COMMENTS BY THE REFEREE

POS FILE NO.: 00.02.02.2

My recommendation is that the paper not be published for the following reasons:

1. Chalmers may be a good philosopher of mind (I don't know—it's not my area), but his pronouncements in philosophy of physics are hardly worthy of extensive discussion and interpretation. This debate seems to be largely provoked by what the author perceives as a 'misinterpretation' of Chalmers' apparently Everettian interpretation of quantum theory. Yet, as the author notes, Chalmers does not seem to display even passing familiarity with the formalism of quantum theory, much less with the contemporary work in philosophy of physics. Why should we care about interpreting his views on quantum theory?

   I certainly do not mean to be drawing uncrossable disciplinary boundaries. I'm only insisting that we need not bother to worry about reinterpreting the views of people who have not given evidence of knowing much about the theory in the first place unless there is a good, specific reason to believe that they have somehow happened upon an important, novel insight.

2. That being said, I have a number of problems with the paper.

   (a) Byrne and Hall's reading of Chalmers' principle OPUS (as given on p. 350) is obviously correct, and the argument outlined in section 2 is obviously correct. There really cannot be any way around this fact. The author presents section 3 as an 'alternative' interpretation of Chalmers, but this 'alternative' interpretation involves an explicit modification of Chalmers' principle, which the author then very misleadingly presents as if it were a quotation from Chalmers (!). The fact (pointed out by the author) that Chalmers elsewhere says things that do not so clearly mean what he says on p. 350 only indicates that he is himself not clear what he means (which returns us to my first point, above).

   (b) On p. 9, the author writes: "When a state ϕ is a state of an observer who has the belief that the measurement outcome was "up" in the orthodox theoroy, the dynamics will tell that she will

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write "up" in her lab-book". Really? How do we know? Has the author applied quantum theory to this case and gotten this result? Is there any reason whatsoever—apart from unbridled optimism—to think that one *would* get this result if only one could apply quantum theory to this case? I find pronouncements such as this one to be worse than optimistic, and frankly I don't see the point of discussing things that require us to make such bold assumptions.

(c) The author presents, albeit very briefly, an argument to the effect that the Everett interpretation has as much 'substantive content' as the orthodox interpretation. I have two remarks. First, having as much substantive content as the orthodox interpretation is not exactly a cause for celebration, given that the orthodox interpretation is as problematic as it is. (So what's the point of this argument, exactly?) Second, even so the author's argument is not very convincing. It relies on the idea that an observer may be ignorant about how a branch will 'split' even after the splitting has occurred. Ignoring all of the (extremely difficult) questions about personal identity, splitting, and so forth raised by such statements, one may ask what justification there is for supposing that an observer cannot be aware of the result of a measurement just as the result occurs. The author's response: "to ensure this we may ask the observer to keep her eyes close[d] during the measurement." So the definition of probability in the Everett interpretation replies on observers complying with our request that they not look at measurement apparatuses 'too soon'?

Reply to the referee

In part 1 of the report, the referee discusses seemingly the main reason of his negative recommendation. He claims that Chalmers is not an important philosopher of science and, therefore, his views on the issue "are hardly worthy of extensive discussion and interpretation". (Note that 21 pages devoted to the Byrne and Hall paper might give an impression that the Philosophy of Science does not agree with the referee.) But, in fact, Chalmers as a philosopher is not the main issue of the discussion. From reading abstracts and conclusions of Byrne and Hall paper and of my critical comment one can clearly see that the main issue is the feasibility of many-worlds-type interpretations of quantum theory. This issue, I believe, is of crucial importance; it is a topic of contemporary work in philosophy of physics. Detailed analysis of Chalmers's writings is only a testbed for making general conclusions about this important question.

In part 2 of the report, the referee mentions three problems which he saw in my paper, but he does not make any evaluation about validity of my criticism of the general conclusions of Byrne and Hall. In what follows I will discuss these problems in detail.

In point 2a, the referee agrees with my analysis in section 2, but doubts that the alternative interpretation (section 3) is what Chalmers really had in mind. The referee claims that my quotations indicate that Chalmers is not clear about what he really means and this is another reason why we need not bother to worry about reinterpreting his views. However, for this discussion, it is not really an issue, what exactly had Chalmers in his mind writing his book. My point here is that the fact that OPUS as spelled out in Chalmers book is incorrect, cannot be a basis for rejection of all many-worlds-type interpretations as suggested in the Byrne and Hall paper.

I am grateful to the referee for pointing out the miss-print (due to a sloppy usage of the "cut and paste" method) resulting in the misleading presentation of OPUS' as quotation of Chalmers.

In point 2b the referee says that "he does not see the point of discussing things that require us to make" an assumption that quantum mechanical laws are valid for macroscopic objects such as an observer. But in this way he dismisses the whole literature on this subject. Although nobody can make a detailed quantum mechanical calculations of evolution of macroscopic bodies, there are many arguments claiming that such calculations can or cannot lead to the phenomena we experience. Various modifications of quantum laws (GRW-type collapse theories, addition of nonlinear terms to the Schroedinger equation etc.) are motivated by the claims that quantum mechanical laws are not consistent with our experience. Thus, the discussion of the consistency of the assumption is relevant for numerous works in the foundations of quantum theory.

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In 2c the referee makes two remarks.

In the first he writes that showing that the Everett interpretation has as much 'substantive content' as the orthodox interpretation "is not exactly cause for celebration, given that the orthodox interpretation is as problematic as it is." However, the sentence of the referee itself explains why it IS a good "cause for celebration". The substantive content of the orthodox interpretation is sufficient for understanding our experience. The difficulty with the orthodox interpretation is that it has severe physical problems: there is no satisfactory scientific account for the collapse. The Everett interpretation is free of these physical difficulties. Thus, showing that it has as much substantive content as the orthodox interpretation solves an important problem in the foundations of quantum theory.

Note that all comments of the referee until this point were not against my claims but against the topic itself, i.e., against publication of the Byrne and Hall paper. The last remark (second remark in 2c) is the only one against my claims. The referee points out a difficulty in my proposal for introducing probability in the Everett interpretation for a hypothetical case in which splitting of the worlds occur simultaneously with the splitting of the awareness of the observer. It is a true difficulty. However, it does not prevent the mathematical definition of probability in the Everett interpretation through the concept of "measure of existence" as it appears in my papers to which I refer in the text of the manuscript. The difficulty is for interpreting it as the "ignorance probability", because in such situation there is no one ignorant of the result of the experiment. The difficulty pointed by the referee can be solved. The observer who obtained a particular result can ask himself "what bet I should put on a particular result based on all my memories accept the result of the last measurement?" This is somewhat awkward construction, but not uncommon in discussions of similar questions (cf. Parfit discussing manipulation of the records in the brain). Anyway, it is needed only for hypothetical situation in which no object splits its wave function before splitting of the wave function of the brain of the observer; I can hardly imagine physical implementation of such situation.

COMMENTS BY THE REFEREES

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1. Comments by Referee A:

The paper is an attempt to justify the use of the philosophy of mind to resolve the quantum-mechanical measurement problem. It does not succeed. The author claims that approaches to the philosophy of mind (Chalmers' in particular) can justify the probability postulates of standard quantum-mechanics in many-worlds approaches. However, just when a concrete proposal is offered, the author says too little, referring to some of Vaidman's work on this. But Vaidman is not clear, in the cited piece, on how this probability proposal is to work (for example he doesn't explain how to connect the ignorance probability with the probabilities of QM – the main issue is why anyone in a low probability world should be using the QM postulates in the first place). Thus the author's (not very clear) appeal to this work is not helpful.

Perhaps, instead of trying to defend sometimes Chalmers, and sometimes many-worlds, the author should choose a particular tack. I suggest the latter, since, by the author's own arguments, the Chalmers approach fails (and it isn't clear how the author's version is supposed to make us confident that the philosophy of mind is a suitable foundation for many-worlds QM).

In any case, as the paper stands, it is too disjointed and lacks argumentation that is crucial to its purpose.

Date: August 14, 2000.