PROBABILITY AND THE MANY WORLDS INTERPRETATION OF QUANTUM THEORY

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Two works related to the concept of probability in the framework of the many-worlds interpretation are presented. The first deals with recent controversy in classical probability theory. Elga and D. Lewis argues that Sleeping Beauty should have different credences for result of a fair coin toss in a particular situation. It is argued that when the coin is replaced by a quantum coin, the credence is unambiguous and since one should not expect a difference between classical and quantum coins, this provides a particular resolution of the controversy. Second work is an analysis of a recent criticism by Byrne and Hall of Everett-type approach presented by Chalmers. It is shown that the criticism has no universal validity and that the Byrne and Hall rejection of any Everett-type interpretation of quantum mechanics is unfounded.

1 Introduction

The talk I presented in this conference analyzed the meaning of recent experiments claiming to close last loopholes in testing Bell's inequality. I argued that the really important task in this field has not been tackled yet and that the leading experiments claiming to close locality and detection efficiency loopholes, although making very significant progress, have conceptual drawbacks. The important task is constructing quantum devices which will allow winning games of certain correlated replies against any classical team, and I proposed a novel game of this type named an "impossible necklace game". However, this work has been recently published (Vaidman, 2001a), so I present here, instead, two recent unpublished works which I discussed extensively in the conference (mostly walking around the beautiful Växjö lake).

The two works presented here are closely related to the main theme of the conference: the meaning of probability in quantum mechanics. The first one was done together with Simon Saunders (see the discussion of the classical case in Vaidman and Saunders 2001) and it is presented in Section 2. It analyzes recent controversy in classical probability theory by transforming it to the framework of quantum theory. It is surprising that quantum theory, with its serious difficulties in dealing with the concept of probability, can help in the analysis of a classical probability problem. The second work (presented in Section 3) defends the many-worlds approach to quantum mechanics against recent criticism by Byrne and Hall of the Chalmers book that presented the many-worlds interpretation of quantum mechanics as the most promising approach.

Before going to these sections I want to express my understanding of the main discussion in this conference. A very significant part of the conference was allocated to the analysis of Bayesian probability in the framework of quantum theory (see, e.g., Caves, Fuchs, and Schack, 2001). In my rough understanding, Bayesian approach denies existing underlying ontology and defines probability from several axioms related to properties of the concept of probability. In the conference we witnessed a very impressive progress in the Bayesian probability program. However, many participants were not ready to accept this approach due to difficulties of physics without ontology. I am fully accept the criticism of frequency approach to probability, but I am not ready to give up the ontology.

My ontology is the many-worlds ontolgy (Everett 1957). It turns out that in spite of having the ontolgy, I do need some kind of Bayesian approach to probability. The many-worlds interpretation has a serious difficulty with dealing with probability and my solution of this difficulty (Vaidman 1998, 2001b) uses an approach that adopts most if not all principles of Bayesian approach. So if before the conference I thought that Bayesian approach is orthogonal to my views on probability, now I consider it as a complementary one. The many-worlds approach resolves the difficulties of satisfactory description of the collapse of a quantum wave and the Bayesian approach solves the difficulty of the concept of probability in a deterministic theory such as the many worlds interpretation in which all possible outcomes of experiments are actual.

2 Sleeping Beauty in Quantumland

2.1 Lewis - Elga controversy

Lewis's comment (2001) on Elga's paper (2000) has far reaching consequences. If Lewis is right, then the approach to credence for an event as the value of an "intelligent bet" on this event (e.g. Sklar 1993) does not have universal applicability. The betting approach to this question (Aumann et al. 1997) leads to Elga's result which Lewis contests. I believe, however, that Lewis's approach is untenable, and thus the universality of the betting approach to probability has not been breached.

I give two arguments in favour of Elga's conclusion, the first classical, the second quantum mechanical. It is of interest that a quantum mechanical analysis can be brought to bear on this classical probability problem.

The first is as follows: consider the following three experiments, the first of which is Elga's. I claim the credence of Beauty is the same in each case.

> (i) Beauty sleeps during the week. On Sunday a fair coin is tossed. If the result is Heads (H), Beauty is awaken on Monday only, if the result is Tails (T), Beauty is awaken on Monday and Tuesday. On every awakening, Beauty is told nothing, but must answer the question: "What is your credence for the coin to be H?" After the conversation her memory of the awakening is erased and she sleeps again.
>
> (ii) Beauty sleeps for 100 years, uninterrupted save for awakenings according to procedure (i), which take place every week, following a coin toss on each Sunday of each week.
>
> (iii) Beauty sleeps for 100 years, uninterrupted save for 7827 awakenings. Using a classical random number generator to determine the order, on 2609 awakenings a coin is placed H, and on 5218 awakenings a coin is placed T. On each awakening, the same procedure is followed as in (i).

Experiment (ii) is not just a repetition of experiment (i) 5218 times. In the latter, on every awakening, Beauty knows which week it is. In (ii) Beauty does not know which week it is and, therefore, she does not know which coin in the sequence the question is about. However, since all the coins are fair, Beauty's situation is the same in all weeks and thus the lack of information which week it is cannot make a difference in her credence.

Experiments (ii) and (iii) are also not identical. The probability in experiment (ii) that one will obtain exactly 2609 H will is very small. However, the probability that this number will be different from 2609 by more than, say, 100, is also very small. Therefore, the probability that the relative frequency of H will be significantly different from 1:2 is negligible. Thus, although I do not have an argument according to which Beauty has to give exactly the same answer in (ii) and (iii), I can argue that these answers cannot differ significantly. Since our job is to decide between credences 1/2 (Lewis) and 1/3 (Elga), this is enough.

In experiment (iii) it is obvious that Beauty should give credence 1/3 for H. If there is no significant difference in Beauty's answer in case (iii) and (ii), and no significant difference in her answer in case (ii) and (i), then in Elga's experiment Beauty should give the answer 1/3, and not 1/2 as Lewis claims.

There is a conceptual difference between (i) and (ii) (and a similar difference between (i) and (iii)). In (i) the Beauty was asked a question about an uncentered proposition: "What is the state of the coin?" In contrast, in (ii) Beauty was asked a question about a centered proposition: "What is the state of the coin of *this* week?" The unusual feature of Elga's experiment, that Beauty must alter her credence about an uncentered proposition with no new uncentered evidence, is not present here. But this does not alter the fact that Beauty must give the same answer in case (ii) and (i).

Apart from the small statistical difference between (ii) and (iii) already mentioned, the two differ in another aspect. In (ii) there is something which corresponds to Lewis's number 1/2: in half of the weeks of Beauty's sleep the coin is H, and in half it is T. In contrast, in experiment (iii), nothing corresponds to 1/2. However, knowledge of this statistical structure to her string of awakenings, in case (ii), is not knowledge that Beauty can use, since never on awakening does she learn where in the string she is located.

2.2 The inconsistency of the Lewis approach

In order to see how the difference between the one-week experiment and the many-weeks experiment arises, and in order to show the inconsistency of Lewis's approach, consider an experiment of the kind (ii) but limited to two weeks. A fair coin is tossed twice. The credence of Beauty $p(H)$ can be calculated as the sum of conditional probabilities on the outcomes of the coin tosses:

$$p(H) = p(H|HH)p(HH) + p(H|TT)p(TT) + p(H|HT)p(HT) + p(H|TH)p(TH) \quad (1)$$

It is uncontroversial that $p(H|HH) = 1$ and $p(H|TT) = 0$. Given that one of the outcomes is H and another T, Beauty knows that there are three awakenings: one H and two T. Therefore, the conditional credences of Beauty for these cases are $p(H|HT) = p(H|TH) = \frac{1}{3}$. According to Lewis, Beauty has equal credence for all possible outcomes of the coin tosses: $p(HH) = p(TT) = p(HT) = p(TH) = \frac{1}{4}$. It then follows from (1) that Beauty's credence for H on awakening during the two weeks is $p(H) = \frac{5}{12}$. This is in contradiction with the assumption that there should be no change between Elga's one-week experiment and the similar two-week experiment. Therefore, unless Lewis rejects this very natural assumption, his approach is inconsistent.

On the analysis that I favour there is no such difficulty. On awakening, Beauty's credences for the four outcomes of the coins tosses should not be identical, they should be weighted according to the number of awakening corresponding to these outcomes. Thus $p(HH) = \frac{1}{6}$, $p(TT) = \frac{1}{3}$, and $p(HT) = p(TH) = \frac{1}{4}$. Using (1), it follows that $p(H) = \frac{1}{3}$, just as in the one-week experiment in accordance with Elga's argument.

2.3 The quantum coin experiment

An entirely independent argument to the same conclusion follows from the interpretation of probability in the Many-Worlds Interpretation (MWI) of quantum mechanics (an elaboration of the Everett approach (1957)). Consider the toss of a quantum coin, say, for a simple example, the observation of a photon, incident on semi-transparent mirror, as either reflected (R) or transmitted (T). According to the MWI, the world splits in two: one (the R-world) in which the photon is observed as reflected, and the other (the T world) in which the photon is observed as transmitted.

Elga's experiment, but with such a quantum coin, is very similar to the "sleeping pill" experiment (Vaidman 1998), which was introduced to give a possibility of an ignorance interpretation of probability in the framework of the MWI. According to this approach, the observer assigns the probability for outcomes of a quantum measurement in proportion to the "measures of existence" (Vaidman, 1998) of the corresponding worlds, the modulus squares of the amplitudes of the corresponding branches of the universal wave function. (See Saunders (1998) for a discussion of a similar concept of "measure" .) There is no direct meaning for this probability for the person who is going to perform the quantum measurement (and who is put to sleep for its duration): there is no information, centered or uncentered, that he is then ignorant of. The meaning for probability is given through the (identical) credences of the two successors of the experimenter, on awakening, in centered propositions, namely the propositions "I am in the R-world" and "I am in the T-world". For each of these successors, there is a fact of the matter as to the outcome of the experiment and he is ignorant of this fact.

It is worth remarking that the problem of the MWI recently posed by Peter Lewis does not arise in this approach. He argued (Lewis 2000) that a believer in the MWI should agree to play "quantum Russian roulette", provided that death is instantaneous . The large "measures" of the worlds with dead successors is a good reason not to play.

There is a difference between Elga's experiment and the "sleeping pill" quantum coin flipping. On awakening, Beauty is not only ignorant of which world she is in, she is also ignorant of which time she is at in T-world. There are three mutually exclusive propositions: "I am in an H-world on a Monday", "I am in a T-world on a Monday" and "I am in a T-world on a Tuesday", and she must assign credences in them summing to unity. Failing any other information which could discriminate between the three cases, her credences should be in proportion to the "measures" of the corresponding worlds, which happened to be exactly the same. Therefore, her credence in each should be the same, namely one third.

There is an important difference between Elga's experiment with a quantum and a classical coin . Classically, Beauty's credence concerns a proposition, "the coin is H", and this along with the entire sequence of events are located in a unique world. It is for this reason that the proposition is reckoned to be uncentered. But using a quantum coin, on the MWI, that is no longer so. Quantum mechanical outcomes are in different worlds, and propositions about such outcomes, such as "the photon is T" should be read as tacitly indexical, "in *this* world the photon is T"; they are properly speaking centered propositions. To revert to our previous discussion, the difference between case (i) and case (ii), using a quantum coin, does not concern the centeredness or otherwise of the proposition Beauty's credence is about. In both cases it is centered.

On switching to a quantum coin in Elga's experiment, and on interpreting quantum mechanics in terms of the MWI, one loses the unusual feature that Lewis found so objectionable: that Beauty must change her credence in an uncentered proposition, although her uncentered evidence has not changed. Lewis may even *agree*, in this case, that Beauty's credence should change, for it is credence in a centered proposition - and if so he will presumably agree that her credence in H is one third. But the quantum coin is considered to be the best possible implementation of a fair coin: Beauty should not have different credences in classical and quantum experiments.

3 Byrne and Hall on Everett and Chalmers

3.1 Introduction

Byrne and Hall (1999) criticized the argument of Chalmers (1996) in favor of the Everett-style interpretation. They claimed to show "the deep and underappreciated flaw in *any* Everett-style interpretation". I will argue that it is possible to interpret Chalmers's writing in such a way that most of the criticism by Byrne and Hall does not apply. (Recently I have learned that Chalmers himself (2000) partly accept the criticism, so my interpretation of his writing might differ from his original proposal.) In any case the general criticism of Byrne and Hall of the many-worlds interpretation is unfounded. The recent recognition that the Everett-style interpretations are good (if not the best) interpretations of quantum mechanics has, therefore, not been negated.

It is probably impossible to present an interpretation of quantum mechanics in unambiguous way without writing equations. Chalmers's presentation of Everett-style interpretation also can be understood in different ways. Instead of equations Chalmers used some technical jargon of quantum theory, however, some words like "substates" have no clear meaning even for physicists. Byrne and Hall (BH) interpreted Chalmers's jargon in a way which leads to contradictions. In this note I will argue that by taking a more positive approach, one can see in Chalmers's writing a consistent (although not necessarily very persuasive) argument.

In the second part of their paper BH claimed to show not only that Chalmers has failed to establish his Everett-inspired interpretation, but that "anything resembling it should not be taken seriously". Their first point is of a general character: if the spaces of states in two theories are identical but the dynamics is not, it is not obvious that the interpretation of these states in the two theories must be identical too. BH point out that this is the situation regarding the interpretation of quantum states in the orthodox and the Everett interpretations. I will argue that although their general argument is correct, its application is not. There is enough similarity between the dynamics that makes the identification plausible. The second point of BH is that the Everett-style interpretation has less "substantive content" than the orthodox interpretation. This is because in the Everett (many-worlds) interpretation there is no counterpart of "outcome probabilities", the concept of the orthodox interpretation associated with a system in a superposition of eigenstates of some variable. I will argue that the definition of the probability of an outcome in the framework of the many-worlds interpretation which I recently proposed solves this difficulty and makes this BH criticism obsolete.

In Section 3.2 I will adopt the BH interpretation of Chalmers and will show (in a different from BH way) how it leads to a contradiction. In Section 3.3 I propose an alternative interpretation of Chalmers's writing which leads to a consistent argument. In Section 3.4 I critically analyze the general arguments of BH against the Everett-style interpretations. Finally, in Section 3.5 I summarize my defense of the many-worlds interpretation.

3.2 Byrne and Hall interpretation and a contradiction in the Chalmers argument

The central thesis of Chalmers quoted by BH is the principle of *organizational preservation under superposition*:

> OPUS
> If a computation is implemented by a system in a maximal physical state P, it is also implemented by a system in a superposition of P with orthogonal physical states.(Chalmers, 350)

Consider a simple model: a computer which performs calculations in a classical way. If at time t_0 the computer receives a classical input (a particular punching of its keyboard), then it evolves in time is such a way that it is always in a "classical" state. This means that all the registers of the computer at all times are in some definite states (exited or not exited) i.e., not in a superposition of excited and not excited. Suppose that P corresponds to a computation of a square of a number 5, while Q corresponds to a computation of a square of a number 10. Denote $|P(t)\rangle$ a quantum state of the computer at time t performing the calculation of the square of 5, while $|Q(t)\rangle$ a quantum state of the computer at time t performing the calculation of the square of 10. In the two computations at any time the registers must be in different states, therefore, $|P(t)\rangle$ is orthogonal to $|Q(t)\rangle$. Thus, according to OPUS the computer in a quantum state

$$|R_+(t)\rangle \equiv 1/\sqrt{2}(|P(t)\rangle + |Q(t)\rangle), \tag{2}$$

also implements computation of the square of 5. The quantum state

$$|R_-(t)\rangle \equiv 1/\sqrt{2}(|P(t)\rangle - |Q(t)\rangle), \tag{3}$$

is orthogonal to $|R_+(t)\rangle$. BH read Chalmers in such a way that OPUS can be applied to $|R_+(t)\rangle$ and $|R_-(t)\rangle$, i.e., that the superposition $1/\sqrt{2}(|R_+(t)\rangle - |R_-(t)\rangle)$ also implements computation of the square of 5. But,

$$\frac{1}{\sqrt{2}}(|R_+(t)\rangle - |R_-(t)\rangle) = \frac{1}{2}[(|P(t)\rangle + |Q(t)\rangle) - (|P(t)\rangle - |Q(t)\rangle)] = |Q(t)\rangle. \tag{4}$$

The state $|Q(t)\rangle$ corresponds to the computation of the square of 10. It corresponds to the punching of a different input, it has different registers activated during the calculation, it has different output. Clearly, it does not implement computation of the square of 5.

Applying this direct reading of Chalmers, BH reached somewhat different contradiction which lead them to reject Chalmers's approach.

3.3 An alternative interpretation of Chalmers

It is possible to read Chalmers in another way such that the contradictions of the type described in the previous section do not arise. Let us make the following modification of the OPUS principle:

> OPUS′
> If a computation is implemented by a system in a maximal physical state P *which is not a superposition*, it is also implemented by a system in a superposition of P with orthogonal physical states.

This modified principle can be applied to P and Q, but it cannot be applied to R_+ and R_- and, therefore, one cannot reach the contradiction described above as well as the contradictions described by BH.

One might see that OPUS′ is what Chalmers actually had in mind even though he did not say it explicitly. Indeed, another way to see the difference between OPUS (as read by BH) and OPUS′ is that in the latter it is required that P corresponds to a *single* experience. Chalmers's first *definition* of the OPUS principle is:

> If the theory predicts that a system in a maximal physical state P gives rise to an associated maximal phenomenal state E, then the theory predicts that a system in a superposition of P with some orthogonal physical states will also give rise to E. (349)

The word "associated" hints that Chalmers meant that there is only one experience ("phenomenal state E" in Chalmers's notation) corresponding to physical state P.

In fact, BH saw a possibility of reading OPUS as OPUS′. The "(Version of) OPUS" described in their section 5.2.3 is essentially OPUS′. They rejected this because they understood that Chalmers denies the existence of *preferred basis*. BH are correct in their criticism that without preferred basis there is no way to distinguish between quantum state which is a "superposition" and a state which is not a "superposition". Thus, the modification of OPUS to OPUS′ cannot be done without assuming preferred basis.

We can read Chalmers in such a way that we do not run into inconsistency: Chalmers only objects to the claim that the *mathematical* formalism of quantum mechanics, i.e. the Schrödinger equation, leads to preferred basis. He cannot object to the existence of preferred basis, but he views it as arising from his theory of consciousness. This reading of Chalmers is justified by the following quotations:

> Everett assumes that a superposed brain state will have a number of distinct subjects of experience associated with it, but he does nothing to justify this assumption. It is clear that this matter depends crucially on a theory of consciousness. A similar suggestion is made by Penrose (1989): "... a theory of consciousness would be needed before the many-worlds view can be squared with what one actually observes" (348)

> ... last three strategies are all *indirect* strategies, attempting to explain the discreteness of experience by explaining an underlying

discreteness of macroscopic reality. An alternative strategy is to answer the question about experience *directly.* (349)

Before discussing quantum mechanics, Chalmers argues for a *principle of organizational invariance*:

> POI
> Given any system that has conscious experiences, then any system that has the same fine-grained functional organization will have qualitatively identical experiences. (249)

The main difficulty which BH see in putting together the *principle of organization invariance* together with OPUS follows from the same misinterpretation of Chalmers. If there is no preferred basis then they have reasons to say:

> ... perceptual experience is (more or less) *entirely illusory.* When you seem to see a voltmeter needle pointing to '10' your perceptual experience is probably veridical: the needle (if, indeed, we can sensibly speak of such a thing) is not pointing to '10' or anywhere else.

However, accepting preferred basis, even if it is defined by the concept of experience itself, resolves the difficulty: the pointer does point to '10' and in addition, in parallel worlds, to other values too.

Chalmers claims that his *independently motivated* theory of consciousness *predicts* that even in the world which is in a giant superposition there are subjects who experience a discrete world. He bases his argument on "the claim that consciousness arises from implementation of an appropriate computation." Taking the model of a simple computer presented above, we can follow (at least approximately) his proof on p. 350. Projection of the superposed state on "the hyperplane of P" might mean projection of the quantum state of the computer in a "superposed" state at the initial time on the state corresponding to the input of calculating square of the number 5 which leads to quantum states of the various registers at later times corresponding to this calculation. The parallel between the calculation and experience yields the desired result, but accepting this parallel is relying on our experience. So, if we read Chalmers as BH do, that he claims to *deduce* "what the world is like if the Schrödinger equation is all" without the guide of our experience, then they have a valid criticism. However, Chalmers admits that Schrödinger equation cannot be all:

> ... the only physical principle needed in quantum mechanics is the Schrödinger equation, and the measurement postulate and

> other basic principles are unnecessary baggage. To be sure, we need psychophysical principles as well, but we need those principles in any case, and it turns out that the principles that are plausible on independent grounds can do the requisite work here. (350-351)

I feel that these "independent grounds" are connected with our experience in a stronger way than one might imagine reading Chalmers. But this fact cannot lead to rejection of this approach as BH claim.

3.4 Byrne and Hall against any Everett-style interpretation

BH start their argument by pointing out that the orthodox quantum theory and the Everett interpretation formally defined on the same "family of state spaces" and that the difference is only in dynamics. Then they say that because of the difference in dynamics it does not follow that the quantum state corresponding to a particular experience in the orthodox theory will correspond to the same belief (if at any) in the framework of the Everett theory.

This might be considered as a criticism of Chalmers if one reads him as saying that Everett theory *predicts* what our experiences should be, but usually this connection is *postulated* in Everett-style theories. There is a strong motivation for this postulate. The orthodox theory is defined only on a (tiny) part of the space of all quantum states: macroscopic quantum systems cannot be in a "superposition states". The dynamics of the allowed states between quantum measurements is *identical* to the dynamics of the quantum states in the Everett theory. Let us discuss the example analyzed by BH at the end of p.385. When a state ϕ is a state of an observer who has the belief that the measurement outcome was "up" in the orthodox theory, the dynamics will tell that she will write "up" in her lab-book. The dynamics of the state ϕ in the Everett theory leads to the same action. This justifies considering ϕ to be a "belief vector" in the Everett theory too.

BH proceed with their criticism claiming that Everett's interpretation has less of "substantiative content" because when a quantum system is in a superposition of eigenstates with different eigenvalues of some quantity $\mathbf{M}$, the orthodox interpretation associates probabilities to the various outcomes, while the Everett theory does not.

It is true that there is a difficulty with the concept of probability in the framework of the Everett-style interpretation. The Everett theory is a deterministic theory and it does not have a genuine randomness of the collapse of the orthodox interpretation. A deterministic theory might have the concept of *ignorance* probability, but it is not easy to find somebody who is ignorant of the result of a quantum experiment: it is senseless to ask what is the probability that an observer will obtain a particular result, because she will obtain *all* results for which there are a non-zero probabilities according to the orthodox approach. It seems also senseless to ask what is the probability of the observers in various branches (these are persons with the same name and the same memories about events which took place before the measurements, but who live in different branches corresponding to the different outcomes) to obtain various results, since obviously the probability to obtain the result "$\mathbf{M} = m_i$" in branch "j" is 1 if $i = j$ and it is 0 if $i \neq j$. These are not the quantum probabilities we are looking for.

However, BH cannot dismiss the Everett-style interpretations without even discussing current proposals to deal with this problem (Lockwood *et al.* 1996, Saunders 1998, Deutsch 1999, etc.) Here, I will sketch my proposal for solving this difficulty (Vaidman 1998, 2000). The splitting into various branches occurs usually before the time when the observers in these branches become aware of the outcome of the measurement. (To ensure this we may ask the observer to keep her eyes close during the measurement.) Thus, an observer in each branch is ignorant about the outcome of the measurement and she can (while any external person cannot!) define the the *ignorance* probability for the outcome of the measurement. She will do so using standard probability postulate: the probability of an outcome is proportional to the square of the amplitude of the corresponding branch. Moreover, since observers in *all* these branches have identical concept of ignorance probability and since they all are descendants of the observer who performed the experiment, we can associate probability for an outcome of a measurement for this observer in the sense that this is the common ignorance probability of her descendants in various branches.

The fact that I have used a probability postulate here does not spoil the argument: I had to show that substantive content of Everett interpretation is not less than that of the orthodox interpretation. The latter has the probability postulate as well. What was done here (and what was not trivial from the beginning) was to present a way which allows to *define* probability in the frame of the many-worlds interpretation. This definition also resolves the difficulty recently discussed by Lewis (2000). He argued that a believer in the many-worlds (minds) interpretation should agree to play a "quantum Russian Roulette" provided the death is instantaneous. Indeed, the instantaneity makes it difficult to establish the probability postulate, but after it has been justified in the wide range of other situations it is natural to apply the postulate for all cases.

The last argument of BH relies on their claim that Everett-style interpretation lacks "statistical algorithm". Since the ignorance probability defined above generates the same statistical algorithm as the the orthodox theory, this argument does not hold either.

3.5 Summary

The main claim of BH is "that *any* Everett-style interpretation should be rejected". The basis of their argument is the observation that neither Chalmers nor anybody else can answer the question: "What the world is like if the Shrödinger equation is all?" It is true that this question is much more difficult to answer in the framework of the Everett-style interpretation relative to interpretations which do not have multitude of worlds. "The world is everything which exist" is not a valid definition. Moreover, the Shrödinger equation itself cannot define the concept of a "world". The world is the concept defined by conscious beings and it requires the analysis of the mind-body connection. Chalmers's theory of consciousness provides an answer. One might argue how substantial his answer is, but even if there is no a detailed answer to this question today, one cannot reject the Everett interpretation. It suffices that Everett's theory is consistent with what we see as our world. It is so superior to the alternatives from the physics point of view, because it avoids randomness and action at a distance in Nature (e.g., see Vaidman 2000), that it is still preferable in spite of the fact that it is less satisfactory from the philosophical point of view. Therefore, even if BH were able to point out a difficulty in obtaining the interpretation out of the "bare theory" this would not be enough for rejecting the Everett interpretation. Moreover, I have argued that the BH have not presented persuasive arguments showing the difficulty. Their first argument is that it is not obvious that the correspondence between quantum states and classical properties in the orthodox quantum mechanics can be transformed as it is to the Everett interpretation. This argument does not take into account the similarity in dynamics which justifies the identification. Their other arguments rely on the well known difficulty in the interpretation of probability in the many-worlds interpretation disregard recently proposed solutions of this problem.

In summary, BH were not able to show a flaw in Everett-style interpretations. The temptation to appeal to the philosophy of mind in interpreting quantum mechanics, in particular, the idea that a theory of mind might help rescue from the difficulties with standard interpretation is still very attractive. Indeed, the Everett-style interpretation which says that physics is described in full by the Schrödinger equation is the most satisfactory from the physics point of view. What is left is to complete Chalmers's work, i.e. to elaborate the connection between the quantum state evolving according to the Schrödinger equation and our experience.

Acknowledgments

It is a pleasure to thank David Chalmers, Adam Elga, Christopher Fuchs, Andrei Khrennikov, Simon Saunders and Ruediger Schackfor for illuminating comments. This research was supported in part by grant 62/01 of the Israel Science Foundation and the EPSRC grant GR/N33058.

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