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Probability in GRW theory

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Abstract

GRW theory postulates a stochastic mechanism assuring that every so often the wave function of a quantum system is 'hit', which leaves it in a localised state. How are we to interpret the probabilities built into this mechanism? GRW theory is a firmly realist proposal and it is therefore clear that these probabilities are objective probabilities (i.e. chances). A discussion of the major theories of chance leads us to the conclusion that GRW probabilities can be understood only as either single case propensities or Humean objective chances. Although single case propensities have some intuitive appeal in the context of GRW theory, on balance it seems that Humean objective chances are preferable on conceptual grounds.

Keywords: GRW theory; Quantum measurement problem; Chance; Humean best system; Propensity; Frequentism

1. The spontaneous localisation approach to quantum mechanics

The formalism of quantum mechanics (QM) allows for superpositions of macroscopically distinguishable states. This stands in contradiction to the fact that we experience objects as having determinate properties. Reconciling this feature of the quantum formalism with everyday experience is the infamous measurement problem. In response to this problem von Neumann suggested that upon measurement the Schrödinger dynamics is suspended and the system collapses into some eigenstate of the measured observable with a probability given by Born's rule. This suggestion faces many well known problems. What counts a measurement? At what point in the measurement process does the state of the

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system collapse? And why should properties of physical systems at all depend on there being observers?

One way to circumvent these difficulties is to change QM in a way that avoids reference to observers. This can be achieved by incorporating collapses into the basic evolution of the system: collapses happen as a consequence of the basic laws governing a physical system and do not need to be tacked onto the theory as an occasional measurement-induced interruption of the 'actual' time evolution. This has far-reaching consequences in that it requires an alteration of the basic equation of QM, the Schrödinger equation, to which a stochastic term is added bringing about the desired reduction of the wave function. As a result, the wave function no longer evolves deterministically; instead it evolves according to a stochastic process that is similar but not equivalent to the Schrödinger evolution.

The idea to remould QM along these lines has been around since the 1970s, but it had its first breakthrough only in 1986 when Ghirardi, Rimini and Weber presented a workable formulation of the sought-after stochastic dynamics, now commonly referred to as 'GRW theory'.[1]

Before discussing the theory in some detail, it is worth getting clear on what the theory is expected to achieve. According to its progenitors, the theory has to satisfy three requirements.

- *Requirement 1*. The theory has to solve the measurement problem; that is, the dynamical laws have to be such that superpositions of macroscopically distinguishable states are suppressed immediately.
- *Requirement 2*. Standard QM is a universal theory in the sense that its application is not limited to a particular domain: microscopic and macroscopic objects alike are governed by the fundamental law of the theory, the Schrödinger equation. The fundamental law of GRW theory must be universal in the same way.[2]
- *Requirement 3*. The theory has to be empirically adequate; in particular, it has to reproduce the well-known 'quantum behaviour' when applied to microscopic objects and classical behaviour when applied to macroscopic objects.

The theory is based on two sets of assumptions; the first is concerned with the nature of the localisation processes, and the second with when they occur.

The localisation process. Three assumptions are needed to pave the ground for a mathematical formulation of the localisation process. First, a choice needs to be made about the basis in which the localisations occur. GRW theory regards position as the relevant basis and posits that so-called hits[3] lead to a localisation with respect to position.[4]

Second, at what level are hits effective? GRW theory posits that the elementary constituents of a system (the molecules or atoms from which it is built up), rather than the

[1]The original paper is Ghirardi, Rimini, and Weber (1986). Bassi and Ghirardi (2003) provide a comprehensive survey. Semi- and non-technical presentations of the theory can be found, among others, in Bell (1987), Ghirardi (1997a, 1997b, 2001, 2004), Ghirardi, Pearle, and Rimini (1990), and Rimini (2001).

[2]Rimini (2001, p. 137) refers to this as 'computational covariance'.

[3]As we shall see later on, there are important differences between the collapses postulated by von Neumann and the localisation processes of GRW theory. For this reason we do not refer to the latter as 'collapses' and call them 'hits' instead.

[4]This choice is partially motivated by conceptual reasons, and partially by the fact that the localisation mechanicsm of GRW theory can be shown not to work for variables other than position.

entire system, are subjected to hits. Third, the hits change the state (the wave function) of the affected constituent, and not its density matrix. It is important to bear this point in mind, in particular because the basic equation of motion of the theory will be formulated in terms of density matrices.

A localisation process transforms the state $|\psi\rangle$ of the system into another, more localised state,

$$|\psi\rangle \longrightarrow \frac{\hat{L}^k_{\mathbf{x}}|\psi\rangle}{\|\hat{L}^k_{\mathbf{x}}|\psi\rangle\|}, \tag{1}$$

where the localisation operator $\hat{L}^k_{\mathbf{x}}$ is a linear, self-adjoint operator localising the kth particle around the point $\mathbf{x}$ in three-dimensional physical space. The localisation centre $\mathbf{x}$ is chosen at random according to

$$P_k(\mathbf{x}) = \|\hat{L}^k_{\mathbf{x}}|\psi\rangle\|^2. \tag{2}$$

The choice of this distribution assures that the predictions of GRW theory do not differ significantly from the predictions of standard QM as it ensures that the probability for hits is high in those regions in which the standard QM probabilities for collapses are high too.

The localisation operator is a Gaussian:

$$\hat{L}^k_{\mathbf{x}} = \left(\frac{\alpha}{\pi}\right)^{3/4} \exp\left[-\frac{\alpha}{2}(\hat{\mathbf{q}}_k - \mathbf{x})^2\right], \tag{3}$$

where $\hat{\mathbf{q}}_k$ is the position operator for the kth particle and α is a constant defined by $1/\sqrt{\alpha} = 10^{-7}$ m, which is the distance between the peaks of localisation of two terms in a superposition above which the superposition is suppressed.

The occurrence of localisation processes. When and how often do localisation processes occur? GRW theory assumes that these occurrences constitute a Poisson process. Generally speaking, Poisson processes are processes characterised in terms of the number of occurrences of a particular type of event in a certain interval of time τ, for instance the number of people passing through a certain doorway during time τ. These events are Poisson distributed if

$$p(E = m) = \frac{e^{-\lambda\tau}(\lambda\tau)^m}{m!}, \tag{4}$$

where E is the number of events occurring during τ, $m = 0, 1, 2, \ldots$, and λ is the parameter of the distribution. The mean value of the Poisson distribution is λ, and hence λ can be interpreted as the average number of events occurring per unit time (i.e. λ can be interpreted as a mean frequency). Furthermore, and this is crucial for what follows, the probability of an event occurring during the infinitesimal interval dt is λdt.

The mean frequency of the distribution governing the hits of the kth constituent is λ_k, for all k. Nothing in principle rules out that there be different frequencies for every microconstituent. However, the theory assumes that they all have the same frequency: $\lambda_k = \lambda_{\text{micro}}$ for all k. Numerical considerations show that $\lambda_{\text{micro}} \cong 10^{-16}\,\text{s}^{-1}$.

On the basis of these assumptions one can derive the fundamental equation of motion. From a technical point of view, as Eq. (1) indicates, the reduction mechanism transforms a pure state into a mixture (which is also intuitively plausible if we adopt an ignorance interpretation of mixtures: we do not know what the localisation centre will be). From

Eqs. (1) and (2) we then get:

$$|\psi\rangle\langle\psi| \longrightarrow \int_{R^3} \mathrm{d}^3x P_k(\mathbf{x}) \frac{\hat{L}_{\mathbf{x}}^k|\psi\rangle\langle\psi|\hat{L}_{\mathbf{x}}^k}{\|\hat{L}_{\mathbf{x}}^k|\psi\rangle\|^2} = \int_{R^3} \mathrm{d}^3x \hat{L}_{\mathbf{x}}^k|\psi\rangle\langle\psi|\hat{L}_{\mathbf{x}}^k. \tag{5}$$

We can now define

$$T_k[|\psi\rangle\langle\psi|] := \int_{R^3} \mathrm{d}^3x \hat{L}_{\mathbf{x}}^k|\psi\rangle\langle\psi|\hat{L}_{\mathbf{x}}^k, \tag{6}$$

with which Eq. (5) becomes

$$|\psi\rangle\langle\psi| \longrightarrow T_k[|\psi\rangle\langle\psi|]. \tag{7}$$

Notice that in case the initial state of the particle is a mixture ρ rather than a pure state, the effect of the localising process remains the same: ρ changes into $T[\rho]$.

Now consider the change of the density matrix ρ during the interval $\mathrm{d}t$. The total change of ρ during $\mathrm{d}t$ is the sum of the changes due to the Schrödinger evolution, $(\mathrm{d}\rho)_\mathrm{S}$, which governs the system when no hits occur, and the changes due to the hits, $(\mathrm{d}\rho)_\mathrm{H}$, weighted by the respective probabilities that they occur:

$$\mathrm{d}\rho = p_\mathrm{S}(\mathrm{d}\rho)_\mathrm{S} + p_\mathrm{H}(\mathrm{d}\rho)_\mathrm{H}. \tag{8}$$

The Schrödinger time evolution of a density operator is given by $\mathrm{d}\rho/\mathrm{d}t = -(\mathrm{i}/\hbar)[\hat{H}, \rho]$, where $\hat{H}$ is the Hamiltonian of the system. From this we immediately get

$$(\mathrm{d}\rho)_\mathrm{S} = -\frac{\mathrm{i}}{\hbar}[\hat{H}, \rho]\,\mathrm{d}t. \tag{9}$$

From Eq. (7) we obtain

$$(\mathrm{d}\rho)_\mathrm{H} = T_k[\rho] - \rho. \tag{10}$$

Because the hits are Poisson distributed we have $p_\mathrm{H} = \lambda_k\,\mathrm{d}t$ and $p_\mathrm{S} = 1 - \lambda_k\,\mathrm{d}t$. Putting these expressions together and dividing by $\mathrm{d}t$ yields

$$\frac{\mathrm{d}\rho}{\mathrm{d}t} = -\frac{\mathrm{i}}{\hbar}[\hat{H}, \rho] - \lambda_k(\rho - T_k[\rho]), \tag{11}$$

which describes the effect of time evolution of the kth particle on the state of the system. We obtain the equation of motion of the entire system by summing over all particles:

$$\frac{\mathrm{d}\rho}{\mathrm{d}t} = -\frac{\mathrm{i}}{\hbar}[\hat{H}, \rho] - \sum_{k=1}^{n} \lambda_k(\rho - T_k[\rho]), \tag{12}$$

where $\lambda_k = \lambda_\mathrm{micro}$ for all k. This is the fundamental equation of GRW theory.[5]

How does the theory fare with the requirements mentioned at the beginning?

Requirement 1. One can prove that under the GRW dynamics superpositions of macroscopically distinguishable states are reduced almost immediately to one of its terms

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[5]Notice that we retrieve the standard Schrödinger equation if we let all λ_k tend towards zero, which, of course, means that no hits occur.

with the appropriate probabilities (see Bassi & Ghirardi, 2003, pp. 42–3 for explicit calculations). This solves the measurement problem.[6]

Requirement 2. The fundamental equation of the theory, Eq. (12), does not come with any specification about acceptable values of n, nor about the values of other parameters in the equation (such as the mass of the object). Hence, prima facie $n = 1$ is not ruled out and the theory is applicable to a single macroscopic object. However, macroscopic objects consists of many microscopic objects and it now needs to be shown that the effective motion of n microscopic objects is the same as the one obtained from applying Eq. (12) to the macro object directly.

To this end GRW prove (1986, Section 6) that if we start with a system composed of n microscopic particles, described by Eq. (12), then the dynamics of the centre of mass of the system separates from its internal dynamics and is described by

$$\frac{\mathrm{d}\rho}{\mathrm{d}t} = -\frac{\mathrm{i}}{\hbar}[\hat{H}, \rho] - \lambda_{\mathrm{macro}}(\rho - T[\rho]), \tag{13}$$

where the index k has been dropped in T (i.e. $T[\rho]{:=}T_1[\rho]$) because there is only one object, the centre of mass. The relation between the macro- and the microfrequencies is given by

$$\lambda_{\mathrm{macro}} = \sum_{k=1}^{n} \lambda_k. \tag{14}$$

Assuming that all λ_k have the same value (see above) this reduces to: $\lambda_{\mathrm{macro}} = n\lambda_{\mathrm{micro}}$. Given that a macroscopic object is made up of about 10^{23} microconstituents, this implies: $\lambda_{\mathrm{macro}} \cong 10^7$.

This is the sought-after result: the equation describing the reduced dynamics of the centre of mass has exactly the same form as Eq. (12) and the value of λ_{macro} assures that the system's state, should it ever evolve into a superposition, is reduced almost immediately to one of its terms.

Requirement 3. The GRW formalism reproduces QM predictions (given by Born's rule) for measurements carried out on microscopic objects. As these have been confirmed to high degree, GRW is empirically adequate as regards microscopic systems. To show that the theory is also empirically adequate as regards the behaviour of macroscopic systems, GRW prove that the position and momentum mean values are not affected by the stochastic term in that they coincide with what Schrödinger evolution predicts: $\langle\hat{\mathbf{q}}\rangle_{\mathrm{S}} = \langle\hat{\mathbf{q}}\rangle_{\mathrm{GRW}}$ and $\langle\hat{\mathbf{p}}\rangle_{\mathrm{S}} = \langle\hat{\mathbf{p}}\rangle_{\mathrm{GRW}}$. Furthermore they prove that that Ehrenfest's theorem holds true for the GRW dynamics and that the expectation values for $\hat{\mathbf{q}}$ and $\hat{\mathbf{p}}$ follow the classical trajectories. These results are limited in scope because they are only valid for a free particle, but they provide evidence that the theory predicts the correct results at least in a simple case. Given that the relation between classical and QM is notoriously beset with riddles, this is not a bad starting point.

Before we begin our discussion of how to interpret the probabilities introduced in GRW theory, let us point out that its successes notwithstanding, this theory is not without problems. The dynamics of the theory does not preserve the required symmetries of wave functions describing systems of identical particles. Moreover it is an 'aesthetic' drawback

[6]Strictly speaking this is only true if we assume that the so-called tails problem can be solved, an issue that has been controversially discussed (see for instance Frigg, 2003). In what follows we assume that indeed it can be and that GRW theory provides a viable solution to the measurement problem.

of the theory that although the state reduction happens at the level of the wave function rather than the density matrix, the fundamental equation of the theory is expressed in terms of the density matrix; ideally one would like to have an equation governing the evolution of the state vector itself. Both these difficulties are overcome within the so-called CSL model (for 'continuous spontaneous localisation').[7]

The model belongs to the same family of proposals as GRW theory as it also solves the measurement problem by an appeal to spontaneous localisation processes and satisfies the other requirements as mentioned earlier in this section. The essential difference is that the discontinuous hits of GRW theory are replaced by a continuous stochastic evolution of the state vector in Hilbert space (similar to a diffusion process). Accordingly, the mathematical apparatus of the CSL model is different from that of GRW theory, but the leading ideas as well as the physical implications remain unaltered. For this reason we think that the following discussion of the interpretation of GRW probabilities *mutatis mutandis* carries over to the CSL model.

2. Probabilities in GRW theory—preliminary remarks

GRW theory belongs to a family of approaches to quantum theory that has been labelled 'quantum theories without observers'. These approaches renounce an appeal to observers to ensure that quantum objects have definite properties. In GRW theory this aim is accomplished by adding a stochastic term to the fundamental equation of the theory. As a result, probabilities are a basic aspect of the evolution of a physical system and do not in any way depend on there being observers who perform measurements—in fact the notion of a measurement does not appear in the theory at all. Ghirardi is explicit about this:

> I would like to stress that they [the spontaneous processes of localization in space] are to be understood as fundamental natural processes that owe nothing to interactions with other physical systems or to deliberate actions on the part of conscious observers. On the contrary, the idea is that the space-time in which physical processes develop exhibits some fundamentally stochastic, random aspects, which induce precisely the spontaneous localizations of the microscopic constituents of the universe. (Ghirardi, 2004, p. 406)

> […] no observer carries out any measurement: nature itself (Einstein's God?) chooses to induce such a process according to random choices but with precise probabilities. (Ghirardi, 2004, p. 409)

This feature of GRW theory rules out subjective probabilities as possible interpretations of the probabilities in GRW theory; these must be objective probabilities (or chances, as we shall say).

What are the options for understanding their nature? Philosophical reviews of the interpretive options regarding objective chances traditionally mention several possible accounts: the classical interpretation, logical probability, frequentism, propensity theories, Humean Best Systems accounts, and accounts that understand 'probability' as a theoretical term (see for instance Galavotti, 2005; Gillies, 2000; Howson, 1995; Hájek,

[7]The model was originally suggested by Pearle (1989) and Ghirardi et al. (1990). Bassi and Ghirardi (2003, Chapters 7 & 8) provide a comprehensive survey; short and less technical statements of the model can be found in Ghirardi (1997b), and Ghirardi et al. (1990).

2003; Mellor (2005)). For quantum mechanical probabilities in general, and GRW probabilities in particular, the first two options can be discounted immediately.

The remaining theories come in different variants. We will discuss each in turn, focussing on their ability to serve as an interpretation of GRW probabilities. Needless to say, each of these theories is open to various well-known objections, which need not be repeated here; we touch upon them only if the criticisms bear on the relevance of a particular account to probabilities in GRW, or, vice versa, if GRW bears on the criticisms. Among the issues that we cannot discuss here are in particular questions surrounding the ability of the different approaches to rationalise the so-called principal Principle (PP); for a discussion of these see, for instance, Hall (2004) and Hoefer (2007).

3. Frequentism

Frequentism is the view that the probability of a particular event *A* (getting heads when tossing a coin, say) is the relative frequency of *A*'s in a series of trials, i.e. the fraction of trials on which *A* occurs. Different versions of frequentism differ in how they flesh out this idea. Actual (or finite) frequentism takes the probability of *A* to be the relative frequency of *A*'s in a series of actual trials. Hypothetical frequentism associates probabilities with limiting relative frequencies within suitable infinite sequences of trials, presumably non-actual.

Frequency accounts (of any stripe) do not sit well with GRW theory, both for conceptual and technical reasons. As has been pointed out by many (among them the founding fathers of QM),[8] probabilities in QM refer to single cases. This is true in GRW theory as well, which gives us the probability for the occurrence of some particular event when the next hit occurs. In fact, GRW theory assigns probabilities to events no matter how often they actually occur. They may not occur at all, or only once. In cases of these sorts, the actual frequencies simply cannot match the quantum mechanical probabilities, nor even come close to them in general.

This closes the door on actual frequentism, but leaves hypothetical frequentism unscathed. The best formulation of hypothetical frequentism is von Mises' (1939).[9] His theory is based on the notion of a collective, an infinite sequence *S* of attributes selected from a finite or denumerably infinite set of attributes, satisfying the axioms of convergence and randomness (roughly, the first says that for each attribute the relative frequency of that attribute in *S* tends towards a finite limit, and the second requires that there is no recursively specified infinite subsequence of *S* in which this is not true and in which the relative frequencies differ from those in *S*). It can be shown that these axioms imply that successive members of a collective are probabilistically independent (Gillies, 2000, p. 106; Howson, 1995, p. 15). This condition, as von Mises himself emphasises, is often not satisfied if successive results are produced by the *same* device or system. Hence, a frequentist interpretation of the probabilities in sequences thus produced is only possible if one can prove that the dynamics of the system is such that subsequent events are indeed independent.

[8] See Galavotti (2001) for a survey.

[9] Of course, with some ramifications due to later writers; but none of these ramifications matters to our argument.

This is not the case for at least one of the two random processes involved in GRW theory. While the Poisson distributed occurrences of hits are independent,[10] subsequent localisation events are not. Let $H_{\mathbf{x}}$ be a hit with centre $\mathbf{x}$ and regard the $H_{\mathbf{x}}$ as the space of attributes of the frequentist's sequence.[11] Let t_1 and t_2 ($t_2 > t_1$) be the two instants of time at which two consecutive hits occur and assume that the hit at t_1 leaves the system in a state centred around point $\mathbf{x}_1$. Then consider the probability that the second hit is centred around $\mathbf{x}_1$ as well, $p(H_{\mathbf{x}_1}$ at $t_2|H_{\mathbf{x}_1}$ at $t_1)$. Some calculations using Eq. (2) soon reveal that $p(H_{\mathbf{x}_1}$ at $t_2|H_{\mathbf{x}_1}$ at $t_1)$ equals almost one, while $p(H_{\mathbf{x}_2}$ at $t_2|H_{\mathbf{x}_1}$ at $t_1)$ is vanishingly small, where $\mathbf{x}_2$ is a point more than $1/\sqrt{\alpha}$ away from $\mathbf{x}_1$. By the same token, $p(H_{\mathbf{x}_2}$ at $t_2|H_{\mathbf{x}_2}$ at $t_1)$ equals almost one, while $p(H_{\mathbf{x}_1}$ at $t_2|H_{\mathbf{x}_2}$ at $t_1)$ equals almost zero. Hence, the different $H_{\mathbf{x}}$ are not probabilistically independent. And they had better not be! The absence of independence is what guarantees the regular behaviour of macroscopic objects. If the pointer is at $\mathbf{x}$ after the interaction of a measurement device with the system, we expect it to stay there. Independence would imply that macroscopic objects would jump around randomly, hardly something that an empirically adequate theory can predict. For this reason consecutive hits do not form a collective and von Mises' scheme is inapplicable to GRW theory. Given that von Mises' scheme (suitably ramified) is the best frequentist game in town, this leaves the frequentist with empty hands.[12]

The frequentist might now counter that this objection is spurious because it builds on a mischievous choice of the attribute set. The relevant attributes are not, so the objection goes, the hits $H_{\mathbf{x}}$ themselves, but the shape of the wave function at the instance of a hit.

This suggestion does not further the frequentist's cause. Because of the way the hit mechanism is defined—a hit amounts to multiplying the pre-hit wave function with a Gaussian centred around $\mathbf{x}$—the wave function always bears traces of its entire history and is not 'reset' in the way it is after a von Neumann collapse. As a consequence, the system never has *exactly* the same wave function at two different instants of time between which at least one collapse has occurred.[13] Hence, attributes in this new attribute set never recur. This stands in contradiction to von Mises' requirement that each attribute has to recur

[10]Poisson distributed events are independent in the following sense: the number of events in two disjoint (i.e. non-overlapping) intervals are independent random variables that follow themselves a Poisson distribution.

[11]Notice that there is a further problem at this point. Von Mises' definition of a collective requires that the set of attributes be finite or denumerably infinite, but the set of all $H_{\mathbf{x}}$ is non-denumerable because $\mathbf{x}$ ranges over R^3. However, we think that this problem can be solved by either suitably redefining a collective or discretising space.

[12]von Mises (1939, Chapter 6) discusses sequences that do not satisfy the axiom of randomness and formulates a procedure to reconstruct them as a combination of two sequences that are collectives. In this way, he argues, his theory is applicable to (at least some) sequences that are not collectives. However, the procedure he outlines is not applicable in the case of GRW because it involves a probability distribution over initial conditions that has simply no place in GRW theory.

[13]*Proof*. Let $|\psi_0\rangle$ be the wave function of the system at some (arbitrary) instant. The claim that the shape of the wave function is repeatable amounts to claiming that for some number of hits $m > 0$: $|\psi_0\rangle = |\psi_m\rangle := (1/N_m)\hat{L}^{k_m}_{\mathbf{x}_m} \ldots \hat{L}^{k_1}_{\mathbf{x}_1}|\psi_0\rangle$, where $1/N_m$ is a normalisation constant. This is possible under two circumstances: either (a) all $\hat{L}^{k_i}_{\mathbf{x}_i}$, $i = 1, \ldots, m$, are the identity function; or (b) the result of the multiplication of the $\hat{L}^{k_i}_{\mathbf{x}_i}$, $i = 1, \ldots, m$ is the identity function. However, GRW theory stipulates that the hit functions are Gaussians and thereby rules out that either of these conditions can be true: (a) the identity function is not a Gaussian and therefore not admissible; (b) the multiplication of any number of Gaussians never yields the identity function. Hence the shape of the weave function is not repeatable.

infinitely many times. Again, we have to conclude that GRW theory cannot be squeezed into the frequentist's corset.

Finally, let us briefly address the question of why frequentism has at least some initial plausibility in standard QM while it is so fundamentally at odds with GRW theory. The reason for this is that the events that are meant to form the collective (and fail to do so in the case of GRW theory) are entirely different in the following way. In the context of standard QM one considers quantum systems prepared in a well-defined state $|\psi\rangle$, which is then measured. It is then (usually more or less tacitly) assumed that we either have a large collection of systems all prepared in this state, or, if we make repeated trials with the same system, that the system is prepared in state $|\psi\rangle$ before every measurement. Of course, these measurements are independent. GRW hits are completely different. There is no 'time out' between hits to 'reset' the state; hits occur and they act on whatever the system's state is in the aftermath of the previous hit and the unitary evolution between two hits. The theory does not leave any room for the kind of state preparation that is presupposed when a frequency interpretation of standard QM probabilities is given.[14]

4. Humean best system accounts

In a series of papers Lewis (1980, 1986, 1994) developed a novel Humean approach to objective chance, variations of which have been suggested by Loewer (2001, 2004) and Hoefer (2007). We call this family of accounts 'HBS views', for 'Humean best system'. What the members of the family have in common is the Humean stance, a kind of nominalism with respect to chances that eschews irreducible modalities, powers, necessary connections and so forth, and the claim that while the objective chances supervene on the patterns to be found in the actual events making up the world's history (the 'Humean mosaic'), they do not supervene *simply*, as for instance is the case with actual frequentism.

Lewis' account (1994) is in fact a proposal for how to understand laws of nature as well as objective probabilities. Lewis invites us to consider all deductive systems that make true claims about the Humean mosaic, and, perhaps, also make assertions about the probability of certain events happening in certain circumstances. A contingent generalisation is a law if and only if it appears as a theorem or axiom in the best system (or all the best systems if there are several equally good systems). If it happens that the best system includes laws giving probabilities for various types of events, rather than only strict universal generalisations, then the objective chances in our world are just what those laws say they are (Lewis, 1994, p. 480). In what follows we will refer to chances thus defined as 'HBS chances'.

The best system is the one that strikes the best balance between simplicity, strength and fit. The latter notion is specific to Lewis' account and therefore needs introduction. A theory that assigns chances to events also assigns a chance to certain courses of history, among them the actual course of history. The fit of a theory is defined to be that chance; that is, the fit of a theory is the likelihood that it assigns to the actual course of events. By stipulation, systems that do not involve chances have perfect fit. From this it follows that a

[14]One might try to salvage a frequentist interpretation by claiming that this sort of independence is available in GRW theory as well: the collective, by definition, is a set of systems prepared in the same quantum state, which then are hit under the dynamics of the theory. This, however, is only possible if we allow for collectives that do not have more than one actual member, the rest being fictional entities. Building a frequency interpretation on such a collective seems patently absurd.

theory T_1 has a better fit than a theory T_2 if the probability that T_1 assigns to the actual course of history is greater than the probability that T_2 assigns to it. As an example consider a Humean mosaic consisting of a finite sequence of 10 coin tosses: HTHHTHTTHT. Theory T_1 says that the chance of getting heads is 0.5; theory T_2 says that the chance of getting heads is 0.1. The probability that T_1 assigns to the actual course of history is greater that the probability that T_2 assigns to that history: $0.5^{10} > 0.1^5\,0.9^5$ Hence T_1 has better fit than T_2.

Can GRW probabilities be interpreted as HBS chances? The issue we need to address first is what the Humean mosaic consists of. While Lewis did not specify what exactly the Humean mosaic in fact contains in our world, he did insist that it be Humean in the sense of not involving, intrinsically, any necessary connections between distinct regions. He suggested that this requirement is best met by an ontology based on space–time points plus local field quantities representing material stuff (e.g. electromagnetic fields, perhaps mass and charge densities, and so forth). First appearances notwithstanding, this squares rather well with GRW theory. The theory is formulated against the background of a classical space–time, which is in line with Lewis' position. However, there are questions about the existence of relevant local field quantities. The theory's basic object, the wave function, exists in a $3n$-dimensional configuration space, whereas the relevant classical space is three-dimensional. Whether this makes for a serious mismatch depends on how one interprets GRW theory.[15] One possibility is to view GRW theory as a 'wave only theory', i.e. as a theory whose basic ontology consists of the $3n$-dimensional wave (such a position is suggested, for instance, in Clifton & Monton, 1999). On the basis of such an interpretation it would indeed be difficult to define a Humean mosaic along the lines suggested by Lewis (although, perhaps, P. Lewis, 2004 provides a remedy). However, there are other interpretations of the theory which do not give rise to difficulties of that sort. As Bell (1987, pp. 204–205) pointed out, although the wave-function 'lives' in a $3n$ dimensional Hilbert space, the GRW hits are localised in ordinary three-dimensional physical space in that each is centred around a particular point $\mathbf{x}$. Two interpretations in particular make this fact palpable: the mass density interpretation (see Ghirardi, Grassi, & Benatti, 1995; Ghirardi, 1997c; Monton, 2004) and the flash interpretation (which was somehow alluded to by Bell, 1987 and which was then worked out by, among others, Tumulka, 2006). The former introduces a continuous matter density in three-dimensional physical space, whose shape is determined by the wave function. Accordingly, a hit amounts to a localisation of the mass density around the point at which the hit occurs. On the flash interpretation, the primitive ontology of the theory consists of flashes, which are localised at the points where a hit occurs; an object then is understood as nothing but a swarm of such flashes. We have some reservations about the metaphysical plausibility of the flash interpretation (how do ordinary objects 'emerge' from a swarm of flashes?), but this need not occupy us here. What matters in the current context is that both the matter density interpretation and the flash interpretation give rise to a Humean mosaic of the kind that Lewis envisaged. The mass density is a field which is defined at every point $(\mathbf{x}, t)$ of a four-dimensional space time. This is exactly what Lewis envisaged and hence the Humean mosaic of GRW theory with a mass density interpretation can be defined exactly as suggested in Lewis' original account. On the flash interpretation, the Humean mosaic is a 'pointilist picture' consisting of the flashes occurring at certain $(\mathbf{x}, t)$.

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[15]For a discussion of the problem of interpreting GRW theory see Lewis (2005).

Assuming that the mosaic is as just described, does GRW theory qualify as the Humean best system? Let us discuss each requirement in turn. First, is GRW theory Humean? Yes, it is. Hits are occurrent events and the theory does not make reference to any hidden powers or mechanisms explaining these occurrences, which would be unacceptable from a Humean perspective. GRW themselves are explicit about this: 'we do not consider […] the problem of physical origin of these localizations for microscopic systems […], but we simply postulate that they occur. In this sense we say that they are spontaneous' (1986, p. 471).

Second, is GRW a system in the relevant sense? The answer to this question is less straightforward. A system in Lewis' sense encompasses all (or at least all basic) sciences; i.e. it is total science. GRW theory is not a system of that kind. So, strictly speaking HBS is not applicable to GRW. There are two responses to this problem. (a) One can argue that although GRW theory itself is not a system of the kind required, every system of that kind (present or future) needs to incorporate GRW theory, or something very much like it (we mentioned further developments in Section 1). The main obstacle for this take on the matter is that, orchestrated efforts notwithstanding, no generally accepted relativistic version of a GRW type theory has been formulated yet. As Ghirardi admits, this is a serious problem and unless a relativistic version can be formulated the programme cannot be regarded as providing a true fundamental theory (Ghirardi, 2004, pp. 419 and 436). However, progress is being made (e.g. Tumulka, 2006) and there are reasons to remain hopeful that a relativistic spontaneous localisation theory will eventually be forthcoming and that GRW theory can be understood as part of a best system in Lewis' sense. (b) In Hoefer's (2007) version of HBS no system of the 'total science' variety is required. He argues that the HBS criteria can be applied to individual theories irrespective of whether or not they form part of an all-encompassing system. From this point of view there simply is no question of whether GRW theory is a *system* of the right kind; it is a theory about non-relativistic objects, and that is all we need. We regard either of these responses as reasonable and therefore conclude that GRW theory falls within the scope of HBS theories of chance.

Third, is GRW theory the *best* system in the sense that it strikes the best balance between simplicity, strength and fit?[16]

(a) Fit. There are two questions: (i) Is Lewis' notion of fit applicable to the random processes postulated by GRW theory? (ii) If so, how good is the fit? The first question is best answered by looking at each of the random processes in turn. The occurrence of hits is governed by a Poisson distribution. This distribution gives the probability for there being a certain number of hits in a particular interval of time. This interval can be chosen to be the unit interval, in which case Eq. (4) gives us the probabilities for one hit, two hits, etc. to occur in each unit time interval. This is exactly the kind of information we need to apply to the above notion of fit: we look at consecutive unit intervals, count how many hits occurred, calculate the probability of the actual history of the system and compare it with what alternative theories would say. If GRW's Poisson distribution assigns a higher probability to the actual history than its competitors it has better fit. In the case of localisation process things are less straightforward. Eq. (2) is a probability *density* and hence the probability of there being a hit exactly at $\mathbf{x}$ is always zero and accordingly all

[16] To be more precise, the question either is whether GRW theory forms part of the best system if you stick with Lewis' original proposal, or whether it is the best theory about its own domain if you side with Hoefer's views.

possible histories have zero probability.[17] A possible solution is to put a grid on space (i.e. coarse grain space) and look at the (finite!) probabilities that the centre of collapse is within a certain cell of the grid. As these cells can be chosen arbitrarily small (as long as they have finite measure), the shift from a continuum to a grid in order to judge fit does not seem to be problematic. In this way we get the finite probabilities of discrete localisation events that we need in order to apply Lewis' notion of fit.[18]

With this in place, we can now turn to the second question and ask how good the theory's fit is. There is no direct way to tell because GRW hits *per se* are unobservable and experimental results are our only basis to come to a judgement about how good the fit of a theory is. GRW theory reproduces the predictions of standard QM, at least within the range of experimental testability.[19] Given that standard QM is highly successful in the sense that its probabilistic predictions match the measured frequencies perfectly, GRW theory is equally successful. If we now assume that hits indeed do exist (this is a substantial 'if', we shall come back to this point below), then we have good reasons to believe that the actual hits match the theoretically postulated ones rather closely; if they did not, we would see experimental violations of basic predictions, which we do not.

(b) Strength. As we just mentioned, the theory reproduces the predictions of standard QM, which is, from an instrumental point of view, a highly successful theory with a large set of consequences which, so far, were all empirically confirmed. Hence, GRW theory is on par with standard QM in terms of strength, which makes it a very strong theory.

(c) Simplicity. GRW theory does not get the highest scores when it comes to simplicity, as standard QM is arguably the simpler theory. However, since standard QM is beset with a serious conceptual problem, the measurement problem, it is not in the race for the best system at all. So the question is whether GRW theory is simpler than other serious contenders. This is difficult to judge because GRW so far is the only game in town (other theories of the same type can be shown not to be empirically adequate or suffer from other serious problems; see Bassi & Ghirardi, 2003 for a survey). Hence GRW wins by default, as it were.

Hence, we conclude that if hits actually do occur, then GRW theory qualifies as the best system and the probabilities occurring in it can be interpreted as Humean objective chances. However, hits of the sort postulated by GRW theory (and that we assumed to be part of the Humean mosaic) are unobservable and it is therefore debatable whether we should assume the Humean mosaic to include them. If we decide that we should not, then matters open up. Now if, for example, point particles actually exist and their continuous trajectories form part of the mosaic, then presumably Bohmian mechanics, or something like it, strikes the best balance between strength and fit. However, so far there is no experimental evidence telling against one or the other view of what the Humean mosaic consists of and as long as this is the case GRW theory is a serious contender for the best system, and its probabilities can be understood as objective Humean chances.

[17]This problem is not specific to GRW; it also crops up in standard QM.

[18]In a recent paper Elga (2004) has pointed out that Lewis' notion of fit fails to be informative in systems with infinitely many random events. As time is unbounded in GRW theory it falls within this category. However, the solution that Elga suggests also works for GRW theory and hence this problem need not concern us here.

[19]For instance, there are differences in what the two theories predict about superconductors, but the effects are so small that they cannot be detected (Rimini, 1995). See Ghirardi (2001) and Benatti, Ghirardi, and Grassi (1995) for a general discussion of GRW and experiments.

5. Propensity accounts

While the propensity view of objective probabilities can be traced back at least to C. S. Peirce, it has enjoyed an unbroken chain of advocates in more recent times largely because of the work of Karl Popper, who reintroduced the view in philosophy of science precisely to provide an interpretation of probabilities in QM. A number of authors have offered views that deserve to be called propensity views, even though some reject the label itself; a partial list would include Mellor (1971), Giere (1973), Fetzer (1981), Humphreys (1989), Miller (1996), and Gillies (2000).

What all propensity views have in common is the attribution of dispositions or tendencies to chancy systems, dispositions that are in some sense quantified by the objective probabilities we attribute to such systems. This attribution is meant to be taken in a strongly realist fashion as this tendency is regarded by its proponents as an 'ingredient' of reality (Hall, 2004), and certainly is held not to be reducible to Humean, purely occurrent, facts. So two consequences are commonly shared by propensity theorists: (a) if the world is governed by deterministic laws, then there are in fact no propensities; (b) two possible worlds might coincide completely concerning the Humean mosaic of facts and events, yet have different propensities. Typically this claim is motivated by having us consider two possible worlds in which the occurrent facts are the same, but (we are told) different probabilistic laws govern the two worlds.

Propensities are dispositions, but dispositions to what? There are two ways of answering this question, and hence two main types of propensity theory. *Single case* propensity theories say that propensities are non-surefire dispositions to produce outcomes in trials or instantiations of the setup. So, for example, a two-dice-rolling setup will have a tendency of strength $\frac{1}{36}$ to produce the outcome double-six. These tendencies may be thought of as analogous to forces, though forces that do not always succeed in 'pushing' the system in the direction they point. *Long run* propensity theories deny that this is the right way to think of the setup's propensities; instead, they say that the setup has a tendency to produce double-six with a frequency of approximately $\frac{1}{36}$, when a *long series of trials* is performed.

In the context of GRW theory the long run propensity view is a non-starter. The aim of this view is to explain the long run frequencies, which the frequentist takes as a given, by grounding them in specific properties of the setup, guaranteeing that certain long run frequencies would be produced if trials were indefinitely extended. However, as we have seen in Section 3, in a world governed by GRW theory there are no sequences of hits involving precisely the same outcome-attributes for which limiting frequencies could be defined and subsequent events are not probabilistically independent, which precludes a frequentist understanding of GRW probabilities. Hence there is simply no explanandum and long run frequentism becomes obsolete.

By contrast, single case propensities seem to be a very natural interpretation of GRW probabilities.[20] For one thing, the textual explanations that accompany the equations of most presentations of GRW theory—and such texts always play a critical role in establishing a theory's content; no physical theory is just its equations—are very naturally

[20] Milne (1985) presents a neat argument for the conclusion that propensities, as expounded by Popper, cannot explain the two slit experiment, and quantum behaviour more generally. However, the problem seems to lie with Popper's particular version of propensities and not with propensities *per se*. For a further discussion of Milne's argument see Suárez (2004), and for a discussion of quantum propensities in general, Suárez (2007).

read as ascribing inherent tendencies to collapse to the wave functions of *individual* quantum systems (see for instance the Ghirardi quote in Section 2); there is no talk of what statistics one should expect to find after repeated measurements of identically prepared systems. Moreover, physicists impose no constraints on what sorts of possible worlds should be taken seriously. We may discuss lone-particle worlds and few-particle worlds.

This impression bears out when we look at the details of the theory. The GRW dynamics incorporates two coupled random process; they are coupled in the sense that one provides the trigger for the other. When a hit occurs, the system chooses a hit centre according to Eq. (2) and the state changes according to Eq. (1). The probability of the next hit being centred around $\mathbf{x}$ depends on the shape of the wave function immediately before the hit and, as we have seen in Section 3, each hit is unique in the sense that the same wave function never recurs. So it seems natural to say that for every possible localisation event $H_{\mathbf{x}}$ the wave function has a (single case) propensity to undergo this particular localisation (assuming, as before, a discretisation of space to guarantee these probabilities a finite non-zero value).

The occurrence of a localisation can be understood along the lines of tossing a coin, where the occurrence of a hit plays the role of the landing of the coin. But what triggers that hit to occur? There does not seem to be a triggering condition of the same kind present. Indeed there is not; but none is needed. Not all propensities need to have triggering conditions of the kind we find in the case of the coin flip. Consider Miller's example of his probability for survival 1 year from today, which he explains as the 'propensity for today's world to develop in a year's time into a world in which I am still alive' (Miller, 1994, p. 189). This propensity need not be triggered by anything; the world today just has the propensity develop in this particular way. If anything, this propensity is conditional on the entire state of the universe now, which is not a trigger in the way a throw is a trigger when throwing a dice. The occurrence of hits according to GRW theory follows the same pattern. There is a chance of $\lambda\,\mathrm{d}t$ for each elementary constituent to decay during $\mathrm{d}t$ and all that is needed for this is that the thing *is* an elementary constituent because it is, according to the theory, a fundamental aspect of such constituents that they undergo hits with probabilities given by the theory. Hence also the second random process postulated by GRW can be understood on the basis of the single case propensity view.

What about the canonical objections raised against single-case propensities? Some of these have little or no bite in the GRW context. The reference class problem does not arise in any variant of QM. Once a system's quantum state is specified, the probabilities for all relevant events are fixed and GRW theory itself tells us that no further facts about the system are relevant to its chances of doing this or that. Hence, GRW theory rules out any reference class problem.

Humphreys' paradox takes to task the propensity theory—taken as an interpretation of *all* objective probabilities—for the oddity of temporally backward-looking probabilities of the sort that Bayes' theorem often lets us calculate.[21] It simply does not seem right to ask what is the propensity of a coin to be tossed given that it has come up heads. But the

[21]Humphreys' original paradox, properly speaking, is an argument to the effect that propensities cannot be probabilities, because if they are so regarded one can derive contradictory conclusions. Humphreys (1985) uses a quantum-mechanical setup to derive the paradox. Humphreys' own view is that some probabilities do represent causal propensities, but that causal propensities *per se* cannot be probabilities in the sense of satisfying all the axioms and theorems of the probability calculus. For a recent discussion, see Humphreys (2004).

advocate of a propensity account of GRW probabilities is under no obligation to say that *all* objective probabilities are single-case propensities. Instead she can assert that GRW propensities are all forward-looking in time; and should someone calculate backwards backward-looking probabilities these would have to be understood as subjective probabilities grounded objectively on GRW probabilities. This is a response that any advocate of objective quantum probabilities will wish to make; it is a remarkable fact of quantum theory that the probabilities directly given in the theory (using the Born rule), as well as the GRW hit probabilities, are always forward-looking.

In sum, single case propensities are not only a natural interpretation of GRW probabilities, the context of GRW theory also makes it possible to dispel some well-known objections to this interpretation.

6. Probability as a theoretical concept: the no-theory theory

Despite a long history of successful use of probabilities in many sciences, there has never been a clear consensus in support of one of the traditional philosophical theories of probability—not even within a single scientific context. In light of this record of failure, it is natural that some philosophers have come to question whether we are right to *try* to come up with an interpretation of probability in terms of other concepts. Rather than looking to explain objective probability, or chance, in terms of something else, perhaps we should take it as a new, *sui generis* theoretical concept, for which we can have at most an implicit definition, provided jointly by the mathematical axioms (e.g. Kolmogorov's) and by the concept's uses in various scientific theories. The most recent advocate of this theoretical-concept approach is Sober (2005), and he calls his view the 'no-theory theory' (NTT from here on). He describes the view as follows:

> In view of the failures of these interpretations, my preference is to adopt a *no-theory theory of probability*, which asserts that objective probability is not reducible to anything else. Frequencies provide evidence about the values of probabilities, and probabilities make (probabilistic) predictions about frequencies, but probabilities don't reduce to frequencies [...]. Instead, we should view objective probabilities as theoretical quantities. With the demise of logical positivism, philosophers abandoned the idea that theoretical magnitudes such as mass and charge can be reduced to observational concepts that are theory-neutral. We should take the same view of objective probabilities.
>
> If we reject the need for a reductive interpretation of objective probability, what does it mean to say that a probability is objective? Taking our lead from other theoretical concepts, we can ask what it means to say that mass is an objective property. The idea here is that mass is a mind-independent property; what mass an object has does not depend on anyone's beliefs or state of mind. The type of independence involved here is conceptual, not causal—it is not ruled out that an object have the mass it does because of someone's beliefs and desires. The next question we need to ask is epistemological—what justifies us in thinking that mass is an objective property? If different measurement procedures, independently put to work by different individuals, all lead to the same estimate of an object's mass, that is evidence that mass is an objective property. The matching of the estimates is evidence that they trace back to a common cause that is 'in' the object [...]. (2005, p. 18)

We do not think that NTT succeeds in providing a new account of chance that brings the endless disputes over the correct interpretation of objective probability to an end. At least within the context of GRW theory (and QM more generally), NTT collapses either into a propensity view or the HBS account, depending on how theories and laws are understood by the NTT advocate.

If theories and laws are understood in some sort of empiricist way, as simple/elegant summaries of actual regularities, then the NTT view of quantum probabilities is basically the same as the HBS account. Objective chances are defined by the role they occupy in the theoretical framework that provides a simple/elegant systematisation of matters of fact. By contrast, if the NT theorist embraces a non-empiricist view of laws such as those of GRW, then the view becomes a variant of the propensity view. This could go in one of two basic ways. First, if the NTT theorist stresses the analogy with mass and insists that the objective probabilities found in the theory's equations correspond to some genuine dispositional property 'out there' in individual physical systems, then the view becomes a variant single-case propensity view. Or, second, if the NTT theorist does not take this dispositional-grounding route, but nevertheless adopts a strong metaphysical view of laws of nature (e.g. the necessitarianism of Dretske, 1977; Armstrong, 1983), then the view becomes a variant of a propensity view in which the 'tendency' or power inheres in the laws themselves, or perhaps in 'the world' as a whole (as in the accounts of Miller and the later Popper, 1990). Sober (2005) does not take a clear stance on the nature of laws found in theories such as GRW, so it is not easy to say whether his view will be in danger of collapsing into a variant of HBS or a variant of the propensity view. He appears to want to steer a course between the two, but we feel this only appears possible because the account is left vague concerning the nature of laws.

In sum, whatever its merits in other disciplines, within the context of GRW theory NTT does not offer anything over and above the options already discussed.

7. Conclusion

Our examination of how the viable theories of objective probability fare in the context of GRW theory has left us with just two candidates standing: HBS and single case propensities. This is as far as physics takes us in this debate; when adjudicating between the last two contenders we have to draw on conceptual resources.

Proponents of HBS accounts argue that they enjoy two (closely related) advantages over single case propensities. The first is that HBS accounts can rationalise the so-called principal principle (PP), roughly the proposition that our subjective probabilities for an event to happen should match what we take to be its objective chance (see Lewis, 1980; Hoefer, 2007). The second has to do with frequency tolerance, i.e. the ability of an account of objective chance to accept the possibility that the actual relative frequencies of chance-governed events be different than the objective chance itself. Single case propensities have unrestricted frequency tolerance because, first, *any* sequence of physically possible events, no matter how 'improbable', is logically compatible with the propensities ascribed by GRW and, second, the 'true' single-case propensities governing particles in our world might be radically different from what GRW say they are, the apparent agreement of the latter with observations being merely a *highly* 'improbable' accident. This problem is avoided in HBS accounts, which incorporate a significant but not unlimited amount of frequency tolerance. On the one hand the requirement that fit be maximised assures that the chance of an event is as close as possible to its relative frequency because the closer the chances are to the relative frequencies that better the fit of the theory. On the other hand, HBS accounts allow for a certain mismatch of frequencies and chances if this mismatch is compensated by a gain in simplicity and/or strength.

In our view, that gives HBS accounts the lead position in the race for the most plausible account of GRW probabilities. However, whether or not one regards the ability to rationalise PP and limited frequency tolerance as arguments in favour of an HBS account depends on one's philosophical commitments. Contrary to our convictions, some believe that frequency tolerance actually is a *good* thing because there is no logical connection between chances and relative frequencies. For someone of this persuasion there simply is no problem here and the limited frequency tolerance of HBS accounts would not go into the books as an advantage. Similarly, the sceptic about HBS points out that one of the account's alleged crown jewels, its ability to justify PP, is, upon close examination, beset with impurities. For one thing, proponents of propensity views are prepared to regard PP as something like an analytical truth which, as such, simply is in no need of further justification. For another thing, they point out that the HBS justification of PP is not free of problems either (Strevens, 1999; but see also Hoefer, 2007).

There are further objections that are typically raised against the HBS account. Many of them are based on intuitions that go against the basic character of the approach. For instance, Humean chances are radically non-local: what the chance of a particular outcome is in *this* system, at time t, is not grounded in facts about this system and its immediate environment alone, but instead in a huge variety of occurrent facts, spread all over space and time.

This, however, need not worry the proponent of an HBS account too much. Whether or not one shares Humean intuitions may well be a matter of philosophical taste, and, especially in the context of QM, a critic can not really get much mileage out of the fact that a theory is non-local. Of course, the non-locality of HBS chances is of an entirely different nature than the non-locality brought about by the QM formalism; events elsewhere affect the objective probability of events here and now in a logical way that has nothing to do with physical interactions of any kind. But there is no reason think that this logical non-locality is worse than quantum non-locality, which is what everybody has to deal with. Quite the opposite. Logical non-locality is perfectly normal: little James becomes an orphan instantly when is parents die in a car crash at the other end of the world. This is the kind of non-locality the HBS theorist has to accept as a result of his theory of laws, and this seems benign enough.

Finally, it is worth noticing that an HBS approach need not deny the existence of propensities (or dispositions or tendencies relevant to the production of outcomes) *per se*; she may be agnostic about the existence or non-existence of propensities, and merely insist that either way, they are not what makes the *objective chances* be what they are. In any world in which the true propensities radically mismatch the produced frequencies, the HBS chances will still exist and still be apt for guiding expectations, while the true/hidden propensities (should anyone somehow come to know them) would not be. Thus a Humean approach to chance is, unsurprisingly, well suited to philosophers of QM who are metaphysically cautious.

In sum, it seems to us that the HBS account currently looks like the more convincing option. However, the final jury in this question is still out and it remains to be seen whether the propensity theorists can substantiate their take on PP and frequency tolerance.

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