Can Quantum Mechanics be shown to be Incomplete in Principle?

Abstract:

The paper presents an argument for the incompleteness in principle of quantum mechanics. I introduce four principles (P0–P3) concerning the interpretation of probability, in general and in quantum mechanics, and argue that the defender of completeness must reject either P0 or all of P1–P3, which options both seem unacceptable. The problem is shown to be more fundamental than the measurement problem and to have implications for our understanding of quantum-mechanical contextuality.

1 Introduction

Einstein, as is known, held that quantum mechanics is an incomplete description of its domain of application. He contributed much to the research making his claim precise – in the EPR argument and beyond. Can quantum mechanics (QM) be regarded as incomplete? Today, the question is well researched and is answered as follows: Yes, it can be so regarded, but only with highly peculiar consequences. The completing structure of hidden variables cannot be one of context-independent variables and, for a system with space-like separated subsystems, must lead to nonlocal dependencies among the variables. But Einstein's original position is ill-captured if we portray it as a quest for the possibility to take QM as incomplete. Einstein did not, as if out of fondness for classical physics, *want* to regard the new theory as incomplete. Rather, he thought that it *had to* have this feature – because of its probabilistic structure. This intuition of a necessary or structural incompleteness of QM has received less attention in the literature than it deserves. In this paper, I want to explore it and transform it into a substantial argument.

My question is: *Can QM be shown to be incomplete in principle?* Unconditionally, the answer is probably no. QM has a widely accepted axiomatic basis that is sufficiently vague to be compatible with the standard expression of completeness. There are, however, natural ways to make one of the axioms precise – *principles* of interpreting probabilities in a physical theory – upon which QM can be shown to be in a conflict with that standard expression. Conditional upon these principles, I suggest, the title question of this paper must be answered in the affirmative. What this result shows is that the price for assuming completeness is far higher than we tend to think. Moreover, it has interesting implications for the kind of contextuality that we think is realized in QM.

The paper is structured as follows. I introduce, in sec. 2, the axioms of QM, in as neutral a fashion as possible, and a standard expression for completeness. I present, in sec. 3, four principles (P0–P3) ruling the interpretation of one of the axioms and, if necessary, motivate them. P0, the strongest principle, has the special status that it seems trivially true, but that, upon its rejection, we can assume QM to be complete. I discuss P1–P3 and show that to reject any one of them, though logically possible, has unacceptable consequences for QM. The reader is warned that this (still preparatory) section is long and patience-taxing. In sec. 4, I present the main argument and show that, given P0 and P1, QM and the completeness assumption (COMP), yield a contradiction. In sec. 5, I show that, given we reject P1, QM +P0 + COMP make it unintelligible how to maintain P2. In sec. 6, I show that, given we reject P1 and P2, QM + P0 + COMP make it problematic to maintain P3. The upshot is that, if we want to assume COMP, we either must reject P0 or all of P1–P3. Since the former is unacceptable we are left with rejecting the latter. But this seems likewise unacceptable. I suggest that COMP must fall, for principled reasons. In a concluding section (sec. 7), I sum up the argument and sketch implications for our understanding of contextuality.

I should have liked to support Einstein's intuition by a succinct formal argument. Instead, my argument is long, ramified, and often informal. I make a whole bunch of preparatory remarks and, beyond the main argument, explore more and more exotic possibilities. My apology is this. The main argument, in sec. 4, is indeed short and formal, but the crucial principle (P1) will inevitably seem naïve and invite objections. A near century of reflection on the riddles of QM has reshaped our conceptions of probability and measurement so as to make P1 seem not only naïve, but also expendable. I will show that this impression is mistaken, but to do so need supplementary arguments of a less formal character. As the decisive axiom of QM is open to so much interpretation, the formal argument cannot stand on its own.

2 Axioms of QM and the standard expression of completeness

QM, I assume, is minimally encoded in the uncontroversial portion of the usual set of axioms. In von Neumann's projection operator formalism they can be given as follows:

A1 Any QM system S is associated with a unique Hilbert space $\mathcal{H}$ and its state is represented by a unique density operator W (t) on $\mathcal{H}$, a function of time.

A2 Physical quantities $\boldsymbol{A}$, $\boldsymbol{B}$ …, (called observables) with values $\boldsymbol{a_1}, \boldsymbol{a_2}, \boldsymbol{a_3}\ldots, \boldsymbol{b_1}, \boldsymbol{b_2}, \boldsymbol{b_3}\ldots$ possibly pertaining to S, are represented by Hermitian operators **A, B** …, with eigenvalues $a_1, a_2, a_3\ldots, b_1, b_2, b_3\ldots$ on $\mathcal{H}$.

A3 S evolves in time according to $W(t) = U(t)\, W(t_0)\, U(t)^{-1}$ where $U(t) = \exp[-i\boldsymbol{H}t]$, a unitary operator, is a function of time and $\boldsymbol{H}$ is an operator representing the total energy of S.

A4 If S is in state W (t) and $\boldsymbol{A}$ is an observable on S, then the expectation value $\langle \boldsymbol{A} \rangle (t)$ is:

$$\langle \boldsymbol{A} \rangle (t) = \mathrm{Tr}\,(W(t)\, \boldsymbol{A})$$

A2 motivates an identification of physical observables and their mathematical representatives and I will not need to distinguish them. I will also, for simplicity, restrict myself to one discrete and nondegenerate observable $\boldsymbol{A}$ throughout. Finally, I will mostly restrict A4 to probabilities, i.e. expectation values of yes-no observables of type $\mathbf{P}_{\boldsymbol{a_i}}$ (where, as usual, $\mathbf{P}_{\boldsymbol{a_i}}$ is an operator projecting onto the ray containing $|\boldsymbol{a_i}\rangle$). Let $\boldsymbol{a_k}$ always be some fixed value of variable $\boldsymbol{a_i}$. Let '$[\boldsymbol{A}] = \boldsymbol{a_k}$' abbreviate the proposition that S has value $\boldsymbol{a_k}$ of $\boldsymbol{A}$ and let 'p (($[\boldsymbol{A}] = \boldsymbol{a_k}$)' mean the probability that $[\boldsymbol{A}] = \boldsymbol{a_k}$. Then, since $\langle \mathbf{P}_{\boldsymbol{ak}} \rangle (t) = p([\boldsymbol{A}] = \boldsymbol{a_k})$, A4 takes on a simpler, very familiar form:

A4′ If S is in state W (t) and $\boldsymbol{A}$ is an observable on S with eigenvalue $\boldsymbol{a_k}$, then the probability that S has $\boldsymbol{a_k}$ is: p ([$\boldsymbol{A}$] = $\boldsymbol{a_k}$) = Tr (W (t) $\mathbf{P}_{\boldsymbol{ak}}$).

(Variants of A4′ go by the names 'Born Rule' or 'Statistical Algorithm'.) A4 itself will play a role only in the discussion of principle P1 in the following section. In the technical argument, it will suffice to use the special case A4′. Note that the latter leaves implicit and thus unclear the role of parameter t on the left side of the equation. This defect will be remedied in one especially obvious way in sec. 4 and an alternative will be discussed in sec. 6.

For illustration of an assumption that is controversial as an axiom of QM, consider von Neumann's famous Projection Postulate: If S is found to have value $\boldsymbol{a_k}$ of $\boldsymbol{A}$ as a result of an $\boldsymbol{A}$ measurement, then S's state is $\mathbf{P}_{\boldsymbol{ak}}$ immediately after this measurement. Von Neumann's Postulate is motivated by a certain *interpretation* of QM, as encoded in A1–A4, and if we want distinguish the minimal axioms from all interpretive material, the Postulate clearly must be kept away. Indeed, if we confine ourselves to A1–A4, we can say that QM is minimally encoded in these latter axioms and that any interpretation of the physical theory flowing from A1–A4 is *an interpretation of QM*. Had we included the Projection Postulate we would have barred, e.g., the whole group of modal interpretations from being interpretations. Note also that A2 is deliberately vague such that a theory that prohibits all but one operator to represent a genuine physical quantity, i.e. Bohmian Mechanics, can count as an interpretation of QM. If we agree that A1–A4 encode QM in the sense of being necessary (but perhaps not sufficient) axioms for the full theory we can unambiguously say what an *interpretation* of QM can do. It can interpret the axioms or add new ones. Bringing in the Projection Postulate or assumption P0, below, would be such additions. The insistence of inserting the words 'upon a measurement of $\boldsymbol{A}$' into A4 would be such an interpretation, likewise the restriction on A2 in Bohmian Mechanics. An interpretation cannot, however, exchange one or more axioms from A1–A4 or non-trivially alter one of them as they stand, on pain of losing its status as an interpretation of QM.

Apart from one technicality it is only A4 and A4′ that will play an explicit role in what follows. The technicality concerns A2. Familiarly, the identification of observables and Hermitian operators is taken to imply that *co-measurable* observables are to be identified with *commuting* operators. I will use this idea (which is not strictly a consequence of A2, but motivated by it) only in the following, still preparatory section. Apart from this, A1–A3 will just have the function to illustrate that QM contains one unique and unambiguous parameter t. I will, in sec. 5, consider a modification of A4′ that introduces a second time-index and deny it the status of a mere interpretation on the grounds that it alters the uniqueness of the index in A1–A4, hence alters QM.

I now need an expression for the *completeness* of QM and start with the, again familiar, *eigenstate-eigenvalue link* (EE):[1]

EE [$\boldsymbol{A}$] = $\boldsymbol{a_k}$ (t) if and only if S is in state $\mathbf{P}_{\boldsymbol{ak}}$ (t).

For future reference, I split up both directions of EE into EE1 and EE2:

EE1 If S is in state $\mathbf{P}_{\boldsymbol{ak}}$ (t), then [$\boldsymbol{A}$] = $\boldsymbol{a_k}$ (t).

EE2 If [$\boldsymbol{A}$] = $\boldsymbol{a_k}$ (t), then S is in state $\mathbf{P}_{\boldsymbol{ak}}$ (t).

Arguably, EE2 can be viewed as a precise version of von Neumann's Postulate. A trivial consequence (but one that it will be good to have before our eyes, separately) is the following expression for completeness (COMP):

[1] For the expression and a clear formulation see Fine [1973], p. 20. Apparently, the first one to assume the link, though not explicitly, was von Neumann (see van Fraassen [1991], p. 247, Clifton [1995], p. 34, fn.1).

COMP If S is not in state $\mathbf{P}_{\boldsymbol{a}_k}$ (t), then not [$\boldsymbol{A}$] = $\boldsymbol{a}_k$ (t).

I will refer to the events denoted by '[$\boldsymbol{A}$] = $\boldsymbol{a}_k$' and '[$\boldsymbol{A}$] = $\boldsymbol{a}_k$ (t)', in A4′ and COMP as *QM events*. Whether or not the proposition denoting a QM event must carry a time-index will be a matter of extensive discussion. Note that our axioms and assumptions so far imply a double identification of QM events. Firstly, A4 and A4′ establish the link of QM and its empirical tests. The expectations and probabilities of A4 and A4′ are the predictions that can be confronted with our observations. For this to be possible, the propositions denoting QM events in A4 and A4′ must be of the same logical structure as propositions that report such observations and denote the *observed QM events*. Especially, a probability of the form 'p ([$\boldsymbol{A}$] = $\boldsymbol{a}_i$)' is tested by asserting different tokens of '[$\boldsymbol{A}$] = $\boldsymbol{a}_i$' or its negation and any alteration in the former will have to be matched in the latter, and vice versa. This identification of expressions for predicted and observed QM events is trivial and will be neither questioned nor argued further throughout the paper. Secondly, A4 and A4′ establish an indirect, i.e. probabilistic connection from QM states to QM events. Now, EE establishes a direct link from QM states to QM events and back and COMP establishes such a link from QM states to QM events. I here also assume that QM events are of the same kind throughout, i.e. that EE and its kin talk about the same kind of QM events that A4′ yields probabilities for. This identification *will* be questioned, albeit only hypothetically, in sec. 6.

In the next sections, I will sometimes have to differentiate values of parameter t in discussions about measurement. Then, t_1 always is the onset of measurement of S by means of an $\boldsymbol{A}$-measurement apparatus, t_2 the time when S possesses a value of $\boldsymbol{A}$, and t_3 the end of interaction.

3 Four principles and their motivation

I now introduce four principles [P0–P3]. They all concern the interpretation of probability in general or within QM, hence will influence eventual readings of A4′:

[P0] If a theory assigns an event a non-zero probability, then, given the theory's truth, this event is possible.

[P1] All statistical expressions in QM have their usual statistical meanings.

[P2] All events that are assigned probabilities in QM are explicitly temporal events.

[P3] QM probabilities can be defined by a standard probability calculus.

These principles need some motivation. Also, it will be necessary to reframe them in a more formalized manner or draw consequences from them (numbered 'P0–P3' without pointed brackets), for the arguments to come, and it will be illuminating to explore consequences of rejecting any one of them. I do all this at once, for each principle in turn.

[P0] will later be used assuming the weakest form of possibility: logical possibility. Thus, it is best reformulated as follows:

P0 If, for a proposition A (describing an event) a theory T yields another proposition p (A) > 0, then it is not the case that T, A $\vdash \bot$.

P0 seems beyond reasonable doubt, but it also follows from very natural assumptions about probability. Assume (P0 (a)) that contradictions have probability zero; (P0 (b)) the conditional probability formula: p (A $\wedge$ B) = p (A | B) p (B); (P0 (c)) that the probability space for the probabilities delivered by T can be expanded so that p (T) is well-defined; and (P0 (d)) that p (T) > 0. The non-trivial assumption is P0 (c). However, it can be made plausible for all major conceptions of probability. Consider probability being defined as a subjective degree of belief.[2] Then it is rational to define p (T | A) for a theory in order to be able to express that T's prediction A, if it comes out true, raises your degree of belief in it: p (T | A) > p (T). However, if p (T | A) is well-defined, then p (A) and p (T) are well-defined on the same space. Consider, alternatively, probability being defined via conditional probabilities understood as ratios of proportions of logically possible worlds. p (T) then can be defined as p (T | L) where L is a logical triviality and p (A | T) is defined, on the same space as p (T), as the ratio of the proportion of logically possible T-worlds where A is true to the proportion of logically possible T-worlds. Consider, finally, probability being defined as the limiting relative frequency of possible outcomes in a hypothetical infinite sequence of trials of an experiment. Let a trial of an 'experiment concerning T' be an explicit statement of T with possible 'outcomes' True (T = 1) and Not-true (T = 0). Then 'T = 1' is an outcome as the event reported by A. Thus, p (T) can be defined as p (T = 1) on a superspace of the probability space where p (A) lives. Given P0 (a–d), the argument for P0 is very simple. Assume, by P0 (c) and P0 (d), that p (T) > 0. Assume also that p (A | T) > 0. Then, by P0 (b), also p ($A \wedge T$) > 0, whence, by P0 (a), $A \wedge T$ is not a contradiction.

My next principle [P1] says that all statistical expressions in QM have their usual statistical meanings. Especially, the *expectation value* in A4 is defined like the *expected value* in statistics. As follows: Let E be a set of events, ΔE the set of probability weights on E, V a subset of the real numbers, and f a mapping f: $E \rightarrow V$. Then the expected value of f in a state $\omega \in \Delta E$ can be defined as: E (f, ω) = $\Sigma_{x\in E}$ f(x) ω(x). This definition has two elementary and important features: Firstly, every summand in E (f, ω) consists of two factors that are functions of the same event $x \in E$. Secondly, suppose that the $x \in E$ are events all pertaining to a certain time t. We will then interpret each x as x (t), an event happening at t and interpret f (x) as f (x (t)), the numerical value associated with x (t). Hence also E (f, ω, t) = $\Sigma_{x\in E}$ f (x (t)) ω(x (t)), for a time-dependent expected value. Every summand f (x (t)) ω(x (t)) in E(f, ω, t), for each x and fixed t, is the expected value of x (t) weighted by the probability for it and the sum E(f, ω, t) the expected value *simpliciter*. Do both features carry over into QM? Well, given [P1], they do. Firstly, the QM expectation values easily can be defined as special cases of the definition: For an observable $\boldsymbol{A}$ with eigenvalues $\boldsymbol{a_i}$, define ω($\boldsymbol{a_i}$) = Tr (W (t) $\mathbf{P}_{\boldsymbol{ai}}$) and $\boldsymbol{A} = \Sigma_i$ f ($\boldsymbol{a_i}$) $\mathbf{P}_{\boldsymbol{ai}}$, where f is the identity. Then $\langle \boldsymbol{A} \rangle$ (t) = Tr (W (t) $\boldsymbol{A}$) = Σ_i f ($\boldsymbol{a_i}$) ω($\boldsymbol{a_i}$).[3] What about the time-index in Σ_i f ($\boldsymbol{a_i}$) ω($\boldsymbol{a_i}$)? The $\boldsymbol{a_i}$ can be identified with our previously introduced QM events [$\boldsymbol{A}$]= $\boldsymbol{a_i}$. Adapting the second feature from the statistical expected value, they will be QM events happening at a time t, if such a time is introduced. Now, such a time is introduced because $\langle \boldsymbol{A} \rangle$ (t) is time-dependent in QM. Hence, explicitly: $\langle \boldsymbol{A} \rangle$ (t) = Σ_i f ($\boldsymbol{a_i}$ (t)) ω($\boldsymbol{a_i}$ (t)). But this yields an interesting consequence, using the identity of events that is now established for the summands in $\langle \boldsymbol{A} \rangle$ (t). ω($\boldsymbol{a_i}$) now is the probability that [$\boldsymbol{A}$]= $\boldsymbol{a_i}$ at t. Hence, also W (t) $\mathbf{P}_{\boldsymbol{ak}}$ in A4′ is the probability that S has $\boldsymbol{a_k}$ at t. A similar result follows, if we move to p ([$\boldsymbol{A}$] = $\boldsymbol{a_k}$) = $\langle \mathbf{P}_{\boldsymbol{ak}} \rangle$ (t) = Tr (W (t) $\mathbf{P}_{\boldsymbol{ak}}$). The latter expression, because it is a special expectation value equals Σ_i f($\boldsymbol{p_i}$ (t)) ω($\boldsymbol{p_i}$ (t)), where the $\boldsymbol{p_i}$ are the values of $\mathbf{P}_{\boldsymbol{ak}}$ with $\boldsymbol{p_i} = \boldsymbol{\delta_{ik}}$. Again, every summand f($\boldsymbol{p_i}$ (t)) ω($\boldsymbol{p_i}$ (t)), for each $\boldsymbol{p_i}$ and t, is the expected value of either [$\mathbf{P}_{\boldsymbol{ak}}$]= 1 at t or [$\mathbf{P}_{\boldsymbol{ak}}$] = 0 at t, weighted by the probability for it, and their sum is the expected value *simpliciter*. [P1], thus, in our context has an interesting consequence for QM probabilities in A4′:

P1 In A4′ QM probabilities, being of the form p ([$\boldsymbol{A}$] = $\boldsymbol{a_i}$) = Tr (W (t) $\mathbf{P}_{\boldsymbol{ai}}$), are to be interpreted as p ([$\boldsymbol{A}$] = $\boldsymbol{a_i}$ (t)) , i.e 'the probability that S has $\boldsymbol{a_i}$ of $\boldsymbol{A}$ at t'.

[2] For a meaningful integration of this conception of probability into QM see, e.g., Caves *et al.* [2002].

[3] I here follow Wilce's exposition (see [2006], sec. 3) of a standard approach to generalised probability theory due to Foulis and Randall. Note, however, that Wilce effectively defines f and ω on the domain $\mathcal{H}$ (adapted to the present notation: f: $\mathcal{H} \rightarrow V$, with f ($|\boldsymbol{a_i}\rangle$) = $\boldsymbol{a_i}$. and ω: $\mathcal{H} \rightarrow [0, 1]$ with ω ($|\boldsymbol{a_i}\rangle$) = Tr (W (t) $\mathbf{P}_{\boldsymbol{ai}}$). This means to identify the *events* in this generalised approach to probability with QM *states*. Given the standard interpretation of a random variable f (followed by Wilce), which has, as its domain, a set of outcomes of an experimental test, we would now have to identify QM events (the outcomes) and QM states, i.e. adopt EE. This must be avoided here in order not to beg any question concerning EE and its derivatives and it can be avoided by the simple changes proposed.

What about rejecting this principle P1? Well, first of all, it would necessitate a new definition of the QM expectation value. One of the mentioned statistical features would have to be changed explicitly. We would have to find a way to interpret the time-dependent QM expectation value not as a sum of factors, each of which is an event at t weighted by the probability for this event. Consider again $\langle \boldsymbol{A} \rangle$ (t) = Σ_i f ($\boldsymbol{a_i}$(t)) ω ($\boldsymbol{a_i}$(t)). Either the identity of $\boldsymbol{a_i}$(t) in each summand f ($\boldsymbol{a_i}$(t)) ω ($\boldsymbol{a_i}$(t)) would have to be broken or the time-index would have to be reinterpreted as no longer indexing events happening at t. Both options are unexplored in the literature and would call for drastic revisions of the statistical concepts of QM.

But there is more to be said about giving up P1. A little reflection shows that P1 encodes an assumption of faithful measurement – something we know to be in a strong tension with COMP.[4] Consider that A4′ is interpreted using P1. Then a state W (t) yields probabilities for QM events of type ‘[$\boldsymbol{A}$] = $\boldsymbol{a_i}$ (t)’. Now, if W (t_1) is S’s state at t_1, the onset time of an $\boldsymbol{A}$-measurement, then the events, for which A4′ encodes probabilities, all refer to t_1. Assuming the empirical testability of QM, as explained, the reports about observed QM events will have to do the same, which is to say that they report properties S has at t_1. Notwithstanding any reflection on measurement apparatus, this is a clear statement of faithful measurement. Given this fact, many interpreters will not accept P1. Von Neumann, in his classic 1932 treatise, rejects faithful measurement outright. This is clear when he says that W (t) does not encode the probabilities of what values different copies of S have, ‘but only with what probability they take on all possible values’.[5] This is a clear rejection of faithful measurement and, hence, an implicit one of P1, too. But the same goes for some interpreters who, in contrast with von Neumann, qualifiedly reject COMP, i.e. all who, like him, postulate a transition from possible to actual value upon measurement of a non-eigenstate of the measured observable.[6] As I just argued, there is a serious problem for this line of thought: Without P1, the notion of an expectation value loses its ordinary statistical meaning without acquiring another one. This may seem a tolerable lacuna, but we will see that there is a slippery slope from rejecting P1 to rejecting P2 and P3, which it seems far more outlandish to give up.

A second problem arises that is internal to QM. If P1 is given up, the time-index in W (t) does not transfer into the probabilities of type ‘p ([$\boldsymbol{A}$] = $\boldsymbol{a_i}$)’ to index the QM events of type ‘[$\boldsymbol{A}$] = $\boldsymbol{a_i}$’. Suppose now that we wish to describe the interaction of S and a measurement apparatus within QM. (The motivation for and technical execution of this idea are too well-known to repeat them here; see, e.g., Redhead [1987], pp. 53-54.) We let the interaction run from t_1 to t_3. Regardless of the exact nature of the state at t_3,[7] we will be able to calculate probabilities for the values of the pointer observable, say $\boldsymbol{K}$, at t_3. But these probabilities no longer will be for the event that the apparatus possesses value $\boldsymbol{k_i}$ of $\boldsymbol{K}$ at t_3, but just that it possesses $\boldsymbol{k_i}$ (at some time, within an interval?). This indefiniteness of our $\boldsymbol{K}$ predictions is highly dissatisfying, for we certainly test the $\boldsymbol{K}$ values (values of the pointer observable)

[4] See, e.g. Redhead [1987], p. 89 for the role of faithful measurement in the derivation of the Bell inequality.

[5] Von Neumann [1955], p. 206; see also ibid.211.

[6] Von Neumann’s idea of a transition of S, upon $\boldsymbol{A}$-measurement, from not having to having a value of $\boldsymbol{A}$ is certainly sanctioned by collapse interpretations and by an orthodox Copenhagen-style interpretation, if construed as in Bub [1999], pp. 187-190. For modal interpretations the situation is more complex. Although they all reject EE, many of them endorse von Neumann’s transition. The Bohm theory, if viewed as a modal interpretation with position as a fixed preferred always-determinate observable (see Bub [1999], pp. 163-173), does, of course, not assume any such transition for position. Those modal interpretations, however, that take the *measured* observable to be the preferred one either directly endorse the transition or leave room for assuming it. Thus, in van Fraassen’s version there is an explicit ‘transition from possible to actual value’ during measurement ([1991], p. 288). One may argue that, given van Fraassen’s constructive empiricism, we should stay entirely agnostic about the situation before measurement and not assume a *real* von Neumann-style transition occurring in S upon measurement, but there is at least room here for doing so. Modal interpretations with a more realist bend expressly assume the transition as, e.g., the Kochen-Dieks interpretation where ‘lots of observables can have definite values […], but when they do, those values will usually be *acquired* in an irreducibly stochastic way’ (Clifton [1995], p. 34; my italics).

[7] Regardless, that is, of whether the final compound state is pure or a mixture.

at definite times. So, if we want to include the apparatus in the QM description, ¬ P1 breaks the structural identity of predictions for pointer observable values and observation reports about such values that I took as unquestionable at the end of the previous section. So, ¬ P1 calls into question the empirical testability of QM apparatus predictions.

Now as we will see, the positive suggestion behind removing parameter t from 'S has $\boldsymbol{a_i}$' is that t indexes a disposition at the onset of a measurement interaction to display (i.e. have) $\boldsymbol{a_i}$ at some later time. Again, accounting within QM for the compound system of S and the apparatus imposes this reading also for the pointer observable, i.e. creates the latter's disposition at t_3, the end of measurement to display $\boldsymbol{k_i}$ at some later time. What we would thus need to repair the defect mentioned and evaluate $\boldsymbol{K}$ predictions empirically is additional information about this later time.

Let's move on to [P2]. Without any motivation, I concretize it as follows:

P2 Any expression of the form '[$\boldsymbol{A}$] = $\boldsymbol{a_i}$' in QM can be given a time index, i.e. there is a parameter t in the formalism of QM such that the expression is read as '[$\boldsymbol{A}$] = $\boldsymbol{a_i}$ (t)', i.e. 'S has $\boldsymbol{a_i}$ of $\boldsymbol{A}$ at t'.

If P2 is false, then measurements are not state preparations. This can be seen as follows. To keep the structural identity of predicted and observed QM events, the latter must, given ¬ P2, lose their index together with the former. Obviously, a preparation fixes a state vector that (due to A1 and for A3 to make sense) has a time-index, but without P2 an observed QM event, denoted by '[$\boldsymbol{A}$] = $\boldsymbol{a_i}$', will no longer bear one. First of all, EE2 cannot be put to use, because its antecedent will not be matched by an appropriate proposition. This seems a small wonder. If I want to position principles against COMP and if P2 is such a principle, then it will surely be in conflict not only with COMP, but also its converse, EE2. True enough. If there is a structural conflict between QM and COMP, then preparation just cannot go along the lines of EE2. But the problem here is more general. Given ¬ P2, even a more sophisticated prescription for state preparation will not be able to build upon measured values of an observable. What Redhead has called 'the state-preparation aspect' of measurement ([1987], p. 52) will not result from our ascertaining of a QM event.

This defect points toward a more fundamental problem. Assume plausibly that in a fundamental physical theory, we are able to model temporal events explicitly, i.e. to model events that happen at certain times with explicit reference to these times. Certainly, if one of the QM events, for which A4′ gives probabilities, really happens, then it *does* happen at a certain time, although ¬ P2 commands that this time must not have turned up within A4′. By these lights, QM is not a fundamental physical theory. This is certainly an unacceptable consequence. Shouldn't we be able to model the temporal QM events explicitly in QM? If yes, P2 must remain in force.

Finally, ¬ P2 aggravates the problem, mentioned in connection with ¬ P1, concerning a QM treatment of the measurement apparatus. Above we found that, given ¬ P1, the obvious time-index provided by the (now system-cum-apparatus) state W (t) will not carry over into the predictions for pointer observable $\boldsymbol{K}$'s values. So, the structural match between $\boldsymbol{K}$ value predictions and observations of such values becomes problematic. Upon ¬ P2, however, what we need – i.e. some time-index for the $\boldsymbol{K}$ predictions to match the observations – is explicitly denied us. As I took the structural identity to be non-negotiable, the result is that QM + ¬ P2 cannot deliver testable empirical predictions for apparatus observable $\boldsymbol{K}$. (The measurement problem – about which more below – is a quite different problem, since it is an incoherence of QM + COMP with $\boldsymbol{K}$ value observations, while ¬ P2 will be seen to be an attempt to first of all make COMP coherent with QM + P0.) I emphasise that this last problem only concerns the apparatus observable $\boldsymbol{K}$ that we test with the naked eye. It is consistent to maintain both that those QM events '[$\boldsymbol{A}$] = $\boldsymbol{a_i}$' of A4′ that refer to S do not themselves have a time-index *and* that propositions of predicted and observed QM events are structurally alike to guarantee empirical testability. Namely, we might assume that p ([$\boldsymbol{A}$] = $\boldsymbol{a_i}$) (with a reference to t_1 to be determined) is a disposition, at t_1, of S to take on $\boldsymbol{a_i}$ at some $t_2 \in [t_1, t_3]$ that must remain unknown. Observation of value $\boldsymbol{k_i}$ of $\boldsymbol{K}$ at t_3 will permit us to conclude the $\boldsymbol{A}$ value that S has adopted during $[t_1, t_3]$, but not necessarily the exact time when S had it. In this case, neither our predictions nor our (indirect) observations of S will be time-indexed. They will be structurally alike and testability of the A4′ predictions will be guaranteed. (A QM treatment of the apparatus though will be blocked, since pointer value predictions and observations are structurally different, as explained.) It is this, somewhat artificial, possibility to which [P3] and the whole argument of sec. 6 answer.

The final principle, [P3], says that QM probabilities can be defined by a standard probability calculus. In order to transform this idea into a concrete prescription, I have to rehearse some well-known facts about probabilities, in QM and in general. Usually, a probability space for QM system S is not constructed from the set of events referred to by the ‘$[\boldsymbol{A}] = \boldsymbol{a_i}$’, but more directly on the Hilbert space $\mathcal{H}$, from the set $S(\mathcal{H})$ of closed subspaces of $\mathcal{H}$. This construction clearly exposes the non-Boolean structure of $S(\mathcal{H})$ and hence the non-classical structure of the space $\mathcal{H}$ itself and of QM, in general. Recall how we go about this construction. Using EE1, we can assume that every closed subspace of $\mathcal{H}$ (e.g., the space L_{ak} onto which $\mathbf{P}_{\boldsymbol{ak}}$ projects), identifies a possible QM event on S (i.e. that S has $\boldsymbol{a_k}$). We can thus try to define a probability function $\mathcal{P}$ from the set $S(\mathcal{H})$ of all closed subspaces of $\mathcal{H}$ into [0, 1]. We find that, for a given state W (t) and a given observable $\boldsymbol{A}$, there is exactly one function $\mathcal{P}$ such that it is a probability measure (i.e. obeys the Kolmogorov probability axioms) and such that $\mathcal{P}$ $(L_{ai}) = \mathrm{Tr}\,(W\,(t)\,\mathbf{P}_{\boldsymbol{ai}})$, for all $\boldsymbol{a_i}$ of $\boldsymbol{A}$. This suggests that $\mathcal{P}$ and the function p in A4′ are identical and justifies *ex post* our use of EE1. However, we also find that $S(\mathcal{H})$ does not form a Boolean algebra. Accordingly, $\mathcal{P}$ cannot be defined as a Kolmogorov probability function (i.e. cannot obey the axioms) on the whole set $S(\mathcal{H})$ without qualification, but there must be defined as some kind of generalization. A standard definition of such a generalized probability function is given by Hughes ([1989], pp. 220-222), following Hardegree and Frazer ([1981]). This definition fixes the sum of two probabilities only for mutually orthogonal subspaces, i.e. leaves the sum of probabilities undefined for QM events corresponding to observables that are not co-measurable. Note, however, two facts: In A4′, probabilities for a combination of QM events corresponding to two non-commuting observables cannot be produced, because the choice of a unique observable is prescribed in the conditional clause. So, the possibly undefined combinations of QM events cannot arise, if the axioms of QM include A4 or an equivalent. Also, note the following mathematical fact about the function $\mathcal{P} : S(\mathcal{H}) \rightarrow [0, 1]$. For observable $\boldsymbol{A}$, let $S(\boldsymbol{A}) \subset S(\mathcal{H})$ be the set of subspaces each spanned by one of $\boldsymbol{A}$’s eigenstates, then the restriction of $\mathcal{P}$ to $S(\boldsymbol{A})$, $\mathcal{P}\,|_{S(\boldsymbol{A})}$, is a Kolmogorov probability function (see Hughes [1989], pp. 222-223).

These last two observations highlight that the non-Boolean structure of $S(\mathcal{H})$ and hence the non-Kolmogorovian structure of $\mathcal{P}$ cannot be turned against assuming that the function p in A4′ obeys the Kolmogorov axioms and can be identified with $\mathcal{P}\,|_{S(\boldsymbol{A})}$. To the contrary: Since the established general construction of $\mathcal{P}$ entails that its restriction $\mathcal{P}\,|_{S(\boldsymbol{A})}$ obeys the axioms, we have an argument saying that the probabilities in A4′ must obey them.

Consider, as a second familiar fact, the treatment of conditional probabilities in QM. We can ask the question what the probability is of one QM event conditional on another. Again, the standard answer uses the set $S(\mathcal{H})$ and a possible justification, again, would be that, by EE1, any one-dimensional element of $S(\mathcal{H})$ represents a QM event. The standard answer then goes thus: Notice initially that, when we allow for degenerate observables, the notion of a QM event generalises such that to any element of $S(\mathcal{H})$ there corresponds one QM event. Now, let L_A and L_B be any two elements of $S(\mathcal{H})$, with associated projection operators $\mathbf{P}_{\boldsymbol{A}}$ and $\mathbf{P}_{\boldsymbol{B}}$, and define, for a given state W (t), the function P: $S(\mathcal{H}) \times S(\mathcal{H}) \rightarrow [0, 1]$ by $P(L_A \mid L_B) = \mathrm{Tr}\,(\mathbf{P}_{\boldsymbol{B}}\,W\,(t)\,\mathbf{P}_{\boldsymbol{B}}\,\mathbf{P}_{\boldsymbol{A}}) \,/\, \mathrm{Tr}\,(W\,(t)\,\mathbf{P}_{\boldsymbol{B}})$. The claim that this function properly generalizes classical conditional probability can be argued by a special case: If $\mathbf{P}_{\boldsymbol{A}}$ and $\mathbf{P}_{\boldsymbol{B}}$ represent co-measurable observables (i.e. commute), then P reduces to classical conditonalisation. Again, the details are presented by Hughes ([1989] pp. 223-226, this time drawing on Bub [1977]), who concludes his account with the following remark: ‘…nothing in this discussion of quantum conditionalization bears directly on the question of whether the expression ‘p $([\boldsymbol{A}] = \boldsymbol{a_k})$’ should itself be regarded as a conditional probability’ (ibid. 226, notation adapted).

Finally, recall a bifurcation in the history of approaches to conditional probability. The majority tradition introduces a probability as a function obeying the Kolmogorov axioms and then defines a conditional probability via p (A | B) = p (A ∧ B) / p (B) (for p (B) ≠ 0). There is another tradition, probably founded by Popper, that regards conditional probabilities as fundamental and constructs unconditional probabilities as special cases. I will call these approaches Popper-style approaches and, to characterize them, rely on an observation of van Fraassen's on the literature on two-place ('irreducible conditional') probability. All important approaches to date, says van Fraassen, share two definitive features that can be used to define a two-place probability measure:[8]

Let R = <U, F> be a pair such that U is a non-empty set and F is a sigma-algebra on U. Then a two-place probability measure p (…|…) on R is a function from F × F into the real numbers such that:

1. For any A in F, the function p (…|A) is either a one-place probability measure on R or else has constant value = 1 ('reduction axiom').
2. For all A, B, C in F: p (B ∩ C | A) = p (B | A) p (C | B ∩ A) ('multiplication axiom').

A one-place probability measure then can be defined, Kolmogorov-style, by the familiar three axioms (which is how van Fraassen does it; see ibid.) or the Kolmogorov properties can be derived from other axioms (which is how Popper himself does it; see Popper and Miller [1994]). Identifying intersection among subsets of F with conjunction of events (sets of elements of U) as usual, the multiplication axiom becomes: For all A, B, C in F: p (B ∧ C | A) = p (B | A) p (C | B ∧ A). We then see immediately how this axiom and the Kolmogorovian definition p (A | B) = p (A ∧ B) / p (B) (of which the above P0 (a) is a trivial transformation) hang together: Each is a trivial theorem in the approach not containing it as an axiom or definition. There are thus two established ways to define a conditional probability from a set of axioms: Use either the Kolmogorov axioms or a set of axioms including the multiplication axiom. The same goes for unconditional probability. It can be either defined directly, Kolmogorov-style, or indirectly, as a limiting case of a Popper-style axiom set. Again, a definition will presuppose either the Kolmogorov axioms or a set of axioms including the multiplication axiom.

Consider now the question whether QM probabilities obey the probability axioms. The question depends on how we interpret them. If they are fundamentally unconditional they must directly obey the Kolomogorov axioms. If so, it is trivial that they indirectly obey the axioms of a Popper-style system, including the multiplication axiom. Vice versa, if the QM probabilities are fundamentally conditional, they must directly obey a Popper-style system, including the multiplication axiom and, as a trivial consequence, will indirectly obey the Kolmogorov axioms.

Now, to prepare for an argument given in sec. 6, consider the question whether QM probabilities are fundamentally *conditional* probabilities or not. As Hughes has pointed out, this question is independent of the general task of constructing conditional probabilities on $S(\mathcal{H})$.[9] Consider, hypothetically, that we have reason to regard QM probabilities as fundamentally *unconditional*. In this case, we cannot directly check whether they obey the Popper axioms, notably the multiplication axiom. We must instead check the Kolmogorov axioms. Hypothesise, by contrast, that we have reason to take QM probabilities as fundamentally *conditional*. Then we cannot directly check whether they obey the Kolmogorov axioms, but must check the Popper axioms. This observation leads me to specify [P3] as:

[8] See van Fraassen [1995] pp. 352-354 for the definition and its discussion. Van Fraassen lists Popper, de Finetti, Carnap, Reichenbach, Renyi, Harper, Field, and himself as adherents of this tradition (ibid. pp.371-372).

[9] Note also that one can accept the plausible idea that all probabilities are conditional on certain preconditions (Hájek [2003] makes a splendid case for the tradition advocating this idea) and still be undecided about this question. Namely, one might think that all relevant QM preconditions (our previous knowledge about S encoded in its density operator plus the observable we have decided to measure on S) are mentioned in the conditional clause of A4′, so that function p in its main clause can technically be viewed as an unconditional probability. Or one could think that, for some reason, not all preconditions are so mentioned, and that function p must fundamentally be a conditional probability. This latter line of thought will be explored in sec. 6.

P3 QM probabilities either (if they are unconditional probabilities) directly obey the Kolmogorov axioms or (if they are conditional probabilities) directly obey the Popper axioms.

Note (once more) that we need not make provision for the generalized probability $\mathcal{P}$ on S($\mathcal{H}$). If we want to interpret the function $\mathcal{P}$ as a probability, then, because of the necessity to pick one observable ***A*** in A4′, we are restricted to the set S(***A***) and $\mathcal{P}\,|_{S(\boldsymbol{A})}$ is Kolmogorovian. Whether this latter structure is derived from conditional probabilities is a separate question. I take it that the Kolmogorov and Popper axioms are standard calculi. I see no other approaches on offer, and certainly no third approach has become standard during the last century. If these two approaches are accepted as the standard ones, P3 follows from [P3].

Suppose, finally, that one rejected P3. This would not only mean to invoke another definition of QM probability (one that, I take it, does not yet exist). It would also create a consistency problem. If one rejects P3 one denies that p in A4′ is Kolmogorovian (because it obeys either set of axioms). But for any $\mathcal{H}$ a $\mathcal{P}$ can be constructed such that $\mathcal{P}\,|_{S(\boldsymbol{A})}$ is Kolmogorovian. Hence, one must deny the identity of p in A4′ and the function $\mathcal{P}\,|_{S(\boldsymbol{A})}$ which means to deny that the function $\mathcal{P}$ is a generalized probability function. Again, like in the cases of P1 and P2, denying principle P3 has dramatic consequences for QM, indeed calls for transformation of the theory, as we know it, into something else that has yet to be developed. As I will now show, given P0, one must deny all of P1–P3 to maintain COMP.

4 The main argument: QM + P0 + COMP → ¬ P1

In an important 1991 paper, Halpin has argued as follows. No interpretation of QM, assuming it to be complete, can interpret the probabilities as unconditional, since ‘all interesting physical quantities … have no values until measured.’ Presupposing COMP, this is indeed true for the values of observable ***A*** when a system S is not in an eigenstate of ***A***. Halpin argues that ‘… it would be wrong to assign nonzero probability to something which is certainly false.’[10] Here, Halpin presupposes something like P0 as a trivially true principle, and from this platform argues that QM probabilities should standardly be interpreted as conditional upon measurement. Namely, to ’assign nonzero probability to something which is certainly false’ comes to a violation of P0. Violating P0 ‘would be wrong’, i.e. is no rational option in the interpretation of QM. But Halpin does not point out that respecting P0 produces a conflict of QM and COMP, given that a second principle is adopted: P1.

Initially note that, given P1, the Born Rule A4′ can be rendered more exactly:

A4″ If S is in state W (t) and ***A*** is an observable on S with eigenvalue $\boldsymbol{a_k}$, then the probability that S has $\boldsymbol{a_k}$ at t is: $\quad$ p ([***A***] = $\boldsymbol{a_k}$ (t)) = Tr (W (t) $\mathbf{P}_{\boldsymbol{ak}}$).

Now suppose (1) that S is in a state W (t_1) ≠ $\mathbf{P}_{\boldsymbol{ak}}$ (t_1), for some value t_1 of t, such that from A4″ it follows that 1 > p ([***A***] = $\boldsymbol{a_k}$ (t_1)) > 0. Assuming that a theory contains all its consequences, QM + P1 will contain A4″. Now, let QM + P1, COMP, and (1) be integrated into one theory, QM′. The argument then is simple:

1	[1]	S is in state W (t_1).	(1)
1, A4″	[2]	p ([***A***] = $\boldsymbol{a_k}$ (t_1)) > 0.	[1], (A4″)
1	[3]	¬ (S is in state $\mathbf{P}_{\boldsymbol{ak}}$ (t_1)).	(1)
1, COMP	[4]	¬ ([***A***] = $\boldsymbol{a_k}$ (t_1)).	[3], (COMP)

By assumption, A4″, COMP, (1), are members of QM′ which thus entails both line [2], i.e. that a certain proposition is assigned a positive probability, and line [4], i.e. that the negation of that proposition is true. Hence, QM′ entails p ([***A***] = $\boldsymbol{a_k}$ (t_1)) > 0, but also: QM′, [***A***] = $\boldsymbol{a_k}$ at t_1 $\vdash \bot$, in

[10] Halpin [1991], p. 37. Halpin does not here explicitly assume QM to be complete, but presupposes “the received view of QM, the Copenhagen interpretation”, which obviously implies COMP.

contradiction with P0. Thus given P0, QM′ cannot be true. (Alternatively, given P0 (a–c), QM′ cannot have a positive probability of being true.)

The argument presupposes that QM′, the integration of QM, P1 and assumption (1) is a *theory*. Is the integration of (1) an innocuous step? Of course, we can add suitable propositions to QM to create a theory that contradicts virtually any other proposition. But (1) is a trivially admissible state assignment that QM must be consistent with. So, the integration of (1) into QM′ is innocuous indeed, but the one of P1 is not. A4″, COMP, (1) are in conflict with P0, where A4″ is a direct consequence of A4 and P1. P0 itself follows from assumptions about probability (i.e., P0 (a–d)) that stay unaffected when we distinguish quantum from classical probability, hence is immune to rejection. Similarly (1), a trivially admissible state assignment, is not negotiable, but P1 and COMP are. Given P0, either COMP is false and QM is incomplete or P1 is false.

5 First supplementary argument: QM + P0 + COMP + ¬ P1 make P2 implausible

Consider now that we reject P1, in order to keep QM compatible with COMP. We will then need an interpretation of A4′ that (a) uses propositions of type ‘$[\boldsymbol{A}] = \boldsymbol{a_k}$’ without *that* time-index t that is referred to in W (t), to avoid the above contradiction, and (b) assign a new place to it on the left side of the equation in A4″. There is an obvious way to do both, namely to keep the original time-index on the left side of the equation, but identify it with the time of measurement, i.e. the time of the onset of the measurement interaction. Halpin, as I said, has urged (presupposing, of course, COMP) that QM probabilities throughout must be interpreted as conditional upon measurement – apparently because he anticipated the above easy argument. The idea is widely spread among interpreters and Halpin claims no originality for his proposal. His merits lie elsewhere and will be discussed below.

Halpin’s proposal creates a new possibility for locating the time-index: the onset of measurement. It is the only locus in view.[11] (At this point, my argument cannot progress as rigidly as before, because it is impossible to prove or argue conclusively for Halpin’s alternative possibility as the *only one* remaining. I must *assume* that, apart from interpreting the probabilities as unconditional and as conditional upon measurement, there is no further way to do it. And I must *assume* that this latter possibility creates the only alternative for locating the time-index.) We should not, however, prejudge whether the Born probabilities are further construed as conditional probabilities or probabilities of conditionals, or something else. The possibilities will be explored below.

What happens, if we take the A4 probabilities as conditional upon measurement and let the time-index refer to the onset of measurement? If we strip the ‘$[\boldsymbol{A}] = \boldsymbol{a_k}$’ propositions of their time-index (to meet (a)), do we have a second one available for them? The plain answer is no. As witnessed by A1 and A3, QM just does not provide states with two time-indices to feed into A4. Even setting aside the physical meaningfulness of a state with two time-indices, introducing such states would constitute a tampering with the axioms in a fundamental respect. Given the strictures on interpretation imposed in sec.2, we would no longer be interpreting QM as introduced by A1–A4. So, given that we want QM *as introduced* to be compatible with both COMP and P0 and therefore reject P1, we must assume that propositions of type ‘$[\boldsymbol{A}] = \boldsymbol{a_k}$’ do not bear a time-index at all, which implies that, next to P1, also P2 is rejected.
I should emphasise that neither the contradiction in sec. 4, nor the problem presently discussed, are versions of the measurement problem. To set up the latter, we surely need COMP. Presently we explore a more fundamental tension of COMP and the axioms of QM. Their conjunction is inconsistent, given the trivial P0 and the reasonable P1. The problem is rooted in an ambiguity in A4′, which is resolved by P1 in the most natural way. Now to formulate the measurement problem, we

[11] Anticipating the formalization of sec.6, we can write ‘p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ (…) given that $M_{\boldsymbol{A}}$)’ or ‘p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}$ (…))’, where the blanks indicate possible argument places for t. One might, thinking of QM probabilities as dispositions, propose the tagging for time of the probability itself, i.e. ‘p (…) ($[\boldsymbol{A}] = \boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}$)’, but this does not create a third possibility, since S surely must possess the disposition at the onset of measurement, hence at $M_{\boldsymbol{A}}$.

must use some form of QM besides COMP. Presumably, we use the axioms, thus have some interpretation of them. Ideally, we should be in possession of an *unambiguous* interpretation of all the axioms. But we have spotted an ambiguity in one axiom (the role of the time-index in A4′), we have attempted to disambiguate it in a straightforward way (i.e. using P1), and have contracted a consistency problem with COMP and P0. This problem exists before we can even begin to formulate the measurement problem, where we would be using both the disambiguated axioms and COMP. The same goes for P2. If we let go of P1 we must also abandon P2, and we must do this to keep QM consistent with COMP and P0 – still before we can begin to think about a measurement problem. This latter problem takes the axioms and COMP for granted, expands the QM representation to the compound system of object and apparatus, models an ideal measurement and shows that the reduced final state of the apparatus is not such that, given COMP, the pointer shows a result. This is not an internal inconsistency of QM + COMP, but a conflict of them with what we observe in QM measurement apparatus. Indeed, the whole exercise of formulating the measurement problem would be quite pointless, if the axioms and COMP were inconsistent from the start. The right diagnosis, thus, is that when setting up the measurement problem we tacitly *disambiguate A4′ not via P1*, but in another way. The measurement problem then informs us that COMP still engenders problems, this time no internal inconsistencies, but contradictions with experience.

I must elaborate on what I mean by disambiguating A4′ not via P1. The probabilities encoded in state W (t_1) are generally understood as dispositions of S *at* t_1 to display a property, say $\boldsymbol{a_k}$. Let's follow this line of thought. Let's again assume that the state entails $0 < p ([\boldsymbol{A}] = \boldsymbol{a_k}) < 1$. In this case, the disposition picture means that the eventual display or actualization of QM event '[$\boldsymbol{A}$] = $\boldsymbol{a_k}$' happens at a time later than t_1. (This is straightforward from the idea that a nontrivial disposition can be actualized or not. For this to be consistently possible, the disposition itself and its possible (non-)actualization must refer to different times.) This, in turn, means that '[$\boldsymbol{A}$] = $\boldsymbol{a_k}$' is not indexed by t_1, the index from state W (t_1). P1 thus is tacitly rejected and my argument from the previous section does not get off the ground. Also, this disposition picture allows us to assume that A4′ and COMP are not, by themselves, inconsistent. Now we can start to reason about the QM of measurement devices and develop the measurement problem.

However, by the previous arguments and the one to come, I mean to show that the disposition picture cannot work. Abandoning P1 is in itself problematic and leads to giving up other reasonable principles, like P2. I emphasise that it is one thing to interpret the probabilities calculated from W (t_1) as dispositions, but quite another thing to interpret them as dispositions possessed at t_1. It's this latter line of thought I object, not the former. Of course, the disposition picture is motivated by the weighty mathematical arguments for the completeness of QM. But the price to be paid is much higher than we usually think. First of all, an interpretation rejecting P1 incurs a high debt in itself: The notion of a QM expectation value must be re-defined in a way distinct from the statistical one. A QM expectation value can no longer be the sum of values at t_1, weighted with the probabilities for having these values at t_1. More seriously, there is a problem about those QM events relating to pointer observable $\boldsymbol{K}$. Given an interaction within [t_1, t_3], W (t_3) will yield K value predictions that we would like to interpret as predictions for pointer positions at t_3. But this is denied us by $\neg$ P1. We are left with temporally unspecified apparatus predictions and hence an unclear connection with apparatus observations. Now, $\neg$ P1 implies $\neg$ P2 and still worse consequences: Measurements, as a matter of principle, not practice, are no preparations. Measurements never ascertain full-fledged events as we know them in physics, i.e. events that *explicitly* happen at definite times. Finally, predictions about $\boldsymbol{K}$ values are non-time-indexed, hence are in conflict with the time-indexed observations of pointer values we can produce.

These difficulties certainly weigh enough to make any interpretation along the lines of $\neg$ P2 highly unattractive. However, it is not altogether nonsensical. One may think that COMP is so well-confirmed as to motivate departing from P2, despite the ugly consequences. We could think of the A4′ probabilities as referring to a time t_1 where S has a disposition or propensity for displaying a value at some time t_2 that must remain unmentioned in QM – except that we assume $t_2 \in [t_1, t_3]$. (Perhaps, the well-known Ghirardi-Rimini-Weber model of wave-function collapse by means of a non-unitary mechanism can be cast in this mould. I shall not pursue this further.) In this case, we still get a version of A4′ with meaningful, i.e. empirically testable probabilities for values of S: Reading the $\boldsymbol{A}$-

measurement apparatus pointer value at t_3 in any single case allows to infer S's value of ***A***, but not the exact time at which S possesses it.[12] Frequencies of pointer values can test the probabilities despite the missing time reference, since the latter are now read as dispositions at t_1 to manifest values of ***A*** at some unspecified later time. (Recall, however, from sec. 3 that this interpretation cannot be expanded to a system including the apparatus, given my structural identity prescription and our time-indexed ***K*** value observations.). So, ¬ P2 seems to leave us with a consistent, even if unattractive, way to integrate COMP with QM and P0.

6 Second supplementary argument: QM + P0 + COMP + ¬ P1 + ¬ P2 make P3 implausible

For any inhomogeneous mathematical equation, a minimal syntactical requirement is that a parameter (like t) can appear only if it could explicitly appear *on both sides*. A4, in this sense, was explicit, but the role of the time-index was not altogether clear,[13] and I purposely left it unclear where in A4′, on the left side of its equation, t has a place. The ambiguity in A4′ is most naturally resolved in A4″, which led to the main argument against COMP. Rejecting P1, and hence A4″, we must find a new way to disambiguate A4′. As I have argued, there is but one ersatz position for the index: the onset of measurement on which QM events are taken to be conditional. Since importing a second time-index into QM is out of the question, we must now take the conditioned QM events to bear no time reference, at all, i.e. must also reject P2. But relocating the index to refer to the onset of measurement is a new way to disambiguate A4′ that we must discuss. Writing '$M_{\boldsymbol{A}}$ (t)' for 'S is measured for A at t', the disambiguation can be framed thus:

A4‴ If S is in state W (t) and ***A*** is an observable on S with eigenvalue $\boldsymbol{a_k}$, then the probability that S has $\boldsymbol{a_k}$ is: $p([\boldsymbol{A}] = \boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}(t)) = \mathrm{Tr}(W(t)\,\mathbf{P}_{\boldsymbol{a}k})$.

Among interpreters of QM, it is a commonplace that the probabilities it generates are conditional on suitable measurements. But A4‴, as all versions of A4, is a conditional itself, and one might argue that its antecedent, by mentioning an observable ***A***, tacitly refers to an appropriate procedure for measuring ***A***'s value and thus incorporates the condition of a suitable measurement. This could be taken to show that despite the general condition, QM probabilities need not be explicitly conditional: A general conditionalising on measurement serves no perceptible technical purpose (in contrast with the structure of conditional probabilities on S($\mathcal{H}$) that I sketched in sec. 3). In A4‴ however, the conditionalising measurement has the important function to absorb the time-index that we need on the left side, but can no longer attach to the QM event itself. Hence, the condition must now be made explicit *within the equation* of A4‴.

This conditionalising does not imply that the expression 'p ($\boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}$ (t))' automatically turns into a conditional probability. Halpin, in the mentioned 1991 paper, has done us the important service of investigating the different possibilities for its logical analysis. He collects three options that are on offer in the literature, finds them wanting and advocates a fourth one. (Since the list of possibilities is open, here the argument can again not be rigorous.) I adapt Halpin's list to the present case, where we read the expression 'p ([***A***] = $\boldsymbol{a_k}$)' in (A4′) as 'the probability that S has $\boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}$ (t)'. As usual, I write 'p (…|…)' for a conditional probability and '>' for the connective 'if …then…', forming a conditional (that can be indicative or subjunctive). Following Halpin, I write '$>^p$' for 'if … then with probability p …', the probabilistic conditional. Halpin's list (including as (d) his own proposal; see his [1991] pp. 43, 54-55) now proposes four possible meanings for 'p ([***A***] = $\boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}$ (t))' in A4‴:

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[12] There is an operationalist position (does any real interpreter hold it?) that would take QM to be only about pointer-reading predictions and would thus prohibit inferences from the ***K*** value in the apparatus to the ***A*** value in S. However, this is an uninteresting option in the context of defending COMP with reference to S. Moreover, this position would be left with the problem of unindexed ***K*** predictions vs. indexed ***K*** observations.

[13] Recall my discussion, in sec. 3, of parameter t's role in a classical expected value and in a QM expectation value. If the latter are identical in meaning with the former, then t in <***A***> (t) of A4 has a clear role, i.e. $\langle \boldsymbol{A} \rangle (t) = \Sigma_i\, f(\boldsymbol{a_i}(t))\, \omega(\boldsymbol{a_i}(t)) = \mathrm{Tr}(W(t)\,\boldsymbol{A})$. If there is no such identity, the role remains unclear.

(a) ‘p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ | $M_{\boldsymbol{A}}$ (t))’, such that it is a conditional probability;
(b) ‘p ($M_{\boldsymbol{A}}$ (t) > $[\boldsymbol{A}] = \boldsymbol{a_k}$)’, such that it is the probability of a conditional;
(c) ‘$M_{\boldsymbol{A}}$ (t) > p ($[\boldsymbol{A}] = \boldsymbol{a_k}$)’, such that it is a conditional with a probabilistic consequent;
(d) ‘$M_{\boldsymbol{A}}$ (t) $>^p$ $[\boldsymbol{A}] = \boldsymbol{a_k}$’, such that it is a probabilistic conditional;

Halpin offers arguments against one reading of (a) as well as against (b) and (c) and then advocates his own proposal (d). I will briefly sketch his arguments, but offer an additional one for each case from the present, special context: A4′′′ is formulated to save COMP and it is asked whether P3 can be maintained. Indeed, I will check for each of Halpin’s proposals whether it meets P3 (allows for a definition by an established calculus), whether it secures a clear position for the time-index on the left side of the equation in A4′′′, and whether it saves COMP, the assumption about QM that A4′′′ is meant to save. None of the proposals, it will turn out can meet all conditions. (This does not tell against Halpin’s very convincing case for (d), which is of course made outside the context of A4′′′ with its special restrictions on t.)

6.1 Proposal (a) contradicts P3

Consider, firstly, proposal (a), where the conditional probabilities are defined from unconditional ones in the familiar way: p (a | b) := p (a ∧ b) / p (b). In the present context, event b is a measurement, e.g. $M_{\boldsymbol{A}}$ (t). Halpin quotes an argument by van Fraassen and Hooker [1976] against this analysis. Since the set of possible measurements in typical cases is nondenumerable, probability theory cannot guarantee for every member M_A of the set that p($M_{\boldsymbol{A}}$ (t)) differs from zero. There are cases that necessarily remain undefined, while A4′ generates probabilities for them. But there is a more fundamental problem. Kolmogorov’s definition *presupposes* that there are unconditional probabilities of the form ‘p ($M_{\boldsymbol{A}}$ (t))’, defined by his familiar axioms, from which we can define the conditional ones. But where should we get them in our context? A4 and its variants concern all the probabilities there are in QM and we presently consider that all these are interpreted as conditional probabilities. There simply are no others inside QM and importing them from elsewhere is, in a fundamental physical theory, not a reasonable option. If one took each of the p ($M_{\boldsymbol{A}}$ (t)) to be itself conditional on some super-measurement (to make it subject to A4′′′) this would create an obvious regress problem. (Both these points are argued by Hájek [2003], pp.306-307.) So, the Kolmogorov definition is simply inapplicable.

A Popper-style definition of conditional probabilities as primitives thus is the only way of realizing proposal (a). However, in our context the proposal runs into technical difficulties. Given A4′′′, the requisite space of events contains two distinct classes of events, non-time-indexed and time-indexed ones. If p is a two-place function from pairs of events into the real numbers, then A4′′′ licenses values of p if and only if its second entry, but not the first, is time-indexed. A4′′′ gives all the QM probabilities there are, answers all admissible questions about them. Of course, A4′′′ cannot deliver probabilities of the following form: p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ | $[\boldsymbol{A}] = \boldsymbol{a_j}$), p ($M_{\boldsymbol{B}}$ (t)| $M_{\boldsymbol{A}}$ (t′)), but neither can it deliver mixed probabilities like: p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ | $[\boldsymbol{A}] = \boldsymbol{a_j}$ ∧ $M_{\boldsymbol{A}}$ (t)) and p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ ∧ $M_{\boldsymbol{A}}$ (t))| $M_{\boldsymbol{B}}$ (t′)). (A4′′′) simply does not know how to treat these expressions. This restriction makes it impossible to formulate probability axioms which the QM probabilities fulfil.

We have done the required preparations above. All established axiomatic approaches to conditional probability start from a two-place function p: and share the multiplication axiom: For all a, b, c in F: p (b ∧ c | a) = p (b | a) p (c | b ∧ a). The QM probabilities, construed as in (A4′′′), cannot obey this axiom. The reason is, of course, the strict separation of non-time-indexed and time-indexed events as elements of the sets for the first and second entry, respectively, in p (…|…). Given that b is time-indexed, (A4′′′) does not define p (b ∧ c | a) and p (b | a). Given that b is not so indexed, (A4′′′) does not define p (c | b ∧ a). Accordingly, either p (b ∧ c | a) and p (b | a) are undefined or p (c | b ∧ a) is, and the QM probabilities never obey the axiom. In sum: If QM probabilities are interpreted as in A4′′′, further specified by Halpin’s (a), they cannot be conditional probabilities in any established calculus. This means that, given Halpin’s (a), P3 cannot be met.

6.2 Proposal (b) contradicts either P3 or P0 or ¬P2

Halpin argues against proposal (b) by showing that it leads to a probabilistic version of conditional excluded middle, something which most analyses of conditionals avoid and which is particularly implausible in a QM context.[14] Let's, however, supplement his argument by another one from P0–P3. Consider the idea that in QM the probabilities of conditionals are conditional probabilities. This is essentially the interpretation of Born probabilities, reconstructed by van Fraassen and Harper as the central tenet of Bohr's philosophy of physics (see their [1976], esp. pp. 231-236). But, in the context of A4‴, we can bypass discussing their proposal, as well as questions of probabilities of conditionals and conditional probabilities in general, and just repeat the argument against their identification, from (a). If the QM probabilities are fundamentally conditional probabilities *and* obey A4‴, there is no established probability calculus supplying the necessary axioms. So the (b) probabilities, though probabilities of conditionals, must be unconditional probabilities and directly obey the Kolmogorov axioms.

A theory of probability assignments to conditionals where these are unanalysable primitives has not been explored in the literature. Certainly, its value is questionable, as treating propositions of type 'A>B' as unanalysable primitives renders the conditional '>' semantically inert. But this must not concern us here. Instead, we must consider whether a reading of A4‴ along these lines is consistent with P3 and COMP. Note that A4‴ prescribes a state W (t) and an observable $\boldsymbol{A}$ for function p, hence forces the choice of a measurement $M_{\boldsymbol{A}}$ (t). So, we have a set of events denoted by propositions of the form '$M_{\boldsymbol{A}}$ (t) > S has $\boldsymbol{a_j}$' for the eigenvalues $\boldsymbol{a_j}$ of $\boldsymbol{A}$ (where j = 1, 2, 3…). These events will necessarily be disjoint and so S($\boldsymbol{A}$), the set containing them, will not form a sigma-algebra. We can, however, easily define a function p: P(S($\boldsymbol{A}$))→ [0, 1], where P(S($\boldsymbol{A}$)) is the powerset of S($\boldsymbol{A}$), by p ($\boldsymbol{a_k}$) = Tr (W (t) $\mathbf{P}_{\boldsymbol{a}k}$), if $\boldsymbol{a_k} \in$ P(S($\boldsymbol{A}$)) is the singleton event denoted by '$M_{\boldsymbol{A}}$ (t) > S has $\boldsymbol{a_k}$', by p ($\boldsymbol{b}$) = p ($\boldsymbol{a_1}$) + … + p ($\boldsymbol{a_n}$), for $\boldsymbol{b} \in$ P(S($\boldsymbol{A}$)), not a singleton, with $\boldsymbol{b} = \boldsymbol{a_1} \cup \ldots \cup \boldsymbol{a_n}$, and set p (∅) = 0. This function will trivially obey all the Kolmogorov axioms. Hence, Halpin's proposal (b) interpreted this way presents a way to respect P3 (which all other proposals violate).

But this is not what a defender of COMP can wish for. Recall the identification, at the end of sec. 2, of types of QM events in all versions of A4 and in EE and COMP. Taking now events of type '$M_{\boldsymbol{A}}$ (t) > S has $\boldsymbol{a_j}$' as unanalyzed primitives in A4‴ means to decree that all QM events are of this type. Then the events for which A4‴ calculates probabilities are the very ones which EE and COMP link to QM states. This will mean that the antecedent in EE2 and the consequent in COMP must now mention another type of event. Explicitly:

COMP′ If S is not in state $\mathbf{P}_{\boldsymbol{a}k}$ (t), then not ($M_{\boldsymbol{A}}$ (t) > [$\boldsymbol{A}$] = $\boldsymbol{a_k}$).

COMP′ says that if S is not in an $\boldsymbol{A}$-eigenstate, then there is no *implication* from an $\boldsymbol{A}$-measurement to any value of $\boldsymbol{A}$ – something which, at first glance, seems quite reasonable. But on a closer look, we are back on square one. Suppose that we have again a state W (t_1) ≠ $\mathbf{P}_{\boldsymbol{a}k}$ (t_1), for t_1, such that < $\mathbf{P}_{\boldsymbol{a}k}$ (t)> is positive. Then, given the present reading of A4‴, the probability p ($M_{\boldsymbol{A}}$ (t_1) > [$\boldsymbol{A}$] = $\boldsymbol{a_k}$) is positive, but COMP′ also declares that it is the case that not ($M_{\boldsymbol{A}}$ (t_1) > [$\boldsymbol{A}$] = $\boldsymbol{a_k}$). This situation is familiar from sec. 4: A4‴ + COMP′ entails p ($M_{\boldsymbol{A}}$ (t_1) > [$\boldsymbol{A}$] = $\boldsymbol{a_k}$) > 0 and, at the same time, A4‴ + COMP′, $M_{\boldsymbol{A}}$ (t_1) > [$\boldsymbol{A}$] = $\boldsymbol{a_k}$ |– ⊥. Hence, a version of QM that includes A4‴ + COMP′ again contradicts principle P0.

Consider, finally, the (exotic) idea of interpreting QM *without* identifying all types of QM events. We might take EE and COMP to refer to a second type of QM events, distinct from the ones mentioned in A4‴. EE and COMP, staying as before, then still mention time-indexed events of type '[$\boldsymbol{A}$] = $\boldsymbol{a_i}$ (t)', while A4‴ is an algorithm for probabilities for events of type '$M_{\boldsymbol{A}}$ (t) > [$\boldsymbol{A}$] = $\boldsymbol{a_j}$'. But this proposal reintroduces time-dependent QM events and is thus incoherent with the negation of P2 we presently hypothesise.

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[14] I forgo sketching Halpin's treatment of the Stalnaker conditional ([1991] pp. 40-42, 47) which licenses conditional excluded middle, but implies a type of counterfactual definiteness that allows to derive Bell-type inequalities. The latter fact is shown in Halpin [1986]).

6.3 Proposal (c) renders COMP vacuous or contradicts ¬ P2

Against proposal (c), Halpin has argued on semantic grounds as follows.[15] The major semantic analyses of conditionals in terms of possible worlds all render *modus ponens* a valid inference. So from A, A > B one can conclude B. This means that in a typical case, *after* a measurement M_A (t), it is true that $0 < p([\boldsymbol{A}] = \boldsymbol{a_k}) < 1$. Since QM probabilities are best understood as chances, i.e. objective, not epistemic, probabilities, option (c) has the paradoxical result that the QM event described by "$[\boldsymbol{A}] = \boldsymbol{a_k}$" is still chancy after the ***A*** measurement is over.

Persuasive as the argument is, it can again be supplemented with another one from our principles. If, in A4‴, we read 'p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ given that $M_{\boldsymbol{A}}$ (t))' as '$M_{\boldsymbol{A}}$ (t) > p ($[\boldsymbol{A}] = \boldsymbol{a_k}$)' this has the effect of cutting '$M_{\boldsymbol{A}}$ (t)' out of the equation in A4‴. But A4‴ must assign a position to the time-index on the left side of the equation, to disambiguate A4′, and proposal (c) fails to do so, for certainly it is only its consequent that can appear in the equation. There is an easy repair: Make the consequent inherit the index from the antecedent. Write '$M_{\boldsymbol{A}}$ (t) > p_t ($[\boldsymbol{A}] = \boldsymbol{a_k}$)', where '$p_t$ ($[\boldsymbol{A}] = \boldsymbol{a_k}$)' is the probability at t for the event that $[\boldsymbol{A}] = \boldsymbol{a_k}$ (at some undefined later time). Then the left and right sides in A4‴'s equation both have an explicit dependence on t. But again there are implications for the nature of QM events. Suppose (respecting the identification, from sec. 2, of QM events across versions of A4′ and COMP) that all QM events are of a type such that A4‴ calculates probabilities for them. Then, EE and COMP will associate QM states referring each to a definite time with QM events referring, by assumption, to no definite time. These statements will be vacuous and no longer express the completeness of QM. Take the new version of COMP and specify it to t_1: If S is not in state $\mathbf{P}_{\boldsymbol{a_k}}$ (t_1), then not $[\boldsymbol{A}] = \boldsymbol{a_k}$, where the consequent explicitly does not refer to a specific time. Of course, if '$[\boldsymbol{A}] = \boldsymbol{a_k}$' is true at some time this is perfectly compatible with '$[\boldsymbol{A}] = \boldsymbol{a_k}$ (t_1)'. On the other hand, suppose (now denying the identification from sec. 2) that there are two types of QM events such that, despite the new type of events now referred to in A4‴, EE and COMP stay untouched. Then, since these latter mention QM events, we have again a violation of ¬ P2.

6.4 Proposal (d) contradicts P3 or collapses into proposals (b) or (c)

Finally, consider Halpin's own proposal (d) ([1991] pp. 54-55). The new conditional '$>^p$' is given a semantics in terms of possible worlds as follows. Let s(A) be the set of those worlds where (a conditional statement) A is true that are most similar to our world. Then 'If A, then B' (understood as the indicative or subjunctive conditional) is true iff B holds at all worlds in s(A) and 'If A, then B might be true' is true iff there are worlds in s(A) where B is true. Let $\mathcal{B} = \{w \in s(A) \mid B \text{ is true at } w\}$. Then 'A $>^p$ B' ('If A, then with probability p B is true') is true iff $\mathcal{B}$ is measurable and $P_{s(A)}(\mathcal{B}) = p$.

However, this proposal is a purely semantic one. Which probability calculus is going to underpin it? Halpin himself sees his proposal as the appropriate semantic construal of conditional probabilities and, having argued against a Kolmogorov-style calculus himself, proposes a Popper-style calculus to provide the syntax. But, as we saw, no such calculus is applicable in our context because they all share the multiplication axiom which the A4‴ probabilities cannot obey. P3 is violated. One might consider an analysis of 'p (S has $\boldsymbol{a_k}$ given that M_A (t))' along the lines of (b) or (c), i.e. an analysis that respects the expression's conditional character, but nevertheless does not interpret the QM probabilities themselves as conditional probabilities. But Halpin rightly discredits proposals (b) and (c) and we have seen additional arguments against them in our context.

In sum: If Halpin's list exhausts the possible analyses of 'p ($[\boldsymbol{A}] = \boldsymbol{a_k}$ given that M_A (t))', then there is no possible interpretation of A4‴, respecting COMP, P0, ¬ P2, that can be based on an established probability calculus. Hence, the negation of P2 entails the one of P3.

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[15] See Halpin ([1991], pp. 48-52). Halpin finds this option advocated by Skyrms (see indeed his [1982] p. 44). The same proposal is made by Hughes as a component of his quantum event interpretation (see [1989], p. 303). Of course, neither Skyrms nor Hughes presupposes something like ¬ P2, so Halpin's arguments, but not mine, are applicable.

7 Conclusion

My argument for the principled incompleteness of QM has been complex. The main argument was simple and relied on the easiest way to disambiguate A4′, i.e. principle P1. The obvious loophole is not to accept P1, although, as I have argued, it follows when the familiar connection of A4′ probabilities with A4 expectation values is combined with the classical statistical definition of an expected value. Upon rejecting principle P1, it is straightforward that one has to reject also P2. But giving up these two leads to a conflict with P3: A version of QM denying us the exact time reference of QM events will have no formal connection with the established calculi of probability. I have shown that giving up any one of P1–P3 has unacceptable consequences for QM. Since P0 is sacrosanct, it is COMP that should be abandoned.

Many interpreters of QM do not accept COMP, anyway. Can they look at this set of arguments with calm? It depends. Problems arise for those modal interpretations that endorse von Neumann's transition from possible to actual value upon measurement (see above footnote 5). It should be clear that my arguments ultimately put them in a less comfortable position than their dismissal of COMP suggests. Advocating the transition is to negate P1, hence the main argument of sec. 4 is inapplicable. But all the problems of negating P1, and as a consequence also P2 and P3, remain – a fact that clearly speaks against this group of interpretations. Bohmian Mechanics can be construed as a modal interpretation, but does not include anything like the von Neumann transition. Hence, it is an exception. Since there is only one observable (position) that is faithfully measured, both P1 is accepted and COMP negated. All problems resolved! I do not want to advocate Bohmian Mechanics, in particular, as the interpretation of choice because it has well-known vices: the quantum potential as such and the problems with relativistic contexts. But I am impressed to see Bohmians straightforwardly present, for the continuous observable position, a disambiguated version of A4′ that amounts to a version of A4″, hence respects P1,[16] while other interpretations are less outspoken about this crucial point.

What are the morals? I want to draw a positive conclusion for the interpretation of QM. Apart from Bohmian Mechanics, there is another option that I find particularly interesting. I have shown that QM + P0 + P1 $\rightarrow \neg$ COMP and have tried to make it plausible that P1 does not have a viable alternative. $\neg$ COMP means that if S is not in a certain state at t_1, there is no implication, at all, that it does not have a certain value of $\boldsymbol{A}$ at t_1. In particular, if S is in some state W (t_1) $\neq$ $\mathbf{P}_{\boldsymbol{a_i}}$ (t_1), we may still assume that it has $\boldsymbol{a_i}$ at t_1, for some i. Without further argument, I take this to be equivalent to saying that for any observable $\boldsymbol{A}$ on S and for any state W (t_1), S has some value of $\boldsymbol{A}$ at t_1. This condition is called value definiteness (VD), in the literature. If the equivalence is granted, I have shown that QM + P0 + P1 $\rightarrow$ VD (where $\neg$ P0 is unintelligible and $\neg$ P1 is implausible). Now, as I said in the introduction, QM can be assumed incomplete only with strange consequences. Briefly, the situation is this. Kochen-Specker-type arguments (which have been simplified considerably in recent years)[17] show that definite values of the QM observables (VD) and non-contextuality (NC) of these values entail a contradiction with QM. Schematically: QM $\rightarrow \neg$ (VD + NC). Note that it does not make sense to give up VD and retain NC. So we are left with either $\neg$ (VD + NC) or VD + $\neg$ NC. My argument can be viewed as a vote for the latter option, i.e. for a contextual hidden-variables interpretation of QM.

[16] In a recent paper, Duerr *et al.* [2005] ask us to consider 'a universe of N nonrelativistic particles whose positions we denote by $\boldsymbol{Q_1}$ (t), …, $\boldsymbol{Q_N}$ (t)'. These are particle trajectories indicating different possible positions of S at t, for any t. The authors introduce the Schrödinger equation and Bohm's equation of motion, then write: 'The configuration Q (t) = ($\boldsymbol{Q_1}$ (t), …, $\boldsymbol{Q_N}$ (t)) is random and $|\ \psi\ (t)\ |^2$-distributed at every time t, prob (Q (t) $\in$ dq) = $|\ \psi\ (q, t)\ |^2$dq'.

[17] For a simple and unified form of the Bell and Kochen-Specker theorems for two and three particles, see Mermin [1990]. For the simplest Kochen-Specker type argument for one particle, see Cabello et al. [1998], for a proof that it is the simplest one see Pavičić et al. [2005].

I have no clear idea about such an interpretation, but one more thing can be said about it. Contextual hidden variables have been discussed here and there in the literature and two groups have been characterized. Shimony has dubbed one version ‘environmental contextualism’ and characterised it is as follows: The hidden values are relational properties whose existence depends on ‘the state of the physical environment with which the system S interacts’ (Shimony [1984], p. 29). On Shimony’s reading, environmental contextualism denies that it makes sense to attribute an ***A*** value to S in isolation. It is only when S and an ***A***-measurement apparatus meet that such a value comes into existence. So, VD (value-definiteness as just introduced, i.e. concerning ***A*** values of S in isolation) cannot be part of this brand of a contextual hidden-variables interpretation. The only classical feature (at least on Shimony’s analysis) this contextualism wants to defend against QM orthodoxy is determinism: S’s ***A*** value supervenes deterministically on the S-cum-apparatus state (see Shimony [1984], pp. 34-35). There is a second brand, which Shimony finds spelled out in work by Gudder and calls ‘algebraic contextualism’ (see Gudder [1970], Shimony [1984], p. 29). Here observables have values relative to a context which is a maximal Boolean subset of the set S($\mathcal{H}$) of closed subspaces of $\mathcal{H}$. Though a non-maximal observable can be a member of different subsets, this brand of contextualism denies that it has the same value in both cases. Now, consider that the choice of a maximal observable singles out a maximal Boolean subset. Then Gudder’s construction can be seen to be (at least technically) equivalent to a proposal made by van Fraassen ([1973]) and discussed by Redhead ([1987], pp. 134-135; see also Stairs [1990]) under the name of ‘ontological contextualism’. A non-maximal operator that is a function of different maximal operators does not represent the same observable in contexts defined by the different maximal ones; a fortiori these observables need not have the same value in these contexts. This idea can be fleshed out as follows. A measurement context ideally singles out one maximal observable and operator on $\mathcal{H}$ and so fixes the non-maximal observables several of which can be represented by the same operator on $\mathcal{H}$. Again, we have a relativity to measurement contexts, but it concerns not the S properties, but the observables on S, singled out for observation by confronting S with a certain measurement device. There is no reason here to question that an observable’s value has been determinate all along, hence VD can be respected.

Of course, I am not even sketching an interpretation, but only vaguely pointing toward one. However, my argument against COMP was also a vindication of VD in the context of QM. And so it singles out not only a hidden-variables contextualism as the major alternative to Bohmian Mechanics, but also speaks against one version of such a contextualism and for another. If the two mentioned brands of contextualism exhaust what is on offer, then my argument selects one of them: ‘ontological contextuality’, whatever this may ultimately come out to be. Since QM is so well-confirmed and is so very likely incomplete, a serious competitor for Bohmian Mechanics is a hidden-variables contextualism. The latter’s key feature of ontological contextuality is, I submit, the candidate for a deep ontological feature in Nature and deserves our closest theoretical attention.

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