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The relative states and many-worlds interpretations of quantum mechanics and the EPR problem

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Abstract. The basic contributions to the concept of the many-worlds interpretation of quantum mechanics are analysed. It is found necessary to divide them into two classes corresponding to (a) the relative-states interpretaion (RSI), which avoids problems of measurement, but is found generally unconvincing, and (b) the many-worlds interpretation (MWI), which is more comprehensible, but has all the problems of the von Neumann scheme. The EPR problem is tackled using each interpretation. The RSI finds no problem, while the MWI meets the same difficulties as conventional interpretations.

1. Introduction

In two recent publications, Page (1982a), and Kunstatter and Trainor (1984) have analysed the famous argument of Einstein, Podolsky and Rosen (1935) (EPR), that quantum mechanics cannot give a complete description of physical reality. They use the many-worlds interpretation of quantum mechanics. Although they agree on the formal analysis they come to contradictory conclusions. Page (1982a) claims that, in this interpretation, the EPR analysis is 'simply incorrect'; quantum mechanics can completely describe what may actually be predicted about any system. Kunstatter and Trainor (1984), on the other hand, while holding the many-worlds interpretation in high regard, state that EPR reality cannot be described completely by quantum mechanics in this or any other interpretation.

In the present paper it is argued that this disagreement results from a fundamental ambiguity concerning what is taken by different authors as constituting the many-worlds interpretation. We shall slightly simplify this ambiguity by saying that what is presented by different authors as the many-worlds interpretation, includes, in fact, two different interpretations. The first, put forward most clearly by Cooper and van Vechten (1969) is called here the relative-states interpretation of quantum mechanics (RSI). The paper by Zeh (1970) appears very close to this extreme position. We shall also argue that the original work of Everett (1957a, b) is close to this position in spirit. (Everett 1957a has been reprinted as Everett 1973.) This interpretation, we suggest, has been used by Page (1982a).

We further suggest that the subsequent ideas of de Witt (1968, 1970), though deriving from the considerations of Everett, in many ways are rather contradictory, and should be considered as constituting a different interpretation. It is to this interpretation that we shall from now on restrict the term many-worlds interpretation

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(MWI). It is this interpretation that we believe is the basis of the work of Kunstatter and Trainor (1984).

2. The legacy of von Neumann

Before discussing Everett's ideas, let us first consider the problems he saw his interpretation as solving, or at least avoiding. The problems lie, of course, in the area of quantum measurement, in the most widely held theory of von Neumann (1955). In his formulation quantum mechanical changes are of two types. First there is the continuous deterministic change of state of an isolated system according to the Schrödinger equation which applies at all times other than those corresponding to a measurement. At these times the second type of change takes place, a discontinuous process in which the wavefunction collapses to one of the eigenfunctions of the operator corresponding to the observable being measured.

One may regard this dichotomy as unsatisfactory in principle. More serious though is the fact that it pre-supposes a clear criterion as to whether or not an interaction between two systems should be regarded as a measurement by one system (the observer) of the other (the observed). One could hope that it would be possible to regard both observer and observed as subsystems of a combined system which would then change continuously according to the Schrödinger equation, no change of the second type taking place. Everett (1957b) also points out that there is no way in which the von Neumann scheme may provide generally for approximate measurements in which observer and observed interact only weakly and for a limited time.

The elimination of the concept of the observer from quantum mechanics thus appears a desirable goal advocated by several authors from different points of view (Bell and Nauenberg 1966, Maxwell 1972, Singh and Whitaker 1982). These authors do not provide much concrete help in going about this task, while most of the present paper is devoted to a study of how Everett and followers manage to do so and the problems they meet.

It should be mentioned, though, that according to what might be called the 'official' Copenhagen interpretation no such problem exists. In an article written with the aim of clarifying this interpretation which was welcomed by Heisenberg and Rosenfeld, Stapp (1972) calls von Neumann's approach the 'absolute-ψ' interpretation, and claims that it and the Copenhagen interpretation are 'diametrically opposed'. In the Copenhagen interpretation, he says, it is emphasised that the wavefunction describes evolution of probabilities, not of actual things. In a measurement the specification of the system is changed, the probabilities of the outcome of the same or different measurements must change, and hence it is natural that the wavefunction changes also.

There is one problem, though, also present in the von Neumann approach that the Copenhagen interpretation certainly cannot solve. This problem was particularly acute for Everett, who was concerned with the problem of quantising general relativity, and would thus need to be able to write down the wavefunction of the whole Universe. Naturally for such an object there can be no external observer, performing measurements and producing von Neumann collapses of wavefunction. Equally, writing down the wavefunction of the Universe is unthinkable in the Copenhagen interpretation. Stapp (1972) stresses that, in this interpretation, the laws of quantum mechanics are applied to a given physical system, prepared in a specified manner, and later examined in a specified manner. Clearly the whole Universe could be neither prepared nor examined by an outside observer.

3. Everett's solution

The solution to the problems proposed by Everett, and all the authors discussed here, is, as is well known, to do away with the collapse of wavefunction. Let us discuss this in a little more detail.

In all interpretations, in a measurement there is an initial correlation of wavefunctions of observed system and observer. Consider a measurement of observable O, where the associated operator $\hat{O}$ has eigenfunctions ϕ_n and corresponding eigenvalues E_n. If the initial wavefunction of observed system is

$$2^{-1/2}\{\phi_1+\phi_2\},$$

the correlation procedure may be roughly represented by:

$$2^{-1/2}\{\phi_1+\phi_2\};\sum_i a_i\alpha_{E_i}\rightarrow 2^{-1/2}\{\phi_1\alpha_{E_1}+\phi_2\alpha_{E_2}\}. \tag{1}$$

Here the α's represent the states of the observer. If he (or it, if we are really considering a measuring device) is in state α_{E_n}, this implies that a measurement has been made with the result E_n. On the left-hand side there is merely a linear combination of the α's; we do not need to know anything about the a_i as we are not interested in the state of the observer before the measurement. The right-hand side of (1) shows the state of the observed system to be correlated with that of the observer.

(This form of wavefunction will last, of course, only for a short time (Peres 1980). It assumes a highly simplified observer system (measuring device) with only a very few degrees of freedom. These degrees of freedom, influential in the original correlation, cannot be isolated from the other degrees of freedom of the system. The further evolution of the observer system is irreversible. In this paper, though, we do not need to consider this further process.)

According to von Neumann or Copenhagen the correlated system is not left in the state given by the right-hand side of (1), but wavefunction collapse takes place according to

$$2^{-1/2}\{\phi_1\alpha_{E_1}+\phi_2\alpha_{E_2}\}\rightarrow\phi_m\alpha_{E_m} \tag{2}$$

where m may be 1 or 2 with equal probabilities.

According to Everett, however, no such collapse takes place, and the total system is left in the state given by the right-hand side of (2) not (1). Such a procedure, if it may be carried out successfully, obviously removes at a stroke the problems mentioned in the previous section. The need to specify, arbitrarily, whether a given subsystem is to be regarded as an observer, is removed; Wheeler (1957), in a note supporting Everett's conjecture, emphasises that the theory of observation becomes just an example of the theory of correlations between subsystems.

Clearly, though, at least as many questions are raised as are answered. In practice we always recognise the observer system (measuring device) as being in one eigenstate giving one reading for the measurement, rather than in a linear combination of eigenstates each giving different readings.

What can one say about the 'other' components? The distinction we consider to be present among Everett's followers is between those who deliberately give minimal answers to this question, and those who attempt to provide a detailed answer.

4. The relative states interpretation (RSI)

The extreme case of those who provide a minimal (in fact zero) response to the question of the other components is the paper by Cooper and van Vechten (1969). This was written twelve years after Everett's work, though without knowledge of it. When, before their paper was published, they became acquainted with Everett's work they recognised the ideas as 'quite similar'. (Other authors, such as Jammer (1974) and de Witt (1971) certainly accept the paper as being in the spirit of the many worlds interpretation.)

The ideas are highly individual and thought provoking; the mind of the observer is placed emphatically inside the realm of the Schrödinger equation. It is possible to identify the formal analysis with that of Everett with the express condition that the α's in (1) and (2) represent, or at least include, the state of the observer's mind. It is frankly admitted by the authors that the total wavefunction of observed system plus observing apparatus plus mind remains in the linear combination corresponding to the right-hand side of (1). What is important, they state, is that the mind *is*, in fact, in a particular state, knowledge of which need not be reflected in the wavefunction. Rather they concentrate on the fact that there must be agreement between ourselves and other observers and detectors as to which of the two states, the system (including the mind) is actually in. (As to the other component, they remark, perhaps a little laconically, that if we know the system is in state ϕ_1, we will not ask the value of the coefficient of ϕ_2.)

Their paper represents the extreme position we categorise as the relative-states interpretation. Close to it is the equally interesting paper by Zeh (1970). He first shows that, because of the dense energy spectra of macroscopic systems, such systems, even when separated by large distances, must be strongly correlated in their microscopic states. Thus where we have referred to a wavefunction of the observing system, Zeh would replace that with a wavefunction for the whole of the rest of the Universe. One is therefore led towards an interpretation of the RSI or MWI type, and a wavefunction analogous to the right-hand side of (1) not (2).

Zeh discusses superposition of macroscopic states by analogy with a right-handed sugar molecule for which a state $\psi_R \pm \psi_L$ has been prepared. The two components behave practically independently. Similarly if ψ_R and ψ_L represent states for different pointer positions for a macroscopic measuring device, each state will evolve independently, producing different final states, which will be correlated with different observer states. The different components will be, in Zeh's term, 'decoupled', a procedure he identifies with wavefunction collapse.

Zeh does not appear to take the other component particularly seriously. It may be omitted, he says, 'pragmatically'. Since it cannot be observed its only purpose is 'to save the consistency of quantum theory'. The question of whether the other component exists after a measurement is 'meaningless'. (What *is* meaningful is that there is no contradiction within a given component.)

Before leaving Zeh's paper, it is worth remarking that he used his analysis of superpositions of different macroscopic states to give a plausible explanation of superselection rules (Wick *et al* 1952).

We turn lastly in this section to the original work of Everett (1957b). Again we feel he attributes *comparatively* little importance to the components of the wavefunction other than that appropriate for a given observer (though admittedly more than the authors of the two other papers discussed in this section). He writes, for example, that throughout a sequence of observation processes 'there is only one physical system representing the observer', and (rather mildly) that it is 'unnecessary to suppose' that all components bar one are destroyed.

Rather than dwelling on the existence or otherwise of the other components, Everett stresses other aspects of the theory. First he lays great emphasis on the initial correlation between observer and observed subsystems. He thus gives his formulation the title 'relative-states interpretation' (not 'many-worlds interpretation'). Jammer (1974) justifiably comments that this part of the work only 'follows the trodden path of the conventional theory'.

Secondly, like the authors of the other two papers discussed in this section, he stresses consistency within a given world component. One may, in fact, question whether this is really surprising; despite the sophistication of his use of measure theory in Hilbert space, in his final construction any one of his world components corresponds rather directly to the only component in conventional theory, for which such consistency would not be in doubt.

Lastly he puts emphasis on his claim that the probabilistic features of quantum mechanics are 'derived from the theory itself' rather than being postulated in advance. Wheeler (1957) says that the problem of multiple observers 'solves itself' not by 'adding the conventional theory of measurement'. Such claims appear to rest on Everett's proof that the only choice of measure consistent with certain requirements reproduces the results of the conventional theory of observation. It might be suggested, though, that his results depend crucially on certain features of standard quantum theory, such as normalisation of states and wavefunction, which are provided so that the conventional theory of measurement is consistent. Thus it appears plausible that Everett's analysis is, at least in part, parallel to, rather than independent of, conventional formalism. This can be only a brief comment on a subtle and important matter. For similar analysis to that of Everett and other comments see Hartle (1968), Graham (1970), Ballentine (1973), Mugur-Schächter (1974), and de Witt (1968, p 330).

Our concern here is not the detailed criticism of any of the papers discussed. Each of them introduces startlingly original ideas. However, to the extent that they are prepared to disregard unwanted world components, they may fail to convince most other physicists. Jammer (1974) points out that for ten years after Everett's ideas were published they attracted so little attention as to be described as 'one of the best kept secrets of the century'.

5. The many-worlds interpretation (MWI)

The position of this paper is that the work of de Witt (1968, 1970), which, in contrast to the papers just discussed quickly attracted a great deal of attention, should be regarded, not merely as an elaboration of that of Everett, but as constituting an entirely new interpretation. Rather than ignoring, or at least playing down, the remaining world component(s), de Witt lays great stress on them. The two terms on the right-hand side of (1), he claims emphatically, both exist, in two different worlds. We thus call de Witt the progenitor of the MWI as contrasted with the RSI; the MWI has been elaborated in particular by Graham (1970).

To our mind the clearest indication that the other components and the other worlds are not to be regarded in any way as a mathematical fiction is given by de Witt (1971) as part of his response to a series of criticisms (Ballentine *et al* 1971) of his 1970 paper.

On an aircraft about to crash, he would acknowledge, he says, that there are 'other guys' on the aircraft destined to land safely, but would worry about himself in his own world component. It may be the frankness with which de Witt meets the challenge of the other components which has led to the surge of interest in the MWI; this interest has, perhaps, been stimulated rather than the reverse by the novelty and strangeness of the solution.

We claim here that the change from the RSI to the MWI again creates as many problems as it solves. We point out that it is wishful thinking to suggest that a measurement according to the MWI (as distinct from the RSI) is represented by (1) on its own, without acknowledgement that the two terms on the right-hand side do correspond to different worlds. (The latter point we insist on as non-trivial!)

Once this is recognised it is clear that most of the problems Everett initially wished to solve, which disappear in the RSI, re-appear in the MWI. The distinction between what is an 'observation' and what merely is an 'interaction' is just as important in de Witt's formalism as in that of von Neumann. For von Neumann it determines whether there will be wavefunction collapse; for de Witt whether there will be world splitting. The treatment of approximate measurements is as awkward for de Witt as for von Neumann. Whereas the RSI is ideal for addressing the quantisation of general relativity, the re-emergence of the observer in the MWI is again counter productive. Neither does de Witt help matters much in his 1970 paper with his statement that 'the Universe is constantly splitting into a stupendous number of branches, all resulting from *measurement like interactions* between its myriads of components' (our italics). Here he wishes to get round the problem of what must be considered a measurement by allowing *any* interaction to be so categorised. However all systems interact continuously with all other systems. We have already, in this paper, met Zeh's demonstration that the states of macroscopic systems are strongly correlated even when they are very long distances apart. This can be no real answer.

Ballentine (1973) has argued along these lines. He suggests that there is an ambiguity in the MWI since it is not known into linear combinations of which set of eigenstates a wavefunction must be decomposed for world-splitting purposes. He says that in practice the representation chosen is that which diagonalises the observable being measured, but suggests that this is 'contrary to the spirit of Everett's programme' We hope to have demonstrated that it is certainly contrary to Everett and the RSI, but in no way contrary to de Witt and the MWI.

We mention lastly de Witt's claim, following on from that of Everett, that the probabilistic features of quantum mechanics are derivable from his formalism, that the MWI formalism 'yields its own interpretation' (de Witt 1970). In response to the criticisms mentioned above, he reduced this claim (de Witt 1971) to the one that the MWI is the only interpretation of quantum mechanics that adds nothing to the formalism. The view of the present paper must be that this is true of the RSI, but decidedly not of the MWI. World splitting is certainly not 'nothing'!

6. Further remarks on the RSI and MWI

It may perhaps be suspected that our distinction between the RSI and the MWI is a little tendentious. It will be frankly admitted that detailed exegesis of a few of the papers discussed, particularly the enigmatic contributions of Everett, does not provide fully convincing evidence of the authors' intentions. It cannot, though, be disputed that there is an unbridgeable gulf between Cooper and van Vechten on the one hand, and de Witt on the other. A few more observations from the literature will now be added as further evidence.

First we note that Cooper (1973), in a later paper mainly restating his previous ideas, discusses de Witt's world splitting, and is critical of it.

Secondly we consider an interesting paper of von Weizsäcker (1973), who certainly does not interpret Everett as implying reality for the 'other' component. He regards Everett's analysis merely as saying that, for the observer who sets up the system to be observed, as given on the left-hand side of (1), the unreduced wavefunction is correct as long as he does not know the outcome of the observation. Everett, he suggests, merely redefines words so that what would normally be termed 'possible' is termed 'actual'.

Thirdly we mention remarks of Page (1982b), who, as previously mentioned, is an advocate of what we would term the RSI. In conformity with that he appears quite keen to accept Margenau (1963) as being consistent with an Everett-type interpretation, despite the fact that the paper mentioned certainly does not advocate world splitting.

7. The actual and 'vulgar' EPR problems

We now move on to consider the EPR experiment according to the RSI and the MWI. Since some of the very brief discussions we shall mention (notably those of Everett (1957b) and Zeh (1970)) certainly do not do justice to the subtlety of the EPR argument, we give in this section a brief account of this in the form presented by Bohm (1951) and Bohm and Aharonov (1957).

A particle of spin zero decays into two identical spin-$\frac{1}{2}$ particles moving in opposite directions along the x-axis (of the rest frame of the decaying particle). The wavefunction may be written as

$$\psi = 2^{-1/2}\{\sigma_{1Z}^{+}\sigma_{2Z}^{-} - \sigma_{1Z}^{-}\sigma_{2Z}^{+}\}. \tag{3}$$

A measurement is now made of σ_{1Z}. Such a measurement cannot disturb particle 2, but nevertheless tells us the value of σ_{2Z}. EPR say that this implies that σ_{2Z} represents an element of physical reality. The same considerations apply, though, if one made the measurement on the y- rather than the z-component of particle 1, so σ_{2y} must also represent an element of physical reality. However since the corresponding operators do not commute, the laws of quantum mechanics do not allow both σ_{2y} and σ_{2z} to be elements of physical reality, and this is the EPR contradiction. The conclusion of EPR was that the present structure of quantum mechanics is not complete. If the problem they present is accepted, the response must be addition to, or modification of, the basic laws of the subject.

In this way it is contrasted with a much simpler argument which is occasionally (incorrectly) taken to be the EPR argument. This is *merely* the prediction of action at a distance, an observation on one subsystem affecting instantaneously a second subsystem spatially separated from the first. We term this the 'vulgar' EPR paradox. We note that this argument is directed in fact towards observation rather than theory. It may indeed be presented in the form:—quantum mechanics predicts non-locality which seems impossible, so quantum mechanics cannot be correct. However it may be turned, via the famous work of Bell (1964) to the proof (d'Espagnat 1979) that no theory which accepts realism (the doctrine that observed phenomena are caused by some physical reality independent of human consciousness), normal modes of logic, and Einstein separability, can duplicate the predictions of quantum mechanics in this area. There seems to be fairly good evidence that the quantum mechanical predictions are valid experimentally (Clauser and Shimony 1978, Aspect *et al* 1982) (though there is still discussion on this point as seen in the work of Marshall (1984) and Caser (1984) and references in these papers). To the extent that it is accepted, it appears that the vulgar EPR paradox can have no 'solution'; we just have to accept that the world is not as we might wish, and no new theory or interpretation, consistent with the three conditions above, can change that.

8. EPR according to the RSI

We commence our study of the EPR problem as treated by the RSI with the brief discussions of Everett (1957b) and Zeh (1970). Everett's discussion cannot be said to relate to the EPR dilemma in any way. We have already noted that he pays great attention to consistency of successive observations by different observers in any particular branch. His discussion of experiments of the EPR type is merely a check of such consistency-in particular of the fact that actions of the second observer have no effect on subsequent measurements of the first, when, as in EPR, they deal with separated subsystems. Such a discussion seems unnecessary as inside any branch there is no suggestion of any such problem.

Zeh (1970) claims to dispose of the problem 'straightforwardly'. There are, he says, two world components. If measurements are taken of the z component of spin, in one world component the result will be σ_{1Z}^{+}, and hence σ_{2Z}^{-}, in the other σ_{1Z}^{-} and σ_{2Z}^{+}. Since these components cannot 'communicate' the result is 'in accord with the axiom of measurement'.

Such a comment is indeed terse. (We ignore, incidentally, the fact that, at this one point in his paper, Zeh appears to be closer to what we would term the MWI than to what we call the RSI.) It does, in fact, contain virtually all that is necessary to account for the *actual* EPR paradox on the basis of the RSI. According to the interpretation, the wavefunction does not collapse. It remains as given by (3). Thus the remark of EPR concerning the known value of σ_{2Z} simply does not apply, and this quantity is not established as an element of physical reality. The right-hand side of (3) merely represents a correlation between the values of σ_{1Z} and σ_{2Z}. Equally (3) may be rewritten as

$$\psi = 2^{-1/2}\{\sigma_{1y}^{+}\sigma_{2y}^{-} - \sigma_{1y}^{-}\sigma_{2y}^{+}\} \tag{4}$$

to establish correlation between measurements of σ_{1y} and σ_{2y}, but again without evidence of physical reality.

Of course, while the RSI insists that the wavefunction does not collapse measurements will yield results according to one of the branches. However the RSI insists that it is simply illegitimate to enquire (as EPR do) as to why observations indicate that the observers are in one particular branch.

It is in this sense that the RSI may indeed claim to explain the *actual* EPR paradox, within, of course, its own terms of reference. We have already suggested that many physicists apparently find the refusal of the RSI (as distinct from the MWI) to discuss the other branches, unsatisfactory. Its treatment of the EPR problem may considerably increase their sense of unease.

Of course the RSI has no explanation of the vulgar EPR paradox; no such explanation is possible, as we have explained.

Let us now see how closely the discussion given by Page (1982a) agrees with the one just given. His formalism certainly does so. The only real elaboration is that he includes explicitly the correlation between spin functions and observer wavefunctions, as we did in (1) and (2).

It must be said though that certain emphases of Page are surprising. Rather than stressing the non-collapse of wavefunction in the RSI, his argument appears to rest more on his use of the density matrix. If one represents the state of the system by a density matrix then of course one need not insist on a collapse of wavefunction, as a result of a measurement, whether one uses the RSI, the MWI or any other interpretation. The state of the combined system of observed object plus measuring device may indeed be expressed in a form corresponding to the right-hand side of (1).

As has been stressed before (Whitaker and Singh 1982) use of the density matrix is equivalent to using an ensemble interpretation of quantum mechanics; it is entirely independent of using an RSI or a MWI. The ensemble interpretation, and such an analysis of the EPR experiment, has been championed by Ballentine (1970). Even if it may be admitted in support of such an interpretation, it seems illogical to present it as a *solution* to the EPR problem. As Ballentine (1972) points out, Einstein was an advocate of the ensemble interpretation (see, for example, Einstein 1949), and the conclusion of EPR that quantum mechanics was not a complete theory, was almost certainly intended to point precisely in that direction.

Of course, as Mattuck (1982) shows, even in an ensemble interpretation the act of measurement does produce a change in the probability distribution of some observable—in the EPR case that of total spin. As he points out this is just as good a demonstration of non-locality as the (vulgar) EPR paradox itself, as indeed is any measurement, other than a non-destructive one (Caves *et al* 1980). From this point of view the EPR experiment is not particularly special, but of course it is very vivid, as well as useful for such analysis as that of Bell.

From our perspective, the important point is that, while both RSI and ensemble interpretation have at least partial success in dealing with the EPR problem, their effects should be studied individually. For example, de Witt (1968) stresses that he is not using an ensemble, and (1971) criticises the ensemble interpretation.

It is interesting to look at one further brief account of the EPR problem in this type of interpretation. Jammer (1974) says that the EPR argument has 'lost its point', but the interpretation leaves the question of why the observer finds himself in one branch rather than the other. In our terms Jammer appears to be adopting the RSI.

9. EPR according to the MWI

We now turn to the MWI and see how it deals with the EPR problem. As we have previously stressed, it cannot be accepted that, under the MWI, the wavefunction after measurement is given by the right-hand side of (3), or (4). It is an essential part of the MWI that the two components belong in different worlds. For measurement of the z-component of spin, the wavefunction after measurement must be written as

$$\psi = 2^{-1/2}\{(\sigma_{1z}^{+}\sigma_{2z}^{-})_a - (\alpha_{1z}^{-}\sigma_{2z}^{+})_b\} \qquad (5)$$

while for measurement of the y-component it must be written as

$$\psi = 2^{-1/2}\{(\sigma_{1y}^{+}\sigma_{2y}^{-})_a - (\sigma_{1y}^{-}\sigma_{2y}^{+})_b\} \tag{6}$$

where a and b indicate that the components exist in different worlds. It is emphasised that (5) and (6) are certainly not identical.

How one discusses the EPR experiment in this context depends to an extent on one's model for world splitting. While one is naturally on thin ground here, it seems essential that the 'process' obeys the laws of relativity. After a measurement at a particular point, the world-splitting process will thus take place on a wavefront moving outwards from that point with velocity c. While such a condition may appear rather prosaic for such an exciting event it would surely be accepted as inappropriate for speculative and dramatic ideas to be immune from the constraints on more easily comprehensible theories.

Translation of the EPR argument to MWI terms is, we suggest, as follows. Because world-splitting as a result of the measurement on the first spin moves outwards only with velocity c, a measurement on the second spin, separated from the measurement of the first by a spacelike interval, will find the world unsplit as far as the second spin is concerned. We would expect, then, the second measurement to cause a second world splitting, with values of σ_{2Z} (or σ_{2Y}) in the various worlds uncorrelated with values of σ_{1Z} (or σ_{1Y}). Since correlation *is* observed, this implies that the values of σ_{2Z} *and* σ_{2Y} for each world are somehow pre-ordained, before any observation takes place. This is the equivalent of saying, in the conventional EPR formalism, that σ_{2Z} and σ_{2Y} are elements of physical reality. Since, in any putative component, σ_{2Z} and σ_{2Y} cannot *both* be pre-ordained there is a problem, completely analogous to the usual EPR problem.

Our conclusion, then, which is no surprise after the considerations of § 5, is that the MWI finds the EPR analysis just as large a problem as do more conventional interpretations. The argument appears close to that of Kinstatter and Trainor (1984). We find though their emphasis on the existence or otherwise of 'intelligent observers' unnecessary. The use of (5) rather than (3) and (6) rather than (4) is a direct result of using the MWI rather than the RSI, not a result of demanding 'intelligent observers' in worlds a and b. It is this that is at the heart of the EPR problem in the MWI, as (6) differs from (5), whereas (3) and (4) are identical.

10. Summary and conclusions

The argument of the paper has been that the relative states and many-world interpretations of quantum mechanics (RSI and MWI) should be regarded as distinct. The first is a solution of the measurement problem and the EPR 'paradox', but only in its own terms, which we, along with, apparently, the majority of physicists, find unconvincing. The second is far more specific in its explanations, but, we believe, reintroduces the problems of measurement that the RSI was designed to eliminate. Its failure with EPR 'paradox' is an example of this.

Since we consider that no interpretation of quantum mechanics is without serious difficulty, we do not suggest that the RSI or the MWI should be abandoned. One is, though, a little surprised by the growing popularity of such interpretations, indicated by some of the papers cited here. (It is interesting to note, though that, after his initial advocacy of Everett's work, Wheeler (1979) appears to have turned against the ideas.)

We feel that some of the popularity may be the belief that, however strange the concepts, they do at least explain the facts. In our terms that would be conflating the virtues of RSI and MWI, and ignoring the failures of each.

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