Nonlocality and the Epistemic Interpretations of Quantum Mechanics

Or: Where are Alice and Bob?

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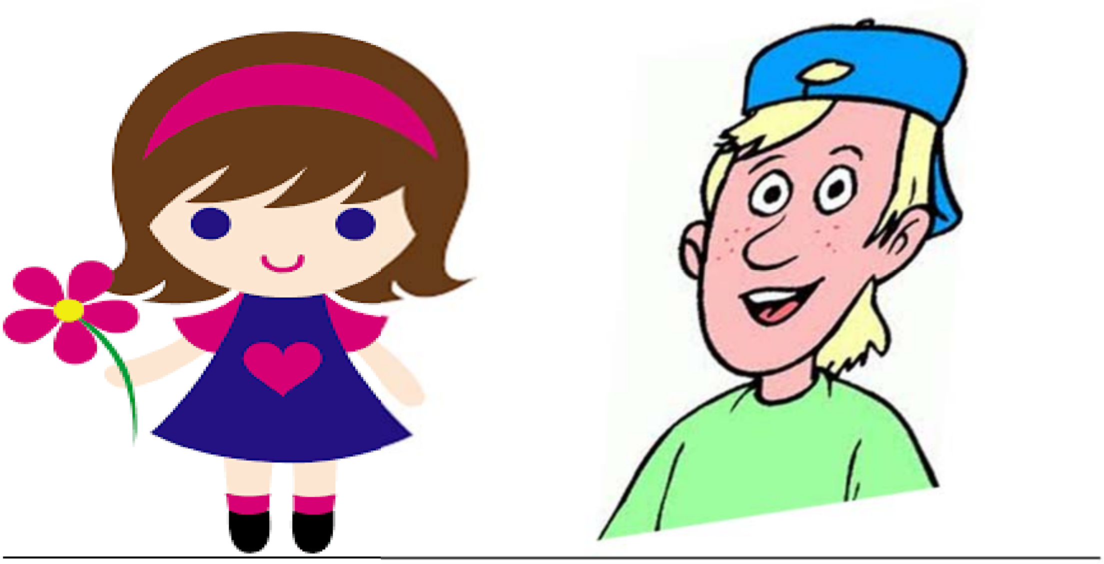

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I. The Problem

Quantum mechanics (QM) is known to be nonlocal in the sense that it sanctions entanglement, where entangled states (even when separated to space-like distances) appear to manifest more correlations than allowed by classical considerations in general and the special theory of relativity (STR), in particular. Although it is generally agreed that nonlocality as such does not violate STR unless the nonlocal correlations can be used to 'signal', i.e., transmit information, and although there are arguments demonstrating that the said quantum mechanical correlations cannot be used to signal, worries about nonlocality and its physical underpinning remain.[1] The worry, to use Popescu and Rohrlich's terms, is that nonlocality violates the spirit, even if not the letter of STR. This potential tension between QM and STR is one of the major problems in the foundations of QM. The question I wish to address in this paper is whether quantum nonlocality is a space time phenomenon, for if it is not, no conflict with any spacetime theory, and in particular, no conflict with STR, should arise.

At first glance, the question does not make sense. Surely, nonlocality must be embedded in spacetime. How else can the very notions of locality and non-locality be characterized, if not by reference to a spatio-temporal framework wherein certain events, but not others could be local or nonlocal, correlated or uncorrelated? More generally, what could it mean for any physical relation *not* to be in spacetime? And yet, this intuitive response has being challenged by a growing number of leading theorists, among them Itamar Pitowsky, Jeffrey Bub and Christopher Fuchs, who wish to disentangle quantum entanglement from any spatio-temporal picture and constraint. Referring to the quantum correlations obeying Bell-type inequalities, Pitowsky writes:

> Altogether, in our approach there is no problem with locality and the analysis remains intact no matter what the kinematic or the dynamic situation is; the violation of the inequality is a purely probabilistic effect. The derivation of Clauser-Horne inequalities… is blocked since it is based on the Boolean view of probabilities as weighted averages of truth values. This, in turn, involves the metaphysical assumption that there is, simultaneously, a matter of fact concerning the truth-values of incompatible propositions. … [F]rom our perspective the commotion about locality can only come from one who sincerely believes that Boole's conditions are really conditions of possible experience. But if one accepts that one is simply dealing with a different notion

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[1] For one such "no signaling theorem" see Cushing (1994) chapter 10, Appendix 2. p.195-196. For more general discussions of the compatibility problem see, for example, Redhead (1987), Maudlin (2011), Stapp (1997), Mermin (1998), Shimony and Stein (2003). The possibility that quantum 'action at a distance' could indeed spread with superluminal speed but, nonetheless, could not be used for signaling and would therefore generate no conflict with STR has already been identified by Heisenberg ([1930] 1949, p. 39). However, Heisenberg only addressed the immediate collapse of the wave function at a distance (i.e., at a distance from the location of the measurement), not entanglement in general. The compatibility problem regarding entangled states, though noted by Schrödinger, came to the fore only after Bell.

> of probability, then all space-time considerations become irrelevant. (2006, pp. 231-232)

In a joint paper Bub and Pitowsky extend this claim to no-signaling

> 'No signaling' is not specifically a relativistic constraint on superluminal signaling. It is simply a condition imposed on the marginal probabilities of events for separated systems requiring that the marginal probability of a B-event is independent of the particular set of mutually exclusive and collectively exhaustive events selected at A, and conversely" (2010, p. 443).

These quotations indicate that the key to the possibility of severing the link between nonlocality and spacetime theories is the derivation of non-locality from probabilistic rather than physical constraints. Since probabilistic constraints are basically logical-mathematical constraints, and since logical and mathematical relations are not considered to be temporal, the possibility of inferring quantum nonlocality from probability theory does indeed transform our understanding of this phenomenon, and may thus alleviate the threat of conflict with STR. A major component of the probabilistic approach proposed by Bub and Pitowsky is an epistemic interpretation of QM that denies the reality of quantum states. Before we can answer the question of whether nonlocality is in spacetime, we must therefore get acquainted with the epistemic interpretation.

According to the epistemic interpretation of QM, state functions -- vectors (rays) in Hilbert space, or the wave function-- do not represent physical states, but rather epistemic states of knowledge or belief or information. How are we to complete this sentence -- knowledge or belief or information about what? Is it knowledge about the actual state of the system, possible state of the system, or results of actual or possible measurement? As we will see, such various answers have in fact been offered or implicitly presupposed. It is more the *negative* aspect of epistemic interpretations – what the state function *cannot* be – that its proponents share. Moreover, there are non-epistemic interpretations of the notion of probability that, when applied to QM, also yield a negative verdict on the reality of the state function, but again differ from one another (and from the epistemic interpretation) in the conclusions they draw from this verdict.

In what follows I review some of the probabilistic interpretations that have been put forward in the long history of attempts to understand QM, noting their merits as well as the difficulties they encounter. I look at the implications of a number of no-go theorems for these probabilistic interpretations, including those of the recent PBR theorem (Pusey *et al* 2012). The latter is significant because it is often advertised as undermining the epistemic interpretation, but as I will argue, it actually suggests an epistemic position more radical than previously ones. Identifying the differences between these interpretations – what exactly they take the said knowledge to be about – is significant not only from the conceptual, but also from the historical point of view, for in addition to correcting a number of specific errors, regarding, for example the relation between Schrödinger's and Einstein's views, it provides a distinction between earlier and current probabilistic interpretations and explains the evolution from the former to the latter. On the basis of this review of probabilistic interpretations – the major part of the paper—we can return toward the end to the issue of nonlocality and evaluate the epistemic theorist's attempt to disconnect nonlocality from spacetime considerations.

II. Probabilistic Interpretations of QM

Worries about the meaning of probability in the context of QM accompanied the theory almost from birth. As early as 1926, right after offering his probabilistic interpretation of Schrödinger's Ψ function, Born also noted its curious implications: although particles follow probabilistic laws, the probability itself transforms "in accordance with the causal principle," i.e., deterministically. Furthermore, QM only answers well-posed statistical questions, and remains silent about the individual process. Born therefore characterized the theory as "peculiar blend of mechanics and statistics,"--"*eine eigenartige Verschmelzung von Mechanik und Statistik*".[2] .

This formulation is almost identical to that given by Jaynes and quoted in the PBR paper (Pusey *et al* 2012, p. 475):

> But our present (quantum mechanical) formalism … is a peculiar mixture describing in part realities of Nature, in part incomplete human information about Nature, all scrambled up by Heisenberg and Bohr into an omelette that nobody has seen how to unscramble.

The majority of quantum physicists were willing to tolerate the paradoxical situation diagnosed by Born. They treated QM both as a probabilistic theory and as a fundamental, irreducible physical theory that replaces the classical equations of motion with the wave equation. The question of whether the concept of probability could sustain this *Verschmelzung* was left hanging in midair. Outside of the orthodoxy, however, there were a few dissenters, among them Einstein, who preferred to bite the bullet and understand QM as a full blown probabilistic theory. In the first few decades, what was meant by this dissident probabilistic view was that QM applies only to ensembles of similarly prepared systems (not to be confused with a many-particle system such as a beam of electrons or photons). The subjective-epistemic understanding of probability as the degree of knowledge or belief is more recent. Note that in the above quotation, Jaynes presupposes such an epistemic interpretation, but the scrambling he refers to does not depend on it. We could replace the subjective epistemic construal of probability with an objective ensemble interpretation and the question would remain the same: What, if anything, does QM say about the individual system?

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[2] Born [1926] (1963), p. 234. "Die Bewegung der partikeln folgt Wharscheinlichkeitsgesetzen, die wharscheinlichkeit selbst aber breitet sich im Einklang mit dem Kausalgesetz aus." "Die Quantenmechanik allenfals nur Antwort gibt auf richtig gestellte statistische Fragen, aber im allgemeinen die Frage nach dem Ablauf sines Einzelprozesses unbeantwortet laesst."

To better understand this question, we can break it down to a number of (inter-related) sub-questions, some of which highlight the merits of epistemic or ensemble interpretations while others reveal their difficulties. (When there is no need to distinguish ensemble from epistemic understandings of probability, I will speak of an E-interpretation).

1. Superposition and other wave-like effects. What do quantum phenomena such as superposition, interference and entanglement mean under an E- interpretation? Such phenomena had a reasonable interpretation in Schrödinger's original vision of wave mechanics, but he too became disillusioned when realizing the wave function was a multi-dimensional wave in configuration space, not an ordinary wave in three dimensional space. Born's interpretation of Ψ as a probability amplitude had the advantage that Ψ no longer needed to be situated in three dimensional space, and the disadvantage that the meaning of superposition and interference became mysterious. Getting a pattern on the screen from the interference of abstract probability waves seems like getting wet from the probability of rain. We could also formulate this question in terms of the meaning of the phase of the wave function. Periodicity appears in all versions of QM, but the expression of this periodicity -- the phase – disappears in the calculation of probabilities. If we are only interested in probabilities and deny the reality of the wave, there is no good explanation for the significance of the phase. An intriguing example which suggests a physical meaning of the phase is the Aharonov-Bohm effect where the phase-change in a force-free region yields an interference pattern. It is difficult to explain this interference from the perspective of an E-interpretation.[3]

2. The uncertainty relations. What is the status of the uncertainty principle on the probabilistic interpretation? If we construe Heisenberg's principle as a statistical law, it should only constrain the expectation values of variables in an ensemble of systems. It should thus be compatible with QM for an individual system to have sharp values of all variables including canonically conjugate ones. Indeed a number of proponents of the ensemble interpretation, for instance Karl Popper, were willing to sanction such a view. Interestingly, in correspondence with Popper in 1935, Einstein explicitly denies this conclusion.[4] Does Einstein, then, maintain that at least one quantum law applies to the individual system, contrary to what an ensemble interpretation would seem to imply? Not quite. The1935 letter to Popper suggests that one cannot violate the uncertainty relations by direct measurement, but can nonetheless circumvent them in the split system described in the EPR paper. The letter is illumination because, following Arthur Fine (1986), it has been repeatedly claimed, first, that the EPR paper does not represent Einstein's position accurately, and second, that the EPR thought experiment was not intended to challenge the uncertainty relations.[5] Einstein's letter casts doubt on both of these claims. He summarizes the EPR argument in terms that are very close to the published version, including the notorious reality criterion which, allegedly, he did not endorse. And he further explains that unlike measurement on a single particle, the two-particle system provides an argument for the simultaneous existence of definite values of both position and momentum and thus tells

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[3] There are different interpretations of the Aharonov Bohm effect (see, for example Aharonov and Rohrlich (2005) chapters 4-6 and Healey (2007) chapter 2), but, none of them seems intuitive from the perspective of an E-interpretation.

[4] The letter is reproduced in Popper (1968) pp. 457-464.

[5] Both of these claims can be found in Harrigan and Spekkens (2010).

against the Copenhagen understanding of the uncertainty principle. The letter leaves no doubt that this conclusion about the uncertainty relations is of great significance to Einstein.

Einstein agrees with Popper's main point, namely that "the Ψ function characterizes a statistical aggregate of systems rather than one single system" adding that "This view makes it unnecessary to distinguish, more particularly, between 'pure' and 'non-pure' cases". At the same time Einstein rejects "*in principle*" Popper's idea of a simultaneous measurement of position and momentum on a single photon. The 'in principle' considerations involve disturbance. Measurement of one variable is bound to distort the other. He then describes the two-particle system of the EPR paper. In this case, unlike the direct measurement Popper had suggested, no interaction between the separated particles can take place. "One can therefore hardly avoid the conclusion that the system B has indeed a definite momentum and a definite position co-ordinate." Thus, Einstein does not see the uncertainty principle as applying to every individual system, but agrees that it is a constraint on simultaneous measurement of conjugate variables and presumably also on the preparation of a system with sharp values of such variables. Needless to say, precisely because he understands QM as an ensemble theory, Einstein is unhappy with QM as a fundamental description of reality: "I do not believe that that we shall have to be satisfied forever with so loose and flimsy a description of nature."

3. Disturbance by measurement: Thought experiments by Heisenberg and others motivated the uncertainty principle by showing how measurement of one quantum observable, say position, changes the value of another observable, in this case, momentum. On the ensemble interpretation, it appears, this disturbance could be avoided, for we are not referring to properties of, or measurements on, the same particles. But then, what is the lesson of the seemingly successful thought experiments? Is it indeed possible to liberate QM from such mysterious disturbance? Not according to current E-interpretations. In recent toy models of the E-interpretation the disturbance is introduced in order to protect the underlying assumption of maximal-and-yet-less-than-complete information (see below).

4. Analogy with statistical mechanics: What is the relation between the probabilistic description of the ensemble and the description of the individual system? The natural analogy was statistical mechanics which, presumably, enables peaceful coexistence between the probabilistic description of an ensemble and the precise description of each individual system in terms of classical mechanics.[6] Unlike proponents of the Copenhagen interpretation, who rejected this analogy outright, Einstein and other advocates of an ensemble interpretation were using it as key to the understanding of QM. The analogy is tricky. The wave function does not seem analogous to the macroscopic thermodynamic parameters and the picture of a more fundamental level than QM, bearing to QM the relation that classical mechanics bears to statistical mechanics, is precisely the picture that advocates of the Copenhagen interpretation opposed. For them QM was a fundamental theory that does not supplement but *replace* classical mechanics. By contrast, proponents of the ensemble interpretation, who regarded the analogy with statistical mechanics as promising, were engaged in attempts to demonstrate the incompleteness of QM and the feasibility of a more fundamental level that would complement the quantum level.

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[6] It turns out that there are serious problems regarding the compatibility of statistical mechanics and classical mechanics, but these will not concern us here. See Albert (2000), Hemmo and Shenker (2012) and the literature there cited. Note, further, that despite the facts that the laws of statistical mechanics are probabilistic, there is no binding reason to construe these probabilities as subjective.

In statistical mechanics, macrostates are characterized by thermodynamic properties such as temperature and entropy and are realized by many-particle systems which could, in principles (even though not in practice) be given a precise (non-probabilistic) characterization in terms of the mechanical properties of each of their constituting particles. The relations between macro and micro properties are probabilistic. Some macroscopic parameters, such as temperature and pressure, are relatively simple averages over microscopic parameters -- the kinetic energies and the momenta of microscopic particles. Others, first and foremost entropy, are well-defined for macrostates, but have no clear physical significance for individual microstates. And yet, even with regard to entropy, for each microstate there is a definite answer to the question of whether it belongs to a certain macrostate (i.e., whether it has a certain entropy). For all macrostates, then, once they are properly defined, the relation between micro states and macrostates is functional: Each microstate belongs to a single macrostate. Not so in the reverse direction: typically, the same macrostate is multiply realized by numerous microstates. As this kind of many-one relation is known in the philosophical literature as supervenience, we can say that in statistical mechanics, macrostates supervene on microstates. We will see that the question of whether an analogous relation of supervenience exists in QM is at the center of the PBR theorem.

5. Collapse. Here lies the great merit of E-interpretations, for whether or not probability is understood subjectively, the fact that measurements on individual systems result in definite values is exactly what one would expect. Since, on an E-interpretation the probability for any particular result is not a physical state of the individual system to begin with, worries about the physical properties of the collapse, its Lorenz invariance etc. are totally misplaced.

6. Philosophical packing: Finally one could also hope that from the perspective of an E-interpretation, the mysterious philosophical aura that accompanied the Copenhagen interpretation, duality and complementarity in particular, would disappear.

The questions enumerated so far pertain to the very *meaning* of QM not to its explanation. In the case of the uncertainty relations, for instance, the question was not why they hold, but whether they hold (for the individual system). I stress this point because one is often told that the search for an explanation is driven by classical intuitions, or that explanations must at come to an end at some point, or that we should take QM at face value and give up the search for deeper explanations. But the questions raised here do not rely on specific classical intuition, and are conceptually prior to questions about explanation. They must be addressed before we can even state what it is that we are supposed to take at face value.

III. Schrödinger versus Einstein

As the years went by, the orthodoxy became exceedingly convinced that there is no analogy between QM and statistical mechanics. QM, rather than supervening on a more basic theory *is* a fundamental, irreducible theory. This conviction discourages an ensemble interpretation, but on the other hand, it fails to provide satisfactory answers to the above questions; it does not separate the probabilistic features from the dynamical. It does not unscramble Jaynes's omelette.

Ironically, an important step towards the clarification of the difference between classical probabilities of the kind we find in statistical mechanics and quantum mechanical probabilities was made by Schrödinger. Ironically – because Schrödinger, who was critical of the Copenhagen interpretation, is generally considered a comrade of Einstein in opposition to the orthodoxy. And yet it was Schrodinger who saw that quantum probabilities are non-classical. Moreover, he actually used this insight to criticize the EPR argument! Schrödinger's insightful assertion of the non-classical nature of quantum probabilities anticipates arguments driven home decades later by the work of Bell, Gleason, Kotchen and Specker, and others. He stressed, further, that his conclusions regarding quantum probabilities are not derived from a particular dynamics but from structural features of the space of events. He made the following pertinent observation:

> One should note that there was no question of any time-dependent changes. It would be of no help to permit the model to vary quite 'unclassically,' perhaps to 'jump.' Already for the single instant things go wrong. At no moment does there exist an ensemble of classical states of the model that squares with the totality of quantum mechanical statements of this moment. The same can also be said as follows: if I wish to ascribe to the model at each moment a definite (merely not exactly known to me) state, or…to *all* determining parts definite (merely not exactly known to me) numerical values, then there is no supposition as to these numerical values *to be imagined* that would not conflict with some portion of quantum theoretical assertions. **([**1935]1983, p. 156)

Consequently, Schrödinger takes the uncertainty relations to constitute neither an epistemic limitation on what we can know, nor a practical limitation on what we can measure or prepare. Rather they constitute a fundamental limitation on the assignment of values to all physical magnitudes (at a particular moment). Accordingly, "The classical notion of *state* becomes lost in that at most a well-chosen half of a complete set of variables can be assigned definite numerical values" ([1935]1983, p.153). Schrödinger therefore goes on to rule out the possibility that quantum probabilities and uncertainties are analogous to probabilities in statistical mechanics, reflecting our ignorance rather than genuine indeterminacy at the level of physical reality.

Schrödinger takes the Ψ function to represent a maximal catalog of possible measurements. It embodies "the momentarily-attained sum of theoretically based future expectations, somewhat as laid down in a *catalog.* ... It is the determinacy bridge between measurements and measurements" (Ibid.p.158). As such, upon a new measurement the Ψ function undergoes a change that "*depends on the measurement result obtained*, and so *cannot be foreseen*" (ibid**.** italics in original.) The maximality or completeness of the catalog--a consequence of the uncertainty relations--entails that we cannot have a more complete catalog, that is, there can be no two Ψ functions of the same system one of which is included in the other. "Therefore, if a system changes, whether by itself or because of measurement, there must always be statements missing from the new function that were contained in the earlier one" (Ibid.p. 159). In other words, any additional information, arrived at by measurement, must change the previous catalog by *deleting* information from it. This is the basis of the 'disturbance' that the measurement brings about. True statements, that had been part of the catalog prior to the measurement, now become false. In other words, at least some of the previous values have been destroyed. This understanding is strikingly similar to that emerging from Spekkens's toy model where disturbance is secures the principle that there is a limit on information transmission.

But now comes entanglement. This new feature also follows from the maximality or completeness of the Ψ function. Schrödinger argues as follows: A complete catalog for two separate systems is, *ipso facto*, also a complete catalog of the combined system, but the reverse does not follow. *“Maximal knowledge of a total system does not necessarily include total knowledge of all its parts, not even when these are fully separated from each other and at the moment are not influencing each other at all”* (Ibid.p. 160, italics in original). The reason we cannot infer such total information is that the maximal catalog of the combined system may contain conditional statements of the form: *if* a measurement on the first system yields the value *x,* a measurement on the second will yield the value *y*, and so on. He sums up: “Best possible knowledge of a whole does not necessarily include the same for its parts…The whole is in a definite state, the parts taken individually are not” (p.161). In other words, separated systems can be correlated or entangled via the Ψ function of the combined system, but this does not mean that their individual states are already determined!

Schrödinger tells us (in f.n. no. 7) that his 1935 paper was written in response to the EPR paper published earlier that year. As mentioned, one tends to think of Schrödinger as Einstein’s ally in opposing the Copenhagen interpretation, and, indeed, Schrodinger’s paper leaves no doubt his unhappiness with “the present situation in QM”. One can therefore easily miss the point that without saying so explicitly, Schrödinger also assumes here the role of Einstein’s critic, launching a much more lucid and effective critique of the EPR argument than that of Bohr.[7] The EPR argument purports to show that the correlations between the remote parts of the system—the conditional statements—entail that each individual state already had a determinate value *prior* to measurement. By contrast, Schrödinger argues, first, that such determinacy is precluded by the uncertainty relations properly understood, and second, that, given his reading of the Ψ function as a maximal catalog of possible measurements, the indeterminacy of individual outcomes makes perfect sense. In other words, whereas the EPR argument seeks to understand the correlations in terms of predetermined values, which amounts to understanding them in terms of common causes, Shrödinger sees that this solution does not work. He therefore suspects that QM might be incompatible with STR.

Here then is a very fundamental difference between Einstein and Schrdöinger: Whereas Einstein endorsed the ensemble interpretation, hoping that it would make QM compatible with a more fundamental theory along the lines of the relation between classical and statistical mechanics, Schrödinger deemed the ensemble interpretation fundamentally flawed. Proponents of the ensemble interpretation, even those who should have been aware of Schrödinger's arguments (Popper, for instance) simply ignored them. The analogy with statistical mechanics was still popular among these thinkers. Only a few (e.g. Ballentine 1970 and Blokhintsev 1968), made an effort to acknowledge the difference between

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[7] Not only is Schrödinger’s critique of the EPR argument missed, he is generally portrayed as voicing the same concerns as Einstein about the Copenhagen interpretation. See, For example, Harrigan and Spekkens (2010) and Maudlin’s “Preface to Third Edition” (2011 p. xiii), where he speaks indiscriminately of “Einstein’s and Schrödinger’s real concerns”.

classical and quantum probabilities, but failed to come up with satisfactory answer to the above questions. In particular, as late as 1970, a few years after Bell, they ignored the difficulty regarding the applicability of the uncertainty relations to the individual system.

Schrödinger's argument had little impact, neither on Einstein whom he criticized nor on the orthodoxy whose position he clarified (and in so doing, actually defended). His crucial point about the non-classical nature of quantum probability has not been recognized and when it was, it was not ascribed Schördinger. His “maximal catalogue of possible measurements” anticipates later formulations (e.g., Pitowsky's “book keeping” device) but these too were offered independently in response to later developments, making no reference to Schrödinger. Schrödinger differs however from such recent allies in that he did not take quantum probabilities to be subjective -- degrees of knowledge or belief. Furthermore, he took entanglement to be in spacetime and therefore worried about the tension between nonlocality and STR.

To sum up the first phase of attempts at an E-interpretation: In this early period the dominant alternative to the Copenhagen interpretation of quantum probabilities is an ensemble interpretation (even if not always under that name) rather than an epistemic interpretation. Unless probabilities are generally equated with states of knowledge or uncertainty, the ensemble interpretation of QM is no less objective than the Copenhagen interpretation. By comparison, recall the current situation in statistical mechanics: Probabilities are understood as subjective by some and objective by others without this difference making a significant difference to the contents and predictions of statistical mechanics. As we will see, in subsequent years the situation in QM turned out to be different: It does make a difference which sense we ascribe to its probabilities.

Beyond the status quo

While Einstein's anti-Copenhagen arguments in the EPR paper and related correspondence left the dispute over the scope of the uncertainty relations at a dead end, later developments managed to move the dispute out of that limbo. Since these developments are familiar and well-researched by physicists and philosophers, I will be brief. Already with Bell it became clear that Schrödinger was right and that if we wish to maintain local causality we must not even *ascribe* definite values to canonically conjugate variables. In other words, contrary to EPR, if local causality is retained (and surely, Einstein wanted to retain that assumption) the uncertainty relations are not mere limitations on statistical distributions, or on actual measurement of conjugate variables. They hold for individual systems and are obeyed, in particular, by individual entangled states at a space-like distance. Despite the force of Bell’s theorem, there were attempts to resist this conclusion and interpret the violation of the classical inequalities by QM as a statistical phenomenon that does not apply to the individual system. The Kochen-Specker theorem and the CHSH theorem (Clauser et al 1969), which can be derived from Gleason's theorem, provide no-go theorems that are *more* definitive on this point. They cannot be interpreted statistically and make a stronger argument for the conclusion that any ascription of definite values to all variables in the classical manner lands in conflict with QM.

In addition to undermining attempts to construe the uncertainty relations as statistical, these no-go theorems also undermine the analogy between QM and statistical mechanics. They suggest that QM does indeed constrain the behavior of individual systems in ways that differ from the probabilistic constraints of statistical mechanics. In other words, the ensemble interpretation as understood by its proponents up to the period surveyed so far gets into conflict with the predictions of QM and with the experiments confirming it.[8] This realization encourages an epistemic interpretation of quantum probabilities. On an epistemic interpretation, the constraints QM imposes are constraints on what one can know about a quantum system or on the maximum of information allowed to pass between quantum systems. Unlike the ensemble interpretation, the epistemic interpretation renders such constraints applicable to the individual system, though only indirectly, via the constraints on knowledge about it. The question of whether, and in what ways, such indirect constraints on knowledge and information can be translated into more direct constraints on the state of the system is intriguing. The PBR theorem and the work by Harrigan and Spekkens on which it is based address this question.

Harrigan and Spekkens (2010) introduce a general distinction between Ψ-ontic models and ψ-epistemic models of QM. In Ψ-ontic models, the Ψ function corresponds to the physical state of the system; in Ψ-epistemic models Ψ represents knowledge about the system. Consequently there are also two varieties of incompleteness: Ψ could give us a partial description of the *physical* state or a partial representation of our *knowledge* about the state. If a Ψ-ontic model is incomplete, it is conceivable that Ψ could be supplemented with further parameters – 'hidden variables'. In this case, the same Ψ function could correspond to various physical states of the system, distinguishable by means of the values of the additional hidden variables. Ψ-epistemic models can also be complete or incomplete but completing them cannot be accomplished by hidden variables of the former kind. If the quantum state gives us *maximal* knowledge about the system, as Schrödinger had urged, it is perhaps not surprising that any attempt to modify Ψ so as to surpass the limits it imposes on our knowledge must result in conflict with QM.

So far these are only terminological clarifications. Harrigan and Spekkens further suggest, however, that if the Ψ functions is understood epistemically, it can stand in a non-functional relation to the physical state of the system, that is, the same physical state may correspond to two different (non-identical but also not orthogonal) Ψ functions. Or, in the probabilistic case, the supports of the probability distributions corresponding to different Ψ functions can overlap for some physical state. This possibility, they claim, only makes sense under the epistemic interpretation of Ψ, for it is conceivable that knowledge about the physical state could change or be updated without any change taking place in the physical state itself. In short, a criterion for a model being Ψ epistemic is precisely the possibility of such overlap of the supports of different probability functions. Let me stress that allowing such a non-functional relation between the physical state of the system and the quantum state represented by Ψ is a non-trivial assumption for it means that the quantum state does not even supervene on the physical state. Recall that statistical mechanics prohibits this situation: while macrostates in statistical mechanics correspond to numerous microstates, a microstate always belongs to a single macrostae. Allowing the epistemic state to vary independently of the physical state presupposes a more subjective understanding of the epistemic state in QM than that of macrostates in statistical mechanics.

Harrigan and Spekkens (who mention neither Schrödinger's interpretation of Ψ surveyed above, nor his objections to Einstein), note that in the correspondence with

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[8] Of course, Bohm's interpretation of QM is not undermined by these results and can therefore maintain the analogy with statistical mechanics. Recall, however, that even here the analogy is incomplete. In particular the equilibrium state excludes knowledge of the individual states – an exclusion that has no parallel in statistical mechanics.

Schrödinger, Einstein's complaint about QM was precisely this -- different Ψ functions correspond to the same physical state. The same complaint appears in the above mentioned letter to Popper. Having described the EPR two-party system, Einstein says:

> Now it is unreasonable to assume that the physical state of B may depend upon some measurement carried out upon system A which by now is separated from B (so that it no longer interacts with B); and this means that two different Ψ-functions belong to one and the same physical state of B. Since a *complete* description of a physical state must necessarily be an *unambiguous* description (apart from superficialities such as units, choice of co-ordinates etc.),it is therefore not possible to regard the Ψ-function as a *complete* description of the state of the system.

Harrigan and Spekkens use their distinction between Ψ-ontic and Ψ-epistemic models to rephrase Einstein as objecting to *the* Ψ-epistemic interpretation of QM.

Building upon the Harrigan-Spekkens analysis, the PBR paper (Pusey, Barrett and Rudolph 2012) raises the question of whether a Ψ-epistemic interpretation of the Ψ-function is consistent with QM. According to the theorem proved in the paper, it is not, namely, if the Ψ epistemic interpretation is accepted and an overlap of the supports of two distinct probability distributions (corresponding to two distinct quantum states) is allowed, a violation of the predictions QM follows. PBR conclude that QM is not amenable to the epistemic interpretation. This surprising result has immediately attracted a great deal of attention. Most readers have taken the theorem at face value: The Ψ-epistemic interpretation is indeed ruled out by the theorem and consequently, the remaining option is the Ψ- ontic interpretations. In other words, the PBR theorem has been advertised as supporting a *realist* interpretation rather than an epistemic interpretation of Ψ. A number of objections to the theorem have been raised on either technical grounds, or on the grounds of a *reductio* argument to the effect that if the theorem were true, it would also rule out Bohm's interpretation, which, however, is known to be empirically equivalent to QM. But regardless of whether these objections hold, the lesson of the theorem, it seems to me, is widely misunderstood. The theorem assumes that the system has a definite physical state (not necessarily known, of course) and takes the epistemic probabilities in question to represent (partial) knowledge or belief *about that physical state*. From the perspective of such an epistemic interpretation, quantum probabilities are indeed analogous to the probabilities of statistical mechanics in the sense that they pertain to distributions of the underlying physical states[9]. It is this analogy and the underlying assumption regarding the existence of a physical state that the PBR theorem rules out! But what if we do not make this assumption? What if we go radically epistemic and deny the assumption of definite physical states? In that case we take QM to be mute about the physical state of the system, interpreting it instead along the lines of Schrodinger, Pitowsky, Bub, Fuchs and others have suggested, as a maximal catalog of possible measurement results, a betting algorithm, a book-keeping device. This option is left untouched by the PBR theorem. Not only is it not undermined by it, to the contrary, in ruling out a more classical probabilistic interpretation, which presupposes the existence of the 'real' state of the system, the PBR theorem in fact strengthens the radical epistemic interpretation. On this reading the theorem constitutes the final nail in the coffin of the dated analogy between the probabilities of statistical mechanics and those of QM.

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[9] As noted, the analogy is incomplete for in the lack of supervenience the underlying assumption of the theorem allows more than statistical mechanics.

Spacetime and Non-locality. With this understanding of the evolution of the epistemic interpretation, it is now time to return to the question posed at the beginning: If the radical epistemic interpretation survives the no-go theorems known thus far, including the PBR theorem, does it mean that non-locality and no-signaling are independent of any conception of space and time and cannot therefore get us into conflict with STR? This was, as we remember the claim made by Pitowsky and Bub. To repeat their crucial point: "If one accepts that one is simply dealing with a different notion of probability, then all space-time considerations become irrelevant."

Bub and Pitowsky draw an analogy between the epistemic interpretation of QM and Minkowski's geometric formulation of STR.

> Hilbert space as a projective geometry (i.e., the subspace structure of Hilbert space) represents a non-Boolean event space, in which there are built in structural probabilistic constraints on correlations between events… just as in special relativity the geometry of Minkowski space-time represents spatio-temporal constraints on events.
>
> There is no deeper explanation for the quantum phenomena of interference and entanglement than that provided by the structure of Hilbert space, just as there is no deeper explanation for the relativistic phenomena of Lorentz contraction and time dilation than that provided by the structure of Minkowski space-time.

To the extent that the worry about nonlocality is only a worry about its explanation, the analogy with STR is indeed illuminating. When different interpretations of a theory are empirically equivalent, they can differ on what they consider to be the fundamental explanatory level without disagreeing on the empirical contents of the theory. Lorentz and Einstein could therefore agree on the relativistic effects of motion (length contraction in the direction of motion and time dilation) while disagreeing on its explanation or the need to identify the physical cause of these effects. In the case of nonlocality, however, the worry is not merely about the fundamental level of explanation, but also, and more significantly, about a potential tension between QM and STR. Such tension, if it exists, is not resolved as readily by the analogy between the probabilistic constraints of QM and the geometric constraints of STR. Bub and Pitowsky do not consider the possibility of such a tension. Rather than addressing the question of a possible conflict between QM and STR, they extend their analogy between the two theories to compare two *independen*t problems, the consistency of the formalism of STR, which is confirmed by its empirical applications and the analogous problem in QM. They say:

> Conditionalizing on a measurement outcome leads to a nonclassical updating of the credence function …This updating is consistent with a dynamical account of the correlations between micro and macro-events in a quantum measurement process. …This amounts to a consistency proof that, say, a Stern-Gerlach spin-measuring device or a bubble chamber behaves dynamically according to the kinematic constraints represented by the projective geometry of Hilbert space, as these constraints manifest themselves at the macrolevel. Similarly, the dynamical explanation of relativistic phenomena like Lorentz contraction in terms of forces, insofar as the forces are required to be Lorentz covariant, amounts to a consistency proof [of the kinematic structure].

This passage indicates two gaps in the analogy Bub and Pitowsky are drawing. First, the fact that the conditionalization procedure derived from the epistemic approach is consistent with the dynamical account of certain quantum processes does not show its consistency with (at least the spirit of) STR, which was the concern cited in the opening of this paper. There was no such consistency concern in the Lorentz-Einstein case because, while providing a novel kinematic-geometric structure, Einstein's predictions did not contradict those of Lorentz. The confidence that there is no conflict between QM and STR is based on the distinction between locality and no-signaling and on the assumption that STR condones nonlocal correlations as long as they do not allow signaling. Once this assumption is questioned, the worry about the tension between QM and STR reappears. This worry is not alleviated by Bub and Pitowsky. Second, according to this passage, the dynamical explanation of QM, just as the Lorentzian dynamical account, serves to establish the consistency of the formalism. Should we not also seek, by the same logic, a dynamical account of entanglement, that is, of the peculiar combination of nonlocality and no-signaling? Bub and Pitowsky think this is unnecessary. Their reason is that the Hilbert structure of quantum events, a structure in which this combination is embedded, is all we need. No further dynamical or causal explanation is necessary.

> Certain principles characterizing physical processes motivate the choice of Hilbert space as the representation space for the correlational structure of events, just as Einstein's principle of special relativity and the light postulate motivate the choice of Minkowski space-time as the representation space for the spatio-temporal structure of events.

The comparison is slightly misleading, however, for (unlike the case of STR) the goal of deriving the full-blown formal structure of QM from physical principles has not yet been achieved. (Bub and Pitowski presuppose the adequacy of the Hilbert space structure; they do not actually derive it from probabilistic considerations.) Remarkable progress has been made by Spekkens who, on the basis of purely information-theoretic constraints designed a toy model of QM. The basic principle of this toy model is the following: In a state of maximal knowledge, the amount of knowledge is equal to the amount of uncertainty, that is, the number of questions about the physical state of a system that can be answered is equal to the number of questions that receive no answer (Spekkens 2005). You will recall that this was precisely Schrödinger's intuition). Spekkens succeeds in deriving from this principle many of the characteristic features of QM, for example, interference, disturbance, the noncommutative nature of measurement and no cloning. And yet, Spekkens also notes that the model fails to reproduce the Kochen-Specker theorem and the violation of Bell-inequalities. In that sense it is still a toy model.

Suppose, however ,that the toy model is further developed into a full-scale model of QM. Should we then give up the quest for physical, rather than information- theoretic, principles that would underwrite quantum non-locality and make its compatibility with STR more palpable? Here we may have come to a philosophical dead end where arguments are scarce and each party sticks to its intuitions. I happen to believe (and am not alone, of course) that in physics, we should aspire to show how the logical-mathematical-probabilistic laws we take to be true are taken care of by physical principles. But I can see that this desideratum is contentious --the "no deeper explanation" alternative may also be viable. As long as the controversy is only about what constitutes explanatory force in physics, philosophical tolerance may perhaps be laudable. But when faced with the threat of tension between our best physical theories, there is no choice but dig into the assumptions on which they are based, both logical and physical assumptions. Clearly, the question about nonlocality is not merely about explanatory force but also about the potential tension with STR. To resolve this tension, the situation must be well formulated in the language of both of these theories and shown to be compatible with either one of them. And here, the fact that the peculiarities of entanglement can be described in terms of information-theoretic constraints is only half the story. The language of STR – the kinematic structure of Minkowski spacetime --is indispensable. There is no choice but bring entanglement back into space and time. The claim that "all space-time considerations become irrelevant" provides no answer to the problem of consistency.

In addition to these gaps in the epistemic interpretation, one should also consider its philosophical price. If quantum states represent states of knowledge or belief of a human observer, does it not make QM excessively anthropocentric? Christopher Fuchs is willing to pay this price.

> Quantum states are not something out there, in the external world, but instead are expressions of information. Before there were people using quantum theory as a branch of physics…there were no quantum states. The world may be full of stuff and things of all kinds, but among all the stuff and all the things, there is no unique, observer independent, quantum-state kind of stuff.

This would be a high price to pay not only for confirmed realists such as Einstein but also for the Copenhagen orthodoxy.

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