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What is Probability?

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What is probability? Physicists, mathematicians, and philosophers have been engaged with this question since well before the rise of modern physics. But in quantum mechanics, where probabilities are associated only with measurements, the question strikes to the heart of other foundational problems: what distinguishes measurements from other physical processes? Or in more formal terms: when are the unitary dynamical equations suspended in favour of probabilistic ones?

This is the *problem of measurement* in quantum mechanics. The most clear-cut solutions to it change the theory: they either add hidden variables (as in the pilot-wave theory), or they give up the unitary formalism altogether (as in state-reduction theories). The two strategies are tied to different conceptions of probability: probability as in classical statistical mechanics (as formulated by Boltzmann, Gibbs and Einstein), and probability as in Brownian motion (with the dynamics given by a stochastic process, as formulated by Einstein and Smulochowski). The former is sometimes called *epistemic* probability, as classical mechanics is deterministic: probabilities arise as a consequence of incomplete description. It is the latter, stochastic, probability that is usually thought the more fundamental, as it enters directly into the fundamental equations.

Indeterminism encouraged a late-comer to the philosophy of probability, Popper's *propensity* approach [12], in which probabilities are identified with certain kinds of properties - "dispositions" - of chance set-ups. But whether thought of in terms of incomplete descriptions, or as propensities, these kinds of probability have puzzled philosophers. By way of contrast, probability as *degree of subjective expectation*, or *subjective weighting*, is much clearer: if that is what probability is one can explain why it obeys the rules that it does (I shall come back to this point later). Neither the epistemic probabilities of classical statistical mechanics, nor the objective probabilities of a stochastic dynamics as in Brownian motion, are happily thought of as subjective expectations. They are grounded, surely, in facts about physical states of affairs, independent of persons altogether. It is true that epistemic probability is often thought of in terms of ignorance, or incomplete knowledge, but it is precisely this link with purely subjective considerations that is so puzzling from the point of view of its role in thermodynamics: heat transfers, surely, take place according to thermodynamical laws, independent of whether anyone is looking, and of what anyone knows. The point is all the more evident in the case of stochastic probability. These epistemic and stochastic probabilities of physics are objective rather than subjective quantities. What, then, are the physical facts that make true some probability statements but not others? What are we trying to get right about when we make judgements of objective probabilities? It is not informative to say it is that there are *chances* that are thus and so; the difficulty, that has long bedevilled the philosophy of probability, is to say what chances could possibly be.

There are well-known, failed, candidates for the role. The most commonplace looks to the *evidence* for probability statements, the observed statistical data. Of course if the question is what chances are, rather than what we believe them to be, we should consider unobserved data as well; in the most simple-minded approach, these two are simply identified - specifically, chances are identified with *long-run* relative frequencies. But the objections to doing this are obvious. How long is long enough? In the short-run we cannot expect chance and relative frequencies to line up exactly, whilst the infinite limit is never actually reached; so what, precisely, are the chances? Presumably a function of the number of trials and the actual relative frequencies; but what function?

One can prove that for repeated trials the relative frequencies of outcomes will approximately match the probability of each outcome (assumed independent), but only in the sense that if $p(n, \delta)$ is the probability that they differ by more than $\delta > 0$, for n trials, then $\lim_{n\to\infty} p(n, \delta) = 0$ (the *law of large numbers*). But this is not to identify probabilities with any actual relative frequencies; so long as n is finite, we still have to do with probabilities (the quantities $p(n, \delta)$). What this and the various other laws of this kind show, rather, is that the concept of probability enters into the very relation between statements of evidence and statements about chance; statistical evidence bears out a probability claim only to some degree - where "some degree" is some degree of probability.

Could the latter probability be subjective rather than objective probability? It could - but only if we assume that subjective probability, at least in such cases, agrees with chance: for we calculate the probability for matching statistics with chances by using those same chances. What underwrites this assumption? This is a question that arises even if one knew what chances really are; why should subjective expectations be set equal to objective probabilities? Put to one side worries about long-run frequencies not being long enough; why, given that the long-run frequency of some outcome is thus and so, should we subjectively expect that outcome to that degree on the very next trial?

We have two questions:

1. What, physically, is objective probability (chance)?

2. Why should subjective probability track chance?

Because of its importance in the philosophy of probability and of rational action, the principle that subjective probability should track chance - or the slightly more involved principle that it should, however much additional information one may have, short of knowledge of what the actual outcome will be, has been dubbed the *principal principle* [10]. We are sure this principle is true, but we are at a loss to say why. Indeed, failing an account of what chances are, it is hard to see how the principle *could* be justified; for it ought to be facts about physical states of affairs that dictate our subjective expectations of future contingencies. What are those facts? The two questions are interdependent.

One can rest on the authority of science. One can say that it is a requirement of any theory of rationality that our beliefs should be based on our best scientific theories. One can take it as an extension of this that we should tailor our subjective expectations of chance outcomes to whatever our best theory says are their chances; the principal principle would then be part of any theory of rational action worthy of the name. Maybe so, but then it will be an unexplained part. It is also a very large part of the theory of practical reasoning, perhaps the largest part (the part that deals with physical contingencies). One would, in the light of our best theories in physics, like to do better. The challenge is to say what it is about the world that makes statements of objective probability true, and why, given such states of affairs, we should act accordingly, with subjective probabilities fixed by the objective ones.

Questions (1) and (2) are now the most important ones in the philosophy of probability. It was not always so; Popper, when he proposed to abandon the link between long-run frequency and chance, wanted an account of probability that made sense of the single-case and that made no reference to human knowledge. Worthy aims; but to suppose that the chance of an outcome of an experiment is a "disposition" of that experiment (and more generally, that the chance of an event given a certain chance set-up is a "disposition" of that set-up) in itself solves very little; for what, physically, are these "dispositions"? Popper was never able to say in terms of categorical physical properties - properties that are not themselves "chancy" or equally in need of explanation. Nor was he able to in classical physics. Even in classical games of chance, where chances are directly related to the symmetries of a chance set-up - the six faces of a die, the two sides of a coin - dynamics comes into it. Throw the die or the coin just so, and the statistics, the evidence for the chances, can be anything you like. The dynamics, it seems, can override chance.

This worry would seem to arise in any deterministic theory. It is made marginally more palatable by putting it in terms of initial conditions rather than dynamics (that certain initial conditions are "typical"). Better still, adopt the picture of a probability distribution as a measure on an ensemble (usually infinite) of hypothetical physical systems, a picture in which dynamics is absent altogether. One can understand its appeal, but only from this narrow perspective. What it is about the actual world that makes the probabilities what they are is never explained. Why we are supposed to have expectations about the real world because of the values of a measure on a fictitious ensemble of imaginary worlds is never explained.

(1) is evidently a hard question, but it is at least a physical question; it is not so clear that (2) is. It would be understandable were physicists to limit themselves to (1), to be answered by physics as usual (as in saying what temperature really is, or what solidity really is); (2) can be left to the philosophers. But we are interested in probability, above all, in quantum mechanics, and there one

is up against the problem of measurement. When it comes to the problem of measurement physics is not its usual self. As we shall see, it turns out that the best answer to (1) so far in evidence also provides an answer to (2) - but at a price.

Our story must proceed in stages. We begin with orthodoxy; next we consider alternatives to quantum mechanics. From then on we consider probability from the point of view of decoherence theory and the unadulterated formalism.

1 Orthodoxy

1.1 Gleason's Theorem

Consider a system a with Hilbert space H^a and inner product $< .,. >$. For $\phi \in H^a$ and any projector $\widehat{P}$ on H^a, the probability $\mu(\phi, \widehat{P})$ is defined by the rule (*the Born rule*):

$$\widehat{P} \text{ is measured in } \phi \ \Rightarrow\ \mu(\phi, \widehat{P}) = \frac{< \phi, \widehat{P}\phi >}{< \phi, \phi >}.$$

From the RHS we see that μ is additive and indeed countably additive over any partitioning $\{\widehat{P}_k\}$ of H^a (any pair-wise orthogonal set of projectors summing to the identity). Looking to the LHS, we suppose that each partitioning corresponds to a particular kind of experiment (one that *measures* $\widehat{P}$). This is where the "intent" of the experiment or the notion of the "observation" that is made comes in; it has proved to be very hard to make do with a purely physical specification of the apparatus instead.

We need a few more definitions in order to state Gleason's theorem. Given a density matrix ρ (positive, self-adjoint, and of trace one) in place of a vector in H^a, the rule is:

$$\widehat{P} \text{ is measured in } \rho \ \Rightarrow\ \mu(\rho, \widehat{P}) = Tr(\rho\widehat{P})$$

where Tr is the trace, yielding a weighted sum over probabilities as defined for the pure case. The set of all projectors on H has an algebraic structure defined by subspace inclusion (a partial ordering). Using it one can define the meet ("and") and join ("or") operations, and under these it is a lattice. It is not of course a Boolean lattice (for which the meet and join operations are distributive), but it has (infinitely) many Boolean sublattices, and it is natural to demand that a probability function on this lattice should be additive on each Boolean sublattice of the total lattice. This is the condition of Gleason's celebrated theorem [8]:

Theorem 1 (Gleason) *Let f be any function on projectors on a Hilbert space H of dimension $d > 2$ to the unit interval which is additive for any set of pairwise disjoint projectors on H. Then there exists a unique density matrix ρ such that for any $\widehat{P}$ on H, $f(\widehat{P}) = Tr(\rho\widehat{P})$.*

Gleason's theorem is a derivation of part of the Born rule, but of course it says nothing about "measurements" or "experiments"; nor, on reflection, is the premise of the theorem so clearly motivated. It is assumed that the probability for an outcome $\widehat{P}$ belonging to one sublattice is the same when $\widehat{P}$ is considered as a member of another. The assumption appears innocuous, but it has non-trivial consequences. For example, let $\widehat{P}_i$ project onto the subspace spanned by $\chi_i \in H^a$, $i = 1, 2$, and let $\widehat{P}_\pm$ project onto $\chi_1 \pm \chi_2$; then $\{\widehat{P}_1, \widehat{P}_2\}$ generates one Boolean sublattice and $\{\widehat{P}_+, \widehat{P}_-\}$ another. Yet if $f(\widehat{P}_1) = f(\widehat{P}_2) = 0$, then by additivity $f(\widehat{P}_1 + \widehat{P}_2) = f(\widehat{P}_+ + \widehat{P}_-) = 0$; and since f is positive, from additivity again it follows that $f(\widehat{P}+) = f(\widehat{P}_-) = 0$. So probabilities for one family of projectors constrain those for another, even though the two do not commute ($[\widehat{P}_i, \widehat{P}\pm] \neq 0$). Should constraints like this be imposed that relate measurements on non-commuting operators? Like that other celebrated theorem in the foundations of quantum mechanics (von Neumann's no-go theorem for hidden variables) the condition of Gleason's theorem may be physically unmotivated.

Of course the premise can be taken as the expression of a phenomenological principal. It is true as goes the statistics of actual experiments; the statistics of an outcome are the same whatever other quantity (so long as it is represented by a commuting operator) is measured. Gleason's theorem, we may suppose, shows us how a phenomenological principle implies a certain mathematical representation of probabilities in quantum mechanics, rather as Kelvin showed how thermodynamical laws imply a certain representation of temperature; and as (although the example is a bit of a stretch) Einstein showed how the relativity principle and the light speed principle imply a certain representation of geometry.

The comparison with thermodynamics is not an altogether happy one. It reminds us that statistical data may well be consistent with deterministic theories. This point was important to Bell [1], who was sympathetic to the idea of introducing hidden variables in quantum mechanics. He sought to understand the observed statistics in terms of averages over states ruled out by Gleason's theorem, *dispersion-free* states, in which every projector has value zero or one. And Bell noticed that the latter are excluded by a much simpler argument than Gleason's: from the result just proved, and from the fact that (again from additivity) if $f(\widehat{P}_\chi) = 1$ and if ϕ is orthogonal to χ then $f(\widehat{P}_\phi) = 0$, it follows immediately that if $f(\widehat{P}_\chi) = 1$ and $f(\widehat{P}_\phi) = 0$ then χ and ϕ cannot be too close ($|\chi - \phi| > \frac{1}{2}|\phi|$). So there can be no dispersion-free states, for if dispersion-free $f(\widehat{P}_\chi)$ must change from 1 to 0 as χ is continuously rotated into ϕ, so it must change for vectors that are arbitrarily close.

According to Bell what is wrong with Gleason's additivity assumption, at the level of the single case, is that it ignores a clear possibility that cannot in principle be ruled out on experimental grounds. In the single case, the values assigned to $\widehat{P}_1$ and $\widehat{P}_2$ may be zero (and that can be discovered experimentally), but one cannot simultaneously measure $\widehat{P}_+$ or $\widehat{P}_-$ so one can draw no conclusion as to the value assigned to them in that context.

1.2 Bohr's Copenhagen Interpretation

Bell's reasoning was faithful to Bohr's *principle of complementarity*, according to which quantum mechanical phenomena cannot be defined independent of an experimental context. Given this and the fact that experiments that measure non-commuting quantities cannot simultaneously be performed, the way is open for results of experiments to defy classical reasoning altogether; it is possible (this Bohr's principle of complementarity) that results obtained by incompatible experiments cannot be consistently fitted together according to the classical ideal of explanation. It is this that makes room for genuine novelty, in quantum experiments, according to Bohr. This argument was made repeatedly in Bohr's published writings.

It is therefore embarrassing, to Bohr if not to Bell - because Bohr was out to interpret quantum mechanics rather than to change it - that by this reasoning a loophole is opened up in Gleason's theorem. It may be that every dynamical quantity has a well-defined value (and every projector has the value 0 or 1) but that values for commuting quantities not in fact measured differ from the values that they would have had if they were. Such a theory is called a *contextual* hidden variable theory.

The same applies to the Kochen-Specker theorem (in effect a strengthening of Bell's theorem). Bohr's complementarity opens a way out for dispersion-free states in that case as well - and a way to understand the quantum probabilities as describing only the statistics.

Leaving so much open, the Copenhagen interpretation offers no definite account of probability and no justification for the Born rule. And even if it could be used to underwrite Gleason's premise, it would give no answer to (1). For according to Bohr, the state is not something physically real; the squared modulus of the amplitude is not a categorical physical quantity. For all that it suggests a link between (1) and (2), it does so at the expense of (1). The Born rule gives the probabilities for the commuting family of $\widehat{P}$'s that the experiment is "intended" to measure. In answering (1), one wants to dispense with the *intentions* (for the probabilities we are after are supposed to be objective rather than subjective). What is it from a purely physical point of view about an experimental apparatus that dictates that it is one set of probabilities that is relevant to the outcomes rather than another? What is the correct choice of sublattice, or equivalently, of basis?

Insisting as he did that the apparatus must be described in classical terms, Bohr was not entirely at a loss to answer this question. Example by example, he tried to show that the basis was dictated by some concrete feature of the apparatus - for example, by whether or not some shutter, screen or diaphragm was bolted to the laboratory bench. This was supposed to work in tandem with Bohr's further thesis that in quantum mechanics one never really went beyond classical concepts (or one or another of a complementary set of classical concepts); that there were, in effect, *no* genuinely quantum mechanical concepts, or none that could function in explanations in the way that classical concepts did. But here Bohr was obviously at a disadvantage; it was part and parcel of complementarity that one could not recover the classical ideal of explanation when it came to atomic phenomena; it was an *a priori* prejudice on Bohr's part that genuinely quantum concepts could never be found that would do better than the fragments of classical physics that Bohr did vouchsafe to us.

Few were prepared to follow Bohr with his analysis of quantum mechanics, and eventually of quantum field theory, in terms of fragments of classical physics. Even restricted to the analysis of measurements, his systems of trapdoors, levers and springs seemed baroque; he never was able to establish any hard-and-fast connection between what was bolted to what and the observable that was supposed to be measured. The doctrine of incomplete explanation was unsuccessful in the other areas where Bohr hoped it would deliver, in biology and psychology. It never offered any insight into the nature of probability.

2 Alternatives to quantum mechanics

2.1 Pilot-wave theory

The alternatives to quantum mechanics are well known. To begin with pilot-wave theory, which retains the unitary equation for the state and supposes indeed that quantum mechanics is universal (so there is a wave-function for the universe), one has underlying dispersion-free states, as Bell wanted, but only for certain dynamical variables (namely configuration space variables, the relative positions and relative velocities). Correspondingly, one has an additional equation - the guidance equation - that dictates the allowable trajectories through each point of configuration space. Consistent with the Bell-Kochen-Specker theorem, definite values are not attributed to every self-adjoint operator in quantum mechanics, independent of context. In fact most operators aren't assigned values at all (for example, only in certain contexts is any component of spin assigned a value). And where a component of spin is assigned a value (in the context of, say, a Stern-Gerlach experiment, for a particular orientation of the magnetic field) no value for any other component of spin is defined.

This is to rehabilitate Bohr's reasoning about experiments (although it can hardly be said to lend support to his general philosophy). But the more clear-cut interpretation of the pilot-wave theory is to suppose that the only real physical quantities are configuration space variables (relative positions and velocities); that all the rest are artifacts of experiments. Dispersion-free states for these quantities are not contextual, except in the sense that they may of course (by the non-locality of the theory) change when the macroscopic apparatus is changed. Bell himself seemed to advocate this position [2].

So how does probability get into the picture? Much as it does in classical statistical mechanics: one probability distribution on configuration space is favoured (as given by the Born rule) for much the same reason that Liouville measure on phase space is favoured in classical Hamiltonian mechanics. The Born rule is said to be the "equilibrium" distribution ("quantum equilibrium"). Once in equilibrium, systems cannot be reliably prepared in the dispersion-free states allowed by the theory (it is this that hides the non-locality). This smacks of conspiracy; but given equilibrium, the situation is in one respect better than in classical statistical mechanics. At least we are in a position to answer (1): chances are determined by certain categorical properties in the world (the squared norms of the components of the wave-function with respect to the position basis) - assuming, as proponents of the pilot-wave theory usually do, that the wave-function is physically real.

Now note the two disadvantages of this approach. The first is that if this is the answer to Question (1), Question (2) seems entirely impenetrable. It is hard to see why our subjective expectations should be concerned with these amplitudes squared. The trajectory in phase space can after all be chosen to give you any statistics you like (leaving the amplitudes completely unchanged); if it is the trajectory we are concerned with - this is what picks out all the actual things that happen as opposed to those that don't - why should something completely *independent* of the trajectory, the amplitudes, be of any relevance?

The second turns this objection around. Why, in any case, suppose the physical probabilities are given by the Born rule? Maybe they float free of it, as classical probabilities can float free from equilibrium distributions in statistical mechanics [16]. There, non-equilibrium is not ruled out by *fiat* as somehow illegitimate (classically the universe is far from equilibrium). But if so, and there is no intrinsic connection between physical probabilities and the amplitudes, what *do* probabilities correspond to? - and we are back to square one.

2.2 State-reduction theory

In pilot-wave theory state reduction is "effective", as components of the state ("empty waves") irrelevant to the guidance equation can be discarded. The alternative is to build it into the dynamics directly. Here, unlike the case of pilot-wave theory, a considerable body of new work in physics and mathematics was needed. The mathematical foundations were laid by Smulochowski, Wiener and von Neumann in the 1920s. Investigations in quantum optics in the late 1950s made use of probability measures in quantum mechanics generalized to sequential sample spaces. They also generalized to continuous sampling; in the mid 1960's Nelson showed how the Schrödinger equation may be related to a continuous Markovian stochastic process. By the end of the 1970s, with investigations in open quantum systems theory by Davis and his collaborators, it was clear how to construct stochastic models to mimic the behaviour of subsystems subject to arbitrary quantum evolutions. The first stochastic theory demonstrably equivalent to standard quantum mechanics across a wide range of applications (all of them, however, non-relativistic) was proposed in 1986 by Ghirardi, Rimini, and Weber. These theories are therefore of comparatively recent origins; what do they say about probability?

The Schrödinger equation is replaced by a stochastic vector-valued dynamical process. In what is perhaps the most elegant example, the continuous state-reduction model [9], this process is assumed to be Markovian and to take the form:

$$d\psi = \left(\widehat{Q}(\psi)dt + \widehat{\mathbf{R}}(\psi)\cdot\mathbf{dB}\right)\psi. \tag{1}$$

Here $\widehat{Q}$ and $\widehat{\mathbf{R}}$ are self-adjoint operators on H that depend on the state ψ; $\mathbf{B}$ is a smooth Markov process with components B_k, $k = 1, 2, 3$ satisfying (here γ is a new fundamental constant):

$$\overline{dB}_i(t) = 0,\ \overline{dB_k(t)dB_j(t)} = \delta_{kj}\gamma dt.$$

The over-bar denotes averaging with respect to the underlying probability space of the Markov process. Each B_k is a map from its index set (time) to random variables (measurable functions) on this space. A probability distribution here as always - mathematically - is given by a measure on a $\sigma-$algebra of sets (a Borel space). What this measure corresponds to physically (Question (1)), and why these values of the measure should dictate subjective probabilities (Question (2)), is as obscure as ever.

Is there then no improvement in clarity about probability in this theory? It would be odd if so; with a stochastic, indeterministic theory, the probabilities are supposed to be in the theory at the ground level. One would think that there be a clearer ontological basis to them than in a deterministic theory. There is this difference: whereas in classical statistical mechanics the measure is defined over a space of one-time data (Cauchy data), each point of which encodes an entire trajectory, in a stochastic theory the measure is defined over the space of trajectories (histories). This, mathematically speaking, is the whole of the difference between a deterministic and an indeterministic theory. The customary distinction between epistemic and non-epistemic interpretations of probability is more rooted in temporal matters than first appears. One may say of a stochastic theory as much as of a deterministic one that probabilities are epistemic: they reflect one's ignorance of which history is ours. The difference is that in the deterministic case one must be ignorant of much more, if probabilities are to make any sense: one cannot know (in complete detail) even a single instant of our actual history.

What of the Born rule? It is built into the dynamics, Eq.(1), in the dependence of the operators $\widehat{Q}$ and $\widehat{\mathbf{R}}$ on the state. This is of the form:

$$\widehat{\mathbf{R}}(\psi) = \widehat{\mathbf{A}} - <\psi, \widehat{\mathbf{A}}\psi>,\ \widehat{Q}(\psi) = -\frac{i}{\hbar}\widehat{H} - \widehat{\mathbf{R}}\cdot\widehat{\mathbf{R}}$$

where $\widehat{H}$ is the Hamiltonian, and $\widehat{\mathbf{A}}$ is a commuting triple of self-adjoint operators transforming as a Galilean 3-vector (remember we only have a non-relativistic stochastic theory). Different choices of the dependence of $\widehat{Q}$ and $\widehat{\mathbf{R}}$ on ψ will lead to a stochastic process yielding statistics at variance with the Born rule. It is therefore clearer in a state-reduction theory than in pilot-wave theory why the squared amplitudes matter to probabilities, since they enter directly into the dynamics. There is no longer the possibility that the dynamics can reliably drive an initial state through a sequence of states in which the relative frequencies of outcomes is independent of the squared amplitudes (the

Born rule). But everything now hangs on the notion of a "reliable" dynamics; the point is the same as before, that one might as well speak of a "typical" trajectory (and of probability as a matter of ignorance as to which trajectory is one's own). And, indeed, from a purely mathematical point of view, one is back to a definition of probability as a measure on history space. What it is about a particular history (the one that is actually realized) that makes it true that the probability of a chance outcome has a particular value is never explained. The link with subjective probabilities is as opaque as ever.

3 Decoherence theory

Returning to the conventional formalism, there are two important lessons that we can draw from pilot-wave theory and state-reduction theory. In each there is a universal state that applies to closed systems; and in each there is state-reduction - merely "effective" in the former theory, fundamental in the latter - yielding states which are well-localized in configuration space (there is a *single* preferred basis). With this it is enough to recover all extant experimental data (at least in the case of non-relativistic applications of quantum mechanics). These are the lessons that are taken to heart in decoherence theory.

Decoherence theory was powered by investigations in open-quantum systems theory just as the theory of quantum stochastic processes was. It has in common with pilot-wave theory the assumption that the unitary equations of motion are both fundamental and universal, and it has in common with state-reduction theories many of the same equations, but derived as *effective* equations, concerning only certain dynamical variables and degrees of freedom of a system. In any given case of decoherence probabilities are defined only for a certain basis or Boolean sublattice of projectors. For systems at ordinary temperatures in which massive degrees of freedom are weakly coupled to large numbers of much lighter ones the basis for which simple, effective equations exist for the massive ones is always (approximately) the same: it is given by projectors onto well-localized regions of the configuration space of the massive degrees of freedom, at least down to atomic dimensions and time-scales of the order of classical thermal relaxation times. Thereby, so long as velocities and momenta are obtained by averaging over these timescales, one obtains coarse-grained trajectories in configuration space and phase space as well. These are classically perspicuous descriptions of atomic and molecular processes (with quantum correction terms added).

Decoherence plays a role in all the major schools of foundations in quantum mechanics. It matters to the pilot-wave theory, for it explains just where and when one can use the reduced state, discarding components of the state (the "empty waves") for the purposes of actually applying the theory and computing trajectories. It matters to state-reduction theory, which stochastically degrades every component of the state save one. Degrade them too soon (before they have decohered) and the predicted probabilities will differ from those of quantum mechanics. And it matters to the consistent histories approach. Histories, in this formalism, are ordered sequences of projectors $\widehat{P}_k$ (of some given resolution of the identity), interpreted as a sequence of events in time, as specified by some discrete time variable and the resolution of the identity used at each time. They are *consistent* insofar as the probabilities for each history are non-interfering, meaning the probabilities are the same whether they are computed assuming the state is reduced at each step (on sequential "measurement" of each projector $\widehat{P}_k$), or assuming it is always the uncollapsed state. Whether or not a set of histories is consistent depends on the basis used at each time, the state, and on the unitary dynamics.

Components of the state decohere, in some basis, when there exist effective, local equations of motion, propagating data along individual branches of the state (referred to that basis), in approximate agreement with (and usually adding small corrections to) the results of the conventional formalism using the measurement postulates. It goes without saying that such histories are approximately consistent - interference effects between histories could hardly be significant if there are such effective in-branch equations. In fact consistency is a much weaker requirement. One can define consistent histories for the simplest imaginable systems, with only a handful of degrees of freedom. One can represent the motion of a single electron in an inhomogeneous magnetic field (the Stern-Gerlach experiment) in accordance with various bases, each of them consistent; one can even smoothly modify one basis into another whilst maintaining consistency [6]. Non-interference by itself is not enough to guarantee that any interesting physics - effective equations - attaches to these histories. In the language of Gell-Mann and Hartle [7], decoherence proper concerns a *quasiclasical domain* and not just a consistent history space.

Finally, decoherence matters to the Everett interpretation. For this interpretation, like all the theories so far considered, there is a wave-function of the universe, and like the consistent histories theory, the unitary dynamics alone is fundamental. As in all of these theories there is a preferred basis - defined by decoherence. We shall say more about the Everett interpretation shortly.

Defining probability in terms of decoherence, one has more or less the same Boolean sublattice of projectors throughout - a coarse-graining of projectors on configuration space (equivalently, on phase space). But with that it is clear that the premise of Gleason's theorem need not apply - indeed, that it has no motivation whatsoever. If probability only makes sense in the context of decoherence, which only arises for certain dynamical variables and in certain situations, why suppose that probabilities can be defined for arbitrary resolutions of the identity, with a non-contextual additivity requirement built in from the beginning? Why suppose probability has any meaning at all in regimes in which dynamical decoherence does not exist? But as we shall see, the Born rule can be derived from alternative premises, that from the point of view of decoherence theory are very natural.

4 A New Derivation of the Born Rule

If probabilities only arise in the context of decoherence - if they are "emergent" - then they will have to have some of the key attributes of decoherence.

- The first is that decoherence typically involves highly degenerate, indeed infinitely degenerate, projectors (a projector onto any non-zero volume of configuration space must have infinite-dimensional range). Call this the *degeneracy* condition.
- The second is that decoherence is only *approximate*; there is no precise boundary below which the probabilities are undefined; except at very low temperatures, there are many orders of magnitude over which projectors can be further fine-grained without loss of decoherence. Call this the *stability* condition.
- The third is that the probability rule should be basis-independent; it is the intrinsic relationship between the universal state and the Boolean lattice of projectors that matters. Call this the *invariance* condition.

Let us make this more precise, taking, for the sake of definiteness, for configuration space C, a family $F(C)$ of coarse-grainings Δ of C, with the natural partial ordering given by the inclusion relation on C. For each $\Delta \in F(C)$ one has an associated Boolean sublattice B_Δ (generated by projectors onto the cells of Δ) of the lattice of projectors on the total Hilbert space H. We are looking for a probability measure $\mu : H \times B_\Delta \to [0,1]$, $\Delta \in F(C)$ on projectors of infinite dimension which is intrinsic to H and stable under variation of Δ..So we require:

(i) Each $\widehat{P} \in B_\Delta$, $\Delta \in F(C)$, $\widehat{P}$ has infinite range (degeneracy)

(ii) For any unitary map $U : H \to H$, $\mu_\Delta(\psi, \widehat{P}) = \mu_\Delta(U\psi, U\widehat{P}U^{-1})$ (invariance).

(iii) For any $\widehat{P} \in B_\Delta \cap B_{\Delta'}$, $\Delta', \Delta \in F(C)$, $\mu_\Delta(\psi, \widehat{P}) = \mu_{\Delta'}(\psi, \widehat{P})$ (stability).
To these we shall eventually add a continuity assumption. Our claim is that under these assumptions, for sufficiently large families $F(C)$ of coarse-grainings of C, the Born rule follows uniquely. The proof in the case of regular polyhedra at *every* scale in C (denote $F_\infty(C)$) is particularly simple, although it has the disadvantage that it is unphysical; at sufficiently small scales decoherence inevitably fails. We shall later consider whether this idealization is really troublesome.

Certain results follow independent of any assumptionson $F(C)$. First a definition. Call a set of orthonormal vectors $\{\varphi_k\}$ *separating* for a set of disjoint projectors $\{\widehat{P}_k\}$, $k = 1, ..., d$, if $\widehat{P}_j \varphi_k = \delta_{jk}\varphi_k$. Then:

Lemma 2 *Let $\{\varphi_k\}$, $\{\widehat{P}_k\}$, $k = 1, ..., d$, be separating, $\widehat{P}_k \in B_\Delta$, and let $\psi = \sum_{k=1}^{d} c_k \varphi_k$. If μ satisfies (ii) and $\psi' = \sum_{k=1}^{d} |c_k| \varphi_k$:*

$$\mu_\Delta(\psi, \widehat{P}_j) = \mu_\Delta(\psi', \widehat{P}_j),\ j = 1, ..., d.$$

Proof. Let $c_k = \exp(i\theta_k)|c_k|$, $k = 1, ..., d$, and let $U_\theta : \varphi_k \rightarrow \exp(-i\theta_k)\varphi_k$, $U_\theta \widehat{P}_k U_\theta^{-1} = \widehat{P}_k$, $k = 1, ..., d$ (such a U_θ can always be constructed); the result is immediate from (ii). ■

Likewise, the overall phase of the vector ψ is irrelevant for the probabilities. Now for the equiprobable case:

Lemma 3 *As in Lemma 2, but let* $|\ c_k|^2$ *=constant. If* μ *satisfies (i), (ii):*

$$\mu_\Delta(\psi, \widehat{P}) = \begin{cases} \frac{1}{d},\ \widehat{P} \in \{\widehat{P}_k\},\ k = 1, ..., d \\ 0,\ \widehat{P} \text{ orthogonal to } \sum_{k=1}^{d} \widehat{P}_k \end{cases}.$$

Proof. By Lemma 2, without loss of generality we may assume the c_k's are all real: $\psi = const.(\varphi_1 + ... + \varphi_d)$. First assume $d > 1$. Define U_π by $U_\pi \varphi_k = \varphi_{\pi(k)}$, $U_\pi \widehat{P}_k U_\pi^{-1} = \widehat{P}_{\pi(k)}$, where π is a permutation (such a U_π can always be constructed, since by (i) every projector has the same dimension). Since $U_\pi \psi = \psi$, from invariance:

$$\mu_\Delta(\psi, \widehat{P}_j) = \mu_\Delta(\psi, \widehat{P}_{\pi(j)}).$$

Choose any $\widehat{P}_k$ (say $k = 1$) and define $\widehat{P}'_1 = \widehat{P}_1 + (I - \sum_{j=1}^{d} \widehat{P}_j)$. Evidently $\{\varphi_j\}$ is separating for $\widehat{P}'_1, \widehat{P}_2, ..., \widehat{P}_d$ and by the same argument μ_Δ is constant on this set as well. Since $\widehat{P}'_1 + \widehat{P}_2 + ... + \widehat{P}_d = I$ and μ_Δ is a probability measure $\mu_\Delta(\psi, \widehat{P}_j) = \frac{1}{d}$, $j = 2, ..., d$. The same argument for any other choice of k yields $\mu_\Delta(\psi, \widehat{P}_1) = \frac{1}{d}$; from additivity again, if $\widehat{P}$ is any projector in B_Δ orthogonal to all the $\widehat{P}_k$'s then $\mu_\Delta(\psi, \widehat{P}) = 0$. The case $d = 1$ does strictly speaking involve an additional (but very weak) assumption: that there are at least 3 disjoint projectors in B_Δ disjoint from $\widehat{P}_1$, denote $\widehat{P}_2, \widehat{P}_3, \widehat{P}_4$. Let $\widehat{P}'_4 = 1 - \sum_{k=1}^{4} \widehat{P}_k$. Since by assumption $\widehat{P}_k \psi = 0$, $k = 2, 3, 4$ $\mu(\widehat{P}_2) = \mu(\widehat{P}_3) = \mu(\widehat{P}'_4)$, by the result already proved (using a permutation that leaves $\widehat{P}_1$ invariant). Let $\widehat{P}'_2 = \widehat{P}_2 + \widehat{P}_3$; again $\widehat{P}'_2 \psi = \widehat{P}'_4 \psi = 0$ so $\widehat{P}'_2$ and $\widehat{P}'_4$ are equiprobable. But then $\widehat{P}'_2$ and $\widehat{P}_2$ are equiprobable, and from additivity $\mu(\widehat{P}_2) = 0$. By the same argument $\mu(\widehat{P}_3) = \mu(\widehat{P}'_4) = 0$, so by additivity $\mu(\widehat{P}_1) = 1$. ■

The case $d = 1$ is the *eigenvector-eigenvalue rule*; this result and the method of proof follows closely the mathematical ideas introduced by Deutsch [5] and Wallace[18], [20]. The next two lemmas and Theorem 6 differ in certain respects, however. We shall come back to the Deutsch-Wallace theorem shortly.

We need the stability condition to go beyond the equiprobable case. The proof for $F(C) = F_\infty(C)$, where C is R^n and H is isomorphic to $L^2(R^n, dx^n)$, is as follows. (Note that condition (i), degeneracy, is no longer needed as an independent assumption.)

Lemma 4 *For any* $\psi \in H = L^2(R^n, dx^n)$, $\Delta \in F_\infty(C)$, *and for any* $\widehat{P} \in B_\Delta$ *and any integer* m, *there exists a refinement* Δ' *of* Δ *and orthogonal projectors* $\widehat{P}_j \in B_{\Delta'}$, $j = 1, ..., m$, *summing to* $\widehat{P}$, *such that* $\left|\widehat{P}_j \psi\right|$ *is constant.*

Proof. Let $\psi' = \widehat{P}\psi \neq 0$ (if zero, the proof is immediate). For $n = 1$, $\int_{-\infty}^{r} \overline{\psi}'\psi' dx$ is a non-negative increasing function of r. By the intermediate value theorem, there are real numbers $r_1, ..., r_{m-1}$ such that $\int_{r_j}^{r_{j+1}} \overline{\psi}'\psi' dx = const\frac{1}{m}$, $j = 0, ..., m-1$, $r_0 = -\infty$, $r_m = \infty$. Choose as projectors the characteristic functions $\chi_{\Delta'_j}$ on R, where $\Delta'_j = [r_j, r_{j+1}]$. The generalization to higher dimensions is obvious. ■

We may then prove

Lemma 5 *Let μ be a probability measure on $B_\Delta \in F_\infty(C)$ satisfying (ii) and (iii). Let $\{\varphi_k\}$, $\{\widehat{P}_j\}$, $k = 1, ..., d$ be separating. Let $\psi = const\sum_{k=1}^{d} \sqrt{m_k}\varphi_k$, $m_k \in Z$. Then:*

$$\mu_\Delta(\psi, \widehat{P}) = \begin{cases} \frac{m_j}{\sum_{k=1}^{d} m_k}, \ \widehat{P} = \widehat{P}_j, \ j = 1, ..., d \\ 0, \ \widehat{P} \text{ orthogonal to } \sum_{k=1}^{d} \widehat{P}_k \end{cases}.$$

Proof. By Lemma 4, we may choose a fine-graining $\Delta' \in F_\infty(C)$ of Δ such that for each $k = 1, ..., d$, $B_{\Delta'}$ contains m_k orthogonal projectors $\widehat{P}_k^j$ summing to $\widehat{P}_k$, satisfying $\left|\widehat{P}_k^j\varphi_k\right| = const.$ Define $\varphi_k^j = \widehat{P}_k^j\varphi_k$, then $\varphi_k = \frac{1}{\sqrt{m_j}}\sum_{j=1}^{m_k}\varphi_k^j$ and $\psi = const.\sum_{k=1}^{d}\sum_{j=1}^{m_k}\varphi_k^j$. By construction $\{\varphi_k^j\}$ is separating for $\{\widehat{P}_k^j\}$ ($m = \sum_{k=1}^{d} m_k$ in all) and the conditions of Lemma 3 apply; so $\mu_\Delta(\psi, \widehat{P}_k^j) = \frac{1}{m}$ and $\mu_\Delta(\psi, \widehat{P}_k) = \mu_\Delta(\psi, \sum_{j=1}^{m_k}\widehat{P}_k^j) = \frac{m_k}{m}$. ■

It is a short step to the general case. We need only assume that μ_Δ is continuous as a map $H \to [0, 1]$ (for fixed $\widehat{P} \in B_\Delta$). We thus obtain:

Theorem 6 *Let μ be as in Lemma 5 and for each $\widehat{P} \in B_\Delta$ let $\mu_\Delta(., \widehat{P}) : H \to$ $[0, 1]$ be continuous in norm. Then for any $\psi \in H$:*

$$\mu_\Delta(\psi, \widehat{P}) = \frac{<\psi, \widehat{P}\psi>}{<\psi, \psi>}.$$

The proof proceeds by constructing, for any ψ, a sequence of vectors in H of the form assumed in Lemma 5 that is separating for $\widehat{P}$ and $I - \widehat{P}$ that converges to ψ.

Having stated the theorem, two caveats. The first is that since it assumes continuity in norm, its mathematical interest is considerably diminished. One of the remarkable things about Gleason's theorem is that continuity of the measure is *derived*. But from a physical point of view, if probabilities depend at all on vectors in H, they surely vary continuously with them. The assumption is physically perfectly natural.

The second is the one already noted, that $F_\infty(C)$ is unphysical. But we should be clear why the idealization was needed. It is because, in Lemma 5, the integers m_k arising may be arbitrarily large, so the number of orthogonal projectors required to sum to each $\widehat{P}_k$ must be arbitrarily large. That is only possible if we allow coarse-grainings of C that are arbitrarily small.

Suppose the scale of the coarse-graining is bounded below; what sort of restriction does this place on these numbers? We are only interested in exploring the probabilitistic structure of the state at the decoherence lengthscale and above (for probability, if emergent, has no meaning at smaller lengthscales). So we may suppose the state is approximately uniform over some region Δ_k of configuration space, near the threshold of decoherence, at the lengthscale $2l$; let Δ' be a refinement of Δ at the lengthscale l; how many disjoint projectors in $B_{\Delta'}$ are there, summing to the projector on Δ_k? The answer, for configuration space of dimension n, in the case of hypercubes, is 2^n. So even taking the limits of decoherence into account, we can derive rational ratios of probabilities using very large integers - numbers that increase exponentially with the number of degrees of freedom. Given our general philosophy, that probabilities are only defined given decoherence and that they should be robust under changes of coarse graining, we can legitimately demand that the distribution $\mu_\Delta(\psi,)$ should be smooth and not only continuous under variations in ψ, at least for macroscopic systems of large numbers of degrees of freedom. It should be effectively constant over variations in ratios of norms of one part in $2^{10^{22}}$.

We come back to the dependence of the Born rule on the *purpose* of the experiment. Although we do not yet have a clear picture of how to interpret quantum mechanics using decoherence theory, we have an unambiguous answer to this question. It is "behaviour" and not "purpose"; it is a matter of what, at the sub-decoherence level, as described in pure quantum mechanics, is reliably correlated (by the measurement interaction) with decohering variables. It is only by virtue of these correlations that probability as ermerging with decoherence has any meaning at the microscopic level.

Here the details are familiar; they follow, in the simplest cases, the von Neumann treatment of measurement processes. The measurement interaction brings about correlations between projectors in B_Δ, $\Delta \in F(C)$, with projectors onto eigenspaces of dynamical variables of individual subsystems a,b, c, ... , described by (possibly finite–dimensional) subspaces $H^a \subset H$. The only limit to this process is the ingenuity of the experimenter. In the case of spin systems of small dimension, it is a plausible claim that in this way one can experimentally realize correlations between projectors in B_Δ and *arbitrary* projectors on H^a.

So long as projectors in B_Δ and on H^a can be reliably correlated in this way - depending on the ingenuity of the experimenter - probabilities assigned to projectors in B_Δ can be assigned to projectors on H^a as well. That is the whole story about probability at the sub-decoherence level. That these correlations are non-contextual follows automatically.

5 The Everett Interpretation

Probability, if only defined in the context of decoherence, must be given by the Born rule. But what is the underlying physical picture? We have spoken of quantum mechanical models of the experimental apparatus, applying quantum mechanics directly to macroscopic systems, but of course decoherence theory does not in itself solve the conceptual problems that follow from this. Lack of clarity on this score makes it hard to answer the questions we are concerned with: (1) What is objective probability? and (2) Why should subjective expectations track these objective probabilities?

If we want clarity as to questions of what exists, we had better look to a realist solution to the problem of measurement. If we want probability to arise only in the context of decoherence, we had better not modify or add new elements to the unitary formalism. This narrows down the available alternatives. There are versions of the consistent histories interpretation that may lay claim to a realist status, but those in which only a single history is realized necessarily forsake the approximate character of decoherence (essential to the derivation of the Born rule that we have given), and require instead some new input to the theory in order to single out a unique history space (to which the one and only history actually realized belongs). The idea of environment super-selection rules and the interpretation of an improper mixture (arrived at by tracing out environmental degrees of freedom) in terms of ignorance has been dropped even by its advocates [22], [23].

That leaves only the literalist interpretation of the state, in which all the branches are physically real. With that we are led to many worlds and to the Everett interpretation: worlds are described by the components of the universal state referred to the decoherence basis. As such, under the unitary dynamics, the evolution from a single component of this basis into a superposition is the evolution of one world into many. Worlds in this sense divide.[1] A chance process is one in which a system is subject to division in this sense.

5.1 Understanding branching

Our objective here is not to evaluate solutions to the measurement problem, only the status of probability within them. In Everett's approach, there is now a clear cut answer to (1): probabilistic events arise only by branching. Branching, or equivalently, the development of a superposition (referred to the decoherence basis), is the basis of all objective physical indeterminism (for quantum mechanics is taken to be both universal and fundamental). The moment of branching is, to use Heisenberg's language, the point at which "potentiality" becomes "actuality". Chances, as quantities, are squares of the norms of the associated transition amplitudes - all categorical physical properties and relations.

Just as important, branching (and therefore this transition) inherits the approximate character of decoherence. One can put this in terms of *vagueness* - that branching is vague, with clear-cut instances but no sharp boundaries. Vagueness permeates ordinary language, but it is pervasive in scientific theories as well. There is no precise physical definition of tables or chairs, no more

[1] They may also, in principle, recombine. It has long been recognized that probability and the arrow of time are intimately related. This relation leads on to others [21]; we cannot do justice to them here.

than of cells or molecules. Vagueness is endemic in the chain of reduction, from ordinary objects to material science, the solid state, and chemistry; from zoology and anatomy to molecular biology and biochemistry. According to the Everett interpretation, extracting quasiclasical phenomenology from the unitary dynamics of quantum mechanics is subject to the same kinds of equivocation as confront any program for recovering higher-level laws from more fundamental ones[19]. The methodological issues are all precisely the same.

If chances arise with branching, but branching depends on the details of the coarse-graining, then chances can only be stable under variations in coarse graining if they satisfy (iii), and hence (with assumptions (i), (ii)) the Born rule - this the result just proved. It replaces Gleason's theorem; probability is not assumed from the outset to be non-contextual and defined for any basis; it is not assumed to have any fundamental significance at all. Probability is "emergent".

One might object that the answer to Question (1) is then not so clear-cut after all; chances arise with branching, but branching, because imprecisely defined, is hardly being accounted for by any precise properties and relations. But the same is true of paradigm cases of successful inter-theory reduction. Reduction is never precise. It is not as though there should be some precise and unique frequency distribution in electromagnetism that corresponds to the colour "red", for example. The point about the reduction in the case of chance is that it be to *categorical* properties and relations (that are not themselves indeterminate, borderline, or chancy); it is that the substrate posited by the reducing theory (the spectrum of waves*)* should not employ concepts just as mirky as those we sought to elucidate (the colour red*)*. This is where Popper went wrong with his account of chance in terms of dispositions.

In fact, in the special case of laboratory experiments (or more generally of "interpreted" phenomena), a more abstract notion of branching is available that is reasonably precise; here one *defines* branches, by sheer stipulation, in $1:1$ correspondence with the different *numbers* assigned to measurement outcomes (as equivalence classes of configurations of the experimental display, that are all taken to represent the *same* numerical outcome). The number of branches is the number of possible pointer positions on the dial. Of course there still remain problems of borderline cases, if for no other reason than that an experiment is always subject to inefficiencies and is always prone to malfunction; but at this level, concerning branches that we count as differing in clear-cut respects, we will be down to a small and finite number. We will be perfectly able to make sense of their number.

The Everett interpretation does well with (1). It does much better than the pilot-wave theory, even though the latter has all the resources of the Everett interpretation - and then some, for it postulates additional structure, namely a particular trajectory. But that is just where the trouble comes in (when one introduces the trajectory); the trajectory may be one in which the statistics are completely different from those predicted by the Born rule. What does probability mean in such a case?

Some have thought that precisely the same worry arises in the Everett interpretation. There too there exist "anomalous branches", in which the recorded statistics do not match the ones predicted by the Born rule. But there is an important difference. According to Everett, there is nothing about a branch of this kind that can ensure it will *continue* to violate the Born rule (for there is no fact of the matter as to what will happen following on from a given branch, so long as every branch is given to division), unlike the situation for anomalous trajectories in pilot-wave theory. Anomalous branches, in the Everett interpretation, are like statistically anomalous segments, each of finite length, in an infinitely extendible sequence of random numbers. They have to exist if the sequence is to be genuinely random, but in no sense is any given subsequence likely to *continue* to be anomalous.

What about (2), the connection with subjective expectations? Why should the amplitudes on branching be our guide for these? But at least it is clear that on branching we *ought* to be concerned with weights for branches. For it is obvious that branching - *personal* branching, literally dividing in two, say - will lead to divided expectations, and this will be so even given *complete* knowledge of the branching process. The two successors may differ widely, yet each will with as much right call themselves the same person as before. In the face of branching there is no 1 : 1 criterion of identity in the forward direction of time. But if one is to make provision for one's successors, one must allocate resources among them. And one can hardly do this without introducing weightings, implicit or explicit, in one's reasoning. One cannot ready oneself for anything and everything.

Philosophers have long disagreed on how, in the presence of branching, questions of personal identity are to be settled [13]; we should make no pretence that in this matter there is any real consensus. But the one response that is really damaging to the Everett interpretation has found few advocates: it is that in the face of branching one should expect *nothing*, *oblivion*. This view is inherently implausible, given that each of my successors is *ex hypothesi* functionally exactly the same as me. Every successor has all of my attributes and memories; every successor professes himself to be me on the basis of physical continuity (and on every other physical criterion). No wonder this view has found few supporters.

5.2 Deutsch's argument

If not oblivion, then divided expectations. If divided expectations, then divided how, and with what weighting? What preferences ought one to have for one process of branching (for performing one choice of experiment), with a given utility in each branch (for each experimental outcome), over another? But just at this point Deutsch's argument comes into play.

Deutsch's strategy, following de Finnetti [4], and before him von Neumann and Morgenstern [17], was to define subjective probabilities (hereafter, *weights*) in terms of the preferences of a rational agent among a set of games $g \in G$ each with some set of outcomes E_k, $k = 1, ..., d$ with associated utilities ("payoffs") - concrete rewards, cash prizes say, that the agent values - belonging to some set $\mathcal{U}$. Call $\mathcal{P} : E_k \to \mathcal{U}$ the *payoff function* for that agent. If rational, the ordering $\preceq$ on G defined by one's preferences should satisfy certain obvious rules (for example, transitivity). The strategy is then to find a strong enough but still plausible set of rules sufficient to ensure that for each game g there exists real numbers $p_k \in [0,1]$ for each outcome λ_k, $k = 1, ..., d$, summing to one, and quantities $\mathcal{V}(g) = \sum_{k=1}^{d} p_k \mathcal{P}(\mathcal{E}_k)$, such that $g \preceq g'$ if and only if $\mathcal{V}(g) \leq \mathcal{V}(g')$. If G is big enough, indeed, one would hope to show that the numbers p_k (weights) for the outcomes in each g are unique. The important point in this is that the p_k's arrived at in this way will be *independent* of an agent's utilities. A rational agent will act as if attempting to maximize the expectation value of her utilities, using these weights as probabilities. It is because of this representation theorem that subjective probability is in so much better shape than objective probability. If this is what probability is, one can explain why it obeys the rules that it does.

Deutsch's remarkable claim is now that the preference ordering of rational agents, in the face of quantum games, can be so constrained that the weights defined by these preferences (independent of their actual utilities) agree with the Born rule.

This result is so surprising that one wants to have an inkling of how it was obtained. Here we shall follow Wallace [18], who has substantially revised and simplified the argument. A quantum game can be played using any quantum experiment, simply by agreeing on various payoffs (positive or negative) on each possible outcome. What is an experiment, according to Everett? It is a special kind of process, involving stable macroscopic objects, described by effective equations, such that states can be attributed to sub-systems (typically molecular), as relative states, that involve unitarily (i.e. as a product state) with respect to the apparatus (this the state-preparation process). Following some unitary evolution preserving this product structure, they evolve into an entanglement with the measurement device. Components of this eventually include macroscopic degrees of freedom (pointer positions).

From a mathematical point of view one introduces a tensor-product in the Hilbert space for a particular branch, distinguishing some microscopic subsystem a with Hilbert space H^a from all the rest. Suppose (for simplicity) that H^a has finite dimensions. The state preparation device produces, in a reliable way, vectors in a certain subspace of H^a (for simplicity suppose 1-dimensional, so a particular state ϕ), in a tensor product with the state of the rest of the apparatus and its environment. The entanglement subsequently brought about is with some set of orthogonal states $\phi_k \in H^a$, $k = 1, ..., d$ of a, with decohering states of the apparatus and environment (grouped together when they give rise to the same pointer-reading). The latter reliably leave behind them a macroscopic trace.

In these models it is useful to introduce numbers λ_k for the states in H^a which have some dynamical significance - eigenvalues λ_k, associated with eigenstates of some self-adjoint operator $\widehat{X} = \sum_{k=1}^{d} \lambda_k \widehat{P}_{\phi_k}$; these replace the E_k's above. The instrument display, meanwhile, registers numbers concretely, so one has some definite assignment of the λ_k's with these numerals (usually taken as the identity). The experiment is converted to a game by specifying a map from these numerals to an agent's utilities in $\mathcal{U}$, which we can model directly in terms of the pay-off function as a map $\mathcal{P} : Sp(\widehat{X}) \to \mathcal{U}$. Suppressing explicit reference to H^a, a quantum game is then given by an ordered triple $< \phi, \widehat{X}, \mathcal{P} >$.

But adopting this schema, we must recognize the arbitrary elements in it. It is obviously possible to compensate for a change in labels λ_k by a change in the pay-off function. This corresponds to a certain arbitrariness in the choice of self-adjoint operator that the experiment is said to measure: whether it is is $\widehat{X}$ with payoff function $\mathcal{P}$, or $f(\widehat{X})$ with payoff function $\mathcal{P}\circ f^{-1}$ (for some invertible f on $Sp(\widehat{X})$). For another arbitrary element, typically the initial product state involving ϕ will be subject to a unitary evolution U on H^a which preserves the product structure. Indeed, the preparation device may best be modelled using a sequence of Hilbert spaces with intertwining operators $U : H^a \to H^b$. From an Everettian standpoint it is now entirely arbitrary which of these is taken to be *the* initial state; the experiment can with as much right be called a measurement of $U\widehat{X}U^{-1}$ in the state $U\phi$ as of $\widehat{X}$ in the state ϕ. There is nothing in the physics to say. It is only the correlations between vectors in H^a with payoffs or pointer readings in $\mathcal{U}$, that is relevant to an experiment; not the vector at which time - this is entirely arbitrary.

We have established two principles. Under our schema for quantum games, the triples $< \phi, \widehat{X}, \mathcal{P} >$, many games can be realized by a single physical process. Since preferences among games should concern the physical world rather than the models used to describe it, they should value games as the same if they can be realized by the same physical process. Let $g \sim g'$ if and only if $g \preceq g'$ and $g' \preceq g$. We require for any unitary on H^a and any invertible f on $Sp(\widehat{X})$ the equivalence principles:

$$\begin{aligned} \textbf{Payoff Equivalence} \quad &: \quad < \phi, \widehat{X}, \mathcal{P} > \sim < \phi, f(\widehat{X}), \mathcal{P}\circ f^{-1} > \\ \textbf{Measurement Equivalence} \quad &: \quad < \phi, \widehat{X}, \mathcal{P} > \sim < U\phi, U\widehat{X}U^{-1}, \mathcal{P} > \end{aligned}$$

This is a schema well-suited to the Everett interpretation, but it can be motivated on other grounds; it can even be motivated operationally [14]. Deutsch's decision theoretic axioms naturally make no reference to the Everett interpretation. The analysis that follows can, therefore, be largely freed from its dependence on Everett. But as we shall see, it then fails to have the foundational significance for probability that we are after. We shall come back to this point in due course.

First Deutsch's decision theory axioms. We will prove only one of his results, and for that we only need two axioms. For simplicity, we assume that $\mathcal{P}$ is linear (so $\mathcal{P}(x_1 + x_2) = \mathcal{P}(x_1) + \mathcal{P}(x_2)$ - this is a convention on the labels λ_k). Let $f_s : R \to R$ be the function $f_s(x) = x + s$, and let $-I : R \to R$ be $-I(x) = -x$. The first axiom is:

Sure-thing principle : *Let* $g =< \phi, \widehat{X}, \mathcal{P} >$, $g' =< \phi, \widehat{X}, \mathcal{P}\circ f_s >$; *then* $\mathcal{V}(g') = \mathcal{V}(g) + \mathcal{P}(s)$.

I am indifferent between receiving $\mathcal{P}(s)$, and then playing game g, and playing g and then receiving $\mathcal{P}(s)$, whatever the outcome. But the latter is g'.

The second axiom is:

Zero-sum rule : *Let* $g =< \phi, \widehat{X}, \mathcal{P} >, g' =< \phi, \widehat{X}, \mathcal{P}\circ{-I} >$; *then* $\mathcal{V}(g) = -\mathcal{V}(g')$.

It must be possible for I and my banker to share exactly the same preferences, and to play the same game: what I am prepared to pay to play g, I pay to him. The most I am prepared to pay should be the least he is prepared to accept. But whereas I play g, he plays g'.

The rational for these principles can also be stated in a way that takes branching explcitly into account. For the first, if I accept $\mathcal{P}(s)$ before playing g, each of my successors inherits $\mathcal{P}(s)$ as well (for the utility too is subject to branching), and the situation at the end is the same as if I had played g'. For the second, if I am prepared to swap with my banker before playing g, being paid what I would otherwise have paid, each of my successors is swapped with my banker's successors, and pays what he would otherwise have been paid; but the latter is just g'.

With that it follows that for $\widehat{X} = x_1\widehat{P}_{\phi_1} + x_2\widehat{P}_{\phi_2}$, $\mathcal{V}(\phi_1 + \phi_2, \widehat{X}, \mathcal{P}) = \frac{1}{2}(\mathcal{P}(x_1) + \mathcal{P}(x_2))$, in accordance with the Born rule. For $\phi_1 + \phi_2$ is invariant under the permutation π of ϕ_1 with ϕ_2, so by payoff equivalence $\mathcal{V}(\phi_1 + \phi_2, \widehat{X}, \mathcal{P}) = \mathcal{V}(\phi_1+\phi_2, U_\pi \widehat{X} U_\pi^{-1}, \mathcal{P})$. By measurement equivalence this is $\mathcal{V}(\phi_1+\phi_2, \widehat{X}, \mathcal{P}\circ\pi^{-1})$. Since $\pi^{-1} = \pi = -I \circ f_{-x_1-x_2}$, by the sure-thing principle and the linearity of $\mathcal{P}$ one obtains $\mathcal{V}(\phi_1+\phi_2, \widehat{X}, \mathcal{P}\circ-I)+\mathcal{P}(-x_1-x_2)$. By the zero-sum rule and linearity again this is $\mathcal{V}(\phi_1+\phi_2, \widehat{X}, \mathcal{P}\circ-I) = -\mathcal{V}(\phi_1+\phi_2, \widehat{X}, \mathcal{P})$. So $\mathcal{V}(\phi_1+\phi_2, \widehat{X}, \mathcal{P}) = -\mathcal{V}(\phi_1+\phi_2, \widehat{X}, \mathcal{P}) + \mathcal{P}(x_1) + \mathcal{P}(x_2)$.

Deutsch called this his "pivotal result", and for good reason: it is the first time that any rational basis has been found to tailor one's subjective probabilities to the quantum mechanical ones. It is also the first step towards proving a general principle. Observe that the argument goes through for any $\phi \in H$ of the form $\phi_1 + \phi_2 + c\phi_3$, where $\widehat{P}_1\phi_3 = \widehat{P}_2\phi_3 = 0$; observe further that the antecedent can be stated as the condition that the Born rule for $x_1\widehat{P}_{\phi_1}$ yields the same value as for $x_2\widehat{P}_{\phi_2}$. So we have proved for any orthogonal projectors:

$$\textbf{Special Equivalence:}\ \text{If } \mu(\phi, \widehat{P}_1) = \mu(\phi, \widehat{P}_2) \text{ then } < \phi, \widehat{P}_1, \mathcal{P} >\sim< \phi, \widehat{P}_2, \mathcal{P} > . \tag{2}$$

Further axioms of decision theory are required to derive the analogous condition in which the vectors in H^a are different but the $\widehat{P}$'s are the same. Combining the two, we have [20]:

General Equvalence : If $\mu(\phi_1, \widehat{P}_1) = \mu(\phi_2, \widehat{P}_2)$ then $< \phi_1, \widehat{P}_1, \mathcal{P} >\sim< \phi_2, \widehat{P}_2, \mathcal{P} >$.

Given the general equivalence condition, the full representation theorem follows from decision-theoretic axioms that are exceedingly weak - axioms which, in point of fact, should be acceptable whatever one's views on what it is proper to believe in the face of personal division [20]. This full representation theorem is then none other than the principal principle (as is the general equivalence rule in the case of equiprobability). But there is an important difference, that the principle has been derived even under the condition that the agent has perfect knowledge. So it holds *unrestrictedly*; there is no room for the rider to the principle that a rational agent should be indifferent between playing two games if the objective probabilities for the same utilities are the same, whatever additional information she has, *provided it does not bear on the actual outcomes of the games* (a rider that has in fact proved notoriously difficult to state with any great precision). There is no need to exclude information of this sort because, of course, she knows everything there is to know about the outcomes of the games. As Wallace has stressed [20], an unrestricted principal principle cannot possibly be accepted, let alone deduced, on any interpretation of quantum mechanics in which only a single history is real; for why be indifferent between two games, even if they have the same probability as given by the Born rule, if you know their actual outcome as well; won't it depend on what those outcomes actually are?

5.3 Measurement neutrality

The general equivalence condition can in fact be stated in a way that is independent of any particular reference to experiments and independent of the schema we have used. As Wallace has shown [20], the derivation of the full representation theorem is all the simpler. Given general equivalence, the representation theorem is altogether unproblematic; the decision theory axioms are so weak that there is no need to consider what it is proper to believe on personal division. And it may be, on the basis of the Everett interpretation, that one can argue for the general equivalence condition directly.

But there is something to be said for the argument we have just sketched. We should hold fast to the belief that on fission, we should not anticipate oblivion; that, as argued by Parfit, psychological continuity is what matters, and not a relation with the formal properties of identity [11]. We should look forward to the same sort of first-person perspective, whatever it is, that we do in the absence of branching. But then our situation is one of subjective uncertainty, in the following sense: there is no *one* perspective that we should look forward to; we must, in some sense, entertain them all, and we must make provision for them all (or as many as we can reasonably survey), weighting them appropriately - *just as we do in the face of uncertainty*. It is only in special situations - for example, when *no* successor has some outcome - that one is entitiled to ignore that outcome completely, and give it zero weight.

These questions lead off into philosophy. The other sort of criticism that can be made of the Deutsch-Wallace derivation of the general equivalence condition (and the special equivalence condition as above) concerns the use of the schema for quantum games. Is this schematization (subject to pay-off and measurement equivalence) sufficiently detailed? Does it tell you everything you really want to know in the context of decisions about real-life experiments?

According to Everett, we certainly can characterize quantum games by triples of the form $< \phi, \widehat{X}, \mathcal{P} >$ (although that is common ground to a wide variety of approaches to foundations). Likewise the Everett interpretation licences payoff equivalence and measurement equivalence (although as mentioned, so can other approaches). But why should a rational agent take absolutely nothing else into account in determining her preferences? Mightened there be other features of quantum measurements (or quantum games) that are worth taking into account?

One can simply *deny* this possibility. A principle to this effectt has been called *measurement neutrality [18]*; it is the suspicion that measurement neutrality is too strong, or rests on unwarranted or incoherent assumptions as to what it is right to believe in the face of personal division, that prompts one to seek a more direct argument for the general equivalence condition. But better is to seek a direct argument for measurement neutrality.

Why believe in this principle? What more can be said of a measurement process, according to the Everett interpretation, not captured in the schema, and what is the rational for ignoring it? There is of course a vast amount of information that is *not* contained in the schema. There is *everything else* happening in each branch; there is the world outside the laboratory (assuming games are played in laboratories), and there are all the detailed goings-on in the laboratory that were unmentioned in the pay-off (including molecular goings-on that are not in fact dectected). But if these are deemed to matter to her, let them be put into her utilities, and let her prize those games whose pay-off functions include them explicitly; still, if she is rational, her preferences will be consistent with the Born rule, along the lines of the argument just given. The real issue, it is now becoming clear, is that the one thing that she *cannot* put into her pay-off function is the amplitude; it is not a possible pay-off for her that can be arranged. For there is no dynamical process, according to Everett, whereby the amplitude of a branch can be measured, and its value exhibited by a display, or otherwise encoded into the pay-off function $\mathcal{P}$. *No* unitary evolution can ever achieve that.

This goes a long way to explaining why the amplitudes should play the part in our mental lives that they do, when it comes to our preferences (appearing, distinctively, in the weights with which we view outcomes, rather than as part of the outcomes), and it makes clearer the generality of our schema (that it can in principle be applied to arbitrarily complex initial conditions and pay-off functions). But it will not do to explain why nothing else but amplitudes can matter to these weights. It is one thing to care about details of the apparatus as goes the physical outcomes produced by it - these should simply be entered into one's utilities. But what about details of the apparatus that effect its dynamical functioning, that are not captured in our simple schema?

There are of course a vast range of physical considerations bearing on the detailed dynamics of an experiment, but it is reasonable to distinguish between those that make a difference to the branching structure and those that do not. We are speaking of a rational agent who may know everything there is to know: for her, subjective probability, weights, arise in the first instance on personal division.

Any dynamics not involving branching concerns the purely deterministic development of the branch (insofar as branches can ever evolve deterministically).

Experiments modelled in the same way by our schema that differ on this are likely to have different efficiencies and will differ in their systematic errors and the ways they are prone to malfunction. There is no reason why rational agents should regard them as precisely equivalent, but equally, just because they concern deterministic effective processes, they seem unrelated to the foundational questions about probability.

It is otherwise with distinctions among experiments that turn on differences in branching. Again, they break down into two kinds. The first concerns the precise details whereby an initial entanglement of microscopic system with the measuring apparatus is produced, including the branching that takes place as progressively larger numbers of degrees of freedom are entangled with it. This takes place over decoherence timescales and is extremely rapid. The second kind concerns subsequent branching unrelated to the coupling of the apparatus to the microscopic system, usually considered in terms of statistical fluctuations (and that go on continuously whether or not any measurement are performed). Of course experiments attempt to control for the latter - this is noise in the signal that is better or worse eliminated - but in no sense is it possible to model this kind of branching explicitly. It is dealt with again at the level of effective equations.

We are left with the key arena in which the initial entanglement is established and subsequently propagated to include large numbers of degrees of freedom - the business of measurement theory proper. Is it not reasonable that a rational agent treat differently experiments that differ in these respects, even if they are modelled in the same way in our schema of quantum games? This is the key objection to the derivation of the general equivalence principle from decision theoretic principles: the amplitudes (and weights as given by the Born rule) may be the same, but the branching structure introduced by the measurement may differ wildly.

5.4 Decoherence, again

The objection that branch number may have a role in dictating preferences has to be understood in the right way. It is not that the number of outcomes counted as distinct when it comes to the pay-off function - equivalently, the number of gradations on the instrument read-out - may not matter to our preferences: that number is already explicit in our schema (in the specification of the triples $<\phi, \widehat{X}, \mathcal{P}>$). It is hard-wired in the instrument display. As such it is available to a rational agent, to be incorporated in her utilities if she will. So she may favour quantum games with five outcomes rather than four, because five is her favourite number; or she may dislike outcomes with thirteen in the display. Anything physically realized in any branch can always be entered into her utilities, and be looked after at the level of her payoff function, without compromising her conduct as a rational agent - and therefore in accordance with the Born rule. She will act irrationally, however, if she believes her liking for fives is a reason to *weight* outcomes of games with five in the display as greater than those without, or to set the *weight* of any outcome with thirteen in it to zero. If she does this, she will have to violate the sure-thing principle or the zero-sum rule in some cases; or to hold the consequences of the Everett interpretation for pay-off equivalence and measurement equivalence to be false. Exactly the same applies if she assumes that each outcome, corresponding to each gradation of the display, should have equal weight: she will be convicted of irrationality or ignorance of quantum mechanics.

The objection is not that. It is that the schema for measurements is leaving something out of account, not that what is does contain can be acted on irrationally. It is that for each outcome with each given pay-off, *the number of branches all with that outcome* has been ignored. Like amplitudes, and unlike the number of gradations on the instrument display, this is not information that can be factored into one's utilities; these are not numbers that can be reliably realized in a branch by any unitary dynamics. All the more reason, then, to think that they may be relevant to agent preferences in the way that amplitudes are, so relevant to her weights.

But the answer to this should now be obvious. *There is no such thing as this number.* The only significance it has in concrete physical terms is what is coded up in the number of instrument gradations. it is true that one can specify such a number theoretically, for a given choice of decoherence basis, but it has no categorical physical significance; it is not part of what is really there.

The reason no other number can be defined has already been rehearsed; it is because decoherence is an imprecise concept. Formally, the number of decohering branches corresponds to the number of decohering projectors - and, one has to add, "for a particular choice of coarse graining on configuration space". There is no such thing as *the finest decohering set of projectors*. The picture, in Everett theory, of the wave function of the universe as an endlessly branching tree, breaks up as one goes into the fine detail. It is no criticism of the Deutsch-Wallace argument that it leaves out of account a physically meaningless quantity. The same applies to the branching that takes place constantly, independent of the measurement of the microscopic system *per se* ("background noise"); there is no such thing as the number of branches produced in this way either. It is no part of any principle of rationality to take note of what is not there.

We have come full circle. A rational agent, who knows everything there is to know about the physical world, will still have preferences among quantum games, and she ought to order her preferences consistently. In so doing (for a sufficiently rich set of games) she will act as if assigning a unique set of weights to outcomes (independent of the utilities that she assigns to them) that have to obey the rules of probability theory. If she believes quantum mechanics to be true, under the Everett interpretation, she will consider the schema for quantum games a reasonable idealisation of what goes on in measurements, subject to outcome equivalence and measurement equivalence. Moreover, she ought to believe in the sure-thing principle and the zero-sum rule (although here there are weaker principles that will do as well), so she ought to conclude from the equality of the norms of amplitudes for outcomes to equality of the weights that she gives to them. She ought to believe in the special equivalence condition.

And so on, to the general condition and the full representation theorem. But the reason she should consider the schema for experiments adequate (although she ought to have quibbles over the neglect of detector inefficiencies and the presence of background noise) is because her subjective weightings depend on branching, and branching depends on decoherence; it is because of what the chances are, in physical terms, that there is no fact of the matter as to the number of branches. And, conversely, it was exactly because decoherence is a matter of approximation, that if chance is to emerge with decoherence, then it had better be stable under changes in coarse-graining - equivalently, under change in branch number - that we were able to isolate the ratios in modulus squares of the amplitudes (for decohering projectors) as the only invariant quantities that could play the role of chance.

Subjective and objective probability emerge at the end of the day as seamlessly interjoined: nothing like this was ever achieved in classical physics. Philosophically it is unprecedented; it will be of interest to philosophers even if quantum mechanics turns out to be false, and the Everett interpretation consigned to physical irrelevance; for the philosophical difficulty with probability has always been to find *any* conception of what chances are, in physical terms, that makes sense of the role that they play in our rational lives.

Still, the Everett interpretation is inherently fantastic; one would like if possible to free the argument from any dependence on it. Yet we encountered again and again points on which the Everett interpretation played a critical role - where the very features of the approach that make it unbelievable were of special salience. Introduce additional elements, over and above the unitary dynamics - whether hidden-variables or an additional stochastic dynamics controlling the state - and the symmetries used to drive the proofs for the individual case *have* to be broken. Try to reformulate the derivation so as to apply to probability distributions over ensembles, and we are back to the same foundational questions about the latter as in classical theory. And meanwhile the very conclusions of the argument become obviously untenable: the unrestricted special equivalence condition that we derived is incoherent if there is only a single history. The arguments we have considered give no hope at all that one can derive the principal principle on any basis but Everett's.

It is ironic that the interpretation of probability in the Everett interpretation has always been thought to be its weakest link. On the contrary, it seems that it is one of the strongest points in its favour.

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