The Everett Interpretation: 50 years on

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1. The problem of measurement

The problem of measurement in quantum mechanics was apparent shortly after the theory's invention, in the mid 1920s, by Heisenberg, Schrödinger, and Dirac. It is recognizably the successor to the problem of wave-particle duality. Its origins therefore lie with the very beginnings of the discovery of the quantum. It has never been laid to rest. It haunts us still. If anything the problem is becoming more acute: for it is widely thought that the principles of quantum theory and general relativity may not be compatible, and that something has to give. The measurement problem may be telling us it is quantum mechanics that must be changed.

From a formal point of view, the problem of measurement is the problem of reconciling two kinds of dynamical evolution in quantum mechanics. The first kind is deterministic and incorporates (or expresses) space-time symmetries. It has been the focus of fundamental research in physics ever since the theory was discovered. It is the *unitary* dynamics. The second is indeterministic, apparently unrelated to any space-time symmetry (indeed non-local), without any dynamical structure, and not the subject of physical investigation. It is the *quantum jump* or, in terms of the state, the *collapse of the wave-function*, onto one of a large number of wave-functions that were previously superposed. The latter kicks in when a measurement is performed.

How is it that the unitary equations of motion can be sporadically suspended in favour of collapse? Does collapse happen only when measurements are performed? On collapse, at least in the case of a repeatable experiment (where the quantity measured can be measured again on the same system), a new quantum-mechanical state is introduced (depending on the result obtained), as given by postulate (the projection postulate). Since, quite generally in quantum mechanics, the state does not specify values for every type of dynamical variables, there is the question of what *sort* of dynamical variable is assigned a value in this way. The answer is that it is the dynamical variable that the experiment is *designed to measure*.

This set of prescriptions is called, loosely, the *measurement postulates* of quantum mechanics. No other theory in physics hosts such oddly conflicting and so oddly top-down principles.

In the face of this physicists have historically tried to see the measurement postulates as a reflection of some sort of philosophical limitation to physical theorizing or the expression of laws - for example, as reflecting the divide between 'the observer' and 'the observed' (von Neumann), or the divide between 'mind' and 'body' (Wigner); or as a way of interfacing two quite different theories, classical mechanics and quantum mechanics (Bohr). If so (it could be argued) the measurement postulates need not signify anything wrong with quantum mechanics; they are a mark of its fundamental status, or a mark of its non-fundamental status (as depending on classical mechanics) – one or other anyway.

It may then have been a strategy worth trying (Saunders 2005), but few find it promising today. For those of us who believe that classical mechanics is not a fundamental theory that can never dispense with, Bohr's strategy is a non-starter. We who keep an eye on the difference between the experiment process as a purely physical process, as opposed to the provider of evidence, have no have time for von Neumann's or Wigner's either. Anyway we suppose that the physics of mentality lie mainly in the province of brain-science. Physically speaking, we suppose measurement processes are continuous with all other kinds of physical processes and that they are, at least as goes their macroscopic structure, describable in common-sense terms. For us, a satisfactory solution of the problem of measurement will define equations which describe this macroscopic structure along with the microscopic processes that give rise to it as of a piece. With that there can be no reason of principle why such equations should be restricted to sub-systems only, or as any the less comprehensive than precursors like Newtonian gravity, Maxwell-Lorentz electrodynamics and its relativistic cousin, or Einstein's theory of gravity. It should, if only at the level of highly idealized models, be applicable to the universe as a whole.

In summary:

1. The problem of measurement should be solved by clear and simple reasoning that can at least schematically be stated in non-relativistic quantum mechanics and can at least schematically be applied to the universe as a whole.

2. The solution should be applicable to relativistic quantum theory as well and specifically to the standard model.

3. 'The observer' should have no special status in the interpretation, likewise 'experiment', 'sub-system', 'environment', unless questions of evidence or beliefs are explicitly invoked. Apart from in the latter role, 'the observer' etc. should be modeled as a physical system or sub-system or physical process, just like any other.

Several theories have now been devised that meet (1) and (3) and which modify the fundamental equations of quantum mechanics - whether by adding to them (hidden-variables) or by modifying the dynamics (dynamical-collapse theories). The best-known are pilot-wave (or de Broglie-Bohm) theory and the GRW theory (after Ghirrardi, Rimini, and Weber) respectively. But this strategy has not so far worked in the relativistic case, whether because there there are real questions of principle, so far unsolved, or mere technical difficulty, so far unresolved. No theory of this kind has successfully met all three of our principles.

Nevertheless, the success of pilot-wave and GRW theory in recovering the empirical content of non-relativistic quantum mechanics is of great importance. Most of the quantum effects discussed historically, in debates over foundations, were non-relativistic, or could just as well have been conducted using non-relativistic examples. The two theories prove, if more proof is needed, that the problem of measurement is not a philosophical problem, and that if not quantum mechanics, then a theory close to it could be taken as a universal (albeit non-relativistic) physical theory. They are theories 'close to' quantum mechanics because they both suppose the wave-function is physically real, and can be defined for the universe as a whole.

That completes our catalog of principles:

4 It is in principle legitimate to view the wave-function as physically real and as applicable to the universe as a whole.

Both GRW and pilot-wave theories, the principal rivals to Everettian quantum mechanics (EQM), hold that there is a wave function for the universe as a whole and that it is physically real. It is common ground to EQM also.

2. Everett's 'relative-states'

The chief virtues of the Everett interpretation as it was originally formulated (Everett 1957) is that it met (2) and (3), and went some way to meeting (1). It meets (2) without any difficulty because it is genuinely an *interpretation* – nothing is added or taken away from the ordinary unitary equations — an interpretation moreover that does not rely on any special features (such as constancy of particle number or the existence of a covariant position operator) of non-relativistic quantum mechanics. It meets (1), if the interpretation works at all, because the unitary equations hold unrestrictedly.

But the interpretation comes with a heavy price: the quantum theory that emerges, purged of the measurement postulates, is *fantastical*. It describes the measurement process in common sense terms, but it only avoids the measurement problem insofar as it describes all physically possible outcomes to such a process as well – *all* as physically real. It is a *many-worlds* theory

Everett did not quite put things this way, however. He showed, rather, that branching at the macroscopic level - the development of a single component of the wave-function into a superposition - will in a certain sense be *invisible*. For suppose we have a unitary dynamical evolution taking the total system (a measuring instrument and a microscopic system, say a spin system) in an initial state Ψ_0 to a final state Ψ_t. Suppose that the spin system initially in the state $|\uparrow\rangle$ couples to the apparatus so as to yield the outcome 'spin-up' with certainty, and likewise 'spin-down' with certainty, when the initial state of the spin system is $|\downarrow\rangle$. Then the dynamics is:

$$\begin{aligned} \Psi_0 &= |\text{ready}\rangle \otimes |\uparrow\rangle \to \Psi_t = |\text{spin-up}\rangle \otimes |\uparrow\rangle \\ \Psi_0 &= |\text{ready}\rangle \otimes |\downarrow\rangle \to \Psi_t = |\text{spin-down}\rangle \otimes |\downarrow\rangle. \end{aligned}$$

In either case - with the spin system initially either in the '$|\uparrow\rangle$' state or the '$|\downarrow\rangle$' state - no measurement postulate is needed. The outcome can be predicted with certainty merely from the unitary dynamics.

Suppose further that '$|$ready$\rangle$' and '$|$spin-up$\rangle$', '$|$spin-down$\rangle$' can be taken as symbolic notations for the wave function not just of the apparatus (before and after the measurement respectively) but of the environment as well, indeed of the entire macroscopic universe (putting to one side the question of how the universe actually evolves – for our purposes we may take it as approximately static). That is, these states describe perfectly comprehensible, ordinary, macroscopic states of affairs.

Now consider the result if the spin system is initially prepared not in the state $|\uparrow\rangle$ (or $|\downarrow\rangle$) but in a superposition of the two, of the form $c|\uparrow\rangle + d|\downarrow\rangle$. This is supposed to yield trouble. But if we consider the final state as dictated by the same unitary evolution:

$$\Psi_0 = |\text{ready}\rangle \otimes (c|\uparrow\rangle + d|\downarrow\rangle) \rightarrow \Psi_t = c|\text{spin-up}\rangle \otimes |\uparrow\rangle + d|\text{spin-down}\rangle \otimes |\downarrow\rangle \quad (1)$$

then each of '$|$spin-up$\rangle$' and '$|$spin-down$\rangle$' states likewise describes a perfectly ordinary, comprehensible state of affairs, in each of which a definite outcome is recorded, just as before. Considering a series of repetitions of the experiment, with the recording instrument storing the outcomes one by one, the superposition is again a superposition of states each of which describes a perfectly comprehensible and ordinary state of affairs - each describing a *sequence* of outcomes, a definite *record of statistics*. The superposition itself, in contrast, cannot be encoded in a record in any branch in this way. It is in this sense *invisible*.

Quantum mechanics, in other words, can describe a sequence of experimental outcomes, in terms of the purely unitary dynamics, at the price that it must describes them all, in a grand superposition.

What of the spin-system measured to have spin-up? Does it really have spin-up, or is there only a *record* that it has spin-up? ? Everett's answer was that *relative to the state* '$|$spin-up$\rangle$' there is the state $|\uparrow\rangle$ - the *relative state* of the spin-system. This provided, too, a way of presenting the basic ideas, without talking explicitly of many-worlds: Everett called it the *relative-state f*ormulation of quantum mechanics. One can see what he means: if one announces, as a good four-dimensionalist should, that all moments of time are real, it helps to add that one event may be 'now' *relative* to another; or if one announces, as a good relativist should, that there is no such thing as absolute velocity, it helps to add that objects may have well-defined *relative* velocities.

Everett had little more to say than this. His contribution was in a way rather minimal. It was minimal in the way that Galileo's contribution was minimal when it came to explaining the Copernican system. As everybody knew, the earth couldn't really be in orbit around the sun, because if it was there would be an immense wind, and falling bodies would fall behind the points from which they are released, and so on. But to this Galileo pointed out that the motion would be invisible so long as everything was in motion together. Everett pointed out that branching would be invisible so long as everything was branching together.

There is, however, a certain difficulty. As with Galileo's principle of inertia, so with Everett's use of states describing the macroscopic (states like '$|\text{ready}\rangle$' and '$|\text{spin-up}\rangle$'). What, precisely, were these states, and why are those states - those sorts of states - the 'right ones' to choose as representing worlds, or as defining relative states? Are there worlds and relative-states corresponding to any old choice of states? In terms of the parallel with Galileo: what are the inertial frames, the 'natural motions'? What is the natural or *preferred* basis, with respect to which the universe is in a superposition?

This is the *preferred basis* problem. It is a problem that arises for hidden-variable theories and dynamical-collapse theories as well: what basis is picked out by the hidden variables? Which states - what sorts of state - result from collapses? The comparison with either theory is invidious: for each, whether hidden-variables or dynamical-collapse, is very largely an attempt to fill out an answer to this question at the level of the fundamental equations. Everett's approach can hardly stay silent on this matter.

Yet silent it was, until recent times. Given that everyone still spoke of measurement in terms of 'the observer', and given the still-prevelllent view that there was some special role for 'the mental' in quantum mechanics, it is perhaps unsurprising that the ingredient thought to be missing from EQM was a theory of *consciousness*. Something has to be added after all in the way of basic principles, needed to make sense of the notion of 'preference' of one basis over another. This was agreed by proponents of the approach as well as by its critics (see e.g. Lockwood (1989), Donald (1994), Kent (1990), and most recently, Penrose (2004)). If any of this right, the critics have the better of it. Bringing in questions of mentality was common ground to philosophically-based solutions to the measurement problem, from von Neumann's to Wigner's, which at least had the virtue of describing only a single world. And of course no such theory was to hand: Everett's schematic model was a complete non-starter as a theory of consciousness. And even if one did make progress in these respects, (3) is clearly compromised. Once consciousness enters into the interpretation of a physical theory, realism is in trouble.

The interpretation was stillborn in another respect. It's raison d'être was to make sense of the unitary, covariant, and deterministic dynamics. How to reconcile this with the probabilistic interpretation of the theory? As conventionally formulated, probabilities only come in to quantum mechanics with the measurement postulates. Thus, it is only the measurement postulates that tells you a superposition like (1) means that one of $|\text{spin-up}\rangle \otimes |\uparrow\rangle$, $|\text{spin-down}\rangle \otimes |\downarrow\rangle$ results, with probabilities $\|c\|^2$ and $\|d\|^2$ respectively. If the superposition actually remains, as there is only the unitary dynamics, in what sense does either state occur with *some probability*?

3. Decoherence theory

The first stage of the Everett interpretation explored more its deficiencies than its strengths. The second stage sets in with the recognition that the basis to be used in defining the branching structure is only effective; it should not matter to the macroscopic description if it is tweaked this way or that; it is a robust dynamical structure to the universal state (or to the unitary orbit of the state) all the same.

Branching, in other words, is a real dynamical structure to the universal state. It is, in a word, *decoherence*. A basis 'adapted' to this dynamical structure is the one to make those patterns clear. But it concerns emergent structure; it is defined only FAPP ('for all practical purposes'); likewise, the equations that depict measurement processes, treating the macroscopic and the microscopic uniformly, are equations FAPP.

The philosophy is an obvious one, if classical worlds are higher-order ontology, structures in the universal state. They are like cells in molecular biology, structures in swirls of atoms and molecules (Wallace 2003). The same philosophy is clear in Ken Wilson's approach to renormalization, in which even supposedly fundamental theories like QED should be viewed as effective theories. They arise, using a demonstrably stable scheme of approximation, through a coarse-graining of an underlying physics that does not have to be known exactly. Similar comments apply to methodology in condensed matter physics, as argued by Anderson. In philosophy of science quite generally on this point there is wide consensus. From nuclear physics to condensed matter physics and biochemistry, the use of approximations and phenomenological equations are the norm. Why think a precise and axiomatic theory exists for any of these fields, that picks out their distinctive features? Why should the situation be any different in the recovery of classicality in quantum mechanics?

Classicality FAPP, it was thought, by Bell among others (see e.g. Kent (1990)), was symptomatic of a failure of realism, but from an Everettian point of view that is simply a mistake. The fundamental theory itself, we grant, must be defined precisely (and without mention of 'observation' or 'experiment' and synonyms), but the classical isn't that; the classical is an empirical consequence of the theory. And in extracting the empirical consequences of a physical theory, everyone is agreed that approximations can and should play a fundamental role.

The change in philosophy went hand-in-hand with the development of method, first in open quantum systems theory (by Davies and others in the 1970s), and then as applied explicitly to measurements (by Zeh and later Joos, Unruh and Zurek in the 1980s). They were methods for defining quasiclassical dynamical equations on the basis of quantum ones. Typically they considered the behaviour of a small number of massive systems weakly coupled to a thermal bath of much lighter ones. In a degenerate description of the lighter particles - on tracing out their degrees of freedom - one obtains equations similar to those in models of stochastic classical systems.

The underlying philosophy, however, insofar as it was supposed to give a solution to the measurement problem, was that classical behaviour was *superselected* (that superpositions of states describing different macroscopic properties were somehow *forbidden*). The idea could hardly have been further from

Everett's. But it was anyway never convincingly defended (see, e.g. Zurek 1991, and subsequent replies).

Another method for defining decoherence was discovered at about the same time. It too was thought to solve the measurement problem all on its own: the *consistent histories* theory of Griffiths, Omnès, and Gell-Mann and Hartle. The mathematical tool is the histories formalism itself (a *history space*), and the criterion of consistency (or decoherence, in Gell-Mann and Hartle's terminology). The *quasiclassical history space*, as defined by Gell-Mann and Hartle (1990), yields the structure of the universal state that we have so far been concerned with: the system of branching and approximately classical worlds. But when it came to the reality of all of these histories, they were circumspect: quantum mechanics 'prefers none over another except via probabilities'.

Indeed, why not suppose only one of these histories is real? But as a one-world theory, the history space can hardly be defined FAPP. Advocates of hidden variable and dynamical collapse theories could easily recast their respective positions in terms of history spaces (see e.g. Kent 1996), whereupon the differences between them very largely concerned the exact definition of the history space. If there is only one world, the universal state has only the meaning of a probability measure on the history space, when what exists is the single history. Why not try to describe it precisely? For a start the consistency condition had better be precisely satisfied (Dowker and Kent (1996)). One is a long way from the perspective of classicality as an effective theory.

It is different if the ultimate reality is the universal state. In that case a history space concerns only an effective level of description of the structure of the state, better or worse suited to extracting useful phenomenological equations. It is not that the structure is perfectly precise but that we need not worry about what it is in its details: the structure itself is emergent, imprecise at its boundaries and in its minutiae, like galaxies and planetary systems. 'Worlds' in the Everett interpretation, as Wallace has emphasized, are really like worlds, planetary systems that are tightly bound together, but only weakly coupled to other words, and systems without precise borders or edges.

4. Probability

Branching, then, is only 'effective'. Then so too is quantum probability, for probabilistic events, according to the Everett interpretation, occur when branching occurs - when an element of the decoherence basis unitarily evolves into a superposition of such elements. This has an important implication for what has long been regarded as a key objection to the Everett interpretation, namely, that if branching really occurs then there is a natural alternative measure over branches to the Born rule: that for which all branches are equiprobable. But if branching only occurs on decoherence, then there is no such measure - none that is stable under small perturbations, that is meaningful FAPP, that applies to branches at the level at which they themselves are defined. There are fat branches and thin ones, as given by the Born rule; there is no number of branches which are fat, no number which are thin, unless postulated by an arbitrary convention.

This is the first of three crucial questions concerning probability. They are:

1. What of branches with records of anomalous statistics?
2. What is the appropriate epistemic attitude to take in the face of branching? Does it make sense to speak of uncertainty?
3. How, if at all, is the Born rule to be justified?

On (1), see Pappineau (1996), Saunders (1998), Lewis (2001), Lewis (2006), Wallace (2006). For (2), see Saunders (1995), (1998), Saunders and Wallace (2007), and (for a negative response) Vaidman (2001), Greaves (2004). The argument at this point was that the Everett interpretation was no *worse* off than any other account of quantum probability, with the answer to (3) that it is justified just as any other physical hypothesis is justified, by its success in practise. That one can hope to do better was shown by Deutsch (1999), who derived the Born rule from certain symmetry arguments and appeal to certain axioms of decision theory. The argument was criticized by Barnum *et al* (2000), but as elaborated by Wallace (2002, 2005a) it was placed on solid ground.

The general idea is this: let rational agents express 'likelihood' relations among quantum experiments M, N, etc.„ whose outcomes - sets of events E, F, G etc. yield dividends whose utility is selected by the agent at will. Let '$E|M \succeq F|N'$ mean that, in the agents expectation 'it is at least as likely that E will happen given M as that F will happen given N'. For an experiment, M, let E_M be the set of all possible outcomes, and let $\emptyset$ be the empty set. Then an ordering of likelihoods is *represented* by a credence function Pr if

- $\Pr(\emptyset|M) = 0$ and $\Pr(E_M|M) = 1$
- If E and F are disjoint then $\Pr(E \cup F|M) = \Pr(E|M) + \Pr(F|M)$
- $\Pr(E|M) \geq \Pr(F|N)$ iff $E|M \succeq F|N$.

We suppose agents are *rational* insofar as they subscribe to the principles:

- **Transitivity** $\succeq$ is transitive: if $E|M \succeq F|N$ and $F|N| \succeq G|O$, then $E|M \succeq G|O$.
- **Separation** There exists some E and M such that $E|M$ is not null.
- **Dominance** If $E \subseteq F$, then $F|M \succeq E|M$ for any M, with $F|M \simeq E|M$ iff $E - F$ is null

(where an event E is *null given* M if $E|M \simeq \emptyset|M$). The first requires that 'likelihoods' are comparable, whether for different outcomes given the same experiments, or given different experiments. 'Separation' requires that some event is possible, 'Dominance' that the likelihood of a set of events is greater than that of any proper subset, whatever the experiment, unless the omitted events are impossible for that experiment, in which case it is the same.

Let *Equivalence* be the principle

$$\textbf{Equivalence}\ F|M \simeq E|N \text{ if and only if } \mathcal{W}_M(F) = \mathcal{W}_M(F)$$

where $\mathcal{W}_M(F)$ is the usual Born-rule probability for outcome F on performance of experiment M. That is, Equivalence is the principle that outcomes of equal weight have equal credence.

We need one more definition. We are interested in situations where there are 'enough' experiments available so that the decision-theory constraints have bite. To this end suppose

> **Weight richness**: A set $\mathcal{M}$ of quantum experiments is *rich* provided that, for any positive real numbers $w_1, .., w_n$, with $\sum_{i=1}^{n} w_i = 1$, $\mathcal{M}$ includes a quantum experiment with n outcomes having weights $w_1, .., w_n$.

We can now state the representation theorem.

Theorem 1 (Deutsch and Wallace) *if the likelihood orderings of a rational agent satisfy Equivalence, then they are uniquely representable by a credence function Pr where* $Pr(E|M) = \mathcal{W}_M(F)$.

Can Equivalence itself be viewed as a principle of rationality? If it can, the Everett interpretation, as goes probability, is in remarkably good shape. The notion of objective probability - chance - has long troubled empiricists. Credence or subjective probability is in contrast perfectly clear; all that we know about objective chances is that they had better be what credences are keyed to. But just *why* credence should track chance can hardly be explained until we know what chance is (and perhaps not even then). Their relation may have the irreducible status of a brute posit, as formulated by the philosopher David Lewis (the famous *principal principle*). No one has ever offered even a hint of a derivation of the principal principle from a physical theory, unless it is the argument that to accept a physical theory in which chance play any role just is to tailor one's credences to those chances. In EQT we do better: it is enough if equal chances have equal credences. And it may be we do *much* better, if the latter principle in turn can be derived. Wallace has provided a variety of arguments to show that Equivalence is indeed rationally compelling, if EQT is true (Wallace 2005a). Nothing comparable has every been achieved for any other physical theory.

There is another aspect to the assessment of EQT as a probabilistic theory, however. It may be that one who believes EQT is true will match her credences to quantum mechanical weights, but what of he who does not? How is he to first be persuaded of the likelihood of the theory? The question can be posed in terms of probabilistic evidence: how is one to update a prior probability measure (credence) over two or more competing theories in the face of given evidence (namely, the observed relative frequencies)? In this, standardly, the principal principle is used. Must one already believe that EQT is true, in order to deduce from the observed statistics that quantum mechanics is better confirmed than some rival? This question was first posed by Myrvold (2005).

The general thrust of the complaint is this: the Everett interpretation undermines so many common-or-garden beliefs so as to threaten the very basis on which evidential claims for quantum mechanics are evaluated. Science, it is maintained, is like a ship at sea, and it must be repaired or modified whilst keeping it afloat (the metaphor is due to Neurath). If sinks under the Everett interpretation.

This question returns us to (2) in our list above, of whether there is any place for uncertainty in EQM. Indeed, failing a positive answer to (2), it might be argued that rational agents can have no notion of a 'likelihood' relation either: if nothing is uncertain, how can any event be more likely than another?

To this two quite different answers have been given. The first is that uncertainty is not needed, neither for the representation theorem - forget about 'comparative likelihood' and just use 'relative weight' - nor for the purposes of confirmation theory, at least not for a Bayesian confirmation theory. This program has been systematically pursued by Greaves (2004, 2006, 2007).

The second is to insist that what is at stake is what our ordinary words actually mean, in a way that is dictated by use. Changing, dramatically, our fundamental understanding of a physical theory (going over to EQT), there may or may not be truth conditions under which our ordinary utterances still come out as true. The new theory may or may not be consistent, in this sense, with what we were previously, pre-theoretically, inclined to say. But it would be perverse to arbitrarily challenge our commons sense assumptions. Water is wet; it would be perverse to conclude, on the basis of its chemistry, that water is not wet because the atoms making it up are not wet; or that colours of things are not real because the atoms that make up those things have no colour – or, for that matter, that desires and intentions are not real because C-fibres firing do not desire and do not intend.

That plunges us back into philosophy, but of a tough-minded and conservative sort. It is broadly functionalist as goes mind and meanings, as Wallace has made clear in a series of papers (Wallace 2003, 2005c, 2006). It is conservative with respect to philosophical and physical principles; it appeals to standard methodology in the philosophy of language, and standard methods of the special sciences and in the specialisms of physics. Indeed, it is not hard to find truth conditions that, in accordance with the principle of charity, makes most of what we pre-theoretically say about uncertainty come out as true (Wallace 2005b, 2005c). They can even be given in a way that preserves bivalence and determinacy of reference (Saunders and Wallace 2007). It satisfies our key principles (1), (2), (3). It yields a better account of probability and the relation between credence and chance than any of its rivals.

These are striking claims. If they are true, and granted that the Everett interpretation is at bottom a literalist construal of the dynamical unitary evolution, it would be astonishing if any other realist interpretation of the theory were possible, that leaves the equations unchanged. The implication , if these claims are true, is that our best physical theory, in place now for almost a century, is telling us that we live in a branching universe.

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