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Self-Locating Uncertainty, Subjective Probability and the Everett Interpretation

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Abstract

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This paper analyses an attempt to reintroduce the probabilistic predictions of quantum mechanics into the Everett interpretation. An overview of the measurement problem and the Everett interpretation are first given, with particular attention paid to the issue of probability within a deterministic theory. An argument is put forth that observers in an Everettian multiverse generically experience a period of post-measurement uncertainty in regards to their location. It is further argued that this uncertainty allows pre-measurement to observers to act as if they are uncertain about future outcomes. This is then the justification for attaching probabilities to the squared moduli of the wave function. It is also noted that there is an assumption of typicality present in discussions of probability within Everett and some arguments to justify this assumption are considered.

Contents

Chapter 1

Introduction

A major focus in the philosophy of physics during the second half of the 20^{th} century has been the attempt to solve the measurement problem in quantum mechanics. Various reformulations and reinterpretations of the mathematical framework of quantum mechanics have been proposed, to varying levels of acceptance. Among the most popular and widely studied is the Everett, or many worlds, interpretation. Its creator, Hugh Everett, suggested that we take seriously the mathematics of quantum physics, eliminate the worryingly unscientific talk of observers that bogs down the standard interpretation, and accept that the full and complete description of the universe is given by the Schrödinger evolution of the quantum wave function. Everett's theory was successful in its primary goal: by eliminating the concept of wave function collapse, the measurement problem is avoided. At the same time, it was susceptible to a host of new critiques. Opponents have suggested that Everett is empirically inadequate and ontologically extravagant. However the most pressing concern comes about as a result of the dynamics of the theory itself.

Everettian quantum mechanics is a purely deterministic theory. This entails that, to use Alan Guth's phrase, everything that can happen will happen. A superposition of states, according to Everett, represents a multitude of actual outcomes. Naturally there is great philosophical interest in the ramifications of this facet of Everett. This interest can be neatly summarised by the following two questions. Firstly, how can one function in a world where there is complete knowledge about the future? Secondly, how can we reconstruct the probabilistic predictions of standard quantum mechanics within a purely deterministic theory?

It the ultimate goal of this paper to examine whether answers to these questions are available. The structure of the paper will be as follows. First, an overview of the development of quantum mechanics and the measurement problem. Following this, an overview of some Everett competitors - alternative proposals for solving the measurement problem.

The next section details the history and development of Everettian quantum mechanics, with particular attention paid to how it solves the measurement problem. A sketch of the metaphysics and ontology of Everett is also given. Some of the more general concerns regarding Everett are detailed and given brief responses. Then, an examination of the issue of probability in Everett. Both sides of the problem are addressed. Firstly, the incoherence problem: how to talk about probability within Everett. Secondly, the quantitative problem: how to justify attaching probabilities to the squared moduli of the wave function. Some attempts to solve these problems are presented and discussed before arriving at the focus of the paper - the post-measurement uncertainty approach first introduced by Vaidman.

Vaidman suggests that we establish our probability on the fact that observers are typically genuinely uncertain about the outcomes of measurements for a period of time after the measurement is performed. It is discussed whether or not this period of time is a genuine feature of Everett, and whether or not it can be circumvented by an observer. The next step is to consider whether this post measurement uncertainty can be the foundation of the illusion of pre-measurement uncertainty, and whether this can form the basis of our account of probability. Finally a more general concern is raised - do accounts of probability in Everett require the assumption that one's experiences are roughly typical of the overall Everettian multiverse?

Finally, the pros and cons of the Vaidman account are weighed up and it is decided whether or not it constitutes a credible attempt to reintroduce probability into Everettian quantum mechanics.

Chapter 2

Quantum Mechanics and the Everett Interpretation

2.1 Quantum Mechanics

The development of quantum mechanics in the early part of the 20^{th} century marked the beginning of perhaps the most intriguing era in the history of science. The new theory showcased unprecedented predictive accuracy and promised stunning new insights into the microscopic nature of reality. Yet these exciting new developments were accompanied by the troubling and seemingly inescapable notion that we had somehow come to the limit of what we could non-intrusively observe and discover about the world. Niels Bohr was a fierce proponent of this line of thought. Heisenberg recalls Bohr claiming that "developments in physics during the last decades have shown how problematical such concepts as 'objective and 'subjective are [and] it is no longer possible to make predictions without reference to the observer or the means of observation. The objective world of nineteenth-century science was, as we know today, an ideal, limiting case, but not the whole reality. [1, p. 114]" The radical metaphysical implications of this kind of thinking cannot be understated, and drew strong opposition from some of the great thinkers of the time. Einstein was particularly vociferous in his rejection of Bohr's ideas, and the subsequent Bohr-Einstein debates have come to typify this crucial period in the history of the philosophy of science.

The two occupied opposite corners on the fundamental question in the philosophy of science; scientific realism versus instrumentalism. Should, as Einstein hoped, we take the content of our best scientific theories to tell us something about how the world is? Or should we agree with Bohr that our theories are merely predictive tools, offering no insight into the nature of reality? A key battleground in the debate was the 1927 Solvay conference in Brussels. In attendance was a staggering collection of great minds, among them the world eminent quantum theorists. Bohr was the victor here: two of his disciples, Werner Heisenberg and Max Born, proclaimed that "quantum mechanics [is] a closed theory . . . whose fundamental physical and mathematical assumptions are no longer susceptible of any modification. [2, p. 435] This seemed to settle the matter - the mathematical framework of quantum mechanics was to be viewed as a predictive tool, rather than any sort of reflection of reality. This approach, loosely codified as the Copenhagen interpretation, became the de facto standard way to think about quantum mechanics. However, as people attempted to probe the foundations of quantum theory, they encountered deeply troubling issues for which Copenhagen had no satisfactory answers.

The trouble is that the Copenhagen interpretation is at best disconcertingly vague, and at worst incomplete. Capturing exactly what is contained in Copenhagen is a nontrivial matter - nowhere is the doctrine precisely defined. The best elucidation of the postulates of standard quantum mechanics is found in the writings of von Neumann [3]. There are, however, recurring ideas which can be taken as the core of the Copenhagen interpretation. Firstly, the quantum state of a particle (or collection of particles) is described by a wave function Ψ. The wave function is the most complete description of the state of the particle possible. Taking the square modulus of the wave function generates the probability density for that particle, where, for example, the normalised value at position x gives the probability of that particle being at position x. If isolated from the environment, the wave function evolves in time, continuously and deterministically according to the Schrödinger equation. When the wave function is in this un-collapsed state, there is no fact about the position of the particle. When a measurement is performed, the wave function collapses to a position eigenstate, and the particle assumes a determinate position with probability decided by its probability density. It is these two mechanisms, the deterministic evolution of the Schrödinger equation and the stochastic, measurement-induced collapse, that make up the standard collapse formalism of quantum mechanics. An obvious, pressing questions presents itself- what exactly is the nature of a quantum measurement?

The postulates of standard quantum mechanics entail that the wave function of a particle evolves deterministically according to the Schrödinger equation until it is observed, at which point the wave function collapses stochastically. However the mechanism which causes wave function collapse is not well understood. For instance, why is it the case that microscopic objects can exist in a superposition of states and have indeterminate position while macroscopic objects are always well localised? The role of the observer in wave function collapse is also contentious, and it is a goal of contemporary philosophers of physics to a genuine observer-free, objective description of quantum mechanics.

2.1.1 Alternative Interpretations

Increasing scrutiny of the difficulties posed by the measurement problem has led to a variety of alternatives to the Copenhagen interpretation. Two of the more promising proposals are discussed briefly here.

The first thing we note when considering the measurement problem is that it is essentially a characterisation of the apparent incompatibility of the microscopic and macroscopic descriptions of the world according to quantum theory. Microscopic objects (i.e. particles) must be allowed to exist in superposition, whereas the macroscopic world of cats, measuring devices and people must always exist in a collapsed state. Drawing on this, Ghirardi, Rimini and Weber suggested that the wave function collapse is an objective feature of the world, and must be tied to particle number. Their suggestion, **GRW Theory**, is that there exists a collapse function which acts on the wave function of a particle at a constant rate *per particle*. The collapse function is sharply peaked and, when multiplied with the smooth wave function, produces a localised particle. Naturally, large collections of particles (e.g. macroscopic objects) will collapse far more frequently than individual particles. Given that, superficially, we want macroscopic objects to always collapse and lone particles to never collapse we can set an upper and lower bound on the value of the collapse function. Of course this means that quantum mechanics, as it stands, is incomplete: the dynamics of the theory must be amended to include the collapse function. There are technical issues with the GRW proposal. For one, given that the collapse function must take the form of a Gaussian, the post-collapse wave function can never be genuinely localised. This is the tails problem: the theory does not actually guarantee the localisation of macroscopic objects. Recent revisions of the theory, more generally **spontaneous collapse theories**, have attempted solutions to this and other problems [4]. The experimental outlook on this class of theories is encouraging. The task is conceptually simple: narrow down the range in which the rate of collapse can exist, either by looking for increasingly small systems which collapse or increasingly large systems which do not [5]. Importantly, according GRW, the probabilistic predictions of quantum mechanics are representative of an objective feature of the world: the stochastic collapse function. to Along similar lines, Penrose has suggested that wave function collapse might be gravitationally induced, owing to the instability of superpositions of deformations in the gravitational field [6].

A second strategy is to propose that the wave function does not give us the complete description of particle. Instead, we propose the existence of a (non-local) hidden variable. This approach dates back to de Broglie in 1927 [7], and was later rediscovered by Bohm [8, 9] hence: **de Broglie-Bohm theory** or **Bohmian mechanics** . Under this scheme, a complete description of a system must also specify the positions of its particles. There is then an additional 'guiding equation' - the **pilot-wave** whose name the theory also occasionally bears - which dictates particles' velocities as a function of their positions. Like GRW, Bohmian mechanics successfully produces all the predictions of quantum mechanics. Unlike GRW, it is entirely deterministic. Given the wave function of a system and the positions of its particles at one time allows us to perfectly predict the positions at a later time. How does this square with the probabilistic predictions of quantum mechanics? The probability enters as an uncertainty over the initial conditions (i.e. positions) of the particles. Given an initial probabilistic distribution, it can be shown that the distribution is maintained by the deterministic evolution of Bohm's theory.

Bohm and GRW are two of the three more widely studied alternatives to standard quantum mechanics. As described above, both theories require tinkering with some aspect of the formalism. Either the Schrödinger dynamics must be altered to include the fundamentally stochastic collapse function, or the wave function must work in conjunction with some hidden variable to produce the evolutions we observe. A third approach is possible: investigate what happens if the wave function and the Schrödinger evolution is taken to be the complete description of the world.

2.2 The Everett Interpretation

Everett's great insight was to ask what happens to the quantum mechanical formalism should we omit the troublesome second postulate, and instead imagine the world described entirely by the deterministic evolution of the wave function according the Schrödinger equation. The advantages this route presents are obvious. There is no difficult talk of observers in our physical theory, and since there is no wave function collapse there is no measurement problem. Furthermore, in contrast to the proposals of GRW and Bohm outlined in Section 2.1.1 there is no need to either modify the dynamics or posit hidden variables. This aspect of Everett should not be understated. The theoretical predictions of quantum mechanics have been so accurately and extensively verified that there is a strong argument to the effect that we should remain as conservative as possible to the dynamics of the theory. At the same time, the physical implications of the mathematical framework set out by Everett are not immediately obvious. Some illustration is necessary.

2.2.1 Everett in practice

If there is a fundamental tenet of Everettian quantum mechanics it is that the wave function is all there is. The entire history and future of the world is determined by the evolution of the universal wave function. This evolution is always continuous and deterministic, and at no point does the wave function collapse. Consider the following example. A particle evolves into the following 'superposition' of states[1]:

[1] As we shall see, a superposition in Everettian quantum mechanics does not have the same physical connotations as in the standard theory. Superposition is in quote marks here to indicate this.

$$\Psi = \frac{1}{\sqrt{2}} |A\rangle |\Uparrow\rangle + \frac{1}{\sqrt{2}} |B\rangle |\Downarrow\rangle$$

Where $\Uparrow$ and $\Downarrow$ represent the outcomes of a spin-state measurement performed on the particle, and A and B represent the state of everything else in the world including, importantly, whether some attendant observer sees the outcomes of the measurement as up or down. In the standard case, measurement of the particle collapses the wave function in accordance with the Born rule, the world ends up in either state A or state B. There is no such collapse in Everett. Instead, the world evolves into both state A and state B. Thus the superposition is not, as in the standard case, a description of some mixture of states, but is simply a description of two distinct states. Wallace puts this nicely: "macroscopic superpositions do not describe indefiniteness, they describe multiplicity [10, p. 37]." Note that this offers some closure as to the fate of Schrödinger's cat. The cat certainly is both alive and dead, but not in the physically impossible way described by observer led quantum mechanics. Instead, the cat is simply alive in one 'place' and dead in another. These places, while not explicitly a part of Everett's mathematical formalism (which is just, after all, quantum mechanics minus the collapse postulate), are certainly a part of the greater Everettian picture. Various terms are used to describe them interchangeably: worlds, branches or branch-worlds. The totality of all such worlds is generally terms the multiverse.

The informal picture of Everett is then the following: as the universal wave function evolves into various superpositions, the universe branches or splits, yielding disconnected worlds where mutually incompatible outcomes occur. And note that these splits are not just going to occur as a result of measurements involving observers and measuring devices. We know that the rate of these splitting events is going to be enormous. Thus the multiverse we inhabit is going to be vast and rapidly expanding. This strikes many as pure science fiction, but we must remember that it is simply a result of taking seriously the mathematics of our most accurate and reliable scientific theory. While this paper is not a defense of the ontological or metaphysical soundness of Everett *per se*, some discussion is necessary.

2.2.2 Living in a Multiverse

At its most basic, the Everett interpretation is the simplest currently available description of the microscopic dynamics of the world. There is the universal wave function, and there is the Schrödinger equation according to which it evolves. That's it. The difficult is clearly going to be how to translate this into a description and explanation of how we actually experience the world. These kinds of questions have been investigated at some length, particularly by Wallace [10]. The solution lies in the concept of emergence, the idea that there can exist phenomena that are dependent upon but not entirely definable in terms of some underlying dynamics. This phenomenon is not unique to Everettian quantum mechanics. Consider the textbook example: thermodynamics and statistical mechanics. While the thermodynamic properties of a substance (it's temperature, pressure, volume and so on) are ultimately dependent on the underlying motions of its constituent particles, they have important causal relations to the other inhabitants of, to use Sellars' term [11], our manifest image of the world in a way that the particles and motions that constitute them do not. In other words, they serve an explanatory role beyond that which can be realistically provided by a complete description of their microscopic states. Thermodynamic phenomena are thus emergent from their underlying dynamics. Wallace argues that Everettian worlds bear the same sort of relation to their underlying wave function.

The result of this kind of thinking is that worlds are, in a sense, vaguely defined. They do not have sharp temporal borders, which means that questions of the form "When do the worlds split?" do not have well defined answers. However this does not pose a problem in the same way that the inability to answer the question "Where does the collapse occur?" poses a problem in standard quantum mechanics. Wave function collapse is part of the fundamental (stochastic) dynamics of standard quantum mechanics. It is a part of the mathematical framework, and so must be explicable. World splitting does not exist in the same relation to Everett. The mathematics of Everett is complete and well understood. All that remains is to demonstrate that it produces our everyday experience of the world. Vaidman provides a useful heuristic: a split is said to have occurred when the wave function has evolved into "a superposition of macroscopically different states[12,

p. 254].” As stated here, what delineates the macroscopic from the microscopic is not a question to which Everett proposes an answer. However Vaidman’s suggestion and the notion of emergence (which carries with it certain macroscopic connotations) provides a basis for Everett to explain how the world seems to us.

A second, more bluntly philosophical objection to Everett stems from Occam’s razor: our ontology is needlessly extravagant. While this is never going to be a death sentence for any otherwise satisfactory theory, it is still worth considering. As noted, the Everettian multiverse is going to be expansive almost beyond comprehension and constantly growing. There is a sentiment that scientific theories should aim to explain only what we observe, and positing the existence of anything beyond what we observe should count against the validity of the theory. This is an accusation that has been leveled against the infinite multiverse of cosmology. This objection is maybe not misplaced - perhaps it is preferable that our theory stop at what we can observe - yet at the same time it does not seem strong enough grounds on which to refute a theory. Furthermore, remember that economising our ontology by, say, adopting GRW would mean sacrificing some of the simplicity of our mathematical framework. Whether simplicity in ontology or mathematics is preferable is endlessly debatable.

A third general concern is the empirical testability of Everett. Simply put, Everett makes no predictions beyond those of standard quantum mechanics, and so is not falsifiable. There are two ways to respond to this. Firstly, as Wallace [10, p. 103] notes, Everett is, save the omission of the collapse postulate, standard quantum mechanics. So almost a century’s worth of empirical evidence for Everett is already available. This does not seem quite enough, and is unlikely to prove convincing to even the mildest Everett skeptic. Secondly, there are suggestions [12, 13] that Everett does make unique empirical predictions. However the proposed test involves, among other things, “a “time reversal” Hamiltonian which could “undo” [a quantum] measurement [12, p. 257].” That being the case, this is probably one for the future. However it is not quite true that Everett makes exactly the same predictions as standard quantum mechanics. In particular, Everett categorically states that wave function collapse never occurs. Any evidence of a collapse (the kind of evidence which is currently being investigated in relation with spontaneous collapse theories [5]) will therefore disprove Everett. Along these lines,

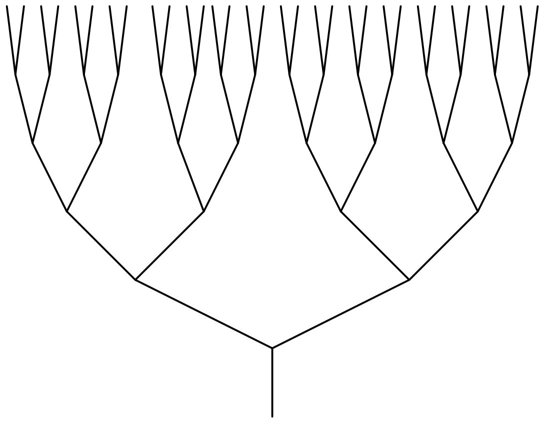

Figure 2.1: An Everettian branching structure

Wallace points out that evidence of superposition of larger microscopic objects - he cites the observed diffusion of buckyballs - is evidence to the positive for Everett [10, p. 103]. Whether this should count as evidence for Everett depends heavily on one's views on more general questions in the philosophy of science. However the broad claim that Everett is fundamentally untestable is not justified.

The above objections to Everett (and more) have been widely discussed, and while doubters still remain, I believe convincing answers have been provided. A far more difficult problem remains, which, if left unanswered, threatens to derail the entire Everettian project.

2.3 A Primer on Probability

The rest of this paper concerns attempts to make sense of the probabilistic predictions of quantum mechanics within the framework of the Everett interpretation. As such, it is essential that we have some understanding of the debate concerning the interpretaTion of probability. The nature of probability is surprisingly difficult to grasp in its entirety, and the correct way to understand it is much disputed.

We might like to think of probabilities as existing objectively and representing some essential fact about how the world works. It is true that our beliefs about probability ultimately boil down to the observation that when a 'chancy' event, like the roll of a die, occurs we observe different outcomes with different frequencies. It is also true that as more chancy events occur, we believe the relative frequency of outcomes approaches the genuine probability for each outcome. For instance, rolling a fair die 6 times could realistically yield a lopsided distribution: three sixes and no ones. If we roll the same die 1,000 times, the relative frequency of each outcome approach $\frac{1}{6}$. We might identify this long-run frequency as the probability, adopting a **frequentist** approach. Yet there is no guarantee that the frequencies approach the correct probabilities, even after 1,000,000 events. To solve this, we might take the probability to be the hypothetical relative frequency as the number of events goes to infinite. This too has problems, as in reality events can only occur a finite number of terms. In addition, it seems we can't say anything about the probability of events that have not yet occurred, or can only occur once. There is also something circular in the logic here: if the probability just *is* the relative frequency of an outcome, what does it mean to say the frequency approaches the probability as the number of events increases?

A second suggestion, widely credited to Popper, is that we think of probability as a tendency or **propensity** that an event will occur. This is another form of objective probability. We then take the frequency of outcomes as evidence of the underlying propensity. This is, I think, the naive conception of probability: some underlying mechanism which tips events this way or that with a certain likelihood. Certainly, this resolves the single-case problem. The problem with propensity accounts is largely metaphysical. What is the nature of the mechanism that induces probability, and what are their causal relations to the events they instantiate? If propensities are to be understood as something like laws of nature, they are completely unlike what we would normally think of as laws of nature. Gravitation, electromagnetism and others physical laws are characterised, if not defined, by their perfect regularity. It is difficult to see how law-like propensities which do not exhibit such regularity could fit into our picture of the world.

Third, we might endorse **classical** probability, whereby we assign equal probability to all outcomes that are equally possible. The six faces of the die are considered equally possible outcomes, so are each assigned probability of $\frac{1}{6}$. The probability here is an epistemic, it represents what you know about the system under discussion. The central tenet of classical probability is the principle of indifference, according to which equal probability is assigned to outcomes. This principle has been objected to on many grounds, a particularly convincing one being the box factory argument by van Fraaseen. A factory produces cubes with side length randomly distributed between zero and one meter. Applying the principle of indifference, we would attach probability $\frac{1}{2}$ to the proposition that the next box produced will have a side length of half a meter. Yet the situation could also be presented as follows: the factory produces cubes with face are between 0 and 1 square meter. In this case, the principle suggests we assign probability $\frac{1}{4}$ to the equivalent proposition (the next cube produced will have face area 0.25 square meters). The principle produces incompatible probability assignments. While classical probability is not defended here, it is interesting to think that decoherence might yield a solution to this problem.

In respect to Everett, it is clear that classical probability will not work (see Section on the branch counting approach).

The fourth approach, and the one that will be incorporated into the account here, is **subjective** probability, where the probability is defined as the degree of belief of a rational agent in that outcome. An agent's degree of belief must then act as a guide to action - it must inform him how he should, say, wager on an outcome obtaining. For instance it is reasonable to think that a rational agent should be willing to buy (or sell) of 50 cents on the promise of receiving \$1 if it lands heads. This is then his degree of belief.

2.4 Probability and Everett

The greatest threat to Everett as a complete account of quantum mechanics is the question of how to deal with probability within its framework. This does not stem any *prima facie* requirement that a theory of quantum mechanics be fundamentally probabilistic as is the case in Copenhagen or GRW. It could be argued that, purely on grounds of taste, the opposite is desirable - Einstein's famous remark about God and dice comes to mind. Instead the issue is, firstly, that we must introduce probability as a guide to action. Probabilistic phenomenon form an important part of our manifest image of the world. Our everyday experiences tell us that in situations where two or more mutually incompatible outcomes can occur, only one of these outcomes is actualised. This seems to be in direct contradiction to the predictions of Everett. Thus we must consider how to introduce the notion of probability to inform inhabitants of an Everettian multiverse how they should behave in respect to the future. Secondly, we must be able to make sense of the probabilistic predictions of quantum mechanics. The evidence for quantum mechanics is statistical - it deals with relative frequencies of outcomes. We must develop some way to incorporate this into our worldview. These issues are presented here, first fairly imporessionistically and then more technically.

2.4.1 Surprise in an Everettian world

Consider the following scenario, as described in standard quantum mechanical language. A particle is prepared in a superposition of spin states, such that the the probability, given by the Born rule, of the outcome 'spin-up' obtaining is close, but not equal, to 1 and the probability of 'spin-down' obtaining is some minuscule, but non-zero, number. Let 'spin-down' occur once out of every 10,000 measurements. On the standard way of thinking (or indeed according to a number of alternative interpretations) the situation is easily understood. Our observer, Adam, should be surprised if he observes spin-down. It is objectively unlikely. More than this, he can quantify exactly how surprised he should be - he can then use his level of surprise as a guide to action, as a guide on how to place bets, and so on. Adam's quantification is easily justified, either by reference to the fundamental chanciness of the dynamics (GRW) or as a function of some probability distribution (Bohm). In any case, there is a straightforward way for Adam to understand the probabilistic predictions of the theory, and to use those predictions to inform his actions.

Now consider the case under Everett. The wave function evolves into two macroscopically different states, decoheres and produces two branches: one in which 'spin-up' obtains and one in which 'spin-down' obtains. Both outcomes occur with certainty, so should Adam be surprised when he observes 'spin-down'? The answer, worryingly, seems to be no. Even before the measurement is performed, Adam can be sure that both outcomes will occur and he can be sure that there will be a descendant of his there to witness each outcome. There is a *Down-Adam* and an *Up-Adam*, both of whom are qualitatively (with the exception of which outcome they observe) identical and both of whom share exactly the same psychological continuity with Adam before the measurement. There seems to be no way for Adam to justifiably expect to become one rather than the other, no matter have 'unlikely' spin-down might be.

The situation is not improved if we alter the number of outcomes. Say a measurement has 1,000 possible outcomes and only one of these outcomes results in a light flashing on a measuring device. Again, Adam can not be surprised if he turns out to be the unlucky Adam-descendant that sees the flash. Even though that he is aware that there are 999 other Adam-descendants that did not see the flash, the question "What are the chances that I turned out to be *this* one?" is meaningless, even incomprehensible. It is guaranteed by the theory that he becomes *that* Adam and every other Adam!

Thinking about how Adam should behave in this kind of world leads to all sorts of non-intuitive conclusions. For instance, if Adam is offered Born rule appropriate odds (say 10,000 to 1) on 'spin-down' obtaining, is he rationally justified in staking his life savings on the bet, safe in the knowledge that *some* Adam will experience this outcome and reap the vast rewards? More dramatically, Lewis offers the following scenario: "if I point a gun at my head and pull the trigger, it looks like Everett's theory entails that I am certain to survive and that I am certain to die. This is at least worrying, and perhaps rationally disabling [14, p. 2]." This point is worth re-stating. For a believer in Everettian quantum mechanics, there is nothing about the future about which he can be uncertain. He knows that every outcome that can occur, will occur. He also knows that he will be there to observe every single outcome, no matter how tiny its wave function amplitude. The deterministic, unitary evolution of the universal wave function makes it such that making decisions about the future seems impossible.

2.4.2 Theory confirmation

Worries like that given be Lewis above may strike some as far-fetched, but they point to a critical deficiency in the Everettian account. Kent captures this well.

> Everettian quantum theory is essentially useless, as a scientific theory, unless it can explain the data that confirm the validity of Copenhagen quantum theory within its domain - unless, for example, it can explain why we should expect to observe the Born rule to have been very well confirmed statistically. Evidently, Everettian's cannot give an explanation that says that all observers in the multiverse will observe confirmation of the Born rule, or that very probably all observers will observe confirmation of the Born rule. On the contrary, many observers in an Everettian multiverse will definitely observe convincing disconfirmation of the Born rule. [15, p. 325]

The evidence for quantum mechanics - the only reason for taking seriously the puzzling physical implications of the theory - is ultimately statistical in nature. The success of the theory is determined by its ability to predict the long run relative frequencies of outcomes. Whether or not we choose to adopt the Born rule as part of our mathematical understanding of the world comes down to whether or not its probabilistic predictions are replicated as relative long-run frequencies. The problem with Everett is that there is no guarantee that all observers will observe the correct frequencies.

Consider the branching structure in Figure 2.1. Tracing the structure upwards simulates performing repeated measurement of, for example, an EPR state. If left-branches are taken to be spin-up and right-branches spin-down, we can see that there are observers, represented by the leftmost and rightmost lines, who observe a (potentially infinite) series of the same result. This is not a problem in and of itself: we can conceive of a coin landing heads X number of times in a row. Yet, with the coin we can justifiably say that such an outcome is unlikely, and, *modulo* certain assumptions, quantify the likelihood of this string of outcomes. We can even take it as evidence that our theory - *this coin is fair* - is false. Not so in the Everettian case. As we have seen, these 'abnormal' trajectories, which deviate so drastically from the Born rule predictions, are guaranteed by the theory. The consequence of all this is that, if we believe in Everett, no evidence - no matter how radically it diverges from the predictions of quantum mechanics - can be used to disconfirm the theory. Observing a million spin-up outcomes in a row is perfectly in keeping with the theory that spin-up should occur only half of the time. While the importance of probability as a guide to action and a source of surprise is indisputable, it is ultimately the question of confirmation which needs to be addressed. If left unanswered, it points to a fundamental empirical inadequacy in Everettian quantum mechanics as a scientific theory which threatens to undermine the entire project. What we need from an account of probability in Everett is a mechanism by which an observer on one of these abnormal trajectories is able to recognise it as such.

2.4.3 Attempted Solutions

On a superficial level, what we need from our theory of probability within Everett is some way to match the Born rule predictions to the 'branch-weighting.' Branch-weighting is easily understood as an intuitive matter but much trickier to define in any precise way. What we want, loosely, is to able to think of the branch-weighting of a world as equal to the propensity of an observer to end up in that branch. This definition is illustratively useful, but should not be taken literally. Attempting to construct a genuine propensity based probability within Everett is a challenge, as coming examples will show.

Several solutions have been proposed. First: **naive branch-counting**. Taking cues from the classical probability of LaPlace, treat all outcomes (branches) as equally possible. Then simply count the number of branches in which outcome A occurs, divide by the total number of branches and you have the probability of outcome A. While this might work in the simplest of cases, e.g. the EPR state considered above, it clearly fails in situations where the branches are not equally weighted. Putting aside the worry that the number of

A second proposal is the **many-minds interpretation** of Albert and Loewer [16]. They suggest a method for re-introducing objective probability into quantum mechanics. Each observer is thought to have associated with it an infinite multitude of minds. While the physical inhabitants of the world - the observer's body, measuring devices - evolve deterministically according to the Schrödinger equation, minds evolve stochastically according to a postulate which replaces the collapse postulate of standard quantum mechanics. The postulate says that the probability that a mind ends up in branch A is equal to the Born rule probability of outcome A, and so on for all other outcomes. Thus the appropriate fraction of minds ends up in each branch. Given the infinite of minds associated with each observer, there is no risk of them becoming *diluted* over the course of repeated observations. Similar proposals have been made that do not invoke infinite sets of minds [17]. While it can be argued that many-minds theories, if taken seriously, do present solutions to the problems posed here [18, 19], there is a serious question about whether or not they should be taken seriously. Firstly, the explicit dualism of these kind of theories is justification enough for many to rule them out as serious contenders. Secondly, the inclusion in the formalism of quantum mechanics of a postulate regarding the probabilistic evolution of minds seems profoundly unscientific. The addition of such a postulate is not of itself fatal to the argument, but it does undermine some of the original motivation for Everett. As such, these kinds of theories have largely fallen out of favour.

The most widely supported and throughly investigated proposal - the **decision theoretic approach** - originates with Deustch [20], and has been greatly expanded upon by Wallace [10] and others. The argument is that the probabilistic predictions of standard quantum mechanics (specifically the Born rule) can be reconstituted by employing the axioms of classical decision theory. As Deustch puts it: "the probabilistic terminology of quantum theory is justifiable . . . provided that one understands it as referring ultimately to the behaviour of rational decision makers [20, p. 14]."[2] Thus the standard collapse postulate is replaced with a behaviour principle, dictating how observers should bet on measurement outcomes. This embracing of subjective probability has shaped the debate on the topic, and if any account is going to be successful it is likely that it will take a similar approach.

Tied into the decision theoretic program is the investigation by Saunders [21] and Wallace [22] into the metaphysics of branch-splitting and identity. Central to the argument is the assertion that an inhabitant of an Everettian world, faced with the knowledge that he will undergo branch-splitting in the near future, can have only one of the following three expectations:

[2] The account detailed in the body of this paper is similar to the decision theoretic in the that it incorporates the behaviour of rational decision makers. However the foundations and methods of each are entirely distinct.

1. I should expect abnormality: some experience which is unlike normal human experience (for instance, I might expect somehow to become both future selves).
2. I should expect to become one or the other future self.
3. I should expect nothing: that is, oblivion.

[23, p. 11]

Option 3 is rejected as absurd, while 1 is taken to be decidedly non-physical, flying in the face of contemporary materialist accounts of the mind. Thus 2 is the most reasonable outcome, and provides the uncertainty upon which our decision theoretic axioms can function. This expectation incorporates the Lewisian conception of personal identity, such the pre-fission there are said to be two observers: one who will experience outcome A, and one who will experience outcome B. There is then room in the theory for an observer to be subjectively uncertain about which observer he is at any given moment. There are numerous objections to this kind of position, which is often characterised as a *semantic argument*. Chief among them is the assertion that option 2 is simply not a possibility in the Everettian description of the world. Greaves [24] argues that option 1 is the correct expectation to have, but has been unfairly characterised. Her argument is that whatever the observer *knows* he will see, he should *expect* to see. Since he knows he will see all outcomes, he should expect to see all outcomes. Option 1 is therefore the correct expectation to have. It is difficult to come down on one side or the other on this point: both views can be convincingly argued despite their incompatibility.

A second objection, given by Lewis [14], is that what is uncovered in the semantic argument is indeterminacy, not uncertainty. Here we must delve deeper into the Lewisian fission picture of identity. Pre-fission, the observer is said to be multiply-occupied, consisting of a number of people who overlap over a certain spatio-temporal slice. The common analogy is something like two divergent roads which combine over a certain stretch. According to the subjective uncertainty proposed by Saunders and Wallace, it is possible to be genuinely uncertain about which of the two roads you are on prior to their splitting. Say the road forks and heads in opposite directions towards New York and Chicago. Asking 'Am I on the road to New York or Chicago?' has no determinate answer before the road splits (except, maybe, 'Yes'). Lewis agrees that there is indeterminacy present here, but that this does not produce any uncertainty: "I cannot reasonably harbor uncertainty as to whether my use of this road [before splitting] refers to the [New York road] or to the [Chicago road], since my demonstrative indicates certain physical features of the world [before splitting], and those are common to the [New York road] and the [Chicago road] [14, p. 5]." The debate on this issue is an old (and important) one in the philosophy of personal identity, and like many such debates there is no uniquely convincing argument on either side. This brings us to a greater problem with the semantic argument.

As argued by Lewis and others, we should be worried that the empirical adequacy of Everettian quantum mechanics - the simple fact of whether or not it can provide a satisfactory explanation of how the world behaves - should rest on "fancy metaphysics of personal identity [14, p. 8]." This is not so much a point to be refuted, but a more general worry that Everett threatens to veer away from actual scientific theory, and become inextricably tied up in some of the less tangible, and perhaps insoluble, questions of analytic metaphysics. For instance, it seems preposterous that the probabilistic predictions of quantum mechanics should depend on how one feels about the statement that there can be more than one person in the same place at the same time. This is not to say the semantic argument is faulty. It can, if one has the correct attitude towards certain metaphysical issues, introduce which resembles uncertainty close enough to make for a workable account. Yet surely an account which works regardless of how one feels about continuous identity, and fission and overlapping persons is infinitely preferable. Such an account is available, and will be discussed in the following section.

Chapter 3

Vaidman Uncertainty

Some lessons can be learned from the previous discussion of attempts to introduce probability into Everett. Firstly, any objective, irreducible probability - probability that is part of the way the worlds works at the most basic level - risks over-complicating our ontology as in the Many Minds proposal. Subjective probability is surely the way to go. Secondly, our account should have as little dependence as possible on our personal choice of metaphysics. As suggested by previous discussion of the semantic argument suggested, it should work regardless of how we understand personal identity or whether we find certain expected outcomes physically unintelligible. Any uncertainty we uncover in the theory should be generic, and as close to indisputable as possible. An analogy that might be useful is to that of Descartes' project in his *Meditations*. We want a bedrock of uncertainty which, like his *cogito*, cannot be avoided or argued around. Such a foundation is found in the after-the-fact uncertainty proposal of Vaidman.

3.1 Vaidman's Proposal

Vaidman's proposal to resolve the probability issue is elegant and easily illustrated [12, 25, 26]. Consider a particle prepared in the following state:

$$\Psi = \frac{1}{\sqrt{3}}\left[\, |1\rangle + |0\rangle + |-1\rangle \,\right]$$

In the standard picture, we would say that each outcome has a Born rule probability of $\frac{1}{3}$ of occurring. As has been established, the Born rule is not so easily justified in Everettian quantum mechanics, since each outcome will occur with probability 1. To this end, Vaidman offers the following thought experiment. Suppose an observer, Adam, is presented with a device capable of measuring the spin of the particle. He is told that, before the state of the particle is measured, he will be given a sleeping pill, and based on the result of the measurement his unconscious body will be transported to one of three identical rooms: Room 1, Room 0 or Room -1. Upon waking, it would seem that Adam is in the following condition. As a believer in the Everett interpretation he will be certain that all three outcomes have occurred, and that there is an Adam in each room. At the same time, he will be genuinely uncertain about which room he is in, about which branch he is in and thus about which outcome has occurred *for him*. To put it another way, Adam will be genuinely uncertain about which of the three post-measurement Adams he is.[1]

If this argument withstands scrutiny, it goes a long way to answering the criticisms of anti-Everettians. Contrary to the popular assertion that there is nothing undecided about the world in Everettian quantum mechanics, Vaidman seems to have discovered just such a fact: the observer can be uncertain about his branch location. Returning to the Descartes analogy is useful here. The period of time, after the measurement is performed but before Adam is offered additional information as to his location, is our equivelant of the *cogito*. If it can be established that this period is a generic feature of Everett, it gives us a base of genuine uncertainty from which to build. Call this period the *Vaidman interval*. So, like Decsartes' project, Vaidman's task if two-fold. First, establish that the Vaidman interval, like the *cogito* is genuinely unavoidable. Secondly, we must see if we can build upon the Vaidman interval, and expand it into a coherent account of probability within Everett. As Descartes found, the greater difficult lay in building from his *cogito* any meaningful body of knowledge. The same will prove to be true of Vaidman's goals. However first we must examine just how robust the Vaidman interval is.

[1]This kind of language risks becoming ambiguous. To be as clear as possible, if any of the three post-measurement Adams utters the phrase "I do not know which of the three rooms I am in" that statement is universally true.

3.2 Is the Vaidman interval avoidable?

As noted, a question that is clearly going to be central to whether or not an account based on Vaidman's approach is successful is the following. Is the period of post-measurement uncertainty generic? That is, does it inevitably and without exception follow measurement-type events? If it were possible to construct a scenario in which the post-measurement uncertainty did not occur, this might be sufficient to fatally undermine the entire project. There are several possible objections that need to be addressed.

Firstly, there is the suggestion that the post-measurement interval, as described in Vaidman's thought experiment, is somehow unnatural or illegitimate. It could conceivably be argued that it arises as an artifact of how the thought experiment is constructed, rather than emerging naturally from some underlying, fundamental fact about the measurement process. This line of thought is suggested by Albert: "the uncertainty we need the uncertainty that quantum mechanics imposes on us is something not to be bypassed, something that comes up whether or not we go out of our way to keep ourselves in the dark about anything." [27, p. 367] This is a reasonable worry. After all, measurements of quantum states rarely involve sleeping pills or identical rooms. However this is quite easily dealt with. Vaidman offers that "the sleeping pill is hardly necessary" [12, p. 254]. We can go further than this: the sleeping pill is entirely unnecessary. It does nothing to generate the post-measurement interval, it merely serves to extend it and make it absolutely clear that the interval does in fact occur. Eliminating it from the thought experiment changes nothing, and in fact makes the scenario far more believable.

To demonstrate this, consider a Stern-Gehrlach device configured such that the result of a spin measurement along a certain axis causes a display to print either $\Uparrow$ or $\Downarrow$. An observer watches the display. Admittedly, the precise chronology of the sequence of events described here is somewhat ill-defined: it is difficult to pin down exactly when each event occurs. However this is not a pressing issue, and there is a sense in which such a question is ill-posed. The vagueness of the actual world-splitting is explained by the emergent nature of the branches, and is a feature of contemporary Everettian accounts. What is, I argue, well defined is the order of events. First, the spin of the particle is measured. Then the display reads either $\Uparrow$ or $\Downarrow$. Finally, a stream of photons bounces off the display and enters the eye of the observer.

The first interval, between measurement and display, is conceptually more difficult and will be dealt with in the next section. The second is straightforward and can be well defined. First, we must accept that it is in the nature of observers that they require some intermediate macroscopic stage - a measurement device - in order to gain access to the results of measurements of quantum states. As a practical matter, where observer is taken to mean 'person', this is an uncontroversial point. Whether more can be said on this point is, again, discussed in the following section. Restricting our purview to people as observers, it is clear that there will always be a finite, non-zero interval between when the display registers its output and when the reflected photons enter the observer's eye. During this interval it is indisputable that the observer can have no information about the outcome of the experiment, and thus no knowledge of which branch he is in. Carroll and Sebens consider a similar case, drawing the same conclusion.

> The timescale for decoherence for a macroscopic apparatus is extremely short, generally much less than 10^{20} sec. Even if we imagine an experimenter looking directly at a quantum system, the state of the experimenters eyeballs would decohere that quickly. The timescale over which human perception occurs, however, is tens of milliseconds or longer. Even the most agile experimenter will experience some period of self-locating uncertainty ... [28, p. 5]

The attempt to quantify the interval in terms of decoherence here is useful, and adds to the notion that the interval is unavoidable. Even if it were possible to eliminate the traditional measuring device and thus the measuring device-observer interval, there is still the time it takes for impulses to travel from the eye to the brain and so on. This establishes comprehensively that decoerence will always occur in a short enough time to enforce the uncertainty interval.

The inverse objection can also be raised: what if the interval it too short? Returning to Vaidman's thought experiment, we note that the interval described above can be magnified to arbitrarily large time-scales, whether through sleeping pills or some other mechanism. Yet if no such mechanism is present, the interval may be incredibly brief. To make obvious the objection, it may be short relative to some biological process - perhaps the firing of a series of neurons - that might be considered the minimum time frame in which an observer can be said to be aware. That is to say, the entire measurement-display-recognition process might theoretically occur before the observer could even become aware that he is uncertain about the outcome. If we require that the observer be aware of his uncertainty in order to establish the Vaidman interval, the argument might fall down here. Ultimately, I don't think awareness of uncertainty is going to be required on our scheme. It is employed in Vaidman's experiment for purely illustrative purposes, and is perhaps misleading. What we require of the post-measurement interval is that it is a finite period of time in which the observer *cannot* be made aware of his location, not one in which he *is* not aware of his location.The case in which he is aware of his uncertainty is then representative of a sufficient, but not necessary, condition for the post-measurement interval. Carroll and Sebens draw the same conclusion, noting that "although the experimenter may not be quick-thinking enough to reason during this period, there are facts about what probabilities they ought to assign before they get the measurement data" [28, p. 5]. While I consider the preceding argument sound, I concede that this point is debatable. For instance, do we really want to say that the observer in Vaidman's sleeping pill experiment is uncertain about his location *before* he wakes up? Ultimately I think this is an issue of semantics, but if one did require awareness as a precondition for uncertainty, more investigation would be required. SO far we have established that in general an observer, where observer ostensibly means person, will experience a Vaidman interval.

The second objection concerns cases where the definition of observer is broadened. Consider Vaidman's assertion that "in a typical quantum experiment, splitting of worlds (creation of superposition of macroscopically different wave packets of macroscopic systems) happens before the time the experimentalist . . . will become aware of the outcome" [26]. What he has in mind here is a case similar to that described above - a human experimentalist interacting with a macroscopic measuring device. As has been argued, it is physically impossible for such an observer to circumvent the interval of Vaidman uncertainty. But what if the intermediate stage - the measurement device - could be eliminated?

Imagine some advanced computer, able to directly measure the quantum state of some particle and then record that state onto some corresponding memory particle within itself. This proposal is along the lines of the quantum mechanical automaton proposed by Albert [29]. The idea here is that the computer is in a sense 'plugged in' to the quantum state, and so able to skirt the intermediate measuring device stage. Ignoring the technical plausibility of this scenario, we can ask whether or not the computer experiences an interval of uncertainty. The answer seems to be no. If this scenario is played out, there does not seem to be any point at which the computer can be said to be uncertain about the outcome of the experiment, and thus about its branch-location. It could be argued that if the interval can be circumvented in *any* case it cannot form the basis of probability. Does this theoretical machine constitute a compelling counter-example? Under the scheme proposed here, the answer is no.

As we know, the underlying dynamics of the Everett interpretation is entirely deterministic: there is nothing probabilistic in the objective description of how the world evolves. Instead, probability is to be understood as a subjective degree of belief or a credence. It is important to recognise that credence as probability only makes sense if there exists some credence-haver with the capacity to experience events in the requisite, subjective manner. I argue that the computer described above does not have this capacity: for him there simply is no concept of probability.

A classical analogy is useful here. Consider a highly sophisticated coin flipping robot, able to instantly and accurately measure the weight of a coin, the force with which it flips the coin, the ambient conditions and any other relevant factors. The robot is then able to predict, with perfect accuracy, the outcomes of flips. I contend that this robot not only *does not* but *cannot* have any conception of probability. The same is true in the Everettian case. To put a finer point on it, neither the computer in the quantum case, nor the robot in the classical case constitute observers in their respective scenarios. This ties in to the ultimate metaphysical structure proposed here: a fundamentally deterministic mechanics, on top of which sits purely subjective, observer-dependent probability. The probability here is to be thought of as something like an emergent phenomenon, only coming into play once a certain level of subjectivity is present. Admittedly, the notion of levels of subjective awareness is difficult to pin down, and even more difficult to quantise. However it seems possible on an *ad hoc* basis to distinguish between the two. As described, the computer and robot exist below the level of probability, while a typical observer surely exists above it. Vaidman himself does not address this two-level approach to probability, but none of what has been laid out here is in contradiction with his writings.

A more detailed analysis of the level at which probability comes into play on this account, sadly, lies far beyond the scope of this paper. From the above description it is clear that the capacity to experience probability is very closely correlated with consciousness, or something like it. Yet, it might come to pass that the computer and the robot obtain some level of consciousness as a function of their complexity. Would it be possible for a conscious being to experience the world non-probabilistically? By the same token, how far down the ladder of organism complexity do we have to go before some living being can be said to experience the world without probability? While these are difficult, and interesting, questions, the task here is only to establish that observers in the traditional sense could have an understanding of probability within Everett. This has, I think, been accomplished. Secondly, the proposal outlined here may strike some as perilously similar to some of the more outlandish consciousness-driven collapse theories of Wigner [30] and others. Wigner, for example, believed that there was something in the nature of consciousness that caused wave function collapse. The resemblance between the two cases is only superficial: while consciousness clearly plays a key role in our account of probability, the consciousness of the observer, as employed here, has no impact on the physical dynamics of the world. It is simply the case that consciousness and probability emerge congruently. In any case, we certainly don't need to endorse any kind of dualist view in order to view probability in this way.

The intent here was to demonstrate that the post measurement uncertainty interval is generic: any observer performing a measurement on a quantum state will experience it. Firstly, it was shown that for any typical observer, there will always exist a Vaidman interval, even if only a brief one. Secondly, anything which could theoretically bypass the interval would not satisfy our criteria of what it is to be an observer. The universality of the post-measurement interval is crucial to the proposal here,so establishing these two points is of vital importance. Like Descartes' *cogito*, the Vaidman interval is genuinely unavoidable. The next task is to establish what we can convincingly build upon this foundation.

3.3 Is the uncertainty too late?

The previous section establishes that there is generically a non-zero post-measurement interval during which an observer is genuinely uncertain about the outcome of a measurement. So, it seems we have succeeded in locating some uncertainty within the Everett interpretation. Is this the kind of uncertainty that allows us to make sense of probability? It is immediately not clear that this is the case. The primary concern is that Vaidman uncertainty only arises after the measurement has been performed. As Albert [27, p. 368] argues, if we wish to use uncertainty based probability as a pre-measurement guide to action, the uncertainty must be present before the measurement is performed. Our task is then to push the uncertainty we have discovered back to before the measurement is performed. To make the situation as clear as possible, what we have so far established is that post-branching observers are genuinely uncertain about their location within the multiverse. They are then able to make bets about what *has* happened along their branch. What remains to be seen is whether this can be translated into uncertainty about what *will* happen along their branch.

A first suggestion: we concede that there is nothing about the future about which an observer is uncertain. However, included in his beliefs about the future is the knowledge that he (that is, all of his descendants) will definitely be momentarily uncertain about his location post-measurement. It is not the case that the observer has forgotten anything over the course of the experiment. We are to imagine that there is a continuous link between his mental state before and after the measurement. It follows that if he is genuinely uncertain after the measurement, he must have been genuinely uncertain before the measurement. The observer is then justified in using subjective probabilities as a guide to action.

Albert rejects this suggestion, arguing that the uncertainty present here is not the kind of thing that can be traced back through psychological continuity. Instead the uncertainty is explained by, he claims, the coming into existence of new subjective entities (the observer's descendants).

> The questions to which that observer's descendants do not have answers are questions which can only be raised in indexical language, and only from perspectives which are not yet in the world at all before the measurement has been carried out. Completely new uncertainties do indeed come into being, on this scenario, once the measurement is done. But those uncertainties have nothing whatever to do with objective metaphysical features of the world, and they are not the sorts of uncertainties that can only arise by means of forgetting. [27, p. 368]

Let's consider the first point: the questions about which the observer can be uncertain can only be raised in indexical language, post-measurement. Indexical language is, loosely, context-dependent: words like 'I' or 'here' whose meaning is dependent upon the place or time at which they are uttered. Ismael provides a good analysis of the issue of indexicality in relation to Everett [31]. It can be distilled to the observation that there is no way to refer to the post-measurements observers without reference to the outcome they observe. For example, we may only be able to refer to a certain descendant as 'Adam that observes spin-up' and so on. Clearly, any question like 'What is the probability that *Adam that observes spin-up* observes spin-up?' is going to be tautologous. Ismael suggests that this is the byproduct of a "linguistic peculiarity [and that] if worlds had names, or came in different flavors (so that the chocolate world or the grape world were identifying descriptions) there would be no problem about interpreting the Born probabilities [p. 780]." She further clarifies this point, pointing out that "the need for indexicals in identifying the relevant events ... makes for linguistic awkwardness, but doesnt have the ontological consequence ... that would undermine the Everett picture [p. 781]." It is difficult to know what to make of this point. Certainly, if worlds could be identified prior to measurement without reference to the outcomes that obtain within them, the game would be over. We could easily understand probabilistic statements as dealing with propositions like 'spin-up obtains in the grape world.' Yet it is fundamental to the theory that no such identifier is available, and to call this a linguistic peculiarity seems to underplay the issue. If it is a peculiarity, it is a profoundly deep-seated one. I see no way to circumvent talk of indexicals, however I agree with Ismael that it need not undermine the entire project.

Let's concede that uncertainty in our account can only exist in respect to indexical statements. This rules out the psychological continuity argument proposed above. The uncertainty we have located is generated by, and entirely dependent on, the branch-splitting mechanism. It simply does not exist before the measurement is made. Vaidman accepts this point, but is ultimately untroubled by it. He returns to his common refrain: "I do not claim that there is a genuine probability in the MWI. There is only an illusion, and all what I am trying to say is that this illusion behaves exactly as if there was a real concept of probability [26, p. 304]." In order to understand what is meant here by illusion, we must remember what function we need our probability (or illusion thereof) to perform. In the broadest sense, we want it to work as a guide to how we should act in the face of branch-splitting events. More specifically, we want it to tell us how we should bet on the outcomes of measurements: remember the quantum Russian roulette of Lewis. This is ultimately what Vaidman means by the illusion of probability: something in our account that can be acted upon as if it were a genuine probability. I think Vaidman's proposal satisfies this.

The probability we are trying to define here is subjective. In the classical case, subjective probability quantifies the participant's degree of belief that an outcome will obtain. This is not quite the definition we desire here, since we know all outcomes obtain. The key is to introduce something to which the Born rule probabilities can be attached. Vaidman's solution is to attach it to the 'measure of existence' of a world, which quantifies a world's susceptibility to interference [12, p. 255], but can be understood (superficially) as equal to the branch-weighting of that world. We can then define a caring measure on a world, whereby the pre-measurement observer cares more about what happens in branches with greater measures of existence. The justification for this is straightforward: the pre-measurement observer is aware that he will become all of his descendants. As such, it is in his best interests to set his caring measure equal to the measure of existence.

As always, the justification for this lies in the post-measurement Vaidman interval. Returning to Adam and the sleeping pill scenario helps to illustrate this.

In this instance, before Adam is given the sleeping pill and the measurement is performed, he is asked what odds he would accept on a wager that he ends up in Room 1. He is then given an amnesia pill, erasing his memory of the bet, followed by the familiar sleeping pill. Upon waking, he is asked what odds he hopes his predecessor accepted. A naive pre-measurement strategy (and one we certainly hope to avoid) is that Adam should be willing to accept any odds on the bet, given that the outcome is certain and one of his descendants will certainly profit. When presented this way, this strategy seems indefensible. The descendant must surely hope that his predecessor set his odds by according to the square modulus of the relevant part of the wave function. As Vaidman puts it: "at that moment all the descendants will be happy if actions according to the behavior principle have been performed. [26, p. 309]" The suggestion here is the following: a rational post-measurement observer will surely hope that his predecessor *had* attached the same credences to each outcome as he *would* presently assign in his state of genuine uncertainty. Thus the pre-measurement observer acts in the same way as he would if he were genuinely uncertain about which outcome will obtain. This is, I think, sufficient justification for utilising the Born rule probabilities.

An important objection to the caring measure originates with Albert [27, p. 361]. His objection is addressed primarily to the decision theoretic approach of Deustch, Wallace and Saunders, but is worth considering in respect to the proposal here. He suggests that acting according to the caring measure may not be the "uniquely rational way of operating" [p. 362] in an Everettian multiverse. He suggests an alternate measure, the fatness measure, to demonstrate this. The fatness measure stipulates that "the degree to which I care about what transpires on a certain branch, then, be proportional to how fat I am on that branch multiplied by the absolute square of the amplitude associated with that branch." While there are suggestions that the fatness measure is illegitimate [22, p. 191] there does not seem to be anything obviously irrational or incoherent about adopting it. This could prove fatal to our argument if unique rationality were a strong requirement in our account. I do not believe this is the case with Vaidman's proposal. All that needs to be demonstrated is that it follows from the existence of the Vaidman interval that there *is* a way to set the observer's credences equal to the squared modulus of the wave function, not that this is the only way they may be set. This may strike some as an unacceptably weak foundation, but it is important to keep in mind the kind of probability we are talking about - subjective, emergent probability. There is nothing ontological at stake here, so this slightly loosened rationality requirement is perhaps more palatable than it might otherwise be. The arguments of this section have made a coherent account of how the Vaidman interval can be, in a sense, extrapolated backwards such that an agent is justified in equating wave function moduli to probabilities, and setting his expectations accordingly. However we are not committed to saying that their is anything about the future about which the observer is uncertain. This is, still, inarguably false. It is simply the case that the facts about how the world will be post-measurement give him sufficient justification to act as if he is uncertain and utilise the moduli accordingly. However an important question remains, regarding whether or not our account turns out to be founded on a questionably justified assumption.

3.3.1 Typicality and Evidence

Countless writers have noted that talk of betting behaviour in respect to Everett is ultimately a red herring even if it is a useful, highly illustrative one. The real question is whether or not Everett can provide an intelligible explanation of the statistical evidence (relative frequencies of measurement outcomes) for quantum mechanics. Albert puts this worry well.

> It is those frequencies, or at any rate the appearance of those frequencies, and not the betting behaviors to which they ultimately give rise, which primarily and in the first instance stand in need of a scientific explanation. And the thought that one might be able to get away without explaining those frequencies or their appearances, the thought that one might be able to make some sort of an end run around explaining those frequencies or their appearances . . . is, when you think about it, mad. [27, p. 360]

This is the correct attitude to adopt, and is the issue on which the current proposal faces the greatest difficulty. There is a pervasive and, as far as I can see, insufficiently addressed problem with the arguments for empirical support of Everett. Consider the logic of the Vaidman argument discussed here. The arguments presented so far suggest that it is generically true that, after measurement but before observation, Everettian descendants are uncertain about their branch-location. We can even argue that this uncertainty is enough to constrain the betting behaviour of the pre-measurement observer such that they act in accordance with the predictions of the Born rule. This betting behaviour rationale even gives us plausible grounds on which to take the observation of outcome frequencies as evidence for or against the truth of quantum mechanics, as we now have a meaningful way to describe certain outcomes, or strings of outcomes, as comparatively likely or unlikely. There, are of course, the previously mentioned objections which must be taken under consideration. Nonetheless, we do have, in principle, a complete proposal for understanding probability within Everett. However, there is an implicit assumption which has slipped under the radar in many discussions.

Consider a typical Everettian branching structure. For simplicity's sake, take each branch to represent a state with a Born rule coefficient of $\frac{1}{\sqrt{2}}$, i.e. a 50/50 coin flip. The mathematics of decoherence tell us that splitting events take place constantly and rapidly. As such, imagine the branching structure extrapolated such that if we freeze it at a moment in time - picture a horizontal slice across - the number of 'end-points' will be potentially be enormous. The observer at each of these end-points will have observed a unique sequence of UPs and DOWNs. It is easily demonstrated that, as time passes and the number of branches increases, the proportion of all observers who have recorded approximately a 50/50 split of UPs and DOWNs will increase. At the same time, the proportion of all observers who have followed abnormal trajectories and recorded abnormal frequencies will shrink. There is only one observer who has recorded all UPs after 10 splits. The same is true after 100 splits, after 1,000 splits and so on. Intuitively, those end-points clustered around the centre of the branching structure observe (roughly) the 'correct' frequencies. We might then describe those trajectories whose end points lie close to the centre 'typical': observers who follow these trajectories will have, roughly, the correct experience of the world. This idea is hinted at by Saunders:

> . . . what I'm discovering by doing experiments and so forth is what happens in my world. Thats what I'm discovering. I hope my world is typical, it may not be typical, I may get deviant outcomes; but thats the language in which the amplitudes are exploratory they quantify typicality. [2] [32, p. 393]

Here, 'world' should be understood to mean a trajectory through the branching structure. The notion Saunders brings up - typicality - is more important than his tone suggests. It is surely not enough to simply hope that one's experiences lie along a typical trajectory. After all, the sole reason that we pay any attention to the predictions of the Born rule is because it reflects the actual frequency of outcomes that have occurred in the history of the world. For instance, it is presumably (and hopefully) true that the relative frequency of UPs and DOWNs in measurements of EPR states in the actual history of this world is approximately 50/50. If this were not the case, we would have reason to abandon the Born rule and develop some alternative mathematical tool which yields the correct observed frequencies. Typicality is then more ingrained the Saunders suggests: it is, I think, implicitly assumed that our initial branching point (the initial split on our diagram) is actually the end-point of a typical trajectory.

Consider the situation if this were not the case. Suppose that, as luck would have it, our origin is actually the end of some wildly divergent trajectory. [3] Our series of recorded outcomes is then completely unlike the 'correct', i.e. typical, split. Our observations are not representative of the multiverse as a whole. Say we have observed 50/50 UPs and DOWNs whereas the typical split is closer to 90/10. I can see no way that we, as the inhabitants of this abnormal trajectory, could ever ascertain that our experiences have been, to date, atypical. Consequently, we would have developed a mathematical framework which predicted a 50/50 split. After all, the only genuine test of theory is whether or not it replicates our recorded observations of the world. Then our theory, despite its historically perfect record of predictive accuracy, is on a wider view incorrect: it does not correlate to the experiences of an observer on a typical trajectory. It seems the only option available is, as Saunders suggests, to operate under the assumption that

[2] This quote is taken from the transcript of an informal discussion, so it is not worth analysing in any great detail. The central idea is, however, unambiguous.

[3] God or some malevolent demon are usually held responsible in these kinds of scenarios.

our trajectory is typical. This is not quite true, as we might instead assume that our trajectory is atypical, but this is counter-productive and equally unwarranted.

Two main problems present themselves here. First, what is the nature of typicality? It is understandable as a heuristic matter - trajectories falling near the centre of our branching structure - but surely something more concrete is necessary. Secondly, if we must assume typicality, how are we justified in doing so: is it to be taken as an *a priori* truth? While Saunders is not forthcoming on this, Everett himself showed some considerable forethought in raising the issue:

> We wish to make quantitative statements about the relative frequencies of the different possible results of observation - which are recorded in the memory for a typical observer state; but to accomplish this we must have a method for selecting a typical element from R superposition of orthogonal states. [33, p. 16]

Everett does propose a solution which ultimately turns out to be unconvincing. He argues that the "the situation here is fully analogous to classical statistical mechanics, where one puts a measure on trajectories of systems in the phase space by placing a measure on the phase space phase itself and then making assertions . . . "which hold for almost all" trajectories. [p. 17]" This analogy does not hold. Consider how we approach phase space in statistical mechanics. Trajectories in phase space correspond to various ways in which the state of the system changes with time. Only one trajectory picks out the actual history of the system, yet there will be many other trajectories which pick out macroscopically identical histories of the system: say an ice cube melting. It is then trivial to formulate the probability that the ice cube will melt and as Everett says, almost all trajectories will follow this course. The assignment of probability is easily justified by the fact that we are typically uncertain about which micro-state of a given macro-state a system is in at any time. We declare that each micro-state is equiprobable and generate our probabilities. Some points of contention have been glossed over here, but the general idea is widely accepted.

Now consider the Everettian analogy, which has been expanded upon by Allori *et al.* They concur with Everett that we should put a measure over Hilbert space and agree that "the most natural measure is indeed the one using Ψ^2 weights. [34, p. 20]" Again, this is circular. It takes for granted that the Ψ^2 branch-weightings (which can only have come about as a result of the recorded frequency of outcomes in the actual history of the world) are themselves typical of other locations in the multiverse! But as has been argued, there is no strong justification for thinking this is the case. The only solution, it seems, is to follow Saunders' suggestion and simply hope that it is the case.

Wallace does address the role of typicality, and suggests a few possible solutions. He references an early suggestion by Geroch, who posited a principle of preclusion to the effect "that it is reasonable to treat certain regions of Hilbert space as precluded i.e. to assume that we are not in such regions because they have very small weight, and that these regions include ones with anomalous statistics. [22, p. 128]" This is essentially the inverse of Saunders' recommendation but, as Wallace notes, is equally unwarranted. It is difficult to imagine how a positive argument for the adoption of a typicality postulate should go, unless it is to be understood as the assertion that we *must* assume typicality in order to function within a multiverse. This kind of argument is not particularly compelling.

Another suggestion is that the dynamics of branching worlds might effectively eliminate these troublesome low-weight branches. This is suggested by Hanson, who argues that the mathematics of decoherence "may allow large worlds to mangle the memory of observers in small worlds. [35, p. 1142]" This would, he claims, allow us to disregard anomalous branches, effectively enforcing an approximate version of Geroch's principle of preclusion. This approach, if it holds up to scrutiny, would be an enormous boost to Everettians. Of course this is a big if: Wallace observes that "Hanson's proposal depends on whether decoherence really delivers the goods, so to speak. The level of noise in the decoherence process is really very small indeed, and to the best of my knowledge neither Hanson nor anyone else has carried out a decoherence-based analysis in a physically realistic situation. [22, p. 130]" In addition we must remember that it is not simply the case that there are one or two anomalous trajectories and the rest are effectively typical. Rather, there is a range of trajectories, whose atypicality increases as we move towards the edge of the branching structure. Whether Hanson's mangling process, which is highly inexact, would eliminate sufficiently many of the atypical trajectories for us to stand behind the principle of preclusion remains to be seen.

A final suggestion in response to the typicality objections laid out here is that Everettians are the victims of an unfair double standard. This response is due to Papineau, who suggests that there is an implicit assumption of typicality in standard, everyday probability speak.

> 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 improbable. Everettians can once more do the same, proceeding similarly on the assumption that our observations are not atypical. [36, p. 214]

At first blush this comes across as something of a Hail Mary - if you consider Everettian probability broken then you must also consider any kind of probability whatsoever broken. However it is worth playing out Papineau's analogy: comparing the multiverse inhabitant who starts out on an anomalous branch to the victim of deceptive statistical evidence. Suppose that there is some unlucky physicist who, throughout the course of his life, has observed measurements of EPR states come out spin-up 90% of the time and spin-down 10% of the time. He has performed a statistically significant number of such measurements, and does not have any evidence to suggest anything other than that 90/10 is the correct long run relative frequency of each outcome. He would surely assign probability 0.9 to the proposition that the next EPR measurement comes out spin-up and probability 0.1 to the proposition that it comes out spin-down. If he then goes on to construct a theory of EPR measurements, its mathematical framework must yield the above probabilities. However, this framework is a result of his poor fortune - the correct theory is one the assigns probability of 0.5 to each outcome. This is the appropriate analogy between the two cases.

So what happens when each participant begin to observe an equal split of spin-up and spin-down? That is, what happens when their observed frequency approaches the correct (in the standard case) or typical (in the Everettian case) frequency? If both are working under a typicality assumption, they will, at least initially, regard the new frequency as erroneous and disregard it. After all, there is nothing in the evidence available to them to suggest that the new frequency is not in fact a deviation from the typical frequency. So it seems that the Everettian is in no worse a position than his non-Everettian counterpart. But there is a clear sense in which their situations are dissimilar. Insofar as the non-Everettians objective probabilities are taken to be a fact about how the world is, then it is a fact about how the world is that his predicament - being continually misled by chancy outcomes - is objectively unlikely. We can conceivably argue that he is simply mistaken about some fact about the world in much the same way he might be mistaken about height of a distant object. The same is not obviously true for the Everettian: it is a fact about how the world (more precisely, the multiverse) is that he (or one of his counterparts) is guaranteed to be continually misled by his observations. The difference, crucially, is that Papnieau's scenario is espistemically possible, whereas the Everettian equivalent is metaphysically certain. I don't imagine that Papineau would find this line of argument convincing. He does succeed in finding potential justification for assuming typicality: if we are happy to employ it in everyday parlance then why not in Everett? If we are happy to make this leap, then this goes a great way to making sense of Everettian probability. All the same, this line of thinking is very hard to swallow. Papineau's *tu quoque* argument is not, in the end, a positive argument for anything. The typicality assumption must ultimately be accepted on the grounds that it is the bare minimum requirement to make sense of Everettian probability. If it is not granted, the entire project is at stake.

Papineau makes additional, albeit less compelling, proposals for Everettians. He suggests that the entire task of rooting out ignorance or uncertainty in Everett is misguided: "Everettians should simply reject the idea that probabilities require ignorance." His justification is that "the squared moduli behave remarkably like orthodox probabilities even for an Everettian with complete knowledge of the future. [p. 207]" However the real issue is why the squared moduli should be taken like probabilities, a question that, he goes on to say, " Everettians are at least as well placed to answer . . . as orthodoxy." While his thoughts on typicality are useful, this approach seems unwise and not especially conducive to progress on the matter. His grand claim is that "that neither orthodoxy nor Everett can do better than simply assert that credences should match the squared amplitudes." This is not a particularly compelling point, as the issues with the orthodox formulation of quantum mechanics are themselves well documented, and no contemporary investigator of the foundations of physics would feel compelled to defend it. The comparison should surely be how Everettian probability stacks up in this regard against, say, GRW. The probabilities in GRW correspond to an objective truth about the world: the frequency of the collapse function. An observer is then perfectly well justified in setting his credences, according to Lewis' principle principal, as equal to this objective chance. So whether or not the orthodoxy is equally troubled by the issues plaguing Everett, there are absolutely equivalent interpretations of quantum mechanics which are entirely untroubled by these issues. The simple assertion that Everettians are justified in employing the squared moduli to set their credences because the orthodox account is equally muddled seems unfair. It is surely preferable to base our account on genuine ignorance if such an account is possible. This is of course the kind of account that has been presented here. Combining Papineau's thoughts on typicality with the Vaidman approach detailed so far does yield, potentially, a complete account of probability within Everett. The most pressing issue is whether or not stronger arguments for a pseudo-typicality postulate are available.

Chapter 4

Conclusions

This paper has presented an analysis and expansion of some ideas, first proposed by Vaidman, to solve the puzzle of probability in deterministic Everettian quantum mechanics. The success or failure of this account can be deconstructed into whether or not it provides satisfactory answers to the following questions.

Firstly, is the Vaidman interval, the period of time during which an observer is uncertain about the outcome of a measurement, generic in Everett? Under the scheme proposed, the arguments for this are robust. In realm where we consider realistic ideas of what constitutes a measurement - humans and measurement devices - the Vaidman interval cannot be circumvented. It follows from the theory that an observer will always be briefly incapable of locating himself within the multiverse. Furthermore, even if there were a being capable of circumventing this time period - the hypothetical quantum mechanical computer considered - its existence would not undermine the Vaidman interval. Such a being would exist deterministically, below the emergent level of probability, and it would be meaningless to construct for it an account of probability. There are further questions that can be raised in respect to this being - what could it mean to exist deterministically in this sense, evolving in the same metaphysical plane as the wave function? However interesting, these kinds of questions are beyond the scope of this paper. Ultimately, there are sound arguments to the effect that this being does not constitute a compelling counter-example to the genericness of the Vaidman interval.

Secondly, can we build from the Vaidman interval to justify setting our pre-measurements probabilities equal to the squared moduli of the wave function? This proved a greater challenge. Off the bat, we must concede that the best we can hope to achieve is, as Vaidman says, the illusion of probability, i.e. something in our account that plays the same role without pointing to any genuine pre-measurement uncertainty. The solution here is to employ the caring measure, stipulating that, roughly the pre-measurement observer should act in a way that maximises the utility of his descendants. The arguments provided should, I think, be convincing. During the Vaidman interval, the observer's descendants will be relieved to hear that he has acted according to this behaviour principle. The point is less sure-footed than the first. While we can conceive of alternate measures, they are markedly less plausible, and in any case I do not think Vaidman is committed to the caring measure being uniquely rational.

Thirdly, does our account rely on an assumption of typicality, and if so is such an assumption justifiable? This is a difficult and important question, and one which many pro-Everettians are guilty of playing down. As to the first part, an assumption of typicality is inescapable. The process of weighting branches according to their amplitude takes for granted that the amplitude is roughly the 'correct' one. Otherwise, as has been demonstrated, observers find themselves unable to update their scientific theories even in the face of contradictory statistical evidence. Unfortunately, none of the possible solutions outlined here are entirely convincing. Saunders' naive assumption of observer typicality comes across as wishful thinking, as no argument for its acceptance is offered. Hanson's suggestion that the memories of observers in small worlds may be mangled is more promising. If it could be shown that low branch-weight worlds were rendered sufficiently 'fuzzy' by the branching dynamics, we might have a strong argument for neglecting anomalous trajectories. However Hanson's suggestion has not been verified, so it remains to seen if it will yield a plausible solution. Finally, Papineau's suggestion that the failure to justify typicality in Everett is no more problematic than it is everyday probabilistic language. Papineau's suggestion is currently the most plausible way to justify typicality in the account presented here. However it is not an argument in the traditional sense, rather the observation that perhaps typicality does not require as much justification as might initially seem the case. In any case, there is clearly room for more investigation into the role of typicality in Everettian quantum mechanics, and a robust, positive argument for its acceptance would be a huge boost for the argument presented here.

The arguments outlined in this paper when taken as a whole constitute a compelling reason to accept that there can be an intelligible account of probability within Everett. The foundation of this approach - the Vaidman interval - succeeds in introducing genuine uncertainty where previously there was thought to be none. The caring measure an behaviour principles outlined by Vaidman provide a mechanism by which pre-measurement observers can justifiably act as if they are uncertain about the future even though this is not strictly true. Finally, assuming that our observations are roughly typical of the multiverse, we can safely attach the squared-moduli of the wave function to the probabilities of their respective outcomes. Nothing in this paper should be taken as an argument for or against the truth of the Everett interpretation. This is a matter that will be decided entirely by evidence and experiment. What is argued is that, contrary to the suggestions of anti-Everettians, there is a way for observers to act, rationalise and develop the framework of their scientific theories withinin a multiverse.

Bibliography

[1] Werner Heisenberg. *Physics and Beyond: Encounters and Conversations.* Harper-Collins, New York, 1971.

[2] Guido Bacciagaluppi and Antony Valentini. *Quantum Theory at the crossroads : reconsidering the 1927 Solvay conference.* Cambridge University Press, Cambridge, UK, 2009.

[3] John Von Neumann. *Mathematical Foundations of Quantum Mechanics.* Princeton University Press, 1937.

[4] Angelo Bassi and GianCarlo Ghirardi. Dynamical Reduction Models. *Physics Reports*, 379(5-6):257–426, June 2003. ISSN 03701573. doi: 10.1016/S0370-1573(03) 00103-0.

[5] Angelo Bassi, Kinjalk Lochan, Seema Satin, Tejinder P. Singh, and Hendrik Ulbricht. Models of Wave-function Collapse, Underlying Theories, and Experimental Tests. *arXiv.org*, page 130, April 2012. doi: 10.1103/RevModPhys.85.471.

[6] Roger Penrose. On Gravity's Role in Quantum State Reduction. *General Relativity and Gravitation*, 28(5):581–600, 1996.

[7] Louie de Broglie. The new dynamics of quanta. In Antony Valentini and Guido Bacciagaluppi, editors, *Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference*, pages 341 – 362. Cambridge University Press, Cambridge, UK, 2009.

[8] David Bohm. A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables. I. *Physical Review*, 85(2):166–179, 1952.

[9] David Bohm. A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables. II. *Physical Review*, 85(2):180–193, 1952.

[10] David Wallace. *The Emergent Multiverse: Quantum Theory According to the Everett Interpretation.* Oxford University Press, Oxford, 2012.

[11] Wilfrid Sellars. Philosophy and the Scientific Image of Man. *Science Perception and Reality*, 2:35–78, 1963.

[12] Lev Vaidman. On schizophrenic experiences of the neutron or why we should believe in the manyworlds interpretation of quantum theory. *International Studies in the Philosophy of Science*, 12(3):245–261, 1998.

[13] David Deutsch. Three connections between Everett's interpretation and experiment. In *Quantum Concepts in Space and Time*, pages 215–226. Clarendon Press, Oxford, 1986.

[14] P J Lewis. Uncertainty and probability for branching selves. *Studies in History and Philosophy of Modern Physics*, 38:1–14, 2007.

[15] Adrian Kent. One World Versus Many: The Inadequacy of Everettian Accounts of Evolution, Probability, and Scientific Confirmation. In Simon Saunders, Jonathan Barrett, Adrian Kent, and David Wallace, editors, *Many Worlds? Everett, Quantum Theory and Reality*, chapter 10, pages 307–354. Oxford University Press, Oxford, 2010.

[16] David Albert and Barry Loewer. Interpreting the Many Worlds Interpretation. *Synthese*, 77(2):195–213, 1988.

[17] M Lockwood. 'Many Minds'. Interpretations of Quantum Mechanics. *The British journal for the philosophy of science*, 1996.

[18] David Papineau. Probabilities and the Many Minds Interpretation of Quantum M echanics. *Analysis*, 55(4):239–246, 1995.

[19] David Papineau. Many Minds are No Worse than One. *The British Journal for the Philosophy of Science*, 47(2):233–241, June 1996. ISSN 0007-0882. doi: 10.1093/bjps/47.2.233.

[20] David Deutsch. Quantum Theory of Probability and Decisions. *arXiv.org*, quant-ph, 1999.

[21] Simon Saunders. What is Probability? In A Eliztur, S Dolev, and N Kolenda, editors, *Quo Vadis Quantum Mechanics?* Spring Verlag, 2005.

[22] David Wallace. *The Emergent Mutiverse: quantum theory according to the Everett interpretation*. Oxford Universiy Press, Oxford, UK, 2012.

[23] David Wallace. Epistemology Quantized: circumstances in which we should come to believe in the Everett interpretation. *The British Journal for the Philosophy of Science*, 57(4):655–689, 2006.

[24] H Greaves. On the Everettian epistemic problem. *Studies in History and Philosophy of Modern Physics*, 38:120–152, 2007.

[25] Lev Vaidman. Probability and the Many-Worlds Interpretation of Quantum Theory. 2001.

[26] Lev Vaidman. Probability in the Many-Worlds Interpretation of Quantum Mechanics. In Yemima Ben-Menahem and Meir Hemmo, editors, *Probability in Physics*, number Chapter 18, chapter 18, pages 299–311. Springer Berlin Heidelberg, Berlin, 2012.

[27] David Albert. Probability in the Everett Picture. In David Wallace Simons Saunders, Jonathan Barrett, Adrian Kent, editor, *Many Worlds? Eeverett, Quantum Theory and Reality*, pages 355–367. Oxford, 2010.

[28] Sean M. Carroll and Charles T. Sebens. Many Worlds, the Born Rule, and Self-Locating Uncertainty. *Quantum Theory: A Two-Time Success Story*, (2013):1–15, 2014.

[29] David Z Albert. A Quantum-Mechanical Automation. *Philosophy of Science*, 54 (4):577–585, 1987.

[30] Eugene Paul Wigner. Remarks on the Mind-Body Question. In *The Collected Works of Eugene Paul Wigner*, pages 247–260. Springer, Berlin, 1995.

[31] Jenann Ismael. How to Combine Chance and Determinism: Thinking about the Future in an Everett Universe. *Philosophy of Science*, 70(4):776–790, 2010.

[32] Simon Saunders. Transcript: Probability. In *Many Worlds? Everett, Quantum Theory and Reality*, pages 391 – 406. 2012.

[33] Hugh Everett. *"Relative State" Formulation of Quantum Mechanics.* PhD thesis, 1957.

[34] Valia Allori, Sheldon Goldstein, Roderich Tumulka, and Nino Zanghi. Many-Worlds and Schroedinger's First Quantum Theory. *The British Journal for the Philosophy of Science*, 61(1):1–25, March 2010.

[35] Robin Hanson. When Worlds Collide: Quantum Probability from Observer Selection? *Foundations of Physics*, 33(7):1129–1150, 2003.

[36] David Papineau. A Fair Deal for Everettians. In *Many Worlds? Everett, Quantum Theory and Reality*, pages 206–227. 2010.