On Relationalist Reconstructions of Quantum Theory

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(Dated: October 19, 2021)

Why I’m not happy with how Relational Quantum Mechanics has addressed the reconstruction of quantum theory, and why you shouldn’t be either.

One appealing feature of Carlo Rovelli’s proposal for “Relational Quantum Mechanics” [1] is that it offered a challenge for those who prefer technical work over slinging sentences: the reconstruction of quantum theory from information-theoretic principles. This appeal was witnessed by the means through which I first learned of RQM, a Wikipedia page written by a fan in 2006 [2] and since trimmed heavily on the grounds that it said many things not explicitly stated in the literature already. (As David Mermin once said, “Writing on Wikipedia is like writing on water.”) Of course, the idea of rebuilding quantum theory on a better foundation long predates Rovelli. In some fashion, it is at least as old as Birkhoff and von Neumann’s explorations. We could also mention Mackey’s challenge and its answer by Gleason, and John Wheeler’s suggested research project for his more promising undergraduates: “Derive quantum theory from an information theoretic principle!” [3]. But Rovelli’s article, along with the 2000 Montreal workshop and Hardy’s derivation not long after [4, 5], served to mainstream the question for the current century.

A few days after I posted a first version of these notes online (https://www.sunclipse.org/?p=3016), Muciño, Okon and Sudarsky independently filed a lengthy critique of RQM on the arXiv [6]. As will become clear over the following sections, I tend to agree with some of their criticisms. However, the overall tone here will be closer to that of Pienaar [7, 8], who evaluates RQM from as sympathetic a position as possible, taking as given the legitimacy of “Copenhagen-ish” interpretations and seeing how RQM fares when evaluated in that context. Moreover, my emphasis will be different from that of all these criticisms, as I will focus upon the reconstruction side of Rovelli’s proposal. Has it borne fruit? When later work claimed to find inspiration in it, how close was the relationship, and did RQM really hold up its end of the deal?

I. ROVELLI’S RECONSTRUCTION POSTULATES

Rovelli’s preliminary stab at reconstructing quantum theory from information-theoretic principles has some appealing features but also a few things going against it, which can be illustrated by quoting the first two postulates that Rovelli proposes.

R1. “There is a maximum amount of relevant information that can be extracted from a system.”

R2. “It is always possible to acquire new information about a system.”

These have a little of the feel of Einstein’s postulates for special relativity, in that they seemingly run the risk of contradicting each other [9]. However, in special relativity, resolving this dramatic tension required overhauling our notions of space and time, whereas it is possible to have R1 and R2 coexist in a much more mundane way. For example, the
Spekkens toy model is a theory explicitly founded upon local hidden variables [10], and both R1 and R2 hold true in it.

Strangely, for a derivation motivated by a “relational” interpretation of quantum mechanics, there is nothing all that strongly relational about the postulates R1 and R2. True, they refer to the information that one system might hold about another, but any statement in the theories of information or probability will be “about relations” in this sense. R1 and R2 neither lean upon nor imply the relativity of physical facts that RQM is supposed to endorse. In that sense, they are no more “relational” than the Central Limit Theorem is. Laudisa and Rovelli say that RQM discards the assumption “that there are variables that take absolute values, namely values independent from any other systems” [11]. Yet R1 and R2, both singly and in combination, are perfectly compatible with the existence of absolute physical quantities.

I will not dwell very long on the rest of Rovelli’s derivation, as the other premises that he asserts are self-confessedly provisional. Two conceptual points do deserve examination, though. First, even granting all of the mathematical choices that Rovelli is willing to make in order to sketch how a future derivation might go, like the presumption of unitary matrices, some postulate is required to ensure that the derivation does not land in a subtheory of quantum mechanics that admits an easy classical emulation. For example, one can take the entirety of Rovelli’s postulates and arrive at the theory of von Neumann measurements upon a single qubit, a theory for which Bell provided a perfectly satisfactory local-hidden-variable model [12]. Rovelli’s stated premises would also be satisfied by the Spekkens toy theory of odd prime dimensions, which provides a local-hidden-variable emulation of qudit stabilizer states and operations [13]. Without something like a further additional postulate of nonclassical structure, it seems difficult to avoid these traps.

The other point of note is that Rovelli expresses the hope that his suggested third postulate could be derived from a consistency condition between observers. However, this consistency condition is ill-posed. The trouble with “consistency” in RQM is fundamental, and one way or another, the difficulty enters with every attempt to define what the consistency condition is supposed to be. For instance, Smerlak and Rovelli write, “It is one of the most remarkable features of quantum mechanics that indeed it automatically guarantees precisely the kind of consistency that we see in nature” [14]. They continue, setting up a scenario involving two observers and a spin system: “Let us illustrate this assuming that both A and B measure the spin in the same direction, say z, that is \( n = n' = z \)” [14]. Immediately, we have trouble: RQM is supposed to reject absolute states, absolute physical properties and everything like that. Who gets to say, then, that these two directions are the same?

In RQM, the quantum state of a system S with respect to another system S’ is an expression of the relation between S and S’. The subject matter of quantum theory is taken to be “information” about such relations, how that “information” is constrained, how it may change and so forth. Any standard of consistency expressed in these terms can only be a consistency standard between informational relations, and by the basic edict to reject absolutes, the informational relations to which it applies must be tied to a specific observer. As Pienaar has recently phrased it, RQM suffers from loose-frame loopholes [7]. Every physical statement within RQM must be relative to some system, but it is often not clear which system that is meant to be, and a condition that is supposed to apply relative to one might actually only be meaningful for another. Until these loopholes are closed, there is little hope that any condition upon intersubjective agreement could provide a helpful
supplement to \textbf{R1} and \textbf{R2}. Indeed, the conjunction of \textbf{R1} and \textbf{R2} is by itself so mundane that the further premises should say that the world is \textit{interesting}, not that everything works out without surprises.

\section{Convexity and the Meaning of Probability}

In this section and the next three following, we will investigate the assumptions employed in the reconstruction work of Höhn and Wever \cite{15, 16}. This work credits RQM as a source of conceptual inspiration, and Rovelli has advertised it as “particulary successful” \cite{17}; a colleague informs me that Rovelli himself finds it the “best completed reconstruction” \cite{18}. Höhn and Wever present formalized versions of \textbf{R1} and \textbf{R2}, supplement them with several further postulates, and derive the quantum theory for systems comprising collections of qubits. Importantly, Höhn notes \cite{15} that their “informational approach is generally compatible with (but does not rely on)” RQM, as well as the Brukner–Zeilinger \cite{19} and QBist \cite{20} interpretations. This raises a significant question. If we put ourselves in the shoes of someone maximally sympathetic to RQM, a true devotee of the interpretation, do we actually have reason to take the mathematical steps of the reconstruction? Does the inspiration translate into algebraic specifics? The goal here is not to dispute any of Höhn and Wever’s mathematics, but to see how good a foundation RQM itself could actually provide for their derivations. As argued elsewhere \cite{21}, their technical steps are of independent interest. Rather than delve into the group theory, we will focus on interrogating axioms and motivations, because philosophy interprets aggression as strength \cite{22}. Can a devotee of RQM honestly claim that Höhn and Wever’s reconstruction puts flesh on RQM’s bones, or is it more the case that Höhn and Wever have provided the ribs and vertebrae too?

First, we should address a couple ideas in RQM that Höhn and Wever do not use. Their reconstruction does not depend upon any inter-observer consistency condition. Moreover, in their approach, an “observer” is a question-asking, information-gathering entity. Thus, while their reconstruction is a completion of Rovelli’s original suggested program (for collections of qubits), it can only derive the concept of a quantum state relative to a decision-making agent, not an arbitrary physical system. This leads us directly into an ambiguity in the RQM literature. While the primary papers on RQM contain statements to the effect that a quantum state is a “bookkeeping” device for physical facts, rather than a fact itself, the examples they provide for “facts” are descriptions like “the spin in the direction \( z \) was \( \hbar/2 \)” \cite{17}. How much daylight is there between this and the claim that the ket \( \ket{z = +\hbar/2} \) is a fact? In order to consistently hold the position that a quantum state is a bookkeeping contrivance, the values that a quantum state can be updated to must also be bookkeeping contrivances. This is a well-known point regarding epistemic and doxastic interpretations of quantum theory \cite{23}. We shall touch more below on how RQM effectively re-ontologizes quantum states, a move that raises problems that sticking fully with the “bookkeeping device” interpretation would avoid \cite{7, 24}.

What, then, about the premises that the reconstruction does invoke?

Axiomatic reconstructions of the quantum formalism often present the notion that \textit{the state space should be a convex set} fairly early in their derivations. The idea is to abstract the fact about quantum theory that, given two density matrices \( \rho \) and \( \rho' \) of the same dimension, then

\[
\rho_\lambda := \lambda \rho + (1 - \lambda) \rho'
\]  

(1)
is also a valid quantum state, for any $\lambda \in [0,1]$. In these approaches to quantum theory, one argues for convex combinations being valid states because an experimenter working on a system can do one preparation with probability $\lambda$ and the other with probability $(1-\lambda)$. The variable $\lambda$ is assumed to take values anywhere in the unit interval. Höhn and Wever invoke a postulate of this type, in the form of choosing one system out of a pair with probability $\lambda$. (Not all reconstructions give convexity an explicit bullet point or pull it out as a numbered postulate, but Höhn and Wever do; see Assumption 2 in [15].)

In order for Eq. (1) to make sense as a representation of the procedure of flipping a coin to choose a preparation, we must be able to prepare a coin with an arbitrary bias. If we relax this, then we find ourselves again in the realm of theories that satisfy $\textbf{R1}$ and $\textbf{R2}$ while being classical. Lacking the full interval can indeed arise naturally, since a coin itself must be a physical system, described by the same general theory as all the others. If the basic objects in our theory have only a discrete set of possible statistical states, then we can only obtain a discrete set of mixtures. Say I have a Spekkensian toy bit to which we have ascribed a maximal information state, and which I plan to measure in some way, such that my probability distribution over the two possible outcomes is uniform. So, I have a “fair coin”, implemented as an object described by my overall theory. Let $S$ be some other system, a collective of toy bits. If I get the + outcome on my coin toy bit, I carry out some preparation procedure on $S$. If I get the – outcome on my coin, I put $S$ through a different preparation process. My previsions for $S$ can access the interior of the state-space geometry, but not continuously, or even densely: I’m just picking up the 50–50 mixtures. The more coins I use, the more internal points I can pick up, but with a finite number of toy bits available to use as coins, I can only get so many of them!

We could close off this path and avoid the Spekkensian trap if we were sure we had the whole unit interval to work with. Unfortunately, the RQM literature provides little guidance regarding the interpretation of probability theory, particularly when it comes to probabilities that are not $1/n$ for some $n$. (In contrast, the primary literature on QBism covers the topic at length — perhaps exhaustively, if not persuasively [3].) Laudisa and Rovelli are insistent that RQM defines information “in the sense of Shannon”, which they take to be a “definition of information that has no mentalistic, semantic, or cognitive aspects” [11]. Such a claim can only be as good as the presumption that the $p_i$ in $-\sum_i p_i \log p_i$ have no such aspects. That is, it presumes an interpretation of probability that grants at least some probabilities an objective status: relative frequencies, propensities, degrees of logical implication, et cetera [8]. The RQM literature leaves the question of which such interpretation to adopt mostly up in the air. We can make a tentative deduction from the fact that for Laudisa and Rovelli, “relative information” measures “the difference between the possible number of states of the combined system and the product of the number of states of the two systems”. Likewise, Rovelli has more recently written that “information” in $\textbf{R1}$ and $\textbf{R2}$ means “nothing else than ‘number of possible distinct alternatives’ ” [17]. In other words, RQM declares objectivity at the cost of having all fundamental probability distributions be flat, so that Shannon’s formula reduces to Hartley and Boltzmann’s.

Consequently, it is not clear that RQM gives one free license to invoke arbitrary values of $\lambda \in [0,1]$. In order to make the reconstruction go, we have to add specifics to the inspiration, and whether RQM can actually sustain the technical statement we need is far from established. Moreover, expunging the “mentalistic, semantic, or cognitive aspects” from probability does the same for quantum states. If $S$ is a qubit and there is a physically correct expectation value for $\sigma_x$, for $\sigma_y$ and for $\sigma_z$, then there is a physically correct quantum
state for $S$, not just a mental bookkeeping device. Once the state is “determined entirely by a specific history of interactions” [11], then we can speak of the state for $S$ relative to $O$ whether or not $O$ is clever enough to calculate it. The “propensity” or “objective chance” inheres in the history of events. (I know I’m not the only one who has read the RQM papers as saying this!)

Schmid has summarized further reasons why the case for convexity is even more debatable than suggested above [25]. Consider again the scenario of flipping a coin to decide between systems, or preparations of a system. What rules out the possibility of a causal influence from the coin? It could be that the procedure $P$ and the procedure “flip a coin, obtain heads and perform $P$” are quite different physically, perhaps because of physical consequences left by the coin flip. The set of all lists of lab procedures has no intrinsic convex structure, and trying to impose such a structure upon it is a tricky business. Schmid points out that an alternative way to justify a convexity condition is to regard mixing inferentially. For example, instead of using a coin to decide between two sets of laboratory procedures, one could consider states of belief about whether the preparation was $P$ or $P'$. The set of these states does have a natural convex structure, inherited from probability theory [26]. Yet now we must confront the question of whether RQM allows this move. Does RQM’s physicalist interpretation of probability allow for $\lambda$ to be read this way? QBism, for instance, would run with it (and declare that those “preparations” are also probabilities [27]). But it’s hard to imagine an RQM devotee wanting to be seen as too QBist.

### III. CONTINUITY

Höhn and Wever make two assumptions that are important for making sure the theory being constructed does not fall into the trap of being the Spekkens toy model. They invoke a continuity assumption, which they phrase as,

[An observer] $O$’s ‘catalogue of knowledge’ about [a system] $S$ evolves continuously and every consistent such evolution is physically realizable.

In the Spekkens toy model, the pure states form a discrete set. One can try introducing continuity by enlarging the state space to be the full convex hull of the pure states, but even then, the states of maximal knowledge will still be that discrete set. Thus, we rely upon another assumption, that

$O$’s total amount of information about $S$ is preserved in between interrogations.

Time evolution carries pure states to pure states, in other words, and such evolutions are continuous, meaning that the pure states of the theory being constructed will form a continuous set as well. In order to make this assumption precise, Höhn and Wever reject the Shannon definition of “information” and derive a new measure, which works out to be the squared Euclidean length of a generalized Bloch vector. This derivation is one of the points of independent interest (the interaction of Shannon and non-Shannon information measures in quantum theory is known to lead to intriguing structures [28]). For present purposes, though, we focus on the motivations, and whether RQM can drive them.

As Höhn and Wever make clear, their assumption of continuity is necessary to narrow down the choice of information measure to their desired form. Given the importance of continuity, then, we must ask how much support the fundamentals of RQM provide for
it. And here again, we run into trouble. Little is said that is definitive, but what is said favors discreteness and granularity. For example, Rovelli writes, “Facts are sparse: they are realized only at the interactions between (any) two physical systems” [17]. And at somewhat greater length [29],

The question of “what happens between quantum events” is meaningless in the theory. The happening of the world is a very fine-grained but discrete swarming of quantum events, not the permanence of entities that have well defined properties at each moment of a continuous time.

If the only thing that flows in between measurements is meaninglessness, on what grounds can we make any assumptions about symmetry groups? At best, continuity is an unmotivated postulate, conceptually additional to the underlying ideas. Likewise, Rovelli writes in a popularized treatment of quantum gravity [30],

The “quantization” of time implies that almost all values of time do not exist. If we could measure the duration of an interval with the most precise clock imaginable, we should find that the time measured takes only certain discrete, special values. It is not possible to think of duration as continuous. We must think of it as discontinuous: not as something that flows uniformly but as something that in a certain sense jumps, kangaroo-like, from one value to another.

If quantum gravity is a world of sudden jumps, then quantum theory must encompass them, and its interpretation cannot presume continuity at a fundamental level. Where, then, do we get the smoothness we need?

We will return to the question of continuity between measurements during the Discussion section, but first, we need to explore how RQM handles measurement events themselves.

IV. ALL MEASUREMENTS ALLOWED? REVENGE OF THE SHYFTY SPLIT

The penultimate numbered postulate of Höhn and Wever is the unrestrictedness of questions: “Every question which yields legitimate probabilities for every way of preparing [a system] S is physically realizable by [an observer] O.” I would argue that this is a good strategy. Quantum theory is very broadly applicable, and a sensible way to understand such a general theory is to start with assumptions that imply the fewest restrictions and then impose additional constraints only when strictly necessary. If we wish to economize on postulates, then we should first see what structures arise out of minimal sets of them, and only later truncate those structures. But RQM gets in the way of this! To see why, we will have to delve into how RQM treats measurements, and the timing thereof.

A commonplace criticism of “the Copenhagen interpretation” is that it leaves unspecified when the process of “measurement” takes over from unitary, Schrödingerian time evolution. According to this critique, subscribing to “the Copenhagen interpretation” is rather like saying that most of the time, gravity is an inverse-square law, but it stochastically switches to an inverse-cube dependence for brief moments. This would be, of course, a pathological feature for a theory to have. It is also rather disconnected from what Bohr actually wrote. To a Bohrian, there is not a sudden shift between different dynamical laws, but instead a contextual change in what language can be applied, in particular regarding when a system should be treated in functional or in structural terms [31]. Most likely, a Bohrian would say
that in order to communicate the result of an experiment to another physicist, we would of course have to describe it unambiguously, and any sufficiently unambiguous description — “In natural language, potentially augmented by the concepts of classical physics,” they might say — would necessarily fix any dividing line in place. (A Heisenbergian would instead have a different kind of dividing line in mind, and say that it can be shifted, but without operational consequence [31]. Still another species of Copenhagener, a Peresian, would agree with what the Bohrian said about languages, and then argue that we can move the quantum-classical “cut” under some conditions, never without consequence but sometimes with effects that are statistically negligible [32].) Everettian interpretations of quantum mechanics shunt the vagueness of the term “observer” over to the question of when wavefunction branches are “separate” [32]. RQM washes its hands of trying to define observers by declaring that any physical system can serve as one, which as we will see just transfers the load to the definition of interaction.

The primary literature on RQM carries forward the ahistorical idea [33] that “the” Copenhagen interpretation is well-defined and identifiable with what is found in “textbooks” [11]. It also opens the question of whether RQM actually manages to evade the critiques leveled at whatever the people out to “destroy the Copenhagen interpretation” imagine it to be. (Declarations that “the” Copenhagen interpretation must be demolished are as plentiful as they are historically and conceptually muddled [34].) The Stanford Encyclopedia article by Laudisa and Rovelli raises this very point and walks into a loose-frame loophole:

The history of a quantum particle, for instance, is neither a continuous line [in] spacetime (as in classical mechanics), nor a continuous wave function on spacetime. Rather, with respect to any other system it is a discrete set of interactions, each localized in spacetime.

The flash ontology of RQM seems to raise a difficulty: what determines the timing for the events to happen? The problem is the difficulty of establishing a specific moment when say a measurement happens. The question is addressed in Rovelli (1998), observing that quantum mechanics itself does give a (probabilistic) prediction on when a measurement happens. This is because the meaning of the question whether or not a measurement has happened is to ascertain whether of not a pointer variable $O_A$ in the observing system $S$ has become properly correlated with the measured variable $A$ of the system $A$. In turn, this is a physical question that makes sense because it can be posed empirically by measuring $A$ and $O_A$ and checking if they are consistent.

Here, we have the puzzling situation that what should be the most fundamental scenario, one system observing another, can only be said to make sense if we bring in a third party. Yet why should properties that exist relative to that third party be at all binding upon the first two systems? The joint state of the first two relative to the third is, by definition, not the quantum state of the second relative to the first. But it is the latter which changes “when a measurement happens”. (This is one place where the concerns in my original notes largely parallel the critique by Muciño et al. [6]. However, their further statement that “the notion of measurement cannot be part of [the quantum] framework at the fundamental level” seems a nonstarter to me, rather like demanding that game theory cannot have “player” as a basic concept [35, §17]. The years have taught me that this is a more fundamental disagreement than I can hope to resolve within a parenthesis, and so I will simply remind the reader that, like Pienaar [7], I am trying to evaluate RQM from the most generous perspective that I possibly can.)
Smerlak and Rovelli’s 2007 paper “Relational EPR” [14] indicates that RQM finds the Heisenberg picture of time evolution “far more natural” than the Schrödinger. In a way, this is an invigorating move: It is tempting to wonder what attitudes physicists would find natural if we were taught all along that observables always change smoothly and unitarily, while states only ever change suddenly and stochastically. However, it does not seem obligatory. Whatever one does with the expectation values $\langle \psi | U_t^\dagger A U_t | \psi \rangle$, one must put the unitaries somewhere, and per the ethos of denying absolutes, none of the ingredients in that expression should be taken as intrinsic to the observed system. Overall, the important thing is that the relative physical condition of the observer and the observed system must vary with $t$, regardless of which mathematical entity is chosen to stash that dependence in.

The term flash ontology suggests rather strongly that the “flashes” are ontological. Something is realized in the flash: A variable takes a physical value, even if only in relation to a single other system. This is at odds with the treatment in Rovelli’s 1998 paper to which the Stanford Encyclopedia article refers [36]. In that paper, Rovelli declares, 

\[ \text{I am not claiming that there is an “element of reality” in the fact that a measurement has happened, or that, in general “measurement having happened or not” is an objective property of the coupled system.} \]

Yet what are the “flashes” supposed to be, other than elements of relational reality? Moreover, the “time of measurement” calculated in the 1998 paper is not the time at which an observer $O$ will have measured a system $S$ (obtaining, one might guess, a “flash”). Instead, it is the time at which the joint state of the combined $SO$ system, relative to a second observer $O'$, will take a particular form. Using this as the means to define timing would mean that only an observer who does not experience the flash can have timing information about it. This is difficult to square with the idea that the state of system $S$ relative to observer $O$ is supposed to encapsulate the history of the interactions between them. Laudisa and Rovelli declare that any quantum state is “nothing more than a compendium of information” that is “determined entirely by a series of interactions: the interactions between the system and a second ‘observing’ system” [11]. In their view, a quantum state “codes the values of the variables of the first that have been actualised in interacting with the second” [11]. If this is the case, then the quantum state of $S$ relative to $O$ will change with each flash, except in the measure-zero set of circumstances where it was already an eigenstate of the observable whose value became actual in the flash. But RQM provides no way to predict, model or even define the times at which these changes occur: The only account of measurement timing it can offer is relative to a second observer $O'$, who is not a party to the flashes between $O$ and $S$. Thanks to this loose-frame loophole, RQM leaves the manner and mode of time evolution essentially underdetermined. The most basic criticism that anti-Copenhagenists aim at what they call “Copenhagen” hits RQM dead center.

RQM is ambiguous on whether some “observables” are physically preferred or not. If any basis can correspond to a valid measurement, then one is entitled to say that a measurement has occurred at any time. On the one hand, the RQM literature insists that there is no special role for human observers or any particular kind of measuring apparatus. On the other hand, Rovelli dodges the preferred-basis problem by invoking just such a distinction [7]. We can do no better than to quote Rovelli at this point [1]:

\[ \text{[G]iven an arbitrary state of the coupled } S-O \text{ system, there will always be a basis in each of the two Hilbert spaces which gives the bi-orthogonal decomposition, and therefore which defines an [operator on the Hilbert space of } S-O \text{ for which} } \]

the coupled system is an eigenstate. But this is of null practical nor theoretical significance. We are interested in certain self-adjoint operators only, representing observables that we know how to measure; for this same reason, we are only interested in correlations between certain quantities: the ones we know how to measure.

In order to avoid having a vacuous interpretation, Rovelli denies a key assumption of the “best completed reconstruction” inspired in part by RQM. Trying to put flesh on the bone, we get organ rejection instead! Until this is resolved, a quantum reconstruction by way of RQM principles has no good reason to presume that all questions are physically permitted.

V. LOCALITY

In the last step of their argument, Höhn and Wever invoke tomographic locality, the idea that the joint state of a bipartite system is fully characterized by statistics for measurements on the two parts and their correlations. In other words, only measurements on the individual halves are necessary, rather than “global” measurements of the pair together. As several other reconstruction programs have done, Höhn and Wever use this postulate to rule out real-vector-space quantum theory. Mathematically, it is successful in that regard.

But as Wootters has asked [37], if local tomography failed, would we really see that failure as much stranger than, say, entanglement? Moreover, if a local account of quantum physics is possible in RQM, it is likely to resemble that provided by QBism, and since the QBist account of physical locality does not demand local tomography, it is doubtful that RQM could either.

QBism is a local interpretation of quantum mechanics, firstly because it ties sample spaces to the agent [38]. That which changes “at a distance” in a Bell scenario, for example, is only the agent’s expectations about what might happen if she were to travel to that distant location and take action there. Moreover, these changes of expectation require no signalling to account for. For a QBist, the idea of spacetime is a conceptual tool that an agent can use in navigating the flux of life; it is part of the character of quantum theory that no change of state requires the introduction of ontological elements that carry information faster than light, as plotted on any agent’s spacetime diagram [39].

This more physical kind of locality is harder to abandon than the tomographic definition, and the latter could be discarded while keeping the former. For this reason (among others), QBist work on quantum reconstruction has avoided relying upon the assumption of tomographic locality [40].

The status of physical locality in RQM is somewhat obscure [41]. However, it does seem that if we want RQM to be a local interpretation, some account like the QBist one is the best that RQM could hope for. Whether such an account might be available is up for debate; the relentless physicalization of probabilities in RQM makes some of the moves available to the QBist inadmissible. Suppose that the joint state of two systems $S$ and $S'$ relative to an observer $O$ is maximally entangled. Then the realization of a variable of $S$ immediately implies a probability-1 prediction about the corresponding variable of $S'$, and so an aspect of the physical relation between $S'$ and $O$ must have changed. Smerlak and Rovelli would have it that this is only a “subjective” change, analogous to a reader’s information about a distant country changing when they read a newspaper article about it [14]. But this runs headlong into RQM’s insistence that information is factive, always to be understood as objectively
Even if we grant RQM an account of physical locality along the lines of the QBist one, RQM would lack fundamental grounds to posit local tomography. Indeed, given the ambiguity of physical locality in RQM, the interpretation seems if anything friendly to abandoning that assumption.

Might we exchange local tomography for a more justifiable premise? (As Wilce has noted, there is a certain “à la carte” aspect to quantum reconstructions, in that axioms can sometimes be swapped out for one another [42].) Barnum, Müller and Ududec have presented an axiomatic derivation of the quantum formalism with a single-system focus [43]. In this approach, later refined by Barnum and Hilgert [44], local tomography is replaced by the observability of energy. Essentially, Barnum and coauthors ask that time-evolution maps be continuous and reversible, and that the generator of any such evolution be an observable, conserved quantity. Could RQM avail itself of such an assumption? Given the concerns we explored in the previous two sections, the outlook is cloudy.

It is also possible to select complex vector spaces over real ones by imposing the condition that a complete set of equiangular lines exist in the four-dimensional case, which amounts to assuming that a particular type of joint measurement is possible upon a pair of elementary systems [28]. The RQM literature’s overall tendency to identify “measurements” with orthonormal bases rather than more general POVMs would incline against a condition of this type.

VI. DISCUSSION

There is a fundamental tension in RQM, as the official writings have had it. On the one hand, the ontology is built from events, and properties instantiate in flashes. Being is discontinuous, stochastic and staccato. On the other hand, probabilities are factive, physicalized, made into properties that sway with all the smoothness of Maxwellian waves. To the extent that the RQM literature’s discussion of event timing has any content — that is, assuming the loose-frame loopholes can somehow be closed — it tells us that probabilities evolve continuously, and so, something of a material kind must always be rolling smoothly. Yet, elsewhere, the physical stuff of nature is said to hop, skip and jump. Does nature facit saltum, or not?

Rovelli has written that loop quantum gravity has the advantage over string theory that the former “addressed upfront the problem of describing the fundamental degrees of freedom of a theory without a fixed background spacetime”, while the latter treats this in an “indirect” way ([45], via [46]). We should apply the same standard to the reconstruction of quantum theory itself. The postulates offered by RQM have no purchase upon Bell–Kochen–Specker phenomena. Only by invoking additional conditions, whose support from RQM is at best equivocal, can we eventually arrive at the quantum formalism, from which we can then prove the violation of a Bell inequality in the standard way. What does this exercise in indirection teach us? Shouldn’t we rather address upfront the idea of a theory without a background of hidden variables?

My informal impression of the philosophy-of-physics community is that RQM has generally skated through, drawing more sympathy than criticism. RQM offers a conceptual tranquilizer, promising that we will be able to understand quantum mechanics without having to make ourselves too uncomfortable. However, resting in this tranquil atmosphere has not provided much in the way of insight. Reconstructing quantum theory from physical
principles could change everything from how we theorize about black holes to how we write undergraduate textbooks. Yet the principles in the derivation that Rovelli deems “particularly successful” are variously satisfied by classical theories, unsupported by RQM, or in conflict with it outright. The tangible benefit of the hard work of reconstruction is the highlighting of the clouds and contradictions in the RQM literature.

I thank C. A. Fuchs, P. A. Höhn, N. D. Mermin, J. L. Pienaar and D. Schmid for helpful comments on various versions of this paper.

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