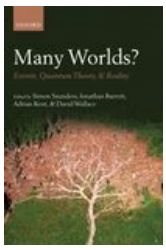

Many Worlds? Everett, Quantum Theory, and Reality

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CHAPTER

7 A Fair Deal for Everettians

David Papineau

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Abstract

Those who criticize the Everett interpretation on the grounds that it makes no sense of probability apply a double-standard, for no other physical theory of probability does any better in explaining probability or in deriving its link with decision theory. In fact, others do worse, for in any one world theory it is a mystery as to why, given that only a single outcome of a chance process occurs, we should nevertheless act so as to maximize expected utilities, which involves all possible outcomes of a chance process. This difficulty does not apply to the Everett interpretation, in which all outcomes happen.

Keywords: probability, frequencies, subjective, objective, propensities, expected utility, principal principle, decision-theory link, statistical inference, rationality

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1 INTRODUCTION

It is widely supposed that the Everettian account of quantum mechanics has difficulties with probability. In this paper I shall argue that those who argue against the Everettian interpretation on this basis are employing a double standard. It is certainly true that there are philosophical puzzles about probability within the Everettian theory. But I shall show that orthodox metaphysics has even worse problems with probability than Everettianism. From this perspective, orthodox metaphysicians who criticize Everettians about probability are a classic case of a pot calling a kettle black.

Why might anybody think that Everettians have trouble with probability? The basic reason is that Everettian quantum mechanics is a deterministic theory. It implies that the conditions obtaining at a given time together with the relevant laws completely fix the future course of nature. True, this future will normally involve a proliferation of branching alternatives, and the Everettian theory will attach numbers to these branches—the squared moduli of their wavefunction amplitudes—that behave numerically like

probabilities. But critics argue that these numbers cannot be understood as probabilities, since they lack any essential connection with ignorance about the future. On the Everettian theory, a knowledgeable subject faced with a chancy situation, like an x-spin measurement on an electron in an eigenstate of z-spin, will know exactly what is going to happen—reality will split into as many branches as there are possible macroscopic outcomes. Given this, the critics argue that the squared moduli of the amplitudes of the branches cannot possibly be probabilities. Probabilities are measures of ignorance about what is going to happen. If the Everettian interpretation holds that there is nothing to be ignorant about, then surely this leaves no place for probability.

I think that Everettians should be unmoved by this line of thought. They should simply insist that the relevant squared moduli are probabilities, despite the lack of any connection with ignorance. In support of
p. 207 this, they can point ↳ out that the squared moduli behave remarkably like orthodox probabilities even for an Everettian with complete knowledge of the future. These numbers will respond to evidence and guide decisions in just the same way that probabilities are supposed to do within orthodoxy. Moreover, if we want to know *why* these numbers should so respond to evidence and guide decisions, it turns out that Everettians are at least as well placed to answer these questions as orthodoxy.

So I think Everettians should simply reject the idea that probabilities require ignorance. They should say that this is simply another of the mistakes imposed on us by orthodox metaphysics. Just as orthodoxy is wrong to suppose that only one of the possible outcomes will occur in a chancy situation, so it is wrong to suppose that the ascription of a non-unitary probability to some outcome is incompatible with knowing for sure that it will occur.

A number of writers sympathetic to Everettianism have recently sought to show that, despite first appearances, some element of ignorance about the future can still be found within the Everettian scheme of things (Albert and Loewer [1988], Vaidman [1998], Saunders and Wallace [2008]). One motivation for this move is to show that Everettians can still avail themselves of ordinary ignorance-based thinking about probability. From my point of view, this motivation is quite misplaced. It presupposes that orthodoxy has a coherent story to tell about probability. But in truth there is no such story. Orthodox ignorance-based thinking about probability is pretty close to incoherent. Everettians are thus doing themselves a disservice when they seek to ally themselves with such orthodox thinking. They will do much better to cut the tie between probability and ignorance and forget about trying to mimic orthodoxy.

Some will feel that it is simply a contradiction in terms to talk about non-unitary probabilities in the absence of ignorance. (What can it *mean* to say that a measurement of x-spin 'up' is 50% probable, if we know for sure that this result is determined to occur?) Indeed some writers who agree with me that Everettians can combine lack of ignorance with quantitative future expectations maintain that even so it is better to drop the term 'probability' for squared wavefunction amplitudes. Thus Hilary Greaves [2004] talks about a 'caring measure' and David Lewis [2004] about 'intensity'. However, I myself see no reason for this squeamishness. I think it is perfectly appropriate to talk about 'probability' even within the context of no-ignorance Everettianism.

Still, if somebody wants to insist that it is a matter of definition that non-unitary 'probabilities' imply ignorance, I am happy to let this pass. There is no substantial issue here, just a matter of terminology (cf. Greaves [2004]). To see this, suppose that we can show that a no-ignorance Everettianism has room for quantities that play just the same role within the Everettian scheme of things as probabilities play within orthodoxy, save that Everettians sometimes ascribe non-unitary values to circumstances that they know are certain to occur. And suppose that the resulting metaphysical theory gives a better overall account of the
p. 208 working of the world and our place in it than ↳ the orthodox picture that makes play with 'probability'. Then what does it matter if definitional niceties require us to withhold the term 'probability' from these quantities, and call them 'schprobabilties' (or 'caring measures' or 'intensities') instead? We've got just as a

good theory, with just the same virtues, even so. The quantities involved might be less familiar than orthodox probabilities, in that futures that are sure to occur can still have non-unitary values. But I trust that nobody who is serious about the interpretation of quantum mechanics will take unfamiliarity per se as a reason for rejecting an otherwise cogent view.

As I said, even on the definitional level I see no reason why no-ignorance Everettians should not hang on to the term ‘probability’ if they like. I think that there is quite enough overlap between the role that squared amplitudes play in Everettianism and orthodoxy respectively to justify Everettians retaining this term even after the tie with ignorance is cut. But, as I said, there is no substantial issue here. If you don't like this usage, feel free to substitute ‘schprobability’ whenever I talk about Everettian probabilities.

In this paper I shall proceed as follows. The next three sections will rehearse a number of background claims about probability that I hope will be uncontroversial. The following four sections will then argue that Everettianism is no worse off than orthodoxy in accounting for the central properties of probability. The final section will then argue that Everettianism is actually better off than orthodoxy when it comes to explaining how probability guides decisions.

2 SUBJECTIVE AND OBJECTIVE PROBABILITIES

Any satisfactory account of probability needs to distinguish subjective and objective probabilities and explain the relation between them.

Subjective probabilities, or ‘credences’, are measures of psychological degrees of belief. You may not be fully convinced that it is going to rain, but you leave home with an umbrella anyway. In such a case you have a partial belief that it will rain—some degree of belief lower than one but higher than zero. In general, degrees of belief manifest themselves in behaviour, as here your degree of belief in rain is manifested by your carrying the umbrella.

A tradition of analysis going back to Ramsey [1926] and continued by Savage [1954] and Jeffrey [1965] equates degrees of belief with dispositions to behaviour, including betting behaviour, and thereby shows that we can ideally quantify agents' degrees of belief by numbers in the range zero to one. There is plenty of controversy about what exactly these analyses establish, but we can bypass this here. For present purposes I shall simply assume that subjective probabilities correspond to agents' subjective expectations of future outcomes and that these expectations guide agents when they assess the relative worth of alternative
p. 209 actions, via familiar calculations of expected utility. ↳

In addition to subjective probabilities, we need also to recognize objective probabilities, or ‘chances’. These are quantities that represent the objective tendency for chancy situations to issue in different future circumstances. There is such an objective tendency for an electron in an eigenstate of z-spin to display ‘up’ on an x-spin measurement. Objective probabilities are prior to any subjective matters. There would still have been objective probabilities in the world even if no decision-making agents had ever evolved.

In a moment, I shall say a bit more about objective probabilities, and in particular about their availability within Everettianism. But first it will be helpful to observe that there is a basic connection between objective and subjective probabilities. You will do well to match your subjective probabilities to the objective probabilities. When making choices, you want your subjective assessment of the probabilities to match the objective probabilities.

This is (roughly) the principle that David Lewis [1980] dubbed the “Principal Principle”. I shall formulate it as follows:

The Principal Principle It is rational to set your subjective probabilities equal to the objective probabilities.[1]

In effect, this principle says that you ought to allow your actions to be guided by the objective probabilities—you ought to bet with the objective odds, not against them. (This follows because subjective degrees of belief are constitutively tied to behavioural choices—having such-and-such subjective degrees of belief simply consists in choosing those actions that maximize expectations of utility weighted by those degrees of belief. So someone who sets their *subjective* degrees of belief equal to the objective probabilities will per se maximize *objective* expected utility.)

3 FREQUENCIES AND PROPENSITIES

Now, some will feel that these brief remarks about objective and subjective probabilities are already enough to show why Everettianism is in trouble with probability. I introduced objective probabilities by talking about the 'tendency for chancy situations to issue in different future circumstances'. But aren't such 'objective tendencies' essentially a matter of long-run *frequencies*? And isn't the problem that Everettianism has no room for long-term frequencies—or rather that it has room for too many of them?

Take some case where we are interested in the frequency of result R on repeated trials of kind T (for
p. 210 example, the frequency of 'up' when we measure ↳ x-spin on electrons in an eigenstate of z-spin). Where orthodoxy sees one actual future, in which there is a definite sequence of results if this trial is repeated, Everettianism must see a multiplicity of futures, given that a plurality of branches will be generated every time the trial is repeated. Orthodoxy can thus happily speak of *the* relative frequency of Rs displayed in *the* sequence of all trials T. But Everettianism cannot do this, for its reality will contain many different actual sequences with different relative frequencies. (Thus, where orthodoxy has, say, the actual sequence of results 'U', 'D', 'D', 'U', 'D', . . . , Everettianism will have this sequence *plus* 'D', 'D', 'D', 'U', 'D', . . . , *plus* 'U', 'U', 'D', 'U', 'D', . . . and so on, for all the different possible strings of outcomes, including ones on which the relative frequency of 'up' is quite different from the objective probability of 'up'.)

This may look like an immediate knock-down reason why Everettians can't have probabilities. If objective probabilities are *the* relative frequencies of Rs in Ts, then Everettians won't have any objective probabilities, for they won't have any unique such relative frequencies.[2] Moreover, if we are interested in the subjective probabilities imposed on agents by the principal principle, then Everettians won't have any such subjective probabilities either, for lack of any objective probabilities to set them equal to.

However, it is important to realize that this whole line of objection hinges on equating objective probabilities with long-run frequencies, and that this equation is by no means uncontentious. Over the past 50 years, many theorists of probability have come to favour *propensity* theories of probability over frequency theories. Where frequency theories aim to *reduce* objective probabilities to long-term frequencies, the propensity theory takes objective probabilities to be primitive quantities that are fully present in each chancy single case. On this view, a particular electron in a z-spin eigenstate will have a 0.5 propensity to display x-spin 'up', quite independently of what any other similar electrons may do. No doubt the frequency of spin 'up' in repeated measurement of similar electrons will tend to be close to 0.5. But this is a consequence of the particular electrons having the relevant propensities, and not the essence of objective
p. 211 probability itself.[3] ↳

Everettians will do well to adopt the propensity approach to objective probability rather than the frequency theory. While the frequency theory is clearly inconsistent with Everettian metaphysics, this is not true of the propensity theory. According to the propensity theory, objective probabilities measure the brute tendencies of particular chancy situations to produce various results. There is no obvious conflict between this and the

idea that all those results actually occur, each weighted by the relevant probability. Indeed the two ideas seem to fit very well together.

There is nothing ad hoc about this appeal to the propensity theory on the part of Everettians. As I said, many probability theorists have come to favour the propensity interpretation over the frequency theory for reasons quite independent of Everettian metaphysics. It will be useful at this point briefly to rehearse some of the reasons that have led them to turn against the frequency theory.

The most obvious difficulty facing the frequency theory is to ensure that there is always a suitable sequence of trials for any event to which we want to ascribe an objective probability.[4] Aren't there some genuinely chancy events that only occur once in the history of the universe? But we don't want to ascribe them an objective probability of one just on that account. This worry led classical frequency theorists to equate objective probabilities not with the frequency of Rs among the *actual* Ts, but rather with the hypothetical limiting frequency that *would* be displayed if Ts were repeated indefinitely (von Mises [1957]).

This suggestion, however, is open to its own objections. Why suppose that there is a definite fact of the matter about what *would* be observed if electron spins *were* measured indefinitely? From a metaphysical point of view this looks highly dubious. Note that it isn't enough to suppose there will be a certain 'number' of x-spin 'up's and 'down's in all the hypothetical future tosses. If there is a denumerable infinity of tosses,
p. 212 then presumably there will be ↳ a denumerable infinity of both 'up's and 'down's. And this fact by itself is compatible with any limiting relative frequency whatsoever, given that different arrangements of the order in which these 'up's and 'down's occur will yield different limiting frequencies. So, if we want the hypothetical future tosses to determine a definite limiting relative frequency, the hypothetical results must come in a definite order. It is very hard indeed to believe that there is a fact of the matter about exactly which ordered sequence of 'up's and 'down's we would observe, if we counterfactually carried on measuring electron spins indefinitely.

These points are not necessarily fatal to any frequency theory. Recent theorists have tended to revert to the equation of objective probabilities with *actual* frequencies, and have suggested that we can avoid the difficulty of single or rarely repeated trials by lumping together superficially distinguishable types of trials into sufficiently large similarity classes.[5] Still, I trust that I have done enough to indicate why the propensity theory strikes many philosophers of probability as an attractive alternative to the frequency account.

4 STATISTICAL INFERENCE

Some readers might be wondering whether the metaphysical attractions of the propensity theory are not undermined by its epistemological shortcomings. By equating objective probabilities with long-run frequencies, the frequency theory suggests an obvious recipe for measuring objective probabilities—namely, by observing frequencies. By contrast, the propensity theory cuts any constitutive link between objective probabilities and frequencies, and so seems to leave us in the dark about how to measure objective probabilities.

However, this contrast is an illusion. The propensity theory can deal with the epistemology of objective probability in just the same way as the frequency theory. To see this, consider in more detail how the epistemology will work on the frequency theory. This theory equates the real probability with the eventual long-run frequency. However, this eventual long-run frequency is not itself directly observable. Even if we have an actual frequentist theory, we will have to wait until the end of time to observe the proportion of Rs among *all* the Ts. And on a hypothetical infinite frequency theory the relevant frequency will be even less directly observable. So frequency theorists need to recognize that in practice we must estimate the real
p. 213 probability from the observed sample frequency in some *finite partial* sequence of Ts. ↳

Now, there is no agreed view about the logic of inferring real probabilities from finite sample statistics. It is easy enough to work out the probability of the observed statistics *given* various alternative hypotheses about the real probabilities. But what exactly is the logic of getting from this to the acceptability of the alternative hypotheses themselves *given* the observed statistics? There are a number of quite different approaches to the logic of statistical inference, including the Fisherian, Neyman–Pearson, and Bayesian accounts. Still, they do at least all agree that such inferences hinge crucially on the extent to which alternative hypotheses about the real probability imply that the observed statistic was itself probable. (For example, how probable are 40 'up's in 100 measurements if the true probability of 'up' is 0.5?) In one way or another, the different theories of statistical inference all take hypotheses about the real probability to be more favoured the more they imply that the observed statistic was likely.

Now, there is nothing in this last thought that is not available to the propensity theorist. On the propensity theory, real probabilities are primitive quantities, and not to be equated with long-term frequencies. But this does not prevent real probabilities from having implications about the probability of observing such-and-such finite statistics in finite samples. This simply falls out of the fact that propensities obey the axioms of probability. (For example, if the propensity of 'up' in one measurement is 0.5, the propensity of 40 'up's in 100 measurements is $^{100}C_{40}\ 0.5^{100}$.) So the propensity theorist can say exactly the same thing about the epistemology of probability as the frequentist—hypotheses about the real probability are to be favoured the more that they imply that the observed statistics are likely.

It is important to realize that the propensity theorist does not deny that there is an important connection between objective probabilities and frequencies. But this is not the frequentist's proposed *reduction* of objective probabilities to long-run frequencies, an equation that is of course rejected by the propensity theorist. Rather it is simply the connection implied by the axioms of probability, namely, that in repetitions of identical and independent trials in which the real probability is such-and-such, it is so-and-so probable that the observed frequency will be thus-and-so. And this connection is of course as much available to the propensity theorist as to the frequency theorist.

It is perhaps also worth observing that, from the perspective of the logic of statistical inference, frequencies in large numbers of repeated trials have no special status. They are just one example of an observed statistic that can yield information relevant to the assessment of hypotheses about true probabilities. To see this, suppose that for some reason you suspect that a given coin is strongly biased towards heads, as opposed to being fair. You toss it just once and see tails. This single toss is already significant evidence that the coin is fair rather than biased, since tails would be less likely if the coin were biased than if it were fair.

The only difference between this case and frequencies in large numbers of repeated trials is that the latter
p. 214 will normally carry far more discriminatory ↳ information. Suppose that we are trying to decide between a range of alternative hypotheses about the true probabilities. A single observation will often be reasonably likely on various different hypotheses from this range, and so be relatively indecisive in choosing between them. But a specific frequency in a large sample will normally be highly unlikely on all but one of the hypotheses being evaluated, and so will favour that hypothesis very strongly over the others. But this is a quantitative difference, not a qualitative one. Single observations also carry just the same *kind* of statistical information as large-sample frequencies.

5 STATISTICAL INFERENCE AND EVERETTIANISM

The last section pointed out that propensity theorists can make just the same statistical inferences as frequency theorists. Let us now apply the moral to the interpretation of quantum mechanics. Given that Everettians will think of objective probabilities as propensities, they too can make just the same statistical inferences.

In particular, Everettians can follow orthodoxy in using observed results to estimate the probabilities of different values for the measured spins, positions, momenta, energies and so on of quantum systems. Just like orthodoxy, they will favour those hypotheses about these probabilities that imply that the observed results were probable, and disfavour those hypotheses that imply that they were improbable.

Now, we can take it that Everettians and orthodoxy alike will conclude from such investigations that the probabilities of quantum events correspond to the squared moduli of their wavefunction amplitudes. Equating the relevant probabilities with these numbers implies a far higher probability for the observed results than any other assumptions.

Of course, Everettians will have to recognize that there are some branches of reality on which the observed statistics will be misleading as to the true probabilities. (For example, there is a branch of reality in which x-spin measurements on electrons in z-spin eigenstates always show 'up'.) But it is not clear that this undermines the symmetry between Everettian and orthodox statistical thinking. For orthodoxy must also recognize that it is always epistemologically *possible* that the so-far observed statistics do not reflect the underlying probabilities. In the face of this possibility, orthodox statistical thinking simply proceeds on the assumption that we are *not* the victims of misleading samples, by favouring those hypotheses that make what we observe probable, rather than hypotheses that make the observations *im*probable. Everettians can once more do the same, proceeding similarly on the assumption that our observations are not atypical.

Of course, the rationale for proceeding in this way remains to be explained. This brings us back once more to the logic of statistical inference. Now, as I said above, there are different views on this issue—Fisherian,
p. 215 Neyman–Pearson, and ↳ Bayesian—and there is no question of going into details here. But we can at least ask whether there is any reason to suppose that Everettianism will be worse off than orthodoxy in accounting for statistical inference. I cannot see why there should be. Everettians take the objective probability of a statistic to be a measure over the branches of reality on which it occurs, where orthodoxy takes it to be a measure of the tendency of the corresponding possible branches of reality to be actualized. Both Everettians and orthodoxy then favour those hypotheses that imply that the statistics we observe were objectively probable. The exact rationale for this move is then the subject of dispute between the different theories of statistical inference. But there is nothing in any of these theories, as far as I can see, that requires an orthodox understanding of objective probability rather than an Everettian one.

Perhaps it is worth making one last comment about statistical inference. Most philosophers concerned with statistical inference have now turned away from the contortions of Fisherian and Neyman–Pearson accounts of statistical inference and embraced the relative comforts of Bayesianism (the reasons are given in Howson and Urbach [1989]). In the Bayesian context, the rationale for statistical inference reduces to the rationale for Bayesian updating. Why should we increase our credence in hypothesis H in proportion to the extent that H makes the observed statistics more probable than they would otherwise be? Now of course Bayesian updating is itself the subject of a huge literature, which again lies beyond the scope of this paper. But one recent and highly persuasive suggestion, due to Hilary Greaves and David Wallace [2006], is that Bayesian updating can be seen as a special case of the decision-theoretic maximization of expected utility. If this is right, then the question of whether Everettianism can account for statistical inference collapses into the question of whether it can account for decision-theoretic expected utility maximization. It is to this question that I now turn.

6 EVERETT AND THE PRINCIPAL PRINCIPLE

Maybe Everettians can mimic orthodoxy in using standard statistical reasoning to infer that the 'objective probabilities' of quantum results are given by their squared amplitudes. But I am sure that many readers will as yet remain unpersuaded that the numbers so inferred warrant being called 'probabilities'. Maybe Everettians can attach numbers to future branches that behave numerically like probabilities. But Section 2 told us that something more is required of objective probabilities. They also need to figure in the Principal Principle. Objective probabilities are quantities to which subjective probabilities and hence decisions ought rationally to conform.

As I said in my introduction, we don't want to get bogged down in the terminological issue of exactly what
p. 216 justifies calling some quantity an objective ↳ 'probability', and in particular whether it needs to be associated with ignorance about the future. But I am happy to concede that at least some association with subjective probability and rational decision is a minimal requirement. There would be little point to calling something objective 'probability' if it had nothing to do with rational decision. It is here that many will feel that Everettianism falls down. Their numbers have no appropriate connection with rational decision. Where orthodoxy can maintain that it is rational to orientate decisions to the squared amplitudes, Everettians are unable to do this.

At this point I think that Everettians should simply *assert* that it is rational to match subjective probabilities to the squared amplitudes. In making this move, Everettians will thereby commit themselves to favouring actions that bring rewards in future circumstances with large squared amplitudes, and attach relatively little significance to how things will turn out in futures with low squared amplitudes. Of course, Everettians won't think of risky choices in quite the same way that orthodoxy does. They won't think that only one future will become real, and that their actions are designed to do well, if things pan out as expected, in *that* future. Rather, they will think that all futures with positive amplitudes will become real, and that their action is designed to maximize utility across all these real futures, weighted by their objective probabilities. But, even so, they will treat the squared moduli just as orthodoxy does in their expected utility calculations.

Still, many will want to ask, are Everettians *entitled* to enter the squared amplitudes into expected utility calculations? I just said that Everettians should 'simply assert that it is rational to match subjective probabilities to the squared amplitudes'. But many will feel that this is not enough. Maybe Everettians can simply assert that it is rational to match subjective probabilities to the squared amplitudes. But cannot orthodoxy go further, and *show why* it is rational to have these subjective probabilities? Those suspicious of Everett will feel that their 'simple assertion' of the principal principle has all the advantages of theft over honest toil. Aren't they just helping themselves to an assumption that can only be properly accounted for within orthodoxy?

The underlying thought here was forcefully stated by David Lewis in 'Humean Supevenience Debugged' [1994]. (He wasn't thinking of Everettianism, but he may as well have been.) 'Be my guest—posit all the . . . whatnots you like . . . But play fair in naming your whatnots. Don't call any alleged feature of reality "chance" unless you've already shown that you have something, knowledge of which could constrain rational credence . . . Again, I can only agree that the whatnots deserve the name of chancemakers if I can already see, disregarding the names they allegedly deserve, how knowledge of them constrains rational
p. 217 credence in accordance with the Principal Principle' [pp.484–5].[6] ↳

Lewis is here suggesting that within an orthodox metaphysical framework certain quantities can be *shown* to deserve the name of objective probability—by showing how knowledge of them constrains rational credence. If this cannot be done within Everettianism—if Everettianism is simply reduced to *asserting* that Everettian squared amplitudes should constrain rational credence, where orthodoxy can explain why—then this would certainly count strongly against Everettianism. However, it will be a central contention of this

paper that there is no real contrast here. Orthodoxy cannot deliver on Lewis's promise. It does no better than Everettianism when it comes to explaining why we should match our subjective probabilities to the squared amplitudes. In the end, orthodoxy too can do no better than simply asserting without further justification that it is a basic requirement of rationality that we should set our degrees of belief equal to the squared amplitudes.

The next two sections will be devoted to this point. That is, I shall show that orthodoxy has no good way of explaining why we should bet in line with the squared amplitudes, as opposed to simply asserting this. In this respect orthodoxy is no better off than Everettianism. The following section will then seek to show that orthodoxy is rather *worse* placed than Everettianism when it comes to accounting for the connection between the squared amplitudes and subjective probabilities.

7 ORTHODOXY AND THE JUSTIFICATION OF BETTING WITH THE OBJECTIVE PROPENSITIES

So—can orthodoxy *justify* setting degrees of belief equal to the squared amplitudes of quantum results? We are taking it that, for both orthodoxy and Everett, standard methods of statistical inference will indicate that these squared amplitudes measure the objective probabilities of those results. And we can also take it, following the last section, that Everettians will follow orthodoxy in asserting *that* it is rational to set degrees of belief equal to these objective probabilities. The crucial question is then whether orthodoxy can do better than Everettianism in showing *why* it is rational to allow these objective probabilities to guide us in making decisions.

As we saw earlier, orthodoxy can choose between two different theories about the nature of objective probabilities. On the frequency theory, objective probabilities are identified with long-run frequencies. On the propensity theory, by contrast, objective probabilities are primitive and fully present in each single case —while of course still having (probable) implications about what sample frequencies will be observed.

In the rest of this section, I shall show that an orthodox propensity theorist can give no good account of why
p. 218 it is rational to bet with the objective probabilities. ↳ In the next section, I shall consider whether orthodoxy can do any better by bringing in the frequency theory of probability.

At first sight it might seem as if orthodoxy has an obvious advantage over Everettianism in explaining why we should bet with the objective probabilities. Cannot orthodoxy just say that betting with the probabilities will bring desired results in *the* actual future? By contrast, Everettians cannot say this, since they don't think there is *the* single actual future, but a branching future which includes every outcome with a non-zero squared amplitude—which means that they have to admit that betting with the objective propensities will bring bad results on some low-amplitude branches, as well as good results on high-amplitude ones.

But the contrast here is spurious. (Don't confuse familiarity with cogency.) After all, orthodoxy *cannot* really say that betting with the propensities *will* bring desired results in the actual future. Good bets don't always pay off. Even odds-on favourites can fail to win. At best, orthodoxy can say that betting with the propensities makes it objectively *probable* that you will win.

But of course Everettians can say this too (or at least that it's objectively 'schprobable' that you will win). And for neither does this amount to any kind of independent justification of betting with the squared amplitudes. If you want a further reason for choosing actions that give a high probability to desired results, it's scarcely any answer to be told that those choices make desired results highly probable.

What about the thought that you will win in the *long run*, even if not in the single-case, if you keep making bets in line with the objective propensities? Here again it may look as if orthodoxy does have a real advantage. There is no Everettian counterpart to the orthodox thought that you will win in the long run if you bet with the objective odds. For, as we saw earlier, Everettians must recognize a *plurality* of long-run futures, encompassing all long-run sequences with non-zero amplitudes, and on many of these sequences, betting with the objective odds will lead to long-run losses.

But this line of thought doesn't help orthodox propensity theorists either. For they cannot really say that you *will* win in the long run, any more than they could say this about the single case. All that they can say is once more that you will *probably* win. After all, if you can be unlucky once, you can be unlucky repeatedly. Of course, if you are betting with the odds every time, then the probability of your losing overall will become smaller the more bets you make. But familiar calculations show that there will remain a finite probability of losing overall for any finite sequence of bets. So once more the putative justification for betting with the objective odds collapses into the empty thought that this strategy makes winning objectively probable.

What about the *infinite* long run? Isn't it guaranteed that you will win if you go on making advantageous bets for ever? Even this isn't obvious, on the current propensity understanding of probability. Let us put to one
p. 219 side for the moment worries about the idea of betting for ever and about the relevance of ↳ distantly deferred guarantees of success. Even so, there remains the point that the axioms of probability imply that infinite sequences where you end up losing remain *possible* (even if they 'almost never' occur in the technical sense that the probability measure of such sequences taken collectively is zero). After all, to take a special case, all specific infinite sequences of tosses with a fair coin are equally likely. So it would be ad hoc to say that some of them—the ones where the limiting frequency of heads differs from 0.5—are absolutely impossible, solely on the grounds that there are far fewer of them. After all, any specific such 'rogue' sequence is just as likely as any specific 'normal' sequence.

So here too it turns out that orthodoxy is no better off than Everettianism. We still can't say that you *will* win if you bet for ever, only that the probability of doing so will be very high indeed, which once more begs the question.

8 ORTHODOXY AND THE JUSTIFICATION OF BETTING WITH THE FREQUENCIES

By this stage the friends of orthodoxy might be thinking that it was a mistake to turn away from the frequency theory in favour of propensities. We have just seen that, if orthodoxy thinks of probabilities as primitive propensities, then it is unable to guarantee a definite gain for somebody who bets with the objective odds, even in the long run. At best it can say that such a gain is highly probable. However, a frequency theory promises to remedy this problem. For on the frequency theory, orthodoxy can indeed assert that betting with the objective odds will guarantee success in the *eventual* long run.

To see how this might work, suppose that the objective probability of R on any trial T can be reductively identified with the eventual frequency of Rs among some all-encompassing class of Ts. Then we can be sure of a positive result if we consistently bet a constant amount at objectively advantageous odds on *all* the Ts in that class. (If the probability equals the frequency, then the objective advantageousness of each bet will ensure that, when you have been through all the Ts, the total amount you won on the Rs will outweigh the total amount you lost on the not-Rs.)

Perhaps this is the kind of thing that David Lewis had in mind when he suggests that his frequency-style analysis of objective probability ('chance') allows us to understand *why* it is rational to match degrees of belief to objective probability. Thus he claims 'And we can well understand how frequencies, if known, could

constrain rational credence' [p.476]. And later he says in similar vein 'I think I see, dimly but well enough, how knowledge of frequencies and symmetries and best systems could constrain rational credences' [p.484].

If Lewis were right here, then it would certainly follow that Everettians are less able to make sense of probabilities than orthodoxy. For, as we saw earlier, there is no possibility of Everettians embracing the
p. 220 frequency theory of probability. Since ↳ they recognize many possible futures displaying many eventual frequencies, it makes no sense for them to talk about probability being the same as *the* eventual long-run frequency. So they have no route to the claim that betting with the objective odds is *guaranteed* to bring eventual long-run success. They remain stuck with the non-explanatory claim that betting with the odds is highly *likely* to bring success.

Still, it remains to be shown that Lewis is justified in claiming that the frequency theory really does afford orthodoxy a good justification for matching credences to the objective probabilities. Lewis says he can see this 'dimly, but well enough'. But he does not elaborate, and it is by no means clear how the story is supposed to go.

Let us put to one side the general worries about the frequency approach to probability rehearsed in Section 3 above. Even if we take the frequency theory as given, there is an obvious worry about the proposed long-run justification for betting with the objective odds. As J.M. Keynes put it, 'In the long run we're all dead'.

We are trying to explain why any particular agent in any particular situation is well advised to bet in line with the objective odds. By way of explanation we are given the thought that, if we were to continue betting like this in every similar situation, then we would be sure to win in the end. It is not at all clear that this is to the point. Why is it an argument for my betting with the odds *now* that this would pay off in the eventual long run?

The problem is particularly clear on the hypothetical infinite version of the frequency theory. On this version, the supposed justification is that you *would* be guaranteed to win, if you were to bet an infinite number of times. It is not at all clear what bearing this counterfactual truth has on agents who will in fact only make a limited number of decisions in their lifetimes.

Nor does the problem go away if we switch to an actual frequency theory. Now the probability is equated with the relative frequency of Rs among all Ts in the actual world. The guarantee is correspondingly that you will win for sure if you bet at advantageous odds on all these actual Ts. This is still of no obvious relevance to actual agents. They aren't going to bet until the end of time, but at most until the ends of their lives. There is no obvious way of showing why such mortal beings are rationally required to adopt a strategy that isn't guaranteed to succeed until the end of time.

To bring out the point, consider some feckless agent with no thought for the future. ('I want some drugs *now*—who cares what happens tomorrow?') Suppose that this agent is offered some wager, and is allowed to decide whether to bet with the objective odds or against. I take it that the rational choice for this agent is to bet with the objective odds. Yet it is clearly of no relevance to this agent that repeating this strategy will pay off at the end of time. This agent just doesn't care about the future. Whatever makes the agent's choice
p. 221 rational, it looks as if it is independent of what will happen in the long run. ↳

I conclude that frequency-based long-run justifications for betting with the odds are looking in the wrong place. The basic fact is that the right choice in any situation is to bet with the objective odds. A special case of this is that you should also bet with the objective odds in a complex trial consisting of repeated single trials. But we can't justify the basic fact in terms of the special case, even given the frequency theory of probability.

Perhaps David Lewis was thinking of something different from a long-run justification when he said he could ‘see dimly, but well enough’ how frequencies could constrain rational credence. But if so he gives us no clue of what the alternative story might be. And it is hard to see what else, apart from a long-run story, would give frequencies as opposed to ‘whatnots’ (like squared moduli of amplitudes) an advantage in constructing the requisite justification.

9 ORTHODOXY IS WORSE OFF

The last two sections were devoted to showing that orthodoxy has no good way of explaining why the squared moduli of wavefunction amplitudes should play the decision-theoretic role that the principal principle requires of objective probabilities. In the end, orthodoxy can do no better than simply assert without further explanation that it is rational to conform credences to the squared amplitudes. Since Everettians can also simply assert this, they are certainly no worse off than orthodoxy in forging a link between credences and squared amplitudes.

In this section, I want to show that Everettians are actually better off with respect to this connection. One possible reason why they might be better off has been explored by David Deutsch and David Wallace. They have sought to show that some very basic assumptions about rationality dictate that Everettian agents should match their degrees of belief to the squared amplitudes (Deutsch [1999], Wallace [2003]). However, I shall not rest anything on this somewhat controversial line of argument (for doubts, see Greaves [2004], Price [2006]). Rather I shall seek to show that, even if the Deutsch–Wallace argument fails, there is another respect in which Everettians can make better sense than orthodoxy of the connection between credences and squared amplitudes.

Go back to the thought that neither orthodoxy nor Everett can do better than simply *assert* that credences should match the squared amplitudes—equivalently, that rational agents should bet in line with the squared amplitudes. There is a sense in which the inability to further justify this claim is far more embarrassing for orthodoxy than for Everett. This is because orthodoxy thinks of the aim of rational action differently from Everettianism. For orthodoxy, an action is successful if it produces good results in the presumed one actual future. For Everettianism, by contrast, an action is successful if it maximizes utility
p. 222 over all future worlds weighted by their squared amplitudes. Given this difference, ↳ it seems as if orthodoxy, but not Everettianism, still faces a further question even *after* it has asserted that rational agents should bet in line with the squared amplitudes. What is the connection between this strategy and the ultimate aim of action, namely, good results in the actual world? But, of course, this is a question that orthodoxy cannot answer, as the last two sections have made clear.

The trouble is that orthodoxy seems to be making its primitive commitment in the wrong place. It commits itself to the maximization of objective expected utility. But that's not what orthodox actions are aimed at. Rather they aim at gain in the one actual future. And this then creates a demand for some further explanation of why maximizing objective expected utility is a good means to that further end. But of course, to repeat, there's no good way for orthodoxy to answer the awkward question it poses. There's no way of showing that betting with the objective odds will bring an actual pay-off.

True, we are assuming that Everettians are also unable to give any further justification for betting with the squared amplitudes. But Everettians have far less need of such a justification. For they regard the maximization of objective expected utility as itself the ultimate aim of action. From an Everettian perspective, there is no sense in which an action that maximizes objective expected utility can nevertheless turn out to be unsuccessful. There is no danger that such an action might prove to have been the wrong choice if some unlikely actual future comes to pass. Since all futures with any probability are sure to happen, an action that maximizes expected utility over all of them cannot be bettered. So Everettians, unlike

orthodoxy, face no awkward and unanswerable further question about the connection between their primitive commitment and some further ultimate aim.

Let me bring out the point by an analogy. Consider a figure that I shall call ‘the committed gambler’. The committed gambler doesn't care about money. Over the years she has already made a fortune. What she prides herself on is making the right bet. She wants to be in the pot if it offers better than 4–1 odds on a flush draw, and to fold if it doesn't. But she doesn't care whether or not she actually makes her flush—that's nothing to do with good poker. For the committed gambler, it is vulgar to be concerned with actual pay-offs. The important thing is to identify bets that offer ‘good value’ and to get in on the action. That's the true mark of success—only amateurs care about the actual fall of the cards.

Now compare the committed gambler with the ‘ordinary gambler’ who just wants to make money. For the ordinary gambler, there's no virtue in being in the pot just because it offers ‘good value’—such a bet will only be a good thing if the flush is actually made and the pot won. Now, faced with the ordinary gambler, we can ask the awkward question I pressed above—why do you think it's such a good idea to maximize objective expected money, when what you want is actual money? And, as we have repeatedly seen, there is
p. 223 no good answer to this question. ↳

But this question gets no grip on the committed gambler. After all, the committed gambler doesn't want the actual money—all she wants is to maximize objectively expected money. So the committed gambler, unlike the ordinary gambler, owes us no further explanation of why her decision strategy is a good idea. From her perspective, maximizing objective expected money isn't a means to some further end, but itself the ultimate aim of action, and so there is no need for any further explanation of its connection to some supposed further end.

The analogy with the knowledgeable Everettian agent should be clear. Like the committed gambler, the Everettian agent doesn't regard the maximization of objective expected utility as some means to a further end, success in the one actual future. Rather, it is itself the essential characteristic of a fully successful choice. So Everettian agents, unlike ordinary agents understood within orthodoxy, face no nagging question of why maximizing expected utility is a good means to actual success—a question to which, as we have seen, orthodoxy has no good answer.

Of course, the analogy between the knowledgeable Everettian agent and the committed gambler is not perfect. To see where it breaks down, we can usefully invoke another analogy. Hilary Greaves [2004] has suggested that a knowledgeable Everettian agent is like someone who adopts a differential ‘caring measure’ over branching futures—the agent will be concerned about these futures to different degrees, in proportion to their squared amplitudes, just as a mother might be concerned about the fates of different offspring to different degrees.

Now, the virtue of Greaves' analogy is that it emphasizes how the choices of a knowledgeable Everettian agent are not risky: their success is not hostage to future fortune. In this respect they are like the choice of a mother who gives more attention to one child than another just because she wants to: there's no sense in which this mother can then fail to achieve what she is aiming at.

But in another way, talk of a ‘caring’ measure can be misleading. It is not as if the general run of Everettian agents will have unusual desires compared with ordinary agents. Just like ordinary agents, they will wish only for food, comfort, fame, the welfare of their offspring, and other normal kinds of success. While their decisions can be modelled by supposing that, in addition, they ‘desire’ to do well more in some futures than others, these extra decision-informing attitudes are not really desires. The rationale for these attitudes is quite unlike the rationale for normal desires. The Everettian agent doesn't favour some worlds more than others because results in those worlds have some extra qualitative virtue, but simply because those worlds have higher squared amplitudes.

Given this, we will do well to distinguish these differential Everettian attitudes to future worlds from normal desires. The obvious way is to mark the difference by styling them ‘probabilities’. But even those who are uneasy about this terminology will still need to distinguish these attitudes from ordinary desires,
p. 224 and to recognize that Everettain choices are designed to maximize the satisfaction ↳ of ordinary desires weighted by these other attitudes. That is, we still need something like the distinction between utilities and probabilities, even after we recognize that the success of a rational Everettian choice offers no hostage to future fortune.

This now enables us better to appreciate the limits of the analogy between the committed gambler and an ordinary knowledgeable Everettian. Both avoid the nasty question that can be put to the ordinary gambler—what's so good about maximizing *expected* money, when what you want is *actual* money? And moreover they both avoid it because they *don't* want actual money. But the reasons are rather different—where the committed gambler doesn't want actual *money*, the knowledgeable Everettian doesn't want *actual* money.

The point is that the committed gambler has non-standard desires—her peculiar history as a gambler means that she has no longer any interest in winning money per se, but rather takes pride in finding ‘good value’ bets. So the reason that she cannot be pressed to further justify her money-maximizing choices is simply that she doesn't care about money. An ordinary knowledgeable Everettian, by contrast, will have normal desires, including a desire for money. The reason the Everettian agent doesn't face the nasty question—why maximize expected money, when what you want is actual money?—is rather that this notion of ‘actual’ money gets no grip on the Everettian. The Everettian doesn't think there is just one actual future, which may or may not contain money, but rather a range of different futures with different squared amplitudes, some containing money and some without.

The similarity between the committed gambler and the knowledgeable Everettian is that both regard the maximization of objective expected utility as the ultimate criterion of a successful action. But for the committed gambler this is because of non-standard desires, whereas for the Everettian it is because of non-orthodox metaphysics. When Everettians weight alternative futures differentially in making choices, this isn't because they are so peculiar as to prize utility-maximizing choices in themselves, but simply because they are committed to weighing all the different futures by their squared amplitudes when making choices. For Everettians, this is a basic and *sui generis* commitment—the commitment to match subjective probabilities to squared amplitudes—and not to be viewed as akin to the committed gambler's peculiar pride in seeking out ‘good value’ bets wherever they can be found. It's not that Everettians desire to make good bets independently of their consequences, like the committed gambler. On the contrary, it is precisely the consequences that they are interested in, and they pursue them in the way dictated by their basic commitment to bet with the objective probabilities.

In making a basic commitment to match subjective probabilities to squared amplitudes, Everettians are akin to ordinary orthodox agents, rather than to the committed gambler. There are no unusual desires in play,
p. 225 just the basic commitment to betting with the objective odds. Moreover, I am allowing that ↳ Everettians are also like orthodox agents in being able to offer no further justification for this basic commitment. Still, the point remains that in another respect Everettians line up with the committed gambler rather than with orthodox agents. For even after the basic commitment has been made, orthodox agents have something further to explain, in a way that Everettians and the committed gambler do not.

According to orthodoxy, the action that maximizes objective expected utility might or might not turn out to have been the right choice: the pot may offer very good odds for your flush draw, and so you bet, but even so you may fail to make the flush, in which case you would have been better off not betting. Given this, we cannot help asking what is so good about maximizing expected utility, given that it may well turn out to be the wrong choice—even though we know that there is no answer to this inescapable question. It counts strongly in favour of Everettian metaphysics that it renders this awkward question unaskable.

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Notes

1 Lewis's own formulation of the principle specifies that we should match our credences about outcomes to our credences about the objective probabilities, rather than to the objective probabilities themselves. For my reasons for stating it more objectively see Beebee and Papineau [1997]. This difference will not matter to the arguments of this paper.

2 A tradition of writers from Everett himself onwards [1957 p. 461] has observed that in the infinite limit the squared amplitude will be zero for the union of all sequences in which the frequency differs from the underlying squared amplitude of R. But this is not to the point in the present context. We are looking for a unique actual sequence so that we can identify the probability of R with its frequency in that sequence. Everett's observation does nothing to ensure a unique such sequence (especially given that any specific infinite sequence in which the frequency *is* equal to the underlying squared amplitude of R will also have a zero squared amplitude).

3 I am not entirely happy about the terminology of 'propensity theory'. This term is often understood more specifically than I intend it here (cf. Gillies [2000]); indeed, Karl Popper's original 'propensity' theory is really a version of the frequency theory. I have in mind any theory that takes single-case probabilities to be basic, and not reducible to anything frequency-like. I was brought up to use 'chance' in this sense, following Mellor [1971] and Lewis [1980]. However, this term could now be misleading, given the extent to which Lewis's later work takes 'chances' to be reductively constituted by actual frequencies [1986, 1994].

4 Are not frequency theories of probability also in immediate danger of contradicting themselves? The axioms of probability imply that there is always room for long-run sequences in which the frequency differs from the probability (even in infinite sequences such divergence isn't absolutely ruled out, even if it 'almost never' happens). Yet the reductive identification of probability with the long-run frequency would seem to mean that any such divergence is quite impossible. However, this worry isn't as bad as it looks. Frequency theorists can say that this supposed contradiction equivocates on the phrase 'the frequency of Rs'. Suppose that in this world there is a definite relative frequency of Rs among Ts, equal to p say. Then a frequency theory of probability does indeed entail that p is the objective probability of Rs in Ts. But this doesn't deny that things *might* have been different, in line with the axioms of probability, and that in different possible worlds, so to speak, there are different limiting frequencies. There is no contradiction between the thoughts that the frequency is (a) p in this world but that (b) it might have been different. Of course, a frequency theory of probability does imply that, if the long-term frequency had been different, the objective probability would have been different too. That is, the objective probability will not be p in those worlds where the long-run frequency differs from p. And this means that frequency theories rule out any possible worlds that display limiting frequencies different from their own objective probabilities. This in itself may strike some readers as odd. But even if it is odd, it is not obviously contradictory.

5 David Lewis's mature 'best theory' Humean theory of chances [1986, 1994] contains many elements of an actualist frequency theory. His 'best theory' aims to be sensitive to symmetries as well as frequencies, but he also asks of the 'best

theory' that it optimize the probability of the actual course of history—and this will tend to fix the probability of Rs in Ts as their actual frequency. See also Loewer [2004].

6 We can see Lewis here as raising the standards for something to qualify as 'objective probability'. Where his earlier work required only that 'objective probability' should be that quantity to which it *is* rational to match your credence, here he seems to be requiring in addition that we be able to '*see how*' knowledge of this quantity constrains rational credence.