What about General Relativity Requires Interpretation?

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1 Introduction

Several eminent philosophers, mathematicians and physicists have in recent years concluded that general relativity requires an interpretation. They arrive at seemingly the same destination having set out from diverse and varied starting points, motivated by problems ranging from the attempt to find a quantum theory of gravitation to the attempt to come to grips with the existence of generic singular structure in solutions to the Einstein field-equation. Some eminent workers in the field do not see the need for such a thing, at least not in any substantive, interesting sense. In this paper, I will sketch a defense for the latter position.

The defense is in two parts. In the first, in order to make the discussion concrete, I propose in §2 a necessary condition for the conclusion that a scientific theory stands in need of an interpretation. In §§3–5, I argue that no problem raised by proponents of the need for an interpretation satisfies the condition. Of course, to conclude from this that general relativity stands in need of no interpretation has only so much interest and force as the necessary condition I propose. The one I do propose seems to me the absolute minimum one should demand of an explication of any inarticulate sense of 'interpretation' relevant to these arguments. To the best of my knowledge, those who conclude that general relativity requires interpretation found their arguments on issues about or results in one or more of the following areas, which I address in turn, bringing to bear the condition posited in §2:

the Hole Argument (§3); the invariance of the theory under the group of spacetime diffeomorphisms (§4); and the search for a theory of quantum gravity (§5).[1] In each case, I conclude that, whatever else one may think of the importance of such problems and issues in their own right, none of them forces on us the need for a new theoretical structure in the terms of which we must interpret general relativity if we are to comprehend it. This should not come as a surprise, I think, for none of these problems and issues requires concepts or terms not already available in general relativity itself for their formulation. Along the way, I point out analogous "problems" in other, non-quantum theories that *prima facie* do not point to a gap in our comprehension of those theories that only an interpretation of some sort could bridge, suggesting that the drive to find an interpretation for general relativity based on analogous problems may have nothing to do with the character of general relativity as a physical theory, but perhaps more to do with unresolved, purely psychological dissonances its conceptual structures leave us with. This concludes the first part of the defense.

In the second part, I compare the situation for general relativity with that for quantum mechanics, a theory almost everyone agrees requires interpretive elucidation of some sort or other. I will focus on the problem of measurement. In quantum mechanics, nothing in the framework itself holds out any promise of answering the question: what is a measurement? This problem has two independent components, the combination of which lends the problem its depth and urgency. First, the question points to a lack of understanding about the production of models of physical systems in the theory; the production of such models, however, is the sole way that theoretical structures can accrue to themselves empirical content, and our comprehension of the way that such models represent physical systems is the sole guarantee we can have of the propriety and the accuracy of those models. Second, the question by its very formulation seems to require the introduction of extra-theoretical conceptual resources in order to come to grips with it, which is to say, one can appreciate the full import of the question only by way of a notion, "measurement", that one cannot express in strictly quantum mechanical terms, perhaps not even in strictly physical terms.[2] None of the problems the proponents of the need for interpretation for general relativity raise shares these features.

I do not conclude that general relativity poses no questions of important and profound philosophical interest, quite the contrary. I conclude only that such problems as there are do not suffer from conceptual incoherence in the very attempt to formulate them, and so do not indicate a lack of comprehension about the nature of the theory as a *physical* theory. We must distinguish between, on the one hand, our understanding of the physical significance of features of the physical world in abstraction from their representation in any particular theory and, on the other, our understanding of the representations a particular theory makes available of those features and how we may and may not employ those representations in that theory. Phenomena we think of as "rotational" provide a good example. There are a multitude of ways to attempt to explicate the intuitive idea that a system manifests rotatory behavior of some kind or other. Some involve observations made

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[1] All the problems mooted in these contexts, and several other problems besides, also appear in discussions of the physical significance of singular structure (incomplete, inextendible timelike curves, causal loops, failures of global hyperbolicity, and so on) in generic relativistic spacetimes. We will not address them in this paper as to do so would not shed more light on the issues than can be had from the topics we do treat. For a discussion of singular structure in relativity and its physical significance in general, see Curiel (1999).

[2] Bell (1987a, 1987b, 1987c) makes this point forcefully and eloquently.

wholly within the context of the system whose rotational status one investigates (the concavity of the surface of water in a spinning bucket); other involve observations made from outside the system (the motion of a spyglass tracking the revolution of a merry-go-round). In Newtonian mechanics, all these different methods yield identical answers to the question, "Is this system rotating, and, if so, what is its angular velocity?" As Malament (2002) shows, this is not true in generic relativistic spacetimes. This situation confronts us with three separate issues: our understanding of rotational motion in Newtonian mechanics; our understanding of rotational motion in general relativity; and our understanding of rotational motion not as treated by any particular theory but as a species of phenomena in the physical world that we attempt to comprehend, in part, by the representations we do have of it in different theories. Our understanding in the first two cases is, in principle, unproblematic. The third case is the one of most interest, because Malament's analysis shows us that the understanding general relativity affords us of the phenomena has modified and extended in profound ways the understanding Newtonian mechanics afforded us. We thus have gained a deeper understanding of rotational motion itself as a physical phenomenon *simpliciter*.

Now, we may not, and, I am sure, in the event do not, possess adequate understanding of many of the features of the physical world that general relativity represents. It may turn out, for example, that an element of general relativity that we find unproblematic in our current state of understanding of the physical world will turn out to require radical alteration as our empirical knowledge increases, as with the notion of "simultaneity" in the move from Newtonian mechanics to special relativity as demanded by the ever-increasing body of experimental data that Newtonian mechanics, as a foundation for Maxwell theory, could not accomodate. Diffeomorphism invariance, for instance, may turn out to be just such an element. Were we to work out what, if anything, there may be of intrinsic physical significance in the phenomena whose representation in general relativity includes in part diffeomorphism invariance of its structure, then perhaps we would see that our understanding of that part of those phenomena was flawed. This, however, would not show that we lacked an adequate understanding of the role diffeomorphism invariance plays in general relativity that only a suitable interpretation could have rectified. It would rather show that general relativity is inadequate as a physical theory in its treatment of those features of the physical world it purports to represent by, in part, the use of diffeomorphism invariance.

In consequence, attempts to improve our understanding of those parts of the physical world should not focus on the significance of the representations of them that general relativity provides; the attempts should, to the contrary, focus on the ways that general relativity fails to provide adequate representations of them, if it does so. In the event, we know of no phenomena that general relativity treats for which it provides inadequate models. Unlike the time prior to the formulation of special relativity, no body of otherwise inexplicable experimental data, in this area of physics at least, cries out for the comprehension that only a dramatic improvement in our conceptual resources could provide. This, to my mind, is the great problem facing physics today, or at least that part of it comprehended by general relativity: not that we understand too little or do not understand clearly enough; rather that we understand too well but do not *know* enough. The next great advance in these areas of physics, I suspect, is not awaiting a Swiss patent-office clerk to revolutionize the conceptual resources we have for understanding the physical world. We are not ready for that yet.

We rather await a Fizeau, a Hertz, a Michelson and Morley, to provide us the body of otherwise inexplicable experimental knowledge that will furnish the raw material for and *drive* the hoped-for conceptual revolution.[3]

2 What Sort of Interpretation Is Required, If Any?

The question we consider ought not be whether general relativity requires an "interpretation" *simpliciter*—until we agree on what one means by the term, a disagreement on the matter would be only an argument about what words we feel most comfortable using to describe the questions the theory presents us with. *De gustibus non disputandum.* The interesting question is rather what one may mean by claiming that general relativity stands in need of one, and whether in any of the interesting senses it does indeed stand lacking.

One of the most minimal and, I think, uncontroversial demands to make of any sense of "interpretation" relevant to us here is that it include at least the idea that such a thing provide an explication of the elements of one semiotical framework in the terms of another. In slightly more formal though no less ambiguous terms, it is a mapping of the syntactic elements of one semiotical framework to those of another in such a way as to preserve some important set of semantical structures.[4] A "semiotical framework" here is a theoretical structure that provides the resources to construct appropriate models of physical systems. Let us say that we have a framework some aspect of the semantic content of which is, for one reason or another, poorly comprehended. One natural route of attack in attempting to comprehend it better is to try to find another semiotical framework that we do comprehend better, in the terms of which we can construct a representation of the poorly comprehended part of the first. The representation must in some important sense faithully recapitulate in the second framework the poorly comprehended structure of the first—it must preserve the structure.

This idea, the "preservation of structure", plays the crucial role. Klein's model of Lobachevskian geometry in the Euclidean plane provides a wonderful example of its significance, as does Helmholtz's (1870) depiction, in the terms of our own naive language of sense-perceptions, of the possible perceptual experiences of a person residing in a world of curved space, and Peirce's (1898, Lecture VIII, pp. 252–253) depiction of the possible perceptual experiences of a person traveling through a multiply connected space. In each case, the intrinsic structure of an unfamiliar, *prima facie* mysterious notion is faithfully rendered in the terms of familiar, understood frameworks. These examples suggest a way to characterize "preservation of structure" a little more precisely and clearly (though not much). Say we have two semiotical frameworks, one, $\mathcal{T}$, to be interpreted in the terms of the other, $\mathcal{S}$. An *interpretation*, then, is an injective mapping $\phi : \mathcal{S} \to \mathcal{T}$ such that for every appropriate model μ in $\mathcal{T}$ of a physical system, the model $\phi^{-1}[\mu]$ defined in $\mathcal{S}$ by the preimages of all elements of μ composed in the same formal structure as in μ is an appropriate model of the same physical

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[3] In this vein, I find it suggestive that I have never read or heard of an experimentalist who complins about a lack of conceptual clarity hindering the experimental search for clues to a theory of quantum gravity, only theoreticians complaining about the difficulties in making theoretical progress.

[4] It differs from translation in at least the fact that we do not require the mapping to have a well defined inverse.

system. One of the most interesting aspects of these interpretations is that, in the end, they make themselves otiose. Once we have an understanding of the semantic content of a system based on that of other, already understood systems, we often no longer require the resources of those other systems in order to employ the resources of the first in explicable, productive ways.

Now, when we think of a physical theory as a semiotical system in the broad sense, the most important set of semantical structures it includes are largely informed by the semantic content of the models it provides of physical systems. Indeed, any semantical structure of any interest accruing to it in its role as a *physical* theory must in the end depend on the semantical content of those models, on our understanding of the way that the semantic content of those models bears on our understanding and knowledge of the physical world. The theory may have other semantical structures of interest to the mathematician, the poet, the painter, the investment banker, *et al.*, independent of its role as a theory of physics, but those do not interest us here. A minimally necessary part of the required understanding of the semantical content of the theory's models lies in the fact that we can use that content as part of the basis for the planning and performance of experiments, the empirical comprehension of the results of those experiments, the construction of theoretical representations of those experiments, and the production of novel predictions and characterizations of physical system based on those theoretical models, all in such a way as to profitably bear on our knowledge and understanding of the physical world, knowledge and understanding both as achieved state and as ongoing enterprise. Bondi (1962, p. 132), in a paper on gravitational energy, puts his finger on the heart of the issue: "*Good physics is potential engineering.*"[5]

A physical theory, then, requires an interpretation in the sense relevant to us here only if we cannot comprehend, without recourse to the resources of an extra-theoretical framework, the semantic content of its models of physical systems. This minimal condition on the adequacy for an explication of "interpretation" has the virtues of weakness, clarity and manifest physical content. I do not think it sets up a straw man.

3 The Hole Argument

Einstein originally formulated the Hole Argument to highlight what he considered to be problems with his inchoate theory regarding determinism.[6] In brief, the Hole Argument claims that a distribution of ponderable matter sources would not by itself serve to fix the physical state (the "gravitational field") of a 4-space that itself contains no ponderable matter but is bounded by that distribution. Let O be an open region in spacetime devoid of ponderable matter but entirely bounded by it. Let a ϕ be a diffeomorphism that is the identity outside a compact subset K contained in O, and is not the identity in the interior of K. Then K applied to the spacetime metric leaves the physical state of spacetime undisturbed outside O but yields a different metric tensor inside O, and thus a different gravitational field. In consequence, the distribution of ponderable matter in a generally covariant

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[5] Italics are Bondi's.

[6] See Einstein (1914) and Einstein and Grossmann (1914) for two versions of the original argument, and Norton (1989, 1993) for historical and critical discussion.

theory does not in general suffice to fix the physical state of the gravitational field.[7] That is the argument.

Einstein saw it, initially, as a flaw in a covariant field theory that a distribution of ponderable matter sources would not by itself serve to fix the explicit form of the metric tensor in a region such as O, but rather would fix it only up to diffeomorphism. He took this to show that such theories manifest an unacceptable form of indeterminism. Consensus today seems to have it that the proper way of understanding the Hole Argument places it rather at the heart of the question about the ontic status of spacetime points. Its lesson, so claimed, is that one cannot identifiably individuate spacetime points without reliance on metrical structure, that there is no "bare manifold of points" under the metrical field, on account of the fact that the action of ϕ is most properly understood as a point-transformation of the manifold.[8] I do not see why one is driven to the latter conclusion. There is no logical contradiction in taking the image of a point under the action of ϕ to be "the same spacetime point" as its pre-image, as depicted in a different presentation of spacetime, irrespective of metric structure. If one wants to respond that bare spacetime points *per se* are unobservable, and so have no place in physics, I would not necessarily disagree, but neither should I think that one requires the Hole Argument to make the point. Nothing I can see militates in favor of taking the Hole Argument as exhibiting something of intrinsic physical significance that our current understanding of general relativity cannot comprehend.

I think one can make a stronger statement: the two views of the argument rest on a misapprehension of the role of ponderable matter in the initial-value formulation of the Einstein field-equation. Einstein's initial understanding of the force of the argument is obviated by the observation that, if one takes a proper spacelike 3-slice in a spacetime and imposes appropriate initial data on it, then the metrical structure everywhere in that slice's domain of dependence is determined, including in any "otherwise empty" hole regions. It is no more a problem that the distribution of ponderable matter by itself does not fix the explicit diffeomorphic presentation of the metric than it is that the distribution of charges in Maxwell theory does not fix a unique solution of the Maxwell equations. Indeed, I should think it much less of a problem, for in the case of the Maxwell field we cannot determine a *physically* unique solution without imposing boundary conditions; otherwise, we are always free to add a field with vanishing divergence and curl to a solution to yield another that will have different physical effects on charged bodies. In general relativity, one does not need to do anything of the sort to determine a physically unique solution.

Likewise, the current view of the force of the Hole Argument is obviated by the fact that the application of ϕ to the metric results only in a different presentation of the same metrical structure. All observers, no matter which presentation of the metric they use in their respective models, will agree on the physically relevant parts of the outcomes of all experiments. I think the current view of the argument also rests on the implicit demand that the distribution of ponderable matter on its own suffice for the determination of all possibly relevant physical structure in the spacetime manifold,

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[7] One can run a similar argument in the context of Hamiltonian mechanics, substituting "phase space" for "spacetime manifold" and "symplectomorphism" for "diffeomorphism". Does that show anything of intrinsic physical significance?

[8] See, *e.g.*, Stachel (1989) and Belot (1996).

including the structure of the manifold as a point-set. On this view, the metrical structure, and all other structure, must be derived from the distribution of ponderable matter. As in the Maxwell case, I think it makes more sense to say that the metrical structure has a subsistence on its own, whatever exactly that may come to, that informs and is informed by the distribution of ponderable matter. In some regions of spacetime, those we think of as occupied by ponderable matter, the curvature has non-trivial contributions from terms involving the Ricci tensor; in others it does not. This fact by itself does not allow one to privilege the metrical structure of one of those kinds of regions over that of the other when addressing questions of the existence of spacetime structure.

In the initial-value formulation of general relativity, for instance, one fixes for the Einstein field-equation initial data in the form of a pseudo-Riemannian metric and its "conjugate momentum" on a three-dimensional manifold.[9] The three-dimensional manifold is to be thought of as a spacelike hypersurface—an embedded submanifold—in the full spacetime that is the resulting solution to the Einstein field-equation with that initial data. Whether the intrinsic Riemann tensor on the initial manifold has a non-trivial Ricci tensor associated with it is irrelevant to the existence of a unique solution in the domain of dependence of the three-dimensional manifold, considered as an embedded, spacelike submanifold in the full spacetime.

To see the Hole argument as a problem in general relativity that requires for its resolution an interpretation of the theory, one must believe ponderable matter to be "privileged" in some deep sense over metrical structure, that is, that Ricci curvature has some sort of metaphysical primacy over Weyl curvature. The desire to privilege one of them must arise wholly from extra-theoretical considerations, which themselves do suggest the need for an interpretation. Because nothing in the theory itself demands or requires or suggests that one privilege one over the other in any deep sense—indeed, nothing in the theory by itself even allows one to do so in any principled way—one must rather assume to start with that general relativity has a particular, preferred conceptual structure imposed from the outside, and then on this basis conclude that the structure is inadequate for the full comprehension of general relativity as a physical theory; but this line of argument is circular.

I believe, with only circumstantial evidence, that the misapprehension about the initial-value formulation has its roots in a particular way of understanding the semantics of the Einstein field-equation. Crudely put, I think that those who hold the view that the Hole Argument leads to indeterminism or bears on the ontic statis of spacetime points assume that the equation expresses some sort of causal statement of the form, "ponderable matter gives rise to, determines, spacetime metrical structure." Consider the fact that the Hole argument can be run in reverse, as it were, pertaining to regions not devoid of ponderable matter. If we fix the metric on all of spacetime, then this will not fix the diffeomorphic presentation of the stress-energy tensor anywhere it differs from zero. Any diffeomorphism we apply to the metric we also must apply to the stress-energy tensor, so changing its form. The only reason I can think of that those who are puzzled by the Hole argument are not puzzled by this circumstance is that, for whatever reason, they privilege ponderable matter over metric structure. g_{ab}, however, is just as much, or as little, a physical field as is T_{ab}, in so far as the Einstein field-equation shows that each determines the other.

I think a more helpful view of the equation can be had by looking at the analogous question of

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[9] See, for example, Wald (1984, ch. 10, pp. 243–268).

the relation between force and acceleration as expressed in Newton's Second Law. Applied force, in Newtonian mechanics, does not "cause" or "give rise to" acceleration in any deep sense. One may as well regard acceleration as just the measure of the applied force as one may regard the applied force as just the measure of the acceleration, with the coefficient of proportionality given by, respectively, the magnitude of the inertial mass and of the inverse of the inertial mass. The attempt to view force as "giving rise" to acceleration in any causal sense profoundly misconstrues the relation between them.

In a deliciously wicked (and entirely just) review of the second volume of Thompson (Lord Kelvin) and Tait's *Natural Philosophy*, Maxwell (1879, pp. 779–780) takes them to task for forwarding a "Manichean doctrine of the innate depravity of matter, whereby it is disabled from yielding to the influence of a moving force unless that force actually spends itself on it":[10]

> [T]he capacity of the student is called upon to accept the following statement [quoted from Thomson and Tait]:—
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> "Matter has an innate power of resisting external influences, so that every body, as far as it can, remains at rest or moves uniformly in a straight line."
>
> Is it a fact that "matter" has any power, either innate or acquired, of resisting external influences? Does not every force which acts on a body always produce exactly the change in the motion of the body by which its value, as a force, is reckoned? Is a cup of tea to be accused of having an innate power of resisting the sweetening influence of sugar, because it persistently refuses to turn sweet unless the sugar is actually put into it?

In this light, one perhaps ought rather say, with all due respect for anachronism, that the equations of motion of physical theories express formal Aristotelian causes rather than efficient ones.[11] Indeed, in so far as the Maxwell equations do determine the distribution of charged matter, one might rather

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[10] Maxwell (1878), in a whimsical review of a whimsical book entitled *Paradoxical Philosophy* (written pseudonymously by P. Tait and B. Stewart), offers as well the following gem:

> "I feel myself compelled to believe," says the learned Doctor [whose work Maxwell is reviewing], "that all kinds of matter have their motions accompanied with certain simple sensations. In a word, all matter is, in some occult sense alive." This is what we may call the "levelling up" policy. . . .
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> [The learned Doctor] can draw no line across the great chain of being, and say that sensation and consciousness do not extend below that line. He cannot doubt that every molecule possesses something related, though distantly, to sensation, "since each one feels the presence, the particular condition, the peculiar forces of the other, and, accordingly, has the inclination to move, and under circumstances really begins to move—becomes alive, as it were;" . . . "If, therefore, the molecules feel something which is related to sensation, then this must be pleasure if they can respond to attraction and repulsion, *i.e.*, follow their inclination or disinclination; it must be displeasure if they are forced to execute some opposite movement, and it must be neither pleasure nor displeasure if they remain at rest."
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> Prof. von Nägeli [, the learned Doctor,] must have forgotten his dynamics, or he would have remembered that the molecules, like the planets, move along like the blessed gods. They cannot be disturbed from the path of their choice by the action of any forces, for they have a constant and perpetual will to render to every force precisely that amount of deflexion which is due to it. Their condition must, therefore, be one of unmixed and unbroken pleasure.

[11] Stein (unpub.a) gives an extended, helpful discussion of many matters relating to this idea.

think any relation of dependence in this case runs the opposite direction. It is interesting to note that the case of the Einstein field-equation is different in a striking way in this regard not only from Maxwell theory but also from Newtonian mechanics. In general relativity, to fix the metric on a three-dimensional spacelike submanifold—as appropriate initial data—determines the solution to the Einstein field-equation, that is, the stress-energy tensor, everywhere both to the future and the past in the domain of dependence of that submanifold, without the need to fix boundary conditions and without the need to fix the physical state anywhere else besides that submanifold, not to its future or past. As we already remarked, the Maxwell equation does not determine the Maxwell field given only the distribution of charges without boundary conditions. In Newtonian mechanics, initial data at a single moment determines the evolution of a system for some interval into the future *only* if the state of the environment is fixed and known during that entire interval. In any event, I think it simpler and clearer to say that metrical structure in general relativity determines the distribution of ponderable stress-energy, as the distribution of ponderable stress-energy determines the metrical structure, nothing more.

I want to emphasize that my argument has no bearing at all on the question itself of whether or not spacetime points, as presumed by and characterized in general relativity, exist in some lofty or mundane sense. I claim only that, properly construed, the Hole Argument does not bear on the question. I am far from convinced that the question of the existence of spacetime points has ever been well posed. What possible difference could an answer to it make, one way or another to the proper comprehension of the performance of an experiment or the proper construction of a model of a physical system in the context of general relativity?

I do know of one context in which a precise formulation and answer to the question conceivably bears on the significance of models of physical systems with more or less clear physical import, the prediction of the spectra of spatiotemporal area and volume in the context of loop quantum gravity by Rovelli and Smolin (1995). In this case, perhaps, one could consider those quanta of size themselves as "spacetime points", or more precisely as a conceptual clarification of the significance and status of the naive, physical idea of spacetime points, as provided by a proposal for a theory of quantum gravity. No matter how one views this case, however, it does not bear on the understanding of general relativity.

4 Diffeomorphic Freedom

The group of diffeomorphisms of the spacetime manifold, in some sense or other, can be thought of as a group of symmetries of the physical phenomena treated by general relativity. Invariance of solutions to the Einstein field-equation under the action of diffeomorphisms, however, is not a true symmetry in the sense captured by Neother's theorem, nor in the sense of a gauge as employed in, *e.g.*, Yang-Mills theory, nor in any other well understood sense pertaining to other physical theories. In consequence there has arisen the issue of what meaning this diffeomorphic freedom has.

I think the most unproblematic and uncontroversial claim one can make about diffeomorphic freedom is that it embodies an irremediable mathematical ambiguity in the apparatus provided by general relativity for the modeling of experiments: the choice of the presentation of the spacetime manifold and metric one uses to model an experiment is fixed only up to diffeomorphism. A comparison is edifying. Classical mechanics, as embodied in Lagrangian and Hamiltonian mechanics, shares similar ambiguities, slightly different in each formulation of the theory. In Lagrangian mechanics, one is free to choose the Lagrangian function itself up to the addition of a vertical 1-form on the tangent bundle of configuration space (or, in more traditions terms, up to the addition of a total-time derivative of a function of configuration coordinates) without changing the family of solutions the Lagrangian determines.[12] In Hamiltonian mechanics, one is free to choose any symplectomorphism between the space of states and the cotangent bundle of configuration space, *i.e.*, the choice of symplectomorphic presentation of phase space, without changing, in a precise sense, the family of solutions the Hamiltonian function determines.[13] One feels no lack of understanding of Lagrangian mechanics, no lacuna in its conceptual resources, merely because one is free to choose the form of the Lagrangian more or less freely, as one is not driven to investigate the ontic status of points in phase space or of the physical quantities whose values one uses to label those points, which ones get nominated 'configuration' and which 'momentum', merely because one is free to choose whatever symplectomorphism one likes in its presentation.

The choice of Lagrangian or the choice of symplectomorphism rests on nothing more than pragmatic considerations of the type adumbrated by Carnap (1956) in his discussion of the choice of a linguistic framework for the investigation of philosophical and physical problems,[14] considerations determined by what R. Geroch calls, somewhat archly and entirely aptly, "psychology".[15] One chooses on the basis of nothing more than what puts one at ease in any of a variety of ways, from pragmatic considerations such as what will be simple or useful for a particular investigation, to those based on historical custom and æsthetic predilection. It is clear in these cases that the existence of inevitable, more or less arbitrary, non-physical elements in the presentation of the models of a theory by itself does not require of one the provision of an interpretation of either Lagrangian or Hamiltonian mechanics. More to the point, it is clear in these cases that the physical significance of the theory's models is not masked or polluted by the unavoidable arbitrariness in the details of their presentations. In the same way, the diffeomorphic freedom in the presentation of relativistic spacetimes does not *ipso facto* demand an interpretation, in so far as it in no way prevents us from focusing on and investigating what is of true physical relevance in systems that general relativity models, what one may think of as the intrinsic physics of the systems. As to what "intrinsic physics" may mean: it is what Alain Connes was trying to get at, I think, during a conversation on the nature of general relativity as a physical theory, when he pointedly asked me what information I would communicate to beings in a different universe in an attempt to describe to them the spacetime metric of our own. It is what all possible observers in all possible states as schematically represented in a model of an experiment would agree on, no matter the presentation of the theory used to produce the models.

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[12] See, *e.g.*, Curiel (unpub.).

[13] *Ad loc.*

[14] This is not to say that I consider the choice of a Lagrangian or a symplectomorphism to be the choice of a Carnapian linguistic framework, only that the sorts of considerations that go into each choice are similar.

[15] Geroch uses the term in conversation and lectures almost to the point of mannerism, but it is a useful mannerism, always illuminating to his interlocutors and audience.

In response to this line of thought, an opponent might claim that the analogy is no good. General relativity, he or she could say, is a fundamental theory of a class of physical systems (relativistic spacetimes), whereas Lagrangian and Hamiltonian mechanics are generic frameworks within which one formulates physical theories such as general relativity. The ambiguity in the presentation of models that diffeomorphism invariance yields must indicate a fact of *physical* significance, in so far as it is a fundamental, ineliminable structure in a *physical* theory. To dismiss diffeomorphism invariance as having no intrinsic physical significance, the opponent would conclude, is not to take seriously enough on its own terms the formal structures of our best physical theory of spacetime structure. Rovelli (2000, p. 118), for example, implicitly suggests a response like this, in his favorable reference to the argument of Stein (unpub.b) that Poincaré's failure to take the Lorentz transformations seriously enough as a formal representation of (part of) the physical dynamics of physical systems makes it plausible that he could not have discovered special relativity himself in the first place, and that in the event it drove his subsequent refusal to accept Einstein's proposal of it.

I agree with Rovelli on the heuristic value of Stein's conjecture that Poincaré's refusal to take the math seriously enough in the context of physical theories hindered his work as a physicist, but I do not think that the same lesson applies here. The important question in both cases is not *whether* we ought to take the math seriously enough, but *what* math we must take seriously. In the years before Einstein's proposal of special relativity, no one knew what the Lorentz transformations meant in the sense that no one knew how to understand them as (part of) a representation of the behavior of physical systems. It was not clear how to devise and perform experiments to probe their manifestation in physical phenomena, or even whether experiments could reveal their effects at all—that is to say, no one knew how to extract what was of intrinsic physical significance from models in which the transformations played a role, whether, indeed, the role the transformations played did reflect anything of intrinsic physical significance. In that state of affairs, we did require an interpretation of the transformations to enable us to advance our understanding of that part of the physical world. Einstein provided that interpretation in the person of the theory of special relativity: the theory itself provided the interpretation of the Lorentz transformations. Now that the theory is in place and the role of the Lorentz transformations in it well understood, no further clarification of their significance *in* the theory is required.

In a similar vein, the comprehension of special relativity's dismissal of the idea of absolute simultaneity did not require an interpretation of the theory, in any sense of the term; it required only that investigators come to terms with the fact that the fundamental principles of the theory do not allow for the rigorous, physically relevant explication of at least some of the fundamental terms of Newtonian physics. Special relativity did have to demonstrate that it could model experiments in such a way as to allow for the comprehension of those models in the terms of the semantics of the experimental physics of the day, both those experiments that Newtonian mechanics could and could not handle, and to do so, moreover, without the need for extra-theoretical resources. It did that, and so it became clear that "absolute simultaneity" in a global sense is not a notion with any natural or even merely reasonable explication in the theory. That demonstration also showed something of deeper significance, that absolute simultaneity is not a notion we should rely on in a search for deeper, better theories, but that fact does not bear on our understanding of special relativity as a theory in its own right.

The same holds for diffeomorphism invariance in general relativity. Before the theory was established, it was not clear what it could have meant for the representation of physical systems to be invariant under the full group of spacetime diffeomorphisms, as Einstein's struggles with the Hole Argument poignantly shows. The establishment and comprehension of general relativity itself provides an interpretation for the significance of diffeomorphism invariance, a significance we do understand well in the context of the theory: to transform a model of a physical system by the action of a spacetime diffeomorphism does nothing more than change the presentation of the model, but does not alter the intrinsic physics that the model depicts. It is an inevitable ambiguity in our mode of presentation in the theory. Perhaps in some other context a structure purporting to explicate the same aspect of our understanding of physical phenomena will have a different significance, but that has no bearing on the quality of our understanding of its role in general relativity.

Perhaps not remarkably, quantum mechanics shares this sort of inevitable ambiguity in the presentation of models. The Hilbert space of a quantum system, including the algebra of self-adjoint operators, does not determine its underlying "configuration space" any more than does the cotangent bundle in Hamiltonian mechanics. Indeed, as is well known, every separable Hilbert space is isomorphic to every other separable Hilbert space, and the algebra of self-adjoint operators on any presentation of one is itself fixed only up to unitary equivalence. In this case, however, because we do not understand the conceptual resources of quantum mechanics as a theory, this ambiguity may point to the need for its physical clarification. We do understand enough to say either way.

It is an interesting fact, surely one worth puzzling over, that all of our physical theories suffer inevitable ambiguity of one sort or another in the models they render of physical systems. It may point to profound questions about our capacity to comprehend the physical world, or point to constraints on the form our comprehension can take or on the content it can achieve. This fact, however, in no case (except perhaps that of quantum mechanics) requires the use of extra-theoretical machinery for us to grasp what is of true physical significance in the models the theories render. In particular, nothing in general relativity by itself demands or suggests that one attempt to understand diffeomorphic theory in any deeper sense than the one sketched here. One can, again, impose extra-theoretical conceptual resources on the theory so as to render the diffeomorphic freedom *prima facie* mysterious, but, without having explained why the obvious and clear conceptual resources the theory makes available do not suffice, to argue on the basis of external ones that general relativity is deficient would be circular.

To clarify the sense in which the group of diffeomorphisms leaves structure invariant in general relativity is a delicate matter, but it does not follow from this delicacy that general relativity stands in need of an interpretation in any interesting sense. Eddington (1923, pp. 120–1) puts it best:

> If we are to surround ourselves with a perceptual world at all, we must recognize as substance that which has some element of permanence. We may not be able to explain how the mind recognizes as substantial the world-tensor $[R_{ab} - \frac{1}{2}g_{ab}R]$, but we can see that it could not well recognize anything simpler. There are no doubt minds which have not this predisposition to regard as substantial the things which are permanent; but we shut them up in lunatic asylums.

5 Quantum Gravity

Many eminent workers in the field of quantum gravity suggest that at least part of the reason we have so far had little if any success in the search for a viable theory points to some lack of understanding of the conceptual resources of general relativity. We require a proper interpretation of general relativity, they argue, in order to find a path to the deeper theory. They tend to focus on two issues in these arguments: the significance of time and temporal evolution in the theory;[16] and the ontic status of spacetime points.[17] We have already discussed in §4 the issue of the ontic status of spacetime points, so we will not repeat it here.

In brief, the problem of time bears on attempts to formulate a theory of quantum gravity in a canonical framework, as, for example, in the attempt to impose a Dirac-style quantization on general relativity considered as a constrained Hamiltonian system. The role time plays in general relativity differs from that in canonical theories in ways so profound as to make it difficult even to begin to see how to reconcile them. One cannot in any principled way impose a preferred temporal frame in a generic, relativistic spacetime. No congruence of timelike curves has any privileged status over any other. A privileged temporal frame, however, lies at the heart of a canonical theory, picked out by the dynamical evolution determined by a system's Hamiltonian operator. That we do not see how to reconcile the role time plays in general relativity with that in canonical theories, however, does not by itself show that we lack understanding of that role in general relativity, or of that role in canonical theories, for that matter. The two may be truly incompatible in a fundamental way, or we may not yet have found the way to reconcile them within the constraints imposed by our understandings of each in the confines of their respective theories. We cannot say in our current understanding of the physical world.

Still, workers in the field hold that the role of time and the ontic status of spacetime points in the theory, and other issues like them, *are* problems because they make it difficult to attempt to "quantize" general relativity in any of the standard ways. The thought then runs that a resolution of the perceived conceptual problems in general relativity holds out the promise of guidance towards a theory of quantum gravity. There are two questions to keep separate here: first, whether it is plausible that the resolution of conceptual difficulties in general relativity, if there are any, would provide clues to a theory of quantum gravity; and second, whether the difficulties that the standard procedures of quantization face when applied to general relativity can in any plausible sense be attributed to a failure on our part in understanding the theory.

Now, if there is one feature of a viable theory of quantum gravity that one can foresee with some assurance, it is that the theory will deal with issues of space, time and spacetime in ways far different than does general relativity, on account of the fact that general relativity has no superposition principle, no uncertainty principle and no measurement problem. If we lack an understanding of the significance of time or of the ontic status of spacetime points as physical phenomena in abstraction from its treatment by any particular theory, and we also believe that general relativity is not a fundamental theory of the world, it is difficult to see why an interpretation of general relativity that

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[16] See, *e.g.*, Kuchař (1992) and Isham (1993) for extended reviews of different forms of the problem and proposed solutions.

[17] See, *e.g.*, Rovelli (2000) for an exposition of the issue.

clarified those problems should give us any richer understanding of the structure of the physical world in such a way as to illuminate a path to a viable theory of quantum gravity. Those who hold that general relativity requires an interpretation in order that it may suggest paths to a theory of quantum gravity *a fortiori* do not view general relativity as a “fundamental” theory, whatever exactly that may come to. This seems to represent a tension in their view, for it is difficult to see even apart from this particular case how an interpretation of any higher-order physical theory could shed light on the conceptual resources of a more fundamental theory to which it reduces. On the face it, the clarification of conceptual structure seems always to point in the opposite direction.

Consider the move from pre-Newtonian physics to the system Newton presents in *Principia.* In light of our present state of knowledge, we are tempted to say something like the following: though it is often claimed that the Ptolemaic and the Copernican systems are observationally indistinguishable, in fact they are distinguishable in so far as each predicts either an angular momentum for each of the planets different from that predicted by the other, or an inertial mass for each planet different from that by the other. If we assume that the inertial mass of each planet is the same in each theory, then, because the effective axis and angular velocity of orbital rotation of each planet is different in each, one will predict for it a different angular momentum than does the other; likewise, if one adjusts the inertial mass of each planet in the different theories so as to make the angular momentum predicted by the one accord with that by the other, then one will have *eo ipso* made the masses each attributes to the planets different from those the other does. (Even in the latter case, the angular momentum of each planet would have a different center in the two theories, but we let that pass.) Strictly speaking, however, the Ptolemaic and the Copernican systems could not in their own time, not even in principle, have made those differing predictions, for the simple reason that neither had the theoretical resources to represent either inertial mass or angular momentum in anything like the senses that the work of Galileo, Huygens and Newton rendered to the concepts. Neither system had a notion of kinematics or of dynamics that would have allowed for the understanding of these quantities. Neither had a kinematical representation of a measure of a body’s inertia (the *vis inertiæ* of *Principia*) adequate for the representation of the motion of a body under an impressed force; and neither had a representation of anything analogous to equations of motion in the sense of Newton’s Laws adequate for the computation of dynamic quantities such as angular momentum. It thus could not have been either wrong or right to make the claim, strictly within the context of either of the two historical systems, that the one predicted different planetary angular momenta or inertial masses than the other, for one cannot meaningfully formulate the propositions in either of them. Indeed, with regard to the more fundamental putative difference between the two systems—whether the Earth or the Sun was at rest—neither had a concept of motion sophisticated enough to lend the question any clear sense, much more to make it amenable to investigation by either theoretical or observational means.

In light of the ground-breaking work of Galileo on inertial mass and motion, of Huygens and Newton on inertial mass and rotational motion, and of Newton on dynamics and his theory of universal gravity, it became clear that, *within the context of the new system of mechanics*, the analogue of the Copernican system is the correct one, as Newton (1726, Book III, Theorem XII, annotation, pp. 419–420) himself pointed out, albeit in a characteristically circumspect and exact manner. It became clear that this was correct, moreover, just in so far as the new system had the conceptual resources to make meaningful the controverted propositions about rest and motion, and the propositions we can now pose about differences between inertial mass and angular momentum. The system of *Principia*, however, did not provide an interpretation of the Ptolemaic and the Copernican systems in the sense relevant to our arguments. It was not a clarification of the notion in the context of the old theory that showed the way to the new theory; rather the recognition that the concept as represented in the old theory was inadequate for the new tasks at hand, which included the development of the idea of the equations of motion of a physical system, provided impetus for and guidance in the search for the new theory. This may be the case with, say, time as represented in general relativity *vis-à-vis* what a theory of quantum gravity will require for its proper treatment of time, but, again, this would not show that our understanding of general relativity itself is in any way deficient. It may show that its conceptual resources are inadequate for the physical questions we now want to ask, but that is a different matter.

Now, for the second question—whether the difficulties that the standard procedures of quantization face when applied to general relativity can in any plausible sense be attributed to a failure on our part in understanding the theory—I would reply that we ought not be "quantizing" anything in the first place. Maxwell and Boltzmann did not "statisticalize" Navier-Stokes theory in order to arrive at a viable account of molecular kinetics. One would not even have known where to start if one were to have attempted to have followed that idea. In the event, Maxwell and Boltzmann had to attempt to formulate the relevant concepts and the relations among them, to clarify them with no particular regard at first for whether or not those concepts constituted in some sense a "proper reduction" or "clarification" of the conceptual apparatus of Navier-Stokes theory. They based their investigations to a large part on the known, well entrenched body of experimental knowledge they were trying to produce a better theory of. This is not to say that they did not use higher-order theories such as Navier-Stokes in their work, to help them, for instance, in imposing useful organization on parts of the body of experimental knowledge. In doing so, however, they did not attempt directly to refine or clarify the conceptual resources of those theories in their search for a better one. For example, Boltzmann's treatment of the Second Law did not depend on the resources of any particular theory. He could deal with the phenomena it pertains to outside the context of any definite theory, as Carnot, S. (1824) himself did in his analysis of heat cycles, which relied on nothing more than phenomenal laws such as those of Boyle and Charles. Only after the new theory had been put into something of a definite form were we in a position to verify that the Maxwell-Boltzmann collision equation, in the context of appropriate approximations and idealizations, does lead to the Navier-Stokes equations.[18]

That we find ourselves in the position of having to attempt to work from the top down in the case of quantum gravity, trying to base our search on the conceptual resources of theories thought to be less fundamental, rather than tackling the formulation of such a theory head on, shows that we lack experimental knowledge, not necessarily conceptual understanding, at least with respect to the resources of general relativity—we need hard experimental data that we do not know how to accomodate in the context of general relativity to move forward in the understanding of the physical

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[18] See, for instance, Sommerfeld (1964, ch. v, §§41–43, pp. 293–318) for a discussion.

regime that general relativity treats. That general relativity in and by itself does not suggest a way to formulate it as a quantum theory does not show that general relativity itself, as a complete physical theory, stands lacking with respect to semantic content, with respect to our understanding of the models it provides of physical systems. I think it rather suggests that our lack of a secure and cogent conceptual understanding of quantum mechanics holds us back, and, because of that very fact, the clarification of quantum mechanics' resources, it seems to me, likely does hold out the promise for clues to a theory of quantum gravity.

6 Comparison to Quantum Mechanics

> In my childhood, the legend was current that only twelve men in the world understood Einstein's theory. Nowadays, relativity is quite tame ; but . . . *nobody* yet understands the quantum theory.
>
> Stein (1972, pp. 367–368)

The theory that philosophers and physicists have before now felt most required an interpretation is quantum mechanics. With regard to the impetus for demanding an interpretation of a theory, the difference between general relativity on the one hand and quantum mechanics on the other could not be more complete : it is the difference between ambiguity in the machinery of the former theory that models experiments, and ambiguity as to what counts, according to the latter theory, *as* an experiment.[19] Quantum mechanics requires at the end of the day a *deus ex machina* to arrive at a mathematical model of an experiment that offers definite predictions for the readings of measurement devices—in modeling a physical system, one must decide at what point in a system's physical evolution the wave-function "collapses" in order to extract a definite prediction for the reading of a measuring apparatus, which is to say in more classical terms, one must decide what moment in the system's interaction with its environment constitutes an "experiment".[20] General relativity, by contrast, while offering up many superficially distinct ways of modeling any given experiment, does not require one to classify different moments in a system's evolution as constituting or not an "experiment" in order to arrive at definite predictions for the readings of experimental apparatus. All moments are treated on an equal footing by the theories. This is not to say that I

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[19] Thermodynamics, in particular the Second Law, has a sort of ambiguity about it similar to that of quantum mechanics. Locally, it is not clear that the Second Law is even valid unless one is extremely judicious in picking one's system of study, which includes selecting the interval of that system's physical evolution during which one will probe it, and *a fortiori* selecting the types of interactions the system will have with its environment, as the appropriate ones for the making of thermodynamical measurements—which is to say that, so far as the Second Law is concerned, it is not clear what constitutes an "experiment" and what does not. I think that this does show that the Second Law requires interpretation in the sense relevant to this paper.

[20] The force of this remark does not depend on one's having something like Born's understanding of quantum mechanics. In order to reconcile the evolution of superposed states encoded in Schrödinger's equation with the extraordinarily precise measurements we make, say, of the positions of supercooled atoms, something has to give somewhere no matter how one tries to understand standard quantum mechanics on its own terms. (Bell 1987, *passim*, makes the same point.) Geroch (1984), for example, makes an interesting case against this view, but I find more compelling the arguments of Stein (1972) in general, and those of Stein (1984) against positions such as Geroch's in particular.

think physics to be only the definite prediction of readings for experimental instruments. As Wigner once remarked, quantum mechanics does not only predict scattering amplitudes ; it also allows us to calculate the specific heat of substances. Both classical and quantum theories share this capacity to illuminate features of intrinsic physical significance about the world, all the more remarkable in light of our lack of understanding of quantum mechanics.[21]

Quantum mechanics demands an interpretation because it is not clear how to model physical phenomena, how to model the outcomes of experiments *simpliciter* : the predictions of "pure" quantum theory are in some sense in contradiction with the outcomes of experiments, but not in such a way as to invalidate the theory but rather to substantiate it—an extraordinary state of affairs.[22] There is no analogous problem in general relativity. We know how to model in the terms of the theory experiments that manifest and probe every phenomena suggested or predicted by the theory, with no inconsistency of any kind, for we understand with no lack of cogency the fundamental, physical terms and principles of the theory in which one articulates its models and draws conclusions on their basis. In quantum mechanics, we do not even know what the fundamental terms and principles are—"measurement" ? "interaction" ? "observation" ?

Rovelli (2000) says,[23]

> In [the] effort [to find a theory of quantum gravity], physics is once more facing *conceptual* problems : *What is matter ? What is causality ? What is the role of the observer in physics ? What is time ? What is the meaning of "being somewhere" ? What is the meaning of "now" ? What is the meaning of "moving" ? Is motion to be defined with respect to objects or with respect to space ?*

I agree wholeheartedly with this assessment.[24] None of these problems, however, *is* a problem in the context of a general relativity. *All* of them *are* problems in quantum mechanics, and, indeed, lie at the bottom of the need of an interpretation for quantum mechanics. The quantum picture of the world is just too different from the classical, however, to make it anything more than merely possible—certainly not *prima facie* probable, and it seems to me, in the event, quite unlikely—that the resolution of subtle questions of interpretation in a classical theory would lead to resolution or even clarification of any of the conceptual problems of quantum mechanics. In the absence of empirical data tht shows us where general relativity goes wrong—for we have none—it makes sense to focus on the theory that we manifestly do not understand.

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[21] Perhaps this shows that the conceptual resources involved in a theory's prediction of the experimental values of dynamic quantitities such as scattering angles, or at least the ways that those resources come into play in that task, differ in a deep way from those in the computation of kinematic quantities such as specific heats ?

[22] ". . . mathematicians, who need only simple axioms about otherwise undefined objects, have been able to write extensive works on quantum measurement theory—which experimental physicists do not find it necessary to read." (Bell 1987b, p. 117)

[23] Italics are Rovelli's.

[24] As a personal prejudice, I would remove "causality" from the list of fundamental problems, as it does not seem to me a notion that one needs or even wants in physics, but that is beside the point here.

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