The Paracletes of Quantum Gravity

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1. Background for Quantum Gravity

The physics community is embroiled in a profound and far-reaching revolution, a revolution that will overturn the bedrock theories of matter, spacetime, and gravity on which almost all of the physical theories of the 20th century depend. This revolution is pressing and urgent and now is the time to choose up sides. At stake is no less than the theory of quantum gravity!

However, since the revolution is now in at least its 55th year, it may be time to take stock and make some suggestions about what might be slowing it down, and about what strategic moves might advance the cause.

In the 20th century physicists developed two of the most successful descriptions of nature ever produced, general relativity and quantum mechanics. General relativity, Einstein's theory of gravitation, rejects the notion of gravitational force and replaces it with a conception of gravitational interaction carried out by changes in the actual geometry of space and time, now spacetime; these changes in geometry reflect the distorting effect of matter and energy in spacetime. The story is complicated in its details, but simple in its conception: the presence of matter distorts the geometry of spacetime where the matter is located, and these changes are propagated at the speed of light throughout spacetime; on the other hand, matter not subject to outside forces travels on geodesics, the straightest paths in spacetime. This mutual interaction between spacetime and the matter-energy fields therein gives rise to

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[1] I am grateful to helpful comments from the attendees of the Synthesis and the Growth of Knowledge conference. I would like to thank Thomas Ryckman especially for helpful clarifying remarks about Cassirer's basic views.

behavior that, in the limit of small masses (planets, moons), reproduces the appearances of Newton's theory of gravity but that deviates from the predictions of that theory in the case of larger masses (suns, galaxies). As is well known, the predictions of general relativity have been born-out by observation, and it has superseded Newtonian gravitation theory.

Quantum mechanics is at the heart of our current best theories of matter. According to quantum mechanics, matter is significantly different than our classical intuitions suggest. Classically we expect that matter (whether it is ultimately grainy or continuous) behaves deterministically, and that given the state of one small bit of matter at one time (its mass, its velocity, its position, the forces acting on it) we can predict, at least in principle, its state at any future time. In practical terms, carrying out this operation proves to be quite complicated, even in the classical picture, since attending to all the forces acting on the bit of matter requires knowledge of the state of many other particles, and since our observations will inevitably be less than perfect. But still it is generally taken to be the case that in principle, once all these niceties are taken into account, the behavior of matter is deterministic classically for generally well-behaved systems. (See Earman 1986 for discussion and important qualifications.) In the quantum theory things are quite different. Even given a specification of the entire state of a quantum system, it is not possible, even in principle, to specify the future values of all variables that can be associated with some bit of matter in that system. Views of how to articulate the situation differ, but most will agree that there is more that one might want to know about a system than is contained in the most complete physical description of that system. For example, we tend to consider it inappropriate to apply at the same time properties like momentum and position with arbitrary precision to the same bit of matter. The predictions of this theory have also been born-out, to an unprecedented degree of precision, and it has superseded classical conceptions of matter.

One additional feature is shared by both quantum mechanics and general relativity:

they take their proper domain of validity to be all of nature. There are regimes in nature where the importance of one or the other for making predictions is minimal, but even in those regimes the theories are supposed to be applicable. It is then important to determine whether the two theories are compatible. For if not, at least one must be wrong. As typically stated, the theories do appear to be incompatible. For quantum field theory – the version of quantum mechanics developed for use with special relativity[2] – is usually taken to require a fixed, non-dynamical spacetime and so it is incompatible with general relativity; and general relativity is a theory about classical spacetime and is usually understood to apply to classical matter fields and so it is incompatible with quantum field theory. One might then conclude that what is called for is a theory that captures the best insights of both theories, a theory that recognizes the indeterminacy of matter and the dynamical character of spacetime, a theory in which the gravitational field is also a quantum field. That theory would be quantum gravity.

There is, in fact, a theory that does incorporate the best features of quantum field theory and general relativity – the semiclassical theory of gravity. The semiclassical theory is really a cluster of theoretical approaches that preserve the classical nature of the gravitational field while showing how to do quantum field theory on a dynamical spacetime. There is near universal agreement in the physics community that the semiclassical theory is untenable as a final theory of gravity. However, despite this widespread agreement, there are very few actual arguments that are offered as support for that agreement. (I will rehearse some of these in what follows.) Moreover, none of those arguments is any good. "Any good" really is the right expression here: of the arguments that aren't obviously false on their face, each is

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[2] This is yet a third fundamental development of the 20th century. The theory claims that all inertial systems are equivalent for the description of nature, and that the speed of light, regardless of the speed of the source of that light, has the same value in all of these frames. It follows from these claims that simultaneity, time intervals, and space intervals are all relative to the inertial frame in which they are measured. Special relativity is related to general relativity in that in very small regions of spacetime, general relativity reduces to special relativity. What to say beyond that is subject to considerable debate.

obviously question begging, or requires a minimal grasp of the physics of general relativity and quantum mechanics to refute, so that it is hard to believe that anyone is persuaded by any or all of them. Indeed I have argued elsewhere (Mattingly 2005, 2006) that it is best to think of these arguments as excuses for not investigating the semiclassical theory rather than as attempts to establish its non-viability. We can understand the community best, I believe, by taking the non-viability of semiclassical gravity as a primitive insight rather than a hard won conclusion. But we can also see that so taking semiclassical gravity, even if it *is* non-viable, obscures any real flaws the theory might have, and thereby obscures the real question that a fully quantized theory of gravity might be an answer to. Since certain field theorists on the cutting-edge of attempts to quantize gravity have now recognized that they may be asking the wrong questions, and since they have invited input from philosophers of science and begun to engage with their projects (see, e.g., Isham 1993, Rovelli 2001), what follows here may be seen as an attempt to show how we might go about finding the right question. I do not know the right question that when answered will produce a theory of gravity for quantum mechanical matter, and certainly I don't know the right answer. What I offer here is a general suggestion for how the practice of physics can itself provide the right question. Attempts to produce, ab novo, a quantum theory of gravity are not necessarily misguided, and no argument I make here should be taken as an attempt to establish that they are. Rather, I want to suggest that there is a method that has historically produced great results, a method that begins with our current practice. The method aims to extend the domain of that practice as widely as possible, aims to elaborate the claims of that practice as precisely as possible, and in pursuit of those aims, finds the basic flaw in the current practice and brings it into view. The method, which I outline below, shows how to transform that flaw into the right question, whose answer is the transformation in our practice that results in a theory that properly unites gravitation theory with quantum mechanics. I call this method the

"Lorentzian strategy"; I will elaborate it below.

2. Cassirer and Scientific Theory Change

For analysts of scientific communities and the roles such communities play in the development of science, quantum gravity provides an important and ongoing field of study. Similarly, theorists of the development of science concerned with questions of the rationality of scientific transitions should be very interested in developments here; the issue of whether and how well practitioners of the various stages, theories, and general approaches are able to communicate with and understand each other is particularly pressing given the importance in the philosophy of science of Kuhn's work (and reactions to that work) detailing what he identified as an important incommensurability between distinct theoretical paradigms. Here I will explore one account of the development of science, the construction of quantum gravity. The account is a partial reconstruction of Ernst Cassirer's philosophy of scientific theory change. There is, I think, some confusion about how best to understand Cassirer's picture of the world and of man's place in it and I therefore do not wish to engage here with his full project, nor with standard interpretations of his contribution to philosophy.

Instead I will outline an interpretation of the philosophy of science in his *Einstein's Theory of Relativity,* and claim that it contains the prescription for a robust methodology of scientific theory change. While much work remains to be done to explicate fully this prescription and fit it into the larger context of Cassirer's philosophy, I restrict my attention to an application of the prescription to the case of quantum gravity. I argue that some key features of a revolutionary break with prior theory are absent in the current state of attempts to integrate quantum mechanics with general relativity, and I suggest that pursuing aggressively a "Lorentzian" strategy (pursuing the semiclassical theory of gravity) is likely to yield more fruit than the alternative strategy of "quantizing the gravitational field."

Michael Friedman (2000) has suggested that we return our attention to Cassirer, in part to bridge the divide between analytic and continental approaches to philosophy that has grown ever wider in the last few decades. Cassirer's pivotal position in philosophy at the beginning of that divide by itself makes it likely that a nuanced understanding of his mature philosophy will provide foundations for that bridge. But in returning to Cassirer we are rewarded in many other ways as well. Something one notices immediately is that what we have, in Cassirer's account of the development of science, may be seen as the implementation of an early version of a dynamical conception of Kant's critical philosophy. Cassirer's is distinct from the version put forth at roughly the same time by Reichenbach and championed by the logical empiricists, and which is now in part the inspiration for Michael Friedman's own conception of the relativized a priori (Friedman, 2001). Others in this volume address Friedman's understanding of the role of philosophy in providing guidance through the conceptual spaces of science that are enlarged by scientific revolutions, and so I won't address that issue here. Cassirer as well suggests that we see our current scientific conceptions as embeddable in a more comprehensive framework as a special case: Newtonian mechanics as the limit of special relativity, special relativity itself the limit of general relativity. But Friedman and Cassirer will differ on their view of whether there is more than one way to effect this embedding properly in any given case: Friedman will claim that considerations internal to the scientific debate cannot settle, in general, which way to extend a given theory and thus we require the separate discipline of philosophy to guide our choice; Cassirer will claim that there is always a best scientific decision to be made at a given juncture and that the critical philosophy illuminates the epistemological imperative behind this decision.

I begin with Cassirer's account of the transition from Lorentzian electrodynamics to Einstein's special theory of relativity. I then extract from that account a general prescription for fundamental theory change in the sciences – the Lorentzian strategy. Note that for my purposes it suffices to assume that this fundamental change proceeds in roughly the way that Kuhn describes revolutions in science generally do. However, fundamental differences in the Kuhnian and Cassirerean accounts emerge that allow one to characterize the revolutions as progressive and rational when described according to Cassirer's prescription.

I follow, in Section 4, with an account of the semiclassical theory of gravity, and in particular, I address premature reports of its death. While there is an important analogy between the semiclassical theory's relation to quantum gravity and Lorentzian electrodynamic's relation to special relativity, there is an important disanalogy between the approach the physics community is taking to the transition to quantum gravity and its approach to the transition to special relativity. One important aim here is to redirect the physics community to the latter approach. We will see that it was important for the transition to special relativity that Lorentzian electrodynamics had gone as far as it had. This general point of Kuhn's is worth emphasizing: revolutions happen under the pressure of anomalies that eventually precipitate a crisis in the earlier framework. And it is important that the revolutionary developments proceed in reaction to those anomalies. Section 4 shows in an abbreviated manner that such burdens are simply not present in the case of semiclassical gravity. Absent these, it isn't clear what a theory of quantum gravity should be fixing.

The Sections 3 and 4 provide, I think, a strong analogical argument that semiclassical gravity is worth pursuing in our quest for a fully quantized gravity theory. I turn finally in Section 5 to canvassing some current physics research for possible routes to quantum gravity that start with the semiclassical theory. There is such work, and it rewards our attention: analysis of the regime where quantum effects (fluctuations, superpositions, entangled states) are of roughly the same magnitude as gravitational effects (tidal forces, massive curvature irregularities, significant gravitational radiation) is ongoing and very interesting. The programs of Barcelo, Liberati and Visser, Verdaguer, Hu, Calzetta, and Martin, and Gunzig and Saa, for example, are, in my estimation, applications of the Lorentzian strategy in quantum gravity.

Let me note here that the situation in quantum gravity has changed significantly in the last decade. It is no longer the case that no approach to quantum gravity is showing progress. And yet the problems seem as severe as they ever were – the new approaches are, at this point, merely mathematical exercises. The "data" appealed to by the various groups of quantum gravitation theorists are very old, and all programs are responsive to it. Thus the data alone cannot help us to choose among approaches. What is needed is either some other way to choose among basic approaches or a method of producing new approaches. I think the Lorentzian strategy can perform both tasks: it provides an impetus to new experiment, and a clear expression of the limitations in the semiclassical theory that will have to be overcome in a quantum theory.

3. The Lorentzian strategy

The Lorentzian strategy is a conservative approach to theory change that requires that we work stepwise with minimal mutilation of the current theory. Given certain resources and presented with certain data, Lorentz set out to produce a fusion of Maxwell's electrodynamics and Newtonian mechanics. In particular he attempts to produce a viable theory of moving charged bodies. He had steady success over the years but was finally presented with empirical and theoretical contradictions.[3]

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[3] Incidentally, it is instructive to see that Lorentz himself provides a counter-example to the radical version of Kuhn's incommensurability thesis. Lorentz is quite capable of understanding, and rationally evaluating his program against that of Einstein. While he sees certain advantages for his program, and indeed thinks that it is possible that his program will be revived in the future, he also can see that Einstein's theory has *scientific* advantages over his own. (See Lorentz 1939.)

Einstein is able to see through this maze of technical apparatus and can finally understand all at once the deep conflict between Newtonian mechanics and Maxwellian electrodynamics that makes them so hard to unify. How did this all happen, and is the story we tell about it generalizable? The honest answer is "I don't know," but my answer here will be "the Lorentzian strategy, and I hope so." In addition to his native genius Einstein had a deep awareness and appreciation of efforts to unite the mechanical and electrodynamical theories. These efforts were precisely the carrying out of the Lorentzian strategy: extending as widely as possible the domain to which the theory is applied, and applying it there with as much precision as possible. This strategy (implicit or explicit) made manifest apparent contradictions between basic principles: one, the constancy of the speed of light for all observers, the other the principle of relativity itself. Einstein showed how to resolve these contradictions, while maintaining the apparently conflicting principles. An investigation of the transition to relativity illuminates another aspect of the philosophy of science that is amenable to a Cassirerean analysis: the nature of scientific theories. It is beyond my purpose here to lay out a full presentation of that view. I have elsewhere begun to address the relation between Kuhn's incommensurability thesis and the theory of scientific theories he appealed to (Mattingly 2004), and while much more remains to be done, the basic insight is this: much of Kuhn's thesis of incommensurability between scientific frameworks relies on a statement view of theories that bears strong resemblance to the logical empiricists' syntactic conception. Ironically, one can carry out much the same analysis and rephrase Kuhn's argument for incommensurability using a model theoretic view of theories along the lines of the semantic conception. But one can show that a hybrid model that is committed to principles as bearers of scientific meaning can both respect Kuhn's historiographical insights into the revolutionary changes in science that punctuate its history, and reject his thesis that science does not progress according to scientific principles.

Here, though, I will focus on just one component of Cassirer's dynamical version of the critical philosophy: the role of what he calls "paracletes of thought" in the development of our scientific theories. In Cassirer's account the paracletes, or awakeners, of thought are apparent contradictions in the older theory that are jointly asserted as axioms for the new theory and which thereby are seen not to conflict after all. In the case of special relativity he finds two pairs, one from the observational side – the Fizeau and Michelson-Morely experiments – and one from the theoretical side – the constancy of the speed of light and the relativity of all motion.

In Fizeau's experiment testing light transmission through liquids, we find that the velocity W of light in a moving medium is $W = w + v(1 - \frac{1}{n^2})$ where w is the speed of light in the medium at rest , v the speed of the medium, and n the index of refraction of the medium. In the Michelson-Morely experiment, on the other hand, we have the famous null result that the speed of light is unchanged with the direction of motion of the earth through its supposed containing medium.

Since we have found that light travels at constant speed regardless of the speed of its source, or of the observer, we advance that constancy as a fundamental theoretical principle. We also assume that any inertial frame is sufficient for the description of natural phenomena, and articulate that assumption in the Galilean transformations between coordinate systems. Here on the theoretical side we are not presented with such a clear apparent contradiction, nor is the resolution *simply* to maintain the truth of both principles. It is necessary to reinterpret the Galilean principle as a special version, in context, of the general prescription of the relativity principle. While the actual equations of transformation must be given up and replaced with their Lorentzian counterparts, we still assert what is regarded as the core content of the principle: that all (inertial) frames are sufficient for the framing of physical law.

Cassirer's discussion of the empirical and conceptual foundations of the theory of relativity makes it quite clear what is at stake. We have taken the theory of Maxwell on the one hand, and the theory of Newton on the other, as far as we can. We have found both an empirical contradiction – the Fizeau versus Michelson-Morely experiments – and an apparent conflict of basic principles – the constancy of the speed of light, and the relativity principle of Galileo. These contradictions, and especially the conflict of basic principles are what Cassirer calls the "paracletes of thought." When we are presented with this basic conflict, we are finally forced to chose between abandoning one of our cherished principles, and abandoning a story about the nature of substance that we had been using to produce the basic conflict. In such an instance, Cassirer claims, the choice is clear, and it is also a scientific choice. Science is in service of the unifying, systematizing drive, and as such, proceeds only according to principles. Thus, when we are finally brought to the point of actual contradiction, we must discover a means to modify the formulation of our basic principles – at whatever cost to our intuitions about the nature of substance, or the essence of matter – in order to bring them once again into harmony.

It is Cassirer's view that only thus can our basic understanding of nature be extended. But if this is so, we must show how we can go about *finding* these points of crisis, a method for producing the awakeners of thought. Whether or not we endorse Cassirer's radical claim – that *only* by identifying apparent conflicts between basic principles, and resolving the contradictions by re-articulating these principles and maintaining them as basic postulates of the new theory – we *can* see the potential advantages of finding such apparent conflicts.

The Lorentzian strategy develops hybrid and apparently transitional theories with an eye toward driving out the kinds of contradiction that were resolved in the move to special relativity. It took careful attention to the twin facts of the constancy of the speed of light and the relativity of motion before we were able to see both the manifest contradiction between the two, as well as the way to resolve that contradiction. In a similar way I suggest, in particular, that it will take sustained attention to the facts of dynamical spacetime and a quantum mechanical stress energy tensor before we can see the actual contradiction between quantum field theory and general relativity (or generalizations thereof). Note that the answer in the case of special relativity was very unlike the apparent question that Lorentz had to deal with. There was a real question of how one could establish a matter theory compatible with the apparent special frame picked out by the ether; a radical relativization of simultaneity was not obviously in the future. So the real question answered by the special theory of relativity was not apparent in advance of Lorentz's efforts. I want to suggest that sustained attention to efforts to extract as much physics as possible from hybrid theories is worthwhile as an important possible source of insight into the true problem that needs to be solved by full-fledged theories.

A partial answer to the question of how Einstein's insight was possible is that the Lorentzian strategy highlighted a conflict between the formulations of basic principles – relativity and the constancy of the speed of light – that was resolved in properly formulating them. Einstein realizes that the actual rule of transformation must depend on other principles, that relativity is not *given by* the transformation function of Galileo (or any other particular function) but that the relativity principle (in concert with the light principle *dictates* the proper transformation function between inertial frames. In partial answer to the question of whether the strategy is generalizable, I offer the example of semiclassical gravity.

We can compare Cassirer's view of Einstein with that of one of the chief architects of loop quantum gravity, itself a chief candidate for the correct theory of quantum gravity, Carlo Rovelli. Rovelli was for years intimately connected with philosophers and historians of science in his position at Pittsburgh University and through his presence on dissertation committees in the Department of History and Philosophy of Science there. According to

Rovelli, one key to understanding Einstein's development of relativity is the latter's twin appreciation of Maxwell's theory and Galileo's principle of relativity. He says:

> Einstein understood that Galileo's great intuition – that the notion of velocity is only relative – *could not be wrong*. I am convinced that in Einstein's faith in the core of the great Galilean discovery there is very much to learn, for philosophers of science, as well as for contemporary theoretical physicists. So, Einstein believed the two theories, Maxwell and Galileo: he assumed that they would hold far beyond the regime in which they had been tested. Moreover, he assumed that Galileo had grasped something about the physical world, which was, simply, *correct*. And so had Maxwell. Of course details had to be adjusted (Rovelli 2001, 116).

The significance for the whole question of how to think about the development of science is contained in this passage. For while the historical impression that Rovelli has of Einstein's theory of science and his development of relativity theory may lack the subtlety of modern views, that impression appears to form part of the core of Rovelli's own theory of science. We see here an important way in which the character of the practice of science develops partly in light of, in indifference to, and in reaction against the work of historians and philosophers of science.

Physicists such as Rovelli, despite their familiarity with philosophical analyses of science, and despite their willingness to engage with philosophers[4] and despite their appreciation of the importance of those historical, philosophical, and sociological studies that undermine simplistic notions of the relentless progress of science toward the absolute truth, are still convinced that they are making progress, that they are learning about the world and advancing beyond the understanding of the world had by their teachers and historical role

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[4] This is a welcome change both from Feynman's "shut up and calculate" injunction of and the blind adherence to naive falsificationism of a few decades past.

models. They are right.

4. Semiclassical Gravity

Is quantum gravity really necessary? Can't we get along without it? Is there truly some crucial reason why all physical fields must be quantum mechanical in nature?

I will use the term "semiclassical gravity" completely generally to apply to any theory that (1) couples a continuous, Lorentzian signature metric to the expectation value (or otherwise suitably averaged and "classicalized") stress-energy quantity (which need not be a tensor field itself) and (2) reduces to general relativity in the classical limit $\hbar \to 0$. Here I can do no more than sketch the standard objections to semiclassical gravity.[5] More extensive treatments can be found in (Mattingly 2005a, 2005b; Callender and Huggett 2001, Introduction) and references therein; what follows is a summary of (Mattingly 2005, 2006). The paucity of evidence and the weakness of what little there is suggest a need to look much harder at the issue of how, and why, and whether semiclassical gravity fails.

Arguments that semiclassical gravity must fail divide nicely into three classes, two of which are based in physics and one in philosophy. There are objections of an experimental nature, of a theoretical nature, and of a metaphysical nature. Let us consider them in turn.

1. Experiment

There are only two experiments that are appealed to in arguments against the semiclassical theory. The first is Page and Geilker's. To test the semiclassical model of gravity they contrive a situation where the position of a large mass is determined by the outcome of a quantum experiment. They then test the gravitational field and show that it is

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[5] Note that even should they succeed, they do not amount thereby to any sort of argument that we should *quantize the gravitational field*, for any number of other options remain. These options include such a radical proposal as deriving both quantum mechanics and relativity from some deeper theory to which they are both only approximations, for example.

affected only by the location of the mass---not partly by the location of the mass and the location of where the mass might have been located had the quantum experiment turned out differently. But since in the semiclassical theory the gravitational field is determined by the expectation value of the mass density, they conclude that the theory is empirically inadequate.

Their analysis is in two parts. It first claims that a model of quantum mechanics that does not involve wave-function collapse cannot be used for semiclassical gravity, because any collapse model must violate the semiclassical Einstein equations. The claim is non-trivial and they do not argue for it except by noting that they don't see how to avoid it. But the burden is on them here in advancing their claim. Moreover, even if their claim is true, it shows only that a modification of their specific model of semiclassical gravity is required, and not that the gravitational field must be quantized. The second claim is then that any no-collapse model requires that expectation values never change. There is *one* version of how to understand measurement in quantum mechanics that *might* support that claim, but they make no argument that we should adopt that version, and most who study this issue agree that we should not. Nor do they show exactly how this interpretation of measurement in quantum mechanics *would* support their claim.

The second experiment suffers from serious flaws of analysis as well. Moreover it hasn't been performed. Eppley and Hannah propose a solar system sized gravity wave detector. They would use it to show that classical wave behavior for gravity is incompatible with the quantum character of matter. I leave aside their argument here except to note that they appeal crucially to an understanding of Heisenberg's uncertainty principle on which it is impossible for a particle to be localized sharply simultaneously in both momentum and position space. This view is rarely defended today by serious students of quantum mechanics since there are empirically adequate interpretations of quantum mechanics for which these relations reflect our ignorance and

are not fundamental features of the world. On the de Broglie-Bohm theory, for example, position and mechanical momentum are always well defined. It is not inconceivable on such an interpretation that one could find a way to measure these quantities simultaneously with arbitrary accuracy; one simply cannot do so using purely quantum mechanical means. Yet the de Broglie-Bohm interpretation is empirically equivalent to the standard, Copenhagen interpretation. Similarly Everett's relative state formulation of quantum mechanics is empirically identical to the Copenhagen interpretation, and particles always have well-defined position and momentum in that formulation. Thus we cannot assume that sharply localizing a particle simultaneously in both momentum and position space is inconsistent with the uncertainty principle. But even granted their argument, their device fails because it is so massive that by the time it was built, it would have fallen into its own black hole. Even by the standards of a thought experiment – one which would be impossible in practice, but conceivable in principle – Eppley and Hannah's proposal fails (Mattingly 2006).

2. Theory

The following remarks are far too hasty, but will have to serve here. General theoretical arguments in the physics literature typically refer either to the incoherence of the semiclassical theory or to its mathematical intractability. But there are satisfiable axioms for semiclassical gravity (see, e.g., Wald 1994). These are not axioms for a full version of the theory, but they *are* sufficient to refute any argument that claims that a theory coupling a classical field to a quantum expectation value is inconsistent. So semiclassical gravity is mathematically consistent. The fact that a classical field can be coupled consistently to a quantum mechanical expectation value is not a guarantee that the semiclassical theory can serve as a final model of gravity, and there are some arguments suggesting that the mathematical problems of constructing such a theory are insurmountable. None of these arguments, however, does more than establish that there are hard mathematical problems that would have to be solved. But after a half century of attempts to construct a full quantum gravity, the fact of mathematical difficulties should perhaps be seen as a challenge to overcome, rather than an excuse not to try.

3. Metaphysics

Sometimes one hears that the unity of nature demands that all physical fields be of one metaphysical type. There are indeed, in the literature on quantum gravity, arguments that attempt to turn quite vague claims about unity into specific arguments against hybrid models of gravity. None of these is very striking. But what is striking is that physicists should take them at all seriously. Without assigning some specific content to the notion of unity it is difficult to see how metaphysical arguments for unity are relevant. Rovelli has offered what seems to be the best attempt at a general argument in favor of quantizing the gravitational field. He tells us that we have learned in the course of the 20th century both that gravity is a dynamical field, and that all dynamical fields are quantum mechanical. Stated this baldly though, the argument is clearly question-begging. But no plausible seeming argument for the general claim has been offered.

It simply has not been established that a semiclassical model of gravity is doomed to failure. Granting even the soundness of arguments prompted by quantum mechanics, which urge a *modification* of general relativity, would not establish that the gravitational field must therefore be quantized. Indeed, semiclassical gravity is a program that grants the former without subscribing to the latter. Some of the most compelling arguments for quantizing the gravitational field are motivated by puzzles that are solved already in the semiclassical regime. For example, there are theorems establishing that general relativity necessarily displays singular behavior. This singular behavior is often taken to show that the theory cannot be complete, and that the gravitational field must be quantum mechanical. But each of these theorems fails in semiclassical theories because of peculiar facts about quantum mechanical matter. That is, coupling the gravitational field of general relativity to quantum mechanical matter fields results in a theory that is not *inherently* singular (Mattingly 2001).

We should recognize that semiclassical gravity is not best understood as a monstrous hybrid that should be eradicated as quickly as possible. Hybrid it may be, but it has a status similar to that of the combination of electrodynamics and mechanics in the latter part of the 19th century – it is the going theory and should be taken seriously. If it turns out to have insuperable flaws, these should be discovered in the course of investigating the theory itself. The payoff of continuing aggressively to pursue the theory is at least two-fold: on the one hand, we learn more about the theory and how it can be used as a tool of empirical investigation; on the other hand, we may learn something crucial about how actually to find a theory of quantum gravity.

5. Lorentzian strategy for quantum gravity

Here I will merely gesture at important work in the semiclassical theory. This work is interesting both for its own sake *as* work in the semiclassical theory, and for what it promises to show us about the weaknesses and limitations of that theory. Many of the objections raised above against the theory are seen to be irrelevant, or easily overcome in this work. And while the theory has not yet broken down, a better sense is emerging for where one should push it to make it reveal the paracletes of quantum gravity.

A very interesting direction is taken by Barceló, Liberati and Visser (2003). They propose the possibility of testing semiclassical gravity using phase perturbations (roughly, distortions in the speed of shock waves) in a physical model of the whole universe – a Bose-

Einstein condensate. Their idea is to tune the interactions of the various components of the model (the elements of the condensate) in order to simulate a Friedman-Robertson-Walker spacetime. Such spacetimes are expected to be very good models of our own universe since, most importantly, they correspond to universes with matter that are expanding in accordance with Hubble's law – where the expansion rate between any two points is proportional to their separation.

Their experiment shows that their model universe naturally divides into two distinct *kinds* of system. They have, on the one hand, a system that looks like an expanding universe much like our own, and, on the other hand, a remainder that looks like particle creation associated with this expansion much like that in the early stages of our universe. While their condensate does not expand, they are able to define a system of coordinates for it such that the radial coordinate is increasing in accord with the Hubble parameter of an expanding spacetime. And while there is no real particle creation in the condensate, there are coherent sound waves (phonons) that behave much like quantum particles. The punchline, of course, is that they calculate that there is a significant opportunity to observe analogues of these effects in our own universe, and test thereby various theories of semiclassical gravity – and that these opportunities are much greater than those associated with other sources of semiclassical scale effects.

So what we see here is a physical model of the a semiclassical universe that can help us to constrain the range of regimes in our own universe that are appropriately described by the semiclassical theory. Of particular interest is that such work will help to model what breakdowns on semiclassical gravity look like as quantum gravitational effects become important. One might expect to see which parts of the semiclassical model are in conflict with each other.

In addition to the analogue effects described by Barceló et al., there are a number of ongoing experiments that are expected to provide sufficient constraints on matter models to eliminate certain of these models for the universe as a whole. The results are very interesting and important in their own right, and suggest that soon candidate models of quantum gravity will be subject to experimental test. But equally interesting for present purposes is how the experiments highlight once again the role the semiclassical theory plays in settling "facts" of cosmological significance. Gunzig and Saa (2004) show that certain models of quintessence (these are theories of exotic matter that is not part of the standard model of particle physics), which are apparently ruled out by singular effects at the transition between attractive and repulsive gravitational regimes, are *not* ruled out in certain models of semiclassical gravity. One crucial point is that the classical action is modified by the inclusion of a total divergence term. While such terms have no bearing in any classical case, here the term, if necessary to reproduce experimental results, would potentially force a modification of the action considered in loop quantum gravity, and hence change its target "reduction regime." That is, changes in the action necessitated by semiclassical considerations produce changes in possible models of one of the most promising candidates for a full quantum gravity.

Finally I mention other work meant to alter the semiclassical theory itself – preserving its basic form while insulating the approach from the apparent problems of the original proposal. Hu and Verdaguer (2002), Calzetta and Hu (2003), and Martin and Verdaguer (2004) have been exploring *stochastic* semiclassical gravity. In the stochastic program a term is added to the matter-energy side of the equation – a term describing the fluctuations in the quantum matter fields. The standard semiclassical theory doesn't include the effects of fluctuations and so is unlikely to be correct in regimes where, for example, fluctuations in matter density are comparable in magnitude to the expected matter density itself. So the addition of an explicit fluctuation term is intended to extend the range over which the semiclassical theory is valid. Without considering specific details I do want to point out that there is a great deal of freedom in the form that such a fluctuation term may take. There is thus a great deal of freedom available now for extending the semiclassical theory to find potential deviations from experiment, and to modify it in the light of those deviations.

Consideration of these three classes of work on the semiclassical theory makes it plausible to suggest that pursuing the semiclassical theory is a viable strategy of the kind undertaken in pre-relativistic electrodynamics. There were attempts to build mechanical models of ether systems dating back to Maxwell's day, attempts to test the theories by consideration of boundary effects, and importantly, efforts to extend the theory by incorporating new classes of interaction into the theory (ether contraction effects, e.g.). These all have analogues here in the semiclassical theory.

I am interested in how the Lorentzian analogy plays out here. And so I want to advance the idea that we may see, on the one hand, an apparently ad hoc maneuver meant to save semiclassical gravity and show how to extend its domain of applicability by a suitable augmentation of the right hand side of the basic equation for semiclassical general relativity. This is the explicit introduction of a stochastic fluctuation term in the stochastic program of Calzetta, Hu, Martin, and Verdaguer addressed above.[6] On the other hand, we may see an attempt to track down the observational defects of the entire class of semiclassical gravitation theories in the observation of quantum effects in the metric either directly or analogically. This is seen in Gunzig and Saa's (2004) and in Liberati, Barceló and Visser's (2003) work, respectively. We see here the theoretical and observational components of the strategy that we saw in the case of special relativity. However, I am sensitive to the danger of forcing the analogy and so want to make clear that the interest of semiclassical approaches for philosophy of science transcends the analogy.

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[6] Of course, they don't see their work in this light, but neither did Lorentz apparently. Instead I believe both "groups" are engaged precisely in the effort to extend their respective theories as far as possible. If the theories never fail, so be it. But if they *do*, then we have ready guides to their replacement.

Perhaps it is useful to phrase the approach this way: the reappraisal I suggest of Cassirer's analysis of the transition to special relativity should occasion a closer look by philosophers of physics into the semiclassical regime of gravitation. But neither should we regard acceptance of Cassirer's view as required in order to make the semiclassical theory interesting to philosophers, nor should we take his view as naively prescriptive of what progress in quantum gravity must look like. Still, if radical new physics ultimately is required for quantum gravity to advance to a complete theory, it is not at all unreasonable, prima facie, to suppose that we will uncover that radical physics in pursuit of the semiclassical program.

Conclusion

Let me close by discussing two important missing components of the view that I am trying to advance. These components are not, I think, crucial for the basic outline that I have presented, but some attention to them would make for a more complete picture.

First, there has been no mention of the quantum mechanical measurement problem. On the face of it, such an omission would seem surprising. After all, the issue of what to do about the apparent fact that the universe is in a definite state despite the absence of an outside observer is behind many of the important approaches to quantum cosmology, and these in turn are behind many programs in quantum gravity as well as setting many of the desiderata for such programs. Moreover some, in particular Roger Penrose, have suggested that gravity may be the way quantum states are collapsed and that to understand the measurement problem one will have to understand the connection between gravity and quantum mechanics.

More attention should be devoted to the issue of measurement. However I think that the issue is irrelevant to the question of how we should go about quantizing gravity (or even whether we should). How the collapse of the wave function takes place (if it does at all) should, I think, be discovered as we go along. My suggestions toward a philosophy of science are, I think, clearly directed more toward the development of the sciences than toward straightforward, traditional problems of interpretation of scientific theories themselves. It would be nice to have a solution to the measurement problem. I don't see that such a solution is required for progress in quantum gravity.

Second, a more complete account of Cassirer's scientific methodology would be of considerable interest here. I have only offered the sketchiest of outlines of part of his view. Producing a more detailed analysis of the relation between Cassirer's views and contemporary physics, and contemporary debates about progress in physics and science more generally, are tasks for another occasion. However, the parts of Cassirer's views that I have singled out seem to provide a solid foundation for beginning to understand both the development of quantum gravity, and the sense in which we are seeing true scientific progress in that development.

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