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Deutsch on the epistemic problem in Everettian Quantum Theory

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ABSTRACT

David Deutsch (forthcoming) offers a solution to the Epistemic Problem for Everettian Quantum Theory. In this note I raise some problems for the attempted solution.

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David Deutsch (in press) offers a solution to the Epistemic Problem for Everettian Quantum Theory. In this note I raise some problems for the attempted solution.

Everettian Quantum Theory is deterministic – it says that when a quantum measurement is made, the laboratory, scientists and entire world split into branches, and for each possible result, there is a branch where that result occurs. The big problem for Everettian Quantum Theory is how to make sense of what other versions of quantum theory identify as objective probabilities. These equations match our observations; if quantum mechanics says a result has a probability of 1/3, then repeated experiments show the result, on average, one time in three.

> There are two roles for probability that we have to make sense of:
>
> **The Practical Problem:** How are we rationally to act, if we interpret quantum mechanics along Everettian lines? Suppose we are faced with a choice between, say, disaster on the spin-up branch and disaster on the spin-down branch. Given only that, whichever choice we make, there will be a disaster branch and a non-disaster branch, how could we ever have grounds for choosing?
>
> **The Epistemic Problem:** How can we justify believing the theory on the basis of our empirical evidence, if we interpret quantum mechanics along Everettian lines? Given only that the theory predicted that the evidence that we have in fact observed would occur on some branch (and that the same is true of every other 'possible' string of evidence), how can we reasonably take our evidence to confirm the theory? (Greaves, 2007, p. 122).

Our focus will be the epistemic problem, but let me first note that Deutsch's proposal for the epistemic problem relies in part on the "decision-theoretic" approach to the practical problem (Deutsch, 1999), so if the decision-theoretic approach is invalid, Deutsch's proposal for the epistemic problem presumably can't get off the ground. But let's set this aside – I think that even granting Deutsch's solution to the practical problem, his attempt to solve the epistemic problem fails.

So let's move on to the epistemic problem. The standard Bayesian theory of confirmation says that:

F confirms H iff $P(F|H) > P(F)$.[1]

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[1] I assume that $0 < P(H) < 1$, which ensures the inequality in the text is equivalent to the more standard $P(H|F) > P(H)$. See Salmon (1975) and Fitelson and Hájek (in press). I've also changed the more standard 'E' to 'F' to avoid conflicting with Deutsch's 'E' for 'Everett/everything'.

What is our evidence according to Everettian Quantum Theory? As all results happen, it seems that all results have a probability of 1 i.e. $P(F|H)=1$. On minimal assumptions,[2] it follows that $P(F|H) > P(F)$, therefore the evidence is guaranteed to confirm H, whatever evidence is found. And this confirmation seems to be too easy. So the problem is not so much 'how can we reasonably take our evidence to confirm the theory?', but must we *always* take our evidence to confirm the theory? How can we avoid easy confirmation? This is the epistemic problem.

Deutsch (in press) suggests an answer to this problem. Before getting to the details of his account, we should make explicit one of the interesting features of his approach. He rejects probabilistic theories of confirmation in favour of a Popperian theory. Roughly, on Deutsch's theory, scientific theories are explanations, and a theory should be rejected when it fails to explain an explicanda and a competing explanation succeeds in explaining the explicanda.

Popperian theories deny the existence of inductive probabilities, which is both their strength and weakness. It is a strength because attempts to construct inductive probabilities are deemed to have failed. It is a weakness because without inductive probabilities we can say so little about scientific theories – we cannot say that a theory is confirmed, or should be believed to any degree. The Popperian can be thought of as being especially epistemically cautious – even if a theory has survived attempts to refute it, we should still not believe it. Perhaps this is a price worth paying in order to avoid error. But there is something strange about Deutsch's appeal to Popper's approach here.

Deutsch suggests that Popperian methodology can solve a problem – the epistemic problem in Everettian Quantum Theory – that Bayesian theories cannot. And I don't see how any problem could be solved by Popper and not also solved by the Bayesian. For Bayesian theories are naturally thought of as logical *strengthenings* of Popper's methodology. Popper's central claim – that theories are rejected when falsifying evidence is found – can be incorporated into Bayesian methodology – as the claim that H is rejected when E is found such that $P(E|H)=0$.[3] Deutsch's additions concerning understanding scientific theories as explanations can also be incorporated into Bayesian methodology. Bayesian theories add inductive or subjective probabilities, allowing them to make further claims, such as that a hypothesis is confirmed by the evidence in non-extreme cases. And this addition can only add to the power of the theory. So it seems that anything that can be explained by Popperian methodology can also be explained by Bayesian methodology. So if the Popperian can explain how to update in an Everettian world, then the Bayesian should be able to as well, by applying the Popperian bit of their theory.

But let's set this worry aside and consider Deutsch's theory. He argues that there can be evidence that is not explained by Everettian Quantum Theory that is explained by a competitor.

He first lays the groundwork by making the following claim about explanation:

Criterion (i). an explanation is bad…to the extent that… (i) it seems not to account for its explicanda…[4]

So if we are trying to explain something, say, a1 (the explicanda), then an explanation is bad to the extent that it seems not to account for a1. One might wonder what the difference is between 'explain' and 'account for'. Why not just say that an explanation is bad to the extent that it fails to explain its explicanda? Deutsch doesn't tell us, and I will argue later that (i) merely leads us round in a circle. But let's press on.

Deutsch then describes the following example:

> Suppose…that two mutually inconsistent theories, D and E, are good explanations of a certain class of explicanda, including all known results of relevant experiments, with the only problematic thing about either of them being the other's existence. Suppose also that in regard to a particular proposed experiment, E makes only the everything-possible-happens prediction…for results a1, a2,…, while D predicts a particular result a1…
>
> Observing the result a1…would be consistent with the predictions of both D and E. Even so, it would be a new explicandum which, by Criterion (i) above, would raise a problem for the explanation E, since why the result a1 was observed but the others were not would be explained by D but unexplained by E.

But why doesn't E explain the result a1? Indeed, E says that *all* possible results will be observed, so it says that a1 will be observed. So E does seem to explain a1 being observed. This is the heart of the problem. Deutsch needs to tell us how E fails to explain the result a1. I am not saying this cannot be done, just that Deutsch has not told us how. Why might E fail to explain a1? Perhaps we need to take into account that result a1 is observed only by agents on the a1 branch. Agents on other branches do not see a1, they observe a different result. Perhaps these other observations are not explained by E.

But Deutsch makes no mention of these post-measurement branches. Instead, he uses his proposed scientific methodology to tell us why E fails to explain a1. But I find his scientific methodology unilluminating. In fact he seems to lead us round a string of definitions.

Deutsch tells us that, given E, a1 is expected *not* to happen, even though it *will* happen:

> under E[5]…a1 is expected not to happen, in the sense defined in Section 2 [see below], even though E asserts that, like every other [result], it will happen… This is no contradiction. Being expected is a methodological attribute of a possible result (depending, for instance, on whether a good explanation for it exists) while happening is a factual one. What is at issue in this paper is not whether the properties 'expected not to happen' and 'will happen' are consistent but whether they can both follow from the same deterministic explanatory theory, in this case E, under a reasonable scientific methodology. And I have just shown that they can.

So Deutsch claims he has shown that, given E, a result both will happen and is expected not to happen. It is not clear to me that he has shown this. Indeed, I don't understand how it is possible.

Quick clarification: There is usually nothing inconsistent with a result being expected not to happen and also happening. Every time you are surprised, something happened that you expected not to. Deustch is defending the consistency of a scenario much stranger than this. For according to E, a1 is *guaranteed* to happen. So Deutsch is defending the consistency of a theory which says that both a1 is guaranteed to happen and that a1 is not expected to happen.

Deutsch does give us some help by defining what he means by 'expected' earlier in the paper, in the advertised Section 2:

> I now define an objective notion…of what it means for a proposed experiment to be expected to have a result x under an explanatory theory T. It means that if the experiment were performed and did not result in x, T would become (more) problematic. Expectation is thus defined in terms of problems, and problems in terms of explanation, of which we shall need only the properties (i)…

[2] Namely, assuming that $0 < P(H) < 1$ and $0 < P(F) < 1$, and that E confirms H iff $P(H|F) > P(H)$.

[3] Compare Howson and Urbach (1993, pp. 119).

[4] Other criteria are added but we won't need them.

[5] Deutsch adds a string of a1 results here, but they don't seem to play an essential role in his argument.