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# The emergence and interpretation of probability in Bohmian mechanics

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**Abstract**

A persistent question about the deBroglie–Bohm interpretation of quantum mechanics concerns the understanding of Born's rule in the theory. Where do the quantum mechanical probabilities come from? How are they to be interpreted? These are the problems of emergence and interpretation. In more than 50 years no consensus regarding the answers has been achieved. Indeed, mirroring the foundational disputes in statistical mechanics, the answers to each question are surprisingly diverse. This paper is an opinionated survey of this literature. While acknowledging the pros and cons of various positions, it defends particular answers to how the probabilities emerge from Bohmian mechanics and how they ought to be interpreted.


*Keywords:* Debroglie; Bohm; Probability; Statistical mechanics; Born's rule

## 1. Bohmian mechanics and the distribution postulate

The Bohm interpretation of quantum mechanics is capable of illustrating virtually every philosophical and foundational conundrum associated with physical probability. One reason this is true is that the Bohm interpretation comes in many forms, both stochastic and deterministic. The other reason is that quantum mechanics is to Bohmian mechanics roughly as statistical mechanics is to classical mechanics; hence the notorious problems in the foundations of statistical mechanics are reprised within the foundations of Bohmian mechanics. The present paper is an opinionated survey of this literature. In it I focus on the

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meaning of Born's rule in a Bohmian universe. After considering various rationales for this rule, I settle on one based on the law of large numbers as the best bet. But this option leaves open how the probabilities in this result ought to be interpreted. When delivering an interpretation of these probabilities, the history of probability warns us of a number of pitfalls. I show how these pitfalls manifest themselves in Bohmian mechanics and then argue that at least one class of interpretations seems to successfully maneuver around them. Before getting to these issues, let us briefly review Bohmian mechanics and its understanding of Born's rule.

Bohmian mechanics is an empirically adequate and logically consistent quantum theory of non-relativistic phenomena. To my mind it provides the most natural answer to the notorious measurement problem. The measurement problem tells us that the wave function evolving according to the Schrödinger equation either cannot be correct or it cannot be the whole story. Collapse theories answer the problem by denying the evolution is always via the Schrödinger equation. Bohmian theories answer by denying the completeness of the wavefunction description of the world—in the non-relativistic theory there are also particles. Originally devised by de Broglie in 1927, it was subsequently rediscovered and improved by Bohm in 1952. More recently, many researchers have developed the approach further, especially the group of Dürr, Goldstein and Zanghi (DGZ).[1] For the purposes of this article, we shall limit ourselves to a brief sketch of the non-relativistic theory. The reader is encouraged to turn to the references mentioned in footnote 1 for treatments of spin, identical particles, quantum field theory, and more.

Bohmian mechanics is a theory of $N$ point particles evolving in $\mathbb{R}^3$. Each particle (indexed by $i$) obeys a first-order equation of motion:

$$\frac{\mathrm{d}x_i}{\mathrm{d}t} = \frac{\hbar}{m_i}\mathrm{Im}\frac{\nabla_i\psi(x_1(t)\dots x_N(t))}{\psi(x_1(t)\dots x_N(t))}, \tag{1}$$

where 'Im' means imaginary part, $m$ is the mass of the particle, and $\Psi$ is a time-dependent, complex-valued function on configuration space $\mathbb{R}^{3N}$. The wave function $\Psi$ evolves according to the familiar Schrödinger equation:

$$ih\frac{\partial\psi}{\partial t} = -\sum_{i=1}^{N}\frac{\hbar^2}{2m_i}\nabla_i\cdot\nabla_i\psi + V(q_1\dots q_N)\psi, \tag{2}$$

where $V$ is the potential energy. Here $q_i$ are the variables in $\mathbb{R}^{3N}$ upon which $\Psi$ depends. The system given by (1) and (2) describes a deterministic theory of the universe, one for which global existence and uniqueness can be proved for almost all initial conditions. Relevant to us later, the theory is also time-reversal invariant and (for most states) Poincaré recurrent. That is, for every history there is also the time-reversed history given by the transformations $t\rightarrow -t$ and $\Psi\rightarrow\Psi^*$; and for every initial condition $(x_1(0)\dots x_N(0), \Psi(0))$, the time development by (1) and (2) will eventually take the system arbitrarily close to this state again.

The theory is perfectly deterministic. Yet the empirical heart of quantum mechanics is the fact that subsystems of the universe (for instance, actual ensembles measured in labs) are probabilistically distributed according to Born's rule. The probability density for finding particles in configuration $q\equiv(q_{1\dots}q_N)$ at time $t$ in volume $d^{3N}q$, i.e., $\rho(q,t)$, is equal

[1]For a selection of their work and links to other Bohmian groups, the reader can visit www.bohmian-mechanics.net.

to $|\Psi(q,t)|^2 d^{3N}q$. To be as successful as ordinary quantum mechanics, Bohmian mechanics must provide a reason why betting in accord with Born's rule is a rational strategy. Put more physically, the Bohmian must explain why particles upon measurement actually will be distributed according to $|\Psi(q,t)|^2$. What is the origin of Born's rule?

Care needs to be taken in understanding Born's rule in a Bohmian world. The wavefunction $\Psi$ in (2) is the wavefunction of the universe, whereas the wavefunction empirically tested by Born's rule is a function of a subsystem of the universe. This distinction is important for what follows. Fortunately, the Bohmian can speak of a subsystem or *effective wavefunction* $\psi$ when—roughly—enough "decoherence" has occurred between the system and its environment (see DGZ 1992).[2] The Bohmian will thus understand Born's rule in terms of $\psi$, not $\Psi$. Since position measurements reveal the positions of actual particles for the Bohmian, the success of Born's rule implies that the Bohmian particle positions of measured subsystems of the universe be randomly distributed according to $|\psi(q,t)|^2$. Thus, the Bohmian must assume, derive or otherwise make plausible the claim that the position probability density is given by

$$\rho(q,t) = |\psi(q,t)|^2, \tag{3}$$

whenever a measurement is made. Let us call (3) the *distribution postulate*(Barrett, 1995). With (3) the Bohmian can explain the occurrence of frequencies in accord with Born's rule and all that follows from that, e.g., why quantum mechanics works, why the uncertainty principle holds, why the 'no superluminal signaling' theorem obtains, and more. Without (3) or something close to (3), the Bohmian runs a serious explanatory deficit.

The project of justifying (3) is often advertised as being very simple due to the following mathematical fact. The Schrödinger equation implies a continuity equation for $|\Psi|^2$. If we substitute $\rho(q,t) = |\Psi\ (q,t)|^2$ into the continuity equation then we get

$$\frac{\partial\rho}{\partial t} + \frac{\partial(\rho\dot{x})}{\partial x} = 0. \tag{4}$$

Eq. (4) will take any initial distribution of particles such that $\rho(q,0) = |\Psi(q,0)|^2$ holds and evolve it in time $t$ to the distribution $\rho(q,t) = |\Psi(q,t)|^2$. The dynamics, in other words, preserves the probability distribution $\rho = |\Psi|^2$. The project of justifying (3) is then presented as simply the problem of finding some reason to think $\rho(q,t) = |\Psi(q,t)|^2$ at some time or other, for one is then guaranteed that it will always hold.

However, this way of framing matters is misleading.[3] What we want to be explained is why a system of particles should have a probability density given by the effective wavefunction, not the universal wavefunction. The effective wavefunction is clearly the relevant object in Bohmian mechanics, for it is the object corresponding to the wavefunction in the usual use of Born's rule within orthodox quantum measurement

[2]Consider a subsystem of a larger system. The subsystem (environment) will have an actual configuration $x \equiv (x_1 \ldots x_N)$ ($y \equiv (y_1 \ldots y_N)$) and a range of possible configurations $q_x$ ($q_y$) in configuration space. The total system is governed by the wavefunction $\Psi(q) = \Psi(q_x,q_y)$. However, we can define a wavefunction for the subsystem by conditionalizing on the actual configuration of the environment, $\psi_t(q_x) = \Psi_t(q_x,y_t)$. This conditional wavefunction will evolve according to the Schrödinger equation when the environment and subsystem do not interact. And more generally, when the universal wavefunction evolves into a wide separation of components in the configuration space of the entire system—see Dürr et al. (1992) for details—we can define an *effective wavefunction* that corresponds to the usual wavefunction in quantum mechanics.

[3]Thanks go to Tim Maudlin and Sheldon Goldstein for emphasizing this point.

theory. Yet the effective wavefunction does not always obtain. It evolves according to the Schrödinger equation when it exists, but the conditions for its existence do not always hold. So we cannot make use of the above guarantee. That the dynamics preserves $\rho = |\Psi|^2$ does not imply that the dynamics preserves $\rho(q,t) = |\psi(q,t)|^2$. The preservation of $\rho = |\Psi|^2$ is relevant to (3) obtaining, as we will see, but it is not sufficient.

## 2. The analogy with classical statistical mechanics

Einstein famously wanted the statistical aspects of quantum theory to take "an approximately analogous position to the statistical mechanics within the framework of classical mechanics" (1949, p. 672). Since Bohmian mechanics is a deterministic and more fundamental theory than quantum mechanics, it is in the perfect position to realize this goal. Let us see how.

Classical mechanics is also a theory of $N$ point particles evolving on $\mathbb{R}^3$. In contrast to Bohmian mechanics, wherein one requires an initial wavefunction and initial locations, here one requires an initial position and initial momentum. Given these values, Hamilton's two equations, the counterparts of (1) and (2), will determine a trajectory for an individual system. Like Bohmian dynamics, Hamilton's equations are also deterministic and time-reversal invariant.

For an ensemble of particles evolving according to the same Hamiltonian, Hamilton's equations define a velocity field on phase space. This field evolves a distribution $\rho_c(x,p,t)$ via a continuity equation

$$\frac{\partial \rho_c}{\partial t} + \frac{\partial(\rho_c \dot{q})}{\partial q} + \frac{\partial(\rho_c \dot{p})}{\partial p} = 0, \tag{5}$$

that may be compared with (4). And just as $\rho_c(x,p,t)$ moves through phase space as an incompressible fluid due to Liouville's theorem, so does the distribution $f(q,t) = \rho(q,t)/|\psi|^2$ in Bohmian mechanics.

Note that in the classical case the uniform distribution is preserved by the dynamics, just as (3) is by the Bohmian dynamics. That is, (5) will evolve $\rho_c(x,p,0)$ = constant in time $t$ to $\rho_c(x,p,t)$ = constant. Since one usually thinks of $\rho_c(x,p,t)$ = constant on the energy surface in phase space as the signature of thermal equilibrium, classical dynamics leaves thermal equilibrium invariant. For this reason, advocates of Bohmian mechanics often regard the $\rho = |\psi|^2$ distribution as *quantum equilibrium* for it too is preserved by the dynamics. The main difference is that thermal equilibrium is stationary whereas quantum equilibrium is time dependent. Another disanalogy is that for the Bohmian, the universe is in the quantum "heat death", whereas we are not yet in the thermodynamic heat death.

Finally, observe that quantum equilibrium and thermodynamic equilibrium are independent of one another. One can be in the first without the second, and the second without the first. Thermodynamic equilibrium is a feature of the wave function of the universe.

## 3. Why justify the distribution postulate?

The status of the distribution postulate has been controversial since Bohm announced his theory. Pauli (1953) and Keller (1953) soon objected to simply stipulating (3). They wanted Bohmian mechanics to work with *any* initial probability distribution; that is, they wanted (3) more or less derived from (1) and (2). Bohm in fact never simply stipulated (3): Bohm (1952, II.7) mentioned that the effects of collisions and other random processes would be to cause any differences between $\rho(x)$ and $|\psi|^2$ to decay with time, and in 1953 he wrote a paper directly on this topic. Neither result succeeds in rigorously showing that any initial probability distribution will work. As a result, for more than 50 years the distribution postulate has been the subject of scrutiny, with many Bohmians attempting to show that the postulate has essentially the same justification as the microcanonical probability measure does in statistical mechanics.

But what is the justification of the microcanonical probability measure in classical statistical mechanics? Few questions in all of the foundations of physics are more vexed and yield such a diversity of answers. Some believe the thermodynamic limit solves the foundational puzzles, others environmental perturbations, others symmetries and ignorance, others mixing dynamics, and others the H-theorem. The answers these theories give are sometimes no more similar than chalk and cheese. Faced with this foundational bedlam, we see that criticisms like Pauli's and Keller's are problematic: one certainly cannot do uncontroversially for the microcanonical probability distribution what they want done for (3), yet presumably neither recommend the rejection of classical statistical mechanics. Today, most Bohmians agree that the distribution postulate has roughly the same justification as the microcanonical probability posit—so long as no one asks what that justification is! If one asks this question, foundational bedlam arises again, this time at the sub-quantum level.

Before delving into the topic further, let us step back and ask *why* the distribution postulate is so needy of justification. Why cannot the theory posit three axioms? Let us call the position that treats (1)–(3) on a par as axioms of the theory the *Default Position*. According to it, we would not ask for a justification of (3) any more than we would of (1). From the Bohmian perspective, there is as much evidence for (3) as for (1) and (2). They are a package deal. So why do few hold this position?[4]

I do not believe that there are any knockdown arguments against the Default Position. But there are a number of considerations that, on balance, lead most to want to do better. The most obvious and easily justifiable rationale for distinguishing (3) arises from parsimony. Two axioms are fewer than three. Despite Nelson's (1985) attempts, there is little prospect of deriving (2) from the rest, and eliminating (1) abandons Bohmian mechanics; hence (3) becomes a natural target.

Some also may feel a philosophical motivation for targeting (3). There is a widespread intuition that *real* laws of nature are dynamical. Real laws *govern* particles, fields, etc., and governing is something that happens in time. Dynamical laws are first-class citizens of the theory, whereas non-dynamical posits are second-class—lacking the right to delimit what is physically possible. If one has this view, (3) does not get to be a law, and so (3) then demands an explanation for it is responsible for such striking quantum mechanical generalizations. Of course, one can challenge this rationale. Physics seems to posit plenty of non-dynamical laws, e.g., the Pauli exclusion principle. And from the perspective of some accounts of laws of nature, the idea that laws must govern temporal evolution is unnatural. If a statement is simple and strong enough that may qualify it as a law of

[4]Bricmont ends his paper recommending that we treat (3) as an axiom (Bricmont, 2001; Bricmont et al., 2001). See also DGZ 1992, Section 4.

nature. There are plenty of perspectives on laws of nature that would permit elevating (3) to first class citizenship.

Perhaps the special nature of (3) makes it needy of justification? On one side of (3) we have what is supposed to be a real field in the world and on the other side we have a probability distribution. Intuitively, (3) links very different sorts of entities, it seems, no matter how one interprets probability. But again, we can imagine responses. What is it for one entity to be 'a very different kind' than another? Are not there plenty of equations in physics linking quite different objects? Besides, some Bohmians do not think of $\psi$ as a real field—at least not in the same sense as the electromagnetic field. The link is then between two objects, neither of whom is clearly understood. It seems premature to insist these sorts of entities, whatever they are, require special justification when linked.

The Default Position seems defensible. The question is then only whether we can do better. This question immediately leads to another: what would "doing better" be? We can imagine a range of possible relationships between (3) and (1) and (2). The weakest would be pointing out that *there exists one* initial condition such that it leads to the Born frequencies for subsystems of the universe. But such a demonstration is not enough—not, as Dürr (2001, p. 128) says, if we want to *explain* why (3) holds. Physics seems like it is shirking its duty if it merely proclaims that there is an initial condition whose evolution leads to what you will see. For some events such an account is permitted. The Chicago Bulls won three basketball championships in a row, yet in "most" solutions to the fundamental equations of motion they never won and never even existed. Here mother intuition does not cry out for a special sort of explanation. The universe began in a state such that subsequent evolution takes us to a Bulls "three-peat", and that is that. Boston Celtics fans can bemoan their fate at living in such a universe, but most would not also complain that physics has sold us short explanatorily. Yet people would complain if Bohmian mechanics explained the quantum regularities the way we just explained the Bulls' feat. The difference stems from the fact that the regularities of quantum mechanics form some of the most striking and pervasive patterns in our universe. To chalk them entirely up to the "chance" first microstate of the universe flies in the face of the fact that we think the quantum generalizations are a stable feature of our world.

The optimal situation would be if we could show that for *every* initial condition the distribution given by (3) held after a certain length of time. However, this is *provably impossible*. There are plenty of perfectly acceptable wavefunctions that are not equilibrium wavefunctions and will not evolve into such. Real wavefunctions for which (3) does not hold are one obvious example (because the particles then have no dynamics according to (1)).

That leaves us with the possibility that some, or better, *most* initial conditions yield (3) for subsystems. If (3) holds for most ways the universe could have been, the mystery over its occurrence is lessened. It is to such attempts that we now turn.

## 4. Dynamical approaches I

The guiding thought behind this approach is that the universe might have begun in quantum nonequilibrium and yet nevertheless evolved into quantum equilibrium. The Bohmian dynamics is supposed to "force" nonequilibrium initial states into equilibrium. Most of the work done on this approach predates the distinction between universal and effective wavefunctions; for the moment, therefore, let us pretend (3) refers to the universal wavefunction, not the effective wavefunction. So understood the desired transition is, strictly speaking, impossible. The conservation Eq. (4) implies that a distribution in nonequilibrium can never get into equilibrium. However, that does not mean that initial distributions such that $\rho \neq |\Psi|^2$ will not in time approach a distribution *observationally indistinguishable* from $\rho = |\Psi|^2$. It is compatible with the dynamics that various distributions initially such that $\rho \neq |\Psi|^2$ nonetheless evolve into $\rho = |\Psi|^2 \pm \varepsilon$, where $\varepsilon$ is within the range of experimental/observational error. In an idea familiar from Gibbs, given a suitable coarse-graining of $\rho$, $\rho$ may "look" like it is in equilibrium for all intents and purposes.

Bohm (1953) initiated this approach, showing that an ensemble of two-level molecules out of equilibrium, when subjected to random perturbations, would approach the equilibrium distribution. Potel, Muñoz-Aleñar, Barranco, and Vigezzi (2002) do something similar. Neither gives a general argument, and it is not clear how to interpret these random perturbations (see Section 6). But others—most prominently Antony Valentini—have pursued this line of inquiry in more detail without adding random perturbations. Valentini argues that a coarse-grained distribution $\overline{\rho_0}$ of configuration variables will relax to coarse-grained equilibrium $\overline{|\psi(q)|^2}$.[5] Recall Gibbs' famous "ink drop" analogy: when the ink and water are stirred, after a time the system will look homogeneous from a coarse-grained perspective. In the present case, $\rho$ and $\Psi$ now play the role of ink and water. The hope is that for complex systems the dynamics will mix the two so that they tend to become indistinguishable at a certain scale. If shown, we could dispense with our need for (3) because coarse-grained equilibrium would hold now no matter the initial state of the universe.

What can be shown? Valentini (1991) and then Valentini and Westman (2005) do essentially two things. First, Valentini shows that an "H-theorem" holds for Bohmian mechanics. The H-theorem is the Bohmian counterpart of Gibbs-Tolman's famous coarse-grained H-theorem (see, e.g., Tolman, 1938). Defining a "sub-quantum entropy" $S$ in terms of a coarse-grained distribution, he argues that $S$ will attain its maximum value over time. Second, Valentini and Westman show how simulations of some nonequilibrium systems evolve to coarse-grained equilibrium. For these systems they also improve on the H-theorem by obtaining some estimates for the time it takes for relaxation to equilibrium to occur.

Valentini and Westman interpret their results as showing that (3) should not be an axiom, but rather, it "should indeed be seen as dynamically generated, in the same sense that one usually regards thermal equilibrium as arising from a process of relaxation based on some underlying dynamics" (p. 255).

Valentini's H-theorem is appealing. By copying one justification of thermal equilibrium, the Bohmian has more ammunition against the position of Pauli and Keller. But there are some reasons to be dissatisfied with Valentini's H-theorem as the ultimate justification of (3). Let me mention two that also arise with Tolman's original coarse-grained H-theorem, and then one more specifically Bohmian worry. First, one must assume that the initial coarse-grained distribution $\overline{\rho_0}$ is equal to the initial fine-grained distribution—what Valentini calls the condition of "no fine-grained microstructure." Should we accept this condition? Given the context, it's not clear that this is a terrific gain. Is it so obvious that

[5]See Valentini (1991) for precise definitions of the coarse grainings.

assuming $\overline{\rho_0(x)} = \rho_0(x)$ is less objectionable than assuming (3)? In both cases we are assuming the early configuration distribution had a rather special profile.

Second, the proof shows that $S(\overline{\rho_0}) \leqslant S(\overline{\rho_1})$ if $\overline{\rho_0(x)} = \rho_0(x)$, but from this it does not follow that $S(\overline{\rho_1}) \leqslant S(\overline{\rho_2})$. Nothing follows about the relative values of two entropies if both are after the initial condition. To get $S(\overline{\rho_1}) \leqslant S(\overline{\rho_2})$, we would then need to invoke $\overline{\rho_1(x)} = \rho_1(x)$. In effect, we need to rerandomize the dynamics. Perhaps there is way of looking at things whereby this rerandomization is caused by the measurement preparation. If not, and maybe even if so, the effective rerandomization looks suspect. If it is not clear that assuming $\overline{\rho_0(x)} = \rho_0(x)$ even one time is less objectionable than assuming $\rho = |\Psi|^2$, it is even less clear that assuming it over and over again is preferable.

Third, as mentioned, this literature does not distinguish between effective and universal wavefunctions. But as stressed at the outset, what is really needed is a justification of (3) with $\psi$ understood as the effective wavefunction. $\rho = |\Psi|^2$ is not sufficient for $\rho = |\psi|^2$ to hold in measurement situations. Even if one proves that the universe as a whole is in quantum equilibrium, we really want to prove that patterns inherent in subsystems of the universe are in quantum equilibrium. Whether any results mentioned survive the move to the proper understanding of (3) is not clear.

What should we make of Valentini and Westman's demonstrations of quantum nonequilibrium states that evolve to coarse-grained quantum equilibrium? Here I believe it is useful to contrast a strong and a weak claim. They seem to vacillate between the two. The strong claim is espoused in the quotation above. It claims one is giving a *dynamical* explanation of the distribution postulate. The weak claim, by contrast, is merely that (3) *could be weakened*, for there are some nonequilibrium states that evolve into states that are observationally indistinguishable from equilibrium.

The strong claim is too unclear to evaluate. Valentini and Westman (Section 6) acknowledge that one will never prove that there exists a monotonic approach to coarse-grained quantum equilibrium for *all* initial conditions. The reason for this is that Bohmian mechanics possesses a time-reversible dynamics. The time reversibility implies that there will be solutions of the dynamical equations that take a system away from equilibrium, rather than toward equilibrium. Bohmian mechanics suffers from Loschmidt's paradox as much as classical mechanics does, as they are well aware. But if more than the dynamics is needed, it is not really clear what "dynamically generated" means in the above quotation. We are back to the fair but imprecise claim that whatever happens in statistical mechanics happens in Bohmian mechanics.

The weak claim is probably true and certainly interesting. The number of Bohmian initial conditions giving rise to a quantum universe is thus enlarged in some sense, alleviating some of our guilt over positing a special initial condition. However, nothing in their treatment shows that *most* or even *many* of the initial states are such as to lead to quantum predictions. If that is what we desire, we can keep the 'weak' version of his claim in mind and still hope for more.

Finally, attention ought to be paid to the evidential disanalogy between the Gibbs–Tolman argument in thermodynamics and Valentini's for quantum mechanics: in the thermal case there is no question that we need to describe the non-equilibrium case—we are in it—but in the quantum case there is *no evidence* that we were ever in non-equilibrium. Valentini hopes that positing a universe in quantum non-equilibrium will turn out to be explanatorily useful, e.g., in cosmology. Until then, the question of how we get to equilibrium from non-equilibrium is not thrust upon us with the same urgency as it is in the thermal case.

## 5. Dynamical approaches II

Another type of dynamical approach mirrors the classical ergodic justification of the uniform measure in thermal equilibrium. Here the idea is *not* to show that quantum non-equilibrium states will inevitably approach equilibrium states. Rather, the idea would be to justify the $|\psi|^2$ density measure used in equilibrium, based on the alleged ergodic feature of the dynamics. The dynamics would then guarantee, for all but a set of measure zero, a quantum equilibrium distribution.

Classically, an ergodic system is one whose infinite time averages are equal to its phase averages, where the phase averages are calculated using the microcanonical measure. If one can argue that what we observe are roughly infinite time averages, then if the system is ergodic we can understand why using the microcanonical measure is justified. Physically, the idea is that ergodic systems spend time in a region of phase space equal to the proportion of that region compared to the full phase space, as measured by the microcanonical measure. Since most of the equilibrium phase space corresponds to a Maxwellian distribution, ergodic dynamics assures us that the distribution will be Maxwellian.

Transferred to the Bohmian case, the reasoning would be something like the following. Assuming a stationary wavefunction (see below), one would first show that the probability the configuration variable $x = (x_1 \ldots x_n)$ was in region $W$ is given by the infinite time limit of the ratio of time it spends in $W$ over the time it spends in the total available space. An ergodic theorem would then link this infinite time average with the measure of the region $W$ given relative to the effective wavefunction $\psi$. This last function would be equal to $|\psi|^2$. So the ergodic theorem would show that the infinite time average is given by $|\psi|^2$. Hence, if one could argue that what we measure are essentially infinite time averages, one would have a justification of the $|\psi|^2$ density.

Little work has been done on this approach. Shtanov (1997) believes equilibrium is justified by ergodic behavior in the classical case and simply assumes/hopes that it holds in the Bohmian case. He then gives a more detailed sketch than above of how, if the Bohmian dynamics is ergodic, this might yield quantum equilibrium. Geiger, Obermair, and Helm (2001) claim to have demonstrated that Bohmian dynamics is ergodic and derived quantum equilibrium from this ergodicity. But I think the claim is really: *given* $|\psi|^2$, the series of measurements and re-preparations leads to a Baker's transformation of the available area in phase space, and hence the trajectory of the measured system is ergodic. In many respects the argument is more similar to one by Dürr, Goldstein, and Zanghi (1992) than the classical counterpart of ergodicity under consideration here; we'll discuss that more detailed program in Section 7.

Since so little has been done we cannot say much about an ergodic justification of (3). At a technical level, some features of Bohmian mechanics may make it easier, others harder. Ergodic theory demands that the measure be invariant in time, and at least in normal (non-quantum gravitational) Bohmian mechanics, it is not. Perhaps proving ergodicity will be aided by the fact that it is only needed for the effective, not universal wavefunction. In any case, the Bohmian ergodic approach clearly will inherit many of the problems one finds in the thermal case, and these are considerable (see van Lith, 2001).

## 6. New dynamics

This strategy exchanges one fundamental dynamics for another. The hope is that a new "improved" dynamics is amenable to a dynamical justification of (3) where the old dynamics was not. In the classical case, this means jettisoning Hamiltonian dynamics for something else. This something else might be, for example, modifying Hamilton's equations via adding a stochastic kick term to the Liouville equation.[6] For our purposes, modifications of (1) are important for two reasons. First, many of the modifications offered in the literature, including the first one by Bohm and Vigier, were intended as justifications of (3). The new dynamics is expected to show improved mixing behavior, taking the nonequilibrium system quickly to equilibrium. Second, many modifications of (1)—done to justify the distribution postulate or not—make the dynamics fundamentally stochastic.

It should not be surprising that there are alternative guidance equations that maintain empirical adequacy. (1) Is attractive because it is very simple and possesses various important symmetries, but other equations are possible. As Deotto and Ghirardi observe, it is obvious that we can add a velocity field to (1) *and still preserve our continuity equation* if the field has the form

$$v_o = \frac{j_o}{|\psi|^2},$$

and $\nabla \cdot j_o = 0$. Deotto and Ghirardi give examples of alternative deterministic guidance equations. But there are also stochastic variations. These come in essentially two kinds: the first is stochastic because stochastic terms have been added to (1), whereas the second is stochastic because the fundamental quantities are assumed to be discrete and bounded.

Regarding the first, Bohm and Vigier (1954) introduced "fluid fluctuations" that they hoped would drive an arbitrary system to quantum equilibrium. Bohm and Hiley (1993) and Peruzzi and Rimini (2000) have done something similar. Also, one can think of Nelson's (1985) stochastic mechanics as a Bohmian theory where (1) is replaced by a stochastic guidance equation. Indeed, there is a whole family of stochastic guidance equations that preserve quantum equilibrium:

$$\frac{\mathrm{d}x}{\mathrm{d}t} = b\,\mathrm{d}t + \sqrt{\alpha}\,\mathrm{d}\omega,$$

where

$$b = \frac{\hbar}{m}\nabla S + \alpha\frac{\hbar}{2m}\frac{\nabla|\psi|^2}{|\psi|^2},$$

and $\mathrm{d}\omega$ is a Wiener process, the simplest continuous Markov process. If $\alpha = 1$, then we get Nelson's mechanics and if $\alpha = 0$ we get Bohmian mechanics (Bacciagaluppi, 1999; Davidson, 1979).

Immediately we should pause to point out that if one treats an additional stochastic kick term as merely an epistemic convenience, reflecting our ignorance over environmental perturbations, then this move does not answer our question. It merely pushes it back a step, for we would be entitled to ask why one chose the probability distribution one did over the perturbations. Assuming some natural probability distribution over the environment is perfectly fine for practicing physics. But if one wants to get at the ultimate reason for quantum equilibrium, it is merely obscurantism.

[6]See van Lith section 14.2 for discussion of two distinct methods of implementing this strategy.

The second approach is an extension of Bohmian mechanics to discrete beables. Indeterminism results because trajectories for time-dependent discrete quantities cannot be continuous, so a differential equation of motion cannot be used—hence a stochastic dynamics is natural. Bell (1987) devised such a dynamics for fermion number density, and Vink and Dürr, Goldstein, Tumulka, and Zanghi (2005) have developed this approach in more detail. Here we keep the wavefunction evolving according to the normal deterministic Schrödinger equation. As in Bohmian mechanics, we then introduce a position-like variable that we will call $o$. The observable $\hat{O}$ corresponding to $o$ has discrete and finite eigenvalues $o_n$. What replaces (1) is now a transition matrix $T$. The transition matrix $T_{mn}\,dt$ gives the probability $o_m$ will jump to $o_n$ in time d$t$. Just like the ordinary Bohmian velocity, the transition matrix is equal to the probability current $J$ divided by the probability density $P_m$, only here these two are discretized (for details, see Vink). This dynamics describes a biased random walk in $\hat{O}$-space. Vink proves that in the continuous position limit, this stochastic dynamics converges to the original Bohmian dynamics. As in the deterministic case, one can add Deotto–Ghirardi currents for alternative empirically adequate trajectories; one also has some extra freedom in the choice of $T_{nm}$ (see Bacciagaluppi, 1999, p. 10; Vink, 1993, p. 48), thereby increasing in some sense the number of possible dynamics preserving quantum equilibrium.

Apart from introducing indeterminism, do the above theories add anything new to the discussion of the distribution postulate? Yes and no. The stated goal of some of these modifications to (1) is often to provide a "dynamical" explanation of quantum equilibrium, in the sense of Section 4 above. Intuitively, one can see that (for example) adding extra fields to the velocity formula might induce more "mixing-like" behavior than already found in (1). But to my knowledge, the situation is not much different than the one surveyed above in 4. There are scattered results: showing convergence to approximate quantum equilibrium with certain dynamics and particular systems, showing lack of convergence for other systems. Certainly there is nothing like a proof that for most initial conditions the approach to equilibrium is inevitable or even probable.

## 7. Typicality

We have not found in the "dynamical" approaches a reason to think "most" Bohmian initial conditions lead to a quantum world. If this is what we want, we do not yet have it. Let us now turn to a strategy inspired by Maxwell and Boltzmann (Maxwell, 1860) (like so many of the others) that explicitly formulates what we mean by "most" and then demonstrates that most initial conditions lead to Bohmian histories wherein Born's rule is appropriate. In thermal physics, Maxwell famously showed that of all equilibrium empirical distributions of velocities, most are Maxwellian. And in non-equilibrium cases, Boltzmann (1872) made plausible the claim that, given an initial low entropy macrostate, most subsequent trajectories head to higher entropy states for long periods. "Most" in both cases is understood with respect to Lebesque measure restricted to a constant energy surface. Dürr, Goldstein and Zanghi do something similar for Bohmian mechanics.

Let us begin slowly, since it is easy to misstate DGZ's position. The main argument is laid out in Dürr et al. (1992), although they individually and jointly elaborate on the position in subsequent papers. What they essentially prove in 1992, as Dürr (2001, p. 128) says, is a law of large numbers (LLN) theorem for the empirical distribution of configuration values for subsystems of the universe. To get a feel for what they do, they provide a useful analogy:

> Roughly speaking, what we wish to establish is analogous to the assertion, following from the law of large numbers, that the relative frequency of appearance of any particular digit in the decimal expansion of a typical number in the interval [0,1] is 1/10. In this statement, two related notions appear: typicality, referring to an a priori measure, here the Lebesque measure, and relative frequency, referring to structural patterns in an individual object. (1996, p. 38)

Understanding the example will help our case. In mathematics, *simply normal* or just *normal* numbers are typical. If we take a number $x = y.x_1x_2x_3\ldots$ then $x$ is normal just in case, for all bases and for all subscripts $i$, 1/10th of the $x_i$'s are zero, 1/10th of the $x_i$'s are one, 1/10th of the $x_i$'s two, and so on. There are two remarkable facts about normal numbers. One is that we know of only one non-trivial normal number, a number (0.1234567891011…) discovered in 1933 by an undergraduate student named David Gowen Champernowne. Pi and many other numbers *look* normal, in the sense that their known relative frequencies are converging towards normalcy. But of course, the finite relative frequencies are compatible with any infinite relative frequencies. What really suggests pi's normality, and every other number's normality, is the second remarkable fact: Borel showed in 1909 that "almost all" numbers $x$ are normal. That is, Borel proved that the complement of the collection of normal numbers in [0,1] is of zero length according to Lebesque measure. It is in this sense that normal numbers are typical.

Borel's theorem is a consequence of the strong LLN in probability theory. The strong LLN says, roughly, that if $X_1$, $X_2$, $X_3$, …, is an infinite sequence of random variables that are independent and identically distributed (iid), then the relative frequency of some property converges almost surely (i.e., with probability one) to the probability of that variable. The weak law says, by contrast, that the convergence happens probabilistically, i.e., with probability strictly less than one. (One can also prove LLN-type results without assuming independence, but rather by showing the sequence is ergodic.) In the case at hand, the probability measure gives each numeral, '1', '2', etc., an equal probability of 1/10. Furthermore, the numbers are iid: for instance, knowing $x_2 = 4$ does not affect the probability of (say) $x_3 = 1$. Applied to the case at hand, the LLN states that if we pick a number uniformly at random between zero and one, then it is *normal* with probability one.

Notice that the decimal expansion of a number is "deterministic." There is nothing chancy about whether the next number of pi after 3.141 is 5 or not. Notice also that normality is a feature of the entire number, not any particular $x_i$. We do not say having a '5' in the sixth decimal location is normal. Normality is a feature of a collective of numbers having a certain pattern.

Return to Bohmian mechanics and Born's rule. Again we have a deterministic theory, and the counterpart of our numbers $x$ are histories of the Bohm particles $Q(t)$. As we found patterns in the $x$'s, we try to find patterns in the configuration variable histories. In particular, the pattern we are interested in is the one wherein the particles for *subsystems* of the universe are distributed according to $|\psi|^2$ when the subsystem merits an effective wavefunction, just as we might look for the relative frequency of fives in a real number $x$. The *a priori* measure used by DGZ, the counterpart to Lebesque measure, is the natural volume measure on configuration space, modified by $|\Psi(q_1 \ldots q_N;\, 0)|^2$:

$$|\Psi(q_1 \ldots q_N; 0)|^2 d^{3N} q. \tag{6}$$

Note that (6) uses the wavefunction of the universe.

To prove a LLN result, one needs iid random variables. It is hardly obvious that one can get this for configurations of subsystems of the universe. After all, one would expect that if we take one subsystem $X$ at time $t$, evolve it to time $t'$, where $t' > t$, then $X(t)$ and $X(t')$ would *not* be independent. In fact, as DGZ point out, if the wavefunction is the ground state, not only is $X(t')$ a function of $X(t)$, but $X(t) = X(t')$, which is as far from independence as it gets. Nevertheless, under the conditions when an effective wavefunction is defined, matters are quite different. In this case, one can show that if we have a collection of sub-systems $\{X_1 \ldots X_n\}$, either at a time or across time, each with the same effective wavefunction, then one can prove that the configurations of these subsystems are in fact iid distributed random variables with respect to (6) conditioned on the environment of the subsystems. That is, the subsystem particle distribution, when effective wavefunctions obtain, is randomly distributed according to $|\psi|^2$, both at a time and at different times. See Dürr et al. (1992) for the precise argument. This is an important result. What DGZ have done, in effect, is argue that a kind of "effective Bernoulliness, and hence an effective ergodicity" holds in Bohmian dynamics (1992, p. 890). When systems are large enough, the positions of Bohm particles in the measured subsystems "forget" the initial distribution. With this independence result in hand, DGZ are then able to prove a LLN result. We will not do that here.

Dürr, Goldstein, Tumulka, and Zanghì (2006) summarize their work as follows:

> In Bohmian mechanics, a property $P$ is typical if it holds true for the overwhelming majority of histories $Q(t)$ of a Bohmian universe. More precisely, suppose that $\Psi_t$ is the wave function of a universe governed by Bohmian mechanics; a property $P$, which a solution $Q(t)$ of the guiding equation for the entire universe can have or not have, is called typical if the set $S_0(P)$ of all initial configurations $Q(0)$ leading to a history $Q(t)$ with the property $P$ has size very close to one, $S_0(P)|\Psi_0(q)|^2 \mathrm{d}q = 1 - \varepsilon$, $0 \leqslant \varepsilon 1$, with "size" understood relative to the $|\Psi_0|^2$ distribution on the configuration space of the universe. For instance, think of $P$ as the property that a particular sequence of experiments yields results that look random (accepted by a suitable statistical test), governed by the appropriate quantum distribution. One can show, using the law of large numbers, that $P$ is a typical property.

With size understood relative to (6), DGZ show that a certain property $P$ of solutions $Q(t)$ is typical. $P$ is a pattern in $Q(t)$, just as manifesting equal frequencies of numerals is a pattern in some real numbers. Here the pattern is more complicated: it is having a configuration such that, when subsystems of it merit effective wavefunctions, the actual subensembles have probability density of positions given by $|\psi|^2$. This pattern is precisely what is needed to justify Born's rule. The claim that $P$ is typical is another way of saying that a LLN result has been proven such that most Bohmian histories $Q(t)$ have property $P$. We finally have a proof that *most* Bohmian universes are such that Born's rule works in them.

Before exulting in success, let's examine the claim. Though we have not gone through the mathematical details, one worry is obvious: one assumes a measure weighted by $|\Psi|^2$ to show that $|\psi|^2$ behavior is typical. The proof seems circular. In reply, note that what is proved is not assumed. The assumption is at the level of $\Psi$ not $\psi$. Nor is one assuming that there is an empirical distribution in accord with $|\Psi|^2$. By analogy, we have not taken an ensemble of 100 people, 90% of whom have black hair, and then (randomly) extracted a subensemble of 50 and found that roughly 90% of these too have black hair. The connection between $|\Psi|^2$ and $|\psi|^2$ is more complicated than that. The objector might respond: granted, the result is not trivial, but still, if one assumed the volume measure was weighted by $|\Psi|^3$ then the argument would show that some other behavior was typical, not Born's rule behavior. So why choose (6) rather than some other measure, e.g., $|\Psi|^3 d^{3N}q$ or the uniform measure? As Bricmont remarks (Bricmont, 2001; Bricmont et al., 2001), (6) hardly seems the a priori natural choice.

The charge "why that measure?" may be attenuated by showing that measures besides (6) will also work. In the case where we have an infinite number of random events, then so long as a measure is absolutely continuous with (6) the LLN result will hold good for that measure too. This is a point familiar from ergodic theory. A measure absolutely continuous with (6) agrees with (6), by definition, on the size of measure one and measure zero subsets. Since (6) assigns measure one to those histories in which Born's rule is appropriate, any measure a.c. with respect to (6) will too. In the more realistic finite case, we also have some freedom in what measure we pick, so long as we do not pick a measure that weighs heavily the formerly very tiny sets of positive measure. See Maudlin (this volume) for further discussion.

However, ultimately we *have* to pick *some* measure or set of measures. If we do not, this style of explanation, wherein one explains a behavior by showing that most initial conditions lead to it, must be abandoned. The objector is then in the awkward situation of denying the explanatory value of some of the *prima facie* most elegant explanations in science. The price of maintaining this austere position is too high.

Assuming we're unwilling to pay this price, the only way to proceed is to see which measures work and whether any have special mathematico-physical features that single them out in some non-arbitrary manner. We do this when justifying the microcanonical measure in statistical mechanics, and we can do so here. To this end, DGZ point out that $|\Psi|^2$ is special because it is preserved by the dynamics, and as such, does not arbitrarily distinguish one time over others. If we chose $|\Psi|^3$ and then backward evolved it to the early universe, the measure would no longer be $|\Psi|^3$. $|\Psi|^2$, by contrast, is time translation invariant. I would also add that (6) is the measure used to demonstrate global existence and uniqueness of the dynamics. The results on global existence and uniqueness of Bohmian trajectories prove that almost all trajectories as measured by (6) do not run into nodes of the wavefunction, do not escape to infinity, and do not run into singularities of the potential. Dialectically, we're assuming we already have a workable global dynamics, so in this sense, (6) is part of the Bohmian "package" and distinguished. Neither of these arguments mark (6) as unique—there may, for instance, be other measures preserved by the dynamics—but these considerations at least provide some additional rationale for the choice of (6).

As things now stand, we have a LLN result. That is it. It is a piece of mathematics. How does this result relate to actual frequencies? This question and others need answers before the result fully explains Born's rule.

## 8. Interpretation of quantum probability

When speaking of probabilities in a deterministic theory one often hears it said that the probabilities are "epistemic," i.e., due to ignorance. The probabilities are interpreted as one's subjective degrees of belief in a proposition. Certainly this interpretation makes it easy to square non-trivial probabilities with an underlying determinism. Since no one knows the complete exact state of the world, one's credence in a proposition need not be one or zero. However, in this Section 1 want to pursue the idea that the probabilities in Bohmian mechanics are objective. Arguably, this is the most natural interpretation and the subjectivist position is one of last resort. It's not merely that *I think* it's likely that the system I'm measuring is in quantum equilibrium; it's likely to be in quantum equilibrium regardless of what I or anyone else believes. Granted, the sophisticated subjectivist (e.g., Skyrms, 1984) has ways of saying something similar. Seriously engaging him or her here, however, would take us too far off course. My hope is that the reader shares the above disposition enough that he or she is at least motivated to search for a non-subjective interpretation of these probabilities.

Three quick comments. First, I do not believe that much of what follows hangs on the fine details of Bohmian mechanics. Equilibrium theory, games of chance, natural selection, and more, all require objective probabilities to play roles similar to that in Bohmian mechanics. The problem is hardly a new one. Second, I'll assume the reader knows the usual litany of objections against the traditional opposed interpretations of objective probability, frequentism and Popperian propensities. We will not challenge the received view that these are inadequate. Third, since other articles in this volume discuss interpretations available only to systems with stochastic dynamics, I will not spend time on that question here. We'll assume that we're working with deterministic Bohmian mechanics. I will only make the point that this assumption may not matter. The only difference between deterministic and indeterministic dynamics is where one puts the measure. While this difference *can* matter, I'm not sure that it should or does according to the best interpretations of probability.

DGZ prove a LLN result for Bohmian mechanics. How should we interpret this? We must be on guard against the danger, to which many succumbed in the history of probability, of thinking one can read off an interpretation of probability from a mathematical result. Consider, for instance, the many people who took the ergodic theorem to imply a "time average" interpretation of probability. While the ergodic theorem may help justify that interpretation, it is more plausibly a necessary rather than sufficient condition for that interpretation. Similarly, with LLN results, there is a temptation we must steel ourselves against to view a LLN result as itself supplying an interpretation of probability. Many textbooks regard the LLN as implying a relative frequency interpretation of probability.[7]

But this is a mistake. The LLN states that in the long run the relative frequency of an i.i.d. distributed random variable and the probability of the relevant outcome (derived from the assumed measure) are with probability approaching or equal to 1 nearly identical. Put loosely, *probability is close to long run relative frequency, probably*. Clearly, the second use of probability needs a non-probabilistic explication to avoid circularity (as does "independence" in iid). The LLN does not provide an interpretation of probability, nor does it even link long run frequencies to probabilities without an understanding of the second probability.

[7]Some Bohmians sometimes *sound* as if they are using the LLN to provide a relative frequency interpretation, even if that is not their intention: "Using typicality one may define probability in terms of *law* of *large numbers* type statements (Dürr, 2001, p. 130).

So what does typicality actually imply? Suppose that you prepare a quantum measurement. You wonder what distribution of particle positions you should expect upon measurement. DGZ come forward and say, "Use Born's rule—this is a great policy in typical histories." You think about it and respond, "why should I expect this universe to be typical? Cannot atypical things happen? Is it *likely* that it's typical?"

Dürr answers:

> Are typical events most likely to happen? No, they happen because they are typical. But are there also atypical events? Yes. They do not happen, because they are unlikely? No, because they are atypical. But in principle they could happen? Yes. So why do not they happen then? Because they are not typical. (2001, p. 130)

Dürr notes that there is a great deal to say philosophically about this position. Indeed there is. We are here reprising a famous episode in the history of probability. In the history of probability, there have been many attempts to answer this problem. Cournot and others tried to answer the problem by stipulating that small probabilities are *impossible*, but this leads to serious consistency problems—Dürr wisely avoids this position in the quote. Kolmogorov instead said that measure one results implied "practical certainty," yet this position gives the probabilities a subjective cast, and so it will not meet our needs for an objective probability.

Our problem appears conspicuously in the ergodic justification of statistical physics. This fact is not surprising, since the ergodic theorem can be read as a LLN result. The ergodic theorem claims that with measure equal to one, the time average probability and the statistical probability are identical. The famous "measure zero" problem for ergodic justifications asks why we should assume measure zero events are unlikely to happen. This is a particularly devastating problem for those who hoped that the ergodic theory provided its own interpretation of probability. But it is equally a problem for anyone pinning the interpretation of probability on the LLN, even if the measure of atypical events is greater than zero.

Although DGZ shun the identification of typicality with likeliness, it is also clear from the Dürr quotation that they do not think one should bet much money on atypical phenomena occurring. Unless we connect their result to empirical frequencies, then it's unclear what the LLN result is doing for us. Like probability, typicality is a guide to our beliefs. It seems incumbent upon us to interpret this "second probability" in the LLN as a probability. The question then is what kind of probability is it?

DGZ express serious reluctance to the idea of interpreting the typicality measure probabilistically. We can understand this and sympathize by looking at their result in terms of the two most dominant interpretations of objective probability, frequentism and propensity theory. Suppose we interpret the second probability in the LLN result in terms of relative frequencies, either Reichenbachian ideal actual frequencies or hypothetical frequencies. According to these theories, the $P(A)$ is the relative frequency of $A$ in either an idealized actual or hypothetical long series of repetitions of an experiment. Interpreting the LLN this way requires a series of repetitions of experiments which are themselves series of repetitions, i.e., in von Mises terminology, a collective of collectives. In the case at hand this unfortunately yields an actual or hypothetical collective of Bohmian universes $\{Q(t), Q'(t)\ldots\}$, each of which is a collective. DGZ are adamantly opposed to this sort of reading, repeatedly saying throughout their work that there is only one universe and that as a result they are not interpreting typicality as a kind of probability. Alternatively, let's try understanding the theory with objective propensity, a la Popper. Again we face extra-worldly implications. What could it mean for the universe as a whole to have a probabilistic disposition to occur? There is nothing outside the universe to trigger the disposition.

An instinctive negative reaction to assigning probabilities to initial conditions is natural and probably even healthy. DGZ are understandably reluctant to dub the universe probable, for it invites quasi-theological pictures of supernatural beings picking the universe out of an urn. That said, in the philosophical literature there are now accounts of chance that do not invite such fantasies. To mention two broad classes, there are "Humean" accounts of chance and "theoretical term" accounts of chance. The most popular Humean theory is one due to Lewis (1994). A recent version of the theoretical term account is Sober's (2005) "No-Theory Theory" account of chance, although views like this have been proposed earlier (Levi, 1990; Sklar, 1979). Let me briefly describe each type of account and how they would understand the probabilities in Bohmian mechanics.

Lewis proposes that the chance of an event or proposition is simply the real numerical value in the closed unit interval assigned to that event by the Best Theory. The Best Theory is the best systematization of nature. According to it, the fundamental laws of nature are simply the axioms of the theory that best systematizes the actual facts. "Best" means that the theory strikes the best balance available between simplicity and informativeness, where simplicity is measured with respect to the concision of the law framed in some language (according to which the natural properties correspond to atomic predicates of the language) and informativeness is measured with respect to how many actual facts the theory implies. The "actual facts" are usually understood in some very broadly empiricist fashion; so long as they do not include nomic or irreducibly probabilistic facts we need not be too fussy about them here. Clearly, the virtues of simplicity and informativeness will trade-off one another. A long list of all the outcomes of all coin tosses is not very simple, nor is the claim "some side or other came up" very informative.

In 1994 Lewis noted that probabilistic laws might be so simple and strong that they may warrant membership in the set of axioms of the Best Theory. He accommodated probabilistic laws by adding the concept of fit to an assessment of strength. Systems score different fit-values for different worlds; the fit is given by the chance the system gives to a world's history: the higher the value, the better the fit. If the Best Theory assigns a one-half chance of fair coins landing heads on coin flips, then the actual world has better fitness than a world wherein coins always land heads. On Lewis' view, actual frequencies are evidence of the chances (the better the match with actual frequencies, the better the strength of the probabilistic law) but not identical with the chances (simplicity will buy some breathing room between the chances and frequencies). Chances supervene upon patterns of events in the one actual world.

For reasons we need not go into, Lewis' theory ascribes only trivial chances (0 or 1) in worlds with fundamentally deterministic laws. Many philosophers, however, have seen that his restriction to stochastic dynamical systems is both undesirable and inessential to the account. Hoefer (2005), Loewer (2001) and Meacham (2005) all modify Lewis' theory of chance to cover deterministic worlds. They each do so with an explicit goal of making sense of the probabilities one finds in classical statistical mechanics and Bohmian mechanics, among other places. On Loewer's theory, for instance, one adds non-trivial objective probabilities to a deterministic theory by placing a probability distribution over the microscopic initial conditions. Each microscopic world history then has a certain probability. These micro-probabilities then imply probabilities of macroscopic events. Thus the probability of some macroscopic event A happening at t is given by this microscopic probability distribution and then conditionalizing over the entire "macroscopic" history of the world up until t. In the case of statistical mechanics, one would use the microcanonical probability distribution over the initial conditions of Newtonian mechanics and conditionalize over the thermodynamic state of the world. Applied to Bohmian mechanics, the probability distribution would be over initial configurations of Bohmian particles. Instead of the microcanonical distribution the Bohmian would use a probability distribution crafted from the measure in (6). And instead of conditionalizing on the macroscopic state of the world one would conditionalize over the quantum state of the world, i.e., the effective wavefunction. The claim, then, is that the Best System of our world includes a probability distribution crafted from the measure in (6) over initial particle configurations of the universe. (In a way, the Default Position of Section 3 returns via the interpretation of the probabilities in the DGZ result.) Conditionalizing on this distribution, one gets non-trivial chances that match up with those given by Born's rule. With a small sacrifice in simplicity, we recover all the quantum mechanical generalizations.

Next, consider the "theoretical term" account. What motivates this theory is the fact that science already posits plenty of objective quantities (theoretical quantities) not definitionally tied to observable quantities. We should think of probability, advocates think, as realists about mass understand mass. Operationalists like Mach tried and failed to identify mass with certain observable procedures; similarly, strict empiricists tried and failed to identify objective probability with (e.g.) frequencies. But this failure to reduce a theoretical term to an observation term does not mean that there are not really masses, nor does it entail that there are not really probabilities. Mass exists. We are warranted in believing in it because it plays a central role in very successful theories. Same goes for probability. It is a central feature of many successful theories.

But what is it? Well, what is mass? It is not usually felt necessary to endow it with some nature over and above what science requires of it. Mass just is that quantity that behaves as the relevant laws of nature say it does. Same for probability. As with other theoretical terms, one does not attribute some nature to probability above and beyond what science requires of it (hence "no theory"); rather one says that it is that quantity that obeys Kolmogorov's axioms and plays the role it does in science. In the case of Bohmian mechanics, when one works through the details of Sober's theory, the macro-chances will be very similar to those found in Loewer's theory.

In both cases the central idea is that chance is implicitly defined by the role it plays in science. That role and the science's success justify positing an objective referent for it. Neither theory equates the chances with the actual frequencies. And in both Loewer's and Sober's versions, chances are typically attached to macroscopic events, making non-trivial probabilities compatible with an underlying determinism. The main difference is that the No-Theory account skips the story about the origin of the laws of nature and chance.

Although both theories have problems, they may satisfactorily solve our problem. Each will interpret the DGZ result as saying that the objective chance is close to one that the relative frequencies in any Bohmian history match the probabilities given by Born's rule. This involves no commitment to indeterminism, an actual ensemble of worlds, or mysterious dispositions. Although this is a subject fraught with controversy, to be adequate they must also be connected to the actual frequencies and rational credence. On a Humean theory, chance would seem to have empirical significance because of the

Humean's connection between chance and patterns of actual facts via the Best Theory. The Humean chances may depart from the frequencies, but they are guaranteed not to depart too much from them—otherwise the chance theory would not be strong. In certain worlds the Humean theory actually reduces to actual frequentism. As for rational credence, Lewis (1994, p. 484) says he can see "dimly" how a Humean account can connect chance to rational credence. Hoefer (forthcoming) tries to spell this out with a consequentialist argument for the utility of setting rational credence equal to chance. Lewis also famously could not see how a non-reductionist theory of chance could constrain rational credence. If the chance of an event is simply an unadorned number, then why should such numbers constrain rational credence? The Humean ties the number to the way the actual pattern is, but the non-reductionist does not. Sober does not say, but he could build these features analytically into the theoretical term he posits. How high a price one pays for this move is controversial. But further exploring this criticism, and others one can imagine for either theory, is beyond the scope of this paper. Suffice to say, we have here advances over earlier theories of chance.

Some readers will not be satisfied by these theories of chance. Neither answers some questions you may want answered. If you want some mechanism that explains *why the patterns in the actual world hold*, then neither theory provides anything like this. If you wanted to locate some antecedently known physical magnitude that already had many of the features you attribute to chance (like an actual frequency or a propensity or a credence), then we did not find one. We posited one. For myself, these vices, where I think they're vices, are outweighed by the virtues of actually having a theory that fits the probabilities used in science, and in this case, Bohmian mechanics.

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