23 open problems in the philosophy of physics

James Read*

On 10th January 2025, I had the privilege of speaking at the launch event of the Radboud Centre for Natural Philosophy (RCNP) at Radboud University, Nijmegen, NL.[1] The prospect was a little intimidating, not only because of the (predictably) illustrious audience, but also (more surprisingly) because my talk was scheduled immediately after a performance composed specifically for the launch event.[2] That performance, titled 'Big Bang variations', wove together a story of the birth of a human being with the birth of the Universe itself, i.e. the Big Bang—and featured avant-garde guitar; it was fabulous. Sadly, there was no such guitar accompaniment to my talk.[3] But still, I think the talk was tolerably successful; this is a paper version of it.[4]

My talk was part of a session titled 'Into the future'. The other speakers (Flavio del Santo, Bryan Roberts, and Sylvia Wenmackers) and I were given the brief to reflect on the future of our field in one way or another. Mulling over what to speak about in advance of the talk, I decided that it would be interesting and fun to identify a set of open problems which, in my view, are worthy of further attention going forward. But when one thinks of 'Open problems in field X', one of course cannot but think of David Hilbert's famous list of open problems in mathematics, presented in Paris in August 1900.[5] Wouldn't it be nice to give an analogous list for modern philosophy of physics, paying thereby some kind of loose homage to the great man?

I'll reproduce the list below, and also justify (if only briefly) why each item is on the list.[6] But before I get there—just as in the talk—I want to begin with some sociological framing. If we're thinking about the future of the philosophy

*Faculty of Philosophy, University of Oxford, UK. james.read@philosophy.ox.ac.uk

[1]The website for the Centre is here: `https://rcnp.science.ru.nl/`. I reccomend it to all as a wonderful place to visit and study.

[2]To be precise, as I understand it, the performance, was (i) adapted from Dutch to English, and (ii) partly modified, for the launch event.

[3]Although I encourage you to read some while reading this note! It will surely heighten the experience.

[4]While I have given a few references in this note, they're far from exhaustive. I hope the reader will forgive me for this.

[5]I'm aware that, in his talk, Hilbert presented only 10 problems; the full 23 followed in a later publication (see e.g. Grattan-Guinness (2000) for the history). But 10 was definitely not enough for me—and since neither 10 nor 23 is a multiple of 3 (the 'trinitarian' symbol of the RCNP—see again their website), I didn't think anyone would object to my plumping for the larger number.

[6]As I stressed in the talk, I take the list to be provisional, so if you (reader) identify some omission, or deem some entry unworthy of being on the list, please do contact me!

of physics, it would be remiss not to mention some obvious things—both good and bad—about where we currently find ourselves.

Here is the bad news: I worry about the long-term prospects for our discipline, given our current climate of, *inter alia*, humanities budget cuts, financial security of universities (e.g., 72% of UK universities forecast to be running deficits by 2025–26[7]), political pressure on universities in the USA given the incoming administration, etc. I am not sure how to mitigate the issues, but we should all be thinking about them. But on the other hand, there is also obvious good news, e.g.: for the time being at least, the philosophy of physics community has (I think) been increasing in size monotonically. Moreover, there is increased diversity (although, of course, there is much more work to be done on this front), increased technical skill, and increased dialogue between philosophy on the one hand and physics, mathematics, and history on the other. All of this should be celebrated.

But enough of the sociological frame; let me turn now to the discipline itself. Before we get to the list of 23, it'll be worth reminding ourselves of what we have achieved collectively as a field over the past few decades. To make that manageable, let me sub-divide our field into the following three sub-disciplines:[8]

A. Philosophy of quantum mechanics (QM).

B. Philosophy of spacetime and symmetries.

C. Philosophy of thermodynamics and statistical mechanics.

What have we achieved in each of these areas? As it turns out—a lot! So time for a collective pat on the back:

- *Ad* (A): major interpretations (at least of non-relativistic QM—henceforth NRQM) are by now well developed (GRW/Bohm/Everett); locality and separability are well understood; no-go theorems (e.g. Bell/BKS/PBR) are well understood; and various alternative approaches (e.g., pragmatism, QBism, relational QM) have been proposed and are still being explored. Perhaps most importantly: the foundations of QM is now taken seriously by many physicists!

- *Ad* (B): The substantivalism/relationalism debate is substantially clarified (including the scope of its applicability); the dynamical/geometrical debate (i.e., whether and when geometry has explanatory priority over material dynamics—the point of inflection for this debate is of course Brown

[7]See https://www.bbc.co.uk/news/articles/c14lv7e61d3o.

[8]Doing so is pretty ubiquitous. Here's one justification for doing so: David Wallace divides up his philosophy of physics reading lists in this way—see https://sites.pitt.edu/~dmw121/resources.html. Note that philosophy of quantum field theory and quantum gravity are conspicuous absences here, although perhaps they could be folded into (1). In any case, I'll come back to them. Note also that more applied physics, e.g. condensed matter or biophysics, is likewise conspicuously absent. I'll come back to that too.

(2005)) is also now substantially clarified and substantially well understood (including scope of its applicability); different spacetime settings for different theories (e.g. GR—but especially Newtonian gravitation) are also well understood; definitions and interpretational significance of symmetries are well understood (many more distinctions are in the offing than were, say, a decade ago, and the role of subsystem symmetries is much better appreciated, etc.); and notions of theoretical equivalence are now well understood.

- *Ad* (C): The source of time asymmetry in thermodynamics is well understood;[9] emergence and reduction are much better understood than they once were; the role and statement of the past hypothesis has been substantially clarified; relations between (neo-)Boltzmannian and Gibbsian approaches to statistical mechanics have been clarified; and we now have technical control over ergodicity, recurrence, and other such notions.

This is all great news, of course. But it raises the question: *where do we go from here?* I'll come back to generalities later—for now, I want to jump into the 23 *specific* open problems in the philosophy of physics upon which I've settled.[10] But just one final thing before I get there, by way of an historical aside.

Hilbert himself spoke in the Sorbonne on the morning of 8$^{\text{th}}$ August 1900. To quote the report on the Congress prepared by Charlotte Angas Scott for the *Bulletin of the American Mathematical Society*, the reaction to Hilbert's talk was "a rather desultory discussion". And as Grattan-Guinness (2000, p. 756) remarked amusingly in a centenary retrospective on the talk: "Maybe Hilbert's manner of delivery was partly to blame: Scott opined that the "presentation of papers is usually shockingly bad," with monotonic utterance exuding boredom; she gave no names, but hinted that eminent ones were not excluded." So—the bar to clear, both for my audience (i.e., not being "rather desultory") and myself (i.e., not "exuding boredom") was rather low. Thankfully, I think we managed to clear it with a sizeable margin!

So now to the list. I'll follow first the classification (A)–(C) above, before turning to questions in the philosophy of quantum field theory (QFT) and quantum gravity, as well as a few more general issues which make the list. To begin, then, here are three open problems in the foundations of quantum mechanics:

1. Develop versions of Bohmian and dynamical collapse theories for relativistic QFT.

2. Develop actual physical models to underwrite theses which deny measurement independence (e.g., superdeterminism).

[9] Here of course I have in mind work on the 'minus first law'—see Brown and Uffink (2001).

[10] To repeat what I said earlier: I welcome disagreement, views about other items which should be on this list, etc.

3. ‘Timpson conjectures’, e.g.: No theory can satisfy: (i) measurement independence, (ii) dynamical locality, (iii) suitable scientific realism, (iv) Bell inequality violation, (v) non-separability, and (vi) unique measurement outcomes (i.e., one-world).

On (1): Wallace (2023) has recently raised a provocative challenge to both Bohmians and dynamical collapse theorists: that there’s not even a genuine case of theoretical underdetermination between their theories and Everett, because neither of those approaches are viable in the context of relativistic QFT. I expect that Wallace will maintain that this challenge cannot be met—but my point is that Bohmians and dynamical collapse theorists must rise to it, if their approaches are to be taken seriously as viable candidates for describing the actual world.

On (2): there is increased recent talk—and, indeed, endorsement—of approaches which deny measurement independence (which, recall, states that the choice of a and b to measure in EPR-type experiments is independent of the λ in the intersection of the past light cones; I won’t explain this further here), perhaps most notably superdeterminism. And yet, setting aside a few toy models, there is no compelling physical theory in the offing which has such a feature; as in the previous case, if such an approach is to be taken seriously, it is incumbent upon us to develop one.[11]

On (3): I owe this idea to my colleague Chris Timpson—the thought is the following: it would be nice to have greater theoretical control over the exact circumstances under which one will of necessity have a many-worlds quantum theory; to this end, one might seek to prove what I dub ‘Timpson conjectures’, such as that presented above, which claims that a many-worlds theory will be inevitable given the stated conditions (i)–(v).

More on quantum mechanics in a minute, when I turn to QFT proper. But next, I’ll move to the philosophy of spacetime and symmetries. Here are these items:

4. Develop a clear understanding of the technical conditions under which material fields come to be ‘adapted’ to a given spacetime geometry.

5. Explore thoroughly the sense in which general relativity is special in the ‘space of spacetime theories’. (This includes systematic modifications of general relativistic structures, e.g., projective and conformal, or smoothness or Hausdorff assumptions, etc.)

6. Understand the physics and metaphysics of singularities and the prospects for singularity resolution in quantum gravity.

7. Understand better the black hole information loss paradox(!).

[11] There is a nice online exchange between Chris Palmer and Chris Timpson, in which the latter raises this challenge for superdeterminism. See https://www.youtube.com/watch?v=y_GtgyGjzPU.

8. Develop a clear understanding of the physics and metaphysics of gauge theories (including gauge theories of gravity and non-abelian Yang-Mills theories).

9. Assess the extent to which cosmological modelling (of dark matter, dark energy, or inflation) must always be underdetermined by evidence.

10. Secure a systematic ontology of waves (including understanding propagation speed, signalling, dissipation, dispersion, etc.)

11. Provide a perspicuous metaphysics for spinor fields.

12. Get a good handle on the conditions under which our best theories of physics are deterministic. (For example in the context of general relativity in the case of cosmic censorship, regularity assumptions, and causal pathologies; as well as in other field theories, etc.)

13. Assess the foundational significance of the fact that the solution spaces of field theories used in physics are already well known not to be 'flat' (in the words of Halvorson (2019)), but richly structured, as e.g. complex manifolds, algebraic varieties, etc.

I take item (4) to be one natural successor of discussions of the dynamical/geometrical debate which arose in the wake of the work of Brown (2005). In those discussions, there was much mention of the conditions under which material fields (either in general relativity or in whatever other spacetime theory) come to be 'adapted to' (i.e., read off/survey/etc.) a given piece of geometric structure. But, as stressed by Weatherall (2020), it would be nice to be able to prove results regarding the conditions for this adaptation, rather than merely to sweep those conditions under the carpet. One can approach this in many ways—either by thinking in terms of the conditions under which one could construct rods and clocks to achieve this task, or the conditions under which small bodies will 'track' geodesics of the given geometrical structure, or the conditions under which one could use (say) light rays and freely-falling particles probe that structure, etc. But in any case, one would like control over exactly when material fields will so 'advert' to geometry, and when they won't.

On (5): this takes up an injunction from Lehmkuhl et al. (2017) to explore the 'space of spacetime theories'—the thought being that we can come to understand better general relativity by investigating spacetime theories in its vicinity, both conceptually and mathematically. On the conceptual side, one can ask questions such as: 'Which theories manifest something like an equivalence principle, and why is that the case?' And on the mathematical side, well: in my view, as philosophers we remain a little too inclined to think of spacetimes merely as pairs $\langle M, g\rangle$, without appreciating that there is a great deal of structure contained in both of those objects. For example, one can tune the projective and conformal structures associated with g. And one can modify topological assumptions too, e.g. Hausdorff.[12] But there remains much more

[12] On which see Luc (2020).

to explore—e.g., smoothness assumptions, the possibility of exotic differential structures, etc.

Item (6) invites us to explore singularities in both the classical and quantum context. In the former, one can ask e.g.: are singularities objects, or properties of spacetime regions?[13] And investigation of singularities in the quantum context is relevant for *inter alia*, item (7)! I.e., the infamous black hole information loss paradox. In a wonderful talk the day before my own, Erik Curiel pointed out that (a) the 'information loss paradox' is in fact a cluster of (at least) six related issues, and (b) hand-wringing about the paradox may well in fact be premature, in the sense that much of it could be an artefact of the use of semi-classical gravity. If singularities are resolved in quantum gravity, this would corroborate these latter thoughts. But in any case—and as Curiel himself stressed—none of this counts as a reason not to study the information loss paradox, for the various proposals which have been made by both physicists and philosophers in seeking to deal with it are often highly illuminating, insightful, and quite beautiful in their own rights. Hence, the information loss paradox is still well-deserving of a place on this list.

On (8): here I have in mind wonderful recent work by e.g. Jacobs (2023) and Weatherall (2016), which ask questions such as: How should we think about the structure of gauge theories? About which part of a gauge theory should I be a realist (perhaps even a 'fibre bundle substantivalist')? Are there alternative ways of formulating gauge theories?[14] The second part of this problem invites us to explore the physics and metaphysics of non-abelian Yang–Mills theories—something on which precious (I would say: shockingly) little has yet been written by philosophers, given its supreme importance in modern theoretical physics. (What is the nature of energy conservation in non-abelian theories, for example? It is a tricky business.)

On (9): there has of course been much excellent work already undertaken by philosophers on cosmological underdetermination. However, there remains much to be done, in particular in connection with very recent work being undertaken by practising cosmologists. What I have in mind are issues such as this: recent work such as Wolf and Ferreira (2023) suggests that our choice of microphysical model of dark energy might well be forever underdetermined by data—and one can, in fact, make the same claims with respect to dark matter and inflationary models. This presents a serious challenge to scientific realism in the context of cosmology—and casts doubt upon the sense in continued model-building.

Item (10) might seem to come out of nowhere. But such an impression arises only because it has been sorely neglected by philosophers up to this point. If one peruses the literature from both physics and engineering, one will find vast divergences of a plethora of questions, including but not limited to: (i) what is a wave? (ii) what is a wave equation? (iii) what is a sensible notion of wave velocity? (iv) what does it mean for a wave to propagate a signal? (v) how to understand effects such as dissipation, dispersion, etc.? So: this item is an

[13] Cudek (2024) takes up this question with great clarity and insight.

[14] See also on this Gomes (2024).

invitation to an ontology of waves.

On (11): as we know, all matter in the universe (i.e, fermionic matter, i.e. not gauge fields) is represented mathematically by spinor fields. And yet, we know that spinors as not geometric objects (see Pitts (2012)), and so do not admit of any straightforward coordinate-invariant representation. So, we lack a perspicuous metaphysics for spinors.[15] Given the profound importance of spinors to modern physics, the fact that the number of philosophy papers written on them could be counted (comfortably) on one hand is nothing short of scandalous.

On (12): of course, whole books have been written on determinism in physics—no less than the magisterial (Earman 1986). And yet, there remains a vast amount to explore, in a vast variety of physical contexts. And so, the longevity of this topic does not preclude its making the list.

On (13): this is a point which has been made by philosophers of physics such as Halvorson (2019) and Fletcher (2016), who respectively have explored the merits of understanding theories as categories, or of imposing topologies on the solution spaces of such theories. A similar point arises, however, in high energy physics, where solution spaces are often taken to be (say) complex manifolds, or algebraic varieties (as in the case on $\mathcal{N} = 2$ supersymmetric Yang–Mills theories), etc. The significance of this rich structure warrants attention from philosophers.

So much for spacetime and symmetries; now to thermodynamics and statistical mechanics. Here, unfortunately, I have only two; I confess this is likely to be my biggest blind spot in assembling this list. (But even Hilbert had blind spots! He confined physics to one item (his (6): "Mathematical treatment of the axioms of physics"), and passed over the achievements of the Italian geometers of the time—see Grattan-Guinness (2000, p. 753).) To repeat myself again, I welcome suggestions for others. But for the time being, here they are:

14. Understand equilibration in quantum statistical mechanics.

15. Obtain a clear understanding of the geometrical foundations of thermodynamics, including fluctuation thermodynamics.

On (14): the issues tied up in equilibration in quantum statistical mechanics are highly knotted, and there remains much to be done. And yet, evidently, equilibrations is one of the *core* issues to understand in the philosophy of statistical mechanics. And so, this item makes the list.

On (15): philosophers are of course aware that thermodynamics can be understood in terms of (say) contact geometries.[16] But this has not yet been studied with anything like the attention it deserves; moreover, there are other approaches to (say) fluctuation thermodynamics (various Riemannian geometries for example) of which (my impression is that) philosophers are largely

[15] Of course, we have spinor bundles. But these are neither natural nor gauge natural—see Fatibene and Francaviglia (2003)—so I stick by the unperspicuity claim.

[16] The expert on this is very likely Bryan Roberts, but as far as I'm aware nothing on the topic has yet appeared in print.

ignorant: the intrinsic structure of such geometries warrants attention, as do the ways in which they relate to one another, and (perhaps even more importantly) the light that they can shed (if any) on the structure of thermodynamics itself.

This completes sub-disciplines (A)–(C). Now to QFT and quantum gravity; I'll take them together:

16. Obtain a clear mathematical and conceptual understanding of both decoherence and the measurement process in QFT.

17. Provide a suitable metaphysics of (classical and quantum) solitons and other non-perturbative effects.

18. Develop a clear understanding of symmetry-related issues specific to QFT (e.g., anomalies).

19. Provide a systematic study of methods of quantisation (inc. modern methods, e.g. BV) and their applications.

20. Obtain a clear mathematical and conceptual understanding of the emergence of spacetime in the various mainstream approaches to quantum gravity.

On (16): decoherence is of course extremely well-studied and understood by philosophers in the context of NRQM, and we have excellent textbooks such as that of Schlosshauer (2007). However, there is a dearth of foundational literature on decoherence in QFT. This is surprising, for can one really say that the measurement is solved (whether by Everett or in some other way—decoherence is very important for Bohmians too, of course) if one lacks a clear understanding of how it works in the context of realistic quantum field theoretic physics? Even in the past couple of years, there has been much excellent work in the physics literature on this topic; in my opinion, philosophers must engage with it too.

On (17): to my knowledge, philosophers have written almost nothing on non-perturbative effects (perhaps one exception is in the context of dualities, on which see the work of de Haro and Butterfield (2025)). But these effects are both (i) of profound importance in theoretical physics, and (ii) such that many puzzling claims are made about them (e.g., both QFT field excitations and quantum solitons are described as 'particles'—can this really be so?). Very similar points carry across to item (18)—e.g., anomalies are known to be very rich geometrically (being associated with topological obstructions to the construction of certain bundles), and yet (to my knowledge) only one philosophy paper has been written on them.[17]

More has been written, of course, on (19), but there is still much more to be done. For example, physicists' preferred (and by wide consensus the most powerful) approach to quantisation is BV–BRST. And yet, few philosophers seem to have looked at it.

[17] I.e., Fine and Fine (1997).

Item (20) is the only quantum gravity-related item on my list, but it is of course of the utmost importance that any candidate quantum theory of gravity be able to recover, in way which is at least reasonably controlled, the structure of classical spacetime in the appropriate limit. But (to my mind, at least), string theory discussions of 'coherent states' are highly inadequate in this regard (surely, in the end, we will need to avail ourselves of something like decoherence here); moreover, deriving the Einstein equations from (say) loop quantum gravity has well-known and longstanding difficulties.[18]

My final category has to do with more general issues. There are three items:

21. Fully explore the scope of EFT-like and scale-relative reasoning in physics.

22. Explore thoroughly the philosophical/foundational significance of the 'open systems view'.

23. Appraise the extent to which alternative approaches to scientific realism, e.g. perspectival/relational/fragmentalism, or informed by e.g. pragmatism, can shed light upon the foundations of physical theories.

On (21): EFT-like reasoning—by which I mean, roughly, considering of ratios of scales of physical quantities; neglecting terms when they are negligible in order to witness 'emergent' physics; etc.—is completely ubiquitous in practice.[19] But philosophers have only recently begun to think in this way; the approach (and its scope), evidently, warrants significantly greater further attention.

Item (22) has to do with a very recent (and to my mind very reasonable) proposal from Cuffaro and Hartmann (2024): that we take open systems (i.e., systems interacting to some degree with their environments) to be the fundamental objects of study in the foundations of physics. This is of course reasonable, in the sense that *in practice* no system is truly isolated from its environment. And yet, the consensus in the philosophy of physics up to this point has been to study closed systems (i.e., systems totally isolated from their environment—effectively, systems regarded as 'cosmologies'). Since this proposal is very recent, the fruits of thinking in an 'open systems' way are yet to be properly understood—but that, of course, is exactly why this item makes the list.

The final item, (23), has to do with alternative approaches to scientific realism which have become increasingly pervasive in many different areas of philosophical enquiry into physics, from quantum mechanics to relativity theory. But what really are the prospects for these ways of thinking? Are they tenable—and can they in fact shed substantial light upon the subject matter? That remains to be seen.

So much, then, for the list. What next? The first thing to address is the topic of omissions. There are a couple of such over which I do feel some element

[18] Philosophers are fond of talking about 'spacetime functionalism' in this context. I don't object to this, but at some point we will need to do the technical work too.

[19] One clear illustrative example of this is the case of geometric optics in general relativity: when the wavelength of light is much less than the ambient curvature scale, the wave effectively propagates in flat spacetime.

of guilt. The first is representation in physics—what does it take for mathematical structure to represent a (subsystem of a) world? This question is of course extremely important, and much good work on it has been undertaken in the context of physics in recent years. But ultimately, I viewed the issue as being rather close to the philosophy of science, so I did not include it. The second (more egregious) omission has to do with more applied areas of physics—condensed matter physics or biophysics, for example. I am completely confident that there is a wealth of conceptual problems to get one's teeth into in these areas. And yet, we have engaged with them so little that to even isolate specific problems is a challenge. It's regrettable that much of my list has to do with 'traditional' theoretical physics—that, too, will in the end have to be addressed.[20]

Some other omissions I feel less guilty about. Namely, old chestnuts such as:

- The past hypothesis :'(
- The hole argument :'(
- The 'usual suspects' in NRQM :'(

I mean, sorry guys, but you are basically old news now. More seriously: it's not that I don't think that doing research on these topics continues to have philosophical payoffs—in fact, I'm continually astounded by the insights that explorations into the hole argument continue to offer, even nearly four decades after the seminal paper of Earman and Norton (1987). Rather, my point is just this: with so much other beautiful and under-explored other physics out there (as exemplified in many of my 23), why need we always return to these same issues time and again?[21]

The next point which I want to make has to do with the rank-order importance of the problems. So far, I have taken an egalitarian attitude. In reality, however, I do regard some as being more pressing than others. For me, the three problems by which I am most moved (at least for now—and this is very likely mostly a matter of personal taste) are (16) (for have we really solved the measurement problem if this is not addressed?), (11) (for have we really understood the metaphysics of all matter if this is not addressed?), and (8) (for Yang–Mills theory is in many respects *the* bread-and-butter of modern theoretical physics, so that we have not directed more attention to it is deeply problematic).

Now to general themes. I didn't consider these when writing the list, but here are two obvious ones which emerge from it:

- The open problems identified are mostly pretty technical.

[20] One final omission is this: the night before I gave my talk, Erik Curiel rushed past me saying that he had thought of another problem. But he did not tell me what it was (I was on a call at the time), and I didn't speak to him again. So I remain ignorant—but tantalised.

[21] Another point to be clear on is that I don't think that the above don't have pedagogical value. In fact, grappling with these topics and the issues which they raise is essential training for a philosopher of physics, and invariably these are some of the first topics which I will set to incoming students on our M.St. in Philosophy of Physics at Oxford. I'm grateful to Frank Cudek for discussion on this point.

- Arguably, most of these open problems are pretty closely connected with actual physics practice.

So I would say the following. (a): Philosophers of physics were able to do very good foundational work in (say) the 1990s and 2000s without necessarily having vast technical facility (although obviously in many cases they did anyway). To some extent, I see that era as coming to an end. (b): Dialogue with actual physics is crucial (I'll come back to this). And (c): from a more practical point of view, we need to think carefully about how to convince our fellow philosophers that those working on such issues are doing *bona fide* important philosophical work—how to convince a panel consisting to those working in logic, metaphysics, etc., to hire someone working on (e.g.) decoherence in QFT?

The final item which I presented in my talk—and which I'll present here too—regarded things which we should strive to avoid as our discipline proceeds into the future. I identified four 'spectres', which are the following:

(i) Charlatanry/dilettantism.

(ii) Hyper-specialisation.

(iii) 'De-occamising' physics.

(iv) Normal science.

As before, I'll take these in turn. I won't dwell on (i), since it's obvious.[22] Suffice it to say that we should always hold ourselves to the highest standards: get people right, not be lazy, not do bad history, etc.

Turning to (ii), i.e. to the issue of 'hyper-specialisation': my point is just that physics is a deeply interconnected thing, and to some extent having good grounding in all areas is (I'd argue) a necessary condition for being a good physicist. And yet, many of us focus on one particular area of physics, at the expense of the rest. This seems at the least regrettable.[23]

Point (iii) is what I call 'de-occamising physics'. I borrow the term 'de-occamisation' from Pitts (2022): the idea is that we can always introduce spurious complexity, and extra symmetries, into our theory. Consider e.g. some theory of a field φ, with dynamical equations $\Delta[\varphi] = 0$. Define $\varphi =: \psi + \chi$. Then there is a new symmetry, $\psi \mapsto \psi + \epsilon$, $\chi \mapsto \chi - \epsilon$. But this is all spurious, unphysical extra complexity, of course. Likewise, if philosophers of physics cook up some issues which were never confusing for physicists, then investigating those issues might not be the best use of our time. To give a couple of examples, some of us (including myself!) have recently become distracted by questions regarding:

[22] I felt a bit guilty even mentioning it to such an erudite audience in the talk. But one thing which came up in a previous talk is that it's not obvious that new papers on some topic always exceed older papers in terms of quality—this is exactly what I have in mind here.

[23] What I have in mind, in other words, is that we should all aspire to the breadth of erudition of scholars such as the RCNP's very own Klaas Landsman.

1. Whether black holes are thermodynamic objects.
2. Whether special relativity is locally valid in general relativity.
3. Etc.

As in the case of the 'old chestnuts', I don't want to claim that doing this work is of no *philosophical* value—to the contrary, I think it can bring a lot of insight (naturally, that's why I have written on the second of these). But it's unclear to me whether working on such topics amounts to shedding light on the subject matter in a way which would be of maximal value—either to practitioners, or to ourselves![24] Here, passages such as the following from Ruskin seem pertinent:

> We want one man to be always thinking, and another to be always working, and we call one a gentleman, and the other an operative; whereas the workman ought often to be thinking, and the thinker often to be working, and both should be gentlemen, in the best sense. As it is, we make both ungentle, the one envying, the other despising, his brother; and the mass of society is made up of morbid thinkers and miserable workers. Now it is only by labour that thought can be made healthy, and only by thought that labour can be made happy, and the two cannot be separated with impunity.[25]

We should strive to be happy thinkers—and that comes from real labour on the pressing first-order problems.

The final 'spectre', (iv), has to do with what I call 'normal science'. One should not take the Kuhnian terminology here too seriously—what I mean by this is just the following. The worry is that we end up algorithmically deploying the conceptual tools which we have at our disposal as an end in itself, rather than either using those tools as a *means* to conceptual insight, or developing new tools to deliver the possibility of novel conceptual insights on the subject matter.[26] To take an example: the resources of categorical equivalence have certainly added a lot to recent philosophy of physics (in my opinion at least). But at what point does endlessly proving the categorical equivalence of X, Y, and Z cease to be a philosophical endeavour? It can still be a worthy philosophical task insofar as its use delivers new conceptual insights on the subject matter, and there is much good work which uses the tools of categorical equivalence in just this way. And we can still develop approaches like this in yet new ways in order to avail ourselves of yet more conceptual tools: this of course is also

[24] In the talk, I suggested that it's windmill-tilting. But I'm grateful to Reward Mulder for stressing to me that value to physics practitioners alone shouldn't be the sole measure of value for our work as philosophers of physics. But even for ourselves, I'd contend that there is more value in exploring my 23 than in exploring many of these 'de-occamised' issues.

[25] John Ruskin, *The Stones of Venice* Vol. II: Cook and Wedderburn 10.201.

[26] Of course, addressing my 23 might look like 'problem solving', and so 'normal science' in a Kuhnian sense. As I said, don't take the terminology too seriously. Addressing any of my 23 will require new conceptual insights, innovations, strategies, etc.—what I am worrying about here is that we as a community neglect to do these these things and end up merely handle-cranking.

good philosophy. But what I have in mind here is a case in which we think that proving categorical (in)equivalence is *per se* the end goal—it is not. Philosophy of physics should be conceptually disruptive, shedding new light on the subject matter in order that we (and the practitioners) come to understand it—i.e., the physical world—better.

So, anyway, there is my list.

Acknowledgements

I'm very grateful to Jeremy Butterfield, Frank Cudek, Erik Curiel, Daniel Grimmer, Eleanor March, Oliver Pooley, Simon Saunders, Chris Timpson, Jim Weatherall, and Will Wolf for discussions on this topic in advance of my talk. I'm also grateful to the audience for subsequent discussions which were far from desultory! Special thanks here to Klaas Landsman, Ruward Mulder, and Fred Muller for probing questions after the talk.

References

Brown, Harvey R. (2005). *Physical Relativity: Space-Time Structure From a Dynamical Perspective*. Oxford, GB: Oxford University Press UK.

Brown, Harvey R. and Uffink, Jos (2001). "The Origins of Time-Asymmetry in Thermodynamics: The Minus First Law". *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 32.4, pp. 525–538. DOI: 10.1016/s1355-2198(01)00021-1.

Cudek, Frank (2024). "Localising the singular structure of spacetime".

Cuffaro, Michael E. and Hartmann, Stephan (2024). "The Open Systems View". *Philosophy of Physics* 2.1, pp. 6–1.

de Haro, Sebastian and Butterfield, Jeremy (2025). *The physics and philosophy of dualities*. Oxford university press.

Earman, John (1986). *A Primer on Determinism*. D. Reidel.

Earman, John and Norton, John (1987). "What Price Spacetime Substantivalism? The Hole Story". *British Journal for the Philosophy of Science* 38.4, pp. 515–525. DOI: 10.1093/bjps/38.4.515.

Fatibene, L. and Francaviglia, M. (2003). *Natural and Gauge Natural Formalism for Classical Field Theorie: A Geometric Perspective including Spinors and Gauge Theories*. Springer Netherlands. ISBN: 9781402017032. URL: https://books.google.co.uk/books?id=nxVzyUpTglIC.

Fine, Dana and Fine, Arthur (1997). "Gauge Theory, Anomalies and Global Geometry: The Interplay of Physics and Mathematics". *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 28.3, pp. 307–323. DOI: 10.1016/s1355-2198(97)00011-7.

Fletcher, Samuel C. (2016). "Similarity, Topology, and Physical Significance in Relativity Theory". *British Journal for the Philosophy of Science* 67.2, pp. 365–389. DOI: 10.1093/bjps/axu044.

Gomes, Henrique (2024). "Gauge Theory Without Principal Fiber Bundles". *Philosophy of Science*, pp. 1–17. DOI: DOI:10.1017/psa.2024.49.

Grattan-Guinness, Ivor (Jan. 2000). "A Sideways Look at Hilbert"s Twenty-three Problems of 1900". *Notices of the American Mathematical Society* 47.

Halvorson, Hans (2019). *The Logic in Philosophy of Science*. Cambridge and New York: Cambridge University Press.

Jacobs, Caspar (2023). "The Metaphysics of Fibre Bundles". *Studies in History and Philosophy of Science Part A* 97.C, pp. 34–43. DOI: 10.1016/j.shpsa.2022.11.010.

Lehmkuhl, Dennis, Schiemann, Gregor, and Scholz, Erhard, eds. (2017). *Towards a Theory of Spacetime Theories*. New York, NY: Birkhauser.

Luc, Joanna (2020). "Generalised Manifolds as Basic Objects of General Relativity". *Foundations of Physics* 50.6, pp. 621–643. DOI: 10.1007/s10701-019-00292-w.

Pitts, J. Brian (2012). "The Nontriviality of Trivial General Covariance: How Electrons Restrict ?Time? Coordinates, Spinors Fit Into Tensor Calculus, and of a Tetrad is Surplus Structure". *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 43.1, pp. 1–24. DOI: 10.1016/j.shpsb.2011.11.001.

— (2022). *First-Class Constraints, Gauge Transformations, de-Ockhamization, and Triviality: Replies to Critics, Or, How (Not) to Get a Gauge Transformation from a Second-Class Primary Constraint*. arXiv: 2212.02944 [physics.hist-ph]. URL: https://arxiv.org/abs/2212.02944.

Schlosshauer, M. (2007). *Decoherence: And the Quantum-To-Classical Transition*. The Frontiers Collection. Springer. ISBN: 9783540357735. URL: https://books.google.co.uk/books?id=1qrJUS5zNbEC.

Wallace, David (2023). "The Sky is Blue, and Other Reasons Quantum Mechanics is Not Underdetermined by Evidence". *European Journal for Philosophy of Science* 13.4, pp. 1–29. DOI: 10.1007/s13194-023-00557-2.

Weatherall, James Owen (2016). "Fiber Bundles, Yang?Mills Theory, and General Relativity". *Synthese* 193.8. DOI: 10.1007/s11229-015-0849-3.

— (2020). "Two Dogmas of Dynamicism". *Synthese* 199.S2, pp. 253–275. DOI: 10.1007/s11229-020-02880-0.

Wolf, William J. and Ferreira, Pedro G. (Nov. 2023). "Underdetermination of dark energy". *Phys. Rev. D* 108 (10), p. 103519. DOI: 10.1103/PhysRevD.108.103519. URL: https://link.aps.org/doi/10.1103/PhysRevD.108.103519.