Towards a Nominalization of Quantum Mechanics

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In this paper I will provide the beginnings of a nominalization of quantum mechanics, or QM. I will not completely nominalize QM, but I will provide what I think is the most important part of such a nominalization, namely, a nominalistic recovery of the algebraic structure of Hilbert spaces.

To *nominalize* QM is to reformulate it so that it no longer contains any reference to, or quantification over, abstract objects, i.e., objects which are aspatial, atemporal, acausal, mind-independent, etc. The reason for wanting to do this is to show that we can be realists about QM without believing in abstract objects.

To argue that our physical theories can be nominalized is not to argue that nominalism—i.e., the view that there are no abstract objects—is true. It is merely to rebut a certain argument *against* nominalism, viz., the Quine-Putnam indispensability argument. I do not think there are any good arguments *for* nominalism, and I have elsewhere (1995) responded to what is widely regarded as the best such argument, viz., Benacerraf's (1973) epistemological argument against the existence of abstract objects. And I should also note here that even if it turns out that empirical science cannot be nominalized, it will not necessarily mean that nominalism is false, because it may be that nominalists can admit that empirical science contains indispensable abstract-object talk, and simply *account* for this fact; indeed, in another paper (forthcoming) I have tried to do just that.

1. How Field nominalizes

The program of nominalization was initiated by Hartry Field (1980), who tried to nominalize Newtonian Gravitation Theory, a classical flat-spacetime theory. David Malament has argued (1982) that Field's method cannot be extended to cover QM. I think Malament is wrong, and below, I will explain why. The first thing to do, however, is explain Field's method. I will concentrate on Field's nominalization of physical quantities, such as temperature and length (Field 1980, Ch. 7). For the sake of simplicity, my presentation will differ somewhat from Field's. For example, I concentrate on length instead of temperature, and I take physical objects rather than spacetime points as the basic entities of my nominalistic structures; but the basic strategy is identical to Field's. I also note (as Field himself did) that my account depends heavily upon work carried out by Krantz, Luce, Suppes, and Tversky (1971) on the foundations of measurement.

For those who have worries about the nominalistic acceptability of spacetime points, my approach might seem superior to Field's. But, ultimately, we might have to use spacetime points anyway. That is, because I use physical objects rather than spacetime points, there are certain aspects of my construction—e.g., the fact that I appeal to concatenations of physical objects which, it would seem, haven't *actually* been performed—that might generate worries about nominalistic acceptability. In this section, I will gloss over such worries, because the reason I use physical objects rather than spacetime points is not that I think spacetime points are nominalistically unacceptable; it's merely that I think Field's strategy of nominalization can be more easily understood in terms of physical objects. Now I *will* consider several worries about nominalistic acceptability that arise from *other* sources, but I will not consider all the worries that one might reasonably have here; there is no need to consider all such worries, because the central aim of this section is not to provide a complete justification of Field's method of nominalization, but simply to familiarize the reader with his general strategy, so that, in later sections, I can address Malament's worry about QM.

I now turn to the task at hand. Ordinarily, when we state the lengths of physical objects, we do so in platonistic terms. We say, for instance, that Ralph's boat is fifty feet long. Thus, we seem to be committed here to the number 50 and also, perhaps, to a numerical functor. That is, length-in-feet can be thought of as a function f from physical objects to real numbers: to say that a boat b is fifty feet long is to say that $f(b) = 50$. And we also *quantify* over numbers in such settings: we say things like "Boat b is more than 50 feet long", which can be symbolized as $(\exists x)(x > 50 \wedge f(b) = x)$.

The basic idea behind Field's strategy for nominalizing such length assertions is to show how to state the length of a physical object by specifying relations it bears not to numbers but to other physical objects. Thus, we would say not that Ralph's boat stands in the foot relation to the number 50, but that it stands in the longer-than relation to Wanda's boat and the shorter-than relation to Warren's boat. Now, of course, this is not enough; if all we could do was compare two physical objects and say which was longer, we would not be able to reproduce what the numbers do for us; this would only give us an *ordering* of physical objects; to get the equivalent of exact length readings, we need to be able to say *how* *much* longer Ralph's boat is than Wanda's boat. To do this, we are going to have to construct an empirical structure which can be embedded in the mathematical structure that we're using here—i.e., the real number line—and then replace the latter with the former. I will now go through this slowly.

The first thing we need to do is define a *concatenation* operation $\circ$ on physical objects, which works in the obvious additive way; thus, if b and c are foot-long rulers, then $b \circ c$ is two feet long. This gives us a nominalistic way of saying that Ralph's boat is fifty feet long: if $a_1, \ldots, a_{50}$ are foot-long rulers, and b is Ralph's boat, then we can say that $(a_1 \circ a_2 \circ \ldots \circ a_{50}) \sim b$, where "$\sim$" means "is the same length as". We can make things easier by choosing some physical object u—e.g., the king's foot, or a stick in Paris—as the *unit* object. Thus, to say that Ralph's boat is fifty units long, we need merely say that it is the same length as u concatenated with itself forty-nine times; symbolically, this can be expressed as "$b \sim 50u$", where "$50u$" is just shorthand for "$u \circ u \circ \ldots \circ u$", where the concatenation operation is performed here forty-nine times. Finally, we can get more fine-grained length readings for physical objects by merely switching to a shorter unit object. For instance, to say that b is 50.5 us long, we need merely define one of the halves of u as u' (i.e., $u \sim 2u'$) and say that $b \sim 101u'$. Or if we want to say that b is 50.346 feet long, we just find a unit object c which is .001 feet long, and claim that b is the same length as $c \circ c \circ \ldots \circ c$ (where the concatenation operation is performed 50,345 times) i.e., that $b \sim 50{,}346c$. Now, obviously, there are pragmatic constraints on how small we can make the unit object; but this is irrelevant, because we are only trying to give a nominalistic treatment of the mathematics that we actually apply, and it's clear that our apparatus is going to give us a nominalistic way of expressing any length assertion that we could ever make, for any such claim will always be in terms of some unit that we actually use.

I've just claimed that our nominalistic apparatus is sufficient for our purposes. But this result can also be *proven* (in a metalanguage that allows platonistic terminology). The general strategy is as follows. First, we take the empirical structure consisting of (i) the set D of all physical objects and all finite concatenations of such objects, and (ii) the concatenation operation $\circ$ and the longer-than relation $\succ$, both of which are defined on D, and we call it E. That is, $E = \langle D, \succ, \circ \rangle$. To show that our nominalistic apparatus is acceptable, we have to construct a homomorphism[1] Φ which takes E into R—where $R = \langle Re, >, + \rangle$, Re is the set of real numbers, $>$ is

[1] The reason Φ is a homomorphism rather than an isomorphism is that there can be many physical objects of the same length and, hence, many physical objects associated with the same real number.

the usual greater-than relation, and + is the usual addition operation—in such a way that > preserves the important properties of $\succ$, and + preserves the important properties of $\circ$. The main property of $\succ$ that needs to be preserved is this: for all *b*, *c* such that $b \succ c$, there is some (sufficiently short) object *u* such that $b \succ nu \succ c$ (where "*nu*" is shorthand for "$u \circ u \circ \ldots \circ u$", where the concatenation operation is performed here *n* minus 1 times).[2] For our Φ to preserve this, it must be that in any such situation, $\Phi(b) > n\Phi(u) > \Phi(c)$. As far as $\circ$ is concerned, what we need to demand is that $\Phi(b \circ c) = \Phi(b) + \Phi(c)$. To construct a homomorphism Φ which satisfies these constraints is to prove a *representation theorem*; the name is supposed to suggest that, once the proof is carried out, facts about R can be used to represent facts about E, i.e., that purely physical length facts about the physical objects in D can be stated in terms of real numbers.

There are many different homomorphisms which would serve our needs here, i.e., which would satisfy the constraints mentioned in the last paragraph. A *uniqueness theorem* tells us what all of these homomorphisms have in common; more precisely, it tells us which sorts of *transformations* of our homomorphism Φ are acceptable. It turns out that, in the case of length, all and only *similarity* transformations are permissible; in other words, if Φ is a homomorphism from E into R which satisfies the constraints discussed above, then if Φ′ is also such a homomorphism, then there is some positive real number *c* such that $\Phi' = c\Phi$. (With different physical quantities, different sorts of transformations are permissible; with temperature, for instance, *affine* transformations are permissible; that is, for any two acceptable homomorphisms Φ and Φ′, there is a real number *b* and a positive real number *c* such that $\Phi' = c\Phi + b$. All of this is just a technical way of stating the obvious facts that (a) any two acceptable length scales, e.g., the foot and inch scales, differ only in the length of the unit, and (b) any two acceptable temperature scales, e.g., Celsius and Fahrenheit, differ only in the size of the degree and the zero point.)

In a nutshell, then, to prove that my nominalization of length is acceptable, I would have to state intuitively plausible axioms about E which would enable me to prove a representation theorem between E and R and a corresponding uniqueness theorem. I will not discuss how exactly this is to be done. It is discussed by Field (1980) and, in much more detail, by Krantz, Luce, Suppes, and Tversky (1971).

Once these theorems are proven, we can use real numbers to state facts about length without believing in the numbers. For we can treat "*b* is 50 feet long" as shorthand for "*b* is the same length as a foot-long ruler concatenated with itself forty-nine times". In short, the point is that if we wanted to, we could say everything we need to say about the lengths of physical objects in nominalistic terms, i.e., by referring only to physical objects and using only nominalistic vocabulary, e.g., "$\succ$", "$\circ$", and "~". (Of course, it's easier to speak in platonistic terms, but from an ontological point of view, that is irrelevant.)

[2] One might worry that this presupposes that matter can be carved up indefinitely. But we can ease this worry merely by shifting from talk of physical objects to talk of *parts* of physical objects (or, of course, to talk of points and regions of spacetime).

One might object that the function Φ, the set D which forms the domain of our nominalistic structure E, and indeed, E itself—which is just an ordered triple—are abstract objects. But this is irrelevant, because nominalists need not believe that any of these things really exist, because they need not believe that their representation theorems, or the proofs of these theorems, are true. All they need to believe is their nominalistic reconstruction of science; but Φ, D, and E do not appear in this reconstruction; they only appear in the proof of the representation theorem for length. In other words, our various representation theorems are not part of our nominalistic reconstruction of science; they are only part of the argument for the claim that that reconstruction is adequate. In other words, representation theorems are designed to convince *platonists* of the adequacy of a given nominalization; they are not part of that nominalization.

Nominalists might try to salvage a nominalistic structure that they can believe in by using Goodmanian sums. They could define E_G as the Goodmanian-sum ordered triple $[D_G, \succ, \circ]$, where D_G is the Goodmanian sum of all physical objects and all finite concatenations of such objects, and $\succ$ and $\circ$ are defined on the parts of D_G. But there doesn't seem to be any reason to go to this trouble, or to bring up the controversial issue of Goodmanian sums, because we are still going to have to take the attitude of the last paragraph with respect to the homomorphism Φ.[3] Thus, we might as well take the same attitude with respect to E and D, because nominalists have no need for a nominalistic structure that they can believe in. They can maintain that they only believe in the objects in D, and that talk of the *sets* D and E serves merely to aid the proof of the claim that it's acceptable to believe only in those objects.

I have only spoken here of the nominalization of physical quantities, such as length. Field did much more than this: he provided nominalistic statements of many *laws involving such quantities*. I will not go into this here, because I have already given enough background to motivate Malament's objection to Field's program, i.e., to see why Malament thinks that Field's method cannot be extended to cover QM.

[3] There's no way to avoid this. The representation theorems we're concerned with establish that a certain relationship holds between a platonistic structure and a nominalistic structure. Thus, to prove these theorems, we are going to have to talk about the abstract objects contained in the platonistic structures, and so there is no sense trying to avoid the claim that Φ is an abstract object.

2. Malament's objection

Malament's argument for the claim that Field's program cannot be extended to QM is short enough to quote in its entirety:

> 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*)? (1982, p. 534)

This objection might seem a bit obscure to those who don't know much about QM, but in this section I will explain exactly what Malament is worried about.

The most important mathematical structures used in QM are Hilbert spaces, and the main use of Hilbert spaces is for representation. For example, we represent the possible pure states of quantum systems with vectors in Hilbert spaces, and we represent observable quantities of quantum systems (e.g., position and spin) with Hermitian operators defined on the vectors of Hilbert spaces. But most important is the representation of *quantum events* (or *propositions*) with closed subspaces of Hilbert spaces: if we let "A" denote some observable, "Δ" denote some Borel set of real numbers that can be values of A, and "(A,Δ)" denote the quantum event of a measurement of A yielding a value in Δ (or equivalently, the proposition which asserts that this event has occurred, or perhaps, will occur) then we can represent (A,Δ) with the closed subspace $CS(A,\Delta)$ of the Hilbert space H in which A is represented, where $CS(A,\Delta)$ is defined as follows: a vector v of H is in $CS(A,\Delta)$ iff there is a probability of 1 that a measurement of A, for a quantum system in the state represented by v, will yield a value in Δ.

Note the use of probabilities here. In classical mechanics, we can think of a state as a function from propositions of the above sort to truth values. QM, however, is a probabilistic theory: it does not (in general) predict with certainty how a quantum system in some given state will behave when we measure it. Thus, instead of thinking of quantum states as functions from propositions to truth values, we think of them as functions from propositions to *probabilities*, i.e., to [0, 1]. Thus, a given quantum state ψ will assign to each proposition, or event, (A,Δ) a real number r in [0,1]; r is the probability that the event (A,Δ) will occur if a state-ψ system is measured for A (or that the corresponding proposition will be true). Thus, each quantum state determines a probability function from quantum propositions (or events) to [0,1]. (The rule for calculating the exact probability r that a particular quantum state ψ assigns to a particular proposition, or event, (A,Δ) is this: r is equal to the inner product of the vector v that represents ψ and the vector v' that results from projecting v onto $CS(A,\Delta)$.)

Applying all of this to a concrete example, if we let "z+" denote the spin-up-in-the-z-direction state, "Sx" denote the spin-in-the-x-direction observable, and "+" denote the spin-up value, then (assuming that z is orthogonal to x) z+ determines a function p_{z+} such that $p_{z+}(Sx, +) = 0.5$. Thus, according to the above manner of correlating quantum events and closed subspaces of Hilbert spaces, we will associate the event (Sx, +) with the closed subspace CS(Sx, +) which contains a given vector v iff v represents a state ψ which determines a probability function p_ψ such that $p_\psi(Sx, +) = 1$.

The last few paragraphs tell us that the following two sets are in one-one correspondence:

S(H), the set of closed subspaces of a Hilbert space H,

and

S(E), a certain set of quantum events (or propositions).

But more needs to be said about S(E), i.e., about precisely which events are contained therein. S(E) is *not* the set of *all* quantum events of the form (A,Δ); rather, it is the set of events (A,Δ) *associated with a given set of mutually incompatible observables*. In the present context, it doesn't really matter what this comes to, because nothing important is going to hang on it. But loosely speaking, two observables are *incompatible* if and only if QM never assigns them determinate values for the same quantum system at the same time. An example of a pair of incompatible observables is position and momentum. But there are also larger sets of mutually incompatible observables, e.g., the set SPN of all of the infinitely many spin-1/2 observables. Now, of course, if we take one of these spin-observables out of SPN, the resulting set will still be a set of mutually incompatible observables; but let us ignore such sets and restrict our attention to classes of mutually incompatible quantum observables which are *maximal* in the sense that there are no observables which are not in the set but which are incompatible with all the observables in the set. It turns out that the set S(E) of quantum events associated with a given class of this sort—i.e., a (maximal) class of mutually incompatible quantum observables, e.g., the set SPN, or the set containing position and momentum—is in one-one correspondence with the set S(H) of closed subspaces of the Hilbert space H in which the given class of observables is represented. Indeed, a much stronger relation holds here: we can define lattice-theoretic predicates on S(H) and S(E) and thereby construct *(non-distributive) orthomodular lattices* out of these two sets which are isomorphic to one another. We can call these orthomodular lattices L(H) and L(E), respectively.

It would take quite a bit of space to give a precise definition of "orthomodular lattice", but in the present context, there is no need to be very precise. All I will say is that an orthomodular lattice is a special sort of *partially ordered set*, where a partially ordered set is an ordered pair <A, ≤>, where A is a non-empty set and ≤ is a reflexive, transitive, antisymmetric relation defined on A. (In our lattices, "≤" will mean "*is included in*"; thus, if *a* and *b* are closed subspaces in S(H), then to say that $a \leq_H b$ is to say that all the vectors in *a* are also in *b*; and if *a* and *b* are events in S(E), then to say that $a \leq_E b$ is to say that whenever *a* occurs, *b* also occurs.) An orthomodular lattice is a partially ordered set that satisfies certain further conditions, e.g., that of having a maximum element and a minimum element, i.e., an ≤-most element and an ≤-least element.[4]

How can we nominalize the parts of QM that use Hilbert spaces in the ways I have been describing? In § 1, we saw that the strategy for nominalizing is to produce a nominalistic structure which can be embedded in the platonistic mathematical structure being used. Thus, in the present case, what we want to do is produce a nominalistic structure which is embeddable in L(H); doing this will show that we can take L(H) as a mere representational device, i.e., as a means of representing various features of our nominalistic structure. In this light, it is easy to appreciate Malament's worry. For as things have been set up, it seems that the closed subspaces that are the elements of L(H) are being used to represent things which are *not* nominalistically kosher, viz., quantum events (or propositions). In other words, the obvious representation theorem which suggests itself is one which obtains between L(H) and L(E); but the problem is that L(E) is *not* a nominalistic structure, because the members of S(E)—whether we take them to be propositions or events—are *abstract objects*. (One might wonder why we cannot take events as nominalistically kosher, since they occur in spacetime. The reason is that there aren't *enough* events which have *already* occurred: in order to get the full structure of an orthomodular lattice, we are going to have to make use of *all* the events in S(E); but many of these events have never occurred, and so we are going to have to take the events in S(E) as abstract objects.) Thus, to replace L(H) with L(E) is just to replace one platonistic structure with another. This is Malament's worry.

[4] For a precise definition of "orthomodular lattice", see Hughes (1989, § 7.3). It is worth noting that the set of events associated with a *single* observable can be structured into a Boolean algebra; it is only when we "paste" two or more of these together that we get the weird non-distributive structures of QM.

What I need to do, then, is find a way of taking the closed subspaces of Hilbert spaces as representing *physical* phenomena of some sort or other; if I can do this, I should be able to construct nominalistic structures out of these physically real things and then prove representation theorems which enable me to replace the mathematical structures in question—i.e., the orthomodular lattices built up out of closed subspaces of Hilbert spaces—with these new nominalistic structures.

3. *The strategy for nominalizing QM*

My thesis is that the closed subspaces of our Hilbert spaces can be taken as representing *physically real properties* of quantum systems. In particular, they represent *propensity* properties, e.g., 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).

Does this mean that I am committed to a propensity interpretation of QM? No. First of all, the most I'm 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 which makes it seem very plausible; in particular, it can be understand 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 it is currently very widely accepted. And secondly, I'm not even committed to BC; I'm merely giving a strategy for nominalizing QM which assumes BC; there may be other ways to nominalize QM which don't assume BC, and if QM experts rejected (the weak reading of) BC, we could try to find one. For now, I merely want to undercut Malament's worry by showing how *one* nominalization of QM would go.

In any event, I need to establish two different claims in order to justify my thesis; first, I need to establish either

(1a) Propensities are nominalistically kosher,

or

(1b) References to propensities can themselves be nominalized away;

and second, I need to establish

(2) Propensities provide a means of nominalizing the parts of QM discussed in the last section; i.e., the closed subspaces of our Hilbert spaces can be taken as representing quantum propensities.

In this section, I will argue for (2) simply by showing how the nominalization goes; in the next section, I will argue (very quickly and sketchily) that (1b) is true and that, even if it isn't, (1a) is true.

In order to establish (2), I have to find, for each Hilbert space H that we use in QM, a set of propensities which corresponds to the set S(H) of closed subspaces of H, and I need to construct a nominalistic structure out of this set of propensities and then prove a representation theorem showing that this nominalistic structure is homomorphic to the orthomodular lattice L(H) built up out of S(H). I am not going to do *all* of this here; instead, I will (a) do some of it and (b) motivate the claim that it can all be done.

I begin by recalling that each quantum state can be thought of as a function from events (A,Δ) to probabilities, i.e., to [0,1]. Thus, each quantum state specifies a set of ordered pairs $<(A,\Delta), r>$. The next thing to notice is that each such ordered pair determines a propensity property of quantum systems, namely, an r-strengthed propensity to yield a value in Δ for a measurement of A. We can denote this propensity with "(A,Δ,r)".

Now, consider the set S(P) of propensities (A,Δ,r) associated with a particular quantum state ψ (and a particular (maximal) class of mutually incompatible observables). I claim that from the set S(P) we can construct a nominalistic orthomodular lattice L(P) which is homomorphic to the orthomodular lattice L(H) constructed from the set S(H) of closed subspaces of the Hilbert space H in which the given observables are represented. This claim can be justified by arguing that L(P) is homomorphic to L(E), i.e., the orthomodular lattice built up out of the set S(E) of quantum events associated with the given class of observables; for as we saw in § 1, L(E) is isomorphic to L(H). How, then, can I argue that L(P) is homomorphic to L(E)? To argue this point in the right way, I would need to provide precise characterizations of L(P) and L(E) and then state and prove a *representation theorem*. (Actually, I'd really need to prove infinitely many representation theorems; for every (maximal) class of mutually incompatible observables will generate a new L(E), and every quantum state associated with that L(E) will generate a different L(P), and my claim is that each of these L(P)s will be homomorphic to that L(E), so I would need to prove a different representation theorem for every L(E)-L(P) pair in QM.) I am not going to prove any of these theorems here. What I want to do instead is provide an informal argument for the claim that *all* of them *do* hold, i.e., that for each L(E)-L(P) pair in QM, L(P) is homomorphic to L(E). I will argue this point in two steps, one dealing with the *domains* of the two sorts of structures and one dealing with the *predicates* (or, to be more precise, the *non-logical expressions*).

The first step is to show that for any given L(E)-L(P) pair, there is a homomorphic correspondence between the domains of L(P) and L(E), i.e., between S(P) and S(E). This can be seen in the following way. S(E) is a set of quantum events (A,Δ). Now, if we choose a particular quantum state ψ, it will determine a probability function p_ψ which assigns to each (A,Δ) in S(E) a unique real number r in [0,1]. Thus, it seems clear that, when we *fix* the state of the system, each event (A,Δ) in S(E) is going to be associated with a *unique* propensity (A,Δ,r) and vice-versa; for (a) we can use the probability function associated with the state in question to assign a unique (A,Δ,r) to each (A,Δ), and (b) we can assign a unique (A,Δ) to each (A,Δ,r)—indeed, the very (A,Δ) to which (A,Δ,r) was assigned in (a)—by merely "erasing" the r.

This last sentence isn't exactly right, because it suggests that there is a one-one correspondence between S(P) and S(E)—i.e., between the (A,Δ,r)s associated with a given state and the (A,Δ)s—but there's actually a many-one correspondence here. The reason is that *every* state-ψ quantum system will have an (A,Δ,r)-type propensity. For instance, every state-z+ electron will have an (Sx, +, 0.5)-type propensity. The reason this is important is that in order to maintain that propensities are nominalistically kosher, I am going to have to treat the (Sx, +, 0.5)s associated with different electrons as *different things*; but there is only *one* (Sx, +), and so the correspondence between the (A,Δ)s on the one hand and the (A,Δ,r)s associated with a given quantum state on the other is going to be many-one rather than one-one. This is exactly analogous to the case of length, where the mapping from physical objects to real numbers is a homomorphism. I will return to this topic in § 4.

The second step of my argument for the claim that L(P) is homomorphic to L(E) is to show that there are nominalistic versions of our (platonistic) lattice-theoretic predicates and operator expressions. When we construct L(H), we do so by defining certain non-logical expressions—most notably, the two-place-relation predicate "is subspace-included in" and the unary-operation expression "the subspace-orthocomplement of"—on S(H); and when we construct L(E), we do so by defining analogous expressions—"is event-included in" and "the event-orthocomplement of"—on S(E). Thus, if I can show that we can lift nominalistic propensity expressions directly off of these platonistic expressions—just as we did in the case of length, when we lifted "$\succ$" and "$\circ$" off of "$>$" and "+"—then (given the result of the last two paragraphs) it would seem extremely plausible to suppose that we can use these expressions to build a lattice L(P) out of the set S(P) which is homomorphic to L(E). Let me begin with the predicate "is included in".

Corresponding to the platonistic predicate "is event-included in", or "$\leq_E$", we can introduce the nominalistic predicate "is propensity-included in", or "$\leq_P$". To say that one propensity is propensity-included in another—e.g., that $(A,\Delta,r) \leq_P (A',\Delta',r')$—is (on the definition I will propose) a *nominalistic* claim, because it is to say something *about quantum systems*; in particular, it is to state a *physical law* about quantum systems. But *what* law? Well, the first suggestion one might make is that $(A,\Delta,r) \leq_P (A',\Delta',r')$ iff it's a law of nature that any quantum system that has (A,Δ,r) also has (A',Δ',r'). But there is a technical difficulty with this definition of "$\leq_P$", and it turns out to be insufficient for the notion of propensity-inclusion that we want to capture. The problem is generated by the fact that propensities are indexed to particular states, or in other words, that the elements of L(P) are (A,Δ,r)s instead of (A,Δ)s; because of this, there will be cases where it is a law of nature that any system that has (A,Δ,r) also has (A',Δ',r'), but where this law holds not because the one propensity is *included* in the other, in the sense we're interested in, but simply because (a) the *only* quantum state that generates the probability r for (A,Δ) is the state ψ associated with the L(P) we're currently working with, and (b) ψ also happens to generate the probability r' for (A',Δ'). In other words, the law holds by accident in such cases, because the only L(P) in which (A,Δ,r) appears is the L(P) associated with ψ.

What I propose is simply to ignore the *r*s in defining "$\leq_P$". We can do this by simply lifting the definition of "$\leq_P$" directly off of the definition of "$\leq_E$". "$\leq_E$" can be defined as follows: $(A,\Delta) \leq_E (A',\Delta')$ iff for every quantum state ψ associated with the given L(E), $p_\psi(A,\Delta) \leq p_\psi(A',\Delta')$. Thus, we can define "$\leq_P$" as follows: $(A,\Delta,r) \leq_P (A',\Delta',r')$ iff it is a law of nature that every quantum system has a propensity to have a value in Δ for a measurement of the observable A which is weaker than, or equal in strength with, its propensity to have a value in Δ' for a measurement of the observable A'. (It is, perhaps, more intuitive to define "$\leq_E$" by saying that $(A,\Delta) \leq_E (A',\Delta')$ iff whenever (A,Δ) occurs, (A',Δ') also occurs. Analogously, we can define "$\leq_P$" by saying that $(A,\Delta,r) \leq_P (A',\Delta',r')$ iff it is a law of nature that any quantum system that has a value in Δ for A also has a value in Δ' for A'.)

Let me explain my strategy here a bit more carefully. Every (maximal) class of mutually incompatible observables in QM generates an orthomodular lattice L(E), and corresponding to each such L(E) is a whole collection of L(P) lattices—in particular, there is one for each state ψ associated with the given class of observables. Now, each of these L(P)s consists of a group of (A,Δ, r)s, and what's more, each (A,Δ) from the given L(E) appears in an (A,Δ,r) *exactly once* in each L(P). (We know this, because the (A,Δ,r)s in a given L(P) are generated from the (A,Δ)s in L(E) by means of a probability function that assigns exactly one real number r to each (A,Δ) in L(E).) Thus, the *only* differences between the various L(P)s associated with a given L(E) are the *r*s that are attached to the various (A,Δ)s. But we can now see that the inclusion patterns in the given L(E) are going to be reproduced in each of the L(P)s; for on the above definition of "$\leq_P$", the *r*s appearing in the various (A,Δ,r)s don't play any role at all in determining what propensities are propensity-included in what other propensities. (We know that this is acceptable, i.e., that no confusion will arise from this, because in any given L(P), each (A,Δ,r) can be uniquely picked out by A and Δ alone.) So whether or not some propensity (A,Δ,r) is propensity-included in some other propensity (A',Δ',r') depends *solely* upon facts about A, Δ, A', and Δ', and indeed, it depends upon the *same* facts that the question of whether $(A,\Delta) \leq_E (A',\Delta')$ depends upon. Thus, it is clear that each L(P) associated with a given L(E) will have the same inclusion patterns as the given L(E)—i.e., that for each such L(P), we will have $(A,\Delta,r) \leq_P (A',\Delta',r')$ iff we have $(A,\Delta) \leq_E (A',\Delta')$ in L(E). So it should also be clear that the above definition of "$\leq_P$" succeeds in capturing the notion of inclusion that we're after, i.e., that it succeeds in capturing the right extension.

Even if all this is granted, one might have misgivings about my definition of "$\leq_P$", for one might doubt that it is really nominalistically kosher. There are two different worries here.[5] First, one might wonder whether it is nominalistically acceptable to speak of a quantum system *having a propensity*, or having one propensity that's *stronger* than another. I will address this worry in § 4. For now, I will only concern myself with the second worry I have in mind, i.e., the worry that it is not nominalistically acceptable to appeal to *values in* Δ, because Δ is a *set* and values in Δ are *real numbers*. The reader may well have been worrying about the appeal to values in Δ all along, i.e., since the first paragraph of this section, when I first began speaking of r-strengthed propensities for yielding values in Δ for measurements of A. And, of course, the reader might *also* be worried about the real number r; I will address the worry about r in § 4 below, when I discuss the nominalistic acceptability of quantum propensities; right now, I will only address the worry about Δ.

It is not difficult to eliminate talk of values in Δ, because these values are values of a *physical quantity*—viz., A—and, therefore, we can dispense with them in the same way that we dispense with real numbers in

[5] Actually, there are *three* worries. The third worry concerns the question of whether nominalists can legitimately make use of the notion of a *law*. I think they can, but I will not try to justify this claim here, because I am only trying to argue that QM doesn't raise any special problems for nominalists, i.e., problems that they don't already have. If nominalists can't account for there being laws of nature, then it would be rather pointless to try to nominalize QM.

connection with other physical quantities, e.g., temperature and length. In other words, we can construct a nominalistic structure consisting of a domain of quantum systems together with *A*-predicates (e.g., "*A*-less-than") defined on this domain, and then formulate a set of axioms which enables us to prove a representation theorem between this structure and the ordinary platonistic one. Thus, we have an ordinary Field-style nominalization of the observable *A* embedded within my nominalization of the probability claims of QM. So, for example, 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. And, of course, the same sort of thing can be done to eliminate the appeal to values in Δ from the definition of "$\leq_P$".

To be sure, there will be differences between the nominalization of length and the nominalization of various quantum observables. But none of these differences raises any impediment to nominalization. For instance, whereas physical objects can take any real number as a value of length, spin-1/2 particles can only have two different values of spin, viz., 1/2 and -1/2. Thus, in nominalizing spin-1/2 observables, the relevant platonistic structure is not going to be the real number line, and our nominalistic predicates are not going to be analogous to our nominalistic length predicates—e.g., we won't be using "spin-less-than". But this doesn't create any problem, because we can simply use *different* nominalistic predicates—e.g., for the observable S*x*, we will want to use "S*x*-up" and "S*x*-down"—to build up a different sort of nominalistic structure.

So it seems to me that the above definition of "$\leq_P$" provides us with an acceptable nominalistic version of "$\leq_E$". Now, aside from "is included in", the only other lattice-theoretic non-logical expression for which we need to find a nominalistic version is the unary-operation expression "the orthocomplement of". (The binary operations "join" and "meet" can be defined in terms of inclusion; if *a* and *b* are elements of a lattice, then *a join b*—i.e., $a \vee b$—is the inclusion-least-upper-bound of *a* and *b*; and their meet is their inclusion-greatest-lower-bound.) I do not want to go into nearly as much detail in defining "$^{\perp P}$"—i.e., "the propensity-orthocomplement of"—as I did in defining "$\leq_P$". I simply want to note that (a) "the event-orthocomplement of", or "$\leq_E$", can be defined by saying that $(A,\Delta) = (A',\Delta')^{\perp P}$ iff for all states ψ, $p_\psi(A,\Delta) = 1$ iff $p_\psi(A',\Delta') = 0$; and so (b) "$^{\perp P}$" can be defined by saying that $(A,\Delta,r) = (A',\Delta',r')^{\perp P}$ iff it is a law of nature that a quantum system has the propensity $(A,\Delta,1)$ iff it also has the propensity $(A',\Delta',0)$.

This concludes my argument for the claim that the various L(P)s that I have described are homomorphic to the L(E)s they're associated with. Now, in order to *prove* that a given L(P) was homomorphic to its L(E), I would have to formally define a mapping Φ that took the given S(P) into the given S(E) and then prove that it was a homomorphism; that is, I would have to prove a representation theorem. I have not done this here, but it seems to me that I have shown that, in any given case, it could be done. For (a) I have made it very clear what the various Φs will look like—in any given case, Φ will take S(P) into S(E) by simply "erasing" the *r*s from the (A,Δ,r)s—and (b) I have given arguments which strongly suggest that these Φs will be homomorphisms. If all of this is correct—and if I can argue that either (a) propensities are nominalistically kosher, or (b) references to propensities can themselves be nominalized away—then Malament's worry has been refuted.

This does not constitute a complete nominalization of QM; what is left unnominalized is the dynamics of the theory—in particular, the Schrödinger Equation. But I don't see any reason why this can't be nominalized in the same general way that Field nominalizes the differential equations of Newtonian Gravitation Theory. It is not *trivial* that this can be done, but I do not foresee any impediments. In any event, Malament's worry has nothing to do with the dynamics of QM, and indeed, I do not know of any arguments against the nominalizability of the dynamics of QM. Thus, it seems to me that if what I am suggesting in this paper is correct, then it shows how the most important and problematic part of the nominalization of QM ought to go.

Any nominalization should come complete with a nominalistic *picture* of what is going on, and before I end this section, I would like to make sure that the picture I have in mind here is clear. My idea, in a nutshell, is this: every quantum system has a bunch of physically real propensities associated with various observables; moreover, since any quantum system is (at any given time) in some particular state ψ, it will always be the case that the collection S(P) of propensities which it *actually, presently has* with respect to a particular (maximal) set of mutually incompatible observables can be formed into a lattice L(P) that is homomorphic to the lattice L(H) which can be constructed from the closed subspaces of the Hilbert space H in which these observables are represented. The important thing here is the nominalistic benefit of switching from events (or propositions) to propensities. I pointed out above that if we're working with events, then in order to get the appropriate orthomodular lattice for a particular case (i.e., 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 which haven't occurred, and hence, to treat events as abstract objects. 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 which 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 which gives rise to an appropriate sort of structure, i.e., a (nominalistic) orthomodular lattice. In other words, all the propensities needed to generate an orthomodular lattice are already contained in a single quantum system. (Actually, every quantum system generates many L(P)-style lattices, one for each of its states; e.g., every system has a spin state which generates one infinite collection of propensities that can be formed into an orthomodular lattice and also a position/momentum state which generates another.)

4. The nominalistic status of propensities

I still need to argue that propensities are nominalistically kosher. (If they're not, then no progress will have been made—I will have merely replaced one platonistic structure with another.) The main worry that one might have here is, of course, that propensities are *properties*, and properties are abstract objects.

There are two strategies that nominalists can adopt here, and I think both are acceptable. The first strategy is to take quantum propensities of the form (A,Δ,r) as the basic entities of our nominalistic structures and simply argue that these things *are* nominalistically kosher. This is the strategy that I have been assuming throughout. But there is another way of proceeding which, I think, most readers will find less controversial, and that is simply to nominalize away the commitment to propensities and build up our nominalistic structures out of quantum systems themselves. That this can be done can be seen in the following way.

Propensities are just physical properties, like temperatures and lengths, and so we can get rid of them in the manner of § 1. The strategy of § 1 was not to introduce a continuum of *length properties* (e.g., being-5-feet-long, or being-17.3-feet-long) which is isomorphic to the real number line; it was, rather, to introduce the comparative length-relation $\succ$ and use this to order ordinary physical objects into a structure which can be embedded in the real number line. Thus, presumably, we can do the same thing here: 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 $(Sx, +, 0.5)$, 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." Now, I do not want to suggest that it is *trivial* that talk of propensities can be eliminated in this way, but I do not foresee any real problems. Thus, in short, it seems to me reasonable to suppose that if we can nominalize length and temperature in the manner of § 1, then we can also nominalize propensities in this manner.

But while it seems clear to me that this strategy will work, it also seems to me that the first strategy will work, i.e., the strategy of taking propensities of the form (A,Δ,r) as basic and arguing that they are nominalistically kosher. I do not have the space to argue this point adequately, but I would like to at least indicate the line of argument that I would use. First, I would carefully distinguish *physical properties*—i.e., properties of particular physical objects, e.g., the temperature of my tongue, or the 0.5-strengthed propensity of some particular $z+$ electron to be spin-up in the x-direction—from *properties-in-abstraction*, i.e., Platonic Forms. I would then admit that if there are any properties-in-abstraction, then they are abstract objects, but I would argue that (i) physical properties are not abstract objects, i.e., they exist in spacetime and are, therefore, nominalistically kosher, and (ii) in order to nominalize QM by means of the first strategy, i.e., the propensities-are-basic strategy, we only need to make use of physical properties, i.e., we needn't appeal to any properties-in-abstraction.[6]

The argument for (ii) would be based upon the fact that all the propensity properties needed to generate an orthomodular lattice are already contained in a single quantum system. The argument for (i), on the other hand, would need to be quite long, but my general strategy would be two-pronged. First, I would try to show that all the arguments for thinking that properties are abstract—e.g., that there are uninstantiated properties—apply only to properties-in-abstraction and not to physical properties. And second, I would provide positive argument for the claim that physical properties exist in spacetime by pointing out that they are *causally efficacious*. For instance, 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

[6] Putnam (1970) seems to adopt something like (i); Field (1980, p. 55) mentions this as a strategy that one might adopt in trying to nominalize physics, but he doesn't pursue the strategy at all.

not have any exact location in *b*) and it seems almost crazy to say that it exists outside of spacetime. What would it be doing *there*? And how could it have causal influence from there?[7]

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