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Against Many-Worlds Interpretations

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Abstract

This is a critical review of the literature on many-worlds interpretations (MWI), with arguments drawn partly from earlier critiques by Bell and Stein. The essential postulates involved in various MWI are extracted, and their consistency with the evident physical world is examined. Arguments are presented against MWI proposed by Everett, Graham and DeWitt. The relevance of frequency operators to MWI is examined; it is argued that frequency operator theorems of Hartle and Farhi-Goldstone-Gutmann do not in themselves provide a probability interpretation for quantum mechanics, and thus neither support existing MWI nor would be useful in constructing new MWI. Comments are made on papers by Geroch and Deutsch that advocate MWI. It is concluded that no plausible set of axioms exists for an MWI that describes known physics.

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Foreword to archive version

Though this paper was written in 1989, I thought it worth putting it on the archive now, since the debate to which it contributes is still very much alive — and since some of the misconceptions which it addresses are still, I fear, a source of confusion.

There is clearly enormous appeal in the idea that, somehow, there simply *cannot* be any real problem in extending quantum theory from the Copenhagen interpretation to a fully satisfactory interpretation of quantum cosmology. It seems to me that faith in that idea now generally survives despite, rather than because of, the many different attempts by distinguished physicists to explain precisely how the quantum theory of the universe should be interpreted.

This is not to dismiss the work of those pioneers. Even the harshest critic would accept that the many-worlds literature, and the work of the various post-Everettian schools, includes a series of well-motivated and ingenious attempts to solve a problem which lies at the heart of modern physics. Whether or not those attempts have succeeded, they have certainly taught us more about the nature of the problem.

And anyone who discusses these problems with colleagues quickly realises that there are thoughtful people who have considered the counter-arguments and still believe that the problem was essentially sorted out, once and for all, by Everett [F1]. There are, similarly, thoughtful people who believe it has been resolved by DeWitt [F2], by Deutsch [F3], by Coleman's exposition [F4] of Farhi–Goldstone–Gutmann [F5], by Gell-Mann and Hartle's consistent histories approach to quantum cosmology [F6], and so on.

My impression, though, is that each of these camps represents a relatively small minority. When any given attempt at something like a many-worlds interpretation is spelled out and analysed, its problems turn out to be sufficiently serious to cause most physicists to look elsewhere. Those who remain adherents tend to have non-standard views on the nature of scientific theories. There are, for example, hard-line Everettians who take Bell's intentionally pathological Bohm-Everett hybrid [F7] as the inspiration for a serious proposal — and happily accept that, when quantum cosmology is properly understood, it becomes clear that

past events generally have no correlation with our present observations. Similarly, there are consistent historians who accept that no useful scientific predictions can be derived from their formalism in the absence of a set selection hypothesis, and yet argue that there is no need for such a hypothesis.

Most people attracted by many-worlds ideas, however, tend to be working in other areas of physics. My impression is that most of these enthusiasts tend not to follow very closely the debates as to what, precisely, any given interpretation means and what its scientific implications are. And understandably so. The basic idea of a many-worlds interpretation seems superficially attractive: it is always unsettling to think that there may be an underlying problem which, at some stage, will have to be confronted, and it is reassuring to be told that there is an apparently unthreatening solution. Advocates of many-worlds interpretations tend, naturally, to stress the interest in their ideas, rather than the problems — which, of course, may not *be* such serious problems from their perspective. And it is hard to pick one's way through the literature on interpretations of quantum theory, and easy to come away with the (unfortunately sometimes correct) impression that nothing very constructive comes of these debates.

It is really that audience — those attracted to the idea of a many-worlds interpretation, but not professionally committed to some particular line of thought — that the paper was, and still is, mainly meant to address.

I have left the published version unaltered, except that references to preprints have been replaced by references to the published papers. The most obvious omission, in hindsight, was any discussion of many-worlds-type interpretations based on the Schmidt decomposition, and more generally any discussion of that school of thought which suggests that the interpretational problems of quantum theory can be completely resolved by a proper understanding of the physics of decoherence. A recent attempt to develop these ideas, and to explain the obstacles that apparently prevent them from succeeding, can be found in Ref. [F8]

A similar paper written today would also cover the consistent histories approach to

quantum mechanics developed in recent years by Griffiths and Omnès and, in the context of quantum cosmology, by Gell-Mann and Hartle. As noted above, that approach too has run into serious problems, discussed in some detail in Refs. [F9-F13].

Foreword references

[F1] Refs. [1] and [19] below.

[F2] Ref. [3] below

[F3] D. Deutsch, Int. J. Theor. Phys. **24** 1 (1985); for a critique see S. Foster and H. Brown, Int. J. Theor. Phys. **27** 1507 (1988). See also Ref. [7] below.

[F4] S. Coleman, *Quantum Mechanics In Your Face*, unpublished lecture.

[F5] Ref. [5] below

[F6] See e.g M. Gell-Mann and J. B. Hartle, in *Complexity, Entropy and the Physics of Information*, Vol. III of *SFI Studies in the Science of Complexity*, edited by W. H. Zurek (Addison Wesley, Reading, 1990).

[F7] Ref. [8] below

[F8] A. Kent and J. McElwaine, Phys. Rev. A **55** 1703 (1997).

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I. INTRODUCTION

There is increasing interest in the foundational problems of quantum theory and possible physical theories which might provide solutions. The subject is fascinating in itself, and is further motivated because theorists continue to find that the barriers to understanding quantum gravity and cosmology are as much conceptual as calculational. Many-worlds interpretations (MWI) are often invoked. Much theoretical work now ultimately depends on the assumption that MWI are consistent with the evident physical world. Clearly, this consistency needs to be examined.

The useful criticisms of MWI do not stem from an inability to accept the picture of multiple branching universes, nor do serious critics merely say that they do not see how to reduce macroscopic physics to an MWI. They assert either that particular physical facts are demonstrably not logically deducible from the axioms of MWI, or else they criticize the axioms proposed (usually on grounds of complexity or arbitrariness). Though it does not seem to be widely recognized, these criticisms have had a substantial effect: modern opponents of state vector reduction, recognizing that Everett's original argument is erroneous, have adopted increasingly sophisticated formulations.

A popular misconception should be removed. Many physicists have the impression that the content of an MWI is *simply* the statement that physical correlations can be calculated without state vector reduction. This statement is certainly consistent with known experiments. It may also be useful in simplifying the axioms of an ensemble formulation of quantum mechanics. However, in this setting the statement does not make a direct connection between the mathematical formalism and physical reality (in particular, it actually says nothing about the existence of many worlds). The proponents of MWI were motivated by greater ambitions; they aimed to produce a simple mathematical theory whose formalism had a well-defined correspondence with physical reality. It will be argued that they did not succeed.

Even in modern formulations MWI still seem seriously inadequate. Thus the aim of this

paper is to argue that the literature neither contains nor suggests a plausible set of axioms for an MWI that describes known physics. Specifically, arguments are presented against MWI proposed by Everett, [1] Graham [2] and DeWitt; [3] and against the possibility of basing MWI on the work of Hartle [4] and Farhi-Goldstone-Gutmann. [5] Some brief comments, in reply to arguments put forward by Geroch [6] and Deutsch [7] in favour of MWI, are also included.

The classic criticisms of MWI are Bell's papers. [8,9] Other published arguments against MWI include papers by Stein [10] and Healey, [11] and comments by Shimony [12] and Wheeler. [13] The arguments below against Everett's and DeWitt's MWI draw largely from Bell's discussions, and also make use of Graham's criticism [2] of Everett's original argument; some of Stein's arguments have also been adopted. Still, we hope that this article may be useful for two reasons. Firstly, we have tried to give a complete and self-contained criticism of all the ideas currently invoked in support of MWI. In particular, we have tried to refute claims that a probability interpretation for MWI can be defined by Graham's arguments about world-counting, or by the work of Hartle and Farhi-Goldstone-Gutmann on the properties of frequency operators. Secondly, we have tried to clarify the logical structure of the MWI, and so to pinpoint their essential problems. This involves the extraction and formalization of what seem to us the fundamental assumptions in the original papers. The attempt may not have entirely succeeded. But we are convinced that the *procedure* is justified, and in fact that axiomatization should have been insisted upon from the beginning. For any MWI worth the attention of physicists must surely be a physical theory reducible to a few definite laws, not a philosophical position irreducibly described by several pages of prose. Moreover, once an axiomatization is achieved, the irrelevance of much of the rhetorical argument surrounding MWI is exposed, and any given MWI's (de)merits become much clearer.

In seeking this sort of clarification, we are not asking for anything extraordinary. Simply, MWI proponents need to specify precisely the mathematics involved in setting up their theory, to state explicitly which of the quantities they define are supposed to be physical, and to explain (in principle, not necessarily in detail) how our observations are to be described

in terms of these physical quantities. Examples of theories which satisfy these criteria are the standard presentations of Maxwell's electrodynamics and general relativity. Here the electric and magnetic fields, matter and charge densities, space-time manifold and metric are taken to be physical quantities, while the gauge potentials and the various coordinate systems on the manifold are only part of the mathematical formalism. (A more profound discussion can be found in [14].) Given standard (e.g. SI) definitions of mass, length, time, charge, current, and so forth, we understand how to identify the physical quantities with the actual objects in our experience — for example, we know how to make a good approximation to a spherical test body of a given mass, and to check that when put into orbit it does very closely follow what we calculate to be a geodesic. (Of course, we really know none of these things with absolute rigour — we are dealing with physics, not pure mathematics, and we tacitly invoke many assumptions whenever we interpret an experiment (that scientific induction and statistical inferences are valid, that no untoward background signal affects the data, that our carefully machined test mass remains internally stable and homogeneous while being transported to the laboratory — we certainly cannot justify this from Maxwell's and Einstein's theories, ...). But, since it is claimed that in a many-worlds interpretation "the symbols of quantum mechanics represent reality just as much as do those of classical mechanics," [3] we should at least demand that an MWI be defined to the same level of rigour that classical theories are.)

The dominant mode of research in current physics emphasizes mathematical structure over problems of interpretation. So perhaps it needs to be said that we see this paper (like those it criticizes) as a discussion of physics, not philosophy. Of course it has often been a useful tactic to pursue calculations without worrying much about their exact interpretation: the development of quantum field theory is an outstanding example. But, ultimately, we need not just to calculate a theory's predictions for familiar types of experiment, but to understand the theory's properties. The reason is that this is crucial in deciding the extent to which a theory has been tested, and hence in designing new tests. For example, once it was shown that standard quantum mechanics predicts non-local correlations, [15] it became

clear that new experiments were needed to distinguish between the standard theory and local hidden-variable theories. Similarly, the physical community ought to be able to, and ought to, decide whether any many-worlds interpretation of quantum mechanics gives a precisely formulated mathematical model in which particular specified quantities have a well-defined correspondence to physics. For if not (and if the same conclusion is reached about other standard interpretations), then it will presumably be generally agreed that precise models ought to be built, and tests designed to distinguish these models from the standard theory. (A prototype model has already been suggested by Ghirardi-Rimini-Weber; [24] it is briefly discussed at the end of this paper.)

After this introduction, the argument proper begins with a brief explanation of what is meant by an MWI. Here there is apparently no controversy in the literature; very similar explanations have been given by proponents of MWI, for example in the introduction to reference [3]. Next the papers by Everett, DeWitt and Graham are considered in turn. This is followed by a discussion of the physical relevance of frequency operator theorems due to Hartle and Farhi-Goldstone-Gutmann; the main claim here is that such theorems neither support, nor aid in the construction of, MWI. Some comments on arguments by Geroch and Deutsch are then offered. A concluding discussion summarizes the problems with MWI and briefly considers alternatives.

II. THE CASE AGAINST

Many-worlds interpretations share two essential characteristics. First, they suppose that there exists a definite physical reality, which can be put into correspondence with parts of a mathematical formalism. This assumption is necessary if an MWI is to have any useful content. It is an assumption made by Everett: [1] "This paper ... postulates that a wave function ... supplies a complete matehmatical model for every ... system without exception." and still more clearly by DeWitt: [3] "The real world ... is faithfully represented solely by the following collection of mathematical objects. ... The use of this word [faithfully] implies a

return to naive realism and the old-fashioned idea that there can be a direct correspondence between formalism and reality. ... The symbols of quantum mechanics represent reality just as much as do those of classical mechanics."[1] There seems to be no dispute in the literature on this point: if a theory is not mathematically realist then it is not an MWI.

Second, they base the mathematical formalism on a state-vector $|\psi\rangle$ which belongs to a Hilbert space V and has a purely hamiltonian evolution. Throughout we shall use the Schrödinger picture and consider non-relativistic quantum mechanics. Thus we can formalize the fundamental mathematical assumptions as:

Axiom 0 There exists a Hilbert space V, a hermitian operator H on V, and a continuum of states $|\psi(t)\rangle \in V$ for $-\infty < t < \infty$ such that

$$H|\psi(t)\rangle = i\hbar\frac{\partial}{\partial t}|\psi(t)\rangle \tag{1}$$

holds for all t.

Different MWI vary in the extra formalism they introduce and in the mathematical quantities which they identify as corresponding to reality. However, any meaningful MWI must include mathematical axioms defining the formalism and physical axioms explaining what elements of the formalism correspond to aspects of reality. Now let us consider specific cases.

A. Everett

Everett's famous paper [1] is admirably clear in its claims. For Everett, the mathematical formalism is fully described by Axiom 0. Physical reality is to be entirely encapsulated

[1] Actually, DeWitt here seems unnecessarily defensive; mathematical realism need not be naive, and has more or less continuously been advocated by distinguished physicists throughout this century.

in the wave function evolution. Everett is quite explicit in rejecting any further interpretational axioms: "The wave function is taken as the basic physical entity with *no* a priori interpretation." Thus we have only Axiom 0 and:

Axiom 1E The graph of the state vector's evolution (that is, the set of coordinates $(|\psi(t)\rangle, t) \in V \otimes (-\infty, \infty))$ is a physical quantity.

Now Everett points out that the Hilbert space inner product defines a measure μ, by setting the measure of a state $\phi = \sum_i a_i \phi_i$ to be $\mu(\phi) = (\sum_i |a_i|^2)^{\frac{1}{2}}$, where the ϕ_i form an orthonormal basis of H. The measure has the property that if $\psi = \sum_j \psi_j$ and the ψ_j are orthogonal, then $\mu(\psi) = \sum_j \mu(\psi_j)$. This measure μ is defined by the Hilbert space inner product, and so by Axiom 0 is part of Everett's mathematical formalism.

However, μ does not appear in axiom 1E, and so is not a *fundamental* quantity in Everett's physical interpretation. Nor does Everett make any attempt to show that μ can be understood as a *derived* quantity in the physical interpretation. This leaves no way to deduce any statement connecting μ with real physics. Since μ is of course precisely what we need to describe the measurement correlations predicted by quantum mechanics, Everett's MWI is inadequate.

The problem is illustrated by a simple example.[2] Suppose that a previously polarized spin-$\frac{1}{2}$ particle has just had its spin measured by a macroscopic Stern-Gerlach device, on an axis chosen so that the probability of measuring spin $+\frac{1}{2}$ is $\frac{2}{3}$. The result can be idealized by the wave function

$$\phi = a\,\phi_0 \otimes \Phi_0 + b\,\phi_1 \otimes \Phi_1 \tag{2}$$

where ϕ_1 is the spin $+\frac{1}{2}$ state of the particle, Φ_1 the state of the device having measured spin $+\frac{1}{2}$; ϕ_0, Φ_0 likewise correspond to spin $-\frac{1}{2}$; $|a|^2 = \frac{2}{3}$, $|b|^2 = \frac{1}{3}$. Now in trying to interpret

[2]This example was apparently first considered by Graham; [2] the following analysis combines his criticisms with those of Bell. [9] Anandan [16] has also briefly mentioned the example, making the parenthetical point of the third criticism below, which he attributes to Aharonov.

this result we encounter the following problems:

Firstly, no choice of basis has been specified; we could expand ϕ in the 1-dimensional basis $\{\phi\}$ or any of the orthogonal 2-dimensional bases

$$\{\cos\theta\,\phi_0 \otimes \Phi_0 + \sin\theta\,\phi_1 \otimes \Phi_1, \sin\theta\,\phi_0 \otimes \Phi_0 - \cos\theta\,\phi_1 \otimes \Phi_1\} \tag{3}$$

or indeed in multi-dimensional or unorthogonal bases. Of course, the information is in the wave function is basis-independent, and one is free to choose any particular basis to work with. But if one intends to make a physical interpretation only in one particular basis, using quantities (such as $|a|^2$ and $|b|^2$) which are defined by that basis, one needs to define this process (and, in particular, the preferred basis) by an axiom. This Everett fails to do.

(A referee objects: "As far as I understand it, the world is ultimately described by quantum mechanics yet it appears classical to most macroscopic observers. Phase information is effectively lost when measurements take place. Exactly how this occurs clearly depends on dynamics. It is not going to be part of a proper axiomatic formulation. ... Criticism of [an MWI] because it does not, through its axioms, tell you how the branching takes place is an example of wanting more from the axioms than they could ever reasonably provide."

It's certainly true that phase information loss is a dynamical process which needs no axiomatic formulation. However, this is irrelevant to our very simple point: no preferred basis can arise, from the dynamics or from anything else, unless some basis selection rule is given. Of course, MWI proponents can try to frame such a rule in terms of a dynamical quantity — for example, some measure of phase information loss. But an explicit, precise rule is needed.)

Secondly, suppose that the basis $(\phi_0 \otimes \Phi_0, \phi_1 \otimes \Phi_1)$ is somehow selected. Then one can perhaps *intuitively* view the corresponding components of ϕ as describing a pair of independent worlds. But this intuitive interpretation goes beyond what the axioms justify; the axioms say nothing about the existence of multiple physical worlds corresponding to wave function components.

Thirdly, in any case, no physical meaning has been attached to the constants $|a|^2$ and $|b|^2$. They are not to be interpreted as the probabilities that their respective branches are realized; this is the whole point of Everett's proposal. It can not be said that a proportion $|a|^2$ of the total number of worlds is in state $\phi_0 \otimes \Phi_0$; there is nothing in the axioms to justify this claim. (Note that if the two worlds picture *were* justified, then each state would correspond to one world, and it must be explained why each measurement does not have probability $\frac{1}{2}$.) Nor can one argue that the probability of a particular observer finding herself in the world with state $\phi_0 \otimes \Phi_0$ is $|a|^2$; this conclusion again is unsupported by the axioms.

It is true that Everett attempts to relate the measure μ to physics by a discussion of the memory of observers. But, given Everett's assumptions, the discussion is actually free of physical content. Thus let Φ be the state vector of an idealized observer who has witnessed N measurements of systems identical to the one above; let $\Phi(i_1, \ldots, i_N)$ describe the state of having witnessed results $(i_1, \ldots, i_N)$, where each i_j is 1 (or 0) if the j-th observation was spin $+\frac{1}{2}$ (respectively $-\frac{1}{2}$); we shall suppress the correlated state vectors of the measured particles. We have the expansion $\Phi = \sum a_{i_1,\ldots,i_N} \Phi(i_1, \ldots, i_N)$. Everett considers the vector

$$\Phi^\epsilon = \sum_{|\frac{i_1+\ldots i_N}{N} - \frac{2}{3}| > \epsilon} a_{i_1,\ldots,i_N} \Phi(i_1, \ldots, i_N) \tag{4}$$

and shows that $\mu(\Phi^\epsilon) \to 0$ as $N \to \infty$ for any $\epsilon > 0$. But no new statement about physics can arise from this purely mathematical derivation. Even supposing the infinite limit were obtained, which in reality is not the case, the fact that $\mu(\Phi^\epsilon) = 0$ cannot imply that Φ^ϵ is physically irrelevant, when no hypothesis has been made about the physical meaning of μ.

(Reference [28] also contains a longer exposition [19] of Everett's ideas; this doesn't seem to depart from Everett's original paper on any point of principle.)

B. Graham

The physical example discussed above is also helpful in understanding Graham's position. Graham criticizes Everett's argument, essentially for the reasons given in the last section's

penultimate paragraph. In fact, Graham interprets the wave function $\phi = a\,\phi_0 \otimes \Phi_0 + b\,\phi_1 \otimes \Phi_1$ as describing precisely two worlds, which (so long as a and b are non-zero) are equiprobable. He claims that, if this were indeed the wave function after measurement, then the probability of measuring spin $\pm\frac{1}{2}$ would be the naive "counting probability" of $\frac{1}{2}$. However, he argues that in a more realistic analysis (in which the measuring device has many degrees of freedom and must be described by a statistical mixture of quantum states) the counting probability of a particular measurement can be shown to *equal* to the standard quantum mechanical probability (defined by μ). This is a remarkable claim, which has attracted strangely little scrutiny.

Let us try to axiomatize Graham's position. In addition to Axiom 0, we suggest:

Axiom 1G A preferred orthogonal basis is defined for the universal wave function, in which each basis element defines a state where all macroscopic measuring devices register definite results.

Axiom 2G Suppose that at time t the wave function $\psi(t)$ has the expansion

$$\psi(t) = \sum_{1 \leq i_1 \leq j_1, \ldots, 1 \leq i_N \leq j_N} a_{i_1, \ldots, i_N} \psi(i_1, \ldots, i_N) \tag{5}$$

where $\psi(i_1, \ldots, i_N)$ is a state in which N discrete acts of measurement, whose possible ranges were $(1, 2, \ldots j_r)$, obtained the results i_r (where r runs from 1 to N). Then the physics corresponding to ψ is that at time t there exist independent classical worlds corresponding to each $\psi(i_1, \ldots, i_N)$ for which $a_{i_1, \ldots, i_N} \neq 0$ (i.e. a maximum of $\prod_{r=1}^{N} j_r$ such worlds), in which the measuring devices have registered the results described by $\psi(i_1, \ldots, i_N)$.

Now these axioms are open to criticism on many grounds — most obviously those of vagueness and complexity. (Note that axiom 1G is an axiom defining further mathematical formalism, yet involves a measuring device — at best a very unwieldy mathematical object.) Ignoring these problems for the moment, let us accept that, for any macroscopic observer, axiom 2G implies probabilistic statements about the state of the observed world at time t:

she finds herself equiprobably in one the $M \leq \prod_{r=1}^{N} j_r$ worlds, and this defines the "counting probability" of any statement about the results of measurements in her world, at that particular instant. What we want to point out is that Graham's claim that the counting probability equals the probability defined by the standard measure μ does *not* follow from his assumptions.

Graham's strategy is the following. He considers an observable X with eigenvalues X_i (i from 1 to n), a large number $N >> n$ of identical systems on which X is defined, and a macroscopic measuring device that measures the *relative frequencies* of the various eigenvalues in the identical systems. Finally he considers an observer examining the output of the measuring device. Then he argues that, from statistical assumptions about the initial state of the measuring device, it can be shown that after the second observation, in nearly all worlds the relative frequencies observed will be very close to those predicted by standard quantum mechanics.

Now we do not think Graham satisfactorily establishes even this last, restricted claim. His argument relies on a quantum statistical mechanical assumption about the probability distribution for the initial (normalized) state ψ of the macroscopic measuring device. What he assumes is that ψ is constrained to lie in some finite (n-)dimensional space $\mathcal{H}$, and that the expansion

$$\psi = \sum_{j=1}^{n} < \psi_j \ , \ \psi > \psi_j \tag{6}$$

of ψ in an orthonormal basis (ψ_j) for $\mathcal{H}$ defines a statistical measure for ψ as follows. Graham sets $< \psi_j \ , \ \psi >= x_j + iy_j$, and takes $(x_1, \ldots, x_n, y_1, \ldots, y_n)$ as coordinates on the $(2n-1)$-sphere S^{2n-1}. Now he defines the statistical measure λ for any set I of states as the ratio

$$\lambda(I) = \frac{\text{surface area of I on } S^{2n-1}}{\text{surface area of } S^{2n-1}} \tag{7}$$

Now this might be a natural statistical measure if the probabilities of events are ultimately based on the standard quantum measure μ, but there seems no reason to believe that it

would be the right statistical measure if the counting probabilities were fundamental in nature. Since the whole point is to derive μ from the counting probabilities, the argument seems circular. (It is of course useless to try and justify equation (7) by empirical data, since these data are supposed to follow from the fundamental assumptions.)

In any case, the proposition that Graham needs to establish is much more general than the one he actually considers. Measuring devices are not typically coupled to the relative frequency operator; it is more normal to record the results of individual observations in sequence.[3] Whether one wants to describe this as a complex observation by a single measuring device or a sequence of observations of identical systems by different measuring devices does not matter. The point is that Graham's argument simply does not cover this common (in fact generic) physical situation. In fact, it is trivial to see that the counting probability of a *general* sequence of observations of independent observables by independent measuring devices could only equal the standard quantum probability if the equality were to hold for a *single* measurement of a general observable. Now the equality does not hold for a single spin measurement, in the simple model discussed at the end of the last section. So Graham would have to argue that the quantum statistical mechanics of a true Stern-Gerlach measuring apparatus produces the counting probabilities $\frac{1}{3}$ and $\frac{2}{3}$ for the measurement discussed, and different counting probabilities in experiments where the electron had a different spin polarization. This is patently absurd, and certainly does not follow from Graham's assumptions about the statistical distribution of the measuring device's initial states. We conclude that counting probabilities are not relevant to physics.

C. DeWitt

DeWitt begins by explicitly presenting a "postulate of mathematical content" and a "postulate of complexity". The latter states that "The world is decomposable into systems

[3]This point, in a related context, is made by Stein [10] (pp. 641-2).

and apparata.", and we take this as meaning that DeWitt would accept Axiom 1G above.[4] However, DeWitt's position on the physical interpretation of the wave function is not clear to us. DeWitt refers to Graham's argument as an improvement on Everett's (see the footnote on page 185 of reference [28]), but does not actually invoke the counting probability at all. He makes no explicit postulate about the correspondence between the state vector and multiple worlds or their content. His discussion (pages 183-6 of reference [28]) of the memory content of automata is mathematically more sophisticated than that of Everett, but unsatisfactory for the same reason — the mathematical statement that a particular component of the state vector has measure zero is not physically interpretable when no physical properties of the measure have been postulated.

(We here have to digress a little, since DeWitt himself appears to make and then counter a point similar to ours: "The alert student may now object that the above argument contains an element of circularity. In order to derive the *physical* probability interpretation ..., we have introduced a *nonphysical* probability concept, namely that of the measure of a subspace in Hilbert space." DeWitt goes on to note that measures are also used in statistical mechanics. Now we are not sure that the alert student is making quite the same objection as ours: our objection is not that a measure is introduced in DeWitt's formalism; it is that its rôle in the interpretation is never explained. DeWitt may thus not be addressing our objection at all. But it *is* sometimes claimed in defence of MWI that there are analogous problems in some branches of statistical mechanics. It does not seem clear that the problems really are analogous, but if they were, this of course would be an argument that statistical physics is not well-founded, not an argument that MWI are valid.[5])

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[4]Although DeWitt suggests his postulates are already implicit in Everett's paper, [1] we agree with Bell [9] that the postulate of complexity is against the spirit of Everett's prose.

[5]Our rejection of this analogy is essentially isomorphic to Bell's discussion of classical and quantum mechanics on page 125 of reference [9].

Although DeWitt is unexplicit on the rôle of the measure, there is a formalization which does not seem in conflict with his writings. This formalization is a version of one due to Bell. [8,9] Classical variables have replaced the de Broglie-Bohm variables in Bell's formalization; we regard this as uglier, but it respects the spirit of DeWitt's axiom of complexity.

Suppose we adopt Axiom 0 and Axiom 1G above, together with:

Axiom 2D At any time t there is a continuum of independent classical worlds $W_s(t)$, labelled by a real parameter $0 \leq s \leq 1$. Physics consists in a statement of the possible classical contents (recorded by measuring devices) of the various worlds, and the (standard real number) measure of the set of labels s corresponding to worlds in which each classical content arises.

Axiom 3D Suppose that at time t the wave function $\psi(t)$ has the expansion

$$\psi(t) = \sum_{r_1 \ldots r_N} \psi(r_1, \ldots, r_N) \tag{8}$$

where $\psi(r_1, \ldots, r_N)$ is a state in which N acts of measurement obtained the results r_i (where i runs from 1 to N and the variables r_i may be continuous or discrete). Then the physics corresponding to ψ is that at time t, there exist classical worlds corresponding to each set of results $(r_1, \ldots, r_N)$, and the set of these worlds has measure $\mu(\psi(r_1, \ldots, r_N))$.

Now it is possible that, with a sufficiently long and careful classification of all known measuring devices, these axioms would become consistent with known experimental results. (Zurek [17] has given a careful discussion of how a preferred basis might be defined by measuring devices.) Our reasons for rejecting an MWI of this type — which appear below for completeness, but are identical to those given by Bell [8,9] — are therefore not strictly empirical. The first objection is that, precisely because this classification would be needed, the axioms should not be taken seriously as possible fundamental physical laws. The reductionist approach — explaining physical phenomena in terms of simple, mathematically precise, quantities — has been extraordinarily successful in almost all areas of physics. It goes

against everything we have learned about nature to propose a theory in which complicated macroscopic objects, whose precise definition must ultimately be arbitrary, are fundamental quantities.

A second objection is that this formulation destroys our concept of history. An observer at time t is in a world whose state (and thus, the observer's memory) has a certain probability distribution; at time t' another world, part of a different distribution, might contain an observer with qualitatively similar physical features. But there is no real connection between the observers, and their memory contents need not overlap at all. The idea of a classical object pursuing a (more or less) definite trajectory through space time has been lost, and we do not see that it can be recovered without extensively amending the formalism. This is a severe loss; it makes the formalism much uglier; it implies that human experience is a sequence of disconnected fictions. But perhaps these are not devastating weaknesses for the non-relativistic theory, considered purely as physics. However, the formalism also seems irreconcilable with special relativity. This is perhaps the strongest physical argument against having distributions of worlds $W_s(t)$ that are independent for each t.

As mentioned earlier, Bell's de Broglie-Bohm MWI avoids the objection of complexity, by working with particle position variables instead of classical variables. It may alternatively be possible to change the MWI above so as to regain the notion of a classical trajectory. The real problem for MWI advocates is to find a formalism which *both* avoids complex anti-reductionist axioms and allows classical trajectories to be defined.

This ends the first part of our discussion. Working from the papers advocating MWI, we have tried to extract axioms that are simultaneously well-defined, plausible as fundamental laws of nature, and consistent with known physics. We have failed; we do not think it is possible.

D. Note on Cooper-Van Vechten

As well as the papers already discussed, DeWitt and Graham's book "The Many-Worlds Interpretation of Quantum Mechanics" [28] contains a paper by Cooper and Van Vechten. [18] In a footnote, Cooper and Van Vechten explain the difference between their interpretation and Everett's. They don't advocate any sort of many-worlds picture, nor do they take the wave function to be a physical quantity; the physical quantities in their interpretation are events occurring in a single world. An event is defined as "the interaction of a system to be observed or prepared with another system which can be put into a state that is irreversible for reasons of entropy" — anything which is usually called a measurement is supposed to fall into this category. The correlations between events are calculated using purely hamiltonian evolution (no state vector reduction is postulated).

Here we just note that this isn't a many-worlds interpretation; its problems are essentially those of some standard interpretations of quantum mechanics, and beyond our present scope.

E. Hartle and Farhi-Goldstone-Gutmann

In a famous paper, Hartle defined and analysed the properties of the relative frequency operator which acts on the Hilbert space of an infinite ensemble of identical systems, by treating the operator as a limit of frequency operators defined on finite ensemble Hilbert spaces. Farhi-Goldstone-Gutmann (FGG) have recently presented a new treatment of relative frequency, in which the operator is defined directly (i.e. without a limiting process) on states representing the infinite ensemble. Formal theorems about the infinite ensemble relative frequency operator are clearly regarded by proponents of MWI as proving something useful to their case (Hartle's work is almost always cited by post-1968 proponents), but it is hard to extract any definite claims about precisely what use these theorems are *in the context of MWI.* (Clearly the theorems *are* useful in reformulating, or examining the foundations of, standard quantum mechanics.) So it is worth looking at the actual impli-

cations of relative frequency theorems for MWI. We examine FGG's results; aficionados of Hartle's should find it easy to translate. It should be stressed that references [4] and [5] do not directly advocate MWI; we make no criticism of reference [4]; a criticism of the physical interpretation which FGG ascribe to their results appears below; however, our main point in this section is to argue against the relevance of frequency operators to MWI, and in this argument we criticize proposals that are not suggested in either paper.

FGG examine the consequences of the following postulates:

Postulate I The states of a quantum system, S, are described by vectors $|\psi\rangle$ which are elements of a Hilbert space, V, that describes S.

Postulate II The states evolve in time according to

$$H|\psi\rangle = i\hbar\frac{\partial}{\partial t}|\psi\rangle \tag{9}$$

where H is a hermitian operator which specifies the dynamics of the system S.

Postulate III Every observable, $\mathcal{O}$, is associated with a hermitian operator θ. The only possible outcome of a measurement of $\mathcal{O}$ is an eigenvalue θ_i of θ.

Postulate IV$'$ If a quantum system is described by the eigenstate $|\theta_i\rangle$ of θ (where $\theta|\theta_i\rangle = \theta_i|\theta_i\rangle$) then a measurement of $\mathcal{O}$ will yield the value θ_i.

These differ from the standard postulates for quantum mechanics in that postulate IV$'$ has replaced the standard postulate:

Postulate IV If the state of the system is described by the normalized vector $|\psi\rangle$, then a measurement of $\mathcal{O}$ will yield the value θ_i with probability $p_i = | < \theta_i \ , \ \psi \ > |^2$.

and in omitting the projection (or state vector reduction) postulate:

Postulate V Immediately after a measurement which yields the value θ_i the state of the system is described by $|\theta_i\rangle$.

The omission of postulate V does not mean that FGG are directly arguing for pure hamiltonian evolution. Their primary concern is to analyse the probabilistic interpretation of a single act of measurement, and their discussion largely ignores the question of whether this measurement reduces the state vector. (In the final paragraphs of reference [5], FGG consider whether postulate V is in fact necessary. They claim that it can be neglected if one introduces a definition of measuring device into the postulates, although they do not necessarily advocate this procedure. This view possibly hints at an MWI rather like the formalization presented in the above discussion of DeWitt's paper. However, FGG's discussion of how postulate V might be omitted is not very explicit; it is clearly not the main point of their paper, and we shall not try to engage with them on this question.)

In order to justify replacing postulate IV by postulate IV$'$, FGG consider the implications of postulates I-III, IV$'$ for an infinite ensemble of identical systems. Let S be the single system. For simplicity, the state space of S is taken to be two-dimensional. Let $\mathcal{O}$ be an observable defined on S, θ be the corresponding hermitian operator with eigenstates $|\theta_i\rangle$ and eigenvalues θ_i, so that $\theta|\theta_i\rangle = \theta_i|\theta_i\rangle$ (for $i = 1$ or 2). Now let S be the system comprising a (countably) infinite number of copies of S, and let $|\psi\rangle$ be some state in S. FGG show that

$$F(\theta_i)|\psi\rangle^\infty = p_i|\psi\rangle^\infty \tag{10}$$

where the vector $|\psi\rangle^\infty$ represents the infinite ensemble of systems all in the state $|\psi\rangle$, the frequency operator $F(\theta_i)$ is associated with the observable that describes the proportion of subsystems in the eigenstate $|\theta_i\rangle$, and $p_i = |<\ \theta_i\ ,\ \psi\ >|^2$.

In summary, one knows from empirical evidence that the probability of obtaining the result θ_i from a measurement of O on the state $|\psi\rangle$ is p_i. FGG have clearly shown that this number can in principle be *calculated* from postulates I-III, IV$'$ without using the Hilbert space norm. This is a new and interesting result in the foundations of quantum mechanics. However, FGG seem to make a stronger claim (in section X of reference [5], and also implicitly in its title: "How Probability Arises in Quantum Mechanics"). This claim is that their formalism implies the physical interpretation of (not merely a way to calculate) the number

p_i, as the probability that a measurement on a single system will obtain the result θ_i.

The latter claim does not seem to be right. It is clear that (unless $|\psi\rangle$ is an eigenstate of the operator θ) postulate IV′ says nothing about measurements of θ on a single system, or of the associated frequency operators on any finite ensemble of systems. The postulate applies only if the single system is part of an infinite ensemble, and then it makes a statement about the quantum state of the entire ensemble. This implies no probabilistic statement about the states of any of the ensemble's members. For the state $|\psi\rangle^\infty$ does not ascribe a definite eigenstate to any individual system in the ensemble. (It can loosely be thought of as a superposition of all states in which the systems are in definite eigenstates, of which a proportion p_i are $|\theta_i\rangle$.)

A famously similar situation arises in a 2-slit diffraction experiment. Suppose we repeat many times an experiment in which a single electron is directed towards a scintillating screen in which there are two slits, beyond which there is a second parallel unblemished screen, and consider one such experiment in which at the time when the electron passes the plane of the first screen no flash is produced. Then if $|\psi_1\rangle$ and $|\psi_2\rangle$ are the wave functions associated with an electron going through slits 1 and 2 respectively, the electron's state after passing the first screen is $|\psi_1\rangle + |\psi_2\rangle$. This is an eigenstate of the hermitian operator $|\psi_1\rangle\langle\psi_1| + |\psi_2\rangle\langle\psi_2|$ with eigenvalue 1. There is a sense in which the statement "the electron went through precisely one slit" would thus follow from FGG's postulates. But one has to be careful in translating the mathematics into colloquial language. In the sense in which the statement follows from the mathematics, it is not a statement about classical events; it therefore does not imply the statement "the electron went through slit 1 with probability $\frac{1}{2}$". (This is of course good, since the last statement is inconsistent with the cumulative diffraction pattern observed at the second screen).

We are not criticizing the standard use of infinite ensembles to define probabilities. If it could be shown, *in the ordinary meaning of the words*, that a proportion p_i of the infinite ensemble will be in the state $|\theta_i\rangle$, we should agree that a probability interpretation is established. But the qualification is crucial. The standard calculus of probabilities applies

to sets of definite, classical events. It doesn't apply, or at least not straightforwardly, to quantum states which describe superpositions of many different sets of such events. The statement "the state $|\psi\rangle^\infty$ is an eigenstate of the relative frequency operator with eigenvalue p_i" is weaker than "the systems in the ensemble are each in definite eigenstates of θ, with a proportion p_i in the eigenstate $|\theta_i\rangle$". FGG establish the first statement, and then deduce a probability interpretation from the second. We see no way to bridge this logical gap in their argument.

So we have concluded that FGG's relative frequency theorem does not *per se* imply a probability interpretation for any individual system. How then could it be used to construct an MWI? There seem to be two possible lines of argument. The first is to suppose that in reality our universe *is* part of an infinite ensemble of identical universes, so that the FGG theorem would apply directly. The second would not require an infinite ensemble. Instead it would use Hartle's observation [4] that frequency operator theorems apply not only to ensembles, but also when a single system is found over time to be in the same state infinitely often. (Whether any given system will return to the same state infinitely often is a complicated question in nonrelativistic quantum theory. To answer the question in reality would seem to require both a quantum theory of gravity and a description of the initial state of the universe. Let us concede that it might be true.) The problem with both lines of argument is that they would describe physics as seen by supernatural observers who directly apprehend (or perhaps explicitly measure, by the sort of methods discussed in section X of reference [5]) the eigenvalues of frequency operators. Unless the postulates were amended so as to specify what is meant by a measurement and a measuring device, these observers would receive data corresponding to the frequency operator eigenvalues of every possible observable; the first type of observer would receive these data at every instant in time, the second presumably at $t = \infty$. In any case, the supernatural observers would have an entirely deterministic impression of physics. Suppose the universe's wave function describes (at least in some components) observers of the ordinary type (humans or automata). Then the supernatural observers would receive a frequency operator analysis (summed over ensembles or

over time, assuming in the latter case that the state vector of a given observer will faithfully be reproduced infinitely often between $t = \pm\infty$) of these ordinary observers' observations and memories. But now we have again reached the problem of how to extract a probabilistic interpretation, albeit in a different guise. The supernatural observer's interpretations make no statements about the memories or observations of individual ordinary observers. In particular, if we are assumed to be the ordinary observers, they do not account for our perception that definite events occur according to probabilistic laws.

We conclude that relative frequency theorems do not seem useful in defining MWI, nor do they seem relevant in deciding whether a given MWI is valid.

F. Comments on Geroch and Deutsch

Some interesting arguments have been put forward by Geroch, [6] in an article intended "to state the content of, and to some extent to advocate" Everett's interpretation. Deutsch [7] has presented thought experiments involving various types of "quantum parallel processing", which are intended to make state vector reduction implausible. We comment briefly on some points raised in these papers.

For Geroch, the notion of "preclusion" is fundamental. Roughly speaking, a region of configuration space is said to be *precluded* if the wave function ψ becomes at some time "small" in the region. The notion is illustrated by physical examples. Geroch then claims that: "Every physical prediction of quantum mechanics can be reformulated as the statement that a certain region is precluded." and that the essence (or "weak form") of the Everett interpretation lies in adopting the language of preclusion for quantum mechanics.

Now there are two obvious objections to this program. Firstly, it begins from a false premise. There *are* physical predictions of standard quantum mechanics which cannot be reformulated in the language of preclusion, for example "If a particle's spin along the x axis is $\sigma_x = +\frac{1}{2}$ and a measurement is made of σ_y, the definite results $\sigma_y = \pm\frac{1}{2}$ will be obtained with probability $\frac{1}{2}$." (The postulates of standard quantum mechanics, as framed

by FGG, were given in the last section as PI-PV. The statement in question follows directly from PIV.) Secondly, the notion of preclusion is actually a rather complicated and vaguely defined one. Stein has discussed this at some length in his reply [10] to Geroch; we briefly summarize (what we agree with Stein are) the problems. Preclusion seems to involve a preferred basis (defined by measuring devices, as discussed in section 2.3) in order to define precisely *which* small components of the wave function are to be precluded. And then, beyond failing to explain how definite results are perceived, preclusion does not explain how to calculate the probabilities of any given result, or of finite sequences of measurements: in short, it evades the question of how a probabilistic interpretation arises for the state vector at any given finite time. So it does not seem that preclusion helps in defining axioms for an MWI. Certainly Geroch does not claim to formulate axioms using preclusion, or even to give a precise definition of preclusion. The point is that such a program could not be carried through, or at least could not produce a good description of known physics. But then surely preclusion cannot really be a fundamental notion?

Both Geroch and Deutsch suggest that there is a useful analogy between the interpretations of general relativity and MWI. In general relativity, they point out, humans sweep out a world-tube. They further note that humans do not simultaneously perceive events occurring at all points within the tube, yet few physicists consider rejecting general relativity for this reason. The intended implication seems to be that the interpretational problems of a given MWI are similar, and so should be regarded as similarly unthreatening to the MWI's status.

However, the two theories are actually radically different. We don't think there is any fundamental obstacle to qualitatively modelling the *content* of successive states of human consciousness using matter distributions on a manifold. Although one does not know how to reconcile the actual structure of matter with the concepts of general relativity, one could presumably design automata with input devices by using idealized compressible fluids. Whether this would produce a satisfactory model of human consciousness or not, it surely wouldn't teach one how to obtain a probabilistic classical interpretation from a Hilbert space vector.

Now the intended point of the analogy may be that general relativity has not been shown to, or perhaps can not, explain the *fact* of human consciousness. If so, the same response applies here as to earlier analogies: the problem of defining or predicting self-awareness is a problem with general relativity; it may be a problem with any mathematical theory of nature; making this observation does not help to define an MWI. So, however intended, the analogy does not seem helpful.[6]

Finally, we note that Deutsch's main discussion involves thought experiments (Deutsch's experiments 2 and 3) whose outcome is quite uncertain. Deutsch assumes that "quantum parallel processing" (which relies on pure hamiltonian evolution of the state vector) will occur during the operation of various computing devices. There is presently no compelling reason for this assumption. (Nor would we necessarily interpret the results Deutsch predicts as compelling evidence for MWI.)

III. DISCUSSION

Everett's original attempt at a many-worlds interpretation was an interesting and well-motivated idea. Everett wanted simultaneously to simplify the axioms of quantum mechanics and to produce a realist physical theory (one which gives an explicit mathematical description of an external reality). However, it is clear that he did not succeed. Successive advocates of many-worlds interpretations, while usually claiming merely to clarify Everett's position, have substantially abandoned his goals. No proponent of MWI (as far as we are aware) has actually produced a complete set of axioms to define their physical theory. It seems that any such axioms must involve the extremely complicated notion of a measuring device, or that the physical reality they define will be one without any notion of timelike trajectory (and so, as Bell [8,9] has pointed out, not obviously consistent with special relativity), or both.

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[6]The points made in this paragraph are greatly elaborated in pages 644-6 of reference [10].

All of this suggests that Everett's original program has degenerated, apparently beyond repair. Although the logical possibility of an MWI consistent with known physics cannot completely be excluded, it seems that the defining axioms of such a theory would have to be extremely ugly and arbitrary. How then can the continued attraction of MWI be accounted for? The question is really one for future historians of science, but perhaps we can suggest two possible answers.

Firstly, the very failure of MWI proponents to axiomatize their proposals seems to have left the actual complexity of realistic MWI widely unappreciated. It may thus possibly be tempting for MWI advocates to assume that there is no real problem; that Everett's detractors either have not understood the motivation for, or merely have rather weak aesthetic objections to, his program. (Hence perhaps the otherwise inexplicable claim by one commentator that "Avoiding this [prediction of multiple co-existing consciousnesses for a single observer] is their [Everett's opponents'] motivation for opposing Everett in the first place." [7]) Secondly, MWI seem to offer the attractive prospect of using quantum theory to make cosmological predictions. The trouble here is that if an MWI is ultimately incoherent and ill-founded, it is not clear why one should pay attention to any quantum cosmological calculations based on it. Perhaps the right attitude should be to explore with interest, but to take any results that depend on MWI as no more than suggestions — not to be taken seriously without independent proof.

Beyond the community of MWI theorists, Everett's legacy seems to be the widespread belief that pure hamiltonian evolution is manifestly the correct description of nature (and "therefore" — although it does not really follow — that there is no problem with the foundations of quantum mechanics and no need to consider alternative theories). Although we have not argued against the possibility of an acceptable physical theory (other than an MWI) involving pure hamiltonian evolution, many arguments against MWI still apply. Most obviously, there is the problem of how a probabilistic interpretation with definite measurements arises from a deterministic wave function evolution which superimposes the state vectors of all possible measurements. So we do not see how to make such a theory precise without

assumptions such as those of de Broglie [20] and Bohm, [21] and are not enthusiastic about the de Broglie-Bohm theory (for ultimately aesthetic reasons — the theory gives a definite physical interpretation for quantum mechanics, but at the cost of introducing two levels of formalism related in a mathematically uninteresting way; it suggests nature is more malicious than subtle). We suspect any better, realist theory with purely hamiltonian evolution would have to augment or replace the standard quantum formalism by radically new mathematical concepts. In short, pure hamiltonian evolution seems a difficult hypothesis to justify (unless perhaps one abandons realism — and *that* seems still less justifiable for a physicist), and certainty that it is correct seems scientifically incomprehensible.

Critics of MWI are often asked what alternative they propose. Of course the criticisms of MWI stand or fall on their own merits, but let us briefly outline our best guess at the actual status of quantum mechanics. Similar views have been expressed by Leggett [22] and Penrose [23] (among others) and in the references cited below. Our guess is that the postulates of standard quantum mechanics (I-V above) are not an ultimate description of nature; the ultimate description is given by postulates as yet unknown, which are probably framed in a different mathematical language. However, insofar as the language of state vectors in a Hilbert space applies to physics, postulates I-V are a good approximation to the behaviour of microscopic systems. In particular, the state vector reduction postulate is an approximate description of what happens to a microscopic system when it interacts with a macroscopic one. For macroscopic systems, either the postulates do not hold (the Schrödinger equation ceases to be a good approximation and the postulate of state vector reduction ceases to be well-defined) or the whole language of state vectors is inapplicable. The distinction between microscopic and macroscopic should be one of degree not of kind, and should follow from the unknown fundamental postulates.

Very interesting models of possible sub-quantum physics, apparently consistent with known experiment and with all the properties required in the last paragraph, now exist. The key idea for these models was put forward by Ghirardi-Rimini-Weber; [24] a very clear exposition and new interpretation of the mathematics has been presented by Bell; [25] a pos-

sible treatment of indistinguishability has been given by Ghirardi et al.; [26] our preferred interpretation, and a treatment of indistinguishability that does not jeopardize locality, can be found in reference [27]. Whether these models (or later theoretical developments along similar lines) are relevant to physics will ultimately be decided by experiment. Theoretical and experimental programs investigating the physics that might underlie state vector reduction seem to us very strongly motivated; one purpose of the present article is to argue against the view — more often stated than explained — that the problems these programs seek to investigate have already been solved.

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