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COMMENT

On the observability of 'many worlds'

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Abstract. In an attempt to search for evidence that gravity is not quantised, Page and Geilker have looked for those components of the wavefunction in an Everett-type interpretation of quantum mechanics which are usually asssumed unobservable, in an experiment involving gravitational attraction between two lead balls. It is pointed out that they use one of several possible interpretations of this type, an interpretation we claim to be untenable, as it is not consistent with a very wide range of experimental evidence.

1. Introduction

In a recent paper, Page and Geilker (1981) (PG) have claimed that they have tested the simplest possible non-quantised theory of gravity, the semiclassical Einstein equations. They did so by searching (unsuccessfully) for those components of the wavefunction of the universe in an Everett-type interpretation of quantum mechanics (Everett 1957, 1973), which are usually ignored. These are the ones which, in the terminology of De Witt (1968, 1970), belong to 'other worlds'. Their work has been criticised by Hawkins (1982) and Ballentine (1982), and Page (1982a, b) has responded.

Judgements as to the importance of the experiment of PG vary enormously. Ballentine (1982), for example, suggests that a less surprising experimental result has seldom, if ever, been published. Yet it must be admitted that if the results *had* been positive, they would have constituted perhaps the most amazing discovery in the history of science, indeed in the history of mankind, access being gained, in principle and to at least a reasonable extent in practice, to other possible modes of development of the universe, and to other conceivable types of existence on the Earth, all as dictated by the 'other' components of the Everett wavefunction. Thus it seems of interest to return to the motivation of the experiment, and to re-analyse the basic assumptions and ideas.

Such a process is of particular interest to the present author because in a previous paper (Whitaker 1985) (I), a study was made of the various contributions to the many-worlds interpretation (MWI) of quantum mechanics, and it was concluded that there was a considerable difference in the approaches of different authors. The conclusion of the present paper is that the results of the experiment being discussed, and a critical analysis of the ideas involved, have rather more to say about which varieties of MWI are tenable than about possible theories of gravitation.

2. Theoretical background

It is generally assumed that the gravitational field must be quantised, and a considerable amount of work has been devoted to studying how the quantisation takes effect (see,

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for example, the volumes edited by Isham *et al* (1975, 1981)). Yet, because of the weakness of gravity, there is no hard evidence that this quantisation actually takes place. Rosenfeld, the first (in 1930) to attempt the problem of quantising gravity, suggested much more recently that there was, in fact, no compulsion to quantise, the question remaining absolutely open (Rosenfeld 1967), and even that there was a strong suspicion that such quantisation would in any case be meaningless (Rosenfeld 1963). Møller (1962) and Rosenfeld (1963) suggested a semiclassical theory in which the expectation value of $T_{\mu\nu}$, the stress-energy tensor, is the source of the gravitational field:

$$G_{\mu\nu} = 8\pi\langle T_{\mu\nu}\rangle. \tag{1}$$

There would appear, though, to be two major problems in intermeshing quantised matter with unquantised gravitational fields (see references in PG).

The first concerns the question of whether semiclassical gravitation would allow one to circumvent the uncertainty principle. Eppley and Hannah (1977) show that, in a measurement of the position of a quantised particle using a non-quantised gravitational wave, and if one assumes collapse of the wavefunctions then either the uncertainty principle is not obeyed, or momentum is not conserved. If, on the other hand, one assumes no collapse of wavefunction, it is possible to send a signal at greater than the velocity of light. PG suggest that these difficulties should not necessarily cause one to abandon the idea of classical gravity, since such unexpected effects have not been ruled out experimentally.

They concentrate rather on the problem of the actual collapse of the wavefunction. In general the right-hand side of (1) will change abruptly at a measurement, if one uses the usual von Neumann (1955) interpretation of quantum mechanics, and this PG (and other authors they cite) regard as unallowable. They therefore turn to the MWI of quantum mechanics in which the wavefunction is not permitted to collapse.

The experiment PG perform to check this possibility is as follows. In a series of measurements, a decision is reached via a quantum-mechanical splitting involving radioactive decay. This decision determines into which of two configurations two lead balls are placed, and the gravitational attraction between the balls is studied. PG suggest that, if the MWI is appropriate, the quantum-mechanical decision procedure will not cause the wavefunction to collapse, and the gravitational field between the balls would be that of the average mass distribution over both configurations.

3. Discussion

The analysis by PG suggests the possibility of access to the other components of the MWI wavefunction, those corresponding to other paths the universe might have followed. As they admit, this takes them far beyond any of the other exponents of the MWI, for all of whom it is a *sine qua non* that no observational consequences different from that of the von Neumann theory follow from their novel interpretation.

Their argument is that their semiclassical MWI gravitation involves the wavefunction as such; there can be no escape, perhaps available in different circumstances, by assuming that, for measurements by a particular observer, only certain components of the wavefunction require consideration.

We suggest here that such a distinction is illusory. In I we claimed that proponents of so-called MWI in fact make use of such diverse assumptions that two distinct interpretations are involved. (These represent, as it were, polar positions.) The first, to which we restrict the term MWI, advocated in particular by De Witt (1968, 1970) and Graham (1970), declares unequivocally that, following a measurement, the various components in the wavefunction exist in different worlds. In I we insist that this is a non-trivial point which must be stressed in writing down the post-measurement wavefunction. In the case of the present paper, it follows that, for an observer in a given world, the appropriate wavefunction must consist of only one component, and $\langle T_{\mu\nu}\rangle$ undergoes a discontinuity at the measurement exactly as in the von Neumann theory. This result agrees with the general position established in I, that the MWI is a clear theory, which gives explicit answers to all questions and suffers no conceptual vagueness, but has no better answers to the dilemmas concerning measurement than has von Neumann.

The paper by Cooper and van Vechten (1969) represents the other extreme, to which we give, in I, Everett's original name of the relative-states interpretation (RSI). We also suggest there that the work of Everett (1957, 1973) and Zeh (1970) is fairly close to this position. In these papers the question of other worlds is greeted with different degrees of doubt, as is that of the reality of all components of the wavefunction bar one (as is fully discussed in I). If one indeed adopts the point of view that all components except one are fictitious in nature, serving only to ensure the continuity of the wavefunction, one is, in effect, back to the MWI discussion of the previous paragraph for the purposes of this paper.

PG clearly adopt the opposite point of view whereby the reality of all components of the wavefunction is taken absolutely seriously and is used in the calculation of $\langle T_{\mu\nu}\rangle$. For more usual calculations, such as those for the probabilities of obtaining various results in a measurement of position or momentum, they would wish, presumably, to ignore the unwanted components, using the argument that they have 'negligible interference with the ones of interest'. The conclusions from these calculations will be, of course, equivalent to those for the collapse interpretation of von Neumann.

The latter argument is, in our opinion, optimistic (though, of course, following many of PG's predecessors). The formula for the expectation value of, say, a component of angular momentum, is surely directly analogous to $\langle T_{\mu\nu}\rangle$ and should therefore be calculated in an analogous manner, using all components of the wavefunction. The idea that it is only the consistency of observations within a particular world component that is of interest, rather than a concern for all components, appears untenable. It would then follow that all experiments would give results corresponding to the full wavefunction, and the state of all components should be available from analysis of routine experiments of all types, not just those involving gravity.

For a second argument, though, we may restrict ourselves to gravitational experiments. PG assume that, if the hypothesis of semiclassical gravity and non-collapse of wavefunction is correct, the results of their experiment would be those for an average of the two world components which have been separated by the radioactivity decision procedure. They ignore all other world components in which, for example, the development of life on Earth may have been on different lines or, more prosaically, PG may just have decided to do another experiment. Presumably, if their background hypotheses are correct, such components should be included, not just in this experiment, but in all similar experiments measuring the gravitational constant. Even if, as PG suggest *could* be the case, the full wavefunction of the universe might not contain components with the astronomical bodies at greatly different positions from those they take in our component, such effects should surely be present in these small-scale experiments. If present they would certainly reduce the results of the experiments to nonsense, and their absence suggests that PG's hypothesis must be incorrect.

4. Conclusions

We suggest that what is being analysed in this study is not principally the possibility of semiclassical gravity, but chiefly the type of many-world interpretation that is tenable. Our analysis suggests that pre-experimental considerations could have ruled out the type of theory required, that in which all components of the wavefunction are to be considered for the calculation of expectation values, and thus for the various probabilities of the results of measurements. (If such an interpretation is required for the tenability of a particular theory of semiclassical gravity, then that theory too must be ruled out.)

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