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Worlds Galore?

Essay Review of: *Many Worlds? Everett, Quantum Theory, & Reality*, edited by Simon Saunders, Jonathan Barrett, Adrian Kent, and David Wallace (Oxford University Press, 2010; xvi+618 pp.).¹

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The 50th anniversary of Everett's 1957 paper has brought in its wake the publication of a number of important books on Everett's life, work and theory. This book provides arguably the most vivid and comprehensive treatment of both state-of-the-art developments within and criticism of the Everett interpretation (hence the question mark in the title!). By 'vivid' I mean a display of argument and eloquence in papers and discussions that at times makes the book literally difficult to put down. The papers were originally given at the two main conferences that marked the Everett anniversary, at the University of Oxford and at the Perimeter Institute for Theoretical Physics (Waterloo, Ontario).² The end result, published in 2010, is a wonderful achievement by the four editors, Simon Saunders, Jonathan Barrett, Adrian Kent and David Wallace, who have managed to combine a vast breadth of diverging views (including their own) into a volume that is a must for anyone interested in the Everett theory.

Chapter 1 is a masterly introduction by Simon Saunders, almost 50 pages long, in which he explains and comments on the material in the book. This introduction is both detailed enough to be a really useful guide to the rest of the book and very even-handed in its treatment of contrasting positions.

Part I (Chapters 2–4, by David Wallace, Jim Hartle, and Jonathan Halliwell) is devoted to discussing the theory of decoherence and its role in modern-day Everett theory. Indeed, it is only with the advent of decoherence – specifically in its ability to create temporally stable structures at the level of components of the wavefunction – that the problem of the 'preferred

¹ It is impossible to list all the people to whom I am indebted for innumerable conversations on Everett. These have shaped my understanding of the subject over the years and include conversations with most of the authors and discussants represented in this volume (as well as others), notably David Albert, Harvey Brown, Hilary Greaves, Jenann Ismael, Wayne Myrvold, Huw Price, Antony Valentini and David Wallace, but above all Simon Saunders, from whom I have learnt most about Everett over the past twenty years or so, and without whose dedication and insights the Everett interpretation would still be in the sad state it was until the late 1980s.

² Of these, I was fortunate to personally experience the Oxford conference.

basis' in Everett (or, more aptly, of the democracy of relative states) has achieved a solution commanding a broad consensus.³

Wallace's chapter is already a classic, and arguably the place to look if one wants to understand the modern-day approach to the ontology of the Everett theory, which is best described by borrowing Dennett's term of 'real patterns' as applied to the structures arising from decoherence within the universal wavefunction. (Throughout this review, when I say 'Everett interpretation' or 'Everett theory' I shall generally mean the decoherence-based approach to Everett's ideas.)

Hartle's and Halliwell's chapters review theoretical aspects of decoherence in the context of the 'decoherent histories' formalism (some of which are highly informative and not much discussed in the philosophy literature – the present author not excepted). This formalism is particularly useful if one is discussing diachronic aspects of decoherence. (A review of a different line of developments originating from decoherence theory is given by Wojciech Zurek in Chapter 13.) Hartle's main concern is with the derivation of quasi-classical realms within quantum theory (including the emergence of classical spacetime). Halliwell's is with the notion of decoherence as arising from conservation laws, as opposed to the more familiar notion of environmentally-induced decoherence. The former notion gives rise to a structure of largely parallel worlds within the universal wavefunction, which is somewhat in contrast with the branching structure usually associated with the latter (and which is arguably the main tool in Wallace's discussion). It seems to me that environmental interactions give rise to much more robust histories and much more interesting ones. There is a sense in which nothing happens in worlds arising from conservation-based decoherence, so rather than structure one has perhaps identified lack thereof. (Even the emergence of classical spacetime may be problematic, if it does not contain any concrete events.) Thus, perhaps, further ingredients are required to provide a fully satisfactory analysis (e.g. Zurek's notion of 'quantum Darwinism', see below).

When above I mentioned 'broad consensus', I meant consensus among Everettians. Indeed, Part II is devoted to criticisms of an ontological picture that is based solely on the wavefunction (or even on a state in an abstract Hilbert space) and patterns within it (Chapters 5 and 6, by Tim Maudlin and John Hawthorne, followed by James Ladyman's reply to Hawthorne and by a partial transcript of discussions relating to Parts I and II). Some of the arguments discussed in these chapters are relevant also to the opposition between Everett and alternative approaches to the foundations of quantum mechanics (in particular pilot-wave theories, an opposition well represented in Part V by Antony Valentini's Chapter 16 and Harvey Brown's reply⁴).

³ Decoherence is crucial in this respect whether one thinks of Everett primarily in terms of 'worlds', i.e. stable structures at a global level, or of 'minds', i.e. in terms of the local decohering variables on which minds arguably supervene (see the further remarks at the end of this review).

⁴ It is equally relevant to the opposition between Everett and spontaneous collapse theories, which is not treated explicitly in this volume (but see the remarks on collapse theories in Maudlin's chapter). There is in fact a line of criticism of collapse theories that the Everettian can adopt (a worrying version of the so-called 'tails problem') that is quite analogous to their critical stance towards pilot-wave theories. The opposition between

Maudlin is very much aware of the kind of game that Everettians (but by no means only Everettians) are engaging in. Given a theory (in this case unitary quantum mechanics), we need to analyse what structures in it play what role, in particular whether the theory contains any structures that behave like the things we wish to model. (Is there something in the theory that behaves like a tiger?) Even granting this structuralist strategy, Maudlin believes that not enough attention has been paid to actually showing that this strategy works in the case of a theory that is solely about the behaviour of rays in an abstract Hilbert space (whether or not this behaviour is unitary or includes a collapse mechanism), and he is sceptical about its chances of success, so that further structure needs to be postulated. I agree that more work could be done on the structural analysis of Hilbert-space, in particular on the issue of the emergence of classical spacetime and of configuration space (e.g. the constitution of spacetime events, the concept and role of quantum reference frames etc.). Indeed, it is no more respectable to help oneself uncritically to the configuration-space representation of the quantum state as it is to help oneself uncritically to some particular decomposition of a density matrix. On the other hand, I believe that crucial aspects of this work are already present in the literature, many of them in the literature on decoherence.⁵ If I understand Maudlin correctly, however, he goes even further and disagrees with a purely structuralist strategy, requiring even of structures that are present (perhaps emergently) in the theory that one postulate which ones are of fundamental ontological import (so-called ‘primitive ontology’).⁶ This is a deep disagreement with Everettians and with structuralists in general (cf. pp. 167-168): how could e.g. the potentials in the Schrödinger equation ever be structured as if they were functions on 3-dimensional space, if this space has no independent existence? (Or turning intuitions around: how could an independently existing space ever determine the form of equations defined on Hilbert space?) I do believe, however, that even if one postulates a primitive ontology one needs to do the same kind of careful structural analysis in order to show that this ontology indeed plays the role that it is supposed to play (as Maudlin himself states: it must not be ‘invisible and intangible’), a partial example of which would be the discussion of subquantum measurements in pilot-wave theory in Valentini’s Chapter 16.

Hawthorne’s chapter is rich in interesting points (taken up in later discussions), but the metaphysical background against which they are formulated is perhaps typical of a general

Everett and spontaneous collapse theories is however more commonly seen as straightforwardly empirical (as part of the alternative between collapse and no-collapse).

⁵ A few relevant aspects are: the significance of certain operators as generators of the symmetry group of the fundamental equations (whether Galilei or Lorentz), representations of the fundamental symmetry group, systems of imprimitivity, techniques for ‘dressing’ systems, techniques for constructing coherent states, the role of such states in decoherence, the emergence of classicality from decoherence, the emergence of spacetime from causal structure (perhaps applied to the concrete events of decoherence), the abstraction of spacetime from the behaviour of classical (or quasiclassical!) physical systems, i.e. reference frames, Poincaré’s insights into the nature of space and their generalisation by Harvey Brown, the work of Julian Barbour, and so forth.

⁶ For instance, in the GRW theory the Schrödinger evolution is interrupted by ‘hits’ (multiplications with Gaussians defined on 3-d space). According to Maudlin, one needs to specify a primitive ontology for the theory, and his favoured option is to promote the ‘hits’ to the status of primitive ontology (GRW theory with ‘flash’ ontology). For a structuralist, flashes are already part of the structure of the theory (they are part of the structure of the equations), and are thus already available (among other things!) as material for analysing whether the theory contains structure that behaves in the right way to describe the everyday world.

disconnection with science in contemporary metaphysics, an Achilles' heel exploited by James Ladyman in his reply.

With Parts III and IV (Chapters 6–9, by Simon Saunders, David Papineau, David Wallace, and Hilary Greaves and Wayne Myrvold, and Chapters 10–12, by Adrian Kent, David Albert, and Huw Price, followed by a partial transcript of the discussions) we turn to the most debated topic of recent years, namely probability in the Everett theory. Two developments have dominated this debate: first and foremost the Deutsch-Wallace derivation of the Born rule from decision-theoretic axioms for an Everettian agent, second the question of whether there is a sensible theory of confirmation that could be applicable to Everett and non-Everett theories alike.

We shall discuss some aspects of these questions in further detail below, but here is a brief orientation for the reader. At least *prima facie* there is a serious problem of how to make sense of probabilities in Everett, since all outcomes of an experiment actually occur. The intuition behind the work of Deutsch and Wallace, described in Chapter 8 (and of Greaves and Myrvold, described in Chapter 9) is that it still makes sense for an Everettian agent to have a preference ordering over bets; this leads to a Savage-style representation theorem in which an agent's preferences are captured by maximising expected utility, with a measure over branches playing the role of the credence measure. Deutsch and Wallace go even further, providing axioms that constrain this measure to be uniquely given by the Born rule (the squared amplitude of the branches). Thus, while for Savage (and arguably in the context of Greaves and Myrvold's approach), the probability measure in the representation theorem could be purely subjective, Deutsch and Wallace claim there is something in the physical world that provides a rational guide for our choice of measure in maximising expected utilities. In the terminology of David Lewis's Principal Principle, such a thing is objective chance.⁷

Simon Saunders' Chapter 6 discusses the problem of probability precisely as the problem of whether one can identify objective chances in Everett, arguing that this task has been solved in Everett at least as well as in the classical case. The main stress in Saunders' chapter, however, is on the question of whether the measure arising in the representation theorems can be thought of as expressing genuine credences (i.e. degrees of belief in the presence of genuine ignorance), or merely the degree to which the agent cares about the different future branches.⁸ Many workers adopt the 'caring measure' approach (which fits better with a 'fission picture', i.e. with 'splitting' agents), while Saunders argues that adopting an 'ignorance picture' is both consistent and does a better job at making sense of everyday talk (bearing in mind that if the Everett theory is true, many everyday alternatives are in fact cases

⁷ Wallace in Chapter 8 among others states that the Deutsch-Wallace approach derives the Principal Principle within the Everett theory. It may be more accurate to state that the principle is accepted as a definition of objective chances and that what is derived is the existence of the latter.

⁸ Note that this is distinct from the question of what utilities to attach to rewards in different branches, as made clear for instance in David Albert's discussion in Chapter 11 (p. 361): the question is not about what utility to attach to winning or losing 1\$ *per se*, but about whether and how much it should matter that I win or lose 1\$ in various kinds of branches (in particular branches with various squared amplitudes).

of Everettian branching).⁹ David Papineau in Chapter 7 takes a slightly different tack, arguing that the situation in the philosophy of classical probability is in such terrible shape that Everett is not so badly off after all. More specifically, he argues that the availability of an ignorance picture does not help even in the classical case (so that Everettians can easily do without it), and that the rationality of letting objective chances (which he reads as propensities) guide credences cannot in principle be shown even in the classical case (so that Everettians need not worry even should the Deutsch-Wallace results not be as relevant as claimed¹⁰). The problem of making sense of probabilities is touched upon also in Part V, in Rüdiger Schack's Chapter 15, and Part VI, both in Max Tegmark's Chapter 19 and Lev Vaidman's Chapter 20.

The nitty-gritty of the current Everettian approaches to probability is then spelled out in Chapter 8 by David Wallace, and Chapter 9 by Hilary Greaves and Wayne Myrvold. Wallace gives a fully axiomatised version of the Deutsch-Wallace theorem (with the advantage, in Wallace's view, of highlighting the locus of disagreement with proponents of serious or even just polemic alternatives to the use of the Born rule in Everett). The final gloss Wallace gives to his derivation is very suggestive: in the Everettian case, symmetry considerations about probability go through to the very end, while in the classical case they are ultimately frustrated by something that breaks the symmetry (and makes only one alternative happen). Greaves and Myrvold discuss their own representation theorem (which allows for more general measures than the Born rule to be used in a many-worlds setting) and their confirmation theory, which applies uniformly to many-worlds and one-world theories. (More below on this issue.)

Criticism is then heaped on the decision-theoretic approaches and on the theory of confirmation in Chapters 10–12 by Kent, Albert and Price. Readers impressed by the beauty and power of the arguments and derivations in the previous chapters will be shaken by the eloquence and persuasiveness of the counterarguments in these further chapters. Kent provides a veritable broadside of counterarguments (some based on ingenious toy examples of many-worlds theories). I think Kent's arguments against the ignorance picture are very strong, as are the ones arguing that the meaning of the branch weights is crucial to the possibility of their use in confirmation theory.¹¹ David Albert argues in his usual vivid way that (in the absence of a viable ignorance picture, insightfully discussed in his section 3.2), the Everett theory lacks the power to explain our empirical experience (of the Born rule), regardless of the fact that it may give us a rational guide for behaviour in betting scenarios.

⁹ I have similar worries with the ignorance picture as with decoherence based on conservation laws: the structure is there in the mathematics, but it does not seem to play any interesting role, at least in terms of the physics. In the case of the ignorance picture, there is an obvious role it can play in the metaphysical picture, but is that enough to make it a 'real pattern'? (Thanks to Alistair Wilson for discussion of this point.) The situation is different in the two-vector formalism proposed by Lev Vaidman in Chapter 20 (see below).

¹⁰ And they might even be in a slightly better position, since according to Papineau they need not explain why maximising expected utility should be a rational strategy for an agent aiming for gains in a single actual world.

¹¹ Others may be defused more easily: e.g., Kent points out that in a theory without objective branch weights, Greaves-Myrvold observers would end up confirming a different theory about branch weights in each world (all equally wrong). But this is just Greaves and Myrvold's own point about the incoherence of the actual frequentist approach to probabilities in a many-worlds setting.

According to Albert, this vitiates the use of Everettian probabilities in confirmation theory (but see our discussion below). Nor does he believe the Deutsch-Wallace approach makes a compelling case for the rationality of the Born strategy (as he funnily illustrates with his alternative ‘fatness’ measure). Huw Price in the following chapter sketches a very appealing alternative picture of probabilities, namely a pragmatist view (i.e. a subjectivist view). He then goes one step further in the criticism of the Deutsch-Wallace approach: the issue, he argues, is not about agreement or disagreement about single decision-theoretic axioms, but about how to set up the decision-theoretic framework in the first place. In a many-worlds theory, agents (before a branching event) have no reason to limit their preferences over possible actions to ones defined in terms of their preferences for outcomes within the future branches. Such agents have more things to have preferences about (specifically, properties of the global quantum state after branching), and this changes the perspective from which to judge the rationality of decision-theoretic alternatives to the Born rule. The published transcripts highlight both the subtlety and the sensitivity of issues surrounding probability (Itamar Pitowsky on p. 405: ‘There’s no sense in which you’re unlucky, that’s the point’). I would like to emphasise in particular Myrvold’s take on the Deutsch-Wallace approach (p. 400), namely that, in a specific sense, the Born weights are the only natural ones that can be defined using nothing but Hilbert-space structure (so the theorem is conceptually akin to Gleason’s theorem), but that this falls short of showing the rationality of adopting them even in a many-worlds setting.

Let me add a few remarks on probabilities in Everett. First of all, I think the evidential problem can be reduced to the decision-theoretic one (even in the fission picture), *irrespective* of whether the decision-theoretic probabilities are genuinely explanatory. Indeed, assume that I believe in the Everett theory, and that the truth of the Everett theory gives me compelling rational reasons to adopt the Born rule as the optimal caring measure for my successors, and I arrange things for my successors accordingly. In worlds in which things do not work out (e.g. I keep losing money), I will start doubting whether the strategy I have been following in caring for my successors was indeed the rationally optimal one. By *modus tollens*, I shall start doubting the truth of the Everett theory (and might start believing more in the pilot-wave theory with a non-equilibrium distribution). Thus, caring measures (despite the fact that they are not claimed to be explanatory of this or that outcome) can be relevant to updating good old degrees of belief (in the assumed theory), at least if we can uphold the strong connection between truth of the theory and adoption of a specific rational betting strategy as in the Deutsch-Wallace approach.

Especially in view of the criticisms by Kent, Albert and Price, however, maybe the link between the probabilities used in decision theory and in confirmation theory should be severed. In the context of confirmation there is probably scope for using Vaidman’s notion of ignorance (the one of the post-experiment observer who has not yet seen the result). Indeed, in confirmation theory we can focus on whether *existing* evidence makes a difference to our belief in a theory. This post-experiment in-world perspective could justify the assumptions (or analogous ones) needed for the existing representation theorems, but now applied to confirmation-theoretic probabilities. The decision-theoretic problem could then be treated

independently, allowing one to take fully into account the additional possibilities emphasised by Price.

There is a somewhat startling consequence one can draw from the Greaves-Myrvold scheme. Recall that (as we shall see below when discussing Valentini's chapter), the predictions of the Everett theory and of pilot-wave theory *under the assumption of equilibrium* are the same, so that from the purely empirical point of view the two theories in this case are indistinguishable. The Greaves-Myrvold framework allows for the use of non-Born (quasi)credence functions while retaining a many-worlds setting. (At least) some such (quasi)credence functions can be generated by taking non-Born distributions and plugging them into de Broglie's guidance equation. Thus the case of non-equilibrium distributions in pilot-wave theory becomes empirically indistinguishable from (and confirmed or disconfirmed to the same degree as) non-Born many-worlds theories in Greaves and Myrvold's framework. The upshot is that, if one adopts their framework, the empirical underdetermination between pilot-wave theory and Everettian many-worlds is generalised also to the non-equilibrium case. One could take the resulting epistemological mess as an argument against Greaves-Myrvold. What I believe saves the day, however, are the non-empirical criteria of theory choice that would be used to break the tie. In the strict equilibrium case, Everettians might conceivably have an advantage of 'economy' over the pilot-wave theorist (see below). But in the non-equilibrium case, the probabilistic predictions that turn out to be confirmed are not given by the quantum state, and it is explanatorily more powerful to assume that there is additional structure present, namely ensembles of systems in configurations distributed (more or less) according to the observed frequencies.

This brings us to the issue of whether the decision-theoretic probabilities in Everett are at all explanatory of observed frequencies in our (initial segment of?) world, which conforms to the Born probabilities to an extremely high degree. (In this sense, our world is not just 'like all the others'.) I shall take the lead from David Papineau, comparing the situation in Everett with that in the classical deterministic case, but I shall focus on the role of typicality (rather than on analysing probabilities in terms of frequencies or propensities). In the classical deterministic case (think of coin tossing, or of classical statistical mechanics), we start with a subjectivist analysis of probabilities and seek to establish whether there are also objective chances in the sense of the Principal Principle. And it would seem that even the most committed subjectivist is happy to use certain short-cuts in setting their priors: in the case of coin tossing we might want to check whether the weight distribution in the coin seems loaded, in the case of statistical mechanics we have theoretical and dynamical considerations we bring to bear on the problem (temporal invariance, ergodic-type considerations etc.). These are all things that are 'out there' in the world (whether as properties of objects or as properties of the dynamics). From the pragmatic standpoint, we are certainly justified in using these short-cuts: they are relatively easy to implement (perhaps adding a conditionalisation on assumed initial entropy levels), and they generally lead to good results (in which case updating on new evidence gives us no reason for substantially modifying our priors). If this justification counts as rational in the sense of the Principal Principle, we are

even justified in calling these probabilities objective chances (although this is clearly open to doubt, cf. the problem of induction).

This picture translates straightforwardly to the Everett case, at least as long as we take probabilities as guides for our in-world expectations (cf. both Price's discussion of the pragmatist view of probabilities,¹² and his critique of preferences based on in-world rewards in Chapter 12). We make an educated guess at setting our priors according to the Born rule (perhaps guided by considerations of symmetry or naturalness – cf. Myrvold's take on the Deutsch-Wallace theorem), and as long as it works for us (in our world or initial segment thereof), we have no reason to change our 'theory of chance'.

But are these 'objective' chances (or these pragmatic short-cuts) *predictive* or *explanatory* of our continuing or past success in our world? This is where Everettians are most hard-pressed (even independently of the question of whether the Deutsch-Wallace approach provides compelling rational reasons for adopting the Born rule). But they might be able to argue also in this case that Everett is no worse off than a classical determinist. For even in the classical case, no matter what good reasons we might have to adopt a certain probability measure, there will always be initial conditions that lead to deviant relative frequencies. In order to be able to predict or explain 'good' long-run behaviour, we have to assume that the detailed initial conditions are not 'atypical'. One could argue that it would be irrational to adopt a probabilistic model if one were not to assume also that the actual world is typical, but that also seems to be a (merely) pragmatic consideration. If all typicality means is the assumption that the initial conditions are such that they lead to the desired behaviour, then it is hardly explanatory of that behaviour.

This is passing a rather harsh judgement on the typicality approach to probability, but Everettians could push that line in arguing that they are actually no worse off than in the classical case. (Indeed, on p. 393 Saunders seems to be using the 't' word precisely in this context: 'that's the language in which the amplitudes are explanatory – they quantify "typicality"'.)¹³ It seems to me that this line of thought, de-emphasising as it does the explanatory role of probabilities, on the whole favours a pragmatist reading of probabilities in Everett.

The subsequent Part V is entitled 'Alternatives to Many Worlds' (although the results described by Wojciech Zurek in Chapter 13 are largely compatible with Everett). Chapter 14 by Jeff Bub and Itamar Pitowsky (with a reply by Chris Timpson) and Chapter 15 by Rüdiger Schack (possibly grouped with each other - and together with Zurek's chapter - because of a

¹² As pointed out to me in personal communication by Price, his discussion in Chapter 12 is *contrasting* pragmatist probabilities and Everettian ones. What I am taking to be the Everettian translation of the pragmatist strategy is to take the Born rule as a guide for our in-world expectations *even though* the numbers it yields may not be subjective degrees of belief (if one rejects the ignorance picture).

¹³ Note that Saunders advocates the ignorance picture of branching, where the analogy is closer. At least one disanalogy, however, remains between the Everett case and the classical deterministic case: in the latter (in the language of the old D-N model of explanation) one can pack more of the explanatory work into the dynamical laws, and push any residual need for explanation on to the initial conditions. The effective laws in Everett are indeterministic, however, so that typicality may be thought to carry a heavier explanatory burden.

common interest in information), are clearly more critical, as is Antony Valentini's Chapter 16 (with a reply by Harvey Brown).

Zurek's chapter is a guide through recent results that have developed out of the research on decoherence pursued since the 1980s in his group at Los Alamos. They are presented as going beyond decoherence – where Zurek uses the term in the more restrictive sense of loss of coherence of the (reduced) state of a quantum system – but they are all about the consequences of entanglement between a quantum system and its environment, specifically: the phenomenon of environment-induced superselection (or 'einselection'), an extremely elegant alternative derivation of the Born rule probabilities (but see also Albert's comments on pp. 362–363), and the insights of 'quantum Darwinism', i.e. the idea that certain einselected states acquire a particular degree of 'objective existence' because they can be observed indirectly and without disturbing them via the redundant information about them stored in the environment. At one point Zurek is critical of the usual identification of Everett worlds via decoherence (cf. Wallace's Chapter 2) because, as he puts it, decoherence requires the notion of tracing over the environment, i.e. of averaging implicitly using the Born rule, so that Everett worlds cannot be defined in this way prior to deriving the Born rule itself. However, I believe this is an artefact of Zurek's focusing on the restricted sense of decoherence. If one looks at the definition of worlds using the decoherent histories formalism, I would argue that the vanishing of interference terms is at most a formal adequacy condition for the 'probabilities' thus defined, and that the task of showing that they indeed play the role of probabilities still lies ahead.

Bub and Pitowsky present the case for viewing the structure of the Hilbert space as providing a probabilistic kinematics embodying certain information-theoretic principles, and they suggest that one can hold fast to a realistic one-world picture, if one gives up on the two 'dogmas' that one must not treat measurements as primitive and that the quantum state represents physical reality. They emphasise the analogy with special relativity, in which they claim the construction of a Lorentz-invariant dynamics (bottom-up rather than top-down) is just a consistency check. But what they take to be the analogous step in quantum mechanics, namely the emergence of Booleanity via decoherence, seems to me more analogous to the derivation of the Galilei transformations as a limiting case of the Lorentz transformations. The crucial 'consistency check' of providing a dynamical theory that will fit the kinematical framework, and is furthermore empirically adequate, is left out. After all, even Einstein worked for more than a decade after special relativity in order to show that also a theory of gravity (and not just a theory of electromagnetism) could be constructed that was compatible with relativity, and ended up modifying the picture of relativity in the process. While admiring Bub and Pitowsky's work, I thus broadly agree with Chris Timpson's spirited reply.¹⁴

Chapter 15 by Rüdiger Schack argues that if knowledge of the universal quantum state is to be gained only through observation (note there may also be theoretical reasons for postulating

¹⁴ I am not sure about Itamar's views on this point (and we can sadly no longer ask him), but Jeff Bub in other writings suggests adopting the 'preferred observable' approach he developed with Rob Clifton. Thus I believe he would agree that the kinematic analysis he provides with Pitowsky is not stand-alone.

a state of a certain form), then Deutsch-Wallace objective chances lose any practical advantage over a fully subjectivist view of the quantum state as a compendium of subjective probabilities. Note that this is a more extreme view of subjectivism than the one I was contemplating above, because Schack sees it as casting doubt on the utility of taking the quantum state as real. Schack's ontology (as in the picture ostensibly painted in Bub and Pitowsky's paper), while containing rational observers and data, is up for grabs. One could instead contemplate a subjectivist approach to probability within the Everett theory, where the ontology is supplied by the quantum state itself.

We then get to the polemic between pilot-wave theory and Everett theory, with Valentini's Chapter 16 and Brown's reply. The target of Valentini's paper is the claim in the Everettian literature that configurations in pilot-wave theory are epiphenomenal, and that the theory already contains Everettian many worlds (most of them miscategorised as 'empty waves'). Valentini has good arguments to bring against this claim and against some of the accompanying rhetoric (which is largely based on historical nonsense). Indeed, pilot-wave theory predicts a whole range of new phenomena that are merely obscured by the prevalent equilibrium conditions (i.e. configurations being currently largely distributed according to the Born probabilities). This observation is underlined by Valentini's analysis of the theory of measurement in pilot-wave theory, which in principle is completely different from the theory of measurement in quantum theory (measurement is theory-laden!). While Valentini does not generally deny that there is structure also in the empty waves, he denies that this mathematical structure has physical significance. To use Wallace's terminology, he denies that these patterns have any predictive or explanatory usefulness, and thus denies them the status of 'real patterns'. At one point, however, Valentini erroneously denies that such structure is even present: while in the example of WKB states (which occurs even in Hartle's Chapter 2) he correctly points out that there is no localised wavefunction 'stuff' one could identify with emergent classical trajectories, he mistakenly states the same for his most realistic example (the case of the decoherent analysis of chaotic systems).¹⁵

However, neither Valentini nor Brown, it seems to me, entirely manage to see all the subtleties in each other's position. To state as Brown does that the decision-theoretic approach provides strong arguments for the Born rule regardless of the possibility of non-equilibrium in pilot-wave theory misses the point that – unless one begs the question about their observability – the actual configurations in pilot-wave theory break the symmetry assumption so crucial for the Deutsch-Wallace derivation (cf. Wallace's gloss at the end of Chapter 8). And while in non-equilibrium the case would be clear-cut in favour of pilot-wave theory (as recognised by Wallace, p. 406), I believe the Everettian has a better case to make in the equilibrium case (even if equilibrium is purely contingent) than Valentini admits. Indeed, assume that (as appears to be the case), our evolution has taken place in a (contingent) regime of quantum equilibrium. Then our perceptual and cognitive capacities will not have evolved to exploit the possibilities provided by the sub-quantum measurements

¹⁵ Indeed, the point of the analysis by Zurek and co-workers is precisely to establish the existence of localised wavefunction components within the overall approximately Liouville distribution defined by the reduced state – the coherence length of the components being much shorter than the overall spread of the state.

analysed by Valentini.¹⁶ Indeed, we will arguably have evolved the same capacities whether we are made of configurations or of wavefunction ‘stuff’, and thus the Everettian can at least argue that there is a profound underdetermination between pilot-wave theory and Everett theory for the case of equilibrium, even if this has just been an approximate and contingent feature of the period on Earth when life evolved. Whether or not the Everett theory has some non-empirical advantages over pilot-wave theory in this case I wish to leave open. What I believe is that ultimately the choice between these different approaches to quantum mechanics – as between collapse and no-collapse approaches – will be empirical (at least given an appropriate theory of confirmation). Equilibrium on all scales and in all regimes is itself fantastically unlikely in pilot-wave theory. Thus, just as an eventual success in predicting non-equilibrium will count as a resounding confirmation of pilot-wave theory, continued failure to find evidence of non-equilibrium will count as an *empirical* disconfirmation of the theory (cf. p. 600) – although to give the theory a fair chance one might have to invest as much into the search for non-equilibrium as in the search for the Higgs boson!

The final Part VI of the book contains further contributions by Peter Byrne (Chapter 17), David Deutsch (Chapter 18), Max Tegmark (Chapter 19) and Lev Vaidman (Chapter 20), together with transcripts from discussions relating to Parts V and VI.

Peter Byrne’s talk was a highlight of the Oxford conference (and I assume of the Perimeter conference, too) relating the recent discoveries about the genesis and very early reception of Everett’s ideas, in particular the tragic head-on collision between Everett and Bohr and his associates, which Wheeler in a sense brought on while desperately trying to avoid it. David Deutsch discusses some visionary ideas about future advances that might come from further developing Everett’s ideas. Some of his remarks, however, set him apart from the other Oxford Everettians, in his readiness to talk about worlds even beyond the established framework of decoherence (but see p. 605). (This explains also part of his rhetoric against pilot-wave theories.) Max Tegmark reviews different levels on which the concept of multiverse is or could be useful in physics, namely regions beyond the cosmic horizon, post-inflation bubbles, Everettian many-worlds, and other mathematical structures. The last of these is arguably rather speculative, and the others can in fact be seen as variants on Everett (because of their origin in quantum fluctuations and spontaneous symmetry breaking). But Tegmark’s point is that several of the issues that arise in the discussion of Everett arise also in other contexts, even were one to consider them in purely classical terms.

Finally, Lev Vaidman reviews Aharonov’s two-vector formalism of quantum mechanics, and proposes that it be used instead in an Everettian context. Vaidman’s main proposal is to use two-vectors as a useful descriptive tool for Everett worlds, but he also wonders about giving them ontological significance, specifically by taking a mixed backwards-evolving state. Note

¹⁶ At least if there is sufficiently strong mixing behaviour in the full component of the wavefunction, say, of the brain after it has effectively disentangled from the part of the environment being perceived. (Note that even mechanisms evolved under these conditions would be able to recognise statistical deviations from the Born rule when presented with them.)

that if the backwards-evolving state has indeed ontological significance, this would provide a physical basis for the ignorance picture of branching favoured by Saunders.¹⁷

This volume is a near-comprehensive guide to the state of the art in research on the Everett interpretation. The only aspect that appears to be underemphasised is the relation between Everett and locality, despite the fact that it is taken to be a selling point of the Everett theory (since it is just unitary quantum mechanics, it can be straightforwardly applied to relativistic versions of the theory). I take it the main options to understanding the relation between Everett and locality are (what I understand is) Myrvold's position of thinking of branching as a hypersurface-dependent concept and my own suggestion of thinking of branching along future lightcones. The choice between these alternatives is in some aspects analogous to the choice between the ignorance and fission picture of branching, but disanalogous in that nothing of ontological import seems to hinge on it, so that it is arguably largely a matter of convention.

Related to the issue of locality is the one sad omission from the volume, namely discussion of the work of another champion of Everett: Hans-Dieter Zeh, one of the pioneers of decoherence and arguably the earliest researcher who emphasised its diachronic aspect. While worlds are global patterns arising from decoherence, Zeh emphasises the significance of the local patterns that decoherence gives rise to in the brain (or whatever the supervenience basis of mentality might be), thus focussing on the 'many-minds' aspect of Everett, rather than the 'many-worlds' aspect emphasised throughout the book. While some versions of many-minds theories are now recognised even by their authors as merely conceptually useful toy models (as one might gather from David Albert's remark on p. 357), I regard the alternative between a global and local perspective to also be largely conventional, so that discussion of many minds in Zeh's sense is in fact discussion of the functionalist foundations of the modern-day Everett programme.

As a final remark: should one say 'Everett interpretation' or 'Everett theory'? While other fundamental approaches to quantum mechanics (pilot-wave theories and spontaneous collapse) are clearly distinct theories, the terminology of 'Everett theory' adopted by several authors in this book does not seem inappropriate, since – with the notable exception of Schrödinger –, Everett was arguably the first to suggest that the pure unitary theory of the wavefunction should be taken seriously as a fundamental and universal theory. This volume is a fitting tribute to his genius.

¹⁷ Vaidman notes that his proposal of an ontological two-state does not fully restore time symmetry, because of the pure-mixed asymmetry of the past and future states. On the other hand, given the patent asymmetry of access we have to the two states (which needs to be better understood), one might consider both states to be mixtures, with us inhabiting (being aware of) only one component of the past state.