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# One world, one beable

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**Abstract** Is the quantum state part of the furniture of the world? Einstein found such a position indigestible, but here I present a different understanding of the wavefunction that is easy to stomach. First, I develop the idea that the wavefunction is nomological in nature, showing how the quantum It or Bit debate gets subsumed by the corresponding It or Bit debate about laws of nature. Second, I motivate the nomological view by casting quantum mechanics in a "classical" formalism (Hamilton–Jacobi theory) and classical mechanics in a "quantum" formalism (Koopman–von Neumann theory) and then comparing and contrasting classical and quantum wave functions. I argue that Humeans about laws can treat classical and quantum wave functions on a par and that doing so yields many benefits.

**Keywords** Quantum mechanics · Hamilton–Jacobi · Koopman · Humean · Nomological · Law of nature · Classical wavefunction

## 1 Introduction

Is the quantum state part of the furniture of the world? This question has hung over quantum theory since its inception. Much is made of the infamous wave-particle duality of quantum theory. What is potentially more disturbing is learning that there is a corresponding duality in where these aspects live: the "waves" seem to exist in an "abstract" high-dimensional configuration space, whereas the particles seem to exist in a "physical" low-dimensional space. Expressed as a Schrödinger wavefunction, the quantum state "lives" in $3N$-dimensional configuration space, where $N$ is the number of particles, and not in familiar 3-dimensional space. This puzzling feature of the

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quantum state leads many to doubt its physical reality. In a letter to Ehrenfest in 1926, Einstein writes

> Schrödinger's works are wonderful—but even so one nevertheless hardly comes closer to a real understanding. The field in a many-dimensional coordinate space does not smell like something real. (Howard 1990, p. 83)

A few months later the high-dimensional waves pass neither the nose nor the stomach test:

> the waves in n-dimensional coordinate space are indigestible ..." (Howard 1990, p. 83)

Others find the wavefunction similarly disagreeable. Some react by insisting that the quantum state is—in Wheeler's memorable terminology—*Bit* and not *It*. That is, they regard the quantum state not as an It such as a table or chair, but as a Bit, an aspect of our knowledge. Others regard the waves as Its and make moves that suggest a more palatable ontology.

Whether the high-dimensionality of the quantum state is a problem is debatable, but I feel that Einstein's digestibility problem arises most sharply in the context where one holds that there is more than just wavefunction at bottom. If one subscribes to an Everettian picture, wherein there is only quantum state, then it is fairly clear that one must regard the quantum state realistically, no matter the state of one's stomach, for there is simply nothing else. If by contrast one subscribes to instrumentalism and holds that science doesn't demand ontic states, then their smell is irrelevant. But if one also posits what Einstein calls "peas"—"hidden variables" or non-wavefunction "beables" (in Bell 2004's memorable terminology) or primitive ontology—*in addition* to the wavefunction, then awkward questions arise about how the low-dimensional peas and high-dimensional wavefunctions interact. Examples of peas include the modal interpretation's value states, Bohmian mechanics' particles or fields, and GRW's mass fluid or flashes.

Here I will advocate a picture that might pass muster with Einstein, one in which there are peas but not wavefunctions in the basic ontology. Unlike recent attempts to make the wavefunction epistemic, the current view takes the wavefunction seriously. It holds that the wavefunction has a "nomological" status (Goldstein and Zanghì 2013). After developing this view, I'll show how the It or Bit debate gets absorbed by a corresponding It or Bit debate about laws of nature. Then I'll motivate the nomological picture in a new way by comparing quantum mechanics with classical mechanics when both are expressed in essentially the same formalism. Classically we don't confuse the peas with the laws. So what differences, if any, demand interpreting the classical and quantum wavefunctions differently? I'll zoom in on a particular worry and then show how a Humean understanding of laws potentially eliminates it, thus enabling the Humean to regard classical and quantum wavefunctions on a kind of par. The resulting view is metaphysically parsimonious and comes with several benefits: (e.g.) it obviates two common criticisms of beable type interpretations, illuminates the PBR theorem, and helps explain why the It versus Bit debate is so difficult and long-lasting. Whether this metaphysical picture is ultimately desirable depends on one's philosophical predilections. To me, it provides an interpretation of the quantum state that I find digestible.

## 2 Lost in space

Descartes held that the mind and the body inhabit different "spaces." The mind lives in an extensionless mental realm whereas the body lives in the extended realm of objects. His contemporaries, Pierre Gassendi and Princess Elizabeth of Bohemia, famously asked how these two objects communicated, that is, how causal influences surmounted the lack of a common space. Wouldn't ripples triggered by an event in one realm fail to reach the shore of the other? How does a thought in the mental realm trigger changes in the pineal gland (Descartes' preferred location for the mind–body nexus)? Philosophers know this problem as the "interaction problem" for Cartesian dualism. While it is one of the more famous problems in the philosophical canon, it is far from decisive against Cartesianism. Better objections are probably methodological in nature, arising from the lack of the mental realm's predictive power and its assault on parsimony. Still, different spaces bring awkward questions. How *does* the interaction work? *Where* are we? Is motion conserved in each space? And more.

A similar set of problems afflict a naive reading of "pea" theories. Because it is the most developed "pea" theory, we'll focus on the deBroglie–Bohm interpretation of quantum mechanics; however, I expect that most of what I say will be true of others, too. deBroglie–Bohm (Bohm 1952) is an attempt to solve the notorious measurement problem. According to this class of theories, the quantum wavefunction evolves unitarily according to a linear wave equation which guides the motion of Einstein's peas (e.g., particles, classical fields, fermion number density). In the familiar non-relativistic case, the ontology appears to be a wave evolving according to the Schrödinger equation and a particle configuration evolving with a velocity given by the conserved current divided by the probability density. No less an authority than J.S. Bell tells us that " …no one can understand this [Bohm] theory until he is willing to think of $\psi$ as a real objective field rather than just a " probability amplitude" (2004, p. 128). Hence the apparent ontological dualism: existence of both $\psi$ and particles.

The quantum state is a ray in a high-dimensional Hilbert space, $\mathcal{H} = \mathcal{L}^2(\mathbb{R}^{3N}, d^{3N}q)$, where $N$ is the number of particles in the universe. Since the Bohmian privileges position, it is more natural to work with the field the wavefunction defines (see below) on a configuration space $\mathbb{R}^{3N}$ than Hilbert space. The peas, by contrast, live in a low 3-dimensional space, $\mathbb{R}^3$. Unless $N = 1$, the field on configuration space and the primitive ontology then live in *different* spaces, e.g., $\mathbb{R}^{3N} \neq \mathbb{R}^3$ (Fig. 1).

Different spaces again invite awkward questions. The field in $\mathbb{R}^{3N}$ "guides" an $N$-particle system in $\mathbb{R}^3$. How does this puppet-master operate the puppet without any strings? [1] And where do we live, in $\mathbb{R}^3$ or $\mathbb{R}^{3N}$? Is motion conserved in

[1] Strictly speaking, because the particle configuration doesn't affect the wavefunction, we have only one half of Descartes' problem. The question here is more like the "causation" problem afflicting the mind–body epiphenomenalist.

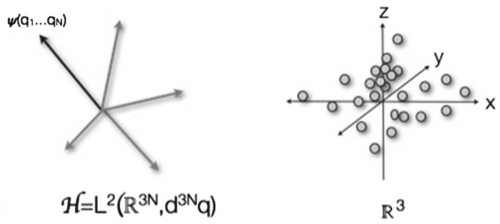


**Fig. 1** High- vs low-dimensional space

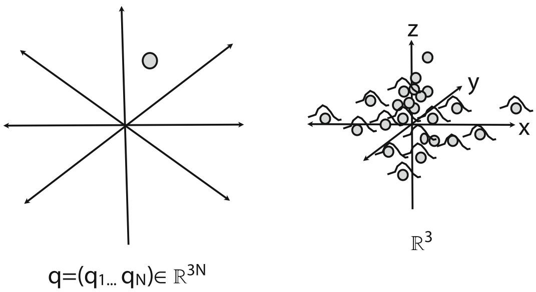


**Fig. 2** Albert vs Norsen

the two spaces? These problems are not threats to the logical coherence or empirical adequacy of the theory, and some may even be said to be badly motivated (Callender and Weingard 1997); still, if not blemishes on the theory they raise the hope that something better might be possible. Call this the problem of being *lost in space*.

The philosophical literature has directed attention to the "two-space" problem (see Ney and Albert 2013). Some respond by trying to stuff the waves and particles into the same space. Two directions are possible (Fig. 2). Albert (1996) champions boosting the particle configuration upstairs into $\mathbb{R}^{3N}$. Here the beable must be a single Bohmian "world" particle. People, planets, particles and all the rest somehow emerge from this world particle. Alternatively, one can kick the field on configuration space downstairs into $\mathbb{R}^3$, providing each particle with its own wave to surf. Doing so is highly non-trivial, yet recently Norsen (2010) has gone some distance toward showing how this might be accomplished. Each view has its merits and demerits. However these stack up, it's fair to say that either is sufficiently radical that having a third option seems worthwhile.

Here we can be guided by the mind–body debate. There one eliminates the interaction problem not by retaining dualism and putting the different kinds of entity in the same space, as Albert and Norsen do, but by eliminating one half of the ontology. Berkeley famously eliminates the corporeal and retains only the mental entities in the mental realm. Materialists and physicalists instead eliminate the mental entities and keep only the material. Here we have two options as well. One is to retain only the wavefunction. Doing so commits one to an Everettian interpretation and all of its attendant challenges. The other is to simply posit the non-wavefunction beables, Einstein's peas, and nothing else.[2] Can this second path be motivated?

## 3 The nomological view

The best response to the above set of concerns is to eliminate them altogether by denying that the quantum state's status is ontological and asserting that it is instead nomological (Dürr et al. 1997; Goldstein and Zanghì 2013). The idea is that the quantum state does not represent an entity in the world, a piece of ontology, but rather is part of the representational structure of the laws of physics. The quantum state is therefore akin to the Hamiltonian function. Like the quantum state, the classical Hamiltonian generates the motion of the beables and also their statistics. It doesn't "live" in ordinary three-dimensional space, for it is defined on an even higher dimensional space than our field on configuration space, namely, phase space, $\mathbb{R}^{3N}$x$\mathbb{R}^{3N}$. Yet no one worries about where the Hamiltonian lives because it is viewed as part of the laws, not the physical world itself. There is simply no expectation that laws be functions over three-space, nor that they be decomposable into functions on three-space.

This conceptual reorientation results in a very satisfying picture. In non-relativistic theory, the world consists simply of a bunch of Bohmian particles or GRW flashes or other forms of Einstein's peas—and that's it. In the Bohmian case, the Schrödinger and guidance equations are then the laws, existing wherever laws exist. We have two spaces, unlike in Albert and Norsen, but no dualistic ontology and therefore no interaction problem. This resolution eliminates various complaints against beable theories, as we'll see below, and it has a number of other attractive features, parsimony high among them.

The biggest intuitive obstacle to the nomological perspective is that the quantum state doesn't *seem* like the Hamiltonian in certain crucial respects. Indeed, the analogy is hardly perfect. Brown and Wallace (2005) and Belot (2012) give voice to various problems. The most important is probably the fact that the wavefunction seems contingent (and hence non-lawlike) because it is variable. It varies by system and with time.

There are reasons to think that such disanalogies can't be decisive. Neither the notion of a law of nature nor our understanding of the quantum state are solid enough to ground objections based on analogies and disanalogies. On the law of nature side, note that some philosophers and physicists believe that "evolving" laws of nature make sense (e.g., Smolin 2013). For them an inference from a quantum state's time-dependence to its not being a law is not sound. Stepping back, one may also point out that our intuitions about what is nomological are presumably formed by comparison with classical physics. Yet one can reasonably ask why we should take lessons about

[2] Another much more radical notion is doing without the wavefunction at all; that is, writing the theory directly in terms of an equation for the beables and nothing more. Suggestive models exist that suggest this path both for GRW-type theories (e.g., Dowker and Henson 2004) and for Bohm-like theories (e.g., Poirier 2010); however, since the latter model is committed to an actual infinite ensemble of "world" trajectories, it seems more like an Everettian theory.

ontology from a theory that we know is wrong. Perhaps quantum theory is *telling* us about the nature of the nomological.

However the burden falls, note that for beable theories, one needs to approach these disanalogies by first distinguishing *universal* and *effective* wavefunctions. The universal wavefunction is the wavefunction of the entire universe. The wavefunctions associated with systems in laboratories, people, planets and other subsystems, by contrast, are effective wavefunctions. Let $Q_t$ be the actual configuration of particles in the universe at a time. In a composite system $Q_t = (X_t, Y_t)$, where $X$ is the actual subsystem of interest and $Y$ is the actual environment. Then a natural definition for a subsystem's wavefunction is the conditional wavefunction $\psi_t(x) = \Psi_t(x, Y_t)$, where we calculate the universal wavefunction $\Psi$ in the actual configuration of the environment (Dürr et al. 1992). The conditional wavefunction will not in general evolve according to the Schrödinger equation, but *when it does*—which it will if the universal wavefunction evolves into a wide separation of components in the configuration space of the entire system—then we call the conditional wavefunction an *effective wavefunction*. Effective wavefunctions correspond to the wavefunctions discussed in quantum textbooks, labs, and so on.[3]

When this distinction is appreciated, we see that the above disanalogies must be approached with caution. Just as offspring can differ from their parents, so too can effective wavefunctions differ from the universal wavefunction. You may be tall and your parents short. Similarly, the universal wavefunction may be simple and the effective one for some subsystem complicated. One may be real and the other complex. One may not vary by system (universal) whereas the other (effective) may vary with system. And perhaps most surprisingly, the universal wavefunction may be time independent and an effective one time-dependent. Many Bohmians have explicitly shown how this might arise in quantum gravity (e.g., Callender and Weingard 1994; Goldstein and Teufel 2001), but the possibility exists even in ordinary non-relativistic theory (Esfeld et al. 2013). Given this distinction, it's not clear that we should infer from the effective wavefunction's variability that the universal wavefunction is variable in *any* way. The universal wavefunction is clearly not variable by subsystem, since it applies to everything, and it's an open question whether the universe as a whole could have different universal wavefunctions. Moreover, the universal wavefunction might not even be time-dependent. The true form of the universal wavefunction is simply a matter of speculation, and therefore, so are the claimed analogies and disanalogies.

## 4 Are laws Its or Bits?

Suppose we accept the view that the quantum state is an aspect of the laws of nature. By itself that perspective doesn't tell us whether the quantum state is It or Bit. Instead it shifts the question from a (roughly) one-hundred-year-old problem to a (roughly) two-hundred-fifty-year-old problem. Since the time of David Hume philosophers and scientists have wondered whether the laws of nature are It or Bit. The nomological

[3] As Goldstein and Zanghì (2013) point out, effective wavefunctions therefore have quasi-nomological status. They are a function over what is nomological, $\Psi_t$, *and* the contingent environment, $Y_t$.

view relocates the question of the metaphysics of the quantum state to the metaphysics of laws of nature.

Although there are scores of theories of laws, a chief division among them is whether laws *govern* reality or are merely particularly useful *summaries* of reality. Governing views understand laws as invisible straightjackets on the world. These straightjackets are part of basic ontology and hence best classified as Its. Humean views, by contrast, understand laws as a particularly powerful summaries of the Its, but not themselves Its. Hence for the Humean they are a special kind of Bit. The It or Bit debate in quantum theory therefore gets subsumed under the It or Bit debate about laws of nature.

Understanding the It or Bit question in this manner helps explain why the debate is so hard. In answering it, we are effectively trying to solve one of the deepest problems in the metaphysics of science. We won't solve it here. Nonetheless, knowing that the problem is deeper than we previously thought is progress, and even better, the views may suggest insights when we compare classical and quantum mechanics later.

### 4.1 Non-Humean quantum states

The governing conception of laws comes in many forms. According to Armstrong–Dretske–Tooley theory, the laws are necessitation relations amongst universals. These necessitation relations are additions to the world of particles, fields, and whatnot. Same goes for theories that understand the laws as a kind of primitive basic entity. These entities are Its (see Carroll 2012 and references therein).

Notice that on the governing view, the 'lost in space' questions reappear. Where do laws live? How do they affect the ontology in spacetime? If real, why no backreaction of the beables on the laws? The puppet-master's ability to move the puppet still seems mysterious. That might prompt the suspicious among us to accuse making the quantum state nomological as simply renaming the problem, not solving it. The original worry was about how the puppet-master moves the puppet, given that the two inhabit different spaces. All that has been accomplished here is that we now call the puppetmaster a law. Big deal.

Although in other contexts I might sympathize with this worry, here I do not. Philosophically, the assimilation is a big deal. Suppose governing is the right way to regard laws. Then the puppet-master question *always* arises. One accepts these awkward questions as the price of providing what is thought to be the best metaphysical theory of laws. The problem, if it is one, arises in classical laws, biological laws, economic laws…everywhere. So from this perspective, a lot is achieved by understanding quantum states as nomological. One realizes that what seemed to be specific problems with interpreting the quantum state emerge as simply the manifestation of these philosophical debates. Two problems become one. Better yet, the one remaining problem is precisely what you were committed to anyway, well before thinking about the reality of the quantum state.

Hence, going nomological is an advance even if one is committed to a governing interpretation of laws. Moreover, since advocates of this view can "place" their governors wherever they like, they may buy some immunity from the awkward puppet-master questions too. That is, there is no reason to think that the primitive governors or necessitation relations “live” in a different space than the beables. To think otherwise is to mistake the mathematical representation of these governors with their physical reality.

### 4.2 Humean quantum states

Hume’s famous skepticism about necessity motivates a quite different picture. Although Hume spoke of causation and not laws, many theories of laws are Humean in spirit. According to these theories, the laws might be elaborate projections of the human mind upon the world, elegant summaries of what happens that are useful for prediction, or other thoughts along these lines. In such theories there may be real observer-independent facts about what the laws are. Just as it’s an observer-independent fact whether a given strike in soccer is a goal—even though the category goal is anthropocentric in origin—so too it might be an observer-independent fact whether a given generalization is part of the most elegant summary of what happens. Yet there is no physical entity that corresponds to these facts. Two worlds containing the same events necessarily have the same laws in them, contrary to what governers believe.

Many theories of laws are Humean in spirit, but probably the most popular one in recent decades is the so-called Mill–Ramsey–Lewis “ Best System” theory (Loewer 1996). This theory says that a true generalization is a law iff it is an axiom of all the best systems, i.e., those axiomatic systematizations that best balance simplicity and comprehensiveness (informative power). Hall (ms) recently formulates the core idea in a way I prefer: (roughly) a proposition is a law iff an ideal observer, someone who is rational and has full information about what is being systematized and embraces our sciences’ standards (which include simplicity and comprehensiveness), declares the proposition a law. Obviously many subtleties arise (see, e.g., Cohen and Callender 2009, and Hall, ms). The details won’t matter here. In the present context the idea is simply that what we’re systematizing are the the beables—Einstein’s peas—in a low-dimensional space. The Schrödinger equation with its wave functions (or equivalently, Heisenberg’s matrices, or Feynmann’s path integrals) then emerge as part of the most elegant summary of how these beables move. The claim is that if an ideal observer could survey all the Bohm particles scattered across spacetime and she cared about our scientists’ standards for evaluating theories, then she would devise the laws of Bohmian mechanics. Or if she systematizes GRW flashes, she would recommend the GRW dynamics. And so on for other primitive ontologies.

This package of beables plus Hume nicely dissolves our “lost in space” problem. Absolutely nowhere in the Humean account of laws is there any hint that the laws devised by the ideal observer must constitute a function (or functional) that is itself decomposable into functions (or functionals) on low dimensional spaces. The laws should be useful, powerful, and true. The inability to express non-trivial wavefunctions on three-space is therefore not a problem, nor even surprising. Furthermore, as part of the laws, the wavefunction is not a beable in the world. It is the result of systematization, not what is systematized. Finally, since “winning” the best system competition is what makes a proposition lawlike, the liberal-minded Humean might be willing to extend the notion of law to novel types of proposition, e.g., initial conditions, time-varying generalizations. If such propositions earn their way into an elegant summary, only an old-fashioned hankering for laws of a classical stripe prevents us from dubbing these propositions laws too.

Whether Einstein would have found such a position attractive is anyone's guess. One can speculate, based on his known appreciation of Hume and fondness for "peas," that he might well have found a position like this digestible.

### 4.3 A Humean trick

An objection to this Humean view immediately suggests itself: the wavefunction doesn't appear to supervene upon the Bohm particle positions. Above I said that two Hume worlds containing the same stuff necessarily have the same laws. Yet we can easily imagine two worlds with the same stuff—the same configuration of Bohm particles—yet described by different wavefunctions. Since wavefunctions are aspects of laws on this position, we seem to have trouble.

Consider a subsystem of the universe consisting of a single particle sitting at a particular location. This situation is compatible with an infinity of possible wavefunctions. Since two are enough to cause trouble, let's concentrate on two subsystem wavefunctions that differ only in their relative phase, $\psi = 1/\sqrt{2}|here > +1/\sqrt{2}|there >$ and $\psi' = 1/\sqrt{2}|here > -1/\sqrt{2}|there >$, where the actual beable configuration corresponds to *here*. Supervenience states that there is no difference in the supervening properties without a difference in the subvenient properties. Here we have a difference in wavefunctions without a corresponding difference in beables.

The key to answering this problem—and the key to avoiding trouble later too- –is to see the world through the eyes of a Humean. To the Humean, the name of the game is devising simple, usable generalizations that will help human agents navigate through the world. Anything that aids this goal is permissible. In particular, one may wish to "add" a magnitude to the system if it will better optimize its virtues. Hall (ms) recently provides a nice example with mass in classical physics. Suppose the fundamental intrinsic property of objects is simply position. Then given the same initial positions, one may not be able to predict—even if one knew everything—what happens next. The physics might be indeterministic. But suppose one "painted" mass onto the objects with position. Perhaps masses are introduced in such a way that they covary with positions in certain systematic ways. Then one can imagine introducing these magnitudes to obtain a greater balance of simplicity and strength. Once mass is introduced, then we have momenta because the position developments will yield a velocity (in a Newtonian formulation). This addition might even allow for a deterministic theory. In this toy example we recover much more strength (e.g., determinism) at the expense of complicating the system by adding mass (Fig. 3). One would be "realist" about the masses, but what makes it true that there are masses is that the best systematization of the distribution of positions in the world that uses masses is optimal. This general strategy is common to Humeans, as they have employed it to understand chance (Lewis 1994), rotation (Callender 2001) and acceleration (Huggett 2006).

Return to our worry about supervenience. Miller (2013) uses the above insight here too. You're an ideal observer looking over the full mosaic of Bohmian particle locations at all times and wondering what wavefunction to use. Is there reason to

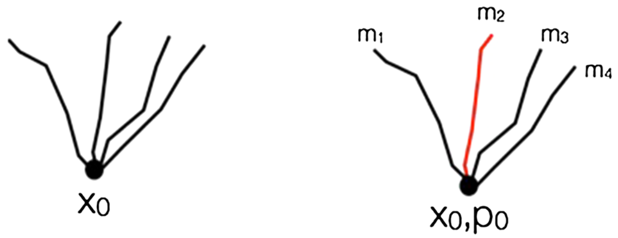


**Fig. 3** Humean "trick": add mass, get uniqueness

posit $\psi$ rather than $\psi'$? Differences in relative phase, of course, are measurable. For example, consider the property measured by the operation associated with operator $\hat{A}$, who yields outcome +1 if the state is $\psi$ and $-1$ if the state is $\psi'$. Then it should be clear, based on the above reasoning, that if the particle is later measured by $\hat{A}$ and the outcome is +1, then that is reason to assign $\psi$ to the system now, all else being equal. Obviously this reasoning can be extended widely—to anywhere where the relative phase "matters" on the whole mosaic. The supervenience, in other words, is global and not local; the wavefunction depends on the full four-dimensional picture, i.e., the particles' full trajectories, not their partial ones.

This is precisely the same as in the case of rotating homogeneous disks (Callender 2001). Non-Humeans have claimed that rotating homogeneous discs are a counter-example to Humeanism. Consider two worlds otherwise empty apart from a single such disc in each. One might be rotating, the other not. Being homogeneous, the Humean cannot appeal to the usual features to tell the difference. Critics of Humeanism consequently say that Humean superveience fails, for the difference between rotating and non-rotating discs doesn't supervene upon the Humean mosaic of basic properties. The right response, I think, is to insist that there is no difference between the two imagined worlds and hence no failure of supervenience. There is only reason to posit a difference when the best system for that world sees a difference. If a speck of dust pops up on one disc one day and rotates, that may be reason to say the disc was rotating all along. The speck doesn't *cause* the rotation; instead it provides a reason to *count* the disc as rotating. Similarly, in the quantum case, later experiments help us determine what counts as having a particular effective wavefunction. One can't look only at the particle subsystem to justify what wavefunction is used. There is nothing mysterious here: these are the very same kinds of reasons quantum physicists use when assigning wavefunctions in the lab, only writ large.

The reader may worry: suppose that the *whole world* is simply a single Bohm particle. That is an allowed solution of Bohmian mechanics. Now we are dealing with a universal wavefunction instead. And there won't be any measurements elsewhere to fix the relative phase because there is nothing else in the world. Hence there is no reason to pick Ψ or Ψ'. The Humean must bite the bullet on this kind of underdetermination of the universal wavefunction. Arguably, this isn't too great a burden to bear. After all, why in the world would an ideal observer surveying such a world even propose $\Psi$ or $\Psi'$ in the first place? With no $\hat{A}$-outcomes (etc.) to cover, a much simpler alternative exists, namely $\Psi'' = |here>$. Perhaps this underdetermination is slightly counterintuitive, but I don't see it as problematic. When there is no reason in the mosaic to pick one wavefunction over another, theoretical virtues will do the rest.

In sum, the core idea in all of these cases is that what makes it true that there is some X in the world (e.g., mass, $\psi$ , rotation, acceleration) is the fact that including X in one's summary of the fundamental makes it the best such summary.

## 5 Classical wavefunctions

My discussion so far has been entirely philosophical. One may rest content with it as it stands, safe in the knowledge that one can dissolve the "lost in space" problem by adopting the nomological interpretation of the wavefunction, and furthermore, that one gets agreeable extra consequences if one adopts a Humean perspective on laws (see below). Yet I believe that insight into the nature of the quantum state is gained by a comparison with the "classical state" in classical physics. There—we presume—we know what is going on, what is beable and what is not. The particles or classical fields are beables, the rest the representational structure of the laws of nature. We don't confuse the two.

The quantum realm looks very different. Here we have a wavefunction evolving in a high-dimensional Hilbert space, inducing a field on a high-dimensional configuration space. These high-dimensional objects "look like" real physical entities, and this raises the "lost in space" question. It may therefore come as a surprise to many philosophers to point out that at this level the situation is not special to quantum mechanics—not in the least. We can easily describe quantum and classical mechanics in the same types of abstract spaces. Quantum mechanics can be described in a Hamilton–Jacobi framework in configuration space, just like classical mechanics; and going the other way, classical mechanics can be formulated in a Hilbert space. In both cases we have classical wavefunctions evolving according to a "Schrödinger equation" in high-dimensional spaces, classical Born's rules, and generally, classical counterparts of most—but of course not all—of what one regards as quantum. We have all the ingredients for a "lost in space" problem. Yet since matters are comparatively clear in classical physics, the problem never arises.

Formulating quantum and classical mechanics in the same abstract frameworks presents us with a wonderful opportunity, the perfect laboratory to investigate the status of the quantum state. In different spaces the comparison is difficult. But in the same formalisms we can look squarely at the differences and see if any mandate a corresponding change in how we treat the wavefunction. Does the step from the classical to the quantum demand a corresponding change in the wavefunction's status, i.e., from nomological to ontological? If so, precisely what features ask for this switch? The best way to investigate this question is to put classical mechanics and quantum mechanics in the same formalism and then probe the differences.[4]

[4] Both theories can be "squashed" together into many abstract formulations, so one has many choices. I'll choose two that I find enlightening, but of course there are many others, e.g., phase space formulations of both theories. I'll count classical statistical mechanics as classical physics. This move is justified in the present context, I believe, by the fact that "beable" interpretations of quantum mechanics often regard quantum mechanics as a kind of statistical mechanics of beables (Dürr et al. 1992).

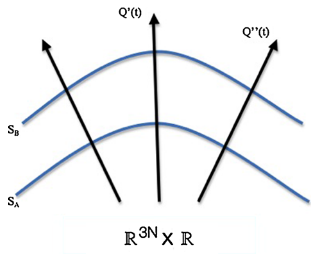


**Fig. 4** Hamilton–Jacobi theory

### 5.1 Hamilton–Jacobi

Hamilton–Jacobi theory is taught in texts of classical analytic mechanics. It is a difficult and philosophically rich version of classical mechanics, one that survives in large part due to the insight it provides into the quantum. The theory is centered on Hamilton's principle function, the action $S(\mathbf{q}, t)$. The action defines wave fronts that evolve with time in the extended configuration space $\mathbb{R}^{3N}$x$\mathbb{R}$. The evolution for a classical particle moving in a potential $V(\mathbf{q}, t)$ is given by a first order partial differential equation

$$\frac{\partial S(\mathbf{q}, t)}{\partial t} + \frac{1}{2}\sum\left(\frac{\partial S(\mathbf{q}, t)}{\partial q_i}\right)^2 + V(\mathbf{q}, t) = 0 \tag{5.1}$$

known as the Hamilton–Jacobi equation. The connection to trajectories is via the vector field on $\mathbb{R}^{3N}$ that $S(\mathbf{q}, t)$ induces:

$$\mathbf{v}(\mathbf{q}, t) = m^{-1}\nabla S(\mathbf{q}, t).$$

The integral curves $Q(t)$ along $\mathbf{v}(\mathbf{q}, t)$ are the possible trajectories of the $N$-particle system, i.e., they solve

$$\frac{d\mathbf{Q}}{dt} = v(\mathbf{Q}(t), t).$$

In essence what we have is a "wave" in the extended high-dimensional configuration space generating the possible trajectories $Q(t)$ (Fig. 4). Initial conditions single out one of them, and we have no problem going from this description to $\mathbf{x}(t) = \mathbf{x}_0 + \mathbf{v}t$ in ordinary space.

Introduce the probability density $\rho(\varphi)$ of finding a particle at $\varphi = (q, p)$ in classical phase space $\Gamma$. Because we assume that the particles are conserved, we can derive

$$\frac{\partial \rho}{\partial t} + \nabla \cdot \left( \rho \frac{\nabla S}{m} \right) = 0 \tag{5.2}$$

which is the well-known continuity equation.

That $S$ defines a wave front suggests consideration of a wave defined as

$$\psi^{(c)} = Re^{i/\hbar S}, \tag{5.3}$$

where $R = \rho^{1/2}$ and $\hbar$ is a constant with units of action. This wave ansatz is our *classical wavefunction*.

Now, using this wavefunction, we can treat $\psi^{(c)}$ and $\psi^{(c)*}$ as new canonical variables such that $(S, -\rho) \rightarrow \left(\psi^{(c)}, \psi^{(c)}*\right)$. Writing the Lagrangian density in terms of these new variables, we can express Eqs. 5.1 and 5.2 together as the "classical Schrödinger equation" derived by Schiller (1962) and Rosen (1964):

$$i\hbar \frac{\partial \psi^{(c)}}{\partial t} = -\frac{\hbar}{2m} \nabla^2 \psi^{(c)} + V\psi^{(c)} + \frac{\hbar^2}{2m} \frac{\nabla^2 |\psi^{(c)}|}{|\psi^{(c)}|} \psi^{(c)}. \tag{5.4}$$

Here $\psi^{(c)}$ is a field, the phase is a solution to the classical Hamilton–Jacobi equation, the normals to $S$ are the classical paths, and $R^2(x, t)$ is the probability density. Note additionally that 5.4 is complex. The "i" arises not for any deep reason, but merely as a result of trying to express two equations, 5.1 and 5.2, as one (Schleich et al. 2013). From 5.1 and 5.2 we derive 5.4, and naturally, the reverse is possible too: by substituting $Re^{iS/\hbar}$ into 5.4, separating into real and imaginary parts, one arrives at 5.1 and 5.2. The classical Schrödinger equation is a kind of compactification of information about an ensemble of conserved classical particles obeying the Hamilton–Jacobi equation. It looks like the ordinary quantum Schrödinger equation except for the odd potential multiplied in the final component of the rhs of 5.4.

Turn to Bohmian mechanics. Although there is a literature surrounding the so-called "quantum Hamilton–Jacobi equation," what I'm about to present is the version appropriate to this paper, namely, a version of Bohmian mechanics via Hamilton–Jacobi (see Holland 1993). As is well-known, Bohm (1952) inserted $\psi = Re^{iS/\hbar}$ into the Schrödinger equation

$$i\hbar \frac{\partial \psi}{\partial t} = -\frac{\hbar}{2m} \nabla^2 \psi + V\psi, \tag{5.5}$$

separated into real and imaginary parts, and derived a "quantum" Hamilton–Jacobi equation and continuity equation, respectively:

$$\frac{\partial S(\mathbf{q}, t)}{\partial t} + \frac{1}{2} \sum \left( \frac{\partial S(\mathbf{q}, t)}{\partial q_i} \right)^2 + V(\mathbf{q}) + Q = 0, \tag{5.6}$$

$$\frac{\partial \rho}{\partial t} + \nabla \times \left( \rho \frac{\nabla S}{m} \right) = 0, \tag{5.7}$$

where $Q$ is the "quantum potential"

$$Q = \frac{\hbar^2}{2m}\frac{\nabla^2|\psi|}{|\psi|}$$

$Q$ is an odd potential, encoding all the peculiarly quantum effects. Otherwise, the physical interpretation is the same as in the classical case: $S$ is a solution of the "Hamilton–Jacobi" equation, 5.7, the solution generates a vector field on configuration space, and the integral curves along this field give the possible trajectories of the $N$-particle system. And just as in the classical case, we can derive a Hamilton–Jacobi equation (5.6) and a continuity equation (5.7) from a Schrödinger equation, (5.5).[5] Although there are plenty of differences (see below), at this coarse level the main one between the classical and the quantum representations is essentially that $Q$ changes sign and equations! Classically it resides in the classical Schrödinger equation (and is multiplied by $\psi_c$) and vanishes in the Hamilton–Jacobi equation; quantum mechanically, it appears in the quantum Hamilton–Jacobi equation and vanishes in the Schrödinger equation. That movement of $Q$ has monumental consequences for the trajectories of beables. Does it also demand that we reify the quantum but not classical wavefunction? Hold this question for a moment.

### 5.2 Koopman–von Neumann waves

One way of stating the "lost in space" problem is by pointing out that the quantum state lives in Hilbert space whereas the beables presumably live in three-dimensional space. Yet classical physics, when framed in Hilbert space, faces precisely the same problem. There is no intrinsic tie between Hilbert space and the quantum. Since work by Koopman and von Neumann in the 1930's (Koopman 1931; Neumann 1932) it's been known that classical physics can be formulated in Hilbert space. In what is known as Koopman's Lemma, Koopman proved that if a dynamical system has a measure $\mu$ on a constant energy surface $\Omega$ of phase space $\Gamma$ and the Hamiltonian flow preserves this measure, then that flow generates a one parameter family of unitary operators $U_t$ on the Hilbert space $\mathcal{L}^2(\Omega, \mu)$. More details are necessary; but the upshot is that since classical mechanics does indeed have such a measure and flow, it can be given a Hilbert space treatment. From there one can develop this Koopmanian approach to mimic quantum mechanics to an astonishing degree.

The goal is to introduce a Hilbert space of complex square integrable functions $\psi(\varphi)$ such that

$$\rho(\varphi) = |\psi(\varphi)|^2 \tag{5.8}$$

can be interpreted as a probability density of finding a particle at the point $\varphi = (q, p)$ of $\Gamma$. We move toward this goal by recalling that in classical statistical mechanics the probability density of particles $\rho(\varphi)$ evolves via the Liouville equation

[5] Note that matters don't work out as simply when we add spin and move away from the Schrödinger equation; see Holland (1993).

$$i\frac{\partial \rho(\varphi)}{\partial t} = \hat{L\rho},$$

where

$$\hat{L} = \left(\frac{\partial H(\varphi)}{\partial y}\right)\left(-i\frac{\partial}{\partial x}\right) - \left(\frac{\partial H(\varphi)}{\partial x}\right)\left(-i\frac{\partial}{\partial y}\right).$$

Here $H(\varphi)$ is the Hamiltonian of the system and we assume a single degree of freedom with canonical variables $x, y$. The Liouvillian operators do not constitute a Hilbert space, so Koopman and von Neumann postulate that complex distributions, $\psi(\varphi)$, which do make up an $\mathcal{L}^2$ Hilbert space, obey the same equation, i.e.

$$i\frac{\partial \psi(\varphi)}{\partial t} = \hat{L}\psi(\varphi). \tag{5.9}$$

Comparing (5.9) with (5.5), one might regard (5.9) as a second classical "Schrödinger equation." Because $\hat{L}$ contains only first order derivatives, the probability density $\rho$ will evolve with the same equation as $\psi$ if 5.8 holds. Making this assumption, Eq. 5.9 implies the classical Liouville equation and shares its empirical content, yet it allows a Hilbert space formulation.

With these ingredients, one can then build an operator formalism for classical mechanics. Koopman and von Neumann define an inner product

$$\langle\psi|\phi\rangle = \int d\varphi \psi^*(\varphi)\phi(\varphi).$$

With this they are able to show that $\hat{L}$ is Hermitian

$$\langle\psi|\hat{L\phi}\rangle = \langle L\hat{\psi}|\phi\rangle$$

and generates unitary evolution

$$\psi(t) = U(t)\psi(0),$$

where $U(t) = e^{i\hat{L}t}$ if the Hamiltonian $H$ is time-independent. That the norm of the state is conserved through time then justifies interpreting 5.9 as the probability density of finding a particle at point $\varphi = (q, p)$ of $\Gamma$. Due to the construction, we know that this will give the same results as the Liouvillian approach to classical statistical physics.

The approach has been developed in many ways. The formalism can be set in either a complex or real Hilbert space (Groenewold 1946; Bracken 2003). The Born's rule analogy can be strengthened (Brumer and Gong 2006). One can rotate to different bases if one likes and find solutions there. We can even describe a "Heisenberg" instead of "Schrödinger" Koopmanian formulation (Jauslin and Sugny 2009).

Perhaps most importantly for us, like the Hamilton–Jacobi framework, this one also can be understood as a "transcendent" formulation over quantum and classical

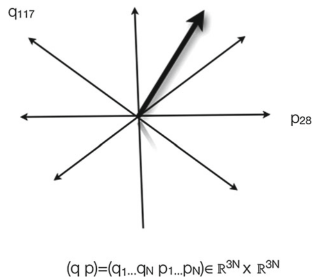


**Fig. 5** Koopman–von Neumann classical physics

mechanics (Bondar et al. 2012). Assume the usual axioms of quantum mechanics but drop the word "quantum": that the states of a system are represented by normalized vectors of a complex Hilbert space and observables by self-adjoint operators on this space, that expectation values $\langle \hat{A}(t) \rangle$ are given by $\langle \psi(t)|\hat{A}(t)|\psi(t) \rangle$, that Born's rule holds, and that the state space of a composite system is given by the tensor product of the system's state spaces. Using these axioms, Bondar et al show that if we assume the position and momentum observables commute, classical physics can be derived, and that if we assume that these observables don't commute, quantum dynamics are derived.

So we find ourselves with a classical formalism in which the states of systems can be represented by the normalized vectors of a complex Hilbert space, where these vectors rotate in Hilbert space, collapsing on states, giving us their probabilities via a classical version of Born's rule. As far as the abstract spaces go, the main difference is simply that the classical Hilbert space is defined via the energy surface in phase space and not configuration space (Fig. 5).

Is the ontology of this theory mysterious? Not at all. We're in friendly territory. Maudlin (2013) asks us to inquire why we posit quantum wavefunctions in the first place. Classically, we know the answer, and the beables are crucial. We arrive at the Liouville equation based on the assumption that the beables—the classical particles—are governed by a set of "guidance" equations, namely, Hamilton's equations. From Hamilton's equations we have a direct route to classical statistical mechanics and then to an operator formalism and classical wavefunctions. The foundation is clear: beables traversing well defined trajectories. There is no temptation to confuse the nomological and the beable. The answer to the counterpart of Maudlin's question here is that the classical wavefunction and counterpart of the Schrödinger equation arise as part of an elegant and condensed representation of the laws governing ensembles of classical beables.

We can imagine being confused classically. Consider a fictional world wherein mathematics developed hundreds of years ahead of physics. Then it would have been in principle possible for a character to exist—call him "von Newton"—that invented an axiomatized operator-based Koopmanian classical physics before Newton came on the scene. The theory would have been empirically adequate. Yet restricted to the classical wavefunction and operator formalism, contemporaries might reasonably have wondered whether the classical wavefunction was It or Bit. Only when Newton introduced his beables—corpuscles with position—and a dynamics for these beables would it have become clear that the wavefunction was only nomological and not itself a beable.

## 6 Difference: $\psi$ as a causal agent

The above discussion shows that *where* the quantum state lives can't be the problem. The quantum and classical wavefunctions live in essentially the same high-dimensional spaces and the beables live in essentially the same low-dimensional spaces. This observation makes pressing the investigation of differences between the two cases. Do any of these differences demand reification of the quantum state?

In either framework, one can discover many differences. Focusing on the Hamilton–Jacobi framework, the differences include:

- $Q$ attaches to the "quantum" Hamilton–Jacobi equation in the quantum case, but to the "classical" Schrödinger equation in the classical case.
- The dynamical equations are linear in quantum mechanics, nonlinear in classical physics.
- $\psi$ is single-valued in quantum mechanics and not single valued in classical mechanics.

Although these differences are of tremendous importance physically, it's hard to imagine promoting any of them into a reason for interpreting the quantum state differently than the classical state. For instance, the dynamics obviously differs over linearity: 5.5 is linear, 5.4 is nonlinear. No doubt, this is a *huge* difference, as every reader knows well. But why should that difference matter to our interpretation of the wavefunction? Physics is filled with linear and nonlinear dynamical equations. We never use that *alone* as a test for whether a function should be interpreted as representing basic ontology or not.[6] Obviously I simplify. The interpretation of the wavefunction should arise from a holistic judgement, one based on all the features of the theory. And we lack an uncontroversial inference of the form: if term X in your theory has feature P, then you must reify X. Still, it's difficult to see why any of the above differences should singly or collectively matter to whether we regard the quantum state as ontological or nomological.

Here is a difference that might matter, however. There is a precise sense in which the wavefunction is forced upon us in the quantum case but not classical case. Holland (1993) glosses this by saying that the wavefunction is not a "causal agent" (Holland 1993) in classical mechanics. By "causal agent" he doesn't mean anything philosophically subtle, but rather merely that something is a causal agent if it's needed to generate the motion of the beables. In classical Hamilton–Jacobi theory, the $S$-function generates a global ensemble. The exact trajectory is therefore independent

[6] Here I stress 'alone' because of course linearity is related to entanglement which is related to the difference I'll mention momentarily. My point is not to de-emphasize the above differences but rather just to point out that more premises are needed to get from these to the reification of the wavefunction. An argument is needed, and one is supplied below. Others based on different formal differences may be possible, but I suspect that they will only reproduce the essence of the 'causal agent' argument below.

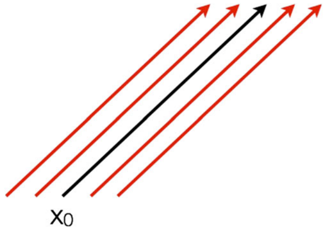


**Fig. 6** Ensemble generated by varying $x_0$

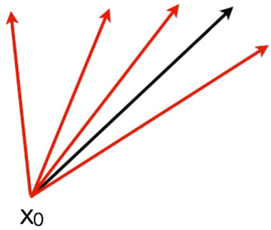


**Fig. 7** Ensemble generated by varying $p_0$

of the wavefunction. Not so in Bohmian mechanics. Here the $S$-function is required to determine the exact trajectory.

Holland provides a pretty example of how the $S$-function is causally inert classically (ibid, p. 37). Consider a free particle evolving according to 5.1 and described with Cartesian coordinates. Solving for $S$, we find $S_1(x, y, z, p_1, p_2, p_3, t) = -(1/2m)(p_1^2 + p_2^2 + p_3^2)t + p_1 x + p_2 y + p_3 z)$., where $p_1, p_2, p_3$ are the components of a momentum vector. Generated by varying $x_0$, $S_1$ describes an ensemble of parallel straight lines (Fig. 6).

where the actual trajectory is colored in black. But we can also construct the function $S_2(x, t; x_0, 0) = (m/2t)(x - x_0)^2$. Generated by instead varying $p_0$, $S_2$ describes an ensemble of parallel straight lines that emanate from $x_0$ with a range of momentum $p$ (Fig. 7).

where again we color the actual trajectory in black. The trajectories generated by $S_1$ and $S_2$ differ in general, but coincide when one picks the same initial location and momentum (the black trajectory). As one sees, the $S$-function doesn't uniquely determine the motion. This is a huge difference between classical and Bohmian mechanics. Its importance cannot be underestimated when comparing the two theories physically. Classically, in some circumstances, it may be convenient to specify a wavefunction, but it is not needed. One can, for instance, obtain a well-posed initial value problem without an $S$-function. In Bohmian mechanics it is needed. One can't get a well-posed initial value problem without it.

Essentially the same point can be made in the Koopman–von Neumann formalism too. Substitute $\psi = Re^{iS/h}$ into the Koopmanian "Schrödinger equation" 5.9 and separate real and imaginary parts. The result here, in contrast to the quantum case, are two structurally identical equations, one for the modulus and one for the phase, with no "mixed" terms, i.e., the phase and modulus don't couple. The physicist Gozzi, quoted by Mauro (2002), states that in this formalism, the “essence” of quantum mechanics is that it is “the theory of the interaction of a phase with a modulus.” As in the Hamilton–Jacobi case, this coupling makes the phase necessary in quantum mechanics in a way it isn’t classically. The general lesson is that while in either quantum or classical formalisms we *can* construct wavefunctions evolving according to “Schrödinger” equations, only in quantum mechanics is something like this forced upon us.

This observation seems relevant to the current investigation. The suggestion was that the classical *S*-function is not part of the ontology but is instead part of the nomological structure. That inference seems fine, and even bolstered, by what we’ve learned, namely, that S doesn’t determine the beables’ motion. But the further suggestion that the *S*-function in the quantum case should be treated like the *S*-function in the classical case now seems deeply problematic. One *S*-function is there for convenience, the other by necessity. While admitting that there are no hard and fast philosophical rules in play, that sounds like a relevant difference, one demanding different interpretations of the classical and Bohmian *S*-functions.

### 6.1 Escape

The fact that the wavefunction is a causal agent in the above sense is the reason why most Bohmians have agreed with Bell that it must be treated ontologically. Putting classical mechanics and Bohmian mechanics in the same formalism allows us to appreciate the stark differences between the two, differences that seem relevant to whether ontology stands behind their respective wavefunctions or not. It is therefore perhaps fair to conclude that the natural or even default interpretation of the wavefunction for a Bohmian is that it is ontological.

If one is a Humean about laws, however, one may not “see” this difference. As we have seen, Humeans routinely posit magnitudes if they are justified by optimizing one’s summary of what one is systematizing. Suppose one is systematizing Bohmian particles with position. Without an *S*-function, we lack a well-posed initial value problem, and therefore potential strength and power. We can’t tell where such a particle will go without this *S*-function. Does that mean the *S*-function is a beable? No, no more so than requiring mass to get a well-posed value problem classically demands that Humeans treat mass as part of the fundamental furniture of the world. Or perhaps a better analogy in the present case, a Humean might justify the postulation of forces as a way of getting the best systematization of the beables without treating forces as themselves beables. Knowing this, the Humean may not be moved by the above “causal agent” argument. He or she will recognize many cases where they would disagree with someone running that form of argument: rotating discs (Callender), mass (Hall), perhaps even accelerations (Huggett). Indeed, as mentioned, Miller (2013) makes precisely this point about the wavefunction in an attempt to understand how it supervenes upon Bohmian particle positions. So Humeans already have independently motivated reasons for not following the “causal agent” argument to its conclusion.

The Bohmian is therefore free to regard the *S*-function or equivalent as he or she would a Newtonian force: nomological, not beable. The same goes for non-Bohmians who posit beables (for whom I assume some version of the causal agent argument exists too). The Humean convinced that she is systematizing Einstein's peas in a low-dimensional space doesn't feel the force of the causal agent argument. With that, beable theorists can enjoy all the attractions of the otherwise parsimonious nomological view of the wavefunction.

To be clear, I have not argued that *only* Humeans can escape the causal agent argument, nor have I argued that Humeans *should* escape it. On the first point, it may be that advocates of a governing conception of laws can motivate ignoring the lesson of the causal agent argument too. On the second point, how the Humean divines the divide between what goes into systematization and what comes out is anyone's guess. I am simply pointing out that for those who wish to systematize Einstein's peas, there is a clear, consistent, well-motivated position that does not include reification of high-dimensional entities such as the wavefunction field on configuration space.

## 7 Lessons

Before concluding, let me point out three implications of this sparse quantum metaphysics.

The first lesson is that some objections to beable-type theories vanish if the wavefunction is understood nomologically. Call these objections the Redundancy Argument and the Action–Reaction Argument. Put forward by Everettians, the Redundancy Argument originates in Everett's doctoral thesis (Everett 1957) and has been repeated ever since.[7] Here is Everett:

> Our main criticism of this view [Bohm's theory] is on the grounds of simplicity—if one desires to hold the view that $\psi$ is a real field then the associated particle is superfluous since, as we have endeavored to illustrate, the pure wave theory is itself satisfactory. (1973, p. 112)

Consider the infamous case of Schrödinger's cat. Put simply, the objection is that Bohm's theory solves the measurement problem by adding one too many cats. This objection may seem a bit rich coming from the Everettian, who already posits a multiplicity of cats, yet there is a point here. Interpreted *a la* Everett, the final state of Schrödinger's cat is an uncollapsed wavefunction with a branch corresponding to a dead cat plus another branch corresponding to a live cat. The Bohmian by contrast insists that there is only one cat, a particle cat, either alive or dead. Suppose the particle cat is alive. Grant that Everett solves the measurement problem and adopt the traditional Bohm picture wherein the wavefunction is part of basic ontology. Then the Everettian can point out that we have one too many live cats: a "wavefunction" live cat and a "particle" live cat. More generally, before adding the particles, the traditional Bohmian posits just as much ontology as the Everettian. If Everett gets you an honest-to-goodness cat, why do you need the particle cat? Bohmians, as Deutsch (1996) puts it, are Everettians in "chronic denial." The argument concludes with the suggestion to take Occam's razor to this bloated ontology and excise the superfluous particles. The

[7] See Brown and Wallace (2005), Deutsch (1996), Wallace (2008) and Zeh (1999).

Action–Reaction Argument, by contrast, doesn't rely on Everett. Here the complaint is that Bohmian mechanics—and theories like it—violate the "action–reaction" principle. The field in configuration space acts on the particle configuration, but not vice versa (Squires 1994; Anandan and Brown 1995). Although there have been attempts to address this issue in Bohmian mechanics, in its standard form the charge is certainly correct. The objection carries force in proportion to how much one believes a theory ought to obey this principle.

Neither objection moves me. To Action–Reaction, I think that it's fair to respond that the evidence for this principle is classical and that quantum theories shouldn't be hostage to classical intuitions (Callender and Weingard 1997). To Redundancy, it's up for grabs whether Everett solves the measurement problem, and anyway, the functional specification of a wavefunction cat is distinct from that of a particle cat (see Lewis 2007a, b; Valentini 2012, Callender, ms). Here, however, I simply wish to register that on the nomological understanding of the wavefunction neither objection gets off the ground. Action–Reaction has no more merit than one objecting to classical Hamiltonian mechanics because the particle positions don't act back on the symplectic structure. And Redundancy loses anything redundant: there is no wavefunction-cat. Far from being an Everettian in self-denial, the Bohmian imagined here is instead a full-throated anti-Everettian, for the two theories posit no shared basic ontology.

The second lesson is that it's *possible* to regard classical mechanics and quantum mechanics as positing the same exact ontology but differing only in the laws. Interpret quantum mechanics in a Bohmian fashion and choose position as the sole beable. One can do exactly the same classically, adopting a "Bohmian" interpretation of Newtonian mechanics and choosing position as the sole beable of Newtonian mechanics. Both theories are then about a spare ontology of entities possessing only position intrinsically. Arguably this "Bohmian" understanding of Newtonian mechnaics is the most natural one. After all, classical mass and charge are theoretical terms. It's natural for a Humean to treat these as features of the nomological too (Hall, ms). We tend to picture Newtonian corpuscles with mass and charge stuck on them, like hats on a peg, but it's perfectly reasonable to understand them as features of the laws.

I stress that I say it's possible, not necessary, to view the basic ontology as unchanged in moving from the classical to the quantum (even understood a la Bohm). We can easily generate mismatching ontologies allowing the Bohmian to pick fermion number density, for instance, as her preferred observable, or the Newtonian to pick velocities. One can also opt for a less minimalist interpretation of either theory, sticking spin or other properties a la Holland on Bohmian particles and mass, charge or other properties upon Newtonian corpuscles. A referee points out an interesting possible asymmetry here. While one can regard mass and charge are intrinsic properties "stuck" onto the Newtonian corpuscles, such an interpretation of mass and charge may be unavailable to the Bohmian (see Brown et al. 1995 and Brown et al. 1996 for some reasons why).

The third lesson is a small refinement of our understanding of what the recent theorem of Pusey et al. (2012) precludes. Let's begin with some terminology. Suppose one considers theories that describe reality with some variables, $\lambda$. Values of $\lambda$ correspond to particular states of reality, or ontic states. When we prepare a system in quantum mechanics, that corresponds to a distribution over these ontic states. The literature surrounding the PBR theorem is interested in the following question: can we prove that pure quantum states must correspond to non-overlapping distributions over ontic states (Hardy 2012)? The reason for interest in this question is perhaps best explained as follows. Consider the state of a system in classical statistical mechanics, the state given by the probability density $\rho$. Clearly, many particular states in phase space (infinitely many) are compatible with one and the same $\rho$. Given $X \in \Gamma$, we cannot deduce a unique $\rho$ (not even close!), so $\rho$ is unlike, say, the energy, which can be so deduced. This fact, the thinking goes, allows $\rho$ to be interpreted epistemically, unlike the energy. Now let $\lambda$ be the quantum counterpart of $X$ and $\psi$ the counterpart of $\rho$. Can one deduce from $\lambda$ the pure quantum state? One can only do that if pure quantum states correspond to non-overlapping distributions over ontic states. Hence the interest of the question: the answer tells us whether the quantum state is like the classical $\rho$, and hence epistemic, or like the classical energy, and hence ontological. Using terminology from Harrigan and Spekkens (2010), call a model *$\psi$-epistemic* just in case there exist pairs of pure quantum states for which the distributions over ontic states overlap and call a model *$\psi$-ontic* models just in case the distributions over ontic states are non-overlapping for any pair of pure quantum states. Then what the PBR theorem shows is that if we assume a condition on separability, the wavefunction is $\psi$-ontic. Being $\psi$-ontic is typically understood as implying that "the quantum state would be written into the underlying reality of the world and we could assert that the quantum state is real" (Hardy 2012).

An instrumentalist or anti-realist doesn't commit to variables $\lambda$. Since PBR requires assumptions on $\lambda$, that brand of epistemicism about the wavefunction is not impugned. Hence it's commonly thought (Leifer 2011) that three positions remain:

1. Psi-epistemic (realist): Wavefunctions are epistemic, and there is some underlying ontic state
2. Psi-epistemic (anti-realist): Wavefunctions are epistemic, but there is no deeper underlying reality
3. Psi-ontologist (realist): Wavefunctions are ontic

and that PBR rules out interpretations in class 1.

Position 1 sounds a lot like the position defended here. Our Bohmian is committed to beables, and hence $\lambda$, but has a Humean, and therefore Bit-like, interpretation of the wavefunction. How is this possible?

There is no mystery here, only a looseness of semantics. As defined above, Bohm's theory is a $\psi$-ontic theory. The reason for this is related to our discussion in Sect. 6: one needs the wavefunction to get the theory's predictions. Hence the ontic states in Bohm's theory are given by $\lambda = (\psi, Q)$, where $Q$ is the point in configuration space representing the particle configuration. The theory is trivially $\psi$-ontic, for sitting in $\lambda$ is none other than the quantum state itself.

The key point, however, is that *not all $\psi$-ontologists believe that $\psi$ is part of the ontology*. We can distinguish between the metaphysics of the two components of $\lambda$. That $\psi$ is part of the ontic state doesn't mean that it needs to be interpreted like $Q$. This point is even clear in classical mechanics. There we think of $\lambda = (X(q, p))$. But in a Newtonian framework, momentum is defined via the forces. So the ontic state really requires the forces too. The state of a system doesn't supervene merely on the positions (consider: a particle at an instant going to the right and a particle at an instant going to the left looik the same positionally). The forces or their equivalents are needed. Yet that doesn't preclude a philosophical analysis wherein one adopts, say, a Machian understanding of force and otherwise treats the positions as beables. Same here. Hence there is at least a fourth position available:

4. Psi-ontologist: wavefunctions are ontic, and there is some underlying ontic state, but the wavefunctions are not beables.

Position 2 is the "philosophical" variant of 1. It agrees with 1 that the wavefunction is not part of the ontic state, but in disagrees with 1 in its "metaphysics," i.e., it denies that $\lambda$ exist and thereby escapes the proof. In a roughly similar way, we might view 4 as the "philosophical" counterpart of 3. Position 4 agrees with 3 that the wavefunction is part of the ontic state, but it disagrees with 3 in its "metaphysics," i.e., it denies that $\psi$ exists ontologically. If we agree to tease out 4 from 3 as described, then we see that Position 3 is what is appropriate when one's beable is the wavefunction (Everett) and Position 4 is what is appropriate when we choose a non-wavefunction beable.

## 8 Conclusion

We have motivated the nomological understanding of the wavefunction. On this picture the It-or-Bit debate, and even the wave-or-particle debate, are absorbed by a larger more philosophical debate over the nature of laws. Although slightly disconcerting, this realization represents progress. With this understanding in hand, I've shown how a major obstacle to the nomological view potentially is removed by adopting a Humean perspective on this larger dispute. According to this attractively parsimonious picture, it turns out to be *possible* to think that the fundamental ontology of the world posited by non-relativistic quantum mechanics is precisely the same as that posited by classical physics. The difference lies only in the dramatic alteration of the laws governing how these beables move. Regardless of whether one explores this option, the main point is that quantum mechanics requires only one type of beable. Put in the form of a rhyming slogan, the moral is "one stuff, that's enough."

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