La Bohume

September 29, 2016

1 Introduction

David Lewis, famously, suggested a certain kind of picture of what the world is like. He called that picture *Humean supervenience*, and described it as follows:

> Humean Supervenience [...] says that in a world like ours, the fundamental relations are exactly the spatiotemporal relations: distance relations, both spacelike and timelike, and perhaps also occupancy relations between point-sized things and spacetime points. And it says that in a world like ours, the fundamental properties are local qualities: perfectly natural intrinsic properties of points, or of point-sized occupants of points. Therefore it says that all else supervenes on the spatiotemporal arrangement of local qualities throughout all of history, past and present and future.[1]

However, there is a concern that Humean Supervenience is inconsistent with our best physical theories.[2] More specifically, there is a concern that the kind of world described by Lewis above—one which is fully and exhaustively characterised by the assignment of intrinsic qualities to points of spacetime—could not be a world described by quantum mechanics.[3] More specifically still, the concern is that the characteristic quantum-mechanical phenomenon of *entanglement* rules out the possibility of giving

[1] [Lewis, 1994, p. 474]

[2] [Teller, 1986], [Maudlin, 2007]

[3] To be strictly accurate, there are good reasons for thinking that Humean supervenience, at least on the letter of the above, is inconsistent with *classical* physics. However, one can get around this by (roughly speaking) taking "local qualities" to be intrinsic properties of infinitesimally small spacetime regions, rather than spacetime points *per se* (see [Butterfield, 2006]). I will ignore this subtlety for the purposes of this essay.

an exhaustive description of the world by describing it point-by-point. So (according to these arguments), insofar as we take quantum mechanics to be *true* (i.e., insofar as we take the actual world to be accurately described by quantum mechanics), we should not take Humean Supervenience to be true either.

More recently, however, there has been a fightback on behalf of Humean Supervenience: it has been argued that, at least if one is a Bohmian about quantum mechanics,[4] then Humean Supervenience remains a consistent option after all. This paper seeks to resist this most recent defence of Humean Supervenience. First, I introduce the relevant pieces of Bohmian mechanics, and indicate the *prima facie* tension between entanglement and Humean Supervenience. Second, I discuss the argument that Bohmian Humeans (from here on out, "Bohumeans") make to render their ontology compatible with Humean Supervenience. This argument rests upon a particular claim: namely, that the wavefunction may be regarded as a Humean summary of the positions of the Bohmian particles over time. I then argue that we can have no good reason to think that this "summarising" claim is true. I conclude with some remarks about the relationship between locality requirements and scientific evidence.

Before I start, I want to clarify that this paper is *not* about whether some suitably modified version of Humean Supervenience is compatible with quantum mechanics. For instance, Loewer and Albert have observed that quantum mechanics, standardly formulated, is straightforwardly compatible with the requirement that qualities be local in *configuration* space, rather than physical space;[5] whilst Darby has argued that we can preserve the "spirit" of Lewis' proposal by allowing that there are fundamental relations *besides* the spatiotemporal relations.[6] All three note that doing so is consistent with Lewis' broader Humean goal of recovering all else (all mental and nomological facts, in particular) from the categorical world, i.e., from a particular distribution of non-modal properties and relations. This is all well and good, but not my concern here. I am exclusively attending to the question of whether the specific variety of Humean Supervenience defended by Lewis (that requiring the locality of *all* fundamental properties in *physical* space) can be rendered consistent with quantum mechanics.

[4] [Esfeld, 2014] claims that the proposed rescue of Humean Supervenience is available to any "primitive-ontology" approach to quantum mechanics, not just Bohmian mechanics. It's not my intention to examine this claim in this essay: in the interests of brevity, I will focus on the specific case of Bohmianism (though see fn. 13).

[5] See e.g. [Loewer, 1996], [Albert, 1996].

[6] [Darby, 2012]

2 Entanglement in Bohmian mechanics

The fundamental entities of Bohmian mechanics are the *particles*: pointlike objects, which have definite positions at all times, and which are held to be the fundamental constituents of macroscropic matter.[7] Thus, if we have N particles, then their collective state at any given time may be represented by an N-tuple $(Q_1, \ldots, Q_N)$ of points of X, where X is the space representing physical space—that is, by a single point $\mathbf{Q}$ in the N-fold configuration space X^N (the N-fold direct product of X with itself). The behaviour of the particles is determined by the *wavefunction*, a function $\Psi : X^N \to \mathbb{C}$, via the *guidance equation*

$$\frac{dQ_i}{dt} = \frac{\hbar}{m_i} \mathrm{Im} \left(\frac{\nabla_i \Psi}{\Psi}(\mathbf{Q}) \right) \tag{1}$$

where m_i is the mass of the ith particle, and ∇_i denotes the gradient associated with the ith productand of X^N. (This is all in the absence of spin: for the purposes of this essay, we need only consider spinless particles.) The wavefunction itself evolves according to the usual Schrödinger equation,

$$i\hbar \frac{d\Psi}{dt} = H\Psi \tag{2}$$

where H is the Hamiltonian.

The challenge for the aspiring Bohumean may now be stated quite succintly: the wavefunction cannot be any part of a Humean Supervenience basis, and hence cannot (for one attracted by Lewis' picture) be interpreted as a fundamental physical component of the world. For, the wavefunction assigns values (complex numbers) to N-tuples of points of space, not to individual points of space. But the Humean Supervenience basis was required to include only local qualities, i.e., those comprising the assignment of intrinsic properties to individual spacetime points (or to point-sized occupants of spacetime points). So the wavefunction is not the kind of local property with which Lewis would be happy, unless there is some way of showing that any given wavefunction can be reduced to (uniquely specified by) some collection of suitably local qualities.

Certainly, there are specific circumstances in which such a reduction is possible: those in which the wavefunction is not *entangled*. For simplicity, let $N = 2$; now sup-

[7] My presentation of Bohmian mechanics here follows that of [Dürr et al., 1992] and [Dürr and Teufel, 2009].

pose that the wavefunction $\Psi(x_1, x_2)$ is a *product* wavefunction,

$$\Psi(x_1, x_2) = \psi_1(x_1)\psi_2(x_2) \tag{3}$$

for some $\psi_1 : X \to \mathbb{C}$ and $\psi_2 : X \to \mathbb{C}$. Then since

$$\frac{\nabla_1(\psi_1\psi_2)}{\psi_1\psi_2} = \frac{\nabla_1\psi_1}{\psi_1} \tag{4}$$

and similarly for particle 2, we find that the general guidance equation (1) decomposes into the two individual guidance equations

$$\frac{dQ_1}{dt} = \frac{\hbar}{m_1}\mathrm{Im}\left(\frac{\nabla_1\psi_1}{\psi_1}(Q_1)\right) \tag{5a}$$

$$\frac{dQ_2}{dt} = \frac{\hbar}{m_2}\mathrm{Im}\left(\frac{\nabla_2\psi_2}{\psi_2}(Q_2)\right) \tag{5b}$$

So in a case such as this, where the joint wavefunction is simply a product of single-particle wavefunctions, we can make the joint wavefunction Humeanistically acceptable by regarding it as a "conjunction" of duly local individual wavefunctions.

The problem, though, is that generic wavefunctions are entangled, i.e., are not expressible as a product of single-particle wavefunctions. Still with $N = 2$, consider as an example

$$\Psi(x_1, x_2) = \frac{1}{\sqrt{2}}\left(\psi_1(x_1)\psi_2(x_2) + \psi_1'(x_1)\psi_2'(x_2)\right) \tag{6}$$

where $\int_X \psi_1^*\psi_1'\, dx_1 = 0 = \int_X \psi_2^*\psi_2'\, dx_2$. The sum (6) cannot be factorised into a single product, and so we cannot treat it as simply arising from some pair of assignments to the points of X individually.

The nearest proxies for individual wavefunctions, in a case such as (6), are the *conditional* wavefunctions.[8] The conditional wavefunction of particle 1, relative to particle 2's being in location Q_2, is given by

$$\Psi_1^{Q_2}(x_1) := \Psi(x_1, Q_2) \tag{7}$$

and similarly for the conditional wavefunction of particle 2, relative to particle 1's being in location Q_1. More generally, given an N-particle joint wavefunction $\Psi(x_1, \ldots, x_N)$, if we select (say) the first $M < N$ particles as a subsystem, then the *conditional wavefunc-*

[8]The below follows [Dürr and Teufel, 2009, chap. 11].

tion of that subsystem (relative to the configuration of the remaining $N - M$ particles) is given by

$$\Psi^{\mathbf{Y}}_{1\ldots M}(\mathbf{x}) := \Psi(\mathbf{x}, \mathbf{Y}) \tag{8}$$

where $\mathbf{x} := (x_1, \ldots, x_M)$ and $\mathbf{Y} := (Q_{M+1}, \ldots, Q_N)$. That is, the conditional wavefunction of the subsystem is obtained by "saturating" the joint wavefunction with the actual locations of the remaining particles.

The importance of the conditional wavefunction is as follows. Suppose that the joint wavefunction is of the form

$$\Psi(\mathbf{x}, \mathbf{y}) = \phi(\mathbf{x})\psi(\mathbf{y}) + \Psi_{\perp}(\mathbf{x}, \mathbf{y}) \tag{9}$$

where $\mathbf{y} = x_{M+1}, \ldots, x_N$ and $\Psi_{\perp}$ and ψ have macroscopically disjoint $\mathbf{y}$-supports; moreover, suppose that the actual configuration $\mathbf{Y}$ of the environment is in the support of ψ (so that $\Psi_{\perp}(\mathbf{x}, \mathbf{Y}) = 0$ for all $\mathbf{x}$). It then follows that the conditional wavefunction $\Psi^{\mathbf{Y}}_{1,\ldots,M}$ is given by the wavefunction ϕ—and furthermore, that the guidance equation for the subsystem's configuration $\mathbf{X} := (Q_1, \ldots, Q_M)$ reduces to

$$\frac{d\mathbf{X}}{dt} = \frac{\hbar}{\mathbf{m}} \mathrm{Im} \left(\frac{\nabla_{\mathbf{x}} \phi}{\phi}(\mathbf{X}) \right) \tag{10}$$

where $\mathbf{m} = (m_1, \ldots, m_M)$. In such a case, we say that ϕ is an *effective* wavefunction for the subsystem. If the subsystem is sufficiently decoupled from its environment, then the effective wavefunction will also abide by Schrödinger's equation; if there is interaction, however, then it will not evolve in this unitary fashion.

It is, however, important to note that although the conditional wavefunctions of the subsystems can be computed from the "universal wavefunction" Ψ and the actual configuration $\mathbf{Q}$, the reverse is not true: the conditional wavefunctions associated to subsystems underdetermine the joint wavefunction. For example, in the two-particle case, one can easily have a distinct pair of joint wavefunctions $\Psi(x_1, x_2)$ and $\Phi(x_1, x_2)$ such that $\Psi(x_1, Q_2) = \Phi(x_1, Q_2)$ and $\Psi(Q_1, x_2) = \Phi(Q_1, x_2)$: that agreement only requires that they coincide on certain surfaces within configuration space. Moreover, in the case where the subsystem and the environment are coupled to one another, it is not just that the conditional wavefunction does not evolve according to the Schrödinger dynamics—in general, there will not be any autonomous dynamics for the conditional wavefunction at all.

One more remark. In the above, I have followed orthodoxy by supposing that the best way to interpret the wavefunction “ontologically” is as a field of some sort (i.e., as assigning properties to points of configuration space). But as [Belot, 2011] points out, Bohmians have a reasonably natural alternative: that of interpreting the wavefunction as representing a collective property of the particles. Each possible wavefunction, on this view, would be a kind of dispositional property which specified, for each possible configuration of the collective of particles, how the particles would behave if they found themselves in that configuration. However, this interpretation would be subject to the same problem as the more mainstream interpretation in terms of fields: at any given time, a collective of N particles is an occupant of N spacetime points, not an occupant of a (single) spacetime point, and so the wavefunction is not the kind of local property that can be safely admitted into the Humean Supervenience basis.

3 The Humean response

As mentioned in §1, I am not going to consider responses that modify the Lewisian statement of Humean Supervenience; my interest in this essay is in responses which preserve the letter as well as the spirit of Humean Supervenience. Doing that requires that everything in the supervenience basis——everything that comprises the fundamental ontology—is local in space and time. As we have just seen, though, the wavefunction is *not* spatiotemporally local in the required sense. So that leaves only one option: deny that the wavefunction is part of the supervenience basis.

The natural next question, then, is what the status of the quantum state is on this picture. If standard Bohmian mechanics is indeed to be recovered, then it had better be the case that the wavefunction—like everything else—supervenes upon the supervenience basis, i.e., upon the motions of the Bohmian particles. We need to be careful, however, about the exact sense in which this supervenience takes place. One might have thought that the supervenience thesis had the following form: given the trajectories of the Bohmian particles, there is a unique wavefunction which could have brought about those trajectories in a dynamically acceptable way. That is, let $\Psi : T \times X^N \to \mathbb{C}$ be an N-particle wavefunction, and $\mathbf{Q}^N : T \to X^N$ be a trajectory through N-particle configuration space, such that Ψ and $\mathbf{Q}$ between them solve the Schrödinger equation (for some specific Hamiltonian H) and the Bohmian guidance equation. Then (the claim goes) there is no distinct wavefunction $\Psi' : T \times X^N \to \mathbb{C}$ such that Ψ' and $\mathbf{Q}$ jointly solve the Schrödinger equation (with the same Hamiltonian) and the Bohmian guidance equation.

This kind of supervenience is not to be had, at least in general: there are distinct solutions of the Schrödinger equation which generate the same motions for Bohmian particles.[9] Consider a Bohmian particle in a box: that is, a particle with one positional degree of freedom, which is confined to the unit interval $[0, 1]$ (but is otherwise free). Then the energy eigenfunctions of the system are of the form

$$\phi_n(x) = \sin(n\pi x) \tag{11}$$

for $n = 1, 2, 3, \ldots$. As an eigenfunction, ϕ_n evolves under the Schrödinger equation only into states which are equivalent to ϕ_n (up to phase). But by the guidance equation, $dQ/dt = 0$ if the wavefunction is ϕ_n, or if it is any wavefunction equivalent to ϕ_n . So any pair of such eigenfunctions are associated to the same Bohmian trajectory: namely, that of the particle remaining at rest. The best that can be hoped for is that cases such as this are exceptional; this is plausible, but it is not clear how to go about proving it.

However, for our purposes here the question is moot. For even if a positive answer to this technical question could be found, it is not one which would be desperately useful to the Humean: for what it would show is that in non-exceptional cases, the Bohmian trajectories *together with the laws of Bohmian mechanics* uniquely determine the wavefunction. And of course, the Humean denies that the laws are to be taken as part of the supervenience basis. However, this also suggests a natural thing the Humean might seek to say instead: that the Bohmian trajectories determine *both* the quantum dynamics *and* the wavefunction. This means that the Humean can finesse the technical question above, by arguing that the wavefunction is determined by the same "best-system" method used to generate the laws. That is, the claim need not be that the Bohmian trajectories uniquely fix the quantum dynamics *and* the specific wavefunction involved in those dynamics: or at least, not in the sense of there being just one dynamics-plus-wavefunction package which would deliver those trajectories. Instead, the idea is that of the candidate packages, precisely one will maximise simplicity and strength (under some appropriate weighting); and *this* package is the one which the Humean takes to be the correct characterisation of what's going on.

This is the strategy advocated by a number of recent authors.[10] In general, these authors seem more or less sympathetic to the idea that the supervenience basis be extremely austere: that it be constituted by nothing other than the Bohmian trajec-

[9]The below is taken from [Belot, 2011].

[10][Miller, 2014], [Bhogal and Perry, 2015], [Callender, 2014], [Esfeld et al., 2014]; [Dickson, 2000] also prefigures some of the relevant ideas.

tories. Such austerity may not be necessary, however. The Bohumean could include other data in their supervenience basis, provided only that such data are appropriately local. (The advantage of doing so is that the richer the supervenience basis, the more plausible it is that the full Bohmian dynamics really will supervene upon it.) For instance, they could perhaps include such particle properties as mass or charge, or (total) spin[11]—provided that such properties are construed as intrinsic properties of the Bohmian particles, rather than characteristics of the wavefunction.[12] They could even include the conditional wavefunction of each particle (relative to the other $N-1$ particles), although this might need some explanation of why the conditional wavefunctions get to be part of the fundamental ontology but the joint wavefunctions do not. In order to not prejudge the question of what should or should not go in the supervenience basis, I will just denote the basis as H.[13]

So, the picture is as follows. We take as given our supervenience basis H. H certainly includes the Bohmian trajectories, and may or may not include other local data (e.g. particle-properties or the conditional wavefunction). In order to specify the best system, we need to then introduce a new piece of theoretical vocabulary: that of the wavefunction, Ψ. The Humean should then claim that the best system for codifying H is one which asserts the following:[14]

- That Ψ is a complex-valued field on $T \times X^N$
- That $\Psi(0, \mathbf{x})$ has such-and-such a value at $\mathbf{x}$, for each $\mathbf{x} \in X^N$

[11]That is, the spin quantum number of the particles; not the projection of the spin along some axis, which cannot plausibly be interpreted as a property of the particle rather than the wavefunction (see e.g. [Dürr and Teufel, 2009, §8.4]).

[12]Note that doing so is not entirely straightforward: see [Brown et al., 1996].

[13][Esfeld, 2014] observes that other primitive ontologies could be used to provide alternative austere supervenience bases; it's not so clear that other primitive ontologies are so amenable to forming the richer bases discussed here, however. For example, if the mass of a particle is to be localised by being taken as a property of the particle, then the primitive ontology for that particle will have to be a point-sized occupant of some spatial point (at each time), as is the case in Bohmian mechanics. In GRWm or GRWf, by contrast, the primitive ontology of the particle is either a region-sized occupant, or else a point-sized occupant of multiple spatial points at each time (and in the case of flashes, sometimes an occupant of no point)—so treating mass as a property of a particle with that primitive ontology would not mean that mass was a local quality.

[14][Bhogal and Perry, 2015] use a best system which postulates a space Q (with the structure of X^N) and a particle ω moving around within Q (whose location at any time is exactly correlated with the configuration of the N particles); the wavefunction is then postulated as a function assigning a complex number to each point of Q, which then acts on ω via the guidance equation. If Q here is intended to simply be defined as X^N (i.e., as the space consisting of N-tuples of points of X), then I take these systems to be essentially the same. If not—that is, if the idea is to stipulate Q's structure separately and then put it into appropriate correspondence with N-tuples of points of X—then it seems to me that the system outlined here will be considerably simpler, at no cost in strength.

- That Ψ satisfies the Schrödinger equation
- That the location of each particle, together with Ψ, satisfies the Bohmian guidance equation

Let us refer to this best system package as B.

[Esfeld et al., 2014], [Miller, 2014] and [Callender, 2014] don't characterise their position as involving a non-standard form of Humeanism. For these authors, it remains the case that only the nomological facts arise from a best-systems analysis; so for them, making this Humean move requires treating the wavefunction as nomological rather than ontological. Although they recognise that this treatment of the wavefunction may require some revision of our usual conception of laws,[15] I think that the more significant novelty is that we are utilising a best system whose vocabulary is not confined to terms referring to individuals and properties in the supervenience basis.[16] Bhogal and Perry, however, do discuss this departure from more standard presentations of Humeanism:[17]

> The way we do this is by expanding the language that candidate systems can be formulated in. As before [i.e., in standard Humeanism], systems can use vocabulary that refers to perfectly natural properties (the properties that make up the mosaic)—what we've called the "base language." But in addition to this they can introduce and use any other vocabulary so long as it comes in uninterpreted.
>
> How does such uninterpreted vocabulary come to have content? It can have content if a system links the novel vocabulary to the base language; that is, if the system contains sentences that contain both novel vocabulary and the already interpreted vocabulary of the base language.[18]

We'll see below some of the effects of this liberalisation.

For now, the important thing is to recognise that the whole strategy turns on the following claim:

Core claim. B is the best systematisation of H.

[15] Callender, in particular, discusses this in detail.

[16] If the wavefunction Ψ did refer to anything in the basis, then we would instead be dealing with something like the Albert/Loewer/Darby strategy.

[17] Albeit one which—as they observe—is prefigured by [Lewis, 1994]'s discussion of chance, and [Hall, 2009]'s discussion of mass and charge.

[18] [Bhogal and Perry, 2015, p. 5]

I now argue that the Bohumeans have not given us any good reason to believe this claim. It bears emphasising that the burden of proof should fall on those making this claim to give us some reason to accept it. A good way to see this is to consider the following view, which I'll call "Bohkleyianism". The Bohkleyan, like the Bohumean, wants to make quantum mechanics consistent with an extremely austere fundamental basis. In fact, the Bohkleyan holds that the only things which fundamentally exist are her own phenomenological experiences. By adopting the Humean strategy, though, she also claims that she can advocate Bohmian mechanics: by introducing new theoretical vocabulary (that of the wavefunctions and the particles), and linking it to her phenomenological vocabulary. In other words, she maintains that the best systematisation of her phenomenological experiences, P, is the system B of Bohmian mechanics. The success of the Bohkleyan strategy will thus depend on the plausibility of the following claim:

Core claim*. B is the best systematisation of P.

So, why should we be Bohumeans rather than Bohkleyans? Evidently, the fundamental ontology of Bohkleyianism is (by construction) more epistemically accessible than that of Bohumeanism. And note that merely overcoming scepticism won't be enough to see off the Bohkleyan. After all, the Bohkleyan believes in the existence of the external world, in just the same way that the Bohumean believes in the wavefunction: the external world supervenes upon the fundamental ontology of her internal phenomenological experiences, just as the Bohumean's wavefunction supervenes upon the fundamental ontology of particle trajectories. So if Bohumeans are not to be Bohkleyans, then the reason can only be that they think **Core claim** is more plausible than **Core claim***. If so, then they surely owe us some account of why we ought to believe **Core claim**, in a way that won't extend to **Core claim***. In other words, if we're allowed to just help ourselves to claims about what best systematises what, then we may as well go the whole hog from Bohumeanism to Bohkleyanism.[19]

[19]cf. [Miller, 2014, p. 582], after discussing a very similar case: "she [the aspiring Bohumean] will seek to place some principled limits on the BSA [best-systems-account] strategy and, in the light of the availability of that strategy, to develop a distinction between her brand of realism and instrumentalism."

4 Defending the core claim

So, how is **Core claim** to be defended? The most explicit way to do so would be to first fix some set of trajectories which is a plausible candidate to represent the actual evolution of the world; then determine some way of measuring the combined simplicity-plus-strength, relative to those trajectories, of candidate "packages" of differential equations and wavefunctions (or other wavefunction-like things, appropriate to differential equations different from Bohmian mechanics); and then show that the package consisting of the guidance equation, Schrödinger equation, and some universal wavefunction are maximal with respect to that measure.

This is an *insanely* difficult problem. First, we need to overcome the formidable hurdles of finding an appropriate means of evaluating candidate packages. Second, even given such a means, it would be extraordinary if the project of finding some set of trajectories for which a Bohmian package is indeed the best system proved to be even remotely mathematically tractable. Third, it is rather opaque what would be involved in showing that a given Bohmian distribution is "a plausible candidate to represent the actual evolution of the world"; but given that at least a necessary condition would be that the distribution contain an unbelievably large number of particles, the prospects for doing so do not look good. In other words, if the Bohumean is going to convince us that the basis H is best systematised by Bohmian mechanics, they aren't going to do so by direct computation.

In this regard, of course, they are in the same boat as standard Humeanism about laws of nature. In general, Humeans have not sought to show directly that such-and-such a theory is the best codification of such-and-such primitive categorical facts. (Although it is worth noting that the direct computation is even less possible for the Bohumean than for the standard Humean, given that we're now allowed to introduce new theoretical vocabulary into the best system. This means that the available systems of equations to consider are not limited to just those equations employing only a fixed stock of variables and parameters (i.e., those ranging over the supervenience basis); rather, we must consider any equations whose variables and parameters include that fixed stock.) Instead, Humeans have usually taken the practice of science itself to provide some reason for thinking that our actual scientific theories—or some extension thereof—are plausible candidates for being the best systematisation of the physical facts. Bhogal and Perry suggest such a response is appropriate to the case of Bohumeanism as well:

> This worry, that mere positional facts wouldn't be complicated enough to distinguish something like Bohmian Mechanics as the best system of that world, strikes us as far too pessimistic. One of the key motivating thoughts behind the best system account is that whatever an ideal scientist, if she was fully rational and knew everything about the state of the mosaic, would take to be the best overall theory given the evidence is the best system of that world.
>
> Actual scientists are not ideal reasoners and they do not have access to the entirety of the facts about the mosaic. Of the elements of the mosaic, actual scientists only have direct access to facts about positions. [...]
>
> If we look to actual scientific practice, we see that physicists, even with access to only a tiny slice of the position facts, have a great deal of confidence that the world is quantum mechanical (and consider this position very well confirmed). If this, in the grand scheme of things, meager set of position facts is enough to satisfy non-ideal working scientists, then we see very little reason to be skeptical that the ideal scientist, with access to all the position facts at our Bohmian world, would settle on a Bohmian Mechanical physical theory.[20]

We might summarise this argument as follows:

1. Non-ideal actual scientists have empirical access to data comprising a small fragment of the facts in the Humean supervenience basis.

2. Those scientists have come up with Bohmian mechanics as the best systematisation of that data.

C. Therefore, an ideal scientist, who had access to all the facts in the supervenience basis, would come up with Bohmian mechanics as the best systematisation of that full set of facts.

Unfortunately, however, both premises of this argument are false.

First, premise 1. The basic idea here is that, in Bell's dictum, "all measurements are measurements of position"; and so—on the view of the world advanced by Bohmians—any experimental data can be characterised in terms of the positions of the Bohmian

[20][Bhogal and Perry, 2015, p. 18]

particles. The problem is that our evidence for quantum mechanics is (famously) *statistical* in nature. It is not that we have direct access to some small number of the Bohmian trajectories, and have successfully stitched those together by overlaying a wavefunction governed by quantum dynamics. What we have instead are individual but imprecise measurements of positions at particular times. By making many such measurements of identically prepared systems, and looking at the frequency distributions of the results, we can obtain high confirmation of the probability densities over such trajectories (on the Bohmian picture). So what we have really woven together into a quantum tapestry are those probability densities, rather than the trajectories themselves; and *on the Bohmian's own account*, those probability densities represent all that can ever be known for sure about the trajectories.

But suppose that scientists did in fact have access to the Bohmian trajectories. Even then, premise 2 would be false: just as a matter of sociological fact, it is false that the scientific community has alighted on Bohmian mechanics as the preferred theory for explaining and systematising quantum phenomena. What they have in fact come up with is "textbook" quantum mechanics: a messy, foundationally unclear, and yet incredibly empirically successful combination of systematic dynamics, particular models, and pragmatic rules for extracting empirical content. One might hold that this is irrelevant, though, given the empirical equivalence between Bohmian mechanics and textbook quantum mechanics:[21] doesn't that show that Bohmian mechanics and textbook quantum mechanics are *equally* capable of systematising the relevant data, and hence that it makes no odds (so far as the argument from scientific practice is concerned) whether scientists have adopted one or the other? In other words, perhaps premise 2 could be replaced by

2′ Those scientists could just as well have come up with Bohmian mechanics as the best systematisation of that data.

But whether or not something is the best system is not invariant under empirical equivalence. After all, textbook quantum mechanics is empirically equivalent to the theory consisting of all and only its observational predictions—but no-one is going to think that *that* theory is a serious candidate for best system. So we cannot use the empirical equivalence of Bohmian and textbook quantum mechanics to argue that they are equally well (or poorly) qualified to be best systems.

Alternatively, one might just think it is *obvious* that no such hodge-podge as textbook quantum mechanics could possibly be the best systematisation of the empirical data.

[21] My thanks to an anonymous referee for raising this concern.

But that's just a reason to think that the argument from scientific practice is not a good argument: if it's clearly false that textbook quantum mechanics is the best system, then that shows that actual working scientists do not always converge upon the best system, not that they have not converged upon textbook quantum mechanics. However, it does suggest a third version of premise 2: perhaps what the argument really ought to have said was

2″ Those scientists *should* have come up with Bohmian mechanics as the best systematisation of that data.

One could defend premise 2″ as follows: from the fact that scientists *have* come up with textbook quantum mechanics, infer that textbook quantum mechanics must be a *very good* systematisation of the data; from its empirical equivalence to textbook quantum mechanics, infer that Bohmian mechanics is at least as strong as textbook quantum mechanics; argue that Bohmian mechanics is simpler than textbook quantum mechanics (and than any of its rival solutions to the measurement problem);[22] and finally, infer that Bohmian mechanics is the best system, at least in the neighbourhood.

But this argument does not work either. First, we need to be a little careful about the sense of empirical equivalence in play here. That empirical equivalence means that *over those situations where both theories apply*, they will generate the same predictions. However, at least as things currently stand, there are many situations to which quantum mechanics, but not Bohmian mechanics, can be successfully applied. Most notably, although it is an ongoing (and important) frontier of research,[23] there is currently no Bohmian version of quantum field theory capable of fully replicating the standard formalism; that cuts off support from the predictive success of high energy physics. Thus, Bohmian mechanics is less strong than textbook quantum mechanics. Of course, this isn't to say that this will always remain the case: extending the scope and range of Bohmian analyses is an important and ongoing research project. The point being made here is just that whilst that project is still ongoing, comparing the two theories on strength will favour textbook quantum mechanics.

Second, even with regards to quantum systems that are in principle analysable in Bohmian terms, there are plenty of examples where doing so is highly unnatural. The standard means of analysing a quantum system involves characterising its dynamics in terms of whatever degrees of freedom are most apt for the problem at hand; but

[22] Note that I'm assuming, here, that the best system is being measured on a classic Lewisian simplicity-and-strength kind of metric.

[23] See [Struyve, 2010] for a survey.

calculating what the particles are up to requires always working in the position basis. So even if a direct Bohmian analysis is in principle available, it may well fall beyond any practical capacity of working physicists. This provides a reason to think that even in those cases where Bohmian mechanics is as strong as textbook quantum mechanics, it may not be simpler—despite its greater conceptual clarity. (A more decisive judgment, unfortunately, would only be possible with more details about how judgments of simplicity are to be made; without that, it's unclear how to trade off mathematical tractability against conceptual or foundational rigour.)

The above points are, of course, familiar from the debates over the measurement problem. So perhaps the defence of premise 2″ is simply that the Bohumean was making a conditional claim: *if* Bohmian mechanics is the best solution to the measurement problem, *then* the threat to Humean Supervenience can be thwarted. Premise 2″ would then appear to simply follow from the assumption that the criteria of simplicity and strength that judge the best system are also those which would be used to judge the best solution to the measurement problem. (Provided, that is, that we put aside the concerns I raised above and identify our experimental data—i.e., the stuff that is grist to the mill of the measurement problem—with the data about Bohmian trajectories.)

However, this appearance is deceptive. The problem is that in premise 2″, we need to understand "Bohmian mechanics" to mean "the formalism of Bohmian mechanics, *plus* a specification of the actual wavefunction" (or rather, a specification of the actual wavefunction at a given time—for brevity, I'll just say "specification of the wavefunction"). In supposing that Bohmian mechanics is the best solution to the measurement problem, we would be understanding "Bohmian mechanics" to mean just "the formalism of Bohmian mechanics". The advocate of Bohmian mechanics as a solution to the measurement problem will take it that there is *some* wavefunction that is most apt for describing the world—but the point is that insofar as they are merely a Bohmian rather than a Bohumean, the "system" that they put forward need not contain any specific claims about what the universal wavefunction is actually like. So, even supposing that Bohmian mechanics is the best solution to the measurement problem, it doesn't follow that Bohmian mechanics together with a specific wavefunction is the best systematisation of the trajectories: the latter is far, far less simple than the former (consider how much harder it would be to fit it on a T-shirt).[24]

Of course, specifying a wavefunction will give much more information about what the trajectories are going to do, i.e., will generate a stronger system. Perhaps, with the

[24]The "T-shirt test" for simplicity is borrowed from David Albert.

right way of balancing simplicity against strength, the former factor will win out and the facts about the wavefunction get counted as part of the best system. In that case, however, a different problem rears its head. If the gains in strength from specifying the wavefunction could be paid for in the coin of simplicity, why would the same not be true of specifying the (initial) positions of the particles? After all, much less data is involved in specifying where the particles are than in specifying what the wavefunction is up to: the former is just $3N$ real numbers, rather than uncountably many complex numbers (one for every point of space). But the gains from this extra data are enormous, since (together with the wavefunction) one obtains a perfect prediction of everything to happen at every moment. That would then, *ad absurdum*, make the initial conditions a lawlike matter.[25] So the Bohumean faces a dilemma, neither horn of which is palatable. If their rules for assessing the best system do not give strength enough weight, then they cannot make it plausible that a direct specification of the wavefunction (at a time) will be included. And if their rules do give strength enough weight to avoid this, then they cannot make it plausible that a direct specification of the particle positions (at a time) will be excluded.

5 Conclusion

To conclude, let's take a brief step back, and think about how Bohumeanism compares to Humeanism about classical physics. The reason the problems in the previous section arise is that the Bohumean is, in one crucial respect, worse off than her classical cousin: the latter could, at least, identify the kind of structure in the supervenience basis (i.e., intrinsic properties of points or pointlike things) with the experimental data that (idealised) science collects, and hence argue that the vast parallel-processor of the scientific enterprise has in fact systematised that data into an optimally simple and strong codification. By doing so, the classical Humean can relieve some of the pressure to make precise the nature of the best systematisation they envisage, or to show that such a thing is even possible, since science itself could be taken as demonstrating a proof of principle. The experimental basis for quantum mechanics, on the other hand, is a poor fit with the supervenience basis of the Bohumean. On the one hand, it is too big: it covers many more situations than those to which Bohmian mechanics (at its current state of development) is readily applied. On the other, it is too small: the proposed supervenience basis (even over some local region) goes far be-

[25] cf. [Hall, 2009, §5.6]

yond what could be gathered by empirical investigation (even in principle). Without this tight fit between the supervenience basis and the empirical basis, I don't see how empirical practice can be a source of optimism that Bohmian mechanics is, indeed, the best systematisation of the supervenience basis.

But this prompts a further question. Classically, a significant component of the motivation for Humean Supervenience has been taken to be *epistemic*: since what we have direct epistemic access to (the thought goes) are facts about intrinsic properties of individual spacetime points or pointlike entities, we should seek a metaphysics founded upon those facts. (This isn't to claim that Humeans are *committed* to this claim about the nature of scientific evidence; it's just that without such a claim, it's not obvious what the advantage is of insisting that all physical facts be local facts.) Now, one can certainly criticise this move, from a premise about what is epistemically available to a conclusion about what is metaphysically acceptable.[26] If, though, the practice of quantum physics does not help the Humean, then we should start to question the antecedent claim too. After all, we do in fact perform entanglement experiments, which (at least on some interpretations of quantum mechanics) constitute the observation of non-Humean facts! So what is going on?

The answer, I contend, is that although individual observations are indeed (somewhat) localised, it just does not follow that those observations cannot provide information about or evidence for irreducibly global goings-on. Prima facie, at least, the way in which one does so is about the simplest imaginable: we simply make multiple local observations, and then *aggregate* those observations. Suppose, for example, that mass was not locally conserved, but was conserved on some larger scale—let's say, on the scale of the Earth. It is straightforwardly possible to accumulate evidence for this hypothesis, by making continuous observations at different points of space, and then comparing the results. Mass disappears here, we find; but we then find that just the same quantity reappeared elsewhere, at exactly the same time. Obviously, no one observer could simultaneously verify the reappearance and the disappearance of the mass. But that's not a problem, given that they can write down their results and compare them, at a later date, with other observers. And clearly, this kind of process is somewhat more involved than the experimental processes needed to confirm or disconfirm a purely local phenomenon—and were the non-local phenomena more widespread (either covering a larger, or concerning more kinds of phenomena), then it might well move beyond our capacities to verify it. But less pervasive non-locality

[26] See [Maudlin, 2007] for a particularly biting critique.

seems like something well within our confirmatory capacities.

The point of this little parable, of course, is that it's more or less exactly what we do to verify the non-local aspects of quantum mechanics:[27] we make simultaneous local measurements in multiple locations, and then bring the results together to compare them. So the simplistic picture of scientific evidence that seemingly motivates the doctrine of Humean Supervenience is long due retirement—and with it, the insistence that our best scientific theories be made to fit that doctrine, at whatever price.

References

[Albert, 1996] Albert, D. Z. (1996). Elementary Quantum Metaphysics. In *Bohmian Mechanics and Quantum Theory: An Appraisal*, volume 184 of *Boston Studies in the Philosophy of Science*, pages 277–284. Kluwer Academic Publishers, Dordrecht.

[Belot, 2011] Belot, G. (2011). Quantum states for primitive ontologists. *European Journal for Philosophy of Science*, 2(1):67–83.

[Bhogal and Perry, 2015] Bhogal, H. and Perry, Z. (2015). What the Humean Should Say About Entanglement. *Noûs*.

[Brown et al., 1996] Brown, H. R., Elby, A., and Weingard, R. (1996). Cause and Effect in the Pilot-Wave Interpretation of Quantum Mechanics. In *Bohmian Mechanics and Quantum Theory: An Appraisal*, volume 184 of *Boston Studies in the Philosophy of Science*, pages 309–319. Kluwer Academic Publishers, Dordrecht.

[Butterfield, 2006] Butterfield, J. (2006). Against Pointillisme About Mechanics. *British Journal for the Philosophy of Science*, 57(4):709–753.

[Callender, 2014] Callender, C. (2014). One world, one beable. *Synthese*, pages 1–25.

[Darby, 2012] Darby, G. (2012). Relational Holism and Humean Supervenience. *The British Journal for the Philosophy of Science*, 63(4):773–788.

[Dickson, 2000] Dickson, M. (2000). Are There Material Objects in Bohm's Theory? *Philosophy of Science*, 67(4):704–710.

[27]That is, according to those accounts of quantum mechanics in which there are non-local physical phenomena.

[Dürr et al., 1992] Dürr, D., Goldstein, S., and Zanghí, N. (1992). Quantum equilibrium and the origin of absolute uncertainty. *Journal of Statistical Physics*, 67(5-6):843–907.

[Dürr and Teufel, 2009] Dürr, D. and Teufel, S. (2009). *Bohmian Mechanics: The Physics and Mathematics of Quantum Theory*. Springer Science & Business Media.

[Esfeld, 2014] Esfeld, M. (2014). Quantum Humeanism,or: Physicalism without properties. *Philosophical Quarterly*.

[Esfeld et al., 2014] Esfeld, M., Hubert, M., Lazarovici, D., and Dürr, D. (2014). The Ontology of Bohmian Mechanics. *The British Journal for the Philosophy of Science*, 65(4):773–796.

[Hall, 2009] Hall, N. (2009). Humean reductionism about laws of nature. Unpublished MS. Available at http://philpapers.org/rec/HALHRA.

[Lewis, 1994] Lewis, D. (1994). Humean Supervenience Debugged. *Mind*, 103(412):473–490.

[Loewer, 1996] Loewer, B. (1996). Humean Supervenience. *Philosophical Topics*, 24(1):101–127.

[Maudlin, 2007] Maudlin, T. (2007). Why Be Humean? In *The Metaphysics Within Physics*. Oxford University Press, Oxford.

[Miller, 2014] Miller, E. (2014). Quantum Entanglement, Bohmian Mechanics, and Humean Supervenience. *Australasian Journal of Philosophy*, 92(3):567–583.

[Struyve, 2010] Struyve, W. (2010). Pilot-wave theory and quantum fields. *Reports on Progress in Physics*, 73(10):106001.

[Teller, 1986] Teller, P. (1986). Relational Holism and Quantum Mechanics. *The British Journal for the Philosophy of Science*, 37(1):71–81.