An Assumption in the Interpretation of Quantum Mechanics

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> 'In the ontological models framework [Harrigan & Spekkens 2007], it is assumed that the probability measure representing a quantum state is independent of the choice of future measurement setting.' (Leifer 2014, 140)

In this recently-unearthed piece I discuss a version of the above assumption, concluding that it is 'very difficult to justify on metaphysical grounds'. I note that abandoning it has an interesting potential payoff, given its crucial role in the no-go theorems of Bell and of Kochen & Specker. There has been increased interest in this option in recent years, under the label of retrocausal models of QM (see, e.g., Price & Wharton 2015, Wharton & Argaman 2020). The present piece may be of interest to diligent historians of this approach. It was written in 1978, while I was a graduate student in Cambridge.

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1: It seems to be almost universally assumed that an interpretation of quantum mechanics under which the various dynamical variables of a system are held to have precise values at all times (a 'PV account of quantum mechanics', for short), or a hidden variables reformulation, must have a property which is expressed in something like the following way:

(1.1) For any individual system s, if we knew the precise values of the dynamical variables (or 'hidden' parameters) p_1^s, p_2^s, ... of s, then we would be able to predict accurately the result of any possible next measurement on s (provided, of course, that s is not subject to external influences in the period before this next measurement - and that if the time evolution of the p_i^s is indeterministic, we know their values at the time of any possible next measurement whose result we want to predict).

We are going to consider the question as to why we should require (1.1), or something similar. But first of all, it will be helpful if we tidy it up a bit - in particular if we eliminate the reference to knowledge and the associated use of the subjunctive, both of which are inessential. The way to do so is to use a notion of theoretical deducibility (written '$\models$'). It will also be helpful if at this stage we agree to ignore the case in which the time evolution of the p_i^s is indeterministic - this is simply a matter of convenience, and with 'possible next measurement' replaced be 'possible present measurement' most of what we are going to say applies directly to this case as well.

With these improvements on (1.1) we obtain, roughly at least:

(1.2) For any individual system s, and any possible next measurement m on s, there is a unique real v such that $P^s \models V(m,s,v)$, where P^s is the conjunction (perhaps infinite) of the statements of the values of each of p_1^s, p_2^s, ... (as in (1.1), and V(m,s,v) is the statement 'the result of the measurement m on s is v'.

Why should we accept as a constraint (1.2), or something like it? And (a closely related question, as we shall see) what does 'possible' mean in a context such as (1.2)?

One reason we might be tempted to give for requiring (1.2) is that all classical physical theories satisfy it (or something very similar). But although this may well be part of the reason why (almost always) we do <u>in fact</u> require (1.2), it isn't on it's own a reason why we <u>should</u> do so. After all, it might be the case that at least one crucial respect in which quantum mechanics differs from classical theories is in that its best interpretation doesn't satisfy (1.2).

Other reasons for requiring (1.2) are related, as I said above, to the question of the meaning of 'possible' in the context of (1.2). To ask this question isn't simply to ask for a paraphrase, of course - rather it's to ask for explication of a notion which will play a crucial and foundational role in any account of quantum mechanics which does involve (1.2). And it is a philosophical question, metaphysical rather than physical.

I don't want to get into the philosophical arguments about this notion of possibility in any detail here, though not because I have no views on the matter. On the contrary, I think there are quite strong arguments favouring one position ahead of others - and I think that whether one accepts this position should have a strong bearing on whether one is prepared to give up (1.2) in giving an interpretation of quantum mechanics.

I think the proper starting point for a consideration of these matters is a certain viewpoint about time and existence, which is usually described as 'determinateness', or 'determinacy' (though Popper calls it 'metaphysical determinism'), and which may be expressed in several ways (for our purposes, at least - I don't claim they all amount to exactly the same thing): as the view that at all times all propositions have truth values (no matter what their tense, if propositions are held to have tenses); as the view that there is no ontological difference between the future and the past or the present (or between the future and the past-and-present); or simply as the view that no sense can be made of the claim by others that there is such a difference, that the future is in some way (and in contrast to the past and/or present) 'not fixed'. I think there a good arguments for a viewpoint of this kind[1]; but what I want to do here is not to present these arguments, but just to draw attention to some of the consequences of the view which they support for quantum mechanics, and particularly for the question whether a realist interpretation of quantum mechanics need satisfy (1.2).

Towards this end, let us note first of all that so long as we assume determinateness then one thing we should say is that with respect to any individual system s at any time, there is as a matter of fact at most one measurement m which is the next measurement on s - and that if there is no such m, then after this time no measurement is (ever) going to be performed on s. More generally, and subject to our acceptance of a certain kind of discreteness, which we'll come to below, we should say that as a matter of fact there is at most one system r which is the next system to interact with s - and that if there is no such r, s is never again going to interact with anything. As determinatists (i.e., as 'proponents of determinateness') we can't claim that there isn't yet a fact of the matter - which means, obviously, that we can't construe 'possible' in (1.2) in terms of some present lack of definiteness in the facts.

What we therefore should do, it seems to me, is to construe it, somehow, in terms of what we know, or believe, about the system s and its future. I think the proper way to do this is to produce an account of chance in the same framework, and, killing two words with one stone, to read 'possible' as, roughly speaking, 'having chance greater than zero' ('possible' in certain contexts only, of course - I am certainly not suggesting that logical possibility can be explained this way). I think such an account of chance can be produced (as I am trying to show in my <u>Probability for Determinatists</u>), and that at least so long as we

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1. Which is not to be confused with the determinist viewpoint, of course.

accept determinateness it has clear advantages over alternative accounts which construe chance, and physical possibility as well, as objective and non-relational - though not because the latter accounts are automatically ruled out by determinateness, as seems to be widely assumed: the existence of modal facts doesn't depend on gaps in the non-modal ones, even though that is how they are usually described.

Be that as it may, it is clear that if we weren't determinatists (by assumption at least), and hence had less scruples about accepting a thoroughly non-relational, realist and objective account of possibility, then we would have at least a good basis for an answer to the question 'Why should we insist on (1.2)?'. Thus we could note firstly that if a PV account or hidden variable theory is such that the result of <u>no</u> possible measurement follows from the values of the dynamical variables involved, then it is obviously trivial - indeed, from a mildly verificationist point of view, meaningless. Secondly, we could point out that if there is as yet no matter of fact as to which among possible measurements will be actual ones, then a theory which caters for some but not all possible measurements will have no defence against a charge of being arbitrary and incomplete. So, we would be able to conclude, any satisfactory such theory must satisfy (1.2).

But what if we are determinatists? We might try to argue for (1.2) on the following lines: consider a system s on which we have a choice as to whether to perform a position or a momentum measurement (say) in the near future, and suppose s is correctly described by a PV account, or hidden variable theory, T, which doesn't satisfy (1.2). Suppose in fact that at least with respect to position and momentum measurements, all that follows from what T says about s is the value given by whatever kind of measurement is in fact going to be made. Then presumably it is deducible from the present parameters according to T whether the next measurement which is going to be performed will be of position or of momentum. So (A) 'it is already determined' whether we are going to measure position or momentum - i.e. 'no free will', etc.; and/or

(B), even worse, if we know what T says about s we can predict what we are going to do in the future in this respect, and then, surely, choose to do the opposite, giving a contradiction from the assumption that T exists and is true - i.e. a reductio proof of the need for a PV account or hidden variable theory to satisfy (1.2).

Let us take (B) first. The derivation of a contradiction rests not only on the assumption of the existence and truth of T, but also, clearly, on the premiss that at least in principle we may know enough about s according to T to derive the nature of the next measurement on s. And it is not difficult to see how this could fail to be the case: if what we can in principle know about s is what we can infer from our knowledge of the history of s, then, without (1.2), it might be that what we would need to know of the description of s according to T in order to derive the nature of the next measurement on s, cannot be known even in principle without actually performing a measurement on s - which, of course, will be the 'next measurement' whose nature we are trying to predict; so there is here no question of succeeding in making such a prediction. Given (1.2), things might be different: to discover the relevant facts about s without actually performing the 'next measurement on s' ourselves to do so, we might perform a measurement which we could have performed on s on another system with the same history (in relevant respects) as s itself, and, using (1.2), argue that the result applies equally to s. It is clear that (1.2) is crucial, so that without it the additional premiss on which the contradiction in (B) rests is unjustified.

Notice the use we are making of discreteness at the quantum level, particularly in giving a clear sense to the notion of 'the next measurement', or 'the next interection' in general (we have no need to regard measurements as anything other than particular types of physical interactions). We are using something like the following picture of how the world is at this scale: discrete particles, whose endurance over periods of time defines their worldlines, interact in certain ways; these interactions are intersections in the worldlines of the particles involved, and the set of all intersections on a given worldline is given the order-type of a subset of the integers by the natural (temporal) ordering on the worldline itself; and each particle has certain properties, the values of which vary from point to point on its worldline, subject to certain conditions - for example, perhaps, that momentum only varies at intersections, and then in such a way that total momentum is conserved. So far there is nothing non-classical about this picture, of course - the question whether (1.2) holds is left open, and amounts to whether certain kinds of generalisations are true of the structure as a whole. We'll come back to this in a little more detail later on.

For the moment, let us get back to (A) above (at the bottom of page 4), which which was essentially the claim that a PV account or hidden variable theory which didn't satisfy (1.2) would raise difficulties for free will. I think the determinatist's answer is clear: the mere existence of a 'present' fact from which is deducible our own future behaviour is no more a problem for free will than the existence of future facts with the same property - there would only be a difference if one were knowable and the other not, which would give rise to (B), which we have dealt with. So (A) is no new problem for determinatists - they have been dealing with the claim that their view is inconsistent with free will for centuries (Leibniz is a notable example) - successfully, in my view, but I don't want to go into that here.

2: Let us try to pin down what is involved in giving up (1.2). As a first step, note that at the core of any physical theory is a classification of its subject matter into kinds - this classification is a prerequisite for the expression of the general statements which comprise the laws of the theory in question. These general laws in turn license certain counterfactual statements (however the notion of 'counterfactual' is itself construed). Roughly speaking, it is our knowledge of how the same kind of thing behaves in the same kind of circumstances which enables us to assert a counterfactual statement about a given thing in certain (non-actual) circumstances.

(1.2) amounts to the requirement that a PV account or hidden variable theory for quantum mechanics support certain counterfactual statements about measurement - 'If we were to measure the momentum we would get x', 'If we had measured the position we would have got y', and so on - and hence it requires that such an account or theory have a certain structure of kinds. In particular, it requires that what <u>kind</u> of entity a system is doesn't depend on what measurements, or interactions in general, it is as a matter of fact going to be subject to in the future - i.e. that type depends on past history but not on future 'history' (note that we are using 'kind' and 'type' in a somewhat deviant sense - we are taking a difference of kind (or type) in the context of a given physical theory, or of part of such a theory, to be any difference at all which is relevant to the lawlike general statements of that theory, or part of theory - so that, for example, in applying part of classical mechanics to the motion of billiard balls, a difference of momentum may constitute a difference of kind).

3: I am suggesting that from a determinatist's point of view (1.2) is a constraint which it is very difficult to justify on metaphysical grounds. Even if I am right, this conclusion would be of very limited interest if it weren't for the well known difficulties in giving a PV account, let alone a hidden variable reformulation, of quantum mechanics. If such an account were available, so as to give us a unitary realist interpretation of quantum mechanics in such a way as to satisfy (1.2), then we would have no reason to feel we should question such a deeply entrenched (and 'intuitive') classical assumption.

As it is, such an account does appear to be impossible. The two major results which seem to prove that this is the case are the theorems of Bell (J. Bell, 'On the Einstein Podolsky Rosen Paradox', <u>Physics</u>, 1(1964), pp195-200) and of Kochen and Specker (Simon Kochen &

E. P. Specker, 'The Problem of Hidden Variables in Quantum Mechanics', J of Math & Mech., 17(1967), pp59-87). However, it is not difficult to see that both these theorems do only apply to the case in which (1.2) is satisfied: Kochen and Specker's because it applies to a 'hidden' phase space which is classical in just this sense - with the assumption that 'if a physicist believes in hidden variables he should be able to predict (in theory) the measured value of every quantum mechanical observable' (Kochen and Specker, p73); and Bell's because it relies on certain counterfactuals about measurement whose justification depends on (1.2) (see N. Herbert & J. Karush, 'Generalization of Bell's Theorem', Found. of Physics, 8(1978), pp313-7; though these authors claim what they call 'counterfactual definiteness' is necessary to define 'locality', which seems to me to be a mistake - we can say simply that a theory is non-local if it implies that there are kinds of situations in which entirely new correlations are suddenly set up between distant points in a regular way; the importance of (1.2) is rather that so long as we allow the precise values of the dynamical variables of two particles to be correlated between their point of interaction and later spatially separated points of observation with the actual nature of the observations concerned, in just the manner that 'counterfactual definiteness' disallows, then there need be no question of sudden and associated changes occurring simultaneously at distant points).

Obviously the fact that certain proofs of the impossibility of a PV account of quantum mechanics depend on (1.2) doesn't imply that in the absence of (1.2) there is such an account. It seems that if we wish to avoid the use of 'measurement' as a fundamental concept in our interpretation, then producing such an account is by no means a trivial matter - for if the nature of its future interactions is to play a role in deciding what kind of thing a quantum system is (in our deviant sense of 'kind' - see page 7), then we shall need some sort of classification of interactions. On the other hand, if we accept that measurements are simply particular kinds of physical interactions, then it seems there is at least some prima facie evidence for the existence of such a classificati n, in the fact of the incompatibility of certain different kinds of measurement.

I have nothing to say here about how the details might go of such a classification, and hence of a PV account which isn't constrained by (1.2) - my intention has been simply to argue that (at least for a determinatist) the strategy of looking for such an account is not at all the objectionable one it might at first appear. I want to finish with a remark about the effect of giving up (1.2) on our ideas of the nature of measurement.

It is often said that quantum mechanics has overthrown the classical view of measurement, that it denies the separability of the measured system from the measuring apparatus, and so on. However, what is often at the base of claims of this kind is simply the Heisenberg position: any measurement involves at least a certain level of uncontrollable disturbance of the measured system, in contrast to the situation according to the classical view, under which this disturbance is at least in theory reducible without limit. Heisenberg's view of measurement does nothing to question what we might call the <u>contingency</u> of measurement - the notion that the physicist, or the measuring apparatus, stands outside the enduring world, simply 'probing' at it at points which need depend in no particular way on the structure of the world itself, able to catch nature 'acting naturally' (even if, like a flash photographer at a crowded party, always destroying the 'natural course of events' in doing so). The incompatibility of certain pairs of measurements is not in the least inconsistent with this underlying contingency (it is simply as if our photographer had a choice as to whether to take each picture in black and white or in colour).

Giving up (1.2), on the other hand, would involve modifying our view of measurement (in quantum mechanics) in this respect - no longer could we take it that at the quantum level there is such a thing as 'catching the world acting naturally'. So we would have a much stronger sense than Heisenberg's to the claim that in quantum mechanics there is no sharp separation between the measured system and the measuring apparatus. What we would have, in fact, would be something much closer to Bohr's view of measurement, even if achieved in a very different way.

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