Studies in History and Philosophy of Modern Physics 47 (2014) 68–89

Contents lists available at ScienceDirect

Studies in History and Philosophy of Modern Physics

journal homepage: www.elsevier.com/locate/shpsb

Change in Hamiltonian general relativity from the lack of a time-like Killing vector field

J. Brian Pitts

Faculty of Philosophy, University of Cambridge, United Kingdom

ARTICLE INFO

Article history:
Received 26 October 2013
Received in revised form
23 May 2014
Accepted 24 May 2014
Available online 21 June 2014

Keywords:
Constrained Hamiltonian dynamics
General relativity
Problem of time
Quantum gravity
Variational principles

ABSTRACT

In General Relativity in Hamiltonian form, change has seemed to be missing, defined only asymptotically, or otherwise obscured at best, because the Hamiltonian is a sum of first-class constraints and a boundary term and thus supposedly generates gauge transformations. Attention to the gauge generator G of Rosenfeld, Anderson, Bergmann, Castellani et al., a specially *tuned sum* of first-class constraints, facilitates seeing that a solitary first-class constraint in fact generates not a gauge transformation, but a bad physical change in electromagnetism (changing the electric field) or General Relativity. The change spoils the Lagrangian constraints, Gauss's law or the Gauss–Codazzi relations describing embedding of space into space–time, in terms of the physically relevant velocities rather than auxiliary canonical momenta. While Maudlin and Healey have defended change in GR much as G. E. Moore resisted skepticism, there remains a need to exhibit the technical flaws in the no-change argument.

Insistence on Hamiltonian–Lagrangian equivalence, a theme emphasized by Mukunda, Castellani, Sugano, Pons, Salisbury, Shepley and Sundermeyer among others, holds the key. Taking objective change to be ineliminable time dependence, one recalls that there is change in vacuum GR just in case there is no time-like vector field ξ^α satisfying Killing's equation $\pounds_\xi g_{\mu\nu} = 0$, because then there exists no coordinate system such that everything is independent of time. Throwing away the spatial dependence of GR for convenience, one finds explicitly that the time evolution from Hamilton's equations is real change just when there is no time-like Killing vector. The inclusion of a massive scalar field is simple. No obstruction is expected in including spatial dependence and coupling more general matter fields. Hence change is real and local even in the Hamiltonian formalism.

The considerations here resolve the Earman–Maudlin standoff over change in Hamiltonian General Relativity: the Hamiltonian formalism is helpful, and, suitably reformed, it does not have absurd consequences for change. Hence the classical problem of time is resolved, apart from the issue of observables, for which the solution is outlined. The Lagrangian-equivalent Hamiltonian analysis of change in General Relativity is compared to Belot and Earman's treatment. The more serious quantum problem of time, however, is not automatically resolved due to issues of quantum constraint imposition.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Modern Physics*

1. Introduction

1.1. Hamiltonian change seems missing but Lagrangian change is not

It has been argued that General Relativity, at least in Hamiltonian form, lacks change, has change only asymptotically and hence only for certain topologies, or appears to lack change with no clear answer in sight (*e.g.*, Anderson, 1962a; Belot & Earman, 2001; Earman, 2002; Huggett, Vistarini, & Wüthrich, 2013; Isham, 1993; Rickles, 2006). Such a conclusion calls to mind earlier philosophical puzzles, whether ancient (the paradoxes of Zeno, whom James Anderson mentions Anderson, 1962a, 1962b, and the views of Parmenides, whom Kuchař mentions Kuchař, 1993) or modern (the argument concluding that real time requires something contradictory and hence is impossible by McTaggart, 1908, mentioned in a memorable philosophical exchange Earman, 2002; Maudlin, 2002). The new conclusion, following apparently with mathematical rigor from our standard theory of gravity, is not as readily ignored as Zeno, Parmenides and McTaggart. On the other hand, if one breathes the fresh, clean air of numerical relativity from time to time, it is difficult not to notice that there really is change in GR.

E-mail address: jbp25@cam.ac.uk

http://dx.doi.org/10.1016/j.shpsb.2014.05.007
1355-2198/

Thus one might suspect that any formal Hamiltonian results to the contrary are mistaken. Such a conclusion is all the clearer if one recalls that the Lagrangian GR formalism and 4-dimensional differential geometry are not thought to have any analogous problem. Either the canonical standards are inappropriately strict, or the 4-dimensional Lagrangian standards are too loose. But no one thinks the latter.

The Earman–Maudlin philosophical exchange provides a good starting point (Earman, 2002; Maudlin, 2002). Maudlin displays liberal amounts of common sense about point individuation, observables (in the non-technical sense of what can be observed), *etc.*, whereas Earman displays standard glosses on standard mathematical physics. Neither common sense rooted in scientific practice nor a common interpretation of mathematical physics is to be taken lightly, but can one have both? Is there a point in which mathematical physics becomes so bizarre as to undermine itself by excluding grounds for any possible empirical confirmation (Healey, 2002)? Earman has elsewhere composed an "Ode" commending the Dirac–Bergmann constrained dynamics formalism to philosophers (Earman, 2003). The reader of the Earman–Maudlin exchange gets the impression that each side declares victory. The progress of physics has been so great, and often enough counterintuitive, that beating back Poisson brackets with appeals to common sense does not yield full conviction, and rightly so. If Earman unwittingly exhibits "How to Abuse Gauge Freedom to Generate Metaphysical Monstrosities," as Maudlin's subtitle claims, then what is the right way to handle gauge freedom? On this question Maudlin is less full than one would prefer. Ultimately I will side with Maudlin's common-sense conclusions, though not his dismissive view of the Hamiltonian formalism. Change will be defended not in defiance of or indifference to mathematical physics, but through careful engagement in it and reform motivated by more solid mathematical physics—in line with Maudlin's invocation of the gold standard formulation of GR in terms of Einstein's equations and 4-dimensional differential geometry.

1.2. Maudlin's and Healey's critiques in G. E. Moore's style

One could affirm real change in GR without attending at all to arguments about the Hamiltonian formalism, because nothing about the Hamiltonian formalism's treatment of change could be more decisive than the meaning of the presence or absence of a time-like Killing vector. This claim bears a resemblance to the response to skepticism by Moore (1939), as well as the spirit of Maudlin's and Healey's responses to Earman (Healey, 2002; Maudlin, 2002). But the Moorean-like fact, in my view, is not (or not only) some deliverance of common sense, accessible by simple bodily gestures (Moore's displaying his hands, Samuel Johnson's kicking a stone), but rather (or also), a deliverance of Lagrangian field theory. Yet this is no justification for dismissing the Hamiltonian formalism. It is, rather, a call for reform.

The necessity and incompleteness of such an approach resembles Norman Malcolm's discussion of Moore's philosophy[1]:

> Two things may be said against Moore's method of refutation. (Footnote: This must be taken as qualifying my previous statement that Moore's refutations are *good* ones.) In the first place, it often fails to convince the author of the paradox that he is wrong.… In the second place, Moore's style of refutation does not get at the sources of the philosophical troubles which produce the paradoxes.… Although Moore's philosophical method is an incomplete method, it is the essential first step in a complete method. The way to treat a philosophical paradox is first of all to resist it, to prove it false. Because, if the philosopher is pleased with his paradox, fancies it to be true, then you can do nothing with him. It is only when he is dissatisfied with his paradox, feels refuted, that it is possible to clear up for him the philosophical problem of which his paradox is a manifestation. (Malcolm, 1968, pp. 366–367).

If Malcolm's praise seems too strong (because Moorean antiskeptical arguments are not always necessary and not always good), the point remains that Moore-style arguments such as Maudlin's and Healey's are sometimes good, incomplete, and yet inspirational unraveling a flawed skeptical argument, as is the case here. The considerations presented above largely fill the gap left by the Moorean style of defense of change in GR.

2. Lagrangian interpretive strategy brings clarity

Attending to the Lagrangian formalism of General Relativity and to 4-dimensional differential geometry holds the key to clarity in all these matters. It seems to be widely agreed on diverse grounds that the Lagrangian formulation of mechanics (broadly construed) is more fundamental than the Hamiltonian one (Curiel, 2014; Gotay & Nester, 1979).[2] It is also widely believed that the two are equivalent (apart perhaps from topological restrictions), or at least that they should be. Yet there are controversies in the literature on constrained dynamics about whether such equivalence actually holds, and various proofs presented have too narrow a scope (such as addressing the equivalence of equations of motion but neglecting to address the equivalence of the gauge transformations). One possible view is described by Pons, Salisbury, and Sundermeyer (2010):

> [t]he position on one side is that there ought to be no debate at all [about the physical interpretation of General Relativity or any generally covariant theory] because the phase space formalism is equivalent to the formalism in configuration-velocity space, and no one has claimed that any interpretational problem exists in the latter framework. Entire books have been devoted to the experimental tests of GR, and this very language implies that observables exist - alive and kicking. Thus the entire debate must be a consequence of misunderstandings. (Pons et al., 2010, p. 3).

Such a view suggests a Lagrangian-first interpretive strategy.

This view does not seem to be the view of Kuchař, though he, very unusually, is willing to tinker with the Dirac–Bergmann formalism to uncover real change in General Relativity (Kuchař, 1993). Kuchař's reinterpretation of the Hamiltonian constraint is not systematic—the common-sense arguments about observing temporal change work equally well for the momentum constraint and spatial change. Neither is Kuchař's view clearly inspired by the need for equivalence with the Lagrangian formalism. He allows that observables should commute with the momentum constraint $\mathcal{H}_i$, because we cannot directly observe spatial points. But he denies that observables should commute with the Hamiltonian constraint $\mathcal{H}_0$. He notes that one cannot directly observe which

[1] I owe this reference to Jim Weatherall.

[2] There is, to be sure, a Hamiltonian derivation of geometrodynamics (Hojman, Kuchař, & Teitelboim, 1976). Whether one can seriously imagine someone first finding GR by that means is another matter. One risk of a freestanding Hamiltonian view is the temptation (resisted by these authors but not others) to forget that one only learns what the canonical momenta mean physically by virtue of the equations $\dot{q} = \delta H/\delta p$. By contrast the Lagrangian lacks those *a priori* physically meaningless dynamical quantities. That is one clear respect in which the Lagrangian formalism is more fundamental than the Hamiltonian. For such reasons, it is best to direct one's thoughts to the Hamiltonian action $\int dt(p\dot{q} - H)$ rather than the Hamiltonian itself.