Many Worlds? An Introduction

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> This problem of getting the interpretation proved to be rather more difficult than just working out the equation.
>
> P.A.M. Dirac

Ask not if quantum mechanics is true, ask rather what the theory implies. What does realism about the quantum state imply? What follows then, when quantum theory is applied without restriction, if need be to the whole universe?

This is the question that this book addresses. The answers vary widely. According to one view, 'what follows' is a detailed and realistic picture of reality that provides a unified description of micro- and macroworlds. But according to another, the result is *nonsense*—there is no physically meaningful theory at all, or not in the sense of a realist theory, a theory supposed to give an intelligible picture of a reality existing independently of our thoughts and beliefs. According to the latter view, the formalism of quantum mechanics, if applied unrestrictedly, is at best a fragment of such a theory, in need of substantive additional assumptions and equations.

So sharp a division about what appears to be a reasonably well-defined question is all the more striking given how much agreement there is otherwise, for all parties to the debate in this book are agreed on realism, and on the need, or the aspiration, for a theory that unites micro- and macroworlds, at least in principle. They all see it as legitimate—obligatory even—to ask whether the fundamental equations of quantum mechanics, principally the Schrödinger equation, *already* constitute such a system. They all agree that such equations, if they are to be truly fundamental, must ultimately apply to the entire universe. And most of the authors also agree that the quantum state *should* be treated as something physically real. But now disagreements set in.

For the further claim argued by some is that if you allow the Schrödinger equation unrestricted application, supposing the quantum state to be something physically real, then without making any additional hypotheses, there follows a conservative picture of the small macroscopic, consistent with standard applications of quantum mechanics to the special sciences, a picture that extends to the biological sciences, to people, planets, galaxies, and ultimately the entire universe, but only insofar as this universe is one of *countlessly many others, constantly branching in time, all of which are real.* The result is the *many worlds theory*, also known as the *Everett interpretation* of quantum mechanics.

But contrary claims say this picture of many worlds is in no sense inherent in quantum mechanics, even when the latter is allowed unrestricted scope and even given that the quantum state is physically real. And if such a picture *were* implied by the Schrödinger equation, that would only go to show that this equation must be restricted or supplemented or changed. For (run contrary claims) this picture of branching worlds fails to make physical sense. The stuff of these worlds, what they are made of, is never adequately explained, nor are the worlds precisely defined; ordinary ideas about time and identity over time are compromised; the concept of probability itself is in question. This picture of many branching worlds is inchoate. There are realist alternatives to many worlds, among them theories that leave the Schrödinger equation unchanged.

These are the claims and counterclaims argued in this book. This introduction is in three parts. The first is partisan, making the case for many worlds in the light of recent discoveries; the crucial new datum, absent from earlier discussions, is decoherence theory, which in this book takes centre stage. Section 2 is even-handed, and sketches the main arguments of the book: on ontology, the existence of worlds; on probability, as reduced to the branching structure of the quantum state; and on alternatives to many worlds, realist proposals that leave the Schrödinger equation unchanged. The third and final section summarizes some of the mathematical ideas, including the consistent histories formalism.

1 THE CASE FOR MANY WORLDS

1.1 Realism and Quantum Mechanics

As Popper once said, physics has always been in crisis, but there was a special kind of crisis that set in with quantum mechanics. For despite all its obvious empirical success and fecundity, the theory was based on rules or prescriptions that seemed inherently contradictory. There never was any real agreement on these matters among the founding fathers of the theory. Bohr and later Heisenberg in their more philosophical writings provided little more than a fig-leaf; the emperor, to the eyes of realists, wore no clothes. Textbook accounts of quantum mechanics in the past half-century have by and large been operationalist. They say as little as possible about Bohr and Heisenberg's philosophy or about realism.

In what sense are the rules of quantum mechanics contradictory? They break down into two parts. One is the *unitary* formalism, notably the Schrödinger equation, governing the evolution of the quantum state. It is deterministic and encodes spacetime and dynamical symmetries. Whether for a particle system or a system of fields, the Schrödinger equation is linear: the sum of two solutions to the equation is also a solution (the superposition principle). This gives the solution space of the Schrödinger equation the structure of a vector space (Hilbert space).

However, there are also rules for another kind of dynamical evolution for the state, which is—well, *none* of the above. These rules govern the *collapse* of the wavefunction. They are indeterministic and non-linear, respecting none of the spacetime or dynamical symmetries. And unlike the unitary evolution, there is no obvious route to investigating the collapse process empirically.

Understanding state collapse, and its relationship to the unitary formalism, is the *measurement problem* of quantum mechanics. There are other conceptual questions in physics, but few if any of them are genuinely paradoxical. None, for their depth, breadth, and longevity, can hold a candle to the measurement problem.

Why not say that the collapse is simply irreducible, 'the quantum jump', something primitive, inevitable in a theory which is fundamentally a theory of chance? Because it isn't only the collapse process itself that is under-specified: the time of the collapse, within relatively wide limits, is undefined, and the criteria for the kind of collapse, linking the set of possible outcomes of the experiment to the wavefunction, are strange. They either refer to another theory entirely—classical mechanics—or worse, they refer to our 'intentions', to the 'purpose' of the experiment. They are the *measurement postulates*—('probability postulates' would be better, as this is the only place where probabilities enter into quantum mechanics). One is the *Born rule*, assigning probabilities (as determined by the quantum state) to macroscopic outcomes; the other is the *projection postulate*, assigning a new microscopic state to the system measured, depending on the macroscopic outcome. True, the latter is only needed when the measurement apparatus is functioning as a state-preparation device, but there is no doubt that *something* happens to the microscopic system on triggering a macroscopic outcome.

Whether or not the projection postulate is needed in a particular experiment, the Born rule is essential. It provides the link between the possible macroscopic outcomes and the antecedent state of the microscopic system. As such it is usually specified by giving a choice of vector basis—a set of orthogonal unit vectors in the state space—whereupon the state is written as a superposition of these. The modulus square of the amplitude of each term in the superposition, thus defined, is the probability of the associated macroscopic outcome (see Section 3 p.37). But what dictates the choice of basis? What determines the time at which this outcome happens? How does the measurement apparatus interact with the microscopic system to produce these effects?

From the point of view of the realist the answer seems obvious. The apparatus itself should be modelled in quantum mechanics, then its interaction with the microscopic system can be studied dynamically. But if this description is entirely quantum mechanical, if the dynamics is unitary, it is *deterministic*. Probabilities only enter the conventional theory explicitly with the measurement postulates. The straightforwardly physicalistic strategy seems *bound* to fail.

How are realists to make sense of this? The various solutions that have been proposed down the years run into scores, but they fall into two broadly recognizable classes. One concludes that the wavefunction describes not the microscopic system itself, but our *knowledge* of it, or the information we have available of it (perhaps 'ideal' or 'maximal' knowledge or information). No wonder modelling the apparatus in the wavefunction is no solution: that only shifts the problem further back, ultimately to 'the observer' and to questions about the mind, or consciousness, or information—all ultimately philosophical questions. Anti-realists welcome this conclusion; according to them, we neglect our special status as the knowing subject at our peril. But from a realist point of view this just leaves open the question of what the goings-on at the microscopic level, thus revealed, actually are. By all means constrain the spatiotemporal description (by the uncertainty relations or information-theoretic analogues), but still *some* spatiotemporal description must be found, down to the lengthscales of cells and complex molecules at least, even if not all the way to atomic processes. That leads to the demand for equations for variables that do not involve the wavefunction, or, if none is to be had in quantum mechanics, to something entirely new, glimpsed hitherto only with regard to its statistical behaviour. This was essentially Einstein's settled view on the matter.

The only other serious alternative (to realists) is *quantum state realism*, the view that the quantum state *is* physically real, changing in time according to the unitary equations and, somehow, *also* in accordance with the measurement postulates.

How so? Here differences in views set in. Some advocate that the Schrödinger equation itself must be changed (so as to give, in the right circumstances, collapse as a fundamental process). They are for a *collapse* theory.

Others argue that the Schrödinger equation can be left alone if only it is supplemented by additional equations, governing 'hidden' variables. These, despite their name, constitute the real ontology, the stuff of tables and chairs and so forth, but their behaviour is governed by the wavefunction. This is the *pilot-wave* theory. Collapse in a theory like this is only 'effective', as reflecting the sudden irrelevance (in the right circumstances) of some part of the wavefunction in its influence on these variables. And once irrelevant in this way, always irrelevant: such parts of the wavefunction can simply be discarded. This explains the appearance of collapse.

But for others again, no such additional variables are needed. The collapse is indeed only 'effective', but that reflects, not a change in the influence of one part of the quantum state on some hidden or 'real' ontology, but rather the change in dynamical influence of *one part of the wavefunction over another*—the *decoherence* of one part from the other. The result is a branching structure to the wavefunction, and again, collapse only in a phenomenological, effective sense. But then, if our world is just one of these branches, all these branches must be worlds. Thus the *many worlds* theory—worlds not spatially, but *dynamically* separated.

This concept of decoherence played only a shadowy role in the first 50 years of quantum mechanics, but in the past three decades it has been developed more systematically. As applied to the wavefunction to derive a structure of branching worlds it is more recent still. It changes the nature of the argument about the existence of worlds. The claim is that the worlds are dynamically robust patterns in the wavefunction, obeying approximately classical equations. They are genuine discoveries, the outcome of theoretical investigations into the unitary formalism, not from posits or hypotheses. And *if* this is so, it puts collapse theories and pilot-wave theories in a different light. It shows them as modifying or supplementing quantum mechanics not in the name of realism, and still less because of any conflict with experiment, but because reality as depicted by quantum mechanics is in conflict with a priori standards of acceptability—at the fundamental level, as too strange, at the emergent level of worlds, as insufficiently precise or as ontologically too profligate.

But if decoherence theory makes a difference to our understanding of the quantum state, as applied to sufficiently complex many-particle systems, it is not so clear that it touches that other ground on which the Everett interpretation has been rejected—that if quantum mechanics is a purely deterministic theory of many worlds, the idea of objective probability is *simply no longer applicable*. Failing a solution to this, the 'incoherence problem', the Everett interpretation does not provide an empirical theory at all. And with that the argument against modifying quantum mechanics on realist grounds completely collapses.

We shall see how this argument played out in the first three decades of the Everett interpretation in a moment. In fact, it is just here, on the interpretation of probability, that inattention to the concept of decoherence was most damaging. And looking ahead, there is a further important sense in which the argument over many worlds has been changed. In recent years, with the development of decision-theory methods for quantifying subjective probability in quantum mechanics, the link between probability in the subjective sense and an objective counterpart has been greatly clarified. Specifically, it can be shown that agents who are rational, in order to achieve their ends, have no option but to use the modulus squared branch amplitudes in weighting their utilities. In this sense the Born rule has been *derived*.

That goes with other arguments about probability. From a philosophical point of view this link with rational belief, or credence, has always been the most important—and baffling—of the roles played by objective probability. If it is shown to be played by these branching structures then they *are* objective probabilities.

Meanwhile other puzzles specific to the picture of branching worlds (particularly to do with personal identity) can be solved or sidelined.

These claims need to be argued one by one. The first step is to get a clearer understanding of the early debates about many worlds, in the first quarter-century since their appearance.

1.2 Early History

The many-worlds interpretation of quantum mechanics was first proposed by H. Everett III in his doctoral ('long') dissertation, written under his supervisor J.A. Wheeler. It was cut down to a quarter of its size on Wheeler's insistence, but in this form it won a PhD. It won him a ringing endorsement by Wheeler too, when it was published shortly thereafter, as ' "Relative State" Formulation of Quantum Mechanics' (Everett [1957], Wheeler [1957]).[1] The main mathematical ideas of this paper are explained in Section 3.

Ten years later it was endorsed again, but this time with rather greater fidelity to Everett's ideas, by B. DeWitt [1967, 1970] (introducing the terminology 'many worlds' and 'many universes' for the first time in print). DeWitt, in collaboration with his PhD student N. Graham, also published Everett's long dissertation, as 'The Theory of the Universal Wave Function'. This and a handful of much smaller articles made up their compilation *The Many Worlds Interpretation of Quantum Mechanics* (DeWitt and Graham [1973]).

But as such it had a notable deficiency. It lacked an account of why the wavefunction must be viewed in terms of one sort of multiplicity rather than another—and why, even, a multiplicity at all. What reason there was to view the quantum state in this way (as a superposition of possible outcomes) came from the measurement postulates—the very postulates that Everett, by his own admission, was intent on doing away with.

This is the 'preferred basis problem' of the Everett interpretation (Ballentine [1973]). If the basis is determined by the 'purpose' of the experiment, then the measurement postulates are blatantly still in play. DeWitt, who was more interested in applying quantum mechanics to gravitating systems (hence, ultimately, to the entire universe) than in getting rid of any special mention of experiments in the definition of the theory, went so far as to postulate the existence of apparatuses as an *axiom*.

But Everett was able to derive at least a fragment of the Born rule. Given that the measure over the space of branches is a function of the branch amplitudes, the question arises: What function? If the measure is to be additive, so that the measure of a sum of branches is the sum of their measures, it follows that it is the modulus square—that was something. The set of branches, complete with

[1] This story is told by Peter Byrne in Chapter 17.

additive measure, then constitute a probability space. As such, versions of the Bernouilli and other large number theorems can be derived. They imply that the measure of all the branches exhibiting anomalous statistics (with respect to this measure) is small when the number of trials is sufficiently large, and goes to zero in the limit—that was something more.

It was enough to raise the prospect of a frequentist account of quantum probability—meaning, an account that identifies probabilities with actual relative frequencies—and to raise the hope, more generally, that the probability interpretation of quantum mechanics was itself *derivable* from the unitary formalism. DeWitt was further impressed by a result due to his PhD student N. Graham [1970] (and discovered independently by J.B. Hartle [1968]): for the k^{th} possible outcome of an experiment, it is possible to construct a 'relative-frequency operator', of which the grand superposition of all possible outcomes on N repetitions of the experiment is an eigenstate in the limit as $N \to \infty$. The corresponding eigenvalue, he showed, equals the Born rule probability for the k^{th} outcome in a single trial.

Another way of putting it (DeWitt [1970 pp.162–3]) was that the components of the total superposition representing 'maverick' worlds (recording anomalous statistics at variance with the Born rule) are of measure zero, in the Hilbert space norm, in the limit $N \to \infty$. But whilst formal criteria like these may be required of an acceptable theory of probability, they are hardly sufficient in themselves. They do not explain probability in terms of existing states of affairs, which invariably involve only finitely many trials. To suppose that states whose records of outcomes are large are 'close to' states in the infinite limit, and therefore record the right relative frequencies (or that those that do are more probable than those that do not), is to beg the question.[2]

These are defects of frequentism as a theory of probability; they are hardly specific to the Everett interpretation. But in an important respect frequentism in the context of many worlds seems to fare worse. For assuming (as Everett's original notation suggested) that branches are in one-one correspondence with sequences of experimental outcomes, the set of all worlds recording the results of N trials can be represented by the set of all possible sequences of length N. In that case, there is an obvious rival to the Born rule: this set of sequences has a natural statistical structure independent of the amplitude, anyway invisible, attached to each world. A priori there are many more of them for which the relative frequency of the k^{th} outcome is close to one half, than of those in which it is closer to zero or one.

This is to treat each distinct sequence as equiprobable. Predictions using this rule would have been wildly contradicted by the empirical evidence, true, but that only goes to show that the probability rule in the Everett interpretation

[2] Ochs [1977] and Farhi et al. [1989] offered improvements on the rigour of the argument, but not on its physical significance.

is not forced by the equations—that the Born rule, far from being an obvious consequence of the interpretation of the quantum state in terms of many worlds, appears quite unreasonable.

Attempts to patch up this problem only made clearer its extent. The difficulty—call it the 'combinatorics problem'—was first pointed out by Graham [1973]: he suggested that experiments must involve as an intermediary a thermodynamic system of large numbers of degrees of freedom, for which the count of states did reflect the Born rule quantities. But few found this argument persuasive. And the problem highlighted the deficiencies of Everett's derivation of the Born rule: for why assume that the probability measure on the space of branches is a function only of the branch amplitudes? This assumption, given the picture of many worlds, now seemed ad hoc.

Failing a solution to the preferred basis problem, the theory was not even well defined; if the problem is solved by evoking a special status to experiments, the theory was not even a form of realism; however it is solved, the very picture of many worlds suggests a probability measure at odds with the statistical evidence. It is hardly surprising that the Everett interpretation was ignored by J.S. Bell, when he posed his famous dilemma:

> Either the wavefunction, as given by the Schrödinger equation, is not everything, or it is not right. (Bell [1987 p.201])

But the situation looks quite different today.

1.3 Ontology and Decoherence

Decoherence theory has its roots in Ehrenfest's theorem[3] and the very early debates about foundations in quantum mechanics, but its development was slow. It remains to this day more of a heterogeneous collection of techniques than a systematic theory. But these techniques concern a common question: under what circumstances and with respect to what basis and dynamical variables does a superposition of states behave dynamically just as if it were an incoherent mixture of those same states? This question already arises in conventional quantum mechanics using the measurement postulates (the choice of the von Neumann 'cut'): at what point in the unitary evolution can the latter be applied? If too early, interference effects of salience to the actual behaviour of the apparatus will be destroyed.

The 1980s saw a plethora of toy models attempting to answer this question. Many relied on the system–environment distinction and the Schmidt decomposition (see Section 3 pp.43–4). Together, apart from exceptional cases, they defined a unique basis, with respect to which at any time mixtures and superpositions of states were exactly equivalent, under the measurement postulates, for

[3] This theorem is explained and improved on by Jim Hartle in Chapter 2.

observables restricted to the system alone or to the environment alone. But not for other observables; and the basis that resulted was in some cases far from well localized, and sensitive to the details of the dynamics and the system-environment distinction.

In contrast, states well localized in phase space—wavepackets—reliably decohere, and even though elements of a superposition, evolve autonomously from each other for a wide class of Hamiltonians. With respect to states like these, Ehrenfest's theorem takes on a greatly strengthened form. But decoherence in this sense is invariably approximate; it is never an all-or-nothing thing.

Very little of this literature made mention of the Everett interpretation: it was hoped that decoherence would solve the measurement problem in a one-world setting. And where Everett's ideas were involved, it was the concept of 'relative state', as formulated (in terms of the decoherence of the environment) by W.H. Zurek [1982]. According to Zurek, such states need not coexist; rather, macroscopic quantities were subject to 'environmental superselection', after the idea of superselection rules, latterly introduced, prohibiting the development of superpositions of states corresponding to different values of super-selected quantities like charge. But if so, surely decoherence, like superselection, has to be exact, returning us to the Schmidt decomposition.[4]

The exception was H.D. Zeh's 'On the interpretation of measurement in quantum mechanics' [1970], which engaged with Everett's proposal more comprehensively. In it, Zeh set out the idea of dynamical decoherence as a stability condition. He gave the example of sugar molecules of definite chirality; in a superposition of left- and right-handed molecules, each term evolves by a dynamical process which, however complicated, is almost completely decoupled from the motion of the other. Dynamical stability, he proposed, was the key to defining the preferred basis problem in the Everett interpretation.

But Zeh's argument was qualitative, and in subsequent publications (Zeh [1973], Kübler and Zeh [1973]), which did give detailed calculations, he used the Schmidt decomposition instead. This still chimed with Everett's idea of the 'relative state' (Section 3.2), but the idea of dynamical stability was marginalized. And an inessential one was added: Zeh spoke of the need for a 'localization of consciousness' not only in space and time but also in 'certain Hilbert-space components' [1970 p.74], an idea he attributed to Everett. That fostered the view that some high-level hypothesis about mentality was needed if the Everett interpretation was to go through.[5] But if the Everett

[4] The attempt to define the preferred basis in terms of the Schmidt decomposition was taken to its logical conclusion in the 'modal' interpretation, as developed in the mid 1990s by D. Dieks, G. Bacciagaluppi, and P. Vermas, among others. But this ran into an embarrassment of technical difficulties (see, in particular, Bacciagaluppi [2000]). The extension to the N-body case was restrictive, the basis thus selected was defective, particularly in the case of quantum fields, and no non-trivial Lorentz covariant theory of this kind could be found.

[5] Taken up by M. Lockwood in his *Mind, Brain and Quantum* [1989] and by J. Barrett in *The Quantum Mechanics of Minds and Worlds* [1999] (for commentaries, see my [1996a, 2001]).

interpretation recovers the elements of quantum chemistry, solid state physics, and hydrodynamics, as the effective theories governing each branch, questions of mentality can be left to the biological sciences. They need no more intrude in the Everett interpretation than they do in a hidden-variable or collapse theory.

Add to this mix the consistent histories formalism of R. Griffiths [1984] and R. Omnès [1988], and especially as developed by M. Gell-Mann and J.B. Hartle [1990, 1993]. In this approach a division between subsystem and environment is inessential: the key idea is the coarse-graining of certain dynamical variables and the definition of quantum histories as time-ordered sequences of coarse-grained values of these variables (Section 3.3). The wavefunction of the universe—in the Heisenberg picture—is in effect the superposition of these histories. The choice of variables—of the history space—is equivalent to the choice of preferred basis.

But this is to put the matter in Everettian terms. The consistent histories theory was based rather on a certain formal constraint, required if a space of quantum histories is to have the structure of a probability space: the 'consistency condition' (Section 3.4). Meanwhile the quantum state, in the Heisenberg picture, could be viewed as *no more* than a probability measure on this space, of which only one history, it seemed, need be real.[6]

The goal was once again a one-world interpretation of quantum mechanics. But for that, fairly obviously, the history space had to be fixed once and for all. It was clear from the beginning that there were many consistent history spaces for a fixed initial state and Hamiltonian—which one should we choose? But it was thought that at least the actual history of the world and its history space up to some time, once given, dictated the probabilities for subsequent events unequivocally. Far from it: as F. Dowker and A.P.A. Kent shortly showed, a history space up to some time can be deformed into any one of a continuous infinity of other history spaces for subsequent times, preserving the consistency condition exactly (Dowker and Kent [1996]).

But this difficulty does not apply to the marriage of decoherence theory in the more general sense with the consistent histories formalism, as carried through by Gell-Mann and Hartle and J.J. Halliwell in the 1990s and 2000s.[7] Decohering histories in the latter sense are robustly defined. But decoherence (and the consistency condition) obtained in this way is never exact. On a one-world

Everett made no mention of consciousness, although he did speak of 'experience'. Zeh has continued to insist on the need, in the Everett interpretation, for a special postulate concerning consciousness (see e.g. Zeh [2000]).

[6] This turns the wavefunction of the universe into something more like a law than a physically existing thing. See Antony Valentini in Chapter 16 for criticism of an analogous proposal in the context of pilot-wave theory.

[7] Chapter 2, by Jim Hartle, is a general review; Chapter 3, by Jonathan Halliwell, is a detailed study of the important example of hydrodynamic variables.

interpretation, 'sufficiently small' interference ϵ is then the criterion for probabilistic change at the fundamental level; what value of $\epsilon > 0$, precisely, is the trigger? Worse, there is no algorithm for extracting even approximately decohering histories for *any* Hamiltonian and *any* state. How are the latter to be constrained? This is at best a programme for modifying quantum mechanics, or replacing it.

It is otherwise if decoherent histories theory is in service of the Everett interpretation—defining, among other things, the preferred basis. In that case it hardly matters if, for some states and regimes, decohering histories are simply absent altogether: the fundamental reality, the wavefunction of the universe, is still well defined (Zurek [1993], Saunders [1993, 1995a]). Context-dependence and inexactitude as to how it is to be broken down into recognizable parts is to be expected.

It is a further and quite distinct question as to what kinds of parts or worlds, governed by what kinds of equations, are thus identified. The ones so far discovered are all approximately classical—classical, but with dissipation terms present that reflect their quantum origins.[8] In the terminology of Gell-Mann and Hartle [1990], such a set of decoherent histories is a 'quasiclassical domain' (in their more recent writings, 'realm'). Might there be non-classical realms, other preferred bases, involving sets of equations for completely alien variables? Perhaps, but that need not pose any difficulty for the Everett interpretation. There is no reason to think that the kind of under-determination of consistent history space by past history, discovered by Dowker and Kent, applies to realms.

And now for the killer observation (Wallace [2003a]): this business of extracting approximate, effective equations, along with the structures or patterns that they govern, is *routine* in the physical sciences. Such patterns may be high-level, 'emergent' ontology; they are fluids, or crystals, or complex hydrocarbon molecules, ascending to cells, living organisms, planets, and galaxies. Equally they are atoms and nuclei, as modelled (with great difficulty) in quantum chromodynamics and electroweak theory, or phonons, or superconductors, or Bose condensates, in condensed matter physics—the list goes on and on (what *isn't* emergent ontology?). It is in this sense—so the claim goes—that worlds are shown to exist in the wavefunction, rather than be put in by hand. They are investigated just as is any other emergent ontology in the special sciences.[9]

If so, doesn't it follow that many worlds *also* exist in pilot-wave theory? As formulated by L. de Broglie in 1927 and by D. Bohm in 1952, the 'pilot

[8] Going the other way—given that worlds are defined in terms of states well localized in position and momentum space—consistency follows trivially, and it is relatively easy to see (from Ehrenfest's theorem) that states like these obey approximately classical equations (see Hartle, Chapter 2). Convinced Everettians such as DeWitt, Deutsch, and Vaidman saw no need for decoherence theory in consequence. (It went almost unmentioned in Deutsch [1997] and in Vaidman [1998], [2002]; when DeWitt did take note of it (De Witt [1993]), he applied it to branching in the absence of experiments. When induced by experiments 'Everett has already dealt with it'.)

[9] This argument is reprised by David Wallace in Chapter 1.

wave' is just the wavefunction as given by the Schrödinger equation, so it is mathematically, structurally, identical to the universal state in the Everett interpretation. Indeed, decoherence theory plays the same essential role in dictating 'effective' wavepacket collapse in that theory as it does in the Everett interpretation.[10] There follows the gibe: pilot-wave theories are 'parallel universe theories in a state of chronic denial' (Deutsch [1996 p.225]).[11]

A similar consideration applies even to those models of dynamical collapse in which, from an Everettian point of view, the amplitudes of all worlds save one are only suppressed (they remain non-zero—and, 'for all practical purposes', are subject only to the unitary evolution). It applies, uncomfortably, to the only realistic (albeit non-relativistic) collapse theories so far available, those due to G.C. Ghirardi and his co-workers (Ghirardi et al. [1986], [1990]), in which the collapse attenuates but does not eliminate altogether components of the state. In these theories all the structures of the wavefunction of the universe as goes (what on Everettian terms would be) *other* worlds are still there, their *relative* amplitudes all largely unchanged. This is the so-called 'problem of tails'. On any broadly structuralist, functionalist approach to the physical sciences, these structures to the tails are still real.[12]

The distinctively new feature of the Everett interpretation today is not only that the preferred basis problem is solved; it is that the very existence of worlds, of a multiplicity of patterns in the wavefunction, each obeying approximately classical laws, is *derived*. The fact that they make an unwelcome appearance in every other form of quantum-state realism, from which they can be removed, if at all, only with difficulty, proves the point. But there is more.

1.4 Probability and Decision Theory

Decoherence bears on the probability interpretation if only because it explains why there is a plurality at all. It shows that *some* kind of statistical analysis is perfectly reasonable. But it also undercuts at a stroke the combinatorics problem. For decoherence comes in degrees; there is no good answer to the question—How many decohering worlds? Numbers like these can be stipulated, but from the point of view of the dynamical equations, they would amount to arbitrary conventions. There is no longer a statistical structure to the set of

[10] As Bohm effectively acknowledged when considering the problem of when 'empty waves' in pilot-wave theory could be ignored: 'It should be noted that exactly the same problem arises in the usual interpretation of the quantum theory, for whenever two packets overlap, then, even in the usual interpretation, the system must be regarded as, in some sense, covering the states corresponding to both packets simultaneously.' (Bohm [1952 p.178 fn.18]). Here Bohm referenced his textbook on quantum mechanics published the previous year, containing an early treatment of decoherence (Bohm [1951 ch.6, 16, sec.25]).

[11] The argument is made in detail by Brown and Wallace [2005]. In Chapter 16, Antony Valentini gives a reply, followed by a commentary by Harvey Brown.

[12] On this point see Tim Maudlin in Chapter 4.

branches independent of the amplitudes. The Born rule no longer has an obvious rival (Saunders [1998]).

At this point, whatever DeWitt's hopes of deriving the probability interpretation of the theory from the equations, one might hope to settle for the probability rule as a *hypothesis*—much as is done, after all, in conventional quantum mechanics. The ontology—the branching structure to the wavefunction—may not force any particular probability measure, very well; then choose one on empirical grounds. The Born rule is a natural candidate and recovers the probabilistic predictions of conventional quantum mechanics. Why not simply postulate it?

But that would be to reinstate a part—a small part, given that decoherence theory is now dictating the basis, but a part nonetheless—of the measurement postulates. And on reflection, it is not a wholly uncontentious part. There is a puzzle, after all, as to whether, being among a superposition of worlds, we are not in some sense in them all. About to perform a quantum experiment, if the Everett interpretation is to be believed, all outcomes are obtained. We know this in advance, so where is there any uncertainty? And this problem can be lumped with the preferred basis problem: why not let the questions of personal identity and preferred basis follow together from a theory of mentality, or a theory of computation, or of quantum information (Albert and Loewer [1988], Lockwood [1989], Barrett [1999])? Or be posited, in terms of new axioms, at the level of worlds (Deutsch [1985])?

The appeal to mentality is in the tradition of Wheeler and Wigner rather than that of Everett and DeWitt, and we have turned our backs on it. But still, it makes it clearer why it is unsatisfactory to simply *posit* a probability interpretation for the theory. If there is chance in the Everett interpretation, it should be identified as some objective physical structure, and that structure should be shown to fill all (or almost all) the chance-roles—including, plausibly, the role of uncertainty. It cannot just pretend to fill it, or fill it by decree (Greaves [2004]). However, that may turn out to be more of a linguistic matter than is commonly thought. As argued by Papineau [1996], the notion of uncertainty appears to play no useful rule in decision theory.

But there is another chance-role, what the philosopher D.K. Lewis has called the 'principal principle', that all are agreed is indispensable. Let S be the statement that the chance of E at t is p, and suppose our background knowledge K is 'admissible' (essentially, that it excludes information as to whether E happened or not): then our credence in E, conditional on S and K, should be p.

Here 'credence' is subjective probability, degrees of belief. It is probability in the tradition of F.K. Ramsey, B. de Finetti, and L. Savage. Credence is what matters to decision theory, statistical inference, and statistical test. So long as the notion of free will and agency is not in question in the Everett interpretation—or no more so than in classical mechanics or pilot-wave theory—the Ramsey–de

Finetti operational characterization of credence in terms of an agent's betting behaviour will still be available. If, indeed, their criterion for consistency among bets—the 'no Dutch book argument'—still makes sense among branching worlds, this all in itself is a solution to the incoherence objection (that the concept of probability simply *makes no sense* in the Everett interpretation): for still, an agent must make reasoned choices among quantum games.

Very well: probability in the sense of credence may still be implicit in our behaviour even in a branching universe. But we have been given no reason as yet as to why it should track some objective counterpart. It seems positively odd that it should track the mod-squared branch amplitudes, anyway invisible, as required by the Everettian version of the principal principle.

But no one-world theory of objective probability does very well when it comes to the principal principle. Of course it can be made to *sound* rather trite—why shouldn't our degree of subjective uncertainty be set equal to the degree of objective uncertainty?—but that is little more than a play on words. The answer must ultimately depend on what, concretely, 'degrees of objective uncertainty' (chances) really are. For physicalists, they had better be fixed by the physical facts—perhaps by the entire sequence of physical facts of a chance process, or even of all chance processes. But then what sort of physical facts or quantities, exactly? How can any normal physical quantity, a 'Humean magnitude'[13] (like mass or relative distance or field intensity), have such a special place in our rational lives? It is hard for that matter to see how a *problematic* quantity like 'potentiality' or 'propensity' can play this role either. But if objective probabilities float free of the physical facts altogether, it is even harder to see why they should matter.

This dilemma was stated by Lewis in a famous passage:

> The distinctive thing about chances is their place in the 'Principal Principle', which compellingly demands that we conform our credences about outcomes to our credences about their chances. Roughly, he who is certain the coin is fair must give equal credence to heads and tails . . . I can see, dimly, how it might be rational to conform my credences about outcomes to my credences about history, symmetries, and frequencies. I haven't the faintest notion how it might be rational to conform my credences about outcomes to my credences about some mysterious unHumean magnitude. Don't try to take the mystery away by saying that this unHumean magnitude is none other than chance! I say that I haven't the faintest notion how an unHumean magnitude can possibly do what it must do to deserve that name—namely, fit into the principle about rationality of credences—so don't just stipulate that it bears that name. Don't say: here's chance, now is it Humean or not? Ask: is there any way that any Humean magnitude could fill the chance-role? Is there any way that an unHumean magnitude could? What I fear is that the answer is 'no' both times! Yet how can I reject the very idea of chance, when I

[13] After the philosopher D. Hume, who insisted that nothing was available to inspection other than 'matters of fact'. The problem that follows is strikingly similar to Hume's 'problem of induction' (the problem of identifying 'causes' rather than 'chances' in terms of matters of fact).

know full well that each tritium atom has a certain chance of decaying at any moment? (Lewis [1986a pp.xv–xvi]).

Why hasn't Lewis already given the answer—that chances are made out in terms of history, symmetries, and frequencies? According to 'naive frequentism', probabilities *just are* actual relative frequencies of outcomes. But we know the deficiencies of this. Unfortunately, it seems that no *sophisticated* form of frequentism is workable either. Lewis put the problem like this: not even all facts about the actual world, future as well as past, could pin down facts about probability. If a history-to-chance conditional (the chance is thus-and-so given such-and-such a sequence of events) is made true by some pattern of events, past and future, there must be a chance that that pattern happens; there must be a chance it *doesn't* happen. But the pattern that may result instead may yield a completely different history to chance conditional for the original pattern. Chance, if supervenient on the actual history of a single world, is 'self-undermining' (Lewis [1986a]).[14]

Now for the punchline: *none* of this is a problem in Everettian quantum mechanics. The self-undermining problem is fairly easily solved. And much less obviously, the principal principle can be *explained*. For replace Lewis's Humean tapestry of events by an Everettian tapestry of events, connected not only by relations in space and time but also by the new fundamental relations introduced by quantum mechanics (the transition amplitudes), and then (as argued above) it has the structure of a collection of branching, approximately classical histories. Lewis's question, of why, given this, we should think that the branch amplitudes should dictate our rational credences, is answered thus: an agent who arranges his preferences among various branching scenarios—quantum games—in accordance with certain principles of rationality, *must* act as if maximizing his expected utilities, as computed from the Born rule.

This argument was first made by D. Deutsch in his paper 'Quantum theory of Probability and Decisions' [1999]. It was, in essence, a form of Dutch-book argument, strengthened by appeal to certain symmetries of quantum mechanics. But it hinged on a tacit but relatively powerful assumption subsequently identified by Wallace as 'measurement neutrality' (Wallace [2002]). It is the assumption that an agent should be indifferent as to which of several measurement apparatuses is used to measure a system in a given state, so long as they are all instruments designed to measure the same dynamical variable (by the lights of conventional quantum mechanics).

In fact Deutsch [1999] made no explicit mention of the Everett interpretation. If, indeed, experimental procedures are appropriately operationalized, measurement neutrality is effectively built in (Saunders [2004]). But that can

[14] For these and other deficiencies of one-world theories of chance, see David Papineau in Chapter 7. Papineau also defends the claim that the notion of uncertainty plays no useful role in decision theory.

hardly be assumed in the context of the present debate, where it is disputed that the Everett interpretation recovers the measurement postulates, FAPP ('for all practical purposes'), or underwrites ordinary operational procedures.

In the face of this, Wallace [2003b, 2007] offered a rather different, two-part argument. The first was a formal derivation of the Born rule from (rather weak) axioms of pure rationality, given only, in place of measurement neutrality, 'equivalence'—the rule, roughly, that an agent should be indifferent between experiments that yield exactly the same amplitudes for exactly the same outcomes. The second part consisted of informal, pragmatic, but still normative arguments for this rule. This paper attracted wide comment, but primarily by way of allegedly rational—bizarre perhaps, but rational—counterexamples to the Born rule. Wallace's arguments for equivalence were not addressed (or not by Baker [2006], Lewis [2007], and Hemmo and Pitowsky [2007]).

The upshot: pragmatic and rational constraints force the compliance of an agent's expected utilities, as computed from his credences, with his expected utilities as computed from the Born rule. The result is as good a solution to Lewis's dilemma as could be desired—as good as those rational and pragmatic constraints are judged reasonable.[15] Obviously they are somewhat idealized; it is the same with the Dutch book arguments of de Finetti and Ramsey, where an agent's utilities are supposedly quantified in terms of (relatively small) financial rewards; but so long as nothing question-begging or underhand is going on, Wallace's result is already a milestone. Nothing similar has been achieved for any one-world physical theory.

Why is that exactly? Is it for want of perseverance or ingenuity? Perhaps; the same general strategy is available to any other physical probability theory that implies meaningful pragmatic constraints, independent of its interpretation in terms of probability (examples that come to mind include classical statistical mechanics and pilot-wave theory). But Everettian quantum mechanics is special in another respect. As Wallace points out, at the heart of his arguments for the equivalence rule (and Deutsch's original argument) is a certain symmetry—the case of the equi-amplitude outcomes—that cannot possibly be respected in any one-world theory. A tossed coin in any one-world theory must land one way or the other. However perfect the symmetry of the coin, this symmetry cannot be respected (not even approximately) by the dynamics governing its motion on any occasion on which it is actually thrown. But it can in Everettian quantum mechanics.[16]

This link with rationality is not all of the meaning of physical probability, however. It is not even the only link needed with credence. The two come

[15] In Chapter 8 by David Wallace these constraints are written down as axioms, and the entire argument is formalized.

[16] This point, in the related context of quantum mechanical symmetry-breaking, was earlier recognized by Zeh [1975]. (To ward off any possible confusion: this is *not* Wallace's equivalence rule, but only a very special case of it.)

together also in statistical inference—in inference from observed statistics to objective probabilities, in accordance, say, with Bayesianism. This applies, above all, to confirming or disconfirming quantum theory itself; to confirming or disconfirming statements about probabilities made by the theory on the basis of observed statistics.

The difficulty in the case of the Everett interpretation is that failing an antecedent understanding of branch amplitudes in terms of probability, the predictions of the theory don't speak of probabilities at all: they speak only of branch amplitudes. The theory predicts not that the more probable statistics are such-and-such, but that the statistics in higher amplitude branches are such-and-such. Suppose such-and-such statistics *are* observed; why is that reason to believe the theory is confirmed? Or if not, that it is disconfirmed?

The principal principle normally does this job, converting probabilities, as given by a physical theory, into degrees of belief. But we lack at this point any comparable principle for converting branch amplitudes into degrees of belief. The quantum decision theory argument is in this context unavailable—it forces the principal principle only for an agent who *already accepts* that his pragmatic situation is as dictated by the branching structure of the wavefunction. If you believe that that is true, then you are already halfway to believing that Everettian quantum mechanics is true. And if you don't, then the gap is as wide as ever.

Call this the *evidential problem*. It was a relative newcomer to the debate over the Everett interpretation (Wallace [2002], Myrvold [2005]). However, a general strategy for solving the problem was rather quickly proposed by H. Greaves. Define a more general (Bayesian) confirmation theory in which the principal principle governs, not credence, which necessarily involves the notion of uncertainty, but 'quasicredence'—which, say, quantifies one's concerns (a 'caring measure'), rather than uncertainties—subject to two constraints: conditional on the proposition that E occurs with chance p, it is to be set equal to p; and conditional on the proposition that E occurs on branches with weight p, it is to be set equal to p.

If quasicredences are updated by Bayesian conditionalization, and if degrees of belief in theories (whether branching or non-branching theories) are marginals of this quasicredence function, then they behave just as one would desire in the context of rival theories. That is, the resulting confirmation theory can adjudicate between a chance theory and a weighted-branching theory, and between rival weighted-branching theories (if there are such), and rival chance theories, without prejudice to any (Greaves [2007]). Most important of all, it passes the obvious test: it does not confirm a branching theory come what may, whatever the branch weights.[17]

[17] In Chapter 9, Hilary Greaves, in collaboration with Wayne Myrvold, essentially derives this confirmation theory as a Savage-style representation theorem.

Evidently, it also bypasses the question of whether or not there is any uncertainty in the Everett picture—of whether or not Everettian quantum mechanics is a theory of probability at all—a strategy she had introduced earlier (Greaves [2004]). It has been dubbed the 'fission programme'. The evidential problem, in other words, can be solved regardless of one's views on questions of uncertainty in the context of branching. Both Deutsch and Wallace similarly avoided any appeal to the notion of uncertainty.

But now suppose, for the sake of argument, that all these arguments *do* go through. If so many of the chance-roles can be shown to be played by branching and branch amplitudes, can they not be identified with chance (Saunders [2005], Wallace [2006])? Is it any different in the identification of, say, thermal quantities, with certain kinds of particle motions? Or temporal quantities, with certain functions on a spacetime manifold? There remains the question of what sense, if any, attaches to the notion of uncertainty—given, for the sake of argument, complete knowledge of the wavefunction—but contra Greaves and others, a case can be made on the side of uncertainty too. For take a world as a complete history, and ourselves at some time as belonging to a definite world. There are vast numbers of worlds, all exactly alike up to that time. We do not know what the future will bring, because we do not know which of these worlds is our own (Wallace [2006], Saunders and Wallace [2008]).[18]

The issue is in part—perhaps in large part—a matter of how we *talk* about future contingencies. There is already a comparable difficulty in talk of the past and future, and of change, in the 'block universe' picture of four-dimensional spacetime—and plenty of scope there to interpret our ordinary talk in nonsensical terms. Lots of philosophers (and some physicists) have. For most of us, however, in that context, it is more reasonable to make sense of ordinary talk of change in terms of the relations among events ('before', 'after', and 'simultaneous'—or 'spacelike'), treating words like 'now' as we do spatial demonstratives like 'here' (Saunders [1995a, 1996b, 1998]). We make sense of ordinary talk of time and change in terms of the physics, not nonsense.

Or take the example of sensory perception: what do we perceive by the senses, if physics is to be believed? Nothing but our own ideas, according to most philosophers in the 17th and 18th centuries. We directly see only sense data, or retinal stimuli; everything else is inferred. Hence the 'problem of the external world'. Well, it may be a problem of philosophy, but as a proposal for linguistic reform it is a non-starter. That would be another example of bad interpretative practices.

A better practice, by a wide margin, is 'the principle of charity' (Wallace [2005]): interpretation (or translation) that preserves truth (or, this a

[18] I argue the case for this account of branching in Chapter 6.

variant, that preserves knowledge). But some may conclude from all this that if all that is at issue is our ordinary use of words, rather less hangs on the question of uncertainty than might have been thought—and on whether branching and branch amplitudes 'really is' probability. The success of the fission programme points to that.

But whether the fission programme can be judged a success, whether, indeed, any of the arguments just summarized really succeed in their aim, is what this book aims to discover.

2 THE ARGUMENTS OF THE BOOK

The book is structured in six parts. Parts 1 and 2 are on ontology in the Everett interpretation, giving constructive and critical arguments respectively. Part 3 is on probability; Part 4, critical of many worlds, is largely focused on this. Part 5 is on alternatives to many worlds, consistent with realism and the unitary formalism of quantum mechanics. Part 6 collects chapters that are friendly to many worlds but essentially concern something other than its defence—the origins of the theory, its reception, its open questions.

Ontology, probability, alternatives, and open questions; we take them each by turn.

2.1 Ontology

A general objection on the grounds of ontology is that decoherence theory does not do what is claimed because decoherence is only approximate and context-dependent. In some regimes it is too slow to give classicality, or it is absent altogether.

These and related arguments are addressed by David Wallace in Chapter 1. There he presents an outlook on realism in general and on 'emergence' in particular. The extraction of quasiclassical equations—a whole class of such, one for each history—is an example of FAPP reasoning as it operates across the board in the special sciences, according to Wallace. The framework is broadly structuralist and functionalist, in roughly the sense of D. Dennett's writings. It may be true that one has to know what one is looking for in advance, by means of which effective, phenomenological equations are obtained, but it is the same for extracting equations for protons, nuclei, and atoms from the field equations of the Standard Model. Likewise for quasiparticles in condensed matter physics, or (a big jump this) living organisms in molecular biology—and from thence to anatomy, evolutionary biology, and the rest. The fact that in certain regimes decoherence is absent altogether—that classicality, branching, and worlds, are absent altogether—is, says Wallace, scarcely a difficulty. It is not as though we need to recover a theory of biology for all possible regimes of molecular physics, in order to have such a theory for some.

If Wallace's reading of the extraction of classicality from the quantum is correct, it had better apply to decoherence theory as it is actually applied. In Chapter 2 Jim Hartle gives an overview of the field of decohering histories, while in Chapter 3 Jonathan Halliwell gives a detailed model in terms of hydrodynamic variables, one of the most realistic models to date. Readers are invited to judge for themselves.

A second objection is at first sight more philosophical, but it can be read as a continuation of the first. How does talk of macroscopic objects so much as get off the ground? What is the deep-down ontology in the Everett interpretation? It can't just be wavefunction, argues Tim Maudlin in Chapter 4; it is simply unintelligible to hold that a function on a high-dimensional space represents something physically real, unless and until we are told what it is a function *of*—of what inhabits that space, what the elements of the function's domain are. If they are particle configurations, then there had better *be* particle configurations, in which case not only the wavefunction is real.

Here one can hardly take instruction from the special sciences, where instrumentalism (or at least agnosticism) about ontology at deeper levels is a commonplace. In any case it would be question-begging, by Maudlin's lights, because, failing an account of what exists at the fundamental ontology, we do not have emergent structures either. But on that point Wallace and Maudlin differ profoundly.

But don't the chapters by Hartle and Halliwell prove otherwise? No—not according to Maudlin. They help themselves to resources they are not entitled to in the context of realism. Physicists indifferent to the question of realism in quantum mechanics may well speak of a function over particle configurations;[19] others may speak in the same way—doing the same calculations, even—but with a hidden-variable theory in mind. But when the topic is realism in quantum mechanics, commitments like this have to be made explicitly. Compare the situation in pilot-wave theory and collapse theories, where in recent years the question of fundamental ontology has received a great deal of attention. So, if it is denied that particles or fields exist and that only the wavefunction is real, then the wavefunction is not a function of particle or field configurations. So of what is it a function?

One can try to treat this challenge as only a verbal dispute—very well, let's speak of 'quantum-state realism' or 'structure-of-the-state realism' instead. But the objection is at bottom a request for *clarification*, for an *intelligible* account of the microworld. So what does it consist in, exactly? Or even in outline? (See also Section 3 p.44.)

[19] Hartle and Halliwell both steer clear of questions of realism in quantum foundations.

Agreed, this question of fundamental ontology is important. It is a shame that it has been paid so little attention. That is the main complaint made by David Deutsch in Chapter 18—Maudlin finds an unlikely bed-fellow. The difference between them, to put it in Bayesian terms, is in their priors: Maudlin unlike Deutsch is sceptical that any solution is possible. Maudlin sees pilot-wave and collapse theories as examples of how ontological questions should be settled, but he doubts that anything like the methods used there can apply to many worlds. There again, much of that debate has been driven by the challenge that their ontologies too contain many worlds, and devising ways by which they can be eliminated.

Is there some metatheoretic perspective available? Are there general philosophical guidelines for conducting debates like these? John Hawthorne in Chapter 5 tries, with certain caveats, to say what they might be. His is a metaphysical image to counter the naturalistic one given in Wallace's chapter. He reminds us of the long-standing concern in philosophy over how the gap between the 'manifest' image and fundamental ontology—or the 'fundamental book of the world', as Hawthorne puts it—can be bridged. He proposes a demarcation between 'conservative' and 'liberal' strategies, where the former is straightforwardly an identification of macrodescriptions with descriptions at the fundamental level. The latter in contrast involves 'metaphysical generational principles'; these he (rightly) thinks are rejected by Everettians. But if only identifications of the former kind are available, their task, thinks Hawthorne, is much harder. Typical of identificatory projects in science—by means of 'bridge principles', for example, as was popular in logical empiricist philosophy of science—are 'uncloseable explanatory gaps', bridge principles that are claimed to be true 'but you can't see for the life of you, no matter how much you look, why they are true while certain competing principles are false' (p.149). That is particularly familiar in philosophy of mind where the explanatory gap between descriptions in terms of consciousness and physicalistic descriptions is widely acknowledged. According to Hawthorne, the Everett interpretation threatens to bring with it too many new, uncloseable explanatory gaps.

But if the example is the mind–body problem, isn't functionalism precisely an answer to that? Perhaps—but there at least some input–output facts about stimulus and behaviour are uncontroversially in place. Not so in the Everett interpretation, argues Hawthorne, where 'it is hard to know what the take-home message of the functionalist is in a setting where none of the fundamental-to-macro associations are given' (p.150).

The second lesson that emerges from Hawthorne's analysis is the importance of what he calls 'metasemantical' principles—broadly speaking, theories of how semantical rules ought to operate for connecting predicates to ontology (fundamental or otherwise). One can pay lip service to the macro-image that still fails to square with one's favoured metasemantical principles—some kind of fudge is needed. Very well, so take a theory in which, say, 'all there is to the world is configuration space'. Then the best package, all things considered, is one that has ordinary macropredicates (like Wallace's example, 'tiger') pick out features of configuration space. Everettians, says Hawthorne, are then tempted to argue as follows:

> But this shows that certain features of configuration space are *good enough* to count as tigers. And then the line of reasoning proceeds as follows: 'Even if there were extra stuff—throw Bohmian particles or whatever into the mix—we have agreed that the relevant features of configuration space are good enough to count as tigers. So whether or not that extra stuff is floating around, you should still count those features of configuration space as tigers.' (pp.151–2)

But introduce 'extra stuff', and it may be its credentials to count as things like tigers simply *swamp* those of the configuration-space features that you were stuck with before.

It may be, but does it? According to James Ladyman, in his reply to Hawthorne, the credentials of the 'empty waves' of pilot-wave theory seem no better or worse than the occupied ones (we shall have to revisit this argument when we come to Part 5 of the book). Ladyman wonders too if they are mostly about philosophical intuitions that we have no good reason to trust—which are themselves the object of empirical investigation in cognitive science. But more importantly, and the point to which he devotes most attention, he thinks Hawthorne's alternative methodologies (the conservative and liberal strategies) are not exhaustive. For example, identifications in the physical sciences are generally dynamical—'it is the dynamics of how hydrogen bonds form, disband, and reform that gives rise to the wateriness of water and not the mere aggregation of hydrogen and oxygen in the ratio of two to one' (p.158). It is one of many devices used by Halliwell and Hartle that go beyond Hawthorne's two-part distinction, according to Ladyman (he lists a number of them). When it comes to the explanatory gap between the quantum world and the macroworld, contrary to Hawthorne's claim that none of the fundamental-to-macro associations are given, Ladyman concludes, 'it must be acknowledged that Halliwell and Hartle do much to close it' (p.159).

Here is an entirely different line of attack. Might some of the devices used by Halliwell and Hartle be question-begging, in view of later discussions of probability? According to Adrian Kent in Chapter 10 and Wojciech Zurek in Chapter 13, any appeal to decoherence theory must already presuppose the idea of probability. Decoherence theory employs reduced density matrices and the trace 'and so their predictions are based on averaging' (Zurek, p.414). In the estimation of Kent, it shows that certain operators 'approximately quantifying local mass densities approximately follow classical equations of motion with probability close to one . . . in other words, the ontology is *defined* by applying the Born rule' (p.338). The criticism is potentially damaging to those, like Saunders and Wallace, who seek to identify probability (or at any rate identify the quantities that a rational agent should treat as if they were probabilities) with some aspect of the ontology. Allow that the branching structure of the universal state involves objective probabilities in its definition and their arguments all but evaporate.

Zurek and Kent surely have a point: those working in decoherent histories talk freely of probabilities in their interpretation of branch structures. Witness Jonathan Halliwell in Chapter 3 in his derivation of quasiclassical hydrodynamic equations from the unitary formalism:

> The final picture we have is as follows. We can imagine an initial state for the system which contains superpositions of macroscopically very distinct states. Decoherence of histories indicates that these states may be treated separately and we thus obtain a set of trajectories which may be regarded as exclusive alternatives each occurring with some probability. Those probabilities are peaked about the average values of the local densities. We have argued that each local density eigenstate may then tend to local equilibrium, and a set of hydrodynamic equations for the average values of the local densities then follows. We thus obtain a statistical ensemble of trajectories, each of which obeys hydrodynamic equations. These equations could be very different from one trajectory to the next, having, for example, significantly different values of temperature. In the most general case they could even be in different phases, for example one a gas, one a liquid. (p.111)

But here Halliwell, like Hartle, assumes that the mod-squared amplitudes of histories can be interpreted as probabilities: he is neutral on whether all of these histories exist. Everettians at this point must speak in terms of amplitudes instead. The key question for them is whether the notion of the 'average values' of the local densities, on which the amplitudes are peaked, presupposes the notion of probability, or whether they are called the average values of the local densities because they are the values on which the amplitudes are peaked. The latter will follow if and when it is shown that the amplitudes can be interpreted in terms of probabilities—this, they say, is a task that can come *after* the delineation of the branching structure.

2.2 Probability

But is it Probability?

Chapter 6 by Simon Saunders makes the case for identifying branching and squared norms of branch amplitudes with chance processes and objective probabilities. To that end he identifies three roles played by chance. They can at best be measured by statistics, and only then with high chance; they guide rational action in the same way that objective probabilities are supposed to guide rational action, as spelled out by the principal principle; and chance processes involve uncertainty. His argument in a nutshell: all three of these roles are played by branching and branch amplitudes in Everettian quantum mechanics. Since they are more or less definitional of chance—no explanation of them is given in any conventional physical theory of probability—anything that plays all these roles should be identified with chance.

Of these the link with statistics is a straightforward dynamical question. Amplitudes cannot be measured directly in the Everett interpretation because the equation of motion is unitary. They show up at best in the statistics of repeated trials, but only on branches of comparatively high amplitude. This, says Saunders, can be uncontroversially explained.

For the argument for the link with rational action, we are referred to Wallace's chapter. The rest of Chapter 6 is on the link with uncertainty. It argues, in brief, that branching implies a form of 'self-locating uncertainty'—uncertainty as to which branch is our own. He reminds us that here there is a difficulty well known to philosophers. Suppose a large number of distinct histories are real, but that they share common parts, rather in the way that roads can overlap or, well, the way branches of a tree can overlap. Metaphysicians have considered worlds like these; they call them 'branching'. But then:

> The trouble with branching exactly is that it conflicts with our ordinary presuppositions that we have a single future. If two futures are equally mine, one with a sea fight tomorrow and one without, it is nonsense to wonder which way it will be—it will be both ways—and yet I do wonder. The theory of branching suits those who think this wondering is nonsense. Or those who think the wondering makes sense only if reconstrued: you have leave to wonder about the sea fight, provided that really you wonder not about what tomorrow will bring but about what today predetermines. But a modal realist who thinks in the ordinary way that it makes sense to wonder what the future will bring, and who distinguishes this from wondering what is already predetermined, will reject branching in favour of divergence. In divergence also there are many futures; that is, there are many later segments of worlds that begin by duplicating initial segments of our world. But in divergence, only one of these futures is truly ours. The rest belong not to us but to our otherworldly counterparts. (Lewis [1986b p.208])

The initial segments of diverging worlds are only qualitatively, not numerically, identical.

Why not just choose divergence, then? Because things are not so simple for physicalists. Their metaphysics, if they have any, is constrained by physical theory. Indeed, Everett introduced the term 'branching' by reference to the development of a superposition of records of histories, ultimately in terms of vector-space structure, not by the philosophers' criterion of overlap. According to Saunders, this concept of branches finds a natural mathematical expression in the language of the consistent histories formalism with its attendant Heisenberg-picture vectors—an inherently tenseless four-dimensional perspective. Do worlds thus represented overlap, in the philosophers' sense, or do they diverge? The answer, he says, is *underdetermined* by the physics;[20] either metaphysical picture will do. But if either can be used, better use the one that makes sense of ordinary linguistic usage, rather than *nonsense*.

But this argument for the identification of branching and branch amplitudes with objective probability is of no use in explaining the Born rule; on the contrary it depends on it. And whilst, according to Saunders, it gives an indirect solution to the evidential problem, the remaining chapters of Part 3 favour rather the fission programme due to Hilary Greaves, in which—if only as a tactical move—talk of uncertainty is eschewed. David Papineau in Chapter 7 goes further: he questions the very desirability of an account of quantum probability in terms of uncertainty. According to him rational choice theory is better off without it. The strategy of maximizing one's expected utilities, he argues, faces a difficulty if what one really wants is the best utility—but this problem disappears in the fission picture.

Adrian Kent in Chapter 10 challenges the argument for uncertainty directly. To bring in linguistic considerations, Kent insists, is simply a *mistake*: nothing of significance to fundamental physics could turn on such questions. And the bottom line, the real reason there can be no uncertainty in the face of branching, is that there is nothing in the physics corresponding to it. Take the case of Alice, about to perform a Stern–Gerlach experiment. If she were to be unsure of what to expect, there would have to be 'a probabilistic evolution law taking brain state $|\mathrm{O}\rangle_A$ to one of the states $|i\rangle_A$' (p.346). There is no such law; indeed, 'nothing in the mathematics corresponds to "Alice, who will see spin-up" or "Alice, who will see spin-down" ' (p.347). Kent disagrees with the arguments of Part 1 that there are such laws, albeit only effective laws. He disagrees with Saunders that branch vectors are just the needed mathematical quantities.

The Born-rule Theorem

In Chapter 8 Wallace provides a formal derivation of the Born rule, making it properly speaking a theorem. Mathematically inclined readers are invited to check its validity for themselves.

But are his axioms reasonable? They are in part pragmatic constraints—constraints on the range and kind of acts that are available to an agent if the branching structure of the wavefunction is what Everettian quantum mechanics says it is. Another ('state supervenience') is an expression of physicalism: it says that an agent's preferences between acts should depend only on what state they leave his branch in. Others again are more overly rationalistic—rules that are applicable more or less whatever the physical theory.

[20] A point remarked on, but not taken properly to heart, in Saunders [1998 pp.399–401]. The presumption, that Everettian branching is branching in the philosophers' sense as well, is widely shared by philosophers of physics.

Of these there are only two. The first is that an agent's preferences must yield a total ordering on his available actions. The reason for this is not so much that the claim appears plausible (although Wallace thinks it is), rather it is that 'it isn't even possible, in general for an agent to formulate and act upon a coherent set of preferences violating ordering' (p.236).

The other is 'diachronic consistency': suppose an agent on performing act U has successors all of whom prefer act V to act V'; then that agent had better prefer U followed by V to U followed by V'. The rationale is roughly the same. *Local* violations of this rule may be possible, Wallace admits; thus I disingenuously tell my friend not to let me order another glass of wine after my second; but '[i]n the presence of widespread, generic violation of diachronic consistency, agency in the Everett universe is not possible at all' (p.237). Diachronic consistency is constitutive of agency, in Wallace's view.

As for the point of the axiomatization, it is that rather than pursue largely sterile arguments over the intuitive plausibility (or lack of it) to various counterexamples to the Born rule, attention can be shifted to the general principles that putatively underlie our actual epistemic practices. To that end, for each alleged counterexample to the Born rule, Wallace identifies the relevant axiom or axioms that it most obviously slights.

Chapters in Part 4 are uniformly in disagreement with Wallace's conclusions. According to Huw Price in Chapter 12, the key problem is that in moving from one world to many there is 'something new for agents to have preferences *about*' (p.370). He gives an example from political philosophy. Use of the Born rule, in that context, would amount to a form of utilitarianism (maximizing expected utility according to a certain credence function), but to that there are well-known alternatives. Why not impose some form of distributive justice instead, in which the lot of the worse off is disproportionately weighted? This is a developed and much-debated theory in political philosophy; it is simply not credible to contend that it is *irrational*. It may be that the amplitudes will have to enter into any quantitative rule, there being no a priori count of successors, but no matter: the rule thus amended will still reflect distributive rather than utilitarian goals, and hence differ from the Born rule.

It is a good question whether Price thinks this argument is independent of the notion of uncertainty. He grants that ('subjective') uncertainty may make sense in the context of branching on a certain metaphysics of personal identity, at least as first-person expectations go (citing Wallace [2006]), but he denies that it can account for uncertainty more generally. For example, he doubts whether it makes sense for events occurring long after an agent can possibly hope to survive. And, in short, he insists that metaphysical questions of personal identity be kept separate from decision theory.

But Everettians on that point can guardedly agree. Where then is the source of disagreement? His counterexample violates one or other of Wallace's axioms, obviously. That doesn't bother Price: he concludes that that is only to show that they tacitly smuggle in presuppositions appropriate to a one-world theory. As it happens, Wallace identifies the relevant axiom as more obviously a pragmatic constraint (a continuity axiom), but the real disagreement between them is closer to the surface. Price insists that, in decision theory, considerations of rationality pertain to a *single* moment in time (the time at which a decision is made). For this reason, decision theory has nothing to do with questions of personal identity even of the most deflationary kind. Price will therefore reject Wallace's axiom of diachronic consistency directly. On personal identity, he says, '[t]hese issues are essentially irrelevant to classical subjective decision theory, for whom the only "I" who matters is the "I" at the time of decision' (p.377).

Adrian Kent in Chapter 10 seeks to undermine the Born-rule theorem at several levels. There is a problem with the very idea of 'fuzzy' ontology or theory. Kent wonders how, if the branching structure is fuzzy, the mathematical precision required of the Born-rule theorem can be sustained. Mathematical precision, moreover, is not just desirable: according to Kent, one has an 'obligation to strive to express one's ideas in mathematics as far as possible' (p.346). That is the mistake of arguments from the philosophy of language: they still bring assumptions, it is just that since expressed only in words they are the more vague. Kent speaks at this point specifically of a theory of mind. Here he rejects the broadly functionalist stance of Everettians on questions of mentality. They in turn will readily welcome mathematical models of neural processes, or for that matter linguistic behaviour, but see no special role for either in quantum foundations.

Like Price, Kent offers a number of counterexamples to the Born rule. One is the 'future self elitist', who cares only about the best of his successors ('the rest are all losers'). Another is the 'rivalrous future self elitist', who cares in contrast only about the one that is the best relative to the others—someone like this will see an advantage in impoverishing all of his successors save one. And he points out the variety of (conflicting) ways in which notions like these can be quantified. They may not be particularly edifying forms of caring, true, but they are surely not *irrational*—or not when directed at a community of other people, none of them oneself.

Kent addresses Wallace's rationality axioms explicitly. In the case of inter-temporal consistency, he concludes that whilst on some occasions an agent may reasonably be required to be consistent over time, on other occasions he may not. When an agent's utilities change over time, inter-temporal consistency, Kent thinks, is impossible. His conclusion:

> The best it seems to me that one might hope to say of diachronic consistency in real-world decisions is that pretty often, in the short term, it approximately holds. Clearly, that isn't a strong enough assumption to prove an interesting decision theoretic representation theorem. (p.342).

David Albert's criticisms in Chapter 11 chime with many of Kent's. He adds the concern that an analysis of probability in the physical sciences in terms of the betting strategy of a rational agent is to simply *change the topic*—it isn't what a theory of physical chance is about. What we should be doing is *explaining the observed statistics*—in effect, our task is to solve the evidential problem. No inquiry into the nature of the pragmatic constraints on rational actors that might follow from Everettian quantum theory can ever be relevant to *that* question. The fact that an agent is required to *believe* the theory is true for Wallace's Born-rule theorem to even get going shows it is irrelevant.

But even on its own rather limited terms, Albert continues, Wallace's arguments are unsatisfactory. Counterexamples to the equivalence rule, and therefore to the Born rule, can easily be constructed. The one Albert favours is the one generalized by Kent as the 'rivalrous future self elitist': the successors that matter are those that the agent considers better in comparison to the others, specifically by being *fatter* than the others (this gives them extra gravitas). No matter if the rule is absurd (it was intended to be funny), or difficult to carry through in practice, it is not *irrational*. Albert further insists that pragmatic constraints should have nothing to do with questions of what it is right to do. In fact, Wallace's response is that Albert's 'fatness rule' violates inter-temporal consistency rather than any of the more obviously pragmatic constraints—but, of course, the latter are needed in the deduction as well. More fundamentally: for Wallace the distinction between rationality rules and pragmatic rules is anyway only a matter of degree.

The Evidential Problem

How then is Everettian quantum mechanics to be confirmed or disconfirmed by statistical evidence? The theory only says that statistics conforming to the Born rule obtain on branches of comparatively high amplitudes, whereas anomalous statistics obtain on branches of comparatively low amplitude. How is that to be empirically checked?

Recall the answer given earlier by Greaves [2007]: a general theory of statistical inference can be defined, that applies equally to branching and non-branching theories (without prejudice to either). Very well: such a confirmation theory can be defined, but why should sceptics embrace it? In Chapter 9, in collaboration with Wayne Myrvold, she argues that they must. Greaves and Myrvold show that the process of Bayesian conditionalization (updating of credences) can itself be operationalized in terms of betting preferences, where the latter are constrained by Savage's axioms. The process of statistical inference from the outcomes of an experiment, treated as 'exchangeable' in de Finetti's sense, follows in train.

This takes some unpacking. The operational definition of an agent's conditional credences $C(E|F)$ is well known from Ramsey's and de Finetti's writings:

it is the betting quotient that an agent is prepared to accept for event E, on the understanding that the bet is called off if F does not happen. It is easy to show that unless this credence satisfies the probability axiom:

$$C(E|F) = \frac{C(E \,\&\, F)}{C(F)}.$$

a Dutch book can be constructed by which an agent is bound to lose, whatever happens. Note that the credence functions on the RHS are defined prior to learning that F.

As it stands, this says nothing about how an agent's credence function should be updated in the light of new evidence. But let this be on the model of a 'pure learning experience', in Greaves and Myrvold's terminology: then, they show, $C(.|F)$ should indeed be her updated credence function (Bayesian conditionalization). For suppose:

$P7$. During pure learning experiences, the agent adopts the strategy of updating preferences between wagers that, on her current preferences, she ranks highest.

Then in pure learning experiences an agent's preferences among wagers, in conformity with Savage's axioms and $P7$, automatically induce an ordering of preferences on updating strategies. Bayesian conditionalization comes out as optimal.

Meanwhile, Greaves and Myrvold remind us, de Finetti's original representation theorem already shows how an agent who treats the order of a sequence of outcomes on repeated trials of an experimental set-up as irrelevant (as 'exchangeable', in de Finetti's terminology), and who updates her credences by conditionalization in accordance with Bayes' theorem, is *inter alia* committed to treating the outcomes of the experiment as if they were associated with definite, if unknown, probabilities.

Putting the two together, the result is an operational characterization of the entire process of Bayesian statistical inference. It is in fact a representation theorem just as much as is the Born-rule theorem—like it or not, agents who subscribe to the axioms $P1 - P7$, and who believe certain experiments are exchangeable, *have* to act as if they were updating their quasicredence functions, in the manner proposed by Greaves [2007], and accordingly updating their credences in theories. Add the requirement that one's priors not be fixed dogmatically (they can be as small as you like, but not zero), their axiom $P8$, and the resulting confirmation theory passes a variety of non-triviality tests as well. Most importantly: it *doesn't* follow that because (in some sense) everything happens, according to Everett, the theory is confirmed come what may.

The authors' challenge is now as follows. Set up the entire system of axioms in accordance with the background assumption that one has a conventional theory of chance. Now entertain the possibility that the Everett interpretation is true. How much of the framework has to be changed? The answer, according to

Greaves and Myrvold, is 'none of it' (p.284). None of their axioms make explicit mention of uncertainty, chance, or probability (and nor, so they claim, do they do so implicitly).

To all of this a variety of the objections to Wallace's methods apply. Some of them, for example Price's counterexample in terms of distributive justice, are addressed explicitly by Greaves and Wallace (see their 'answers to objections'). But the main objection, according to Albert, is that the very focus on wagers and games is misguided. Preferences of rational agents in their gambling strategies, however regimented (as in Savage's axioms), can have nothing to do with the task of *explaining* the statistics actually observed. At most they tell us how much we should bet that we will find evidence E, if we believe a scientific hypothesis H is true, not with what the probability of E would be if H were true (what we ordinarily take as an explanation, if the probability is sufficiently high, of evidence E). And betting, in the fission picture, at least once the structure of branching and amplitudes are all known, is a matter of *caring about* what goes on in some worlds, not *beliefs about* what happens in those worlds. In Albert's words:

> But remember (and this is the absolutely crucial point) that deciding whether or not to bet on E, in the fission picture, has nothing whatsoever to do with guessing at whether or not E is going to occur. It is, for sure. And so is $-E$. And the business of deciding how to bet is just a matter of maximizing the payoffs on those particular branches that—for whatever reason—I happen to care most about. And if one is careful to keep all that at the centre of one's attention, and if one is careful not to be misled by the usual rhetoric of 'making a bet', then the epistemic strategy that Greaves and Myrvold recommend suddenly looks silly and sneaky and unmotivated and wrong. (p.364).

The objection is not quite that information about self-location can have nothing to do with beliefs about whether a physical theory is true—or if it is, it is Objection 5, as considered and rejected by Greaves and Myrvold. It is that the process of confirmation in accordance with the axioms $P1 - P8$, in the case of branching worlds, is no longer *explanatory*. Indeed, the axioms themselves may no longer be reasonable. Could they be corrected? But there may be no reasonable rules *at all* by which one can statistically test for a theory of branching worlds, say Albert and Kent. One can always concoct rules by which agents in each branch will arrive at beliefs about weights of branches, on the basis of the statistics in that branch; but they would arrive at those beliefs even if a branching worlds theory were true in which there *were* no branch weights (Kent's 'weightless' case pp.325–6).

Mightn't a similar pathology arise in a one-world theory in which there is no law, deterministic or probabilistic, governing the outcomes of experiments? Again, the inhabitants of such a world will conclude, falsely, that another theory is true—one that does assign the observed outcomes weights (namely, for experimental set-ups treated as exchangeable, weights numerically equal to the observed relative frequencies). But, says Kent, there is an important difference.

In the case of many worlds, the inhabitants of each world are led to construct a spurious measure of importance that favours their own observations against the others'. '[T]his leads to an obvious absurdity. In the one-world case, observers treat what actually happened as important, and ignore what didn't happen: this doesn't lead to the same difficulty' (p.327).

A related disquiet, as made vivid by Kent's example of a 'decorative' weight multiverse (pp. 327–8), is that in the Greaves–Myrvold approach the notion of branch weight is treated as a *primitive*, with different assignment of weights counted as different theories. On one theory they may be given by the moduli squared of branch amplitudes, but on another—possibly, a theory with identical dynamics and universal state—the weights are an entirely different set of numbers altogether. So (as Albert puts it) there is either some additional physical fact about the world (giving up on the main goal of the Everett interpretation, which is to make do with the unitary theory), or else the branch weights are some non-physical facts that are supposedly confirmed or disconfirmed by the observation of relative frequencies.

We have seen this disquiet before. It is the same as Lewis's: surely branch weights cannot, any more than objective probabilities, float free of the physical facts. They should be dictated by them essentially. But on this point, say Greaves and Myrvold, their arguments are entirely neutral (pp.397–8). The objection, if pressed, anyway can be met by the Deutsch–Wallace theorem; and if it isn't pressed, then it is hardly a difficulty of their confirmation theory that this freedom is permitted. *Something* is measured, they claim, in the way that probabilities are, by an agent who obeys their axioms: any theory that predicts the value of that quantity is thus subject to empirical test.

2.3 Not (Only) Many Worlds

The remaining parts of the book bring in wider considerations. Part 5 is on realist alternatives to many worlds consistent with the unitary formalism of quantum mechanics. They go against the claim that the Everett interpretation is forced by realism alone. Part 6 is about open questions—historical, methodological, and conceptual—inspired by many worlds.

Alternatives to Many Worlds

Wojciech Zurek in Chapter 13 sketches a picture of reality in which the quantum state has a qualified ontological status consistent with a one-world reading. It is only a sketch: he cites a sizable literature (by himself and his co-workers) for the details. From an Everettian point of view, a key difference lies in his notion of 'objective existence'. This notion only applies, according to Zurek, to 'classical' states—'einselected' states—states that can be investigated in a 'pragmatic and operational' way. 'Finding out a state without prior knowledge is a necessary condition for a state to objectively exist' (p.424). This is only possible for states that 'survive decoherence'—of which multiple copies can be extracted and distributed in the environment. Survival in this sense is 'quantum Darwinism'. Meanwhile decoherence theory is not a good starting point for understanding the origins of the classical, for (in line with his complaint already mentioned) it already involves probability. Zurek substitutes ideas from information theory instead. They, and the requirement that 'evolutions are unitary', are his core principles. From them he attempts to derive those aspects of the measurement postulates that do not involve collapse.

That seems to suggest that the Schrödinger equation has unrestricted validity. But is it true in Zurek's view that the universe as a whole can be assigned a wavefunction? He says on the one hand that to whatever extent there remains a measurement problem in his framework it is solved by Everett's relative state formalism: that explains 'apparent collapse'. He notes that 'even if "everything happens", a specific observer would remember a specific sequence of past events that happened to him'. But on the other hand:

> The concept of probability does not (need not!) concern alternatives that already exist (as in classical discussions of probability, or some 'Many Worlds' discussions). Rather, it concerns future potential events one of which will become a reality upon a measurement. (p.425)

In Chapter 14 Jeff Bub and Itamar Pitowsky offer a more overtly one-world, information-theoretic account of reality. In it Everett's ideas play no role. Quantum-state realism is rejected altogether, rather than being circumscribed as in Zurek's approach. So what does exist in their picture?

Measurements, to begin with. The key idea is not only to reject the view that the quantum state is something real; it is to reject the idea that measurement cannot figure as a primitive. They are both of them 'dogmas'. The dogma about measurement (what they call 'Bell's assertion', citing Bell [1990]) is:

> [M]easurement should never be introduced as a primitive process in a fundamental mechanical theory like classical or quantum mechanics, but should always be open to a complete analysis, in principle, of how the individual outcomes come about dynamically. (p.438)

Dispense with this and quantum-state realism and the measurement problem is exposed as a pseudo-problem.

To be more specific, the measurement problem breaks down into two parts, the 'big measurement problem', namely, 'the problem of explaining how individual measurement outcomes come about dynamically', and the 'small measurement problem', which is 'the problem of accounting for our familiar experience of a classical or Boolean macroworld, given the non-Boolean character of the underlying quantum event space' (p.438). The latter they are happy to phrase as 'the problem of explaining the dynamical emergence of an effectively classical probability space of macroscopic measurement outcomes in a quantum measurement process'. Decoherence theory is the answer to the small measurement problem; but the big measurement problem should be recognized for what it is, a pseudo-problem.

Why precisely does the big problem go away if measurements are primitive and the quantum state is a matter of degrees of belief and nothing else? Because 'probability' is a primitive too: 'probabilities (objective chances) are "uniquely given from the start" by the geometry of Hilbert space' (p.444). This, and inherent information-loss, an 'irreducible and uncontrollable disturbance', follow from a deeper principle, the 'no-broadcasting' principle.

Bub and Pitowsky ask us to rethink the ways in which realism works in the physical sciences. They make a detailed parallel with the special theory of relativity: no-broadcasting (and no-cloning) and no-signalling are analogues of Einstein's relativity and light-speed principles. Minkowski spacetime is the associated 'constructive' theory—its geometry explains Einstein's phenomenological principles. Analogously, the geometry of Hilbert space explains Bub and Pitowsky's information-theoretic principles. Just as Minkowski spacetime suffices, they say, to explain length contraction and time dilation, independent of any dynamical principles, Hilbert space suffices to explain the structure of quantum mechanical probabilities, independent of any dynamical analysis. In either case (in special relativity or in quantum mechanics) a dynamical analysis *can* be provided—but as a consistency proof, not as an explanation. In special relativity this involves the explicit construction of a dynamical model (it doesn't matter which, so long as it respects the spacetime symmetries). In quantum mechanics it is the 'small' measurement problem, answered by providing a construction in decoherence theory (it doesn't matter which, so long as it models the 'same' experiment) of an effectively classical probability space of macroscopic outcomes. It is because the latter is provided that their theory, in their estimation, qualifies as realist.

But is that sufficient? Omitted, according to Chris Timpson in his commentary on Bub and Pitowsky, is provision of a dynamical account of how one among these macroscopic outcomes is realized—precisely a solution to the big measurement problem. According to Timpson, 'forgo this and they forgo their realism'. In every other one-world realist interpretation–or revision—of quantum mechanics, there is an account of how one rather than another individual outcome comes about dynamically. The argument from no-broadcasting or no-cloning may show that measurement involves an irreducible, uncontrollable information loss, but that doesn't make it *indescribable*; there is nothing in the parallel with special relativity to support that contention. Bub and Pitowsky are entitled if they wish to reject the view that the measurement process—specifically, a process by which individual outcomes are obtained—be dynamically analysed, says Timpson, but the charge that it is a dogma is unargued. The claim that it can be eliminated, compatible with realism, is unsubstantiated. On the contrary, he insists, it is rather directly implied by realism.

The general advantages of an anti-realist view of the quantum state are pressed by Rüdiger Schack in Chapter 15. His perspective, like that of Bub and Pitowsky, is that of quantum information theory. In the context of Bayesian updating of beliefs on repeated measurements, Everettians have to *assume* that the same quantum state is prepared on each trial. This, says Schack, is a problem (the 'problem of repeated trials') that simply disappears if the quantum state is purely epistemic. Assumptions about the apparatus are still required, true, but they are part of an agent's priors, to be updated in the light of evidence. 'This raises the question of whether the concept of an objective quantum state has any useful role to play at all' (p.473), a question he answers in the negative.

At least in the pilot-wave theory we have a clear-cut one-world form of realism. Or do we? In Chapter 16 Antony Valentini responds to the argument that realism about the pilot wave implies many worlds.

His argument is in effect to grant that whatever the situation in equilibrium pilot-wave theory, in which the probability distribution of the Bohmian trajectories is as given by the Born rule, the charge does not apply to the non-equilibrium theory. And (his argument continues) there is every reason, if pilot-wave theory is true, to expect non-equilibrium behaviour, just as in classical statistical mechanics—it would be a conspiracy theory if the full range of dynamical behaviour in principle permitted by the theory were to be forever and in principle concealed.

But then, given a reliable source of non-equilibrium matter, one can perform 'subquantum' measurements, measurements that can be used to probe occupied and empty waves and can tell the difference between them. They will not behave as on a par. Pilot-wave theory considered in this way must in principle differ from Everettian quantum theory. Thus Valentini concludes:

> At best, it can only be argued that, if approximately classical experimenters are confined to the quantum equilibrium state, so that they are unable to perform subquantum measurements, then they will encounter a phenomenological appearance of many worlds—just as they will encounter a phenomenological appearance of locality, uncertainty, and of quantum physics generally. (pp.500–1)

In the presence of non-equilibrium phenomena, such observers will quickly discover the explanatory and predictive failings of these appearances. Therefore there is no reason to reify them—they are 'merely mathematical'. The 'basic constituents' of ordinary matter are the Bohmian particles, not wavepackets, or parts of the wavefunction indexed by the particles.

The reality of the pilot wave as a whole, however, is not in doubt. As Bell said, in a remark quoted by Valentini approvingly, 'no one can understand this theory until he is willing to think of ψ as a real objective field . . . even though it propagates not in 3-space but in $3N$-space' (Bell [1987 p.128]). For Valentini, the bottom line is its contingency: ψ simply contains too much contingent structure to be thought of as an elliptical way of stating a physical law.

But aren't worlds—patterns in the wavefunction—contingent structures too? And don't supposedly intrinsic properties of Bohmian particles like charge or mass (both gravitational and inertial mass) act, in experimental contexts, as if associated with the pilot wave rather than the particles? So asks Harvey Brown in his reply to Valentini. Most tellingly in his eyes:

> [T]he reality of these patterns is *not* like locality and uncertainty, which are ultimately statistical notions and are supposed to depend on whether equilibrium holds. The patterns, on the other hand, are features of the wavefunction and are either there or they are not, regardless of the equilibrium condition. (p.514)

It seems that we are at a stand-off: patterns in the wavefunction are epiphenomenal in a non-equilibrium theory of Bohmian trajectories, but Bohmian trajectories are epiphenomena in the Everettian theory of quantum mechanics. But not really: on this point experiment will decide. As Brown freely admits, if as Valentini hopes we were eventually to observe exotic statistics of the sort he predicts, 'Everettians would have to throw in the towel'. But he doubts that pilot-wave theory really offers grounds for that hope, even taken on its own terms.

Not Only Many Worlds

The final chapters in Part 6 of the book are by contrast friendly to Everett, but they break new ground. In Chapter 17 Peter Byrne tells the story of how Everett's ideas were initially received, and how they were encouraged and ignored—and, in certain respects, suppressed. In the 1950s and 1960s, the dead weight of Bohr's authority was clearly in evidence. But in David Deutsch's estimation, the level of debate scarcely improved in the two decades following. The reason? Because the worth of the theory should have been demonstrated at the genuinely quantum mechanical ('multiversial') level, apart from universes. Worlds, universes, are essentially the *classical* structures in quantum mechanics. Too much of the debate, according to Deutsch in Chapter 18, concerned realism in general, distorting scientific judgements in foundations. How odd, he asks, is this:

> Schrödinger had the basic idea of multiple universes shortly before Everett, but he didn't publish anything. He mentioned it in a lecture in Dublin (Schrödinger [1996]), in which he predicted that his audience would think he was crazy. Isn't that a strange assertion coming from a Nobel Prize winner—that he feared being considered crazy for claiming that his own equation, the one that he won the prize for, might be *true*. (p.544)

And how odd would it seem, Deutsch continues, if Everettian quantum theory were to be widely accepted, to talk of it as the 'interpretation' of quantum mechanics. It would be like talking of dinosaurs as the 'interpretation' of fossil records, rather than the things in the theory that explain them.

But Deutsch's main complaint is the same as Maudlin's: there has been too little progress with the really foundational questions about ontology in quantum mechanics. He goes further in demanding progress in a range of areas—probability in cosmology, quantum computers, relativistically covariant information flows—on the basis of an unfettered quantum mechanical realism. Progress on these fronts, he says, is what will settle the matter. Deutsch asks much of Everettians.

He has some takers. The links with cosmology are explored in more detail by Max Tegmark in Chapter 19. He compares and contrasts Everettian worlds with multiple universes as they arise in inflationary cosmology—or multiplicities, even, in a sufficiently large single universe—more or less independent of quantum mechanics. How do they differ? His list includes the evidential problem (under three headings), several aspects of the debates over probability, reasons for which other worlds are unseen, and more. The answer, he concludes, is surprisingly modest: decoherence, Hilbert-space structure, replaces spatiotemporal structure in explaining the invisibility of other worlds, and enters directly in the definition of probability, but in all other respects the issues are essentially unchanged. One thing he does not mention, however, is the question of whether uncertainty in a branching Everettian universe really is like uncertainty in the cosmological multiverse. He is (rightly, if the arguments of Chapter 6 are correct) insensitive to the distinction between diverging and overlapping worlds. But on this point Deutsch, who is clearly well disposed to the idea of overlap (and well disposed to the analogous manoeuvre in the case of classically diverging worlds of taking observers as sets of worlds, see p.202), may be disappointed.

Lev Vaidman in Chapter 20 takes up Deutsch's challenge more directly: what else is there in quantum mechanics apart from the universes? Vaidman considers a very specific suggestion. It is possible, in ordinary quantum mechanics, to introduce a backwards-evolving wavefunction coming from the future outcome of an experiment, as proposed by Y. Aharonov and his collaborators. The so-called 'two-vector' formalism has been put to practical use in the theory of 'weak' measurements (see Aharonov and Vaidman [2007] for a recent review): it should be available to Everettians too.

Or so Vaidman concludes. Of course in the global perspective of the Everett interpretation there is no *one* outcome—a backwards-evolving state must be introduced for every branching event—but in the case of measurement events, they have just the same uses that Aharonov advertised. All save one, perhaps the most important: it does not, according to Vaidman, define a time-symmetric theory. That is a disappointment. On the other hand, he speculates, the backwards-evolving vectors may perhaps also serve to underpin the notion of uncertainty. At the very least, it is a tool for the definition of a quantum event as part of a unique history.

Other items on Deutsch's list get little or no further mention. For better or worse, in this book we are still labouring over the question of 'interpretation'—if not, at least for the most part, the virtues of realism. Were familiarity, common sense, and intuition among them, no doubt the Everett interpretation would be rejected out of hand; but those were never the hallmarks of truth.

3 ADDENDUM: FROM RELATIVE STATES TO CONSISTENT HISTORIES

3.1 The Measurement Postulates

Measurements on a system S are formally characterized in terms of a self-adjoint operator O (an observable) on a Hilbert space $\mathcal{H}$ associated with S, and a state $|\psi\rangle$ (a unit vector in $\mathcal{H}$ up to phase). In practice there may be some uncertainty as to what the state actually prepared in an experiment is (in which case $|\psi\rangle$ is replaced by a density matrix), but we shall consider only the simplest case.

An observable in quantum mechanics is in turn associated with a range of real numbers (roughly, its possible values, or eigenvalues), the spectrum $Sp(O)$ of O. A measurement outcome is a subset $E \subseteq Sp(O)$, with associated projector P_E on $\mathcal{H}$. The most important of the measurement postulates is the rule: the outcome E will be observed on measurement of O when S is in the state $|\psi\rangle$ with probability $\Pr(E)$ given by:

Born rule $\Pr(E) = \langle\phi|P_E|\phi\rangle = \|P_E|\phi\rangle\|^2$.

If, further, the experiment is non-disturbing—on immediate repetition the same outcome E is reliably obtained—then the state must have been subject to the transition

projection postulate $|\phi\rangle \to |\phi_E\rangle = \frac{P_E|\phi\rangle}{|P_E|\phi\rangle|}$.

When E is an eigenvalue of O, the RHS is one of its eigenstates.

Thus for a non-disturbing measurement of O the overall evolution in the Schrödinger picture, in which the state (rather than operators) carries the time-dependence, is of the form:

$$|\phi\rangle \overset{\text{unitary}}{\to} |\phi'\rangle \overset{\text{collapse}}{\to} \frac{P_E|\phi'\rangle}{|P_E|\phi'\rangle|}.$$

In the case of disturbing measurements, if the measurement is probabilistic, that collapse still occurs (albeit the final state may be unknown) cannot be doubted. We may take it as a phenomenological given, independent even, of quantum-state realism.

The final stage of the measurement cannot therefore be modelled unitarily—unless, it may be, if the measurement is *not* probabilistic. Suppose it is indeed fully predictable. Then there is no obstacle, at least for certain kinds of states, for (say) reasonably massive and well-localized clusters of atoms in bound states, well localized in position and momentum space, to giving a unitary description of their motions. The spread of the wavepacket, for such massive systems, is negligible over the timescale of the experiment. Ehrenfest's theorem then takes on a strong form, showing that wavepackets like these approximately follow classical trajectories (see Hartle, Chapter 2). In terms of operators, approximate projections onto states like these form a commutative set of projectors, as shown by von Neumann [1932 pp.402–9]. They are what he called the 'elementary building blocks of the classical description of the world' (p.409). Whether in terms of wavepackets or projections of this form, the unitary equations imply approximately classical trajectories, for timescales much larger than those of the experiment, if the masses are sufficiently large.

To take the example of a Stern–Gerlach experiment for a *deterministic* measurement of electron spin with eigenstates $|\phi_\uparrow\rangle$, $|\phi_\downarrow\rangle$, the registration of the electron at the screen and subsequent amplification processes involve many-particle systems of the sort just described. If we start off with localized states for the 'ready' state of the apparatus $\mathcal{A}$ in state $|\psi^{\mathcal{A}}_{\text{ready}}\rangle$, with $|\psi^{\mathcal{A}}_{\text{reads spin}\uparrow}\rangle$ for the event registering 'reads spin-up', the unitary evolution is:

$$|\phi_\uparrow\rangle \otimes |\psi^{\mathcal{A}}_{\text{ready}}\rangle \overset{\text{unitary}}{\to} |\phi_{\text{absorbed}}\rangle \otimes |\psi^{\mathcal{A}}_{\text{reads spin}\uparrow}\rangle.$$

But then there is nothing, assuming the arbitrariness of the von Neumann cut, to including ever more aspects of the laboratory, including experimentalists and technicians. That is, as built out of the same von Neumann's projectors, one can model 'the observer' $\mathcal{O}$ well. Thus if initially in the state $|\xi^{\mathcal{O}}_{\text{ready}}\rangle$, one has by the unitary formalism:

$$\begin{aligned}
&|\phi_\uparrow\rangle \otimes |\psi^{\mathcal{A}}_{\text{ready}}\rangle \otimes |\xi^{\mathcal{O}}_{\text{ready}}\rangle \\
&\overset{\text{unitary}}{\to} |\phi_{\text{absorbed}}\rangle \otimes |\psi^{\mathcal{A}}_{\text{reads spin}\uparrow}\rangle \otimes |\xi^{\mathcal{O}}_{\text{ready}}\rangle \\
&\overset{\text{unitary}}{\to} |\phi_{\text{absorbed}}\rangle \otimes |\psi^{\mathcal{A}}_{\text{reads spin}\uparrow}\rangle \otimes |\xi^{\mathcal{O}}_{\text{sees spin}\uparrow}\rangle.
\end{aligned}$$

If the apparatus functions properly, and reliably detects a particle in the down state of spin $\downarrow$, a similar schema will apply to that case, when the initial state of the electron is $|\phi_\downarrow\rangle$. The unitary equations, for sufficiently massive systems in states well localized in position and momentum space, appear perfectly adequate to describe such processes—highly schematic, true, but easily refined—so long as they are deterministic.

Of course the trouble with all of this if quantum mechanics is to describe the macroworld is that experiments often *aren't* deterministic, and correspondingly, however well localized the initial states of aggregates of atoms in the apparatus, the apparatus and the observer *don't* end up in states well localized in position and momentum space. For let the initial state be of the form

$$|\phi\rangle = a|\phi_{\uparrow}\rangle + b|\phi_{\downarrow}\rangle,$$

where a and b are constants. Then by the linearity of the unitary dynamics the superposition of the two final states results:

$$a|\phi_{\text{absorbed}}\rangle \otimes |\psi^{\mathcal{A}}_{\text{reads spin}\uparrow}\rangle \otimes |\xi^{\mathcal{O}}_{\text{reads spin}\uparrow}\rangle + \\ b|\phi_{\text{absorbed}}\rangle \otimes |\psi^{\mathcal{A}}_{\text{reads spin}\downarrow}\rangle \otimes |\xi^{\mathcal{O}}_{\text{sees spin}\downarrow}\rangle$$

and this deterministic motion doesn't seem to correspond to anything. Hence the need for the collapse postulate (with the E_k's standing for 'spin ↑' and 'spin ↓').

But note how the measurement problem, on this line of reasoning, as intimated by von Neumann [1932 ch.6], and as used by Schrödinger [1935] (in terms of the 'cat' paradox) and by Wigner [1961] (in terms of the 'friend' paradox) is changed: it is that if you allow the von Neumann chain to extend well into the macroscopic, using von Neumann's building blocks, then you find the unitary equations yield a superposition of states *each one of which tells a perfectly reasonable physical story.*

3.2 Everett's Relative States

With this background in place[21] Everett's contribution, as it appeared in Everett [1957], may seem rather modest: it was to show that on *repeated* quantum measurements (using only the unitary formalism) of the von Neumann kind one obtains a superposition of states, each of which tells a physically reasonable *statistical* story—just *as if* each sequence of states were arrived at by repeated application of the projection postulate after each trial.

Modest or not, the idea required some new notation. Everett gave a model of a quantum automaton $\mathcal{A}$ which combined the functions of the apparatus and the observer, but indexed, not by a single outcome, but by a string of outcomes. Its 'ready' state is $|\psi^{\mathcal{A}}[\ldots\ldots]\rangle$. The measurement interaction is as before the von

[21] Everett had much of it: 'any general state can at any instant be analyzed into a superposition of states each of which does represent the bodies with fairly well-defined positions and momenta. Each of these states then propagates approximately according to classical laws, so that the general state can be viewed as a superposition of quasi-classical states propagating according to nearly classical trajectories' (Everett [1973 p.89]). In a footnote, Everett summarized von Neumann's construction as just discussed (but with no mention of the strong form of Ehrenfest's theorem).

Neumann model. The automaton on interacting with the system S in any of an orthogonal set of states $\{|\phi_i\rangle\}$ evolves unitarily (in the Schrödinger picture) as:

$$|\phi_i\rangle \otimes |\psi^{\mathcal{A}}[\ldots\ldots]\rangle \overset{\text{unitary}}{\rightarrow} |\phi_i\rangle \otimes |\psi^{\mathcal{A}}[_{\ldots\ldots\alpha_i}]\rangle \tag{1}$$

in which α_i characterizes the state $|\phi_i\rangle$ (say, the eigenvalue of an operator in the eigenstate $|\phi_i\rangle$). If the microscopic system is in the state $|\phi\rangle = \sum_i c_i|\phi_i\rangle$, it follows:

$$|\phi\rangle \otimes |\psi^{\mathcal{A}}[\ldots\ldots]\rangle \rightarrow \sum_i c_i|\phi_i\rangle \otimes |\psi^{\mathcal{A}}[_{\ldots\ldots\alpha_i}]\rangle. \tag{2}$$

Suppose that the system in the final state, given by the RHS of (2), is subject to the same interaction again: then there results:

$$\begin{aligned}|\phi\rangle \otimes |\psi^{\mathcal{A}}[\ldots\ldots]\rangle &\rightarrow \sum_i c_i|\phi_i\rangle \otimes |\psi^{\mathcal{A}}[_{\ldots\ldots\alpha_i}]\rangle \\ &\rightarrow \sum_i c_i|\phi_i\rangle \otimes |\psi^{\mathcal{A}}[_{\ldots\ldots\alpha_i\alpha_i}]\rangle.\end{aligned}$$

That is to say: the recorded value, on the second measurement, is precisely the same as the first, for each component of the final, total superposition—*just as if the projection postulate had been invoked at the end of the first process*.

It further follows, if there are n systems in the similarly prepared state $|\phi\rangle$, each of which is independently measured, with the results recorded by $\mathcal{A}$, that:

$$\begin{gathered}|\phi\rangle \otimes \ldots \otimes |\phi\rangle \otimes |\psi^{\mathcal{A}}[\ldots\ldots]\rangle \rightarrow \\ \sum_{i,j,\ldots,k} c_i c_j \ldots c_k |\phi_i\rangle \otimes |\phi_j\rangle \otimes \ldots \otimes |\phi_k\rangle \otimes |\psi^{\mathcal{A}}[_{\alpha_k \ldots \alpha_j \alpha_i}]\rangle,\end{gathered}$$

whereupon a (different) sequence of results is recorded by the automaton in each state entering into the final superposition—that is, in each component, there is a record of a definite sequence of outcomes, a definite statistics.

What about the outcomes themselves, apart from the records? Everett's answer was that they have values in a 'relative' sense—that for each state $|\psi^{\mathcal{A}}[_{\alpha_k \ldots \alpha_j \alpha_i}]\rangle$ in the superposition there exists its *relative* state $|\phi_i\rangle \otimes |\phi_j\rangle \otimes \ldots \otimes |\phi_k\rangle$ of the n-subsystems. There is no 'true' state of a subsystem—only a state of a subsystem, relative to a state of another subsystem. This is the essential novelty of quantum mechanics in Everett's view—in fact, it already followed from the basic structure of entanglement. He summarized the matter thus quite early in his paper:

There does not, in general, exist anything like a single state for one subsystem of a composite system. Subsystems do not possess states that are independent of the states of the remainder of *the system, so that the subsystem states are generally correlated with one another. One can arbitrarily choose a state for one subsystem, and be led to the relative state for the remainder. Thus we are faced with a fundamental relativity of states, which is implied by the formalism of composite systems. It is meaningless to ask the absolute state of a subsystem — one can only ask the state relative to a given state of the remainder of the subsystem.* (Everett [1957 p.143], emphasis original).

That seems to invite a broadly structuralist reading of the wavefunction.

Only much later in the paper did Everett revisit the question of how, precisely, these relational structures can all coexist. But at this point, following on his analysis in terms of automata, he immediately brought the question back to the invisibility of branching — that is, to the question of what is observable. But he did make a pregnant comparison:

Arguments that the world picture presented by this theory is contradicted by experience, because we are unaware of any branching process, are like the criticism of the Copernican theory that the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature because we feel no such motion. In both cases the argument fails when it is shown that our experience will be what it in fact is. (In the Copernican case the addition of Newtonian physics was required to be able to show that the earth's inhabitants would be unaware of any motion of the earth.) (Everett [1957] note added in proof.)

It was Galileo, of course, who supplied arguments as to why the motion of the earth would be unobservable, if the Copernican theory were true. But Everett might have elaborated the analogy. Equally, one might say that in a classical spacetime theory, only relative positions, relative velocities, are real; it is just as meaningless to ask for the absolute state of motion of a system as to ask for its absolute quantum state. But that suggests a rather different question than the one suggested, as a parallel, by Everett. Not, 'why is the motion of the earth invisible?', but 'what is motion?', and the comparison, not with Galileo, but with Descartes.

Descartes gave a purely relational account of motion just as did Everett of value-definiteness. It amounted to motion as rate of change of relative distances, and nothing else. As such it failed to explain the appearances — at best it only described them. Further dynamical principles were needed to pick out the privileged (relative) motions, the inertial motions.

It was the same with Everett's concept of relative states. He advocated the use of von Neumann's 'elementary building blocks', but equally he appealed to the Schmidt decomposition (see below), what he called the 'canonical representation' ([1973 p.47]). At times he wrote as if the superposition principle all by itself guaranteed the dynamical autonomy of components of the universal state ([1973 p.98]). What was missing were dynamical considerations to show that this was so — to pick out the *significant* motions.

Something more than the schematic and idealized dynamics of the von Neumann model or the kinematic Schmidt decomposition of the state was needed.

3.3 Quantum Histories

Equations of this kind were eventually obtained for a variety of many-particle systems—this the burgeoning field of decoherence theory. But dynamics can also be thought of in more structural terms, in terms of the possible histories of a physical system. That fits better with the philosophers' way of thinking of things.

Histories proper, retrodictions in quantum mechanics, were early on recognized as quite different from predicted courses of events. They could be fitted, sort of, into Bohr's interpretative framework, as shown by G. Hermann [1935], in a study that Heisenberg had encouraged. But the subject languished. However, dynamics as structures of histories arose in fields as diverse as optics and general relativity. Much of the impetus to develop Everett's ideas lay in hoped-for applications in quantum cosmology. The quantum histories formalism, as developed by R. Griffiths, R. Omnès, M. Gell-Mann, and J. B. Hartle in the late 1980s, had a variety of sources.

It does Everett nicely. Let $\{P_\alpha\}$, $\alpha = 1, 2, \ldots$ be an exhaustive, commuting set of projection operators on a Hilbert space $\mathcal{H}$, i.e.:

$$\sum_\alpha P_\alpha = I,\ P_\alpha P_\beta = \delta_{\alpha\beta} P_\alpha.$$

They may be taken to be von Neumann's 'elementary building blocks of the classical world' (in fact, if we do this we obtain a quasiclassical domain, in Gell-Mann and Hartle's sense). Let H be the Hamiltonian—again, with no explicit time-dependency. Define the Heisenberg picture operators:

$$P_\alpha(t) = e^{iHt/\hbar} P_\alpha e^{-iHt/\hbar}.$$

For the simplest example of a set of histories, consider histories constructed out of sequences of projectors in $\{P_{\alpha_1}(t_1)\}, \{P_{\alpha_2}(t_2)\}, \ldots, \{P_{\alpha_n}(t_n)\}$,[22] for a sequence of times $t_1 < t_2 < \ldots < t_n$ (the choice of sequence, like the choice of cells on configuration space, is for the time being arbitrary). An individual history α is now a particular sequence $(\alpha_1, \alpha_2, \ldots, \alpha_n)$ and is represented by a *chain* (or in Hartle's language a *class*) operator:

$$C_\alpha = P_{\alpha_n}(t_n) P_{\alpha_{n-1}}(t_{n-1}) \ldots P_{\alpha_1}(t_1).$$

[22] Jim Hartle in Chapter 2 considers more general histories, in which different families of projectors are chosen at different times (with corresponding superscripts on the $P_{\alpha_k}(t_k)$'s).

The operators $C_\alpha^\dagger C_\alpha$ are self-adjoint and positive, but they are not projectors. Acting on the state $|\Psi\rangle$ at $t = 0$ we obtain the *branch state vector* $C_\alpha|\Psi\rangle$. It is the same as the vector (time-evolved back to $t = 0$) that *would* have been obtained in the Schrödinger picture by a sequence of non-disturbing measurements (using the measurement postulates), first of the projection $P^1_{\alpha_1}$ at time t_1 (collapsing onto the vector $\Psi_{\alpha_1}(t_1) = P_{\alpha_1} e^{-iHt_1/\hbar}|\Psi(0)\rangle$), then of the projection P_{α_2} at time t_2 (collapsing onto the vector $\Psi_{\alpha_2\alpha_1}(t_2) = P_{\alpha_2} e^{-iH(t_2-t_1)/\hbar} P_{\alpha_1} e^{-iHt_1/\hbar}|\Psi(t_1)\rangle$), and so on, with modulus square equal to the product of the probabilities for each collapse (as calculated using the measurement postulates). That is, the probability $p(\alpha)$ for a history α is the modulus square of the branch state vector $C_\alpha|\Psi\rangle$

$$p(\alpha) = \| |C_\alpha|\Psi\rangle\|^2 = Tr(C_\alpha \rho C_\alpha^\dagger) \tag{3}$$

where $\rho = |\Psi\rangle\langle\Psi|$ is the density matrix for the state $|\Psi\rangle$ and Tr is the trace ($Tr(O) = \sum_k \langle\phi_k|O\phi_k\rangle$, for any operator O and orthonormal basis $\{\phi_k\}$ over $\mathcal{H}$). Likewise, one can define the conditional probability of α (for $t_n < \ldots < t_{k+1}$) given β (for $t_k < \ldots < t_1$) as

$$p_\rho(\alpha/\beta) = \frac{Tr(C_{\alpha*\beta}\rho C^\dagger_{\alpha*\beta})}{Tr(C_\beta \rho C_\beta^\dagger)}, \tag{4}$$

where $\alpha * \beta$ is the history comprising β (up to time t_k) and α (from t_{k+1} to t_n).

But this interpretation of the quantities $p(\alpha)$, $p(\alpha/\beta)$ as probabilities in the context of the Everett interpretation needs justification. In general, for arbitrary choices of families of projectors $\{P_{\alpha_k}\}$, they have nothing to do with probabilities. The use of the trace in Eqs (3) and (4) is no more than a formal device for extracting squared norms of amplitudes and transition amplitudes; they are relations in the Hilbert space norm, defined—deterministically defined, note, under the unitary equations—to facilitate the structural analysis of the state. At this stage they need mean nothing more.

But we may help ourselves to their obvious structural meaning, when these transition amplitudes are zero or one. We thus talk of anticorrelations and correlations among the associated sequences of projectors, and by extension, the configurations α and β on which they project. In the single-stage case, suppose the latter pertain to different systems, represented by projectors of the form $P_\alpha \otimes I$, $I \otimes P_\beta$. Let $p_\rho(\alpha/\beta) = 1$ and let $\rho = |\Psi\rangle\langle\Psi|$. In the special case where P_α and P_β are one-dimensional with ranges $|\alpha\rangle$, $|\beta\rangle$, then $|\alpha\rangle$ is the relative state of $|\beta\rangle$ in the state $|\Psi\rangle$, in Everett's sense. More generally: if $p_\rho(\alpha/\beta) = 1$ then α is the 'relative configuration' of β in $|\Psi\rangle$.

Here is a connection with the Schmidt decomposition. It is a theorem that for any vector Ψ in the tensor product Hilbert space $\mathcal{H}^{\mathcal{A}+\mathcal{B}} = \mathcal{H}^{\mathcal{A}} \otimes \mathcal{H}^{\mathcal{B}}$ of two systems $\mathcal{A}$ and $\mathcal{B}$, there exists orthonormal basis $\{\phi_k\}$ in $\mathcal{H}^{\mathcal{A}}$, and $\{\psi_k\}$ in $\mathcal{H}^{\mathcal{B}}$, and complex numbers c_k such that

$$|\Psi\rangle = \sum_k c_k|\phi_k\rangle \otimes |\psi_k\rangle. \tag{5}$$

If for $k \neq j$ $|c_k| \neq |c_j|$, then the bases $\{\phi_k\}$ in $\mathcal{H}^{\mathcal{A}}$ and $\{\psi_k\}$ in $\mathcal{H}^{\mathcal{B}}$ are unique. Eq. (5) is the Schmidt decomposition. If these bases diagonalize P_α and P_β respectively, then (for any dimensionality)

$$\sum_{k;\, P_\alpha|\phi_k\rangle=|\phi_k\rangle} c_k|\psi_k\rangle$$

is the relative state of $P_\alpha \sum_k c_k|\phi_k\rangle$ in the state $|\Psi\rangle$. Given this condition, relativization in Everett's sense is a symmetric relation.

3.4 Coarse-Graining and Consistency

The notion of coarse-graining of a parameter space (like configuration space) extends naturally to chain operators, as follows. Let $\{\overline{\alpha}\}$ be a coarse-graining of $\{\alpha\}$, so that each finer-grained cell α is contained in some coarser-grained cell $\overline{\alpha}$ in the parameter space. We can then speak of coarser- and finer-grainings of histories too. Now consider a set of histories with chain operators $\{C_\alpha\}$, and a coarse-graining with chain operators $\{C_{\overline{\alpha}}\}$. Then the two are related by summation:

$$C_{\overline{\alpha}} = \sum_{\alpha \in \overline{\alpha}} C_\alpha$$

where the sum is over all finer-grained histories α contained within $\overline{\alpha}$.

Now for a candidate fundamental ontology (Saunders [1994, 1995]): it is the system of correlations and transition amplitudes among values of self-adjoint dynamical variables and their coarse-grainings—in quantum mechanics, among values of particle variables, in quantum field theory, among values, local in space and time, of field densities. The latter mirrors, roughly, the fundamental ontology in classical general relativity theories, in terms of invariant relations among values of metric and matter fields.

As in general relativity, some order can be introduced by a formal condition. Given a Lorentzian geometry it is useful to introduce a foliation to a manifold—a collection of global three-dimensional surfaces whose tangent vectors are everywhere spacelike. It is useful, considering the structure of a quantum state, to consider families of projectors for which branch state vectors, for histories neither of which is a coarse-graining of the other, are approximately orthogonal:

$$\langle C_\alpha \Psi | C_{\alpha'} \Psi \rangle \approx 0, \; \alpha \notin \alpha' \text{ and } \alpha' \notin \alpha. \tag{6}$$

Such histories are called *consistent* (by Griffiths and Omnès); *(medium) decoherent* (by Hartle and Halliwell). Given consistency, Everett's relativization is a transitive relation even in time-like directions (Saunders [1995b]; it is automatically transitive in spacelike directions by virtue of microcausality).

The coarse-graining of histories exploits Hilbert-space structures, notably, the Boolean algebra of the projectors used to generate those histories. If this is used to turn a history space into a probability space (a Borel space), equipped with a σ-algebra, then the measure must be additive with respect to coarse-graining:

$$p(\overline{\alpha}) = \sum_{\alpha \in \overline{\alpha}} p(\alpha). \tag{7}$$

The analogous condition for the Schrödinger picture state (essentially, single-time histories) is automatically satisfied, given Eq. (3) (Everett turned this reasoning around: assuming additivity, he derived Eq. (3)); it is satisfied by two-time histories as well. But in the general case it fails. The consistency condition as originally defined is the necessary and sufficient condition for additivity in the sense of Eq. (7); the condition as specified, Eq. (6), is slightly stronger but somewhat simpler—this is the condition that is widely used.

It follows too that for any consistent history space there exists a fine-graining $\{P_\alpha\}$ which is consistent and for which, for any $t_n > t_m$ and for any α_n with $P_{\alpha_n}(t_n)|\Psi\rangle \neq 0$, there exists exactly one α_m such that

$$P_{\alpha_n}(t_n) P_{\alpha_m}(t_m) |\Psi\rangle \neq 0$$

(Griffiths [1993], Wallace [2010]). That is, for each α_n at time t_n, there is a *unique* history preceding it—the set of histories can be fine-grained so as to have a purely branching structure (with no recombination of branches). The connection, at this point, with the Aharonov two-vector formalism is immediate (see Vaidman's Chapter 20).

The consistency condition and the quantum histories formalism is widely advertised as providing a generalization of quantum theory as, fundamentally, a theory of probability. As such there is a continuum infinity of consistent history spaces available—new resources, for the exploration of quantum systems, indeed. But from the point of view of Everettian quantum mechanics, consistency is far too weak a condition to give substance to the notion of histories as autonomous and robust dynamical structures, and probability, as associated with branching of such structures, is too high level a concept to figure in the foundations of quantum mechanics. At any rate, consistency holds automatically given decoherence in the sense of quasiclassicality (or realms more generally), itself only an approximate condition, but still our abiding criterion for the existence of worlds.

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