REVIEW ARTICLE

Interpreting Quantum Gravity[*]

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C. Kiefer, *Quantum Gravity* (Oxford: Clarendon Press, 2003), ix + 308 pp., Hardback ISBN 0 19 850687 2 £55.00. C. Rovelli, *Quantum Gravity* (Cambridge: Cambridge University Press, 2004), xxiii + 455 pp., Hardback ISBN 0 521 83733 2 £45.00.

1. Quantum gravity and philosophy

In recent times physicists and philosophers of physics have tended to tread very different paths, and they have generally been a little suspicious of one another. As Michael Redhead points out in the first of his Tarner Lectures, "many physicists would dismiss the sort of question that philosophers of physics tackle as irrelevant to what they see themselves as doing" while "philosophers generally regard physicists as naive people, who do physics in an uncritical way" (1996, pp. 1-2). Reichenbach expressed much the same point even more strongly, suggesting that there is a "mutual contempt in which each misunderstands the purposes of the other's endeavours" (1958, p. xi). This hasn't always been the case, of course. As Reichenbach goes on to say, "[t]he classical philosophers had a close connection with the science of their times" (*loc. cit.*). The division between physics and philosophy is a fairly recent thing. In addition to this, historically, each time a fundamental revolution has occurred in physics—e.g. Newtonian mechanics; the relativity theories; quantum theory, etc...—there has generally been an associated shift to a more critical, reflective attitude towards theory construction. Kuhn appears to suggest that such a shift is necessary for scientific change to occur at all:

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[*] © D. Rickles, 2005. To appear in *Studies in the History & Philosophy of Modern Physics* (December, 2005).

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> It is no accident that the emergence of Newtonian physics in the seventeenth century and of relativity and quantum mechanics in the twentieth should have been both preceded and accompanied by fundamental philosophical analyses of the contemporary research tradition. (Kuhn, 1970, p. 88)

Kuhn seems to hit the nail bang on the head as regards the present situation in quantum gravity[1] when he writes that:

> Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and the debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. (Kuhn, 1970, pp. 90-91)

One finds exactly the proliferation and recourse to philosophy that Kuhn speaks of in the context of quantum gravity. It thus functions as an ideal case study to test the claims of Kuhn—and, indeed, a host of other ideas from the philosophy of science (cf. Callender & Huggett, 2001a, p. 2).[2] But having said all of this, though physicists have been known to take up more philosophical attitudes in times of crisis, 'the philosophers' have previously remained firmly divided from the constructive practice of theory building in physics; waiting in the wings, as it were, until the theories were deemed sufficiently well established to warrant their attentions. This is true of quantum gravity too; but it needn't be the case.

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[1] For the purposes of this paper it will suffice to understand quantum gravity as some quantum theory that has general relativity as a classical limit. Heuristically, it helps to view this as a 'synthesis' of quantum field theory and general relativity—Cao (2001), for example, views quantum gravity in just this way; Rovelli too appears to view loop quantum gravity in this way (e.g. 2004, pp.4-7).

[2] Unfortunately, however, what I take to be the 'best bit' of Feyerabend's (1975) message of "anything goes" hasn't quite been heeded by the researchers of the different programmes—recall that the idea was that the availability of multiple approaches can spark off new research directions in them. However, this requires that the researchers occupying the various camps actually take notice of the other camp's methods, aims, and results. It would undoubtedly prove useful if there were more 'cross-fertilization' across the methods, but the workers tend to restrict themselves to their own pet approach to the exclusion of all else, leading to a fairly insular landscape. Fortunately, there are signs that this is beginning to change.

One of the main aims of this essay review is to highlight the extent to which many of the problems of quantum gravity are predominantly 'philosophical' in their nature and origin.[3] The strange case of quantum gravity provides a place where philosophers might play a role in constructive parts of the foundations of physics - though, as I have already mentioned, this probably will not include involvement in the technical foundations. Cao, believing consistency and conceptual clarity to be of the essence in quantum gravity (since there is no experimental basis), makes a similar point:

> ... this is a rare conjuncture for philosophers to intervene, with a good chance to make some positive contributions, rather than just analysing philosophically what physicists have already established. (Cao, 2001, p. 183)

I agree. There are at least three reasons behind this possibility as I see it:

- These days philosophers of physics are simply better equipped in terms of their command of the necessary parts of mathematics and physics required—many of them did their original training and research in physics, and one often finds them publishing in physics journals.
- Quantum gravity is an area of physics lacking an experimental basis from which to test the various proposals, thus forcing conceptual and mathematical consistency to take center stage.
- Most importantly, it appears that the kinds of conceptual problem that litter the field of quantum gravity are ones that philosophers are already well familiar with, as I have already suggested, and aim to show in more detail.

But philosophers, however, have generally been rather slow to pick up the challenge of quantum gravity, seemingly being more content with flogging poor

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[3] This should not, however, be taken to imply that the solutions to these problems are going to be philosophical too. It is far more likely that a large amount of technical gerrymandering will go towards resolving the difficulties faced: quantum gravity *is* hard. It is unlikely that philosophers will actually be involved in *these* areas of quantum gravity. However, they might, and should, play a crucial role in clarifying exactly what the problems are, where they come from, and how they might be tackled. The foundations of quantum gravity are a mess, and philosophers tend to be particularly adept at tidying such messes up. I do not mean this disparagingly; nor do I mean to align philosophers of physics with Lockean 'under-labourers': the work involved is hard and important.

old non-relativistic quantum mechanics to death! This is made all the more surprising given that many researchers engaged in quantum gravity actively encourage the involvement of philosophers in their discipline. For example, Carlo Rovelli—author of one of the textbooks that is the subject of this review—explicitly voices this opinion:

> As a physicist involved in this effort [i.e., quantum gravity], I wish the philosophers who are interested in the scientific description of the world would not confine themselves to commenting and polishing the present fragmentary physical theories, but would take the risk of trying to look *ahead*. (Rovelli, 1997, p. 182)

Rovelli is a physicist who has contributed to numerous philosophical collections—the passage above is from such a collection—in a bid to bolster philosophers' enthusiasm for his approach to quantum gravity—the approach known variously as *loop quantum gravity*, *loop gravity*, or the *loop representation* (it is arguably the only strong competitor to string theory, and my personal view is that it is in fact far superior).[4] John Baez—a mathematical physicist who has done important work in making loop gravity rigorous, and again is keen to debate with philosophical audiences—also makes a similar point to Rovelli (again writing in a philosophical collection):

> Can philosophers really contribute to the project of reconciling general relativity and quantum field theory? Or is this a technical business best left to the experts? [.] General relativity and quantum field theory are based on some profound insights about the nature of reality. These insights are crystallized in the form of mathematics, but there is a limit to how much progress we can make by just playing around with this mathematics. We need to go back to the insights behind general relativity and quantum field theory, learn to hold them together in our minds, and dare to imagine a world more strange, more beautiful, but ultimately more *reasonable* than our current theories of it. For this daunting task, philosophical reflection is bound to be of help. (Baez, 2001, p. 177)

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[4] The other founders of this approach (e.g. Abhay Ashtekar, Lee Smolin, and others) also demonstrate a heightened sense of philosophical awareness. Like Rovelli, Lee Smolin shows an openness to philosophical issues, and likewise contributes to philosophers' events and books.

Thus, the doors are wide open for philosophers to enter. But few philosophers have chosen to do so.[5] Up until very recently the same might have been said of *special relativistic* quantum field theory, though lately, and largely through the work of the Pittsburgh philosophy of physics group, there has been a definite shift of emphasis from non-relativistic quantum mechanics to relativistic quantum field theory—a very welcome move in my opinion (Cushing, 1988, notwithstanding). As welcome as this shift is, though, *methodologically* the philosophy of relativistic quantum field theory is much the same as non-relativistic quantum mechanics (and likewise subject to Rovelli's gripe): the mathematical and theoretical framework exists (admittedly, modulo certain nasty consistency problems in the interacting theory) and the experimental data is there to confirm this framework. The job of the philosopher of physics is to examine this framework and explicate its relation to the the world (or some set of possible worlds according to which the theory is made true). The uniqueness of quantum gravity as a 'challenge' to philosophers arises precisely from the *lack* of such an established framework and associated medley of confirming experiments, and philosophers would do well to shift their gaze in its direction because of this feature. Moreover, though there are novel interpretive problems in relativistic quantum field theory that aren't in standard quantum mechanics, the novelties involved in bringing together quantum field theory and *general* relativity promise to be far greater, more profound, and more spectacular. Rovelli, in particular, (in the book I review here) does a fine job in bringing these interpretive issues to the surface in a way that develops in one an awe for them and the conceptual foundations of quantum gravity in general.

After this brief motivational section, let us now turn to the two books in question. I begin with a general description and assessment, after which I survey, in the subsequent section, a number of interpretational issues that they raise. I propose to focus on Rovelli's book far more than Kiefer's, since the former's book will, no doubt, appeal to a philosophical audience more so than the latter's. Interpretive issues are never far from the surface in Rovelli's—though that is not to say that

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[5] We can perhaps diagnose the apprehension in two ways. Maybe the 'toll' on entrance is too high. The mathematics required is very hard, and given that philosophers have enough to worry about, this might over stretch them. Or maybe the *incompleteness* bothers philosophers. Quantum gravity is not all wrapped up. There is not a single door leading to it; rather, there are several doors, leading to very different places (string theory, loop gravity, causal sets, etc...). I think that Rovelli's book shows that philosophers need not be bothered by the technical demands; it can be made very palatable indeed. Rovelli also demonstrates that his preferred approach (loop gravity) looks like it has every chance of success—even if it should ultimately fail, it nonetheless provides many insights into gravity, symmetry, and space and time.

they are entirely absent from Kiefer's.

2. Outline and assessment

Beyond their matching titles, and the fact that each is intended to function as a *textbook*, there is in fact very little overlap between these books. They set out to accomplish very different goals. I think that only Rovelli's succeeds *qua* textbook, but the title *Quantum Gravity* is something of a misnomer in the case of Rovelli's book since he restricts his attention to his *loop quantum gravity*—it is not a misnomer for Rovelli; for him the loop gravity approach is in charge of the field since it makes testable (i.e. falsifiable) predictions and provides a genuine merging of quantum field theory and general relativity (see 2004, p. xiv). Kiefer, by contrast, gives a very general overview of several well trodden methods[6]—though *canonical* quantum gravity (labeled "quantum GR" by Kiefer, and of which loop quantum gravity is a 'subspecies') gets by far the most exposure on the grounds that "it is closer to established theories and because it exhibits many general aspects clearer" (2003, p. 21). Both authors are, then, on the side of the loop approach.

The fact that the canonical approaches are closer to established theories (quantum theory and general relativity) certainly makes this class of theory easier to interpret, for we can in many cases carry what we know of the other theories into this different context—though we will be applying them in very *new* contexts (e.g. applying the uncertainty relations to the geometry of space). The relatively 'conservative' nature of loop quantum gravity (being a canonical approach) will, I think, increase the chances of its (or a close relation of it) being ultimately successful in terms of which theory is chosen to function as our theory of quantum gravity. Of course, in the final analysis, it depends on whether it makes the *right* predictions!

As regards possible competition, both books are 'loners'. There is, as Kiefer rightly points out, no comparable book on quantum gravity that has the same scope as his does, at least no *single-authored* monograph. There is a collection

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[6] Kiefer's book struck me as being very much like Chris Isham's magisterial reviews of quantum gravity (see Isham, 1997, for a typically fine example). Although the technical details are presented, it is done without much by way of demonstration: generality is the key aim—but see below. (Kiefer too, like Isham, is intent on unpacking the conceptual foundations of the various approaches.) Thus, I seriously doubt that one could *learn* quantum gravity from this book. However, this is not to say that the book is 'easy going' mathematically speaking. Most philosophers of physics will have problems getting through the complexities, but rewards *can* be found amongst the complexities.

(Giulini *et al.* (eds.), 2003) that is rather more general than Kiefer's, and is also intended to serve as a pedagogical guide.[7] In fact, one of the problems I had with Kiefer's book was that, although it was general, it wasn't quite general enough. If the brief is to cover quantum gravity in all of its forms, then lip service must be paid to the more 'maverick-like' approaches: TQFTs, causal sets, twistors, non-commutative geometry, and the like. It is through these approaches, for example, that we begin to see connections emerging between the 'front-runners': string theory and loop gravity. However, I would not be without Kiefer's discussion of "Covariant Approaches to Quantum Gravity" (2003, pp. 23-60). It is easily the best exposition I have seen of the concepts, methods, and problems of covariant approaches.[8]

Rovelli's book stands alone in being a completely self-contained guide to the mathematical *and conceptual* foundations of loop gravity. One really could learn the theory from scratch from this one book! Not only that, one can get a clear picture of the kinds of philosophical implications of the theory. It is, for this reason, the most important book on quantum gravity to have appeared thus far (at least, as far as philosophy of physics goes).[9] I should, in fairness, point out that there is another textbook on the loop approach by Gambini and Pullin (1996), though they take a slightly different approach to Rovelli and Smolin's—moreover, philosophical issues are rather harder to extract from their book.

As far as background goes, both books require a prior acquaintance with quantum field theory and general relativity. However, Rovelli presents this material from scratch, and the approach to general relativity that he uses is based on the *vierbein* formalism (wherein the gravitational field is a *tetrad* field, rather than a metric field), thus highlighting the connection to gauge theory. Kiefer's book calls upon far more resources from mathematical physics, and no real attempt is made to render the book self-contained. Nor, given his aims, could he have made the book self-contained without it becoming staggeringly large. However, one possibly serious omission (*vis-á-vis* their status as *textbooks*) from both Rovelli's and

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[7] Note that Kiefer is on the editorial team of this book, and also contributes an article.

[8] Although Michael Duff's early article "Covariant Quantization" (1978) comes in at close second.

[9] Rovelli (2004, p. xiv) points out that Thomas Thiemann has a book close to completion on the mathematical foundations of loop gravity (Thiemann, 2001). He suggests that his 'book can almost be read as as Volume 1 ("Introduction and Conceptual Framework") and Thiemann's book as Volume 2 ("Complete mathematical framework") of a general presentation of loop quantum gravity'. This is a fair and true comment; but for philosophers I should think Volume 1 of this pair is sufficient.

Kiefer's books is *exercises*. If one wishes to learn something really well, then one needs to know how to apply it. However, in the case of Rovelli I think that the results are worked out so well, and with enough detail that this gripe loses force. There are some fairly fine-grained derivations of results in Kiefer's book too. Let us now turn to a more detailed examination of these two books.

2.1 Analytical synopsis of Kiefer's book

Kiefer begins by motivating the search for quantum gravity, and outlining the main directions of research. As motivations he cites 'unification', 'black holes', 'the problem of time' (2003, pp. 2-4). (Rovelli, by contrast, does not see such motivational arguments as necessary and simply 'launches in'). In a nutshell: (1) unifying theories has been fruitful in the past, so we should expect it to be fruitful in the future; (2) we need an account of the final stages of black holes since classical general relativity is not up to the job; and (3) the frameworks of quantum field theory and general relativity involve radically distinct conceptions of space and time, and this inconsistency needs to be ironed out. If these motivations are not sufficient he cites a dimensional argument showing the quantum gravitational effects should become non-negligible at the Planck scale (*ibid.*, pp. 4-7). In this case we need a theory to deal with such a regime, and we don't currently have one. His discussion of the various approaches to quantum gravity is far too brief, and it is marred, as is the rest of the book, by the absence of any mention of methods falling outside of the remit of canonical and covariant quantization (I am including string theory in this latter category)—Kiefer employs Isham's distinction between 'primary' and 'secondary' approaches, according to which either heuristic quantization rules are applied to general relativity or else general relativity is derived from a more fundamental quantum theory (as in the case of string theory). I would have liked to see a much more detailed taxonomy of approaches in a book of this kind, perhaps along with a discussion of alternative ways of carving up the plenitude of approaches (i.e. an analysis of the different taxonomies). Chapter 1 also contains a fairly useful discussion of the problems encountered in semi-classical approaches to quantum gravity. I shan't dwell on it, but since there has been some discussion of this topic in the philosophical literature (Mattingly, 1999; Callender & Huggett, 2001b; Wuthrich, 2004), I will simply point out that Kiefer is of the opinion that coupling a classical system (such as a classical gravitational field) to a quantum system (such as quantized field acting as a source of stress-energy for the gravitational field) results in a 'transference' of quantum properties.

Chapter 2 is the best bit of the book as far as I am concerned. It is a very detailed examination of covariant approaches to quantum gravity, beginning with the concept of the graviton and ending with supergravity. The original covariant approaches were perturbative: one decomposes the full spacetime metric g_{ab} into a *fixed* or *background* part and a 'perturbation' p_{ab} about this non-dynamical background. In order to utilize the machinery of quantum field theory, the background part is identified with flat Minkowski spacetime with metrical signature $\eta_{ab} = \mathrm{diag}(-,+,+,+)$. Thus we get:

$$g_{ab} = \eta_{ab} + p_{ab} \tag{1}$$

where the perturbation is small (i.e. the force of the gravitational field is assumed to be very weak). Kiefer demonstrates how the notion of the graviton, with its characteristic helicity and mass properties, flows from the covariant quantization of this theory. The crucial point is that the flat Minkowski background allows for the construction of gravitons from the representation theory of the Poincaré group.[10] Without this we do not get an invariant notion of 'particle' (see, e.g., Wald, 1994). But, as Kiefer points out, this setup is only applicable at low energies; beyond the linearized level the analysis breaks down, and the graviton is lost. However, useful for understanding string theory's claim to quantum gravity is the discussion of Weinberg's analysis of covariant quantum gravity. Weinberg (1995), in his discussion of covariant quantum gravity, showed that, in the vacuum case, one can *derive* the equivalence principle and general relativity from the Lorentz-invariance of the spin-2 quantum field theory of the graviton: the spin-2 theory is *equivalent* to general relativity and *follows* from the quantum theory. The upshot of this is that any theory with gravitons is a theory that can accommodate general relativity (in some appropriate limit). This analysis forms the basis of string theory's claim that it is a candidate theory of quantum gravity: since there is a string vibration mode corresponding to a massless spin-2 particle, there is an account of general relativity (see Kiefer, 2003, p. 34, for more).

The presupposed flat background of covariant quantization methods is a conceptual problem, for sure, but it leads to even worse technical difficulties in the context of general relativity. I am referring, of course, to the non-renormalizability of the theory—*this* was the death-blow to these approaches, not the conceptual troubles.[11] However, there is a missed opportunity by Kiefer here to expound on the way in which the conceptual problem of the fixed background spacetime

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[10] From this point of view, then, there is nothing 'special,' conceptually or technically, about the graviton; it is just another particle, like the photon. Both the photon and the graviton are derived in similar ways, and perform similar functions in the theory. This is one of the reasons behind the general relativists' distaste for such methods.

[11] Indeed, I think it is safe to assume that had the technical troubles of non-renormalizability

and the technical problem of non-renormalizability *interact*, for there is a sense in which the technical problem has its roots in the deeper conceptual problem. This is frequently brought out by those physicists who favour background independent or discrete models of spacetime (e.g. Ashtekar (ed.), 1988, pp. 3-6; Rovelli, 2004, pp. 8-9 and p. 12). Kiefer concludes from this discussion that though the perturbative approach can lead to physical predictions, within the framework of an *effective* field theory—and, indeed, it is surely *required* to extract 'real' physics from whatever quantum theory of gravity we eventually end up endorsing—what is really needed is either a non-perturbative approach or else some unified theory that eclipses standard field theory.[12] The remainder of Kiefer's book is then devoted to such approaches.

Kiefer follows his discussion of covariant methods with a very lucid chapter on "Parametrized and Relational Systems," which he describes as "important conceptual preparation for the canonical quantization of GR" (*ibid.*, p. 61).[13] This is a fair characterization, for many of the problems with quantum general relativity stem from the fact that we include time amongst the dynamical variables. This is bound up with the fact that, like general relativity, such systems are constrained systems when written in Hamiltonian form—much of this chapter is devoted to explicating the machinery of constrained systems. Kiefer begins by considering the much discussed toy example of the parametrized non-relativistic particle. (He also considers the relativistic particle and the bosonic—i.e. non-supersymmetric—string).

Kiefer notes that this example involves the "disguised" use of absolute time as a dynamical variable.[14] This paves the way for a response to Kretschmann's

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not been evident, the approach would have lived on for *much* longer despite the use of the fixed, background spacetime. The reason for this is that the tools of standard quantum field theory had been remarkably successful in quantizing other forces, and there were huge expectations that the same would go for gravity: do unto gravity as one does unto the other forces!

[12] Thus, we need not desert a theory just because it is non-renormalizable. The theory of the renormalization group (see Binney *et al.* 1992), and the programme of effective field theories show us how we might view general relativity as an *effective* field theory that is nonetheless capable of making physical predictions (*cf.* Donoghue 1994, 1996). See Burgess (2004) for a very readable account of this viewpoint. Castellani (2002) offers a nice elementary survey of effective field theories and their philosophical implications.

[13] I should point out that Kiefer falsely attributes Anderson's pioneering work on symmetry groups and absolute objects to Ehlers in the opening spiel of this chapter.

[14] Note that its "recovery" is also possible though the process of *deparametrization*. No such procedure is available in the context of general relativity, there is no distinguished time, and so general relativity is *not* a deparametrized theory (*cf.* Kiefer, *ibid.*, p. 102). Clearly, to prove this, we need to test whether or not the identification between the phase space Γ of general relativity and the phase space Υ of a parameterized field theory goes through. This proposal requires that

objection to the physical content of general relativity's principle of general covariance. Kretschmann's problem, you will recall, was that every system can be converted into a generally covariant theory, so that principle can hardly be said to be a physically significant feature of general relativity. But Kiefer notes that the 'conversion process' comes "at the price of disguising absolute structures which formally appear then as dynamical variables" (*ibid.*, p. 64). This is not the case in general relativity, its general covariance is "natural in the sense that the metric is fully dynamical" (*ibid.*). But here, as in many places, a good place for a discussion of some foundational issue of philosophical interest is nipped in the bud almost as soon as it begins; given this, the claim that this is "conceptual preparation" looks rather thin on content. The chapter finishes with a discussion of Barbour and Bertotti's Machian theory of mechanics. In contrast to the time-reparametrization models, these relational dynamical systems contain no disguised time, and attempt to achieve a purely relational notion of time, relying, as Kiefer says, "exclusively on observational elements" (*ibid.*, p. 83). This kind of theory corresponds more closely, then, to general relativity. But, one would like to see more discussion of both these models and their relationship to general relativity. It is all too brief (though the issues are raised momentarily again in the subsequent chapter: on pages 102-3).

Next come three chapters leading to the formulation of loop quantum gravity. Firstly, let me say that I think that there are better expositions of this material[15]; however, there are many original touches that set it apart from these, and make it a worthy addition to the literature. Also, in the space he has he does a very good job of integrating exposition and asides on the meaning of the formalism. Kiefer begins with the Hamiltonian formulation of general relativity, and introduces the metric, connection, and loop representations of the classical theory—two further chapters then deal with their quantization. There are some very clear, novel, and interesting discussions of the constraints that arise in the Hamiltonian formulation of general relativity (these are the canonical implementation of the 4-d diffeomorphism invariance), including a little on the problems of time and observables (*ibid.*, pp. 103-5). In a nutshell these problems go, respectively, as follows:

- The Hamiltonian constraint of the theory is a first class constraint and should

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there is a canonical transformation $\Phi : \Upsilon \rightarrow \Gamma$ such that $\Phi(\overline{\Upsilon}) = \overline{\Gamma}$. However, there can be no such transformation because $\overline{\Upsilon}$ is a manifold while $\overline{\Gamma}$ is not (*cf.* Torre 1994). Hence, general relativity is not a parameterized field theory!

[15] My personal favourites are the papers in Ashtekar (1988); Ashtekar (1991); Ehlers and Friedrich (ed.) (1994); and Rovelli's book reviewed in this article. Baez and Munian (1994) functions as a nice primer on this material.

therefore be viewed as a gauge transformation. However, since the constraint is responsible for generating the time evolution of the data from one hypersurface to the next it looks as if time evolution corresponds to the unfolding of a gauge transformation. Gauge transformations are, of course, *unphysical*.

- The definition of 'observable' in the context of constrained systems is given as a variable that (weakly) commutes with all the first class constraints. However, since one of these is the generator of time evolution (the Hamiltonian constraint), the observables must be constants of the motion.

As regards the first problem, Kiefer points out that the gauge view can be adopted as long as one realizes that the constraint simply expresses the equivalence of the various evolutions along different foliations. His second response draws on 'view from the outside' versus 'view from the inside' division. There is no conflict with experience, says Kiefer, for our experience of change is a matter our "tracking [on the 'inside'] one part of the variables with respect to the remaining part" (*ibid.*, p. 104). Kiefer's responses, I think, merit further attention from philosophers.[16] However, again, the discussion here is marred by its brevity.

The next chapter's highlights are a 'six step' route to the quantization of Hamiltonian general relativity (in the geometrodynamical formulation) and a discussion of the problem of time in the quantum context. In the latter case, what we get is basically an updated review of the options as laid out in the classic reviews of the problem of time by Isham (1993) and Kuchař (1992). The following chapter considers the quantization of the constraints as they appear in the connection and loop representations. The key results are given, and the quantization of the geometrical operators is sketched. However, I imagine the discussion here will be too terse to follow unless one already has a grip on the concepts (the whole chapter is just 13 pages long!).

In the next two chapters Kiefer deals with the important issues of black hole quantization and quantum cosmology.[17] It is, more or less, incumbent upon the approaches to quantum gravity to say something about these matters, and Kiefer does a good job motivating them. Their is a wealth of detail in these chapters. The chapter on black holes would make a fine review article for readers wishing to

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[16] His treatment of first problem, for example, looks a little like Maudlin's response in his battle with Earman (2002). Maudlin, however, rejects the gauge interpretation.

[17] In fact, I think these chapters would have benefited from coming after the string theory chapter so that that theories approach to these problems could be integrated within the discussion.

get quickly up to speed with the various recent issues. Philosophically speaking, the central issue is that concerning the 'information-loss paradox,' according to which pure states can evolve into mixed states as a result of Hawking radiation (black hole evaporation). The problem here is to explain what has happened to the information (Earman *et al.*, 1999, give an excellent overview of the interpretive options).

There then follows a rather brief treatment of string theory, with some mention of recent themes, including duality, branes, and its particular way of dealing with black hole entropy (as I mentioned above, this latter theme would have been better integrated with the discussion of the loop representations treatment of the entropy). Though this chapter is indeed brief, Kiefer does a very good job presenting it in about the most concise way I have seen. However, what are highly interesting conceptual problems are simply dashed off in minor paragraphs. For example, the notion of a minimal length is mentioned, and compared to the minimum length that appears from the quantization of the geometrical operators in loop gravity (*ibid.*, p. 247). I would have liked to see this developed some more. How are the notions related? Also, it is pointed out that the D-branes can "probe" resolutions smaller than the minimal length. What sense are we to make of this? Philosophers would do well to get stuck into this area, and unpack exactly what is involved in such claims.

Kiefer concludes with what looks to be a general interpretive chapter; however, there are really only two issues: decoherence theory and the arrow of time. Here Kiefer is concerned with the question of how and why—if the superposition is universally valid and applies to the metric field—we observe a classical universe. Kiefer bases the classical appearance of the universe on decoherence. Decoherence is a process that suppresses superpositions of macroscopically distinct states; it generally becomes important for large objects—i.e. systems with many degrees of freedom. The idea is that systems are coupled to their environments; the more degrees of freedom they have (roughly, the more *macroscopic* they are) the more strongly they are coupled, and the quicker decoherence acts. This analysis demands, then, that we specify a 'system' and an 'environment'. In the context of quantum cosmology an environment doesn't make much sense; quantum cosmology is about the universe as a whole, and considers variables that are associated to this object. Kiefer apparently endorses a proposal of Zeh's according to which the universe 'self-measures' itself—e.g. by variables (such as the radius of the universe, or 'scale factor') interacting with other degrees of freedom—and thus supplies its own environment (*ibid.*, p. 266).

Finally, Kiefer suggests that the problem regarding the 'arrow of time' (why there are certain processes for which we do not observe a time-reversed version) might receive its ultimate resolution in the framework of quantum gravity (*ibid.*, p. 274). The root idea is that the expansion of the universe functions as the 'master arrow' for the many other 'arrowed' phenomena. All of these arrows seem to point back to some "special initial conditions," conditions near the big bang. Of course, this is outside of classical gravity's realm, and is where quantum gravity is expected to deliver answers. Thus, Kiefer looks to the Wheeler-DeWitt equation for the origin of irreversibility. I have to say, I found the argument here too quick. But the claim is so philosophically profound that it must warrant some attention from philosophers with interests in asymmetries of (or *in*) time.

2.2 Analytical synopsis of Rovelli's book

Let us turn now to Rovelli's book. We find that it is in three parts dealing respectively with the foundations of general relativity, loop quantum gravity (including spin-foam models), and various odds and ends in the form of appendices—there is also a useful chapter-by-chapter bibliography. There are three appendices in all: one on "Groups and recoupling theory"; one on the history of quantum gravity[18]; and one comprising some philosophical remarks about the enterprise of quantum gravity (basically, on theory change, methodology, realism, and truth—he offers a defense of realism). The latter is an engaging 'plea' for more philosophical reflection on the part of physicists and for more dialogue between physicists and philosophers. It is rare, and heart-warming even, to see a physicist discussing such mattes at all, let alone in a serious physics textbook! This attests to the fact that this is as much a book for philosophers as it is for physicists.

The first chapter is an excellent, 'not too technical,' highly-condensed summary of the entire book, and would function as a ideal primer for philosophers wishing to get quickly acquainted with the central objectives and concepts of the loop gravity approach without getting their hands 'dirty' with the formal underpinnings of the various claims (taking the claims 'on trust,' as it were). The focus here is on bolstering physical intuition[19], and all of the conceptually weighty moves are

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[18] This is nicely done, and shows how loop quantum gravity fits into the bigger picture. However, for a more detailed history see Rovelli (2000).

[19] A good example of this is Rovelli's presentation of loop gravity as a natural synthesis of two historic episodes: the Faraday like idea "that forces are described by lines" and the Einsteinian idea of background independence (2004, p. 17). Each move resolves serious problems faced by the other: background independence clears the problem of 'overcompleteness' traditionally faced by going to the loop basis, and the loop basis clears the problem of controlling diffeomorphism invariance.

laid out clearly for all to see. Basically, this is essential reading for philosophers of physics wishing to engage with loop gravity and, I suggest, for philosophers of spacetime.

The centerpiece of the book is really the generalized framework Rovelli outlines for constructing theories. Rovelli is on a mission to free physics 'from the prejudices associated with the habit of thinking of the world as "inhabiting space" and "evolving in time"' (p. 10). In other words, the book forges a path away from background dependent physics to background independent physics (physics without a fixed, nondynamical metric). His strategy involves a reformulation of mechanics based on a distinction between, what he calls, 'partial' and 'complete' observables. I discuss this below, but for now it will suffice to say that the idea is based on a somewhat unorthodox treatment of constrained systems, according to which no variables are privileged as 'time' and 'space'. One makes a choice, and then forms a complete observable from a pair of partial observables by forming a *correlation* between the two. The theory is then about the evolution of these 'relative quantities', rather than the evolution *in time* of independent quantities.

One thing that puzzled me in this opening chapter—and has puzzled me while reading many articles on loop gravity—was the suggestion of an equivalence between non-perturbative approaches and background independent approaches to quantum gravity. Recall the perturbative expansion that characterized the old covariant perturbation approaches: $g_{ab} = \eta_{ab} + p_{ab}$. Rovelli generalizes this to $e(x) = e_{\text{background}}(x) + h(x)$, so that the background part needn't be Minkowski—note that Rovelli uses a *dreibein* (a triad matrix, or field) $e(x)$, as opposed to a metric, to represent the gravitational field. The idea is that the background is *fixed*, and this suffices to determine coherent notions of, e.g., spacelike separation, and so on, so that one can construct a quantum field theory of $h(x)$. Thus, one 'pulls' the gravitational field off spacetime, and deals with it as a perturbation. Now, Rovelli claims that any approach to quantum gravity that does not make this split is a background independent approach. Thus, he *equates* background independence and non-perturbative methods (in this restricted context). I would like to see an analysis of the connections between these two notions, for *prima facie* they seem distinct: one can surely have a non-perturbative approach that is not background independent? The recent moves in string theory seem to comprise just such a counterexample. So maybe the implication only works in one direction: background independence is sufficient for assuming that a non-perturbative approach has been taken, but not *vice versa*. Likewise, if a pertubative method is in operation, then one can assume that it is a background dependent approach too, but not *vice versa*. Further work needs to be done here, and it will most probably suit philosophers of physics rather than physicists.

Chapter 2 deals with classical general relativity. The presentation is slightly unorthodox in that Rovelli uses the tetrad field to represent the gravitational field, rather than the metric field. The reason for this has to do with the potential addition of matter (fermions) and with its role in quantum gravity. Unorthodoxy arises again, given that this is a physics textbook, in that most of the chapter is concerned not with the presentation of formalities, but with detailed discussion of the physical interpretation and conceptual foundations of general relativity. Thus we find an historico-conceptual reconstruction of the theory; a discussion of general covariance and the hole argument; and a discussion of the difficult problem of what the observables of the theory are. In a section entitled "Complements" there is further, more explicitly philosophical discussion of Mach's principles, the debate between substantivalism and relationalism, the Kretschmann objection, an analysis of the various meanings of *time*, and more. The material in this section will, I expect, prove to be a treasure trove for philosophers. For example, Rovelli carves out a lineage from Cartesian relationalism leading to a form of relationalism about space in loop quantum gravity. He uses the hole argument to argue for a relationalist position, according to which points of spacetime have no *intrinsic* ontological significance—where by "intrinsic" is meant independently of further specifications that serve to pick out a certain point. The upshot of this is that there is no notion of localization on the manifold; localization concerns relations between dynamical objects. This feature, known as background independence, lies at the conceptual heart of loop quantum gravity.

Lee Smolin characterizes background independence/dependence in the context of quantum gravity as follows:

> The background dependent approaches are those in which the definitions of the states, operators and inner product of the theory require the specification of the classical metric geometry. The quantum theory then describes quanta moving on this background. The theory may allow the description of quanta fluctuating around a large class of backgrounds, but nevertheless, some classical background must be specified before any physical situation can be described or any calculation can be done. All weak coupling perturbative approaches are background dependent, as are a number of non-perturbative developments. [...] The background independent approaches are those in which no classical metric appears in the definition of the states, operators and inner product of the theory. [...] [T]he metric and connection

> enter the theory only as operators, and no classical metric appears in the definition of the state space, dynamics or gauge symmetries. (Smolin, 1998, pp. 2-3)

In the context of classical general relativistic physics, of course, the metric—and, therefore, the geometry of space—is *dynamical*: the metric on spacetime is not *fixed* across the physically admissible models of the theory (as it is in, e.g., Newtonian and specially relativistic theories). The geometry of spacetime is affected by matter in such a way that different distributions of matter yield different geometries—the coupling and the dynamics is described by Einstein's field equation. In other words, general relativity does not depend on the fixed metrical structure of spacetime; rather, the metric itself, and hence the geometry, comes only once a matter distribution has been specified (and the dynamical equation has been solved). This 'model-variance' is another way of making sense of background independence. Loop gravity is a direct quantization of classical general relativity, and involves the metric and connection being turned into operators. It is, therefore, background independent too. Many of the conceptually novel and problematic aspects of loop gravity stem from its background independence. Below I briefly consider an argument for relationalism based on background independence that many physicists have endorsed, and that Rovelli endorses in this book.

Firstly, note that quantizing the canonical theory, in the loop representation, led to the application of *spin-networks*, introduced in the 1970s by Roger Penrose.[20] Specifically, in the context of loop quantum gravity it is found that the spin-networks form a basis for the quantum states—the spin-networks are eigenstates of certain geometrical operators. Penrose's original idea was to dispense with the continuous spacetime manifold, and replace it with a combinatorial structure:

> A reformulation is suggested in which quantities normally requiring continuous coordinates for their description are eliminated from primary consideration. In particular, space and time have therefore to be eliminated, and what might be called a form of Mach's principle must be invoked: a relationship of an object to some background

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[20] A spin-network is a graph whose nodes represent 'chunks' of space and whose links represent surfaces separating these chunks. The spin-network (or rather the equivalence class of spin-networks under the group of diffeomorphisms of the spatial manifold) then represents a quantum state of the gravitational field, or of space.

> space should not be considered—only relationships of objects to each other can have significance. (Penrose, 1971, p 151)

Following Penrose's line, the claim of many of those working on loop quantum gravity is that spin-networks point towards a relational conception of space. Why? The reason is connected to background independence (and, though I leave this hanging in the air here, the hole argument: see Rickles, 2005). The central claim is that spin-networks represent quantum space (i.e. a quantized version of the spatial part of the gravitational field). However, in order to accomplish this, the states must, amongst other things, be diffeomorphism invariant. Yet spin-networks are defined on a (compact three-dimensional) manifold, just like the metric was in the classical case. Formally, hitting a spin-network with a diffeomorphism shifts it around the manifold. Thus, we need to impose the constraints (i.e. we need to solve the quantum Einstein equations). This is achieved by taking the equivalence class of spin-networks under these diffeomorphisms, giving us a diffeomorphism invariant *s-knot* (for 'spin'-knot) or 'abstract' spin-network. The idea is that the *s*-knot is 'smeared out' over the manifold; it is not a localized entity—so hitting an *s*-knot with a diffeomorphism does nothing, we simply get the same state back. However, any other fields must then be localized with respect to these *s*-knots; the *s*-knots represent space and define location. Since the *s*-knots are *dynamical* entities—being, roughly, a quantum analogue of the classical metric field—it seems as though localization has been relativized to these dynamical objects. The move to relationalism about space strikes many physicists as ineluctable.

Rovelli sketches the supposed implication—on the understanding that (active) diffeomorphism invariance implements background independence in general relativity—as follows:

> [Diffeomorphism invariance] implies that spacetime localization is relational, for the following reason. If $(\psi, \mathsf{X_n})$ is a solution of the equations of motion, then so is $(\phi(\psi), \phi(\mathsf{X_n}))$ [where ϕ is a diffeomorphism]. But ϕ might be the identity for all coordinate times t before a given $\mathsf{t_0}$ and differ from the identity for some $\mathsf{t} > \mathsf{t_0}$. the value of a field at a given point in $\mathcal{M}$, or the position of a particle in $\mathcal{M}$, change under the active diffeomorphism ϕ. If they were observable, determinism would be lost, because equal initial data could evolve in physically distinguishable ways respecting the equations of motion. Therefore classical determinism forces us to interpret the invariance under $\mathsf{Diff}_{\mathcal{M}}$ as a gauge invariance: we must assume that diffeomor-

> phic configurations are physically indistinguishable. (Rovelli, 1999, p. 3)

Hence, the 'physical' aspects of a system are not given by specifying a single field configuration, but instead by the "equivalence class of field configurations...related by diffeomorphisms" (*ibid*). The observables of such a system are then given by diffeomorphism invariant quantities. Such specifications of states and observables are clearly independent of any background metric: only gauge-invariant quantities are to enter into such specification, and any reference to a background metric (via, for example, fixed coordinates or functions on $\mathcal{M}$) yields non-gauge-invariant quantities. Thus, diffeomorphisms change the localization of fields on $\mathcal{M}$; this is represented in the Hamiltonian scenario by the action of the constraints. However, the localization is a gauge freedom, so any states or observables involving localization to points will not be physical. A *physical* spacetime is, then, given by an equivalence class of manifolds, metrics (and other dynamical fields) under all the actions of the gauge group $\mathrm{Diff}(\mathcal{M})$. Rovelli explicates the step to relationalism as follows:

> [t]he point is that only physically meaningful definition of location within GR is relational. GR describes the world as a set of interacting fields including $g_{\mu\nu}(x)$, and possibly other objects, and motion can be defined only by positions and displacements of these dynamical objects relative to each other. [...] All this is coded in the active diffeomorphism invariance ... of GR. Because active diff invariance is gauge, the physical content of GR is expressed only by those quantities, derived from the basic dynamical variables, which are fully independent from the points of the manifold. [.] [Diff invariance] gets rid of the manifold. (Rovelli, 2001, p. 108)

The ontological conclusion regarding the relationalist conception of space seems to be drawn from two principle ideas: (1) the fact that localization is relational; and (2) the fact that Leibniz equivalence has been imposed—i.e. by solving the diffeomorphism constraint in the move to *s*-knots. These are not independent in this case: relational localization enforces Leibniz equivalence—i.e. (1) implies (2)—since the gauge freedom arises precisely by localizing *absolutely* with respect to the manifold.

The problem is, however, that relational localization cannot itself deliver relationalism about space(time), since, on the understanding that the 3-metric and *s*-knot state represent classical and quantum space, the localization is relativized to space! But this then simply begs the question about the ontological nature of space. In particular, many substantivalists—those of the 'sophisticated' stripe—endorse just such a view in the classical case (i.e. that the metric field should be identified with substantival spacetime: *cf.* Hoefer, 1996), and there is no reason why they shouldn't carry their views over into the quantum context too so that *s*-knots are identified with substantival space. Naturally, since relational localization implies Leibniz equivalence, it follows that substantivalists can endorse that too.[21]

Chapter 3 focuses on Mechanics, and contains Rovelli's attempt to formulate a version of mechanics that is sufficient for general relativistic physics, yet is nonetheless capable of encompassing non-general relativistic physics too. His response is to revise the notions of 'state' and 'observable,' and his motto is "mechanics is about relations between observables" (2004, p.118). In other words, space and time, conceived of as *independent* entities, do not enter into this picture.

> In the nonrelativistic context, time is a primary concept. Mechanics is defined as the theory of the evolution in time. In the definition considered here, on the other hand, no special partial observable is singled out as the independent variable. (Rovelli, 2004, p. 118)

The following chapter, Chapter 4, gives Hamiltonian general relativity (based on a connection variable[22]) the same treatment; and chapter 5 then applies the same insight to quantum mechanics.

Rovelli claims that a number of thorny problems from general relativity and quantum gravity can be cleaned up or resolved by utilizing his distinction between 'partial' and 'complete' observables: a *partial* observable is a physical quantity to which we can associate a measurement leading to a number and a *complete*

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[21] Of course, there are independent arguments pointing to the fact that substantivalists can endorse Leibniz equivalence. What I have sketched here is simply an indirect one.

[22] Until the 1980s, it was believed that general relativity should be cast as a dynamical theory of metrics. (This was the general view. However, Einstein and Schrödinger considered general relativity as a dynamical theory governing the Levi-Civita connection. The Palatini formalism too makes use of a connection. See Baez & Munian, 1994, III.3, for further details.) However, in 1986 Abhay Ashtekar discovered a new set of variables that cast general relativity in the same language as the gauge field theories that the standard model is based on; a kind of 'formal unification' was thus achieved. This crucial step led to the possibility of constructing quantum loop representations of general relativity.

observable is defined as a quantity whose value (or probability distribution) can be predicted by the relevant theory. Partial observables are taken to coordinatize an extended configuration space $\mathcal{Q}$ and complete observables coordinatize an associated reduced phase space Γ_{red}. The "predictive content" of some dynamical theory is then given by the kernel of the map $f : \mathcal{Q} \times \Gamma_{red} \rightarrow \mathbb{R}^n$. What I wish to consider here is (1) how this has a bearing on some conceptual issues in loop quantum gravity, and (2) how we can make sense of this idea at all. Let us begin with (2). I wish to consider his claim that "the *extended* configuration space has a direct *physical* interpretation, as the space of the partial observables" (2002, p. 124013-1, my emphasis). This space gives the *kinematics* of a theory and the *dynamics* is given by the constraints, $\phi(q^a, p_a) = 0$, on the associated extended phase space $T^*\mathcal{Q}$. *Both* are invested with physicality by Rovelli; the partial observables, in particular, are taken to be physical variables. Thus, whereas, for example, Stachel (1993) argues that the kinematical state space of a background independent theory like general relativity has no physical meaning prior to a solution (so that only the dynamical state space is invested with the power to represent genuine physical possibilities; kinematics then being in this sense derivative), Rovelli appears to take both kinematic and dynamical spaces as equally robust. The content appears to be this: there are quantities that can be measured whose values are *not* predicted by the theory. Yet the theory *is* deterministic because it does predict correlations between partial observables. The dynamics is then spelt out in terms of relations between partial observables.

The view Rovelli defends has some immediate philosophical interest since it is non-reductive (i.e. a physical interpretation is given to the phase space *with* symmetries) and yet Rovelli is a self-proclaimed relationalist. Thus, prima facie, we seem to have an instance of a break between possibility counting/geometric spaces and spacetime ontology.

Rovelli distinguishes between two extremes of interpretation with respect to the formal variables of a theory for a system with constraints (I have changed the notation to suit my own):

> It is sometimes claimed that the theory can only be interpreted if one finds a way to "deparameterize" the theory, namely, to select the independent variable among the variables q^a. In the opposite camp, the statement is sometimes made that only variables on the physical phase space Γ_{red} have a physical interpretation, and no interpretation should be associated with the variables of the extended configuration space Γ. (Rovelli, 2002, p. 124013-7)

By contrast, Rovelli invests elements of Γ and $\mathcal{Q}$ (including gauge-*dependent* quantities) with physical reality; indeed, elements of the latter are taken to be "the quantities with the most direct physical interpretation" (*ibid.*). Complete observables—i.e. the quantities we actually measure and are able to predict uniquely (i.e. Bergmann/Dirac observables: *cf.* Earman (2003))—are dynamically determined *á la* Stachel (*op. cit.*):

> Such a quantity can be seen as a function on the space of solutions modulo all gauges. This space is the physical phase space of the theory Γ_{red}. ... Any complete observable can thus be expressed as a function on Γ_{red}. (Rovelli, 2002, p. 124013-3)

Crucially, Rovelli notes that there is an equivalent description of any complete observable "as a function on the *extended* phase space having vanishing Poisson brackets with all first class constraints" (loc. cit.; my emphasis).[23] Thus, we see a formal equivalence between reduced and unreduced spaces at the level of observables, providing one imposes certain conditions of the unreduced, extended, space. In this approach, then, Rovelli distinguishes between what is observable and what there is (i.e. ontology), whereas elsewhere (1997, 2001), in arguing for his relationalism, he appears to assume a direct connection between the two.

Is this a formal move or an interpretive move? Clearly, loop gravity exists independently of this framework, and the partial observables programme has not *modified* loop gravity in any significant way. What it does do is provide a way of making conceptual sense of what loop gravity is about. We can perhaps get a better grip on Rovelli's idea by comparing it with Bergmann's work on observables. In a nutshell, Rovelli's partial observables programme says that Bergmann was wrong—or, at least, only half right. Bergmann's observables are the same as Rovelli's complete observables, and they correspond to quantities that can be predicted by the theory's laws, just as in Rovelli's approach. Neither partial observables, nor any close cousin, appear in Bergmann's approach, for such things are gauge-dependent. By choosing gauge-invariant functions, Bergmann hoped to line up his notion of observables (the gauge-theoretic notion) with the standard 'operational' idea of observables, i.e. those that appear in classical and quantum mechanics. Rovelli says there's more to the story than that: there are quantities that can be measured but not predicted, they are 'partial' in just this sense. In

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[23] Note that Rovelli's use of "extended" here bears no relation to the BRST method, where one literally extends a phase space by adding extra variables. "Extended" here simply means "unreduced".

investing them with physicality Rovelli is making a rather dramatic shift from the standard way of viewing observables in gauge theories; and, indeed, mechanics in general. Since Rovelli intends this shift to apply beyond general relativity and quantum gravity, I think this is an area philosophers should be investigating.

Thus, the concepts of partial and complete observables are rather difficult to make sense of, since the usual way of thinking about gauge-dependent quantities is as unphysical entities. They are indeterministic (that is why they are deemed gauge); the fact that they cannot be determined usually leads one to say that they are unmeasurable too. Bergmann had no problems casting them out and restricting the class of observables to the operational kind, the gauge-invariants. I think we can gain more of an understanding about what is going on here when we look at how Rovelli wields the distinction in the context of specific interpretive problems.

> A pre-GR theory is formulated in terms of variables (such as q) evolving as functions of certain distinguished variables (such as t). General relativistic systems are formulated in terms of variables ... that evolve with respect to each other. General relativity expresses relations between these, but in general we cannot solve for one as a function of the other. Partial observables are genuinely on the same footing. (Rovelli, 2002, p. 124013-3)

The theory describes relative evolution of (gauge-dependent) variables as functions of each other. No variable is privileged as the independent one (*cf.* Montesinos *et al.*, 1992, p. 5). How does this resolve the problem of time? The idea is that coordinate time evolution and physical evolution are entirely different beasts. To get physical evolution, all one needs is a pair $\langle \mathcal{C}, \mathsf{C} \rangle$ consisting of an extended configuration space (coordinated by partial observables) and a function on $\mathsf{T}^*\mathcal{C}$ giving the dynamics. The dynamics concerns the relations between elements of $\mathcal{C}$, and though the individual elements do not have a well defined evolution, relations between them (i.e. correlations) do: they are independent of coordinate time.

The natural interpretation of Rovelli's view is that there is no physical distinction between gauge dependent and independent quantities. This implies that there are physically real quantities that are not predictable, even though we can associate a measurement procedure with them; indeed, Rovelli claims that these variables "are the quantities with the most direct physical interpretation in the theory" (*ibid.*, p. 124013-7). I find this extremely puzzling, since it cuts squarely across the usual understanding of gauge-dependent quantities. For this reason Rovelli's proposal merits serious attention from philosophers.

Before I leave this topic I should just mention one more interesting suggestion that Rovelli makes with regard to the 'flow of time'. Now, it is a consequence of the partial observables approach that time is not in any way *special*: the time variable as measured by a clock is, ontologically, on a par with any other partial observable. Yet there is something of a paradox here, for it is evident that there *is* something rather special about time, it seems that t is singled out: it seems to 'flow,' it seems to have 'direction,' and so on. Rovelli claims that it is statistical mechanics that does the singling out—he calls this 'the thermal time hypothesis". I do not discuss this any further, but it is clear that this idea is full of significance, for both philosophy of physics and time.

Part two introduces the reader to the details of loop quantum gravity, beginning with a chapter on "Quantum space". This is, perhaps, the most satisfying of the book: one gets (in just over 40 pages!) a guide to the mathematics and physics of quantum space, along with the usual proliferation of conceptual insight. As I mentioned above, the central representational device is the spin-network. Spin-networks are a stepping stone to the quantum states of the gravitational field; they are eigenstates of geometrical operators, such as area and volume, with a physical interpretation given by measurements of the geometry of some 3-dimensional surface. Mathematically, a spin-network S is an embedded graph $\langle \Gamma, j_l, i_n \rangle$, where the j_l label the spins of the 'links' of the graph and the i_n label the 'intertwiners' of the nodes where the links meet. The idea is that the number of nodes and links in a region or surface respectively determines the volume or area: the more nodes and links there are, the more volume and area there is. There are operators for the quantum versions of area and volume. Thus, for the area of surface one has the operator:

$$\mathbf{A}_l = 8\pi c^{-3}\hbar G\sqrt{j_l(j_l+1)} \qquad (2)$$

The eigenvalues of this operator have been explicitly computed and found to be discrete—likewise for the volume operator. The physical interpretation of a spin-network is, as I intimated at above, much along the lines of the classical metric on a hypersurface. The difference is, it is quantized in this case. However, if this is the case, then we clearly face the full force of the hole argument (see Rickles, 2005). To overcome this, one adopts a resolution much like the common 'superspace' resolution in the classical case. The trick is to take the physical quantum gravitational field to correspond not to a spin-network, but to a diffeomorphism equivalence class of spin-networks, or an *s-knot*. As Rovelli points out, "going from the spin-network state $\mid S\rangle$ to the s-knot state $\mid s\rangle$ we preserve the entire information in $| \, S\rangle$ except for its localization on the 3d space manifold" (2004, p. 263). Thus, whereas the spin-network state represents a discrete quantized spatial metric, the s-knot state represents a discrete quantum spatial geometry. The chunks of this geometry, says Rovelli, "do not live on the 3d manifold: they are only localized with respect to one another...They are not quantum excitations *in* space: they are quantum excitations *of* space itself" (*ibid.*, pp. 263-4). Rovelli views this as a lesson of background independence.

Rovelli claims that the discreteness result "is a direct consequence of a straightforward quantization of GR. Space geometry is quantized in the same manner in which the energy of an harmonic oscillator is quantized" (*ibid.*, p. 250). The result is strictly only a consequence given that the surfaces or regions of space are *fixed* by dynamical fields that are correlated with the region—indeed, the surfaces and regions are 'born' in such correlations. Any measurement to determine the area of volume of a bit of space will yield a result within the discrete spectrum of the relevant (partial) observable.

Now this immediately raises a worry, and this takes us back to the issues raised previously: since surfaces and regions are understood as coordinate dependent quantities living on the manifold, they are not gauge-invariant, and so are not going to be observables. In other words, the measurements of the geometry of a surface will not be complete observables. A common way of allaying this worry is to gauge-fix these quantities by having them be surfaces, say, of a material field. But Rovelli has a different response based on his distinction between partial and complete observables. He gives the following example to make his point:

> Consider a particle moving on a circle, subject to a force. Let ϕ be the angular coordinate giving the position of the particle, and p_ϕ its conjugate momentum. As we know well, p_ϕ turns out to be quantized. Now, if we write the covariant formulation of this system, we have the Wheeler-DeWitt equation
>
> $$H\psi(t,\phi) = \left(i\hbar\frac{\partial}{\partial t} - \hbar^2\frac{\partial^2}{\partial\phi^2} + V(\phi)\right)\psi(t,\phi) = 0, \tag{3}$$
>
> which, in the language of constrained systems theory, is the hamiltonian constraint equation. Notice that the momentum p_ϕ is *not* a gauge-invariant quantity: it does not commute with the operator H, that is $[p_\phi, H] \neq 0$. This happens for precisely the same reason for which area and volume are not gauge-invariant quantities in GR. But

> this does not affect the simple fact that we *can* measure p_ϕ and we *do* predict that it is quantized. (Rovelli, 2004, p. 265)

Rovelli's answer to the problem of non-gauge-invariance of area and volume matches his response in the case of p_ϕ: these quantities are *partial* observables. Now, I agree that we can say that these quantities are quantized, despite the fact that they are non-gauge-invariant: non-physical things can be quantized. But what does he mean when he says that "we *can* measure" partial observables? It simply is not the case that non-gauge-invariant quantities can be measured, for since they are gauge-dependent they are indeterministic: we won't know *which* partial observable we have measured out of a gauge-equivalence class. So surely partial observables cannot be measured.

It seems that there is something distinctly *modal* going on here, and I take the response to amount to the following: if we *were* to make a 'real' measurement of, say, the area of some surface, then we *would* get a result lying in the spectrum of the (non-gauge-invariant) area operator for that surface. In other words, making a measurement brings the area of the surface into the realm of *complete* observables, so that the quantity is then gauge-invariant. But how are we to make sense of the 'pre-measured' quantity? Does it exist in some 'potential' sense which the measurement then actualizes? Rovelli is not forthcoming on the matter. However, I have argued elsewhere (see Rickles, forthcoming) that the most sensible way to understand this situation is in *structural* terms: the area of a surface is given by correlations with a measuring device, or simply some dynamical field. This is somewhat similar to Stachel's (1993) notion of a dynamical individuation field, according to which the points of spacetime in general relativity are given only once a solution is given—i.e. once a matter distribution has been specified. However, much remains to be said on the matter.

The next three chapters are devoted to 'extensions' and 'applications'. Chapter 7 deals with dynamics and matter, where matter is "anything that is not the gravitational field" (2004, p. 276). According to Rovelli, pure vacuum quantum gravity and quantum gravity coupled to material fields (i.e. fields other than the quantum gravitational field) are conceptually no different, "the second has just some additional degrees of freedom" (*ibid.*, p. 286). Formally, of course, they are strikingly similar: the phase spaces are identical, both belonging to the class of phase spaces that characterizes Yang-Mills theory. However, the ease with which Rovelli makes this declaration masks a hidden commitment to his brand of relationalism. Without this, there are obvious conceptual differences: the former is concerned with empty spaces and the latter with non-empty spaces.

Like Kiefer, Rovelli discusses as *applications* of the theory (early regime) quantum cosmology and black hole thermodynamics. In addition, Rovelli briefly discusses a possible observable effect of loop gravity. In each case, the relevant results are derived from the discrete geometrical operators associated with the spin-network basis.

The most speculative, underdeveloped research crops up in the covariant extension of loop gravity. The natural extension gives a sum-over-paths twist on loop gravity leading to a notion of a "Quantum Spacetime," known as "Spinfoam." The full theory is then a sum-over-spinfoams. The boundary of a spin-foam is a spin-network. Rovelli traces a historical path through a multitude of different "models," leading from 3d to 4d spinfoams. Again, Rovelli is keen to expose the physical meaning of many of these concepts. However, I would liked to have a discussion explaining how the conceptual problems of the canonical theory fare in this context. However, the relationship is, as Rovelli points out (p. 363), still a matter for investigation. It is clear, though, that spinfoams promise to play a central role in the full and final theory of quantum gravity underwritten by the loop representation.

The conclusion wraps the book up with a quick summary of the physical picture that emerges from loop quantum gravity followed by a tour of the achievements and remaining tasks of this approach. There can be a 'peaceful coexistence' between general relativity and quantum mechanics, but it requires some substantive changes in our conceptual scheme: this is, in other words, revisionary physics. It is also revisionary metaphysics, since what is being proposed is a conceptual novel world in which spacetime is missing (i.e. in which dynamics concerns relations between partial observables, rather than evolution with respect to time). Whether we go along with Rovelli's relationalism or not, it is clear that there are many novel conceptual issues to be considered in loop quantum gravity; this review has barely skimmed the surface.

3 Conclusion

Rovelli's book, then, evens the balance between string theory and loop gravity, while Kiefer's exposes some of the details and assumptions of both sides of the 'strings/loops' divide. I think that Kiefer's book is a good place for interested mathematically adept philosophers to begin before considering the particular approaches in more detail. The value of Kiefer's book lies in it's relatively impartial approach with respect to loops and strings; both theories have their problems well articulated. I suggest that Kiefer's and Rovelli's books, coupled with

Zweibach's (2004) book on string theory, would provide sufficient weaponry to enable philosophers of physics to boldly enter the somewhat hair raising territory of quantum gravity. In sum then, this pair of books complement each other rather nicely as far as the philosophical investigation of quantum gravity goes; they will be certain to ease the philosopher's transition to quantum gravity. Both keep an eye on conceptual issues, and explicitly engage with interpretational aspects. Rovelli's book, in particular, comes across as an 'olive branch' to philosophers (and can, perhaps, be viewed as an implementation of Kuhn's depiction of revolutionary science). Though Kiefer's book too deals with conceptual issues, it's usefulness will ultimately rest in its scope: It provides a picture of quantum gravity in broad brushstrokes. However, I think that Rovelli has amply demonstrated that it is from the loop gravity approach that philosophers will reap most benefits. I simply cannot emphasize enough how good a job Rovelli has done in making explicit the physical commitments of the theory.

In sum, Rovelli has written a work that will make serious philosophical work on quantum gravity possible.[24] The formalism is made utterly transparent, and available to any philosopher with a decent grasp of quantum theory and a little differential geometry (i.e. to most philosophers of physics). The conceptual issues are exposed in many places, or at least plenty of earth is removed from atop them, making entry into the field particularly easy. I thank Rovelli for this and recommend that all philosophers of physics with an interest in space and time invest in his book. While I can recommend Kiefer's book to philosophers, I feel I have to issue a 'health warning' alongside it since it is very heavy going on account of the assumptions it makes: This book is only for those with strong backgrounds in mathematical physics.

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[24] Philosophers of physics who are involved in other areas of physics would also find much on interest; especially those working on the foundations of classical mechanics, general relativity, quantum theory, and quantum field theory. I therefore recommend the book as much to them as to those directly interested in quantum gravity.

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