Why I am not an Everettian[1]

Wayne C. Myrvold
Department of Philosophy
University of Western Ontario
wmyrvold@uwo.ca

1. Introduction. In this paper I try to present, as clearly as possible, why I do not believe that we have good reasons to believe that the world is as it is depicted in the Everett interpretation of quantum mechanics. Unlike some critics of Everett, I am not going to present objections on grounds of incoherence or metaphysical extravagance, objections that, if successful, would give a congenital Everettian who was born believing the theory grounds for abandoning it. The question to be addressed is, rather, "should we believe the Everett interpretation in the first place?" (Wallace 2002, 19). The critique will be along lines similar to some that have been pursued before, by others (Albert, Albert & Loewer, Barrett), that the interpretation undermines the empirical evidence that provides the reasons for believing quantum mechanics. These criticisms were raised prior to the development of the decision-theoretic approach to Everettian probability, initiated by Deutsch(1999), and elaborated by Wallace(2002, 2003*a*,*b*), Saunders(2004), and Greaves (2004). This approach has gone a long way towards showing how the Everett interpretation can be made into a coherent view,

[1] This paper formed the basis of a talk presented at Oxford University on February 10, 2005. My views have changed somewhat, largely as a result of ensuing discussions. See Greaves and Myrvold, "Everett and Evidence," also on the conference web site, for one approach to the Everettian evidential problem discussed in this paper. I am particularly grateful to Matthew Donald, David Wallace, Simon Saunders, Paul Tappenden, and Hilary Greaves for helpful comments and discussions.

and may seem to provide grounds for optimism that it will provide the resources for answering the critics. It is the prospects for that that are the subject of this paper.

First, a few words about what the Everett interpretation is. In discussing the quantum measurement problem, J. S. Bell famously remarked, “Either the wavefunction, as given by the Schrödinger equation, is not everything, or it is not right” (Bell 1987, 201). Hidden-variable theories and modal interpretations grasp the first horn of the Bell’s dilemma, denying that the physical state of a system is exhausted by its wavefunction, whereas dynamical collapse theories grasp the second, denying that the Schrödinger dynamics always correctly yield a system’s wavefunction. The Everett interpretation—or, perhaps, Everett interpretations, as it is not clear that there is only one—attempt to avoid both horns of Bell’s dilemma. Thus the interpretations with which we will concerned are characterized by two claims. One is ontological: the state of a system is to be found in its quantum state (represented by a wave function, state vector, or pure density operator), without any additional structure, hence any physically meaningful structures are to be found in the state vector. The second is a dynamical claim, that quantum state evolution is always deterministic and unitary.

Unitary evolution, in a typical measurement situation, leads to a state which is a superposition of states corresponding to distinct measurement outcomes. The Everett interpretation affirms that this is, indeed the correct post-measurement state. It also denies any extra structure that would distinguish one of the branches of this superposition as the one that we should take to be the actually occurring outcome; all branches are to be regarded as equally real or unreal.

Objections to this interpretation have been raised on grounds of alleged incoherence, and on accusations of metaphysical extravagance or of being counterintuitive. The decision theoretic programme is a great advance towards defending the coherence of Everettism, and for the purpose of this paper I will grant the success of this endeavour, either as actuality or as potentiality. The latter sort of objection strikes me as not particularly strong. Even if Ockham's razor would rule the Everett interpretation out of serious discussion (and it is by no means apparent that this is the case; see Brown and Wallace 2005), this does not count as a reason to doubt that something like the account given by the Everett interpretation is true unless Ockham's razor is accorded a status equivalent to an *a priori* synthetic truth. I am too much of an empiricist to be willing to accord it that status. Claims of counterintuitiveness carry no more weight. If Everett is right, the world is a multiverse containing many branches evolving more or less independently, many of which contain near copies of ourselves. This is certainly very different from the way the world is usually conceived, but this, in itself, is not an objection; we should be prepared to learn that the world is very different from the way we think it is, *should we have sufficient evidence to that effect*. The question to be addressed, therefore, is whether we have good evidence that something like the account delivered by quantum mechanics interpreted *à la* Everett is true. For this, we will require some preliminary discussion of the evidential bearing of observations on theories.

2. Sketch of an account of evidential support. The conception of the bearing of evidence on theories that seems to underlie many discussions of interpretations of quantum mechanics, and, indeed, with varying degrees of explicitness, much of the philosophical discussion of the bearing of evidence on theory, is what I will call the *deductive-metaphysical* picture. On this picture, a theory is tested by comparing observations with the observational consequences entailed by the theory in conjunction with suitable auxiliary hypotheses; the theory is disconfirmed if the consequences turn out to be false and we are not willing to reject any of the auxiliary hypotheses; it is confirmed by those consequences that come out true. Such a picture leads, not inevitably but fairly naturally, to the notion that all theories that are compatible with a given body of evidence are equally well confirmed by that body of evidence. This, in turn, leads to empirical underdetermination of theory by evidence and the notion that the choice among theories compatible with the evidence is to be made on extra-empirical grounds, such as simplicity, elegance, or some other prejudice elevated to the status of a principle (see Quine 1990 for one presentation of this picture).

The deductive-metaphysical picture of theory choice, with the attendant assumption that all theories compatible with a body of evidence are equally well confirmed by it, seems to be to an implicit assumption of many of the discussions of the interpretation of quantum mechanics. It is worthwhile making it explicit, because when one does so, it can clearly be seen to be an inadequate account of the relation of theory to evidence.

This is easiest to see in the case of stochastic hypotheses, that is, hypotheses that contain statements about chances. Suppose, for example, we are wondering whether a coin is a fair coin, and consider the two hypotheses:[2]

H_1: The coin is fair.

H_2: The coin is biased two to one in favour of heads.

We do an experiment that consists of flipping the coin 1,000 times, and get 673 heads and 327 tails. This evidence is *compatible* with both hypotheses, but it seems rash to say that the two are *equally well supported* by the results of our coin flip. Clearly, the results of the coin flip experiment count against the hypothesis that the coin is fair, and count much more strongly in favour of H_2. This much should be non-controversial: it is simply false that

[2] For brevity, we shall speak of the coin as being fair or having a bias. Such considerations properly apply to the coin-tossing procedure, or the *chance set-up* being considered, and everything said about the bias of the coin should be taken to be short-hand for talk about the bias associated with the chance set-up.

compatibility with the evidence exhausts the empirical support that the evidence lends to a hypothesis.

As remarked, the inadequacy of the deductivist-metaphysical is easy to see in the case of stochastic hypotheses. The picture is no better when applied to deterministic hypotheses, but this is perhaps less easy to see, and at any rate is of secondary importance, since, even when testing a deterministic theory, the model with which we confront the data is a stochastic one consisting of the deterministic predictions plus experimental error, treated as a random variable with some chance distribution. Quine has famously claimed, on the grounds that all hypotheses compatible with the data are equally well supported by it, that any hypothesis can be defended in the face of any apparently disconfirming evidence because one can always plead hallucination (Quine 1953, 46). This desperate resort will rarely be necessary, as one can always plead experimental error. Anyone who seriously believed that all hypotheses compatible with the data are equally well supported by it ought to take classical mechanics as equally well supported by the experimental data as quantum mechanics; it is, after, logically possible that random experimental errors have conspired to give such excellent agreement with quantum mechanics, masking the fact the world is, after all, classical.

The almost unavoidable response to this last remark is that it is possible, yes, but highly unlikely, and this response is the key to a more adequate account of confirmation. Such an account starts with the recognition that belief in a theory is not an all-or-nothing affair but instead admits of grades; one may be firmly committed to one proposition, less firmly to another, and strongly disbelieve a third. One can argue that such degrees of belief, to the extent that they are rational, should satisfy the axioms of the probability calculus. Other arguments can be given to the effect that a change of belief consisting of learning, with certainty, that a proposition E is true, and nothing else, should be modelled by conditionalization on E.

Thus, as a sort of first-order approximation to scientific inference, we may model the belief state of an epistemic agent by a probability assignment, and learning from observations by conditionalization on the results of those observations. The canons of inference that result from such a picture are meant to have the same status as the canons of deductive logic; they serve as a measure by which to judge the extent to which actual agents are rational. We are not engaged in developing a psychological model of the process by which human beings make inferences, and, indeed, the model would be a very poor one if it were mistaken for such. For one thing, it is too much too ask of an agent with finite cognitive capacities that she assign precise real numbers to propositions as her degrees of belief. It is also too much to ask that her relative judgments of propositions as more or less probable, even if they are rough and qualitative, be compatible with the existence of degrees of belief satisfying the axioms of probability, as, in order to do so, they would have to respect the logical relations between propositions, which are not always transparent to beings with finite cognitive capacities.

It is possible, and in some contexts desirable, to modify the basic model so as to make it more relevant to the epistemic states of beings with finite cognitive capacities. The relevance of evidence already learned to hypotheses might not be immediately transparent to such beings, and so one should admit the possibility of a change of opinion that comes about as a result of cogitation (perhaps induced by conversation with an agent with different opinions); this could include the introduction of a new hypothesis not previously explicitly considered, reflection on which causes one to shift one's degree of belief in other propositions. In addition, the results of observations are not indubitable; it is possible to be mistaken about what thinks one sees and one should take into account that further evidence may lead one to a change of opinion about what seemed to be incontrovertible evidence (hence assigning the evidence probability one is not quite appropriate). What results, when such modifications are made, remains a normative theory of inductive reasoning.

3. Probability. There is a vast literature on interpretations of probability. The name "interpretations of probability" is misleading, however, insofar as it suggests that probability is a univocal concept and the various interpretations are rival candidates for the uniquely correct explication of this concept. Instead, as has long been recognized, there are distinct but interrelated concepts that sometimes get called "probability."

We will be interested in three of these: credences, or rational degrees of belief, physical chances, and relative frequencies in ensembles. These are three distinct though interrelated concepts, and all three of them seem to me to be perfectly intelligible concepts. I will not offer an 'analysis' of any of these concepts, if by analysis is meant an attempt to define the concept in terms of something else; it is not clear to me that this is possible or that it would be useful if it were. I will, however, offer some remarks about each, in turn.

3.1 Credences. We will assume that rational degrees of belief, defined on a set of propositions, satisfy the calculus of probability. There have been attempts to uniquely define the degrees of belief that an ideal agent will place in any proposition on any body of evidence. I do not believe that this project has any prospect of succeeding, nor do I see any reason why it should not be the case that any given body of evidence leaves open a range of probability assignments as reasonable ones in light of the evidence. Theories of this sort are sometimes called "personalist," which unfortunately suggests that we are lapsing into bad armchair psychology. There seems to be a notion that a normative theory must uniquely prescribe degrees of belief, whereas one that leaves a range open becomes at least partly descriptive, rather than normative. This is odd; we don't expect an ethical theory to prescribe a unique moral action in every situation, but only to distinguish between those that are moral and those that are immoral. We will not, therefore, assume that a body of evidence uniquely determines a rational belief function. We will not, on the other hand, assume either that all belief functions satisfying the axioms of probability are equally reasonable. Our personalism will thus be of the 'tempered' sort, to use Abner Shimony (1970)'s apt phrase.

3.2 Chances. In *Surely You're Joking, Mr. Feynman*, Richard Feynman relates his skepticism upon hearing that there is a person named Nick the Greek who makes a living as a professional gambler. The reason for Feynman's skepticism is that he knows that the odds in a casino favour the house; hence, though not impossible, it is highly unlikely that any patron of the casino will consistently come out ahead. Upon meeting Nick the Greek, Feynman learns that he makes his money, not by betting against the casino, but by making side bets with other patrons. He is able to do this successfully because, as Feynman puts it, "he knows the odds," understands probability, and is able to select from the multitude of bets offered him those that are favourable to him.

It seems to me that this story is intelligible, and I expect most readers to agree that Nick the Greek knows the odds. Moreover, these odds that he knows are characteristic of the *chance set-up* that produces the outcomes on which bets are placed, be it a spin of a roulette wheel or a throw of the dice. They are not to be identified with degrees of belief; knowing his own degrees of belief won't give the professional gambler an edge over the drunken tourist. Nor are they to be identified with relative frequencies in a large ensemble. Bets are made on single-case events, and chances are chances of outcomes in single cases. There has been some confusion about this, because often our best evidence about chances comes from relative frequencies, and it can be possible to be virtually certain that the actual chance is very close to the observed relative frequencies.[3]

---

[3] Physical chances seem to be what Popper (1956) calls *propensities*. There is room for some doubt, however, that Popper's propensities are what we are calling chances, because Popper is commonly said to have introduced, not merely the word "propensity," but the very concept (see e.g. Kyburg).

Physical chances, conceivably, could be of two sorts. One possibility is that the fundamental physical laws are stochastic rather than deterministic. That is, the physical state at a time, together with all relevant physical laws, might not uniquely determine future events but instead define a range of possibilities to which the laws assign chances. This seems no less intelligible that deterministic physical laws. The other possibility is that the physical laws are deterministic, but that any chance set-up to which one can attribute a determinate chance generates a range of initial conditions. The chances, in the deterministic case, are symptomatic of incomplete knowledge of initial conditions. It might be the case, however, that the physical nature of the chance set-up prevents an observing agent from having precise knowledge of these conditions (great care is taken to ensure this is a casino situation), and hence the chance represents an optimal betting ratio for *any* agent observing the chance event. Degrees of belief may be subjective, but the optimal betting ratio that any agent could have in a given physical situation is a physical fact about the set-up.

The physicist will want to give a more detailed account of the physical processes that underlie these physical chances, and in particular will want to know what it is about the initial conditions and/or dynamics that makes the chances what they are. Nick the Greek, however, does not need to know these things; he only needs to know what the chances are, and he can know them without knowing much about the physics involved. Moreover, it is important that the chances can be known without knowledge of the underlying physical mechanisms, as the physicist will want to use the known values of these chances as a test of theories concerning these mechanisms.

The concept of chance used here, however, was present from the beginning of the development of the theory of probability, and was used by Cardano, Fermat, Pascal, Huygens. Moreover, Popper is alleged to have created a "propensity theory of probability" (Hacking 1990, 122). The theory of chances, initiated in the 17th century by the just-mentioned authors and developed extensively by subsequent mathematicians, is not one to which Popper has made any discernible contribution.

It is sometimes alleged that the concept of physical chance is mysterious or unintelligible, and attempts have been made to replace it with something else. For example, Lewis (1994), recognizing that physical chances fit into the Humean picture he advocates no better than deterministic physical laws do, attempts to replace them with patterns of actually occurring events, at the cost of incompatibility with the Principal Principle (for a statement of this principle see section 4, below). An appropriate response to such attempts is the earlier advice of Lewis himself,

> Like it or not, we have this concept. We think that a coin about to be tossed has a certain chance of falling heads, or that a radioactive atom has a certain chance of decaying within the year, quite regardless of what anyone may believe about it and quite regardless of whether there are any other similar coins or atoms. As philosophers we may well find the concept of objective chance troublesome, but that is no excuse to deny its existence, its legitimacy, or its indispensability. (Lewis 1980, 269; 1986, 90)

It should be remarked that, whatever metaphysical worries one might have about physical chances, there are no grounds for empiricist worries; physical chances are measurable quantities. We measure the value of a physical chance by performing multiple experiments which are all thought to have the same physical chance for a given class of outcomes; for a sufficiently large number of these, the chance is high that the observed relative frequency of outcomes in this class will be close to the actual value of the physical chance (see section 4, below). It is possible, of course, that the observed relative frequency will be far from the chance we are trying to measure. This procedure is, like any measurement procedure, a fallible one.

3.3. Relative frequency. There have been attempts to eliminate the concept of physical chance and replace it with an analysis in terms of relative frequencies. Frequencies in finite runs will not do, as such frequencies can, and typically will, differ in value from the single-case chance. One therefore attempts to invoke a Law of Large Numbers to the effect that relative frequencies will in the infinite limit certainly approach the actual chance. This is tempting to those who for metaphysical reasons believe that they must deny the existence of single-case physical chances, but the attempt does not succeed. The theorems invoked prove, not that other limiting frequencies are impossible, but that they have probability zero; hence the very statement of the theorem (and also the proof of the theorem) involves a notion of probability distinct from limits of relative frequencies. We use relative frequencies in large ensembles to *test* claims about single-case chances, but single-case chances and relative frequencies in ensembles are not the same.

4. Evidence about chances. It was remarked above that credence, physical chance, and relative frequencies are interrelated concepts. The relation that we will be interested in is: relative frequencies provide evidence about (and hence grounds for changing credences about) chances.

I will go through an example of updating credences about chances in what some readers will consider excessive detail, because, though nothing in the example will be new to most readers, I want these familiar facts to be before our minds when discussing the evidence for quantum mechanics. Consider, then, a coin whose bias we wish to learn about. We assume that each toss of the coin has the same chance λ of coming up heads, and that distinct tosses are independent. Statements of the form $\lambda \in \Delta$, where Δ is a Borel subset of the unit interval, are statements about the chance set-up and hence the right sort of thing to have degrees of belief about. Let my initial credence function be $Cr(\cdot)$. Let us suppose that my initial credences about the value of λ can be represented by a density function f, so that, for any Δ,

$$Cr(\lambda \in \Delta) = \int_{\Delta} f(x)\, dx .$$

Suppose, now, that I flip the coin ten times, and get a sequence, say,

THHTTTTHTT

having m heads and n tails. Call this result—that is, the sequence actually obtained—E. I update my credences by conditionalizing on E,

$$Cr(\lambda \in \Delta) \quad \Rightarrow \quad Cr(\lambda \in \Delta \mid E) = \frac{Cr(E \mid \lambda \in \Delta)}{Cr(E)}\, Cr(\lambda \in \Delta) .$$

Now, to evaluate this, we need to evaluate $Cr(E)$, the prior degree of belief that E would be the outcome of the experiment, and $Cr(E \mid \lambda \in \Delta)$, the credence in E conditional on the chance of heads being in the set Δ. We have our initial credences about the chances, and we know, for any value of x in the unit interval, what the chance of E would be if the chance of heads on a single toss were x: it would be $x^m (1-x)^n$. Lewis (1980)'s Principal Principle[4] tells us how to use these to get the credences we're trying to evaluate. If your degree of belief in the proposition,

[4] Called "Homecoming" by Richard Jeffrey (2004, 20).

The chance of event A is equal to x.

is $Cr(ch(A) = x)$, then, according to the Principal Principle your degree of belief in the conjunction

The chance of A is x, and A will occur.

should be simply x times your degree of belief in the proposition that the chance of A is x. That is,

$$Cr(ch(A) = x \; \& \; A) = x \, Cr(ch(A) = x).$$

Similarly, if your credences about the values of the chance of A are given by a density function f, then your credence in the proposition '$ch(A) \in \Delta \; \& \; A$' should be

$$Cr(ch(A) \in \Delta \; \& \; A) = \int_{\Delta} x \, f(x) \, dx \, .$$

This has been glossed as "our subjective probability should track chance" (Saunders 2004, p?), but, since the Principle applies, not only in situations in which the chances are known with certainty, but also in situations of uncertainty about chances, a better gloss would be: our credences should track our *beliefs* about the chances.

Against the usual background of decision theory, on which decisions are to be made on the basis of maximizing epistemically weighted expected utility, an equivalent formulation is as follows.

Principal Principle, Decision-Theoretic Version. If, somehow, you know that an act A would result in one of the consequences $\{C_i\}$, with chances $ch_A(C_i)$, and you attach values $V(C_i)$ to these consequences, then you should value the act A at the chance-weighted expectation value, $\sum_i ch_A(C_i)\,V(C_i)$.

The Principal Principle is taken for granted in statistical reasoning and almost always passes without comment (in fact, it can be difficult, sometimes, to get statisticians to see that any principle is involved and that satisfaction of the Principle places constraints on credences beyond satisfaction of the probability axioms), and it will be assumed in what follows, as a constraint on reasonable credences.

Application of the Principal Principle gives us

$$Cr(E) = \int_0^1 f(x)\, x^m (1-x)^n\, dx,$$

and,

$$Cr(E \mid \lambda \in \Delta) = \frac{\int_\Delta f(x)\, x^m (1-x)^n\, dx}{\int_\Delta f(x) dx}.$$

Inserting these quantities into our formula for conditionalization gives the result that the density function f is multiplied by a factor proportional to the likelihood function $x^m(1-x)^n$, shown in Figure 1 for the case $m = 3$, $n = 7$.

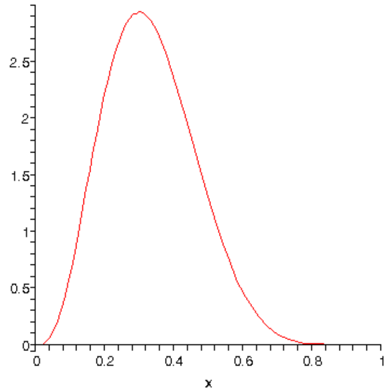


Figure 1

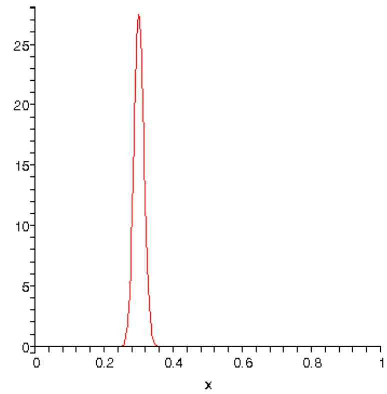


Figure 2

This function is peaked at the observed relative frequency $m/(m + n)$, hence credences about chances close to the observed relative frequency are boosted and credences about chances far from the observed relative frequency, diminished. The larger the number of trials, the more sharply is the likelihood function; Figure 2 shows the function for 1000 trials, with 300 heads and 700 tails.

If my initial density function about the value of λ is flat, indicating complete ignorance about the value of λ (a bizarre state of mind to be in, but let us consider it nonetheless), then my updated density function has the form of the likelihood function (see Figure 2).

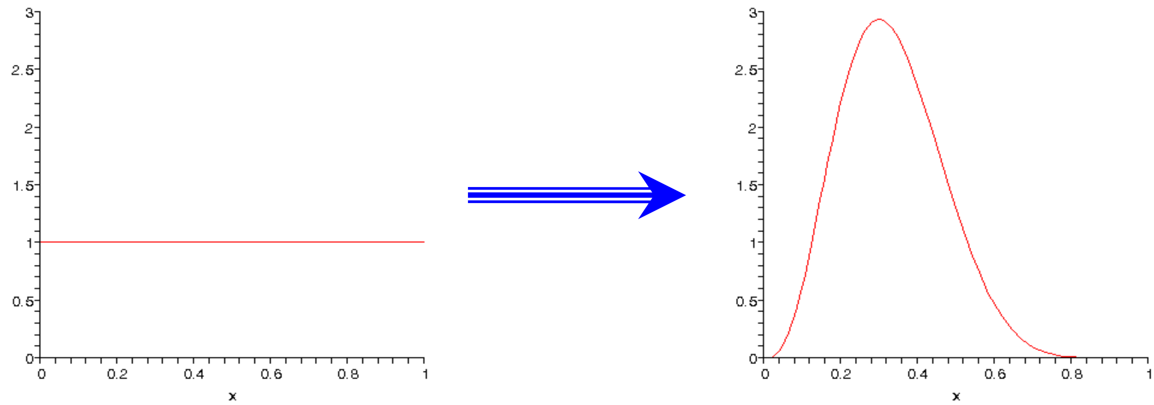


Figure 3.

If, on the other hand, I am initially fairly confident that the coin is fair (perhaps based on inspection of the coin for visible asymmetries), the prior and posterior density functions might look something like those in Figure 4. The peak, initially at ½ , is shifted slightly in the direction of the observed relative frequency, but not much, indicating that my initial confidence that the coin was fair is not shaken much by getting 3 heads on 10 tosses.

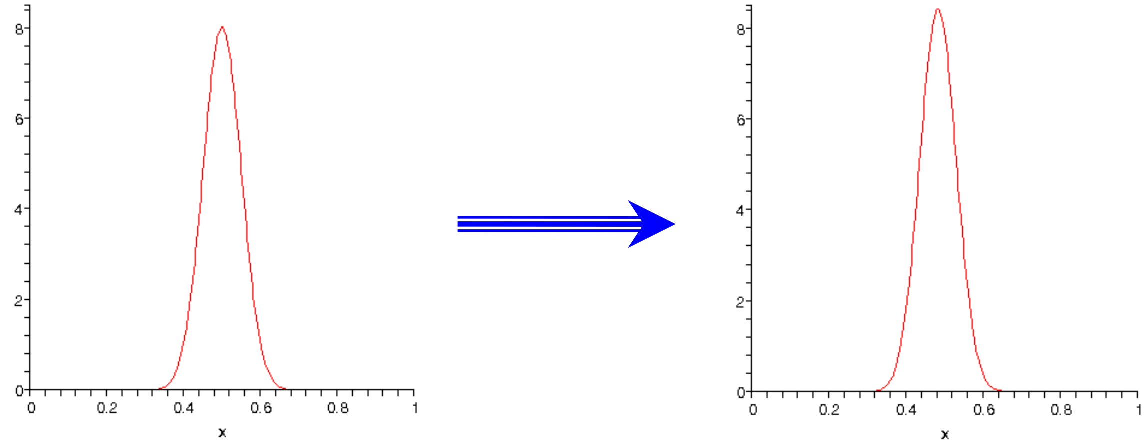
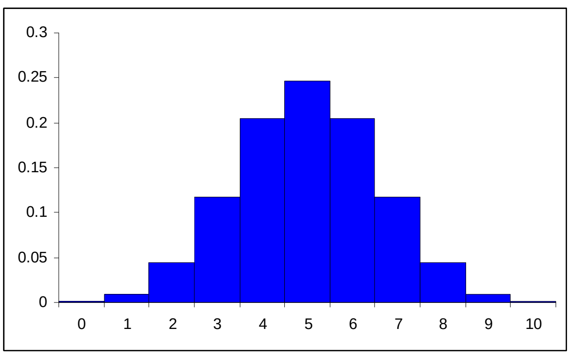


Figure 4.

A very large number of trials results in a likelihood function sharply peaked around the observed relative frequency. Hence agents updating their credences about chances, unless they are excessively dogmatic and assign zero prior credence to the chance being in some interval into which the observed relative frequency happens to fall, will, with a sufficient number of trials, end up with their credences sharply peaked around the observed relative frequency. We can also ask what the chance is that this observed relative frequency is close to the actual single-toss chance of getting heads. If the chance of heads on each of N independent trials is λ, then the chance of getting m heads and $n = N - m$ tails is

$$ch_N(m \;\; Heads) = \frac{N!}{m!\, n!} \lambda^m (1-\lambda)^n .$$

This is shown for a fair coin ($\lambda = ½$), for the cases of 10 flips and 1,000 flips, respectively, in Figures 5*a* and 5*b*.



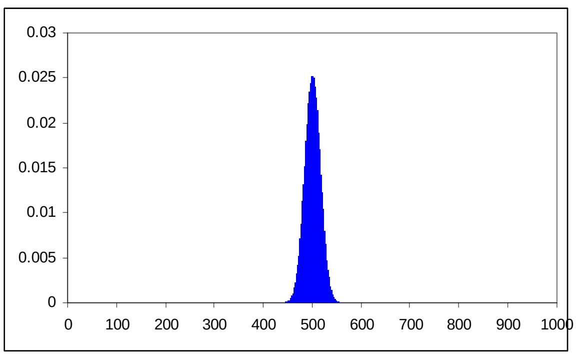


Figure 5*a* **Figure 5*b***

Therefore, for a large number of coin flips, the chance is high that the observed relative frequency of heads will be close to the chance of getting heads on a single flip. Since, for all but excessively dogmatic initial credences about chances, our credences will end up peaked around the observed relative frequency for experiments involving sufficiently large numbers of coin flips, this means that, for such experiments, the chance is high that our credences will end up peaked near the actual chance, and low that they will do otherwise.

Statistical inference is often cast in a guise that makes it appear more akin to the deductivist model. In such a guise, one is advised to accept, on the basis of the results of an experiment such as our coin toss, the proposition that the actual chance lies in a certain interval (say, one standard deviation) around the maximum likelihood value (which, in this case, is the observed relative frequency). For many purposes, there will be little practical difference between adopting such an approach and updating one's credences by conditionalizing on the evidence, and it is often more expeditious to adopt a simple rule of thumb. The rule of thumb, to the extent that it is effective, is effective, not because the results of the experiment entail that the actual chance lies in such an interval, but because conditionalizing on the results leads to high credence that it is.

Substitution of such a quasi-deductive inference rule can be a harmless substitute for conditionalizing on the evidence, when the proposition that one is asked to accept as if it were entailed by the evidence receives a posterior credence close to one after conditionalizing on the evidence. It is important not to lose sight of the fact that, though such rules make statistical inference look somewhat like deductive inference, their rationale is probabilistic.

Our coin-tossing example illustrates the insufficiency of a single notion of probability. We have considered credences about chances, credences about observed relative frequencies, chances of relative frequencies, and chances concerning how our credences will end up.

Since we have been talking about evidence about chances, it is worth making a remark about the notion that knowledge of chances is obtainable *a priori*, on the basis of a principle of indifference.[5] Classical probability theory tended to focus on the special case in which one has a finite number of mutually exclusive and jointly exhaustive alternatives that are equiprobable (a case that manufacturers of gambling devices often take care to approximate). Judgments of equiprobability can often be made on the basis of physical symmetry, and this has given rise to the notion that judgments of chance can be made in a wholly *a priori* manner, on the basis of a principle of indifference that states that two alternatives are to be assigned equal probability in the absence of knowledge of any reason to judge one more probable than the other. This seems like magic, and it is; knowledge of chances is being generated out of subjective ignorance, and, just like stage magic, it is an illusion. The illusion is facilitated, I think, by a feeling that to judge two alternatives equiprobable is to refrain from making a judgment about them. But a judgment of symmetry is a judgment that the two alternatives are symmetrical *with respect to all factors relevant to the chances* (since the alternatives will differ on factors deemed irrelevant), and this is a judgment about which factors are relevant to the chances, which may involve judgments about the physical dynamics of the situation. Such a judgment is to be made on the basis of empirical evidence, and can be either confirmed or disconfirmed by further evidence.

The notion that a principle of indifference of some sort could generate *a priori* knowledge about chances has, perhaps, been encouraged by a conflation of chance and credence. There are many different belief states about chances that will lead to assigning

[5] "that notorious dead horse of the philosophy of probability" (Butterfield 1996, 212).

credence ½ to the proposition that the next flip of a coin will land heads. One is the case of complete ignorance about the chance of heads, exemplified by the flat credence function shown on the left side of Figure 3. Another is the case in which the agent knows, with certainty, that the chance of heads is ½ . It is easy to understand that these two cases differ, as long as one distinguishes chances from our credences about them; failing to do so encourages conflation of the two cases. To conflate the two cases is to conflate complete ignorance about the chance of heads with the certain knowledge that the chance of heads is ½—exactly the sort of knowledge out of ignorance that the principle of indifference is meant to provide.[6]

5. Everettian probability. Quantum mechanics, standardly interpreted, is a stochastic theory; it assigns chances to the results of experiments. Many of the experiments that we take to provide evidential support for quantum mechanics are experiments in which relative frequencies are used to provide evidence about the chances of these results. The cumulative evidence strongly supports the claim that, at least in the domains in which quantum mechanics has been tested, the chances yielded by a quantum-mechanical calculation closely approximate the actual chances. These experiments yield evidence about the values of physical chances independently of whether quantum mechanics itself is true. One can coherently consider alternatives to quantum mechanics while taking the results of such experiments as constraining theories that have some hope of being empirically well-supported.

[6] If there is skepticism that the two belief states in fact differ in any meaningful way (since two agents with these belief states will make the same bets on the next coin toss), such skepticism can be allayed by noticing that the two belief states differ on credences assigned to the results of two consecutive coin tosses. An agent certain that the coin is a fair one will assign credence ¼ to the result *HH*, whereas an agent whose credences about chances are flat will assign credence 1/3 to this result.

The interpretational programmes on either horn of Bell's dilemma take these chances to be physical facts that we can gather evidence about. Dynamical collapse theories, that modify the quantum dynamics, are stochastic theories. They are designed to produce chances that closely approximate those calculated by the usual quantum rules, at least in the domains in which these predictions of quantum mechanics have been tested (since such theories are designed to suppress certain kinds of superpositions, they will inevitably yield some chances that differ significantly from those calculated on the assumption that the wavefunction is that given by Schrödinger dynamics, and these differences should, at least in principle, be testable). On Bohmian mechanics and other deterministic hidden-variable theories, chances are much as coin tosses have traditionally been conceived. The claim that such theories make about chances are, again, subject to empirical test. One can consider the possibility of distributions of the hidden variables that yield chances different from the usual quantum ones, and test to see whether these are realized in nature.

The situation is somewhat different in the Everett interpretation. What happens in an experiment is the splitting of the wavefunction into a number of relatively independent branches, and all outcomes afforded nonzero chance by the usual rules are realized on some branch. On the usual interpretations, the outcome of a spin-*z* experiment will be one of the set of alternatives, {Spin-*z* up is obtained, Spin-*z* down is obtained}. The assumption is that, at the end of the experiment, exactly one of these propositions will correctly describe the state of affairs that obtains after the experiment.[7] On the Everett interpretation, however, this is not the case. One is tempted to say either that all of the alternatives describe the post-experiment state of affairs, or that none of them do. Each result is obtained on some branch. This is what Wallace calls the *Incoherence Problem*. The problem is not one that is solvable by simply postulating that the weights yielded by quantum mechanics are probabilities. The

[7] One could also add to the list the possibility that the experiment fails, perhaps due to some fault in the apparatus, and that no result is obtained. Nothing of import changes if this alternative is added.

problem is not a lack of quantities that can serve as probabilities, but a lack of appropriate arguments for the probability function; there seems to be nothing for the probabilities to be probabilities *of*. If it is not the case, for any of what are usually conceived as outcomes of an experiment, that it will be *the* result that occurs, then it can make no sense to ask for the probability *that* it will be the result that occurs.

One option for the Everettian is to simply reject all probability talk. The theory is, after, a deterministic one, and one applies it in cases in which, for all intents and purposes, the quantum state can be regarded as completely known. The theory is therefore unlike any of the cases in which we traditionally speak of physical chances. The Everettian may simply assert that our usual talk about experiments, involving talk of probability, is guided by a mistaken view of what transpires in an experiment.

This would be well and good—after all, the Everett interpretation paints a picture of the world radically different from any usual one, and we should expect to make some radical changes to our beliefs upon accepting the theory—were it not for what we may call the *Evidential Problem*. Since most of what we take to be evidence for quantum mechanics is statistical in nature, an Everettian who made such a move would be depriving himself of most of what we take to be evidence for quantum mechanics, and, unless quantum mechanics could be afforded the status of a truth known *a priori*, in so doing he would be depriving himself of any reason we have to believe that quantum mechanics gets something right about the world, and hence of any reason we might have to be concerned about interpretations of quantum mechanics in the first place. Such a move would, in Barrett's phrase, be *empirically self-defeating* (Barrett 1999, 116).

Casting statistical inference in the quasi-deductive mode obscures this issue. On the chance interpretation, accepting, as if it were entailed by the evidence, the proposition that the proposition that the actual chance is within a certain specified interval, will, for a well-designed experiment, be a harmless substitute for conditionalizing on the evidence. This rationale for such a rule involved treating the experiment as a chance set-up, and hence is unavailable in the Everettian context. Papineau (2004, 156, fn 3) writes,

> Everettians can advise us to reason in just the standard way, modulo the substitution of intensities for chances: infer that the intensity (and hence quantum mechanical amplitude [*sic*; squared amplitude is meant] is close to the observed frequency, and hope that you are not the victim of an unlucky sample.

The standard justification for reasoning in the standard way does not survive the substitution of intensities for chances. The transition to the Everett interpretation at minimum requires one to revisit the grounds for inferring that the squared amplitude is close to the observed frequency.

The problem would be less sharp if what Wallace refers to as the "Subjective Uncertainty" (SU) viewpoint were a coherent option. Wallace characterizes this viewpoint thus:

> Given that what it is to have a future self is to be appropriately related to a certain future person, and that in normal circumstances I expect to become my future self, so also in Everettian splittings I should expect to become *one* of my future selves. (cite)

If we take seriously the idea that to have a future self is to be appropriately related to a certain future person, then, in an Everettian context, to expect to become *one* of my future selves is to expect that *one* of my successors on the various branches will bear the appropriate relation to my present self. To invoke immaterial minds would be to add extra structure and to deny the ontological claim of the Everett interpretation. Whatever this appropriate relation is, then, it is to be found in structural relations in the wave function. *Prima facie*, however, it seems implausible indeed that my present self will always bear this relation to my successor on exactly one of the branches; however we characterize this relation, it would seem that *all* of my successors will bear this relation to me, as all stand in pretty much the same causal relation to my present self. As Wallace (2003a, 416) puts it, "Each future observer is (initially) virtually a copy of the original observer, bearing just those causal and structural relations to the original that future selves bear to past selves in a non-branching theory." The differences among my successors (at least initially, when the result of the experiment is first revealed) lies in their relations, not to my present self, but to something external, namely, the experimental apparatus and its recorded results. Subjective uncertainty makes no sense in the absence of anything to be uncertain about.

A significant advance towards solving the incoherence problem is represented by the decision-theoretic approach initiated by Deutsch (1999) and further developed by Wallace (2002, 2003*a*, *b*) and Saunders (2004). The strategy can best be illustrated by analogy. If I believe that the consequence of an act A will be one of a set of consequences $\{C_i\}$, to which I attach values $\{V(C_i)\}$ and credences $\{p_i\}$, then the standard Bayesian decision rule tells me to value that action according the epistemically weighted expectation value

$$E^e = \sum_i p_i V(C_i),$$

and to choose, from the set of acts available to me, the one with the highest expectation value. Such an act will have the same value, to me, as an act that results in value E in any event. Thus, for any act whose outcome is uncertain, one can construct another with a certain outcome but equal value.

If one knows the initial state and the quantum dynamics, the outcome of any action will, on the Everett interpretation, be certain, and in many cases will involve a splitting of the wavefunction. The decision-theoretic take on the Everett interpretation recommends that the Everettian value this certain result exactly as much as a non-Everettian would value the uncertain result. The Everettian does not have available a set of credences $\{p_i\}$, which are degrees of belief that C_i will be the (unique) consequence that occurs, but he does have available the norm-square weights $\{w_i\}$ of the branches on which the consequences $\{C_i\}$ obtain. The decision-theoretic approach recommends that the Everettian agent take, as the value of the his action, the quantity

$$E^w = \sum_i w_i V(C_i),$$

which is the average of consequences on the various branched weighted by their Hilbert-space norm. This amounts to weighting the consequences on branches with large norm more heavily than the consequences on branches with small norm. It is for this reason that Greaves (2004) appropriately calls the set of weights a "caring measure."[8]

This may seem like an arbitrary supposition. However, as Saunders puts it,

> 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. (Saunders 2004, p?)

[8] Successor v states of the world.

Once one accepts the basic idea of assigning values to branching states of the world, the question arises as to what these weights should be, and a number of arguments have been made that, in the Everettian context, the only reasonable choice is the norm-square measure (Deutsch 1999; Wallace 2002, 2003*a*, *b*; Saunders 2004). The strategy of such arguments is to introduce a set of constraints, intended to be conditions of "pure rationality," analogous to those invoked in ordinary decision theory, that uniquely determine the choice of weights. As Wallace has stressed, none of this is plausible unless the Everett interpretation is assumed.

For the sake of what follows, I will presume that this programme can be successfully carried through, and grant that an agent who accepts the Everett interpretation should, on rationality grounds, assign values to his actions in the manner by the Everettian decision rule. The weights of branches will, therefore, play, for an Everettian agent who knows the quantum state with certainty, much the same role in making decisions that credences do for the non-Everettian. One might be tempted to say that this means that, for the Everettian, the weights *are* credences, or at least that a distinction between the two makes no difference for the Everettian. This would be a mistake. On the Everett interpretation, the wavefunction is taken to have physical reality, and hence the weights are features about the world that the agent can have belief (hence degrees of belief) about, beliefs that might be wildly mistaken. This is not brought out in the discussions by Deutsch or Wallace, because they restrict themselves, for simplicity, to the special case in which the quantum state is known with effective certainty. It was remarked earlier that it was misleading to gloss the Principal Principle as saying that our credences should track chances; similarly, on the decision-theoretic approach to the Everett interpretation, our actions will be guided, not by the norm-square weights, but by our *beliefs* about those weights.

The approach must, therefore, be able to accommodate states of belief about the wave other than those involving effective certainty. The question arises, then, as to how an

Everettian agent should assign credences about the quantum state, and how they should be updated in light of the evidence. Consider, for example, the situation (not at all unusual) in which an experiment has been performed but the agent does not yet know which of the outcome branches she is on, a situation that at least temporarily arises in every experiment, because the branches will have decohered some time before the experimenter observes the results (and of course, the rest of us have to wait until the results are made public). There is, for each agent on each branch, a determinate matter of fact about which branch she is on, and hence there is, unlike the pre-branching case, something to be uncertain about. How should she distribute her degrees of belief among the alternative possibilities?

It might seem that the answer to this question is completely unconstrained, and that any degrees of belief are as good as any. This is, perhaps surprisingly, not the case; one can argue, on rationality grounds, that an Everettian agent who accepts the decision-theoretical framework should assign her credences, post-branching, in exactly the same way as a non-Everettian who takes what the Everettian regards a branching event to be a chance set-up yielding a unique outcome. If it is known that the initial state branches into alternatives $\{C_i\}$ with weights $\{w_i\}$, then the Everettian agent should set her credence $Cr(C_i)$ that she is on a branch in which C_i obtains equal to w_i—exactly as if she were a non-Everettian who obtained these credences by an application of the Principal Principle. The details of the argument are given in Appendix 2, but, qualitatively, the idea is that the Everettian agent unsure of which branch she is on regards the consequences of her actions as mattering more if she is on a high-weight branch than if she is on a low-weight branch; hence, she should guide her actions as if she believes more strongly that she is on a high-weight branch.

Similarly, one can give an argument on rationality grounds that, in cases in which the Everettian is uncertain about what the quantum state is (and hence uncertain about what the weights are), she should update her credences about these weights in the same way as the non-Everettian applying the Principal Principle to turn observations into evidence about chances. That is, upon observing some experimental results, she should boost her credences about initial states that attach high weights to the observed results and lower her credences about initial states that attach low weights to the observed results, in the same way that a non-Everettian boosts her credences about initial states that confer a high chance on the observed results.

Here again we have an illustration of the insufficiency of a single notion of probability. The Everett theory does away with physical chances, but the caring measures play a similar role, and, like physical chances, must be distinguished from credences.

6. The sirens' song. It might seem that the results mentioned in the last section provide the advocate of the Everett interpretation with all that is needed to solve the evidential problem. Such a solution might run as follows.

Let E be the recorded results of all experiments so far that are relevant to the correctness of quantum mechanics. If we want to know whether or not E supports some hypothesis H, we should ask ourselves what credences we would regard as reasonable for an agent who does not yet know E (a set of credences which, of course, might not consist of a unique credence function, but may include a substantial range), and ask about the extent to which the credibility of the hypothesis is boosted by conditionalization on E. We can take, as a measure of the degree of evidential support lent to H by E, the ratio of posterior to prior credences for H:

$$S(H,E) = \frac{Cr(H \mid E)}{Cr(H)}.$$

Bayes' theorem yields an equivalent form of this which is more useful for our purposes:

$$S(H,E) = \frac{Cr(E \mid H)}{Cr(E)},$$

which indicates that a hypothesis is more strongly supported by the evidence the higher its likelihood $Cr(E \mid H)$. If we are comparing the degrees of support lent by the evidence to two hypothesis, H_1 and H_2, we will have,

$$\frac{S(H_1,E)}{S(H_2,E)} = \frac{Cr(E \mid H_1)}{Cr(E \mid H_2)}.$$

Now, let us compare *QMEverett*, which is the Everett interpretation, with *QMChance*, which can be some interpretation that takes quantum experiments as chance setups and the results of those experiments as providing information about the chances, results that are in excellent agreement with the chances calculated from quantum mechanics in the usual way. The relative degree of support of the two hypotheses is

$$\frac{S(QMEverett,E)}{S(QMChance,E)} = \frac{Cr(E \mid QMEverett)}{Cr(E \mid QMChance)}.$$

A calculation of $Cr(E \mid QMChance)$, in accordance with the Principal Principle, yields a high likelihood and hence a high degree of support for *QMChance*. As for *QMEverett*: for an Everettian agent, the question of whether E is true is a question of which branch he is on, since, for any possible set of outcomes of these experiments, there will be branches on which that set of outcomes is recorded. Branches containing results that, on the usual interpretation, are high-chance results will have high weight; branches with recorded statistics that disagree wildly with quantum expectations will have low weight. We have said that, faced with such a question, the Everettian agent should assign his credences about which branch he is on according to the weights. This yields the same credence about the evidence as that obtained by the non-Everettian. Thus, we should take

$$Cr(E \mid QMEverett) = Cr(E \mid QMChance),$$

which entails that

$$S(QMEverett, E) = S(QMChance, E).$$

Therefore, Everettian quantum mechanics is as well-supported empirically as quantum mechanics on any other interpretation.

7. Lashed to the mast. I hope that the argument in the previous section sounds plausible; nevertheless, it is fallacious. Its error lies in taking $Cr(E \mid QMEverett)$ to mean something like, "the credence I would assign to *E* were I to accept the Everett interpretation." This is suggested by the common way of reading $Cr(P \mid Q)$, as "the credence of *P* given *Q*." Nevertheless, the conditional credence $Cr(P \mid Q)$ does *not* mean "the credence I would assign to *P* were I to accept *Q*"; it is defined simply as the ratio $Cr(P \mathbin{\&} Q)/Cr(Q)$, with both numerator and denominator evaluated according to my *present* credences. Arguments can be given (diachronic dutch book arguments and their cousins, see Appendix 1 for one such) to the effect that, if my current credences are $Cr(\cdot)$, then a change of belief that represents learning *Q* and nothing else should go by conditionalization on *Q*:

$$Cr(\,\cdot\,) \rightarrow Cr(\,\cdot\,|\,Q)\,.$$

It is crucial to the argument that the change of belief not involve a reassessment of my former beliefs, as it is assumed that after the change I continue to endorse my former judgments as reasonable, given my former body of evidence.

The arguments given by Deutsch, Wallace, and Saunders, that an Everettian agent should value the outcomes on the various branches by an amount proportional to the weight of those branches presumes the correctness of the Everettian view, and concern what an agent convinced of the Everett interpretation should do. The further argument, given in Appendix 2 of this paper, concerning how an Everettian agent should assign credences, presumes the correctness of the decision-theoretical analysis, and hence also concerns what a convinced Everettian should do. As such, it says nothing about how an agent who is not yet convinced should assign credences. Accepting the Everett interpretation involves a reconceptualization of the outcome of an experiment, from thinking of it as an event with a unique outcome to thinking of it as a branching event. Thus, even if we know what credences $Cr^{Ev}(\,\cdot\,)$ an Everettian agent would assign, these do not constrain the credences of a non-Everettian agent, and, in particular, we have no reason to require that the non-Everettian assign her credences so as to satisfy

$$Cr(E\ \&\ QMEverett) = Cr^{Ev}(E)\ Cr(QMEverett),$$

which, of course, is equivalent to the assumption made in the argument of the previous section, that $Cr(E\,|\,QMEverett) = Cr^{Ev}(E)$.

So, we are no closer to solving the Evidential Problem. The problem is a deep-seated one. As usually construed, the evidence taken to be evidence for quantum mechanics is evidence about chances; these chances, which can be measured without assuming the correctness of quantum mechanics, are in good agreement with those calculated from quantum mechanics. The observed relative frequencies are regarded as information about the physical chances, and the good agreement between these relative frequencies and the chances calculated from quantum mechanics thus count as evidence that quantum mechanics is getting the chances, construed as physical facts about the world, at least approximately right. On the Everettian view, since the wave function is taken to be physically real, so are the weights of branches. But these weights are not relevant to what the outcome of an experiment will be, as experiments do not have unique outcomes but rather all possible outcomes, on different branches. One argues on normative grounds that the weights should guide our actions, and hence are relevant to *our attitudes and behaviour* towards the divided outcomes of experiments, provided that we first accept the Everett interpretation. That the weights should play such a role is not discovered empirically but is a normative matter, argued for on grounds of pure rationality. On the Everett interpretation, there seems to be no room for the notion that there is something about the world, evidenced by the statistical results of our experiments, that quantum mechanics is getting right. And thus is lost the evidential relevance of these results to the correctness of quantum mechanics.

One way of seeing the point is this. It is conceivable that further experiments will reveal that it is possible to find or construct situations in which the actual chances of some set of outcomes are not equal to chances that can be generated from any quantum state. In such an event we would conclude that quantum mechanics is not right and would search for a better theory. The link between physical chances and quantum amplitudes, to the extent that it is established, has been established empirically, and we could learn on the basis of further empirical evidence that it is not quite right. On the other hand, it makes no sense to imagine an Everettian discovering, on empirical grounds, that valuing branches according to their squared amplitudes is incorrect, and that the actual degrees to which she should value them cannot be generated from any quantum state; on the Everettian view, the link between 'caring measures' and quantum amplitudes is established on normative, not empirical grounds. But, if it makes no sense to talk of evidence that would cast a hypothesis into doubt, then it makes no sense to talk of evidence supporting the hypothesis.[9]

The complaint is not that Everettian quantum theory could not possibility be true; it is that, due to the empirically self-undermining nature of the theory, we would not have evidence that it is true, even if it were. This may seem paradoxical, or perhaps even contradictory, and someone might be suspicious of the very claim that there could be a theory that, if true, could not be known on the basis of the evidence to be true. But it is easy to construct simple examples of propositions that, though logically possible, could not be known to be true. Consider the proposition that there is a bag of gold buried beneath All Souls College. We have to admit that this is a possibility, and, for all we know, it might be true. If true, however, it is likely that nobody knows it; it would be surprising if such a thing were kept secret for so long. Consider, then, the proposition:

There is a bag of gold buried beneath All Souls College, but nobody knows it's there.

This is logically possible, and could be true for all we know, but it could not possibly be known by anyone to be true.

8. Conclusion. The decision-theoretic approach to the Everett theory goes a long way towards establishing the internal coherence of the Everettian view. In particular, it can be argued that an Everettian should behave, in making decisions, the same way that a non-

[9] This is a trivial theorem of the probability calculus: conditionalizing on E boosts the probability of H if and only if conditionalizing on $\sim E$ diminishes it.

Everettian would. Previous discussions have assumed the correctness of the Everett interpretation and that the quantum state is known with effective uncertainty. As has been shown in this paper, the latter assumption can be dropped, and, under conditions of uncertainty about the quantum state, the Everettian will update her credences about the state in the same way as the Everettian conditionalizing on the evidence. A convinced Everettian will not be dissuaded from her view on grounds of internal incoherence.

Greaves (2004, 430) has conjectured,

> If the rational Everettian cares about her future successors in proportion to their relative amplitude-squared measures, then the correct account of confirmation for a branching theory like Everett will determine that (given our actual experimental data) she should regard (Everettian) quantum mechanics as empirically confirmed.

Even if this conjecture is correct, and it can be argued that it is rational for an Everettian agent to believe that Everettian quantum mechanics is empirically well-confirmed, it would get us no closer to solving the Evidential Problem. If it is true that a convinced Everettian should regard the theory as empirically well-confirmed it, it does not follow from this that those of us who do not now believe in the rather fantastic picture of the world painted by Everett should regard ourselves as having good evidence that something like this picture is the truth, or that we are being remiss in our epistemic responsibility to duly consider the bearing of all available evidence on our beliefs if we do not attach a high degree of credence to Everettian quantum mechanics. The issue is not what someone who accepted the theory on a blind leap of faith would subsequently believe, but whether anyone has good reason to believe the theory in the first place. One typically comes to quantum mechanics in something like the usual textbook formulation, which assumes definite outcomes of experiments to which quantum mechanics assigns chances, an assumption that is usually accompanied by vague talk about collapse. Such a formulation is of questionable coherence, but nevertheless, the evidence we have gives us good reason to believe that something is right about the theory, that the theory is worth taking seriously, and that it is worth asking whether one can replace the formulation with a coherent one. Unfortunately, even if the coherence of the Everett interpretation can be defended, the move to the Everett interpretation undermines the evidence one has for believing in quantum mechanics in the first place, and hence any reason one has for being concerned about interpretations of quantum mechanics at all. An advocate of the Everett interpretation ought to be able to regard the usual talk of outcomes and chances as a ladder that is used to reach the Everettian viewpoint, which can be discarded once one had gotten there. The problem is that there is nothing else to stand on.

Appendix 1. Why conditionalize? Suppose that, at some time, your credences are given by $Cr(\cdot)$. You learn a new piece of evidence E. How should you update your credences? The standard answer is that the updating should go by conditionalizing on E. That is,

$$Cr(\cdot) \quad \Rightarrow \quad Cr(\cdot \mid E) =_{df} \frac{Cr(E \,\&\, \cdot)}{Cr(E)}.$$

But why should this be the case, and what would go wrong if one were to do otherwise?

The following argument is closely related to the so-called *diachronic dutch book argument*, which originates with Lewis and was first reported by Teller (1973). This argument has generated a great deal of controversy, because the original presentation did not make clear the conditions under which the argument was applicable and appeared to prove too much——that one should always conditionalize. Subsequent discussion, however (see in particular Skyrms 1990 and Maher 1993, 114–120) has made it clear that the argument is sound provided its scope is restricted to situations in which the change of belief can be treated as a pure learning experience, consisting in adding E to one's store of knowledge and in nothing else. In particular, it is essential that the belief-change not involve a reassessment of the reasonableness of one's credences prior to learning E.

Consider two bets:

I. Prior to learning E, the agent pays pS for a bet on a proposition F with payoff S, conditional on E. That is, the bet goes through only if E; in the event that E does not occur, the bet is called off and the ante returned.

II. The agent makes a bet on F with the same stakes, but only if E. That is, the bet is made after learning E, and only if E occurs; in the event that E does not occur, no bet is made.

Let $Cr(\cdot)$ be the agent's prior credence function, and let $Cr^E(\cdot)$ be the credence function the agent adopts upon learning *E*. Since the two bets result in the same net payoff to the agent, in any event, then it is reasonable to assert that, for any value of *p*, the one bet should be judged favourable if and only if the other is. That is, bet II, judged by the lights of the credence function $Cr^E(\cdot)$, should be judged favourable if and only if bet I is judged favourable by the lights of the credence function $Cr(\cdot)$. That judgments according to the two credence functions mesh in this way means that the agent, prior to learning *E*, regards the shift to Cr^E as a reasonable response to learning *E*, and the agent, after making the shift to Cr^E, continues to endorse his prior judgments as reasonable ones for someone who does not yet know *E*.

The expectation value of bet I is,

$$\begin{aligned} E^I &= Cr(E \,\&\, F)(1-p)S - Cr(E \,\&\sim F)pS \\ &= \big(Cr(E \,\&\, F) - Cr(E)p\big)S. \end{aligned}$$

It is therefore judged favourable, by the lights of the prior credence function $Cr(\cdot)$, if and only if

$$Cr(E \,\&\, F) - Cr(E)p > 0,$$

or,

$$p < Cr(E \,\&\, F)/Cr(E).$$

The expectation value of bet II, as judged by the updated credence function $Cr^E(\cdot)$, is

$$\begin{aligned} E^{II} &= Cr^{E}(F)(1-p)S - Cr(\sim F)pS \\ &= \left(Cr^{E}(F) - p\right)S. \end{aligned}$$

The condition that, for any value of p, Bet II be judged favourable if and only Bet I is, yields

$$Cr^{E}(F) = Cr(E \,\&\, F)/Cr(E).$$

Appendix 2. Meshing credences and weights in the Everett interpretation.

A2.1. Credences about branches. Consider an Everettian agent who knows that a branching event is about to occur, and the post-branching state will consist of a number of branches that can be labeled by consequences $\{C_i\}$, with weights $w(C_i)$. After the event, but prior to learning the outcome, she will be uncertain as to which branch she is on. The question is, to what degree should she believe that she is on branch C_i?

Consider the following two betting situations:

I. The agent places a bet on of the outcomes, C_i, before the branching event. She pays an amount $p\,S$ for the bet, which has a payoff of S.

II. On each branch the agent places a bet on the proposition that she is on the C_i branch, with the same stakes and payoff as in I. We assume that the agent on each branch makes the same bet.

Since the agent has learned nothing relevant to her decisions prior to making the bet in II, it seems reasonable to assert that the bets in (II) should be deemed rational if and only if the bet in (I) is. Let $w(\sim C_i)$ be the combined weight of all branches other than C_i. By the Everettian decision rule, the bet in (I) should be valued at

$$
\begin{aligned}
E^{I} &= [w(C_i)(1-p) - p\,w(\sim C_i)]S \\
&= (w(C_i) - p)\,S,
\end{aligned}
$$

and hence should be judged favourable if and only if $p < w(C_i)$. The bet in (II) will be valued at the epistemically weighted expectation value,

$$
\begin{aligned}
E^{II} &= [Cr(C_i)(1-p) - p\,Cr(\sim C_i)]S \\
&= (Cr(C_i) - p)\,S,
\end{aligned}
$$

and hence should be judged favourable if and only if $p < Cr(C_i)$. The condition that, for any value of p, bet (II) should be judged favourable if and only bet (I) is, yields the constraint on the agent's credences that

$$
Cr(C_i) = w(C_i).
$$

It is easy to see that violation of this condition would render the agent liable to a dutch book; that is, a series of bets each individually deemed favourable whose net result is a certain loss (that is, a net loss on all branches).

A2.2. Updating credences about the quantum state. With the exception of a brief discussion in §8.2 of Wallace 2002, the literature on the decision-theoretic approach to the Everett interpretation takes as its starting point the assumption that the agent has effectively certain knowledge about the quantum state. It should also be possible to accommodate conditions of uncertainty about the quantum state. In order to do this, we need an account of how an Everettian agent should update her credences about what the quantum state is in light of observation.

Consider, then, a situation in which the initial quantum state is believed to be contained in a set $\{s_i\}$ of states, and the agent has initial credences $Cr(s_i)$ concerning what the actual state is. Two experiments will be performed: an *A*-experiment that splits the wavefunction into *A* and ~*A* branches, followed by a *B*-experiment that further splits these branches into *B* and ~*B* branches. We assume that the agent knows the dynamics underlying these splitting events sufficiently well to calculate weights $w_i(\cdot)$ for these branches, on the assumption that the initial state is s_i. After the *A*-branching event, the agents on the *A*-branch changes her credences to $Cr^A(s_i)$, and it is our task to determine what these should be.

Consider the following two bets:

I. Prior to the *A*-split, the agent pays *pS* for a bet on *B* with payoff *S*, conditional on *A*. That is, the bet goes through only on the *A* branch; on the ~*A* branch, the bet is called off and the ante returned.

II. A bet on *B*, with the same stakes, is placed after the *A*-split, and only on the *A* branch. On the ~*A* branch no bet is made.

Each of these betting situations yields the same net return on all branches. Therefore, it is reasonable to assert that, of any value of *p*, the one bet should be judged favourable if and only if the other is, subject to the same caveats as in Appendix 1.

In state s_i, the value of bet I is

$$
\begin{aligned}
E_i^I &= w_i(A \,\&\, B)(1-p)S + w_i(A \,\&\! \sim B)\,p\,S \\
&= (w_i(A \,\&\, B) - w_i(A)p)S.
\end{aligned}
$$

Therefore, the agent values this bet at the epistemically weighted expectation value,

$$
\begin{aligned}
\overline{E}^I &= \sum_i Cr(s_i)\big(w_i(A \,\&\, B) - w_i(A)p\big)S \\
&= \big(\overline{w}(A \,\&\, B) - \overline{w}(A)p\big)S,
\end{aligned}
$$

where $\overline{w}(\,\cdot\,) = \sum Cr(s_i)\,w_i(\,\cdot\,)$. The bet is therefore deemed favourable if and only if

$$
p < \overline{w}(A \,\&\, B) \big/ \overline{w}(A)\,.
$$

For bet II, which takes place after the *A*-event, the agent uses the relative weights $w_i^A(\,\cdot\,) = w_i(A \,\&\, \cdot\,) / w_i(A)$ and the updated credences $Cr^A(\,\cdot\,)$. Bet II is therefore valued at

$$
\begin{aligned}
\overline{E}^{II} &= \sum_i Cr^A(s_i)\big(w_i^A(B)(1-p) - w_i^A(\sim B)p\big)S \\
&= (\overline{w}^A(B) - p)\;S.
\end{aligned}
$$

The two bets will therefore share the same betting threshold separating favourable from unfavourable bets if and only if

$$
\overline{w}^A(B) = \overline{w}(A \,\&\, B) \big/ \overline{w}(A)\,,
$$

or,

$$\sum_i Cr^A(s_i)w_i^A(B) = \sum_i Cr^A(s_i)\frac{w_i(A \& B)}{w_i(A)} = \sum_i Cr(s_i)\frac{w_i(A \& B)}{\overline{w}(A)}.$$

Since this must obtain no matter what *B* is, we must have,

$$\frac{Cr^A(s_i)}{w_i(A)} = \frac{Cr(s_i)}{\overline{w}(A)},$$

or,

$$Cr^A(s_i) = \frac{w_i(A)}{\overline{w}(A)}Cr(s_i).$$

References

Barrett, Jeffrey Alan (1999), *The Quantum Mechanics of Minds and Worlds*. Oxford University Press.

Bell, John S. (1987), "Are there quantum jumps?", in *Speakable and unspeakable in quantum mechanics*. Cambridge: Cambridge University Press, 201–212.

Brown, Harvey R., and David Wallace (2005), "Solving the measurement problem: de Broglie-Bohm loses out to Everett." Forthcoming in *Foundations of Physics* **35**, 517–540.

Butterfield, Jeremy (1996), "Whither the Minds?" *The British Journal for the Philosophy of Science* **47**, 200–221.

Jeffrey, Richard (2004). *Subjective Probability: The Real Thing.* Cambridge: Cambridge University Press.

Lewis, David (1980). "A Subjectivist's Guide to Objective Chance," in Richard C. Jeffrey, ed., *Studies in Inductive Logic and Probability*, Vol. II (Los Angeles: University of California Press), 263–293. Reprinted in Lewis 1986, 83–113.

——— (1986). *Philosophical Papers*, Vol. II. Oxford: Oxford University Press.

——— (1994). "Humean Supervenience Debugged." *Mind* **103**, 473–490.

Quine, W. V. (1990), *Pursuit of Truth*. Cambridge: Harvard University Press.

Saunders, Simon (2004). "What is Probability?" forthcoming in A. Elitzur, S. Dolev, and N. Kolenda, eds, *Quo Vadis Quantum Mechanics* (Springer). (get page #s)

Shimony, Abner (1970), "Scientific Inference", in R. G. Colodny (ed.), *Mind and Cosmos: Essays in Contemporary Science and Philosophy*, Pittsburgh: Pittsburgh University Press, 79–172. Reprinted in *Search for a Naturalistic World View, Vol. I: Scientific Method and Epistemology*, Cambridge: Cambridge University Press, 1993, 183–273.

Skyrms, Brian (1990). "The Value of Knowledge," in C. Wade Savage, ed., *Scientific Theories. Minnesota Studies in the Philosophy of Science XIV* (Minneapolis: University of Minneapolis Press), 245–66.

Teller, Paul (1973). "Conditionalization and Observation." *Synthese* **26**, 218–58.

Wallace, David (2002). "Quantum Probability and Decision Theory, revisited." Available at PhilSci archive (http://philsci-archive.pitt.edu/).

——— (2003a). "Everettian rationality: defending Deutsch's approach to rationality in the Everett interpretation." *Studies in History and Philosophy of Modern Physics* **34**, 415–39.

——— (2003b). “Quantum Probability from Subjective Likelihood: improving on Deutsch’s proof of the probability rule.” Available at PhilSci archive (http://philsci-archive.pitt.edu/).