and the Standard Model. Here reductionist issues do not, at least at first blush, come into play. For here there is no suggestion of a deeper, more fundamental level of description. That is to say, we are not, for example, attempting to reduce a discipline like chemistry with its own laws to physics and in the process to show that its laws can be derived from those of physics, or that there are no new fundamental entities or processes, beyond those of physics, involved in chemical reactions. Rather, we are attempting to evaluate two distinct approaches to constructing a fundamental theory of physics. Already we are at the level of fundamental physical interactions and desire a comprehensive and, yes, unified account of the interactions. We wish to know if unification can decide the issue for us.

To find out, the first step is to answer the question "What is the nature of this unity?" Is it a unity of ontology, of methodology, of predictive content or of something else? A very general division can be made among theses of unification. On the one hand there are theses concerning nature itself—that it is unified in some way. On the other hand are theses about how to do science—the logical form theories must take, rules of thumb for constructing new theories, etc. This division corresponds to Morrison's (1994) and, loosely, to Hacking's (1996) (he introduces a further division into theses of how to do science). The idea expressed by both Hacking and Morrison is that there is no necessary connection between the unity of nature and the unity of scientific method. Morrison uses the example of the electro-weak theory. In constructing the theory, physicists were guided by analogy with electromagnetism. Then later they found that electromagnetism and the weak force could be subsumed under a single theoretical framework and thus unified. Morrison argues that this unification is not complete in the sense of a unified ontology. For example, electromagnetic interactions are mediated by a massless force carrier while the force carrier in weak interactions is massive. This distinction comes from the particular way in which the electroweak symmetry is broken, but nevertheless, it introduces a sharp division in the ontology of the two theories. So here we have a unity of theoretical structure but no unity of ontology.

Ian Hacking has spoken out against the idea of a unified science. He advocates an increasingly popular pluralism in the sciences and, in particular, rejects the idea that some kind of universal method characterizes scientific activity. While remaining neutral concerning his conclusions, I will adopt certain of Hacking's accounts of unity which, I think, illuminate the issue of unification and quantum gravity. Hacking provides a tripartite taxonomy of unification "theses": metaphysical, concerning what there is; "practical precepts"; and theses of scientific reasoning—e.g., logical and methodological imperatives. Of these, only the metaphysical will interest me here. For I am not so much concerned with the implementation of unificationist ideals as with the ideals themselves.

Under the heading of metaphysical theses, Hacking includes three distinct notions—interconnection, structure and taxonomy. The first of these implies that, at root, all phenomena are related in some way, that no class of phenomena can be fully characterized in isolation. His example is Faraday's conviction that light must be affected by magnetic fields. (Lest this example be misunderstood, let me make clear that Faraday's claim here is not that light and magnetism are aspects of the same electromagnetic field, but rather that *all* phenomena are to some degree mutually conditioned.) As Hacking characterizes it, such a thesis is fully compatible with a non-quantized gravity along the lines of QFT in curved spacetime. Clearly such a theory allows for, indeed demands, fundamental interaction between gravitational and quantum fields. If the unity of physics is to be characterized in this way, such unity provides no clear motivation for quantizing the gravitational field. For example, the thesis does not require that all fields share essential features but rather that they have domains of overlap and interaction with each other. In QFT in curved spacetime, we have that.

I will return to the structural thesis which, for Hacking, has the best shot at requiring a quantized gravitational field and first address the taxonomic thesis: "there is one fundamental, ultimate, right system of classifying everything: nature breaks into what have been called 'natural kinds'." By itself this thesis is entirely consistent with a classical gravitational field. It is already commonly supposed that there is something unique about this field that makes it stand out on its own. To claim that all fields, objects and what have you may be uniquely specified according to some overarching taxonomical classification adds little of relevance to the project of finding out what this unique something might be.

On the other hand, a denial of the taxonomical thesis might prompt one to wonder if the semi-classical approach is coherent at all. For example, if the gravitational field cannot be notionally separated from the electromagnetic field, then it makes no sense to quantize one and not the other. I will not address this issue fully, but will attempt to deflect it as follows: if the thesis is rejected we may not *appeal* to taxonomic unification in constructing our theories. But on the other hand, we may still find, as a technical matter, that we are able to construct a theory that does in fact distinguish between the gravitational fields and others. My opinion is that, even among those who deny the taxonomic thesis about *things*, few will maintain the much stronger view that *no* separations can be made between the types of entities, fields, processes etc. that populate the world. It is this stronger claim that is required to undercut the semi-classical approach to gravitation.

Then of Hacking's metaphysical theses, only the structural remains. His characterization of this thesis is somewhat vague. He appeals to Wittgenstein's notion that "there is a unique fundamental structure to the truths about the world." The idea seems to be that we can discover all the truths about the world if we have access to the core truths—these presumably include those of logic, mathematics and some central or fundamental physical principles. As far as I am aware, the primary purpose for the structural thesis is to support ideas about the transitivity of scientific confirmation. For example, Michael Friedman (1983) uses the image of a unified structure of scientific truths to illustrate how such diverse phenomena as gas behavior and chemical bonding can be explicated by appeal to the molecular hypothesis. On this basis, he argues that unified theoretical structures can be better confirmed than can disunified structures. This is because confirmation can come from a wide variety of phenomena. Note though that this use of the structural thesis tells us very little about the nature of the scientific truths involved. The postulation of a unified structure of truths may be useful for many different purposes, but not for specifying the *content* of a good theory.

Hacking alludes to an extreme version of the structural thesis. This version affirms the existence of a single "master law" that is sufficiently rich to allow for the derivation of all other laws from it alone. It is only this version, he says, that would fall should there be "no connection between gravitational phenomena and electromagnetic phenomena." Despite the dubiousness of the existence of such a law, I still do not see any fundamental problem its existence would pose for QFT in curved spacetime. The claim would have to be that some one physical process is at root the sole process operative in the world, and that all other processes are successive concatenations and permutations of this one process. So here's a master law: the quantum fields of the standard model propagate in a curved spacetime and the "back reaction" of these fields on the metric is governed by the semi-classical Einstein equation. This is not very impressive as master laws go, but it seems to satisfy the principle in question.

Finally I want to correct a significant defect in Hacking's account of the metaphysical theses of scientific unity—he does not mention the most obvious of the claims a unificationist might make. This is the claim that there is, at root, one and only one kind of matter.

If the claim is meant as an expression of concern about unduly inflating the number of entities the theory identifies, then there seems no reason to decide in favor of full quantization over the semi-classical picture. There are no extra things associated with semi-classical gravity that are absent in a full quantum gravity. Indeed the graviton, or whatever is presumed to carry the gravitational force, is absent in semi-classical gravity. Here the metric couples directly to the stress energy of the quantized matter field. There is, to be sure, the metric field on spacetime, but it would, in any case, be present in full quantum gravity. The ontological structure of the theory suffers no enlargement in the semi-classical case. A unification strategy based on parsimony of ontology thus affords no advantage to quantizing the Einstein tensor.

One could make the more radical ontological objection that it is precisely the presence in the theory of both classical and quantum fields that decides the case for quantization. Surely this duality is contrary to the very idea of a unified quantum description of nature. But I take this to be nothing other than the point at issue. Does unification, as a guiding principle rule out the co-existence of classical with quantum fields? To answer this question by identifying unity with a thorough-going quantum mechanical description is to not answer at all. One could, presumably level the same sort of objection against the claims of unification of the electro-weak theory. One might adopt a position according to which unification demands electrically neutral force carriers. The suggestion might be that, for a unified ontology, the nature of the fields must be consistent and thus that a charged force carrier violates ontological unity. Again, it might be objected that quantization is just not like properties such as charge and mass but is instead an essential feature of all fields. This claim may be correct. But it can hardly be taken as a principled objection to its own denial. It is a very specific claim—all fields are, of necessity quantized. Such a claim cannot be regarded as simply following from the very idea of unification.

If we adopt the unity of physics as a legitimate consideration in constructing our theories, then, for this principle to do any work, it must be sharpened. As it stands the ideal of unification tells us very little about the nature of the world. It is thus incapable of determining which theories can best describe the world.

What is the point to all this? If very general principles, like the unification of physics, are inadequate as motivations for quantizing gravity, perhaps we should seek elsewhere. For better or worse, the semi-classical project is dead. It appears that diagnoses of its demise are as varied as the programs that have replaced it. In my opinion, there is much to be learned from this observation alone. There is no single motivating principle driving the search for quantum gravity. Instead particular programs may be seen as individual responses: not to a common problem but rather to a common conviction arising from a number of different problems. To know how well a given program has succeeded we must, in part, understand the problems it is meant to solve, and the various approaches have their own sets of problems. Why quantize gravity? That is a question that ought to be reserved for particular programs of quantization, and one whose answer will, ultimately, shed light on the methods and success of these programs.

It is, I believe, worthwhile to observe that no single explanation exists for the conviction that gravity must be quantized. As a corollary, there is no consensus about what is expected from a quantum theory of gravity. To become aware of this point is to recognize the possibility of classifying programs in quantum gravity according to their motivations. In the absence of ready data for evaluating the success of quantization programs, some other criterion must be made available. Quantum gravity is sometimes portrayed as a panacea for the troubles afflicting a world described very well by GR in one regime and in another by QFT. What I wish to point out is that some of these problems may not exist as they are commonly understood. More importantly, I wish to observe that sharpening our ideas about what is wrong with non-quantized gravity will make it clear which of these problems can be expected to submit to solution via quantization and which will not. Such clarity is essential in judging the success of various quantization programs.

My thesis is simple. Standard arguments from physics as well as general (but not necessarily uncontroversial) arguments from the theory of science do not compellingly indicate that gravitation theory should be quantized. I consider this to be an important foundational issue in physics in its own right. But I suggest that, producing better arguments favoring quantization, may, as an added bonus, result in new insights into how best to quantize gravity.

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Notes

[1]Since the presentation of this paper at HFGR5, I have shown that, for example, their measurement device exits only within its own black hole. For a presentation of this and other problems see (Mattingly 2001) as well as (Calendar and Hugget 2001, 6–12). Both of these present arguments that are much more careful than the hasty remarks above.

[2]The raw "many-worlds" understanding of Everett's relative state formulation of quantum mechanics is rarely taken seriously. Most advocates of that formulation now combine it with some version of decoherence or consistent-histories for which the claim that the expectation value after an experiment is the same as it was before the experiment is not at all obvious. See, e.g., (Omnès 1999) for a discussion of how a decoherence reading of the many worlds interpretation prevents interference in a "real" measurement (Chapter 19), and also, in general, how to understand this interpretation without requiring the kind of overlap envisioned by Page and Geilker.