Is It Possible to Nominalize Quantum Mechanics?

Otávio Bueno†‡

Hartry Field (1980) has developed an interesting nominalization strategy for Newtonian gravitation theory—a strategy that reformulates the theory without quantification over abstract entities. According to David Malament (1982), Field's strategy cannot be extended to quantum mechanics (QM), and so it only has a limited scope. In a recent work, Mark Balaguer has responded to Malament's challenge by indicating how QM can be nominalized, and by "doing much of the work needed to provide the nominalization" (Balaguer 1998, 114). In this paper, I critically assess Balaguer's proposal, and argue that it ultimately fails. Balaguer's strategy is incompatible with a number of interpretations of QM, in particular with Bas van Fraassen's version of the modal interpretation. And given that Balaguer's strategy invokes physically real propensities, it is unclear whether it is even compatible with nominalism. I conclude that the nominalization of QM remains a major problem for the nominalist.

1. Introduction. In the last two decades, we have witnessed a growing interest in nominalism in the philosophy of mathematics. This revival arose, in particular, from Hartry Field's provocative nominalization program (Field 1980 and 1989). As Field argued, it is possible to provide reformulations of certain physical theories without any quantification over mathematical objects. In particular, by introducing suitable comparative predicates, Newtonian gravitation theory can be reformulated in such a way that, instead of quantifying over real numbers, only quantification over space-time regions is found.

Even if Field succeeds in his nominalistic treatment of Newtonian gravitation theory, the question arises as to how far Field's approach can be extended. And the charge has been made that it cannot go too far. According to David Malament (1982), Field's nominalization strategy cannot be extended to quantum mechanics (QM), given that—as opposed to what happens with Newtonian gravitation theory—there are *no* suitable nominalist surrogates to quantify over. Given the importance of QM to our overall picture of the world, it is arguably crucial that the nominalist provides a nominalistically acceptable version of that theory.

In a recent work, Mark Balaguer claims to have responded to Malament's objection, by explaining how QM can be nominalized, and by "doing much of the work needed to provide the nominalization" (Balaguer 1998, 114). In this paper, I critically assess Balaguer's proposal, and argue that it ultimately fails. As Balaguer acknowledges, he has *not* completely nominalized QM, but only recovered, in "nominalistic" terms, the algebraic structure of Hilbert spaces. But in order to recover this structure, Balaguer is committed to the claim that "quantum probability statements are about *physically real propensities* of quantum systems" (1998, 120; emphasis added). To say the least, it is questionable whether the commitment to propensities, as real modalities in nature, is an advance at all. This is inconsistent, for instance, with some interpretations of QM—such as Bas van Fraassen's modal interpretation (1991, 273–337). And despite Balaguer's remarks to the contrary, it is unclear that a commitment to real propensities is something that a nominalist can accept. In what follows, I elaborate on these points, indicating the difficulties faced by Balaguer's proposal, and why the nominalization of QM still remains an open problem for the nominalist.

Before moving on, it is important to acknowledge two crucial desiderata for any nominalization strategy for science: (1) A nominalization strategy should be *neutral*. That is, the nominalistic version of a theory T should not settle issues left open by T. Otherwise, instead of providing a nominalization of T, we may end up developing a *rival theory* T′—if new empirical consequences are obtained from T′. Alternatively, if no new empirical consequences are obtained, but T′ settles issues that T leaves open, T′ ends up providing a *different interpretation* of T, instead of simply developing a *nominalistic version* of T (that is, a version of T that does not presuppose the existence of abstract entities). (2) A nominalization strategy should be *ontologically parsimonious*. That is, it should not presuppose nominalistically unacceptable items. Otherwise, the point of providing a nominalization strategy would be defeated.

2. Field's Nominalization Strategy. Balaguer's nominalization strategy is a version of the nominalization program developed by Field (1980 and 1989). There are two steps in Field's strategy. The first step is to claim that a mathematical theory need not be true to be good. It only needs to be *conservative*. A mathematical theory is *conservative* if it is consistent with every internally consistent theory about the physical world. Now, let M be

†To contact the author, please write to: Department of Philosophy, University of South Carolina, Columbia, SC 29208; e-mail: obueno@sc.edu.

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a conservative mathematical theory, and let N be a body of nominalistic claims (i.e., claims that do not refer to mathematical entities). It then follows that every nominalistic consequence obtained from M and N can be obtained from N alone (Field 1980). As a result, if mathematical theories are conservative, mathematical entities are ultimately dispensable in science. Of course, this is only the case if we are in a position to produce the relevant nominalistic premises. And this brings the second step of Field's program: to yield nominalistic premises for particular physical theories. Field (1980) did provide such premises—at least in the case of Newtonian gravitation theory.

Field's program emerged from Hilbert's work in the foundations of geometry (Hilbert 1971). Hilbert developed a synthetic formulation of geometry, which dispenses with metric concepts, and therefore does not include any quantification over real numbers. His axiomatization was based on concepts such as *point, betweenness,* and *congruence.* In the axiomatization, a point y is *between* the points x and z if y is a point in the line-segment whose endpoints are x and z. And the line-segment xy is *congruent* to the line-segment zw if the distance from the point x to the point y is the same as that from the point z to w. After studying the formal properties of the resulting system, Hilbert proved a representation theorem. Given a model of Hilbert's axiom system for space, there is a function d from pairs of points onto nonnegative real numbers such that the following "homomorphism conditions" are met:

(i) xy is congruent to zw iff $d(x,y) = d(z,w)$
(ii) y is between x and z iff $d(x,y) + d(y,z) = d(x,z)$.

As a result, if the function d is taken to represent distance, we obtain the expected results about congruence and betweenness. Ultimately, Field's program is an extension to physics of Hilbert's approach to geometry. By identifying suitable comparative predicates and quantifying over space-time regions, Field (1980) showed how such a program could be implemented in the case of Newtonian gravitation theory.

It is at this point that Malament's objection surfaces. Even if we grant that Field's strategy succeeds in the case of Newtonian gravitation theory, can Field's approach be extended to QM? According to Malament, the answer is *negative:*

> I do not see how Field can get started [nominalizing QM] at all. I suppose one can think of the theory as determining a set of models—each a Hilbert space. But what form would the recovery (i.e., representation) theorem take? The only possibility that comes to mind is a theorem of the sort sought by Jauch, Piron, *et al.* They start with "propositions" (or "eventualities") and lattice-theoretic relations as primitive, and then seek to prove that the lattice of propositions is necessarily isomorphic to the lattice of subspaces of some Hilbert space. But of course no theorem of the sort would be of any use to Field. What could be worse than *propositions* (or *eventualities*)? (Malament 1982, 534)

This is a formidable challenge. And to respond to this challenge, Balaguer developed his nominalization strategy.

3. Balaguer's Nominalization of QM. The main thesis of Balaguer's strategy is clear:

> [T]he closed subspaces of our Hilbert spaces can be taken as representing *physically real properties* of quantum systems. In particular, they represent *propensity* properties, for example, the r-strengthed propensity of a state-Ψ system to yield a value in Δ for a measurement of A (or, to give a more concrete example, the 0.5-strengthed propensity of a z+ electron to be measured spin-up in the x direction). (Balaguer 1998, 120)

So, just as Field quantified over space-time regions to avoid ontological commitment to real numbers, Balaguer quantifies over propensity properties to avoid commitment to vectors in Hilbert spaces. Balaguer then sketches how a representation theorem could be obtained in the case of QM (1998, 120–126).

For the purposes of this paper, I will grant that Balaguer succeeded in providing the relevant representation theorem.[1] My point is that, even if the theorem were established, Balaguer's strategy would still fall short of yielding a nominalization of QM. For there are problems at the *starting point*—the use of propensity properties. From now on, I will focus on the latter.

Balaguer is, of course, aware that propensity properties look suspicious. But his key idea consists in "taking propensities of the form (A, Δ, r) as basic and arguing that they are nominalistically kosher" (Balaguer 1998, 126).[2] Balaguer puts forward two arguments to the effect that propensities are nominalistically kosher. I'll call the first argument the *sufficient condition argument.* The argument comes in two parts: (i) Propensities are *physical* properties (i.e., properties of particular physical objects), and as such, they are *not* abstract, since they exist in space-time and are causally efficacious. In fact, Balaguer argues:

1. However, Balaguer (1998, 121) acknowledges that he only developed a sketch of the relevant result.

2. In Balaguer's notation, "(A, Δ, r)" denotes the quantum event that a measurement of observable A yields a value in a Borel set of real numbers Δ with probability r.

> If we consider a particular particle b, it seems that b's charge causes b to move about in certain ways in a magnetic field; but given this, it seems obvious that b's charge exists in b (although it might not have any exact location in b) and it seems almost crazy to say that it exists outside of spacetime. (Balaguer 1998, 127)

Moreover, propensities are nominalistically kosher because: (ii) Physical properties are *all* we need to nominalize QM. (In particular, no *abstract* properties are needed to run Balaguer's nominalization.) Here is Balaguer's argument for (ii). Initially, he argues that events (or propositions) *are not* nominalistically acceptable—and here Balaguer agrees with Malament:

> The important thing to notice here is the nominalistic benefit of switching from events (or propositions) to propensities. . . . If we're working with events, then in order to get the appropriate orthomodular lattice for a particular case (that is, for a particular set of mutually incompatible observables), we need to make use of the *complete* infinite set S(E) of events associated with these observables, and this will force us to speak of events that *haven't occurred* and, hence, to treat events as abstract objects. (Balaguer 1998, 125)

Note that, according to Balaguer, the fact that events have not occurred yet is sufficient to treat them as abstract objects. He then considers the changes brought by the introduction of propensities:

> But when we make the switch from events to propensities, we hold the state fixed and claim that each such state already generates a set S(P) of propensities that gives rise to the appropriate sort of structure. And since any actual quantum system is always in a particular state, this enables us to claim that any such system already has an infinite collection of propensities that gives rise to an appropriate sort of structure, that is, a (nominalistic) orthomodular lattice. In other words, all the propensities needed to generate an orthomodular lattice are already contained in a *single* quantum system. (Balaguer 1998, 125–126)

And given that a single quantum system already contains all the propensities required to yield the relevant lattice, nothing else is needed.

I'll call Balaguer's second argument to the effect that propensities are nominalistically acceptable the *eliminability condition argument.* The argument runs as follows:

> Propensities are just physical properties, like temperatures and lengths, and so we can get rid of them in the manner [put forward by Field]. . . . We can eliminate references to r-strengthed propensities by introducing propensity-relations that hold between quantum systems. That is, rather than building up structures from things like (Sx, +, 0.5),[3] we can build up structures from the quantum systems themselves. Thus, we will replace sentences like "State-Ψ electrons have r-strengthed propensities to yield values in Δ for measurements of A" with sentences like "State-Ψ electrons are (A,Δ)-*propensity-between* state-Ψ_1 electrons and state-Ψ_2 electrons." (Balaguer 1998, 126)

So, Balaguer's idea, similarly to Field's, is to introduce suitable *comparative predicates* holding between quantum systems, and in this way ultimately to dispense with r-strengthed propensities. As the above passage indicates, Balaguer's proposal depends crucially on establishing statements of the form: "state-Ψ electrons are (A,Δ)-*propensity-between* state-Ψ_1 electrons and state-Ψ_2 electrons." These statements are the *nominalistic* counterparts of *platonistic* quantum mechanical claims of the form "state-Ψ electrons have r-strengthed propensities to yield values in Δ for measurements of A." Thus, given that, in Balaguer's view, propensities are nominalistically kosher, this nominalization strategy goes through.

4. Problems with Balaguer's Nominalization.

4.1. Are Propensities Nominalistically Kosher? As we saw, Balaguer provided two arguments to the effect that propensities are nominalistically acceptable. The second argument stressed the dispensability of r-strengthed propensities—propensities linked to a particular probability assignment r. This type of propensity is problematic for the nominalist given its reference to numbers—the particular value of the probability assignment. As we saw, Balaguer's suggestion was to apply Field's nominalization strategy directly to such probability assignments, introducing comparative predicates over electrons, so that the nominalist could utter statements like "state-Ψ electrons are (A,Δ)-propensity between state-Ψ_1 electrons and state-Ψ_2 electrons."

The problem with this argument is this: to talk about an ordering of propensities, one presupposes that such an ordering is obtained based on the "propensity between" relation. How can such a relation be characterized without presupposing an ordering from the probability assignments themselves? Of course, one could use the ordering from the Borel set Δ of real numbers. But this presupposes a commitment to numbers, which is *not* an option for the nominalist. Note the difference between the ordering suggested by Balaguer and the corresponding ordering put forward by Hilbert in his formulation of geometry: a point y is between points x and z if y is a

3. "(Sx, +, 0.5)" denotes the 0.5-strengthed propensity of a z+ electron to be measured spin-up in the x direction.

point in the line segment whose endpoints are x and z. The only commitment here is to the existence of an intermediate point between points x and z. In other words, *there is no need to move beyond points* to obtain the *between* relation. In contrast, the (A,Δ)-*propensity between* relation is tied to the Borel set of real numbers Δ, and its proper characterization *cannot be provided without reference to such a set*. After all, the propensity under consideration is the propensity that state-Ψ electrons have an observable A with values in Δ (Balaguer 1998, 120).

But is the reference to a Borel set of real numbers acceptable to the nominalist? Balaguer is, of course, aware of the issue, and he claims that one can completely dispense with Δ in considering propensities. After all,

> we will replace sentences like "There is a probability of 0.75 that a state-Ψ electron will yield a value in the closed interval [m_1, m_2] for a measurement of momentum" with sentences like "A state-Ψ electron has a 0.75-strengthed propensity to be momentum-greater-than-or-equal-to a state-Ψ_1 electron and momentum-less-than-or-equal-to a state-Ψ_2 electron," where Ψ_1 is the state of having a momentum value of m_1 and Ψ_2 is the state of having a momentum value of m_2. (Balaguer 1998, 124)

But what are the momentum values of m_1 and m_2? Clearly, m_1 and m_2, being values of momentum, are abstract entities (particular numbers). And so, the attempt to dispense with the real numbers in the Borel set Δ simply replaced one platonistic structure with another.

Balaguer's *first* argument to the effect that propensities are nominalistically acceptable does not fare better than the second argument. As we saw, the argument rested on the assumptions that *physical* properties are *not* abstract and they provide all we need to nominalize QM. After all, "any actual quantum system already has an infinite collection of propensities that give rise to an appropriate sort of structure, that is, a (nominalistic) orthomodular lattice" (Balaguer 1998, 125–126).

The problem with this argument is that Balaguer's claim to the effect that physical properties are not abstract—given that they are causally efficacious—is incompatible with the claim that propensities provide all we need to nominalize QM. For an *infinite collection of propensities* is *not something causally efficacious*. A propensity is only a particular *disposition* to behave in a given way; it is not something that in itself has already happened, or has been actualized.

In response, perhaps Balaguer could argue that propensities *are* substantially different from abstract objects. After all, propensities are actual dispositions, and *as dispositions,* they are completely located in space-time. Consider a simple example. This handful of salt on the table has the disposition or propensity to dissolve if I added a sufficient amount of water—this propensity is the result of the chemical composition of the salt, and as such it is an actual, physical propensity of the salt.

The problem is that we cannot simply stop here. The disposition or propensity presupposes a *modal* component:

> (A) If I *were* to add enough water to the salt, the latter *would* dissolve.

What is the status of *this* claim? In assigning a propensity to the salt, we are presupposing that a claim such as (A) is true. But in virtue of what is it true? Clearly (A)'s truth cannot be the outcome of what goes on in a *possible* world, for a possible world is not *actual* (even if it is thought to be concrete, as is the case of Lewis 1986). In fact, there are different conceptions of possible worlds, but in most conceptions, a possible world is taken to be an *abstract* object (being represented by mathematical objects). And in those conceptions according to which possible worlds are not abstract (Lewis 1986, or Armstrong 1989), *possible worlds are not actual either* (except, of course, for the actual world!). And so, if Balaguer is right about his claim that events are not nominalistically acceptable in general—because some events "haven't occurred and, hence, [we have] to treat events as abstract entities" (1998, 25)—he is forced to assert the same conclusion for dispositions and propensities.

4.2. The Incompatibility between Balaguer's Strategy and the Modal Interpretation. Even if propensities were nominalistically kosher, Balaguer's proposal faces additional problems—in particular the proposal seems to clash with the modal interpretation.

The modal interpretation was articulated as an alternative to von Neumann's interpretation rule (von Neumann 1932, and van Fraassen 1991, 274–278). Von Neumann's rule provides a link between observables and states of a physical system, indicating how to read assignments of values to observables. The link is as follows:

> Observable B has value b if and only if a measurement of B is certain to have outcome b.

Note the logical form of this interpretation rule: a biconditional that links assignments of values to observables and outcomes of measurements. The (apparent) classical flavor of this link is noticeable. But what happens if a measurement is not certain to have a given outcome? In this case, the observable has no value. The classical flavor of the interpretation is thus deceptive: not all observables have values after all—unmeasured observables have no value, excluding the case of certainty (van Fraassen 1991, 274).

In contrast, the modal interpretation distinguishes between two concepts of state: *value* and *dynamic states*. The distinction is motivated by the fact that, in the case of QM, we cannot assume (a) that observables have values, "there to be seen if we look," nor can we assume (b) that the evolution of the physical system is determined completely by what those values are. These assumptions may have been taken for granted in the case of classical mechanics, but the picture changes once we move to the quantum domain. To each of these assumptions, there corresponds a particular concept of state. The *value state* is fully specified by stating which observables have values, and which they are; the *dynamic state* is completely determined by stating how the system will develop if acted upon in a particular way, and how it will develop if isolated (van Fraassen 1991, 275).

Corresponding to these two concepts of state, we have two types of propositions: A *value-attributing proposition,* denoted by $\langle m, E\rangle$, states that observable m actually has a value in E (where E is, typically, a Borel set); A *state-attributing proposition,* denoted by $[m, E]$, maintains that the state is such that a measurement of m must have a value in E (van Fraassen 1991, 275). These propositions have a well-determined body of truth makers: value-attributing propositions are true (or false) depending on value states, whereas the truth values of state attributions depend on dynamic states.

If von Neumann's interpretation rule is rejected, the equivalence between value-attributing and state-attributing propositions is denied. Only one side of the biconditional holds. But which side? Since measurement outcomes are relevant to the state the system is in, it is natural that $[m, E]$ implies $\langle m, E\rangle$; that is, if the state is such that a measurement of m must have a value in E, then m does have a value in E (van Fraassen 1991, 276). In other words, in von Neumann's interpretation rule only the right-to-left conditional holds. This allows van Fraassen to introduce *unsharp* values to observables. After all, if $[m, E]$ is not true (that is, if the state is not such that m must have a value in E), it is still possible that m does have a value in E, although this value may be "unsharp" (van Fraassen 1991, 276, 282–283). Furthermore, crucial information about the physical system is provided by the dynamic state, which remains the basic state to consider, since it gives information about the system's evolution. In this sense, the importance of the actual values of observables derives from the fact that they provide indications about the dynamic state. And once we know *that* state, we are in a position to know how the system will evolve.

As should be clear from the presentation above, van Fraassen's formulation of the modal interpretation *does not* presuppose the existence of real modalities in nature—in particular it *does not* presuppose physically real propensities. Modal notions are, at best, features of the models (van Fraassen 1989 and 1991). The fact that Balaguer's nominalization strategy presupposes the existence of such propensities clearly indicates that this strategy is incompatible with the modal interpretation.

4.3. The Problem with the Broad Claim. Prima facie, the incompatibility of Balaguer's nominalization strategy with the modal interpretation may seem odd. After all, the crucial component of Balaguer's approach highlights the role of modal notions in QM. Quantum theory is inherently modal in Balaguer's view. Of course, this is a fact about QM highlighted by a number of interpretations of the theory (except, perhaps, by the Bohmian view). Balaguer insists on the importance of probability assignments in quantum theory. Given that probability is a modality—a possibility with degrees, as van Fraassen (1980) would say—by emphasizing the role of probability in QM, Balaguer is also emphasizing the importance of modality in the characterization of the theory.

However, despite Balaguer's remarks to the contrary, his nominalization strategy *presupposes* a particular interpretation of QM, and as I indicated, it is incompatible with an important interpretation of the theory. Not surprisingly, Balaguer himself addresses the issue of whether his commitment to propensities ends up being incompatible with some interpretations of QM:

> Does this mean that I am committed to a propensity interpretation of QM? No. First of all, the *most* I am committed to here is the very broad claim—let's call it BC—that quantum probability statements are about physically real propensities of quantum systems; but BC can be understood in a very weak way, a way that makes it seem very plausible; in particular, it can be understood as saying simply that quantum systems are irreducibly probabilistic, or indeterministic; thus, it seems to me that BC is compatible with all interpretations of QM except for hidden variables interpretations and, moreover, that at present it is very widely accepted. And second, I'm not even committed to BC; I'm merely giving a strategy for nominalizing QM that assumes BC; there may be other ways to nominalize QM that *don't* assume BC, and if QM experts rejected (the weak reading of) BC, we could try to find one. (Balaguer 1998, 120)

Of course, BC is indispensable for Balaguer's nominalization strategy for QM. Without the claim that "quantum probability statements are about physically real propensities of quantum systems," there is no object the nominalist is able to quantify over in order to find a nominalistically acceptable replacement for quantum structures. The problem, however, is that the weak reading of BC is *unavailable* to Balaguer: the claim that "QM is irreducibly probabilistic" is compatible with a thoroughly anti-realist reading of probability, a reading that *denies* that there is any real modality in nature. For example, van Fraassen (1991) can easily grant that QM is irreducibly probabilistic, but he certainly would not accept that this entails that there are real propensities in nature (van Fraassen 1989). But without such propensities, Balaguer's nominalization strategy does not get off the ground. Similarly, without the existence of space-time regions (that is, without presupposing a substantivalist reading of space-time), Field's nominalization strategy for Newtonian gravitation theory also does not get off the ground.

So, despite Balaguer's remarks above, his nominalization strategy *does presuppose* BC. But BC is incompatible with the modal interpretation of QM—and in particular, as we saw, with van Fraassen's version of that interpretation.

4.4. The Dilemma about Interpretations of QM. The discussion above immediately yields the following dilemma: either Balaguer's strategy of nominalization of QM is *incompatible* with a certain family of interpretations of QM (the modal interpretation, Bohm's interpretation, and other hidden variables interpretations), or it is *compatible* with such a family of interpretations. If Balaguer's strategy is incompatible with a whole family of interpretations of QM, then the strategy is inadequate, since it is not capturing the underdetermination of interpretations typical of QM. As a result, the nominalization strategy is actually providing a *particular interpretation* of QM, rather than a way of nominalizing the theory. (Thus Balaguer's proposal violates desideratum (1), discussed in Section 1, above.) On the other hand, if Balaguer's strategy is compatible with a whole family of interpretations of QM—in particular, if it is compatible with van Fraassen's version of the modal interpretation—then there won't be suitable (nominalistically acceptable) replacements for quantum structures for Balaguer's strategy to succeed. After all, in van Fraassen's interpretation, there are no real propensities in nature, and without such propensities, Balaguer's account cannot get off the ground. In either case, the strategy does not seem to go through.

The upshot is that we should not expect to settle the issue about which interpretation of QM is adequate (or inadequate) based on whether nominalism in the philosophy of mathematics is true! There is something odd with a nominalization of QM that decides which interpretations of the theory should be rejected based on considerations about the falsity of platonism. In a sense, Balaguer acknowledges this point when he remarks that BC, the crucial assumption of his nominalization, is "compatible with all interpretations of QM" (1998, 120). But, alas, things do not work this way!

It might be argued that Field's own nominalization of Newtonian gravitation theory also was incompatible with a particular interpretation of the theory (a relationist view of space-time). However, the argument goes, this was never taken to be a reason to reject Field's nominalization strategy. I think there are two problems with the analogy between the strategies

developed by Field and Balaguer. First, Field (1989) indicates the reasons why we should prefer a substantivalist view of space-time *independently of the issue of nominalism*. And so, it has never been part of Field's view simply to decide the debate between substantivalists and relationists based on the truth of nominalism in the philosophy of mathematics. Without such considerations, Field's proposal would lose a great deal of its appeal. Second, as noted above, Field (1980) established a conservativeness result to the effect that mathematical theories will not yield new nominalistic consequences from nominalistic premises. As a result, Field's own account does satisfy desideratum (1) above.

The case of QM is, of course, a lot more delicate. Do we have any reason to prefer a propensity interpretation of QM? (As I argued above, anything *weaker* than the claim that there are real propensities in nature will not be of any help to Balaguer; otherwise, there will not be nominalistically acceptable surrogates for Balaguer's strategy to quantify over.) To say the least, it is far from obvious that we should prefer a propensity interpretation. Several perfectly defensible interpretations of QM do not presuppose the existence of real propensities in nature (van Fraassen's modal interpretation being one of them). Unless we have good reasons to reject such interpretations—independently of the nominalism issue in the philosophy of mathematics—Balaguer's proposal remains unmotivated.

5. Conclusion. How does Balaguer's account fare with regard to the desiderata discussed in Section 1? With regard to desideratum (1), is Balaguer's nominalization *neutral*? The answer is clearly *No.* After all, Balaguer's proposal entails the inadequacy of an important version of the modal interpretation of QM (not to mention hidden variables views). With regard to desideratum (2), is Balaguer's account *ontologically parsimonious*? Again, the answer seems to be negative. After all, as we saw, propensities are not compatible with a nominalist view.

In conclusion, Balaguer's attempt to provide a nominalization for QM does not succeed. And, at this point, it is not clear how to approach the nominalization of quantum theory by some other way. I conclude that the nominalization of QM remains a major problem for the nominalist.

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Quantum Mechanics and Ordinary Language: The Fuzzy Link

Peter J. Lewis†‡

It is widely acknowledged that the link between quantum language and ordinary language must be "fuzzier" than the traditional eigenstate-eigenvalue link. In the context of spontaneous-collapse theories, Albert and Loewer (1996) argue that the form of this fuzzy link is a matter of convention, and can be freely chosen to minimize anomalies for those theories. I defend the position that the form of the link is empirical, and could be such as to render collapse theories idle. This means that defenders of spontaneous-collapse theories must gamble that the actual form of the link renders such theories tenable.

1. Interpreting Quantum States. On a realist construal of quantum mechanics, the quantum state determines the truth-values of claims about macroscopic objects. But what precisely is the connection between quantum mechanical descriptions and ordinary language descriptions of objects? The connection suggested by a literal-minded reading of the quantum formalism is the so-called *eigenstate-eigenvalue link,* which says that an object has a given property if and only if its state is an eigenstate of the corresponding operator. But this is widely acknowledged to be at best an idealization, since the states of macroscopic objects will never be *precisely* eigenstates of the properties we tend to attribute to them, such as position.

However, there are various ways of formulating quantum mechanics such that the states of macroscopic objects are generally *close* to eigenstates of the properties we attribute to them; spontaneous-collapse theories in the GRW tradition (Ghirardi, Rimini, and Weber 1986) are paradigm examples.[1] The existence of such formulations suggests a connection be-

†To contact the author, please write: University of Miami, P. O. Box 248054, Coral Gables, FL 33124-4670; e-mail: plewis@miami.edu.

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1. It is worth noting that other formulations of quantum mechanics will need to loosen the link between eigenstates and ordinary language too. The results of this paper are not limited

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