Nancy Cartwright's Philosophy of Quantum Mechanics in Practice

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July 10, 2026

Abstract

This chapter revisits and reassesses Nancy Cartwright's interpretation of quantum mechanics as articulated in Chapter 9 of *How the Laws of Physics Lie*, "How the Measurement Problem is an Artefact of the Mathematics." I argue that her approach is an under-appreciated alternative in the foundations of quantum physics that still has much to recommend it. Cartwright interprets quantum mechanics as a theory of transition processes: what quantum mechanics does is determine the probability that systems that begin in a specified state will transition to another specified state, under conditions typically determined by the environment of the system. I argue that, despite how she frames her project as "a kind of anti-realism," Cartwright's interpretation is best understood as a fully realistic interpretation of quantum mechanics, one that draws its understanding of what in quantum mechanics to be realist about from how the theory is used *in practice* rather than beginning from abstract reflection on the structure of the theory alone. To make clear how Cartwright's interpretive move leads to a form of realism requires some reconstruction, though that reconstruction is fully in line with Cartwright's aims. This realistic interpretation of quantum mechanics is fully compatible with Cartwright's later views concerning the interpretation of theories and theoretical anti-fundamentalism and pluralism.

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"theory is what theory does" – Nancy Cartwright (2008)

1 Introduction

I would like to begin this chapter with a word of gratitude. It is only because Nancy Cartwright took a chance on admitting a physics major with strange ideas—my writing sample was about science and myth, underdetermination and theory-ladenness, and values in science—that I was accepted into a philosophy Ph.D. program, and in no small part due to her instruction and mentorship that I became the philosopher I am today. I had a philosophically rich but highly idiosyncratic undergraduate education that included the philosophical physicist David Finkelstein, known for his work on quantum logic in the 1960's that inspired Hilary Putnam. UCSD, long home to many creative and maverick philosophers, turned out in many respects to be the right place for a graduate student with broad interests and strange ideas to flourish. I thought when I started that I would probably do my dissertation on the philosophy of quantum mechanics, and after re-reading Nancy's work on this topic, I find that our thinking on these topics has so much in common, much more than I think I realized as a student. I don't regret my turn to general philosophy of science or writing the dissertation that Nancy co-chaired, but I am very happy to be revisiting this topic for this volume. Thank you, Nancy, for everything.

In this chapter, I will argue that Cartwright's approach to quantum mechanics, particularly that presented in Chapter 9 of *How the Laws of Physics Lie* (*HLPL*), "How the Measurement Problem is an Artefact of the Mathematics" (1983a), should be better appreciated and has much to recommend it as a live option in the foundations of quantum mechanics. Indeed, the fundamental core of the view she articulates in that chapter is in my view not only a plausible competitor to popular interpretations (such as many-worlds or Bohmian mechanics), it is the best of the available alternatives in several respects. It is also, contrary to Cartwright's claim in the Introduction of *HLPL* that she is articulating "a kind of anti-realism" (2), the best candidate for a *realistic* interpretation of quantum mechanics, properly understood. In the course of articulating and defending Cartwright's view, I will do a bit of reconstruction—there are some claims I think should be dropped and others that ought to be clarified, but the resulting reconstruction is still very recognizably Cartwright's view.

I take the core of Cartwright's view to be as follows: (1) Quantum mechanics (QM) is fundamentally a theory of transition processes. (2) The primary empirical content of QM is transition probabilities. (3) The part of the theory that is referred to as "reduction of the wave packet" or "nonlinear collapse dynamics" is at least as fundamental as the linear, deterministic dynamics (the Schrödinger equation). Her methodological approach or argumentative strategy to articulate and defend her approach is twofold: (4) attend carefully to how QM theory is used *in practice* rather than how it is presented abstractly in textbooks or philosophical discussions and (5) recognize that not all structural features of the mathematics of the theory necessarily represent features of the physical system, which can be uncovered in some cases by exploring different versions of the mathematical formalism (such as that of quantum statistical mechanics). In the rest of the chapter, I attempt to articulate, defend, and extend (1-5).

There is surprisingly little literature on Nancy's interpretation of QM, particularly the view laid out in *HLPL*.[1] One reason is that those interested in the foundations of quantum physics have tended to not be interested in "anti-realist" approaches, which they may see as no better than the so-called "Copenhagen orthodoxy" or the "shut up and calculate" attitude of many physicists. I attempt to re-brand the view as a kind of realism in part because I think it *is*, but in part because I think that re-brand will show its relevance to major debates in philosophy of physics more clearly. These days, Nancy describes her views as "anti-fundamentalism" and "pluralism" rather than "anti-realism," and her earlier philosophy of QM is very much in line with these commitments.

[1]In my search, I found only three really substantive engagements in the published literature: Jeremy Butterfield's (1983) response to the earlier version (Cartwright 1983b), Peter Gibbins's (1984) sympathetic review in *BJPS* of *HLPL*, an article by Niall Shanks (1993), and Brigitte Falkenburg's (2008) chapter in *Nancy Cartwright's Philosophy of Science*. Butterfield focuses on technicalities of Cartwright's use of quantum statistical mechanics that do not figure heavily in my reading of Cartwright's argument. He then moves to a general discussion of Cartwright on theoretical laws that is only loosely related. Gibbins provides an interesting argument that Cartwright's "anti-realism" about QM inspires the rest of the argument in *HLPL*, and he takes a long tangential detour through the finer points of the quantum logic debates (Gibbins's own main interest). Shanks interprets Cartwright as a realist, spontaneous collapse theorist, similar to Ghirardi, Rimini, and Weber (1986; cf. Ghirardi and Bassi 2025); I see this as the wrong way to understand what Nancy is doing. Falkenburg's engagement seems to me the most detailed, but as it already received a thorough reply by Cartwright (2008), I will not treat it in detail here.

A quick qualification on "interpretation": Cartwright has argued, following Hilary Putnam (1962), that scientific theories in general do not stand in need of (philosophical) interpretation, and has turned this very argument back on Putnam's (1965) famous work on the interpretation of quantum mechanics to argue that there is no general problem of interpretation of quantum mechanics (2005). The very idea that a scientific theory is an abstract formal-theoretical structure waiting for philosophers to give a meaning via "interpretation" is rejected. It may thus seem puzzling that I attempt to reconstruct Cartwright's "interpretation" of QM. The tension is merely apparent; Cartwright dissolves rather than solves the measurement problem, and she does so by insisting on looking at the details of how QM is *used* by physicists, not by providing a bizarre re-interpretation of the theory (consciousness collapses wave functions) nor by subtracting from (Everett) or adding to (Bohm) the theory as it is used by working physicists. She does not treat the theory as an abstract-formal structure in need of a interpretation to get its empirical meaning; she resolves or dissolves some puzzles about the theory precisely by *not* doing that. In this way, Cartwright's interpretation of QM is not a problematic "interpretation" of the sort she critiques. She does not critique the project of using the philosophers' tools to interpret the practices of scientists and the role that theory plays within those practices, which is how she approaches the "interpretation" of QM.

2 The Standard Framing of the Measurement Problem

Cartwright begins her discussion with a standard presentation of the measurement problem: "There are two kinds of evolution in the quantum theory," as presented by von Neumann, but they work in almost diametrically opposite ways (163). On the one hand, there is the "continuous and deterministic" evolution according to a linear, unitary, reversible dynamical equation, typically represented with Schrödinger's equation (von Neumann's "Process 2"); on the other, the "discontinuous and indeterministic" process of "reduction of the wave packet," which is described by von Neumann's nonlinear, nonunitary, irreversible "projection postulate" ("Process 1").

According to a standard presentation of quantum mechanics, Process 2 applies everywhere except in a measurement, while Process 1 applies only to *measurement*, and results in the state of the system reducing or "collapsing" to one eigenstate of the observable operator corresponding to an eigenvalue;

the probability that it will collapse into a specific eigenstate is given by the Born rule. The Born rule is sometimes presented as part of the content of the projection postulate (Barrett 2019, 42–43) and sometimes as a separate feature of the theory (Wallace 2019, 286).[2]

Here, then, is *the measurement problem*: what is so special about measurement? How does Nature know when an interaction is a measurement? What is it about measurement that interrupts the usual linear dynamics and causes the "projection," "reduction," or "collapse"? One can make the problem more vivid by applying the abstract possibility of a situation that could be described *either* as a measurement or an entanglement that evolves via the linear dynamics, generating cases like "Schrödinger's cat" and "Wigner's friend."

Though Cartwright does not provide an overview of previous solutions to the measurement problem in the chapter, it seems to me that the main, popular approaches (then and now) are anti-realism (measurement features prominently because the theory is a purely instrumental one), bizarre metaphysical re-interpretations of the theory (e.g., "consciousness" as playing a central role in fundamental physics), denying that there is any process of "reduction" or "projection" (e.g., Everett's many worlds)—and in the process losing much of the empirical content of the theory (Barrett 2017)—, or adding additional theoretical machinery that bridges the gap (realistic collapse theories, Bohmian mechanics).

Cartwright's approach is different from all of these: it is to take the theory as-is, without any bizarre or instrumentalist re-interpretation of it, while arguing that there is nothing special about measurement and thus no special "measurement problem." The appearance that there is a measurement problem is an artifact of the choice of mathematical framing and presentation. To understand why, we need to understand how quantum theory is used in practice, i.e., how physicists use the theory to make predictions and describe physical processes. Understanding this will provide a very different perspective on the measurement problem from the usual approach. There is a significant difference in what it means to take the theory at face value between Cartwright and many other philosophers of QM.

[2]Some presentations heavily emphasize the eigenvector-eigenvalue link, namely, the view that given an observable quantity O, a system in state $|\psi\rangle$ possesses a definite value of O if and only if $|\psi\rangle$ is an eigenstate of $\hat{O}$ where $\hat{O}|\psi\rangle = o_i|\psi\rangle$. It is not clear that this link is an essential feature of the theory as such (Wallace 2019).

3 The Theory of Quantum Mechanics in Practice

One of the enduring legacies of *HLPL* and Cartwright's *ouvre* more generally is the way she shows us how to do philosophy of science by attending to the *actual practice* of science, as opposed to simply focusing on the abstract structure of scientific theories and results as presented in textbooks and papers. As she says in a more recent context, "I urge instead that theory is what theory does," which in this case means, "Look to see how quantum ascriptions are assigned in empirically successful treatments" (Cartwright 2008). On this basis, Cartwright argues that there is nothing special about measurement. Many processes described by physics work analogously to how measurement is typically treated, in that they reduce the wave packet in the same way. These processes include (1) initial preparation of the system and (2) processes like scattering and radioactive decay.

If one looks at how the initial state of a system ψ is typically assigned for an experiment, it is based on the circumstances of the initial preparation of the system. For example, in the description of the SLAC, electrons are initially prepared with a narrow range of directions of momentum by the injector (172). In a basic spin experiment, a Stern-Gerlach device provides a beam of electrons that are spin up in the x-direction. The state is not given as a complicated superposition of entangled system and preparation system; it is typically given by a specific state of interest. Assuming that prior to preparation, the system had a state that could be in a superposition, the initial preparation event "reduces" the state to the state of interest. Similarly, descriptions of scattering events or radioactive decay presuppose a specific transition takes place that would be characterized by a reduction of the state.

Proponents of the traditional approach would respond that while this may be true in practice, *in principle* one could give a complete wave function (state vector) completely characterizing every feature of the system, or indeed give such a function for the universe as a whole in all its detail. It is precisely this sort of "in principle" abstract idea about the theory that Cartwright has taught us to be suspicious of in various ways throughout our career. Is philosophy of physics about the actual practice of physics, or is it about some abstract possibility that is loosely related to an introductory textbook presentation of what physics is? Cartwright has taught us to favor the former.

Cartwright argues that the appearance of two very different sorts of processes is more of a feature of particular choices of formal presentation of the theory rather than being essential to the working theory itself. This

is the sense in which the measurement problem might be an "artefact of the mathematics." She shows this by comparing the standard von Neumann presentation with that of quantum statistical mechanics. The latter replaces von Neumann's two processes with a single, more general equation, with the two processes as special cases. When a unitary operator is used in this equation, you get a deterministic, reversible evolution; when a non-unitary operator is used, you get an indeterministic, irreversible reduction of the wave function.

This might simply be seen to replace the measurement problem with a slightly different, enlarged problem, which Cartwright calls "the characterization problem," i.e., which processes are characterized by a linear, deterministic evolution and which are characterized by a non-linear, indeterministic evolution? This problem stays with us even if we switch the formalism to that of quantum statistical mechanics. We alleviate this problem in part by coming to see that the quantum theory is fundamentally a theory about transition processes and the probabilities of transitions, rather than a theory about the states of dynamical quantities and the evolution of those states. The question is simply one of which kind of operator is appropriate to describe which parts of the transition process, and this will generally be determined empirically. (More about this below.)

It is surprising how the "practice turn" in philosophy of science has hardly made a dent on the foundations of quantum physics. The vast majority of discussions have begun from a version of John von Neumann's (1932) abstract, axiomatic presentation of the theory, which, because it makes "measurement" the special process that switches the system's evolution from Process 2 to Process 1, makes the measurement problem particularly stark. This partly results from the strong influence von Neumann's text has had on the textbook tradition in quantum physics, but it is surprising how resistant the community has been to alternative formulations[3] of the basic theory, much less to looking carefully at how the theory is used in practice.

One notable exception to this trend is David Wallace's (2019) article, "What is Orthodox Quantum Mechanics?", which explicitly attempts to compare the standard presentation in foundational discussions with how the theory is actually used "in practical applications of quantum mechanics"

[3]What I mean by "alternative formulations" is not interpretations like Everett's or Bohm's that modify the basic theory, but alternative presentations or pictures of what the basic theory *is*, such as those given by Heisenberg, Dirac, or quantum statistical mechanics. (More on this below.)

(285). Here, Wallace argues that in physics practice, neither the collapse postulate (Process 1) nor the eigenvalue-eigenvector link play any role. What actually play an essential role, according to Wallace, is the so-called "structural core"—consisting of the representation of states by vectors in Hilbert space, the representation of physical quantities (observables) by operators on that space, and the Schrödinger dynamics (Process 2)—and the Born probability rule. Thus Wallace also rejects aspects of the standard presentation by looking at actual physics practice.

Wallace's conclusion may seen at first blush at odds with Cartwright's argument in *HLPL*, given that the latter explicitly defends "reduction of the wave packet" as an ineliminable part of the theory. But on a closer look, I think their views are compatible, and it has to do with how we think of "reduction" and its relationship to the Born rule. According to Cartwright, there are irreversible, indeterministic transition processes in quantum mechanics that reduce the wave packet. Measurement is one example, but so is radioactive decay. The transition probabilities associated with the outcomes of these processes are given by the Born rule. On Wallace's account, the Born rule is fundamental, but the reduction of the wave packet is not. Other presentations closely link the Born rule to the projection postulate (Barrett 2019, 42–43). From within Cartwright's perspective, I think, the projection postulate and the Born rule for transition probabilities amount to different aspects the same thing. To show why, I need to somewhat reconfigure the presentation of the transitions-as-fundamental view, as compared to Cartwright's own presentation.

4 Preparation, Context, and Measurement

In the course of articulating her account that transition is fundamental, Cartwright is led to say something that seems quite strange to me:

> I have been urging that the interpretation of quantum mechanics should be seen entirely in terms of transition probabilities. Where no transitions occur, ψ must remain *uninterpreted*, or have *only a subjunctive interpretation*: if the system were subject to a perturbing potential of the right sort, the probability for a transition into state —— would be ——. This makes the theory considerably less picturesque. We think in terms of where our microsystems are located, and all our instincts lead us to treat these locations

> as real. It is a difficult matter to give up these instincts; but we know from cases like the two-slit experiment that we must, if our talk is to be coherent. (186, *emphasis added*)

Part of this, I will argue, is correct: we do have to give up considering the position basis as more fundamental and as something that is always determinate, and the transition picture can help us see how to do that. But another part of it is mistaken: the point about ψ where "no transitions occur." It is not that it must remain uninterpreted or merely subjunctive, except insofar as the latter means that we recognize that it is part of an incomplete formula. Just as it makes no sense to ask determinate questions about the motion of a Newtonian system when we have not filled in the information about the relevant forces acting on the system, it makes no sense to ask determinate questions about quantum systems when we have only filled in information about the initial state and intermediate evolution but not the final transition.

The confusion arises, I would argue, because Cartwright is still taking on board an aspect of the common textbook presentation of QM that does not correspond to the actual use: the unqualified treatment of ψ as *the* "state" of a system, i.e., an inherent feature of the system that it carries with it. To see how this is a problem and to find its solution, I would like to step back and think about an example from Dirac's (1958) textbook, *The Principles of Quantum Mechanics*, discussed by Falkenburg (2008) and Finkelstein (1999).

Suppose you have a set of linear polarizers (in Dirac's (1958, 5) example, they are tourmaline crystals). If you arrange two exactly perpendicular to each other, it will block all light that you send through it. If you arrange them exactly parallel, all the light that gets through the first polarizer will also get through the second. If you position the second polarizer at an angle between, a fraction of the light will get through (50% at 45°, or $\sin^2 a$ for any angle a). This is already puzzling if we assume that light is made up of corpuscular photons, rather than waves in the luminiferous ether, and we can capture it quantum mechanically by treating the first polarizer (or the light that passes through it) as a superposition of parallel and perpendicular to the second polarizer (Dirac 1958, 6–7).

Now, if you return to the situation of two perpendicular linear polarizers (where no light is getting through), and you insert a third polarizer in between at a diagonal of 45°, some (25%) light will get through. If you swap any two of the three polarizers, though, you will again block all of the light. This

situation nicely captures part of the strangeness of quantum mechanics. If the polarizers merely filter light according to its preexisting state, you might think that inserting other polarizers between the 0° and 90° polarizers would have no effect. But this is just not what happens. Just as a 45° polarizer transmits 50% of the light that comes from a 0° polarizer, and a 90° polarizer transmits 50% of the light that comes from a 45° polarizer, the combination of 45° and then 90° transmits 25% of the light that comes from a 0° polarizer. On the other hand, if we move from 0° to 90° to 45°, no light makes the first transition and so no light makes the second. The fact that this is true when photos are passed one at a time captures much of what is strange in quantum mechanics. The order of operations is non-commuting.

It can really help us to think through what's going on by applying some terminology and formal notation developed by Dirac (1939), as adapted by Finkelstein (1996, 1999).

The structural core of QM can be captured by three pieces of notation:

- $|\alpha\rangle$ – a *ket vector*, a vector of unit length on a Hilbert space V, representing what is commonly called the "state" of the system, but which should more accurately be understood to represent the initial condition or initial preparation of the system. We might, with Finkelstein, call this an "initial act," "initial mode" (1996, 14), or "injective vector" (1999, 448).

- $\langle\Omega|$ – a *bra vector*, a dual vector on the dual space of V, representing a potential final condition or measurement action on the system. Finkelstein calls this a "final act," "final mode" (1996, 14), or "extractive vector" (1999, 448).

- M – a *linear operator*, a function that takes ket or bra vectors and gives them back (e.g. a constant, a matrix/tensor). These are Finkelstein's "internal or medial acts" or modes (1996, 16–17).

One can transform a ket vector into a bra vector and vice-versa via conjugate transposition. One can create a linear operator M out of the ket vector $|M\rangle$ via the outer product $|M\rangle\langle M| = M$

One combines these three pieces in the following formula:

- $A = \langle\Omega|M|\alpha\rangle$ – The transition amplitude.

We read the right-hand side of the formula from right to left (from α to Ω), from the initial to final mode. The Born rule is thus simply:

- $P = |A|^2 = |\langle\Omega|M|\alpha\rangle|^2$ – The transition probability.

Any QM transition process can be described in this way. Although the way Dirac and Finkelstein present this in terms of "preparations" and "measurements", there is no reason to limit the application in this way, and in practice physicists do not so limit themselves. Nothing requires $|\alpha\rangle$ to be an initial preparation action in a laboratory and $\langle\Omega|$ to be a measurement. What is crucial is that $|\alpha\rangle$ and $\langle\Omega|$ are physically specifiable states or situations that can be related in specific transition processes. $|\alpha\rangle$ could be an unstable atom, and $\langle\Omega|$ could be a stable form after a radiative decay. M would describe the relevant features of the energetic situation and the time between $|\alpha\rangle$ and $\langle\Omega|$. For example, to describe our hypothetical polarization experiments, we can represent our parallel polarizers as the pair of vectors:

$$|\psi_{0^\circ}\rangle = \begin{pmatrix}1\\0\end{pmatrix}, |\psi_{90^\circ}\rangle = \begin{pmatrix}0\\1\end{pmatrix}$$

Then the diagonal polarizers can be written as

$$|\psi_{45^\circ}\rangle = \frac{1}{\sqrt{2}}(|\psi_{0^\circ}\rangle + |\psi_{90^\circ}\rangle)$$

We can represent our entire experiment as:

$$\langle\psi_{90^\circ}|\psi_{45^\circ}|\psi_{0^\circ}\rangle$$

This means that the photon is initially prepared by a 0° polarizer, passes through a 45° polarizer, and is registered after passing through a 90° polarizer. The probability of the photon so initially prepared making this transition would be:

$$P = |\langle\psi_{90^\circ}|\psi_{45^\circ}|\psi_{0^\circ}\rangle|^2 = \left|\begin{pmatrix}0 & 1\end{pmatrix}\begin{pmatrix}\frac{1}{2} & \frac{1}{2}\\ \frac{1}{2} & \frac{1}{2}\end{pmatrix}\begin{pmatrix}1\\0\end{pmatrix}\right|^2 = \left|\frac{1}{2}\right|^2 = 25\%$$

5 Quantum Mechanics as a Theory of Transitions

As Cartwright argues, QM is the theory of such transitions. The most fundamental feature of the theory, empirically speaking, are the transition probabilities.

It is noteworthy, when we think about how the framework is put to use in experimental practice, that the ket vector, which has often been taken by physicists and philosophers as characterizing "*the* state of the system" is often determined less by inherent features of the system than by features of the preparation device. For example, the "state" ascribed to the light in our polarization example from the previous section is the direction of its polarization, as given by the initial *polarizer*. The entire description, in fact, of all three pieces is as much about the meta-system (polarizers) acting on the system (photon) as it is about the system itself.

This is more obvious in the case of the bra vector or final mode, which is often explicitly a characterization of the quantity to be measured, and in such cases depends upon the nature of the measuring device. Similarly, the linear operators occupying the medial position in the Dirac notation often characterize features of the environment. The most important linear operator being the Hamiltonian of the Schrödinger equation, includes environmental features that determine the potential energy of the system (e.g., free particles vs potential wells) and the effect of time between the initial and final act. Again, it is not only measurements that can be characterized in this way, but any transition process whatsoever, including a complete isolated atom that might undergo a radioactive decay. To the puzzle, "If a radioactive isotope is alone and totally isolated, with no one to measure it, does it ever really decay?", one might answer that nothing in QM prevents you from determining the probability at any moment that it does actually decay, nor from characterizing it as an actually decay.

Cartwright follows tradition in treating the ket vector as the "state" of the system, and this leads to some puzzling claims that I quoted above. In turn, I think, the obvious problems with treating the state in this way may be what suggests that treating transitions as fundamental amounts to a kind of anti-realism. But nothing about the mathematics, nor the loose talk of "states" among physics textbooks, requires us to think of the ket vector as the quantum analogue of the classical state. If we stop doing that, we can happily remain realists. We can should be realist about the initial, medial, and final modes as descriptions of the interaction between the system and its environment,

about the fact of transition, and about the transition probabilities given by the Born rule, when the modes have been described correctly. We need not be realist about the continuous, deterministic dynamics of the Schrödinger wave equation treated as a universal law of motion. On the transition view, this is just a particularly important, particularly general, time-dependent medial mode.

Hence, I think it was a mistake for Cartwright to assert, "Where no transitions occur, ψ must remain uninterpreted, or have only a subjunctive interpretation." This is the wrong way to think of the role that ψ plays in the theory. "Where no transitions occur" might be simply a case of $\langle\psi|M|\psi\rangle$, that is, the case where ψ transitions to ψ, which will itself have a specific probability determined by ψ and M. If it is two ideal linear polarizers stacked against each other in parallel, the probability will be 1; in many other cases, such as one in which an unstable atom does *not* decay after time t, the probability of the "no transition" transition will be lower. Alternatively, "no transition" might be the case of $M|\psi\rangle$, which is an incomplete formula waiting to have its final component filled in.

6 How Does Nature Know When to . . . ?

As mentioned previously, Cartwright argues that the "measurement problem" is an artifact of the mathematics because we can replace the canonical Von Neumann formulation of Process 1 and Process 2 (with very different equations) with the quantum statistical mechanics formulation that has only one evolution equation. In this formulation, the use of a unitary operator corresponds to Schrödinger-type continuous, deterministic evolution (Process 2) and a non-unitary operator corresponds to an indeterministic reduction of the wave packet (Process 1). This move, combined with the focus on other non-unitary evolutions such as scattering and decay, dissolves the measurement problem. I have done something similar with our appropriation of Dirac's notation above. Cartwright still ponders a variation of the measurement problem called "the characterization problem," namely, "How . . . does nature know in a given situation whether to evolve the system in a unitary or a non-unitary way?" (200). We are now in a better place to understand why Cartwright thinks this is not a philosophically interesting question, merely an empirical question about which kind of operator is appropriate to describe which parts of the transition process.

In quantum statistical mechanics, you have the single evolution equation that behaves differently depending on which operators you plug in. The energies of the system (and environment) determine the operators, and whether the operators are unitary tells us whether we are looking at a deterministic evolution of the wave function or the indeterministic reduction of the wave function. In the Dirac-Finkelstein formulation, the transition process you seek to describe determines the content of your initial, medial, and final modes, and the specific details of those modes and their interaction determines whether the transition is a deterministic or indeterministic one.

The key point here is that the existence of a structure in the mathematics of the theory (two separate equations, two different types of operators, compatible/commuting and incompatible/noncommuting modes) need not mark a difference in the structure in the physical phenomena the theory is about. In this case, the question of unitary or non-unitary *does* correspond to a physical difference (determinism and indeterminism), but there need not be a deep underlying physical difference at work. Sometimes transitions are guaranteed or forbidden, but this is an extreme case; most are neither.

I think we can push this point further than Cartwright does by considering other formulations besides von Neumann's and quantum statistical mechanics. In the Schrödinger picture, elaborated by von Neumann and others, the state of the system (the ket vector or "wave function") itself evolves over time according to the Schrödinger equation or the projection postulate. This is the picture generalized by quantum statistical mechanics. It is a picture in which the ket vector is presented as a thing that evolves rather than merely the initial mode of the system.

Consider Heisenberg's matrix mechanics and the picture it provides. Here the state is given by the initial ket vector and does not change with time. The time evolution (where present) affects the *observables* rather than the state of the system. What changes over time is not a property of the system but the probability of the transition. Heisenberg's picture fits better with the focus on transitions as fundamental. This may seem to threaten the supposed realism of our picture, given Heisenberg's declared opposition to anything but observable (in principle) quantities and relations between them playing a part in theory (Heisenberg 1925; Wolff 2014). Of course, matrix mechanics proved unwieldy, and even Heisenberg himself made use of wave mechanics in many contexts (Muller 1997, 242–43). Still, thinking about the picture of how things works rather than the specific mathematics is helpful in sorting the essential from the inessential. The path-integral formulation likewise does not

"evolve" the initial state but considers all the possible transitions between the initial and the final mode.

What I take from all this is the importance of attending to scientific practice and considering the variety of mathematical and theoretical approaches to a theory or family of theories in order to avoid what Whitehead (1925) called "the fallacy of misplaced concreteness." Otherwise, we are in danger of treating what is a mere artifact of the mathematics as a serious commitment of the theory, and as with the case of QM, thus taking seriously a pseudo-problem.

7 Is Anything Collapsing?

I have presented Cartwright as treating the so-called "projection postulate," "reduction of the wave packet," or "collapse of the wave function" as equally important to the linear dynamics of the Schrödinger equation. Actually, in a sense, we could understand it as *more* fundamental, as the linear dynamics will always be relegated to the medial position in the complete transition equation. However, when we start from the position of the complete transition equation without baggage of treating $|\psi\rangle$ as an inherent "state" of the system that evolves over time, but rather as a representation of the initial situation of the transition process, I think it is clear to see that there is nothing that is actually "collapsing" or "reducing." Hence even the "reduction of the wave packet" is an artifact of the mathematics that involves a superposition at one stage and a specific probability at another.

There's a symmetry in the transition equation that militates against treating the ket vector as a state. We treat the 45°-polarized light as being in a state of superposition between 0°- and 90°-polarized light. It is common to describe $\langle\psi_{90^\circ}|\psi_{45^\circ}\rangle$ as a process where the state $|\psi_{45^\circ}\rangle$ "collapses" into the state $|\psi_{90^\circ}\rangle$, and adding a second filtration of $\langle\psi_{90^\circ}|$ after the first seems to support that. But we could just as easily treat the bra vector $\langle\psi_{90^\circ}|$ as a "superposition" in the basis of 45° and -45° polarization, and we would get the same result for our transition probability. We could think of the measurement filter as creating a state that moves back in time towards the preparation filter, which then collapses the bra-vector into a pure state with specified probability. Adding a second filtration of $|\psi_{45^\circ}\rangle$ before the first would even support that interpretation, except for the unintuitive backwardness of it all.

I think it is more conceptually coherent and more in keeping with the use of the theory to treat *neither* the bra vector nor the ket vector as the state of

a system, nor to treat linear operators as modifications to the state of the system. Rather, all three are features of the transition process, and understood contextually in that framework. In this sense, "being in a superposition" is not an inherent feature of any system; it is rather determined by the relations between the initial, medial, and final modes of the transition process. 45°-polarized light is not more inherently in a superposition of 0°- and 90°-polarized light than 90°-polarized light is in a superposition of 45°- and -45°-polarized light. The "superposition" is a representation of the relation between the different (we might say "incompatible") bases one could use to represent linear polarizations.

We tend to lose sight of this because of a mistake that Cartwright has already pointed out: the preference for the position basis. It is easy enough to see when we're thinking of polarization of photons or the spin of electrons that we can change the basis and thus transform the superposed state into an eigenstate. It is harder to think about how a particle that lacks a precise position (i.e., its wave packet is "spread out" in space, or it is in a superposition of spatial positions) could be treated in this way. But the way to think of this is that the particle has previously gone through an initial or medial act such as a fairly precise preparation of its momentum or a time-evolution under energetic conditions whose relationship to localization events in position is indeterministic in some degree. Many of the characteristic classic effects of QM have to do with the relationship between the position basis and momentum basis that we represent as superpositions. It is not that a particle with a relatively determinate momentum and indeterminate position is more in a state of superposition than a particle with relatively determinate position and indeterminate momentum. The superposition is a feature of the relation between modes that localize in position or in momentum.

Deterministic transitions are just the special cases where the transition is required or forbidden rather than having a probability between zero and one (Brown 2014). This is determined by the relations between the initial, medial, and final acts, which in turn are determined empirically. We can read them off the relevant mathematical structures only once we have enough empirical knowledge about the relevant phenomena to provide the correct mathematical representation of them.

This, by the way, explains the sense in which Cartwright considers her account of QM in *HLPL* "anti-realist." In her 1975 Paper, "Superposition and Macroscopic Observation," Cartwright defended realism about *quantum superpositions* as real states of systems not to be reduced to mixtures of non-

superposed states and about the universal applicability of the Schrödinger equation. *These* are the claims Cartwright rejected in 1983. In a sense, then, what we have shown is that even superpositions are an artifact of the mathematics, though a deeply important one. Or rather, the sense that superpositions are an inherent fact about the system rather than a relational fact is an artifact. But not only are (non-relational) realism concerning superpositions and the universal applicability of the Schrödinger equation not the *only* way to be a realist about QM, they are not even the right way to be a realist, because they ignore significant features of the theory in question. If Cartwright is correct, that the right account of how the theory works is that it describes chancy transition processes and predicts precisely the probabilities of various transitions, then one can easily be a (local) realist about QM if one is committed to its (approximate) truth (within its domain of applicability). Given the powerful knowledge QM provides in many domains, such commitment is easily defensible.[4]

Returning to Wallace's (2019) claims about QM in practice: he argues that the projection postulate's claim of wave function collapse delivers the wrong answer in cases of repeated and continuous measurement. His argument is right if we assume that the wave function represents *the* state of the system and the projection postulate requires a real process of some*thing* collapsing. But if we situate his claims within the transition equation, we see that single measurement, repeated measurement, and continuous measurement are three different transition processes that would be represented in different ways. We have to presume that the transition actually occurs to make straightforward sense of the probabilities that are predicted by QM. It is in this sense that "reduction of the wave packet" must be defended, not in the sense of a realistic collapse.

8 Beyond the Measurement Problem

Cartwright admits in the Introduction to *HLPL* that her treatment in chapter 9 might "eliminate the measurement problem" but "it would have nothing to say regarding the very perplexing facts about locality brought out by the [Einstein-Podolsky-Rosen (EPR)] paradox" (18). I think here Nancy sells her approach short. The transition process approach and the relationalism it

[4]See Cartwright (1999) for the account of "local realism" and a precise discussion of the domain of applicability of QM.

brings to bear actually do give us some resources for wrestling with the EPR paradox. I will try to give a quick overview of how this would go.[5]

Cartwright's reference to "locality" makes clear that she takes the paradox of the EPR thought experiment to be this: for two "entangled" particles in the EPR state, a subsequent measurement of the position of one would guarantee that the other is in a specific position, while a subsequent measurement of the momentum of one would guarantee that the other has a specific momentum. If the EPR-particles are sufficiently distant, the choice to measure either position or momentum of one seems to non-locally transition the other into a precise position or momentum.

If we were to re-describe the experiment in question in terms of specific transitions, we would see that the final measurements of position and momentum are final modes in different, incompatible bases. This was Bohr's approach to answering the original EPR paper: he specified some of the details required to localize a particle's position or its momentum and showed how you could not do both in a single experiment unambiguously. Since you could not do both experiments with the same setup, and you needed the setup to provide a reference frame to define position and momentum, there was no problem of simultaneous momentum and position nor of nonlocal action.

The paradoxicality of EPR is a product of thinking that the ket vector is a "state" that the system (the two entangled particles in this case) carry with them after the initial preparation, only to be "collapsed" by the measurement of one of the quantities. If that is the right way to understand it, then the wave function itself changes instantaneously across long distances (nonlocal action at a distance), something that seems quite implausible. But if the EPR "state" is just the initial act of a transition process, and the prediction about the possible transitions depends as much on the different final acts as anything, one need not be concerned about any troublesome nonlocality; what nonlocality there is simply reflects the need for a shared reference frame that allows one to unambiguously relate the relevant properties. Here again, the quantum weirdness is another version of the fact that the relation of certain modes to one another is such that we simply get non-deterministic transitions rather than deterministic ones, that certain modes are incompatible bases

[5]The account here builds on my prior account in (Brown 2014), where I articulate a similar relationalism by reflecting on Bohr's views of QM. Note that Falkenberg also points out some similarities between Bohr's and Cartwright's approaches. This makes sense to me; Bohr was always thinking about the use of QM in practice when he provided interpretive remarks, and he was the master of tying abstract theoretical issues to concrete practice.

with others, or that not all operations commute. Bohm's version of EPR and Bell's Theorem results can be explained in similar ways (see Brown 2014, 9–10).

9 Conclusion

Of course, one may wish to ask: in virtue of what are different modes incompatible? Why are there uncertainty relations and non-commuting modes? Why are some modes superpositions of others? One kind of answer is that the theory predicts them once you give the proper mathematical form for the various modes, and the latter is determined entirely by empirical methods. But one may want more than this; one may really be asking, what *causes* explain what is *really going on* beneath the transition processess? This is not an unreasonable question, but I think it is not one that QM answers. In virtue of what, then, do we get the transition probabilities that we do? Again, a reasonable question, but not one that we can get a satisfying answer to from the existing quantum theories. Cartwright argues that Nature does not owe us an answer to this question, in the sense that there may not be one to find. Whether there is or not is a question for future physics. There is at least the possibility that at bottom, there is nothing but process, that everything is flux.[6]

Acknowledgements

I'd like to thank Mathias Frisch for the invitation to contribute to this volume and for the productive opportunity to reflect on my relationship with Nancy's work that it created. My gratitude to Mathias and Hasok Chang for an initial conversation that led me to find the topic to focus on, as well as to Saakshi Dulani for a conversation over lunch that helped me to clarify some points in the paper.

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[6]Compare Nicholson and Dupré (2018)

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