On Philosophy of Quantum Gravity

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Chicago, 29 Sept 2013

Beyond spacetime—and the Bronstein cube

The reconciliation of the quantum with spacetime ideas, especially relativity, does not necessarily mean:
the unification of forces, or even quantum gravity.

Thus the Bronstein cube, though alluring, is too neat.

The limits pictured as edges of the cube:
i) are slippery, even if defined;
ii) are probably not unique;
iii) probably don't commute (except the 4 edges of the classical face);
iv) and anyway, might not be what we want—and not just because quantization is a suspect procedure.

For consider three broad approaches to quantum gravity (or indeed: to classical gravity, ie GR, being emergent):
a) strings; (Harvey, Huggett, Teh, Knox, Vistarini);
b) LQG and its ilk ('fundamentally discrete'); (Fletcher, Oriti, Dittrich, Wuthrich, Vidotto, Schroeren),
c) condensed matter approaches; (Mattingly, Bain).

Thus string theory should not be thought of as saying:
starting from quantum Newton-Cartan theory: let c be finite;
starting from relativistic quantum field theory: let G be non-zero;
starting from GR: let h be non-zero; (that would characterize traditional quantization of GR programs)

It is equally clear that condensed matter approaches should not be thought of saying these things.

Similarly for LQG and its ilk, with the mild caveat that one could cast some of the programmes (but not e.g. causal sets!) as traditional quantizations of GR, i.e. as saying: starting from GR, let h be non-zero.

To sum up: the search for QG is like orienteering in a blizzard—without a map.

Halfway through the woods

Rovelli's metaphor from Dante.

In the reconciliation of relativity and the quantum, it might not be relativity that makes the most compromises. Maybe quantum theory has to mend its ways.

Recall the scandal of the measurement problem (Pashby, Sudarsky); and our lacking a relativistic theory of measurement.

Indeed: What is the physical meaning of a quantum superposition, i.e. complex amplitudes for different configurations?
Just because this question is familiar does not mean we can be blasé about it (except perhaps on a pilot-wave view).

All the more so when the configurations are something that seems more fixed and-or background and-or abstract than positions of bodies.
Namely: geometries or topologies of space; more precisely, for LQG, assignments of values to discrete area and volume operators.

Though Rovelli is right, we should on a more detailed time-scale notice:
a) the 1965-75 explosion in physics; (and in philosophy);
b) the evolution of the philosophy of physics since 1970: it is now in seamless contact with foundations of physics; (Pitts).

This has been a great stimulus to natural philosophy. Examples:
i) bearing of quantum non-locality on philosophical concepts of cause and probability
ii) bearing of quantum indistinguishability on philosophical concepts of identity and individuation.

What use philosophy?

But what are the prospects for philosophers having synergy with fundamental physics, today and tomorrow ?

What could be the role of philosophy, apart from
a) scavenger picking over dead theories for its own purposes; or
b) Greek chorus, camp follower; or at best
c) minor symbiote, like pilot fish for a great white shark?

For philosophers are few, and their conceptual armoury is small and out of date.
(I set aside scholarly, e.g. historical, study: though invaluable, it does not make for synergy with practitioners of fundamental physics.)

Agreed: foundational issues are relevant in today's heuristics for finding tomorrow's fundamental physics.

Specific versions of this, as seen in recent philosophical discussion of historical cases:
a) Belot 1998 on the AB effect: interpreting two false theories where they conflict, and trying to reconcile them, can be a guide to the world, and so of heuristic value for tomorrow's physics ;
b) Weatherall 2013 on the explanation by general relativity of the equality in Newtonian physics of inertial and gravitational mass.

But surely those heuristics are usually best undertaken by a physicist—such as Aharonov or Einstein.

Projects for the young?

The hope for low-lying fruit: i.e. which even a dumb philosopher could spot.

There are famous previous examples of this—agreed, only the third was done by a philosopher.
a) Bell 1964: the locality condition arises in philosophy; cf. Reichenbach.
b) Barbour et al on the Machian foundations of dynamics.
c) Manchak 2009: under-determination in cosmology.

But being unable to see any fruit: here are four . . .

Personal reflections

1. **Beware of beguiling words**

Abstract nouns are usually not univocal. So beware of slogans like: there is/is not time/change.

Conceptual analysis has its place. But recall how much ink was spilt rejecting special relativity's frame-dependence of simultaneity . . . we must be vigilant to avoid such howlers.

But (Knox): we should be sceptical about metaphors masquerading as theoretical philosophical concepts (‘real physical space’, ‘the container’, ‘grounding’).

2. **The vacuum vs. zilch**

'Zilch' is a convenient expression, defined to be necessarily non-referring (Oliver and Smiley *Analysis* 20143).

Beware of mystery-mongering by not admitting that the vacuum is, not the absence of the system, but the system's ground-state.

Similarly in LQG: $|Vac\rangle_{\rm kin}$ is not the absence of the system, but a state, albeit a very bare one. (Compare the empty set in classical causal sets.)

3) **Condensed matter approaches**.
Recall that on these approaches, the traditional idea of quantizing the metric field in e.g. GR, as a way of guessing what is a quantum theory of gravity, looks very misguided.

For it would correspond, in the context of condensed matter, to trying to guess the microscopic quantum theory of the system by quantization of the classical effective equations, which will of course in general be very dependent on a regime of parameter values and maybe on specific states.

This returns us to the general and troubling question: what if anything do we have as a good guide to the fundamental degrees of freedom of any putative quantum theory of gravity?

We can hardly expect these degrees of freedom to oblige us by obeying equations we happen to know from e.g. the microscopic theory of liquids; even if so far, talents such as Volovik and his ilk have found suggestive cases.

It is also striking (fortuitous: perhaps so fortuitous as to be ironic or even embarrassing?) that some of the other programmes which seem to be fundamentally discrete, and so to have some prospect of using a principled starting point for deriving a classical spacetime, turn out to also use some basic ingredients derived from general relativity.

Viz. (i) LQG, which started as a canonical quantization of GR using Ashtekar variables; and
(ii) Quantum Regge calculus and (causal) dynamical triangulations.

Should we take this to be suspicious; and so to cast in a better light the programmes whose dynamical ingredients are apparently not derived from general relativity?

Namely: string theory, causal sets and versions of LQG which are 'GR-free' ? . . . They make strange bed-fellows!

4) **Duality**:

Philosophers and physicists tend to differ in their reactions to a pair of dual theories.

Philosophers allow that there are two theories.
This amounts to: two alternative ways the world could be; or in some cases, in a single possible world, there are two 'regimes/sectors of reality' each described by one alternative.
In short, the theories are different and either of them could be true.
But they would be true in different worlds or in different regimes in a given world.

Physicists tend to deny that there are two theories. They construe them as notational variants, describing a single possible world (or single regime within a world).

Why the difference? Is it just that philosophers have come to reject verificationism, while it lingers among physicists? Maybe. But also:

Philosophers emphasize interpretation, irrespective of heuristics. So, unmoved by verificationism, and used to sceptical scenarios:–
they tend to distinguish possibilities.

But physicists recall several historical cases in which such a pair of apparently distinct theories was a prelude to an advance, i.e. to a formulation of a new theory that elided the apparent difference, and was agreed to be an improvement.

E.g. the formulation of Galilean spacetime, and its elision of different identifications of absolute rest.
E.g. the formulation of generally covariant GR, and its elision of different identifications of which spacetime point is which (the hole argument).

So I think physicists' rationale for denying that there are two theories is, in part, that in previous cases, eliding the apparent difference was agreed to be an improvement.

In short: 'Replicas today suggest Ockham tomorrow.'

This is compatible with Knox's third way to break the tie between the dual theories.
Namely: one is closer than the other to a third theory which is agreed to be more fundamental by:
(i) implying both and-or (ii) having a larger domain then both.