Clarifying the new problem for quantum mechanics: Reply to Vaidman

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Abstract

I respond to Vaidman's recent criticisms of my paper "A New Problem for Quantum Mechanics".

1 Introduction

In a recent letter, Lev Vaidman [1] raises some objections to my forthcoming article [2], in which I argue that there is a new problem for quantum mechanics which is distinct from the standard measurement problem. I am grateful for Vaidman's criticisms, as they provide the opportunity to restate my problem and clarify a key aspect of my argument. Indeed, Vaidman's main objections are based on a misreading of my thesis which I hope to correct here.

2 The new problem involves measurement, too

The purpose of my [2] was to introduce and argue for a new problem, *the control problem*, defined as the incompatibility of the following four claims:

(B1) Preparation: We can successfully prepare quantum states: at least some of our preparation devices are such that, if determinately fed many inputs, they output a non-trivial fraction of those inputs in some specified range of quantum states. (Here the 'inputs' are subsystems, and we define 'the quantum state of a subsystem' in the standard way, as its reduced state.)

(B2) Unitarity: The quantum state of an isolated system always evolves in accord with a deterministic dynamical equation that preserves the inner product, such as the Schrödinger equation.

(B3) Determinate Input: It is always determinate whether or not a subsystem has been input into a given (measuring or preparation) device.

(B4) Competent Measurement: There is always, from a first-person view, an experienced 'observed outcome' of a measurement, even if—from a 'God's eye view'—there is no unique determinate outcome. Moreover, our measuring devices are at least somewhat informative and reliable. For example, if a spin-z measuring device is determinately fed an electron in the eigenstate $\uparrow_z$, it will output 'UP' (and not 'DOWN').

The reader may notice that this fourth assumption, (B4), is about measurement. Yet, the control problem is supposed to be "a new problem" which is "distinct from the standard measurement problem". How can it be "new" if it still involves measurement?

It is new because it does not rely on the following assumption:

(A1) Completeness: The quantum state of a system (e.g. of a measuring device) determines, directly or indirectly, all of its physical properties.

The standard measurement problem, and most of its usual variants, crucially rely on this no-hidden-variables-type assumption [3]. The novelty of the control problem is that it relies on (B1) rather than (A1): a tension with unitarity and determinateness exists even if the wave function does not specify everything, as long as Preparation is assumed. Roughly, this is because we can associate each measurement outcome with a distinct preparation procedure, and then the prepared quantum states can be used as a proxy for the pointer eigenstates. For instance, consider a spin-z measurement. Depending on the outcome, we can plan to either prepare an atom in its ground state ψ or an excited state ψ', where $|(\psi, \psi')|^2 \approx 0$ (or more generally, ρ or ρ' where $F(\rho, \rho') \approx 0$). Here, even though the possible post-measurement pointer states might not be orthogonal (without Completeness, the quantum state of the pointer need not specify the outcome), the final prepared states of the atom are (approximately) orthogonal, and this is enough to contradict Unitarity.[1] So, we get a tension with Unitarity using (B1) Preparation, even without (A1) Completeness.

This tension is worth noting because we might have a candidate theory which rejects (A1) and thus avoids the inconsistency of the standard measurement problem, but still falls prey to the inconsistency (B1)–(B4). As a toy example, consider a theory which posits a basic divide between the 'classical' (macroscopic) realm and 'quantum' (microscopic) realm. In this theory, the wave function does not determine macro-properties like pointer readings. Rather, the distribution of macro-properties at a time t depends in a *chancy* way on the history of the wave function (which evolves deterministically) up to t, and the classical macro-history up to t. Meanwhile, wave functions of micro-systems can still be manipulated in the usual manner. For example, a hydrogen atom can be cooled to its ground state, or heated to an excited state.

This theory avoids the standard measurement problem because a superposition of pointer states is still compatible with a definite macro-reading. It can also account for measurement statistics without positing wave function collapse, through the indeterministic law that connects the micro to the macro. And it can incorporate the effect of initial measurements on future measurements, since the probabilities given by this law are also sensitive to the macro-history up to t.[2] But the theory faces the control problem. As long as the wave

[1] Roughly, the derivation is: By (B1) and (B4), $\uparrow_z \otimes \text{initial} \to \nu \otimes \psi$ and $\downarrow_z \otimes \text{initial} \to \nu' \otimes \psi'$. By (B1) and (B3), either $\uparrow_x \otimes \text{initial} \to \mu \otimes \psi$ or $\uparrow_x \otimes \text{initial} \to \mu' \otimes \psi'$. Either way, we violate Unitarity (B2). For details and a generalization to impure states, see [2, Sect. 4].

[2] This problem of accounting for repeated measurements, the *problem of effect* [3], is closely related to

function dynamics is unitary (or even if it is only CPTP [2, Theorem 3]), preparations which are correlated with measurement outcomes will not prepare in the desired reduced state. In response, the theory could drop the preparation assumption. But then it will need to account for our usual talk of successfully preparing quantum states even after measurements. And unlike Bohmian mechanics, which has the notion of a *conditional quantum state* (obtained by inserting the 'actual' Bohmian positions of particles from the environment into the parent wave function [4, 5, 6]), it is not clear that this theory has the resources to give such an account. Of course, this "theory" is only a toy example. But it illustrates how one might avoid the standard measurement problem and still fall prey to the control problem.

While the control problem is different from the standard measurement problem, it still involves measurement. Thus I emphasize in my article [2, Sect. 1]:

> "On the other hand, the control problem invokes Competent Measurement, and crucially involves a measurement-type interaction (combined with a preparation). So if the measurement problem is defined as the general puzzle that measurement interactions can be problematic under Schrodinger dynamics, then the control problem should be viewed as a specific aspect of it."

3 Vaidman's reading

Vaidman [1] attributes to my article a much stronger and less plausible thesis: that there is an internal inconsistency between preparation and unitarity, and in particular that (B1)–(B3) are incompatible on their own, without any consideration of measurement. Indeed, Vaidman omits (B4) in his exposition. When the role of measurement in my problem later arises, he remarks that "when introducing a new problem in quantum mechanics he was not supposed to include measurement, which is problematic by itself". Of course, my goal was not to set measurement aside, but to point out that a problem can arise even without (A1)

the control problem, however it is not identical. See also [2, Sect. 4.3].

Completeness. This key point about Completeness is not mentioned anywhere by Vaidman.

With this clarification in mind, we can straightforwardly address Vaidman's main criticisms:

“ • There exists a particular set-up in which all [the author]'s claims are found to be true, so we prove by construction that his inconsistency proof fails.

• Demonstrating his ‘new problem’ [the author] presents a set-up involving measurement (which is known to be a problem in quantum mechanics) together with preparation. He tries to derive the inconsistency from the preparation of states, but makes an error (dividing by zero) in his proof.”

Let us take these two points in turn.

3.1 Unitary preparation and measurement

Vaidman's alleged counterexample involves a set-up in Bohmian mechanics, where a pure spin state is successfully prepared through a unitary operation. There is no measurement in his set-up.

I agree that there is no inconsistency without (B4). In fact, I give a similar example of a unitary preparation in Sect. 3.1 of my paper. As I write there, “unitarity and the preparation assumption are perfectly compatible. Schrodinger's theory does not face the same general problem for preparations as it does for measurements” (see also [7]).

The set-up for my problem is a measurement followed by a preparation, where which state is prepared depends on the observed outcome. On a Bohmian analysis, it is indeed true that the systems are not prepared in the desired reduced state, but rather in a desired *conditional* state, as discussed above. So in my terminology, Bohmian mechanics rejects (B1) in favor of:

(B1') Conditional Preparation: Given determinate inputs, our preparation devices can prepare those inputs in desired conditional quantum states (relativized to an extra parameter) but not desired quantum states simpliciter.

Here the 'extra parameter' involves the final Bohmian position configuration of the pointer, with which the choice of preparation was correlated. As I discuss in [2], I think this solution to the control problem is one of the most elegant.

3.2 Measurement, orthogonality, and "dividing by zero"

In analyzing my incompatibility argument in greater detail, Vaidman writes:

> "The inner product of the prepared state is not relevant for calculating the inner product between the final states of the whole composite system for the two alternatives: it is zero due to the measurement which is embedded in the set-up."

Here Vaidman is saying that measurement is already incompatible with the preservation of inner products, (B2) Unitarity, assuming determinateness, and so the role of preparation in my proof is redundant. I suspect that Vaidman is implicitly relying on (A1) Completeness to get this conclusion. Indeed, he seems to be assuming that since the two alternatives involve distinct macroscopic measurement outcomes, they must be represented by orthogonal wave functions. But as discussed, this is precisely the kind of assumption that a theorist who rejects (A1) might deny. For example, in a Bohmian model of the spin-z measurement of an $\uparrow_x$ particle, the two alternatives of an 'UP' and 'DOWN' outcome have the same final wave function. (It is only if we *condition* this wave function on the final configuration of the device (the Bohmian positions) that we get orthogonality.)

Of course, I agree that once Completeness is assumed, we will run into tensions with Unitarity for entirely familiar reasons. The point is to demonstrate a tension that does not hinge on Completeness, and for this, my preparation assumption plays a crucial role and is not redundant.

Vaidman also falsely claims that I make a "technical error" in my proof: "[he] cannot claim that Eq. 8 implies Eq. 9". My reasoning was:

"Unitarity implies [toward contradiction],

$$|(\uparrow_z, \uparrow_x)|^2 \cdot |(\text{initial}, \text{initial})|^2 = |(\text{final}, \text{final'})|^2 \cdot |(\uparrow_z, \uparrow_x)|^{2 \cdot M}. \tag{8}$$

which, since $|(\text{initial}, \text{initial})|^2 = 1$ and $|(\text{final}, \text{final'})|^2 \leq 1$, implies:

$$|(\uparrow_z, \uparrow_x)|^2 \leq |(\uparrow_z, \uparrow_x)|^{2 \cdot M}.\text{"} \tag{9}$$

Vaidman's objection is that $|(\text{final}, \text{final'})|^2 = 0$ and so in concluding Eq. 9 I am "dividing by zero". But there is no division going on. Consider the LHS and RHS of Eq. 8. By $|(\text{initial}, \text{initial})|^2 = 1$, the LHS equals $|(\uparrow_z, \uparrow_x)|^2$. By $|(\text{final}, \text{final'})|^2 \leq 1$, the RHS is less than or equal to $|(\uparrow_z, \uparrow_x)|^{2 \cdot M}$. So if the LHS and RHS are equal, it follows that $|(\uparrow_z, \uparrow_x)|^2 \leq |(\uparrow_z, \uparrow_x)|^{2 \cdot M}$ as claimed.

4 The determinateness assumption

In addition to these main objections, Vaidman presents a third worry for my argument. He writes that the "inconsistency [of (B1)–(B4)], if it exists, does not represent a problem for quantum mechanics," because the assumption (B3) Determinate Input is not plausible and should be rejected anyway. (B3) was intended as a 'single world' assumption, analogous to Definite Outcome:

(B3) Determinate Input: It is always determinate whether or not a subsystem has been input into a given (measuring or preparation) device.

According to Vaidman, this assumption is implausible because it concerns a quantum microscopic subsystem which should not be expected to have determinate classical features.

I agree with Vaidman that the "always" here is too strong. But in the context of my set-up, (B3) is not implausible. In my set-up, there are two preparation devices, one (call it D) designed to prepare D-states, the other (call it D') designed to prepare D'-states. An experimenter measures the spin-z of an electron. Her protocol is that if she obtains 'UP', she feeds a system or ensemble of systems into D; otherwise, she feeds them into D'. Suppose the lab is arranged so that D and D' are on opposite (left and right) sides of the room. (B3) then says that there is a determinate fact as to whether the experimenter goes to the left side and uses D, or goes to the right side and uses D'. Of course, some interpretations, most notably Many Worlds, will reject this assumption (on this interpretation, on one branch she uses D while on another she uses D'). But it is at least initially plausible.

5 Conclusion

Vaidman ends his remarks by noting "The measurement problem of quantum theory is still with us. It has many (sometimes contradicting) solutions in various interpretations with, unfortunately, no consensus yet about the preferred one." On this we are in agreement. The measurement problem remains as pressing as ever. Whether one views Completeness as a defining feature of this problem will affect whether one views the control problem as separate, or yet another species of it. But in either case, I take this additional problem, which involves both measurement and preparation, to be worth pointing out.

References

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