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A loose and separate certainty: Caves, Fuchs and Schack on quantum probability one

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ABSTRACT

Carlton Caves, Fuchs, and Schack (2002) have recently appealed to an argument of mine (Stairs, 1983) to address a problem for their subjective Bayesian account of quantum probability. The difficulty is that on the face of it, quantum mechanical probabilities of one appear to be objective, but in that case, the Born Rule would yield a continuum of probabilities between zero and one. If so, we end up with objective probabilities strictly between zero and one. The authors claim that objective probabilities of one leads to a dilemma: give up locality or fall into contradiction. I argue that this conclusion depends on an overly strong interpretation of objectivism about quantum probabilities.

1. Introduction

Carlton Caves, Christopher Fuchs and Rüdiger Schack (henceforth CFS) have long defended a subjective Bayesian account of quantum mechanical probability. This may seem implausible for probabilities of one, but that case is important for their view. Assigning probability one to a quantum proposition typically generates a continuum of probabilities between zero and one *via* the Born rule. If probability one is objective, it would presumably follow that these other probabilities are as well. Consequently the viability of CFS's program requires them to deny that quantum probability is objective even for probability one. They write:

> The statement that the measurement outcome is 1 with certainty is… not a proposition that is true or false of the system, but an agent's belief – and another agent might make a different prediction. (Caves et al., 2002, p. 267)

In order to make their case, CFS appeal to a paper of mine from some years ago (Stairs, 1983). Though I am flattered by the attention to my work, I do not think their argument goes through. The appearance that it does rests on an overly strong reading of what objectivism calls for.

What follows is not intended as a full defense of objectivism about quantum probability (henceforth we will just say "objectivism.") CFS try to show that if probability one is objective, we face a dilemma: embrace non-locality or fall into contradiction. The main goal of this paper is to show that there is no such dilemma. As for quantum probabilities strictly between 0 and 1, the argument would not be that they must be treated objectively, but rather that nothing CFS say rules this out. I will sketch what I take to be a promising strategy for objectivism about quantum probabilities, but working out that strategy – or any other – goes beyond this paper.

CFS's case breaks into three parts: general arguments on behalf of subjective Bayesianism, a brief against an objective view of state preparation, and an argument that if we treat quantum certainty as objective but accept locality, we wind up in contradiction. I will urge that the general considerations are not compelling, that the case against the objective view of state preparation does not succeed, and that the argument about quantum certainty can be turned aside by some careful reflection on the connection between probabilities, properties and counterfactuals.

2. General considerations

According to CFS, propositions and probabilities lie on opposite sides of a category divide. Probability has an objective

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component: events or facts, which agents can settle unambiguously, and the rules of probability, including the Born rule. However, probabilities themselves are degrees of belief, and are neither true nor false. Probabilities do not follow from facts, and unlike physical parameters, they cannot be determined unambiguously—not even approximately. And though David Lewis's Principal Principle (Lewis, 1986a, 1986b) attempts to bridge the gap between degrees of belief and objective chances, the Principle is off the mark at least in the case of supposedly deterministic examples such as coin tosses. Or so we are told.

We will not spend much time on the category issue. It would be pointless to try to get by without subjective probability, and we can agree that degrees of belief are not facts. Nonetheless, CFS do not show that there could not be objective probabilities, nor that "objective probability" amounts to a category mistake. They are right that probability claims – subjective *or* objective – do not follow from non-probabilistic facts. However, this does not tell us anything about the objectivity of probabilities. The fact that the stuff in the shaker is mostly sodium chloride does not follow from the fact that it is table salt, even though table salt *is*, mostly, sodium chloride; the fact that Mary is thinking of Vienna does not follow from any non-psychological description of her, but this does not threaten physicalism about the mind. Moral claims do not follow from non-moral claims, but moral facts could still supervene on non-moral facts. For all CFS have said, objective probabilities might supervene on physical symmetries, for example, even though the symmetry claims do not *entail* the probability statements.

2.1. The Principal Principle and Humean chance

Accounts of what objective probability might be are not hard to find. Maudlin (2007) provides a lucid discussion of three possible analyses, and one would be hard-pressed to argue that one of them must be uniquely right or clearly wrong. CFS devote some attention to David Lewis's views, and in particular to the Principal Principle as a way of making sense of objective chance. Though they do not say a lot, it will be worth our while to say more.

Roughly, the Principal Principle (PP) holds that our degree of belief in a proposition ought to agree with the objective chance, if there is one and if we know it. More precisely, let A be a proposition. Let X say that the chance of A is x. And let E be any other "admissible" proposition, where "admissible" means, roughly, "does not provide any credence-relevant information about A beyond what knowledge of chances provides." Then PP says that a rational credence function Cr satisfies

$$Cr(A|XE) = x$$

This constraint is silent on the metaphysics of chance. In particular, it does not require that chances be intrinsic dispositions. On Lewis's account, however, nothing that violates PP could reasonably count as chance. Our credences guide our actions, and "chances" that could not be action-guiding even if we knew them are not worthy of the name.

So far, all this says is that *if* anything is worthy of being called chance, it must satisfy PP. That's consistent with there being no such thing. CFS maintain that in at least one case, non-trivial chances cannot exist: deterministic setups such as we usually suppose coin-tossing arrangements to be. The problem is that a fully precise specification of any such chance set-up will fix the outcome, leaving us with chances of 0 and 1.

The immediate reply is that Lewis would agree. He held that in a deterministic world, there are no chances (1986b, 117–121.) If determinism fails, Lewis provides his own account of chance: an extension of what he says about laws of nature. According to Lewis, a law of nature is a theorem of the "best system" of generalizations for describing the totality of events—the so-called "Humean Mosaic." "Best" includes the dimensions of simplicity, strength and fit. On Lewis's view, whether something is a law of nature is a fact about the world itself—about the arrangement of the mosaic. The idea can be extended to chance. We can broaden the range of law-like generalizations to include ones that describe statistical patterns. A candidate probabilistic law will earn its keep in the same way that strict laws do: by being part of the Best System. Such laws would be objective; they would reflect features of the world itself.

Lewis rejects the idea that there can be chances in a deterministic world, but not everyone agrees. Roman Frigg and Carl Hoefer (Frigg and Hoefer, 2010; Hoefer, 2007) argue that objective chances are real even if fundamental laws are deterministic. Our interest is not in the question of whether there can be objective probabilities in a deterministic world, but in the general character of Frigg and Hoefer's scheme, which is closely related to Lewis's.

The phrase "objective chance" suggests a dispositional or propensity account, but Frigg and Hoefer are no friends of hidden propensities. Their point is that when we describe things at the level of lotteries, coin tosses and so on, the world exhibits stable statistical patterns. Chance as described by Frigg and Hoefer is called "Humean Objective Chance" or HOC, and they use the metaphor of "Lewis's Demon" to convey the idea. We imagine a being who knows all the details of the Humean Mosaic of events (HM):

> The demon now formulates all possible systems of probability rules concerning events in HM… The rules in these systems assign numbers to events. These numbers have to satisfy the axioms of probability… but nothing over and above this is required at this stage. Then the demon is asked to choose the best among these systems, where the best system is the one that strikes the best balance between simplicity, strength and fit. The probability rules of the system that comes out of this competition as the best system then, by definition, become 'chance rules'… [T]he chances for certain types of events to occur… simply are what probabilistic laws of the best system say they are. (Frigg and Hoefer, 2010)

These chances are not epistemic. If we knew the mosaic whole and could juggle the details with godlike ease, we would have no use for probability—objective or subjective. But the patterns, if they exist, are part of the world. One way to see the point is to pretend for a moment that frequency is all that matters: to say that among situations fitting a certain macroscopic description, 30% exhibit feature F is to say something about events in the world itself and not our knowledge of it.

Of course, frequency is not all there is to the story, though Frigg and Hoefer describe their view as "a (major) sophistication of finite frequentism." We can get a sense of what the view means for quantum probability by extending Frigg and Hoefer's metaphor. Suppose the demon discovers that the pattern in the mosaic provides not just excellent confirmation for quantum theory, but better than for any rival theory. For this to be true, the actual frequencies could not depart wildly and systematically from the ones we expect based on quantum mechanics. If they did, quantum theory would fall down badly on the dimension of fit. If quantum mechanics is the best fit for the pattern in the mosaic, then quantum probabilities correspond straightforwardly to objective chances: to HOCs.

This is not the only way one could reasonably think about objective probability in quantum mechanics, but it is worthy of being taken seriously not least because of its minimalism; objectivism need not carry large metaphysical commitments.

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That said, Fuchs might object to the idea of the Humean mosaic. It would amount to a Block Universe, and Fuchs is profoundly skeptical of that notion.[1] One might try to make do with truth-values of proposition about the future rather than with the concrete events that are supposed to make up the mosaic, though Fuchs might reasonably deny that propositions about the indeterminate future have truth-values. Fortunately, we need not enter this debate. Whether or not the idea of the Humean Mosaic is to one's taste, a view broadly in the spirit of Frigg, Hoefer and Lewis provides an apparently coherent account of objective probability. (The term "probability" carries less baggage than "chance" and is the one we'll use henceforth.) Thus, it undercuts the "category mistake" charge. Further, the story appears to hold up when we extend it to cover quantum mechanics. In any case, CFS's central argument is about zero-one probabilities. That means we can get by with a weaker claim. All we need are *some* plausible, non-tautological truths about the future, gappy though the set of such truths may be. We can get by with truths associated with exceptionless lawful regularities—laws, if all such regularities deserve that term. We will return to this point later.

3. Objective preparations

On the face of it, arguing that quantum probability one cannot be objective seems like a hard row to hoe. After all, we seem to be able to prepare states; indeed, we seem to do it all the time. And many of the states we prepare seem to provide us with unit probabilities. Just think of preparing a beam of photons all polarized in some particular direction. CFS dub the view that this can be done the "objective preparations" view. If it is correct, we can give objective and, indeed, classical instructions for preparing quantum states. CFS argue that this cannot be so and that subjective probabilities are deeply implicated in state preparation.

Consider a deterministic state-preparation device designed to prepare horizontally polarized photons. Informally, think of it as measuring the polarization of an incoming photon, leaving the state alone if it is horizontally polarized, and flipping it otherwise. CFS borrow a circuit diagram from David Mermin (2006) that schematizes a device of this sort (Fig. 1).

We apply a controlled-NOT gate to an incoming photon and an apparatus qubit, producing the entangled state $\alpha|00>+\beta|11>$. If a measurement finds the photon in the horizontal path (if $a=0$), it is left alone, producing an outgoing state $|0>$. If the photon shows up in the vertical path (if $a=1$), an X-gate rotates the polarization from vertical to horizontal, changing the outgoing state from $|1>$ to $|0>$. Thus, the outgoing state is $|0>$ regardless of the incoming state.

CFS claim that quantum states are subjective probability distributions, and that two agents can assign different states without either being wrong. However, the device just described produces the same output no matter what the input. Why is not it just a fact that it prepares a particular state? The reason, according to CFS, is that even if there is no need for a judgment about the state of the incoming system, the state of the device matters. "Any attempt to give a complete specification of the preparation device in terms of classical facts (i.e., observations or measurements of the device and its method of operating) and thus to derive the quantum operation from the classical facts alone comes up against the device's quantum mechanical nature" (p. 264). To see how, consider an alternative circuit diagram (Fig. 2).

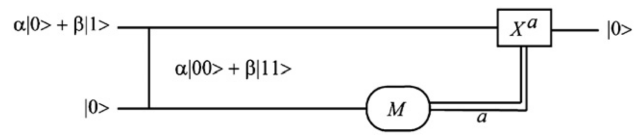

Fig. 1. Preparation device 1.

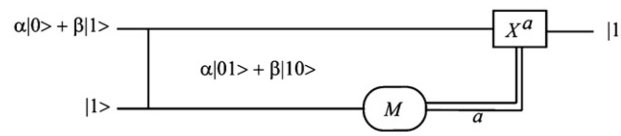

Fig. 2. Preparation device 2.

The procedure is the same: entangle the incoming photon with the apparatus by a CNOT gate, measure the whereabouts of the photon, and use the same instructions as above to determine whether to rotate the polarization. Because the apparatus state is $|1>$ instead of $|0>$, we get a different entangled state after the first CNOT operation, and hence a different state at the end of the preparation procedure.[2] The judgment about which state we prepared apparently calls for a prior judgment about the quantum state of the apparatus. CFS take this to show that in spite of the earlier informal description, we cannot make do with the "factual" classical description.

This argument overreaches. It is true: when we say that an apparatus prepares a specific quantum state, giving a quantum mechanical version of the story calls for assigning a state to the apparatus. We can also agree: non-quantum descriptions of the apparatus do not *entail* conclusions about the quantum state; literally deriving the quantum description of the apparatus from the classical facts alone is impossible for the banal reason that the non-quantum description does not include the relevant vocabulary. But the fact that we cannot give a quantum description of the apparatus without specifying a quantum state does not show that there is no fact about the polarization of the exiting photons. Indeed, CFS's example is a bit puzzling. We have known how to make polarized light for a long time,[3] and we have understood a good deal about its properties at least since 1807. Of course there was well over a century between Malus's discovery of the laws of polarization and the creation of individual photons with specified polarization. However, what modern quantum optics does could not be done without what people knew how to do a century or more before quantum theory was invented. It's simply not plausible that there is no objective preparation procedure for polarization. But what about CFS's analysis?

Go back to Fig.1 and ask what it is doing. It is not providing a conjectural account of some phenomenon where the apparatus state is part of what is at issue. It is a quantum mechanical model of one way to make horizontally polarized light. We can tell technicians who do not know quantum mechanics how to set up devices to do this. Of course any experimenter or lab technician will need to make various judgments, whether quantum claims are at issue or not. But the fact that we make judgments has nothing to do with whether what's judged is a matter of fact.

[1] Fuch's collection of correspondence between 2001 and 2007 is somewhat fancifully titled *My Struggles With the Block Universe*. www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf.

[2] As CFS point out, if we follow the first CNOT operation with a second one, letting the apparatus qubit be the control bit for the second CNOT, we do not even need a measurement.

[3] It has been speculated that the Vikings used a "sunstone"—a polarizing calcite crystal—as a navigational aid. See Hegedüs, Åkesson, & Wehner (2007).

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We can construct a quantum mechanical model of what the device described above does. The model uses notions such as "quantum state" that the technician may not know. The theoretical description of the device goes beyond the laboratory-level description, but this is hardly unique to quantum theory. We "come up against the device's quantum mechanical nature" simply because we need to use the language of quantum theory if we want to tell a quantum mechanical story. Given the workings of that language, this means inserting an apparatus state vector into the description. But we do not start with a subjective quantum probability distribution. Rather, we judge that we have set up a suitable apparatus for preparing horizontally polarized light. We base this judgment on what we know about the behavior of the things we have used to set up the apparatus. That constrains the sorts of quantum mechanical models we can use to describe the preparation procedure. When we assign the (highly schematic) state vector $|0>$ to the apparatus, we encode what we believe on independent grounds: the apparatus prepares horizontally rather that vertically polarized light. Once again, this calls for judgments and judgments can be wrong. But they can also be right, and the fact that we are fallible does not rule out the possibility of objective preparations.

Once the quantum mechanical enterprise is underway, we can bootstrap ourselves into preparation procedures far from 19th-century scientific common sense. But we do not enter the quantum conceptual world by teleportation or barrier penetration. Unless we could make sound judgments that do not presuppose quantum theory, we would have no idea how to do laboratory quantum mechanics.

So far, CFS have not shown any incoherence in the objective preparations view, but they have an ace up their sleeves. Their main argument is that treating quantum probability one as objective leads to a choice between contradiction and non-locality. Showing that might well tip the balance in favor of subjective Bayesianism. But does their argument work?

4. Probability one and correlations

At this point, CFS appeal to my 1983 paper. I argued there that if we combine Kochen and Specker's well-known result (1967) with appropriate correlations and a locality requirement, we can show that locally well-defined classical values are impossible. CFS streamline the argument and take it in a different direction; we'll streamline what they say in turn.

4.1. The argument

Kochen and Specker's original proof was based on a 117-vector construction. Peres (1993) came up with a version based on 33 vectors, which generate 40 orthogonal triples for a total of 57 vectors. For concreteness, think of each vector as corresponding to spin-zero in a particular direction for a spin-one particle. Suppose we are given such a set of triples; call it a *Peres set* and denote it by

$$\{|\alpha_{km}>\} \tag{1}$$

Here k picks out the triple; $1 \le k \le 40$. The index m picks out elements of a triple; $1 \le m \le 3$. Since triples overlap, there will be cases where $|\alpha_{km}> = |\alpha_{jn}>$ even though the indices differ. The set $\{|\alpha_{km}>\}$ has a well-known combinatoric property: there is no way to select exactly one vector from each triple. It is, as we say, uncolorable; we cannot assign a spin value of 0 to exactly one out of every triple of orthogonal directions represented in the Peres set. Thus, value assignments are possible only if they are contextual—only if assigning spin-0 in a given direction does not depend on which other two orthogonal directions we consider at the same time.

Now consider a pair of spin-one systems in the state

$$\Psi = (1/\sqrt{3})(|00> + |11> + |22>) \tag{2}$$

where $\{|0>, |1>, |2>\}$ is an orthonormal basis. This sets up a one-one correspondence between vectors in the two Hilbert spaces. Formally, if

$$|\alpha> = c_1|0> + c_2|1> + c_3|2> \tag{3}$$

is a member of the Hilbert space of the left-hand particle, then $|\alpha>$ is paired with

$$|\beta> = c_1^*|0> + c_2^*|1> + c_3^* 2> \tag{4}$$

In our notation, a triple $\{|\alpha_{k1}>, |\alpha_{k2}>, |\alpha_{k3}>\}$ – corresponding to a measurement A_k – will be paired with $\{|\beta_{k1}>, |\beta_{k2}>, |\beta_{k3}>\}$, corresponding to a measurement B_k where the α's are elements of Alice's Hilbert space and the β's are elements of Bob's. Call systems with these correlations *Peres pairs*. Suppose we believe that measurement reveals pre-existing values, and that these values respect the conditional certainties of Ψ. In particular, if an Alice-Bob outcome pair has probability 0 in Ψ, then it does not occur. The pre-existing values assumption together with the structure of the Peres set now leads to a contradiction. If $|\beta_{k2}>$ is the same vector as $|\beta_{j3}>$, for example, the inner product of Ψ with $|\alpha_{k1}> \otimes |\beta_{j3}>$ is 0 just as surely as the inner product of Ψ with $|\alpha_{k1}> \otimes |\beta_{k2}>$ is. Assigning a_{km} to Alice's system requires assigning b_{jn} to Bob's system whenever $|\beta_{jn}> = |\beta_{km}>$. This rules out consistent local value assignments for Bob's qutrit. By symmetry, the same holds for Alice's system. Whatever is going on in the experiments, it is not a matter of measurement turning up pre-existing, locally well-defined values.

CFS believe this rules out objective probabilities and objective state preparations. They write:

> Let $|\psi>$ be a state prepared by a preparation device, and consider the observable $O = |\psi><\psi|$, which has eigenvalues 0 and 1. If the state is $|\psi>$, a measurement of O will give the outcome 1 with certainty. In the objective preparations view, this certainty is implied by the facts about the experimental set-up, independently of any observer's information or beliefs… Whatever it is that guarantees the outcome is effectively an objective property (Caves et al., 2002, p. 267)

Now apply this to a Peres pair. Suppose Alice and Bob are space-like separated. Alice makes a measurement of A_k and gets the result a_{km}. Adapting CFS's notation to ours, they write

The resulting state of the particle at B is $|\beta_{km}>$. It follows that a measurement of the observable

$$B_{km} = |\beta_{km}><\beta_{km}| \tag{5}$$

on the particle at B gives the outcome 1 with certainty and that a measurement of

$$B_{kn} = |\beta_{kn}><\beta_{kn}| \quad \text{for } n \ne m \tag{6}$$

gives the outcome 0 with certainty. (ibid p. 269)

However, CFS assume a locality condition:

> …a system property at point x in space–time cannot depend on events outside the light cone centered at x. (ibid p. 268)

And so, it seems, treating probability one as objective calls for attributing an objective property to a distant system—a property that was not brought into being by the local measurement. Since the same story holds for any of the Peres triples, the consequence seems to be that we have a painful choice: agree that what we do "here" affects the spacelike elsewhere, or fall into inconsistency.

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5. Off to see the wizard

Let us slow down. In fact, let us leave quantum theory aside for the time being. Here is a tale that does not happen to be true, but is a perfectly good story in spite of that. The example at its core is based on (and isomorphic to) one developed by Liang, Spekkens, and Wiseman (in preparation), who in turn were inspired by a paper by Ernst Specker (1960).

A certain wizard makes wondrous pairs of boxes. Each box has three drawers in a row, labeled 1–3. If you open a drawer, a brilliant light shines from deep inside—either red or green. However, once a drawer is opened, the other two lock shut. Further experimentation turns up something else. Whenever same-numbered drawers are opened on a pair of boxes, the light is the same color: both red or both green. But when different drawers are opened, the light differs too: one drawer red, the other green, though in no predictable pattern. Yet more experimentation reveals that this correlation does not depend on distance, is not disrupted by any sort of barrier, involves no detectable matter/energy transmission and works at space-like separation. Attempts to use the boxes to send messages fail, however. Outcomes are uncontrollable, and what color one observer sees bears no discernible relationship to which drawer the other observer decides to open.

Suppose the wizard teaches us to make the boxes ourselves. The method does not call for hidden wires or signaling devices, but it does require special materials, handled in special ways. Why it works is obscure, but that it works is clear. We experiment in countless circumstances, and the boxes perform flawlessly. We conclude that the regularities are lawful and support counterfactuals: given any such pair of boxes, if the same drawer is/were opened on each, the color of the light is/would be the same, and if different drawers are/were opened, the color of the light is/would be different.

So far we have not mentioned probability because there is a prior point: the story is coherent. Correlations like these could be matters of fact and could be as lawful as any law of nature. But now suppose that Alice and Bob share a pair of such boxes. Alice opens drawer #1 and a glorious green light shines forth. Bob is far away; Alice knows nothing of what he has done. What can she say?

On the one hand, she can make three obvious inferences. She can say that if Bob actually did open drawer #1, the light from the drawer was green. If he actually opened drawer #2 the light was red, and likewise for drawer #3. However, these conditional statements are not counterfactuals, and as will become clear, the difference is important. What she cannot do is infer what *would* have happened if Bob had opened any particular drawer. She cannot say "Were he to have opened drawer #1, he would have seen green light, but were he to have opened either of the other drawers, the light would have been red." Put in the terms that CFS favor, she cannot infer the existence of pre-existing "instruction sets", localized in each box and dictating what color the light will be when a drawer is opened. For suppose the instruction sets exist. The instructions for Alice's drawer #1 guarantee that it will produce green light. If the correlations hold and are explained by local instruction sets, this means that the instructions for Bob's box must be Green for Drawer #1 and Red for the other two drawers. But now we get a contradiction. If Bob's drawer #2 is set to produce Red light, then the correlations demand that the instructions for Alice's Drawer #2 must also be for Red. However, if Bob's Drawer #3 is set to produce Red light, then the correlations require that Alice's Drawer #2 must be set to produce Green (Fig. 3).

Obviously we would have gotten a similar contradiction no matter which drawer we began with and no matter whether we assumed that its instructions were for Green or for Red.

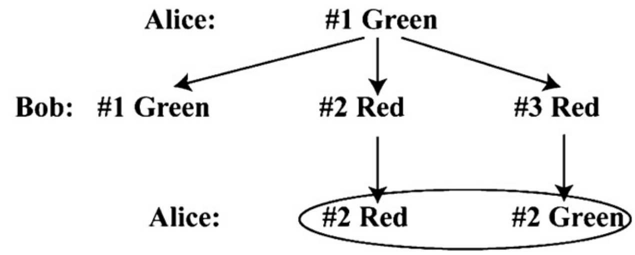

Fig. 3. Box contradiction.

The fact that the correlations cannot be produced by local instruction sets does not show that the modest inferences that Alice can make are incoherent. Alice does not infer that if Bob *were* to open Drawer #2, he *would* find Red light. What she infers is the weaker indicative conditional: if Bob actually *did* open Drawer #2, then the light from the drawer was red. What she can infer amounts, in effect, to this: either Bob did not open drawer #2 (in which case Alice can say no more), or else he did and found red light. The argument shows that if we try to factor the correlations, treating them as nothing more than disjunctions of conjunctions of local instructions, we get a contradiction. But nothing says that correlations have to factor. Moreover, the fact that the correlations do not factor *does not* show that they rest on spooky causal connections.

Many readers will notice that we have described a species of what are sometimes called "nonlocal boxes."[4] The term is potentially misleading; while it's true that the behavior of the boxes cannot be the result of local instruction sets, it does not follow that what happens to one box influences the other. Since our story is made up, there is nothing to stop us from saying that the correlations just *are*. Claims of causal connection do not follow from correlations—even correlations as unusual as these. To give the matter a Humean turn, there's nothing inconsistent in assuming that the correlations are just basic patterns in the mosaic, supervening on nothing more than themselves. The point is not to insist on Humeanism; rather it is that the Humean picture serves as a consistency check. It represents one way of describing a world where the correlations hold but what's done to one box does not bring about what happens to the other.[5] And so we will simply stipulate: in the world of our story, events at one box do not help bring about events at the other. Things are local in the sense of "local" that CFS want to preserve: *causally* local.

The idea that we could look at correlations this way is not new. Arthur Fine, for instance, makes the case in several papers, not least "Correlations and Physical Locality," in which he introduced the idea of *random devices in harmony*. Here's Fine's description:

> The suggestion of random devices in harmony amounts to suggesting that there is a conservation law, established when the systems are together at the source, and which maintains some constant functional relation between the outcomes, regardless of the particular outcomes. It is, to be sure, an indeterministic conservation law. Nevertheless, it is like other conservation laws, in that it functions over large distances and without requiring the exchange of any causal signals. (Fine, 1980, p. 547)

[4] Recent interest in nonlocal boxes stems from Sandu Popescu and Daniel Rohrlich (Popescu and Rohrlich, 1994). The term "nonlocal box" appears to have been first introduced in Barrett, Linden, Massar, Pironio, & Popescu (2005).

[5] The point is also not that there is no room for a concept of causation in a Humean scheme. But not every correlation counts as cause and effect in a Humean world. Bear in mind, for example, that the correlations are time-independent – unlike paradigm causal connections – and since they do not allow signaling, they cannot be exploited to send information. They also do not care about contiguity.

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The picture is of correlations that simply *are*—that are irreducible features of the way events proceed. In the case at hand, this gives us conditional certainties, but of a loose and separate sort.

5.1. Probability

What can we say about probability? Since unit probabilities are the ones that matter most for our purposes, begin with those. In the world of the story, whenever the same drawer is opened on each member of a pair of boxes, the color of the light from the drawers is the same: either both green or both red. This supports counterfactuals: if Alice were to open drawer #2 on her box and Bob were to open drawer #2 on his, then either both would find green light or both would find red. I suggest that these lawful regularities play the same role that objective probability does in the Principal Principle: a rational person who knew them would set her credences accordingly: she would accept (upper-case for results, lower-case for choice of drawer)

$$Cr([A_G B_G \quad \text{or} \quad A_R B_R]|a_i b_i) = 1 \tag{7}$$

It is difficult to see why we should resist also claiming

$$p([A_G B_G \quad \text{or} \quad A_R B_R]|a_i b_i) = 1 \tag{8}$$

where this probability is the objective correlate of the credence. We can simply say that this probability supervenes on the lawful fact that same drawer goes with same-colored light.[6]

Of course, the full correlational story has another piece: it is not just that same drawer yields same colored light; it is also that different drawer yields different colored light. However these two lawful regularities can happily coexist. Similarly the credences and probabilities noted above can happily coexist with

$$Cr([A_G B_R \quad \text{or} \quad A_R B_G]|a_i b_j) = 1 (i \neq j) \tag{9}$$

$$p([A_G B_R \quad \text{or} \quad A_R B_G]|a_i b_j) = 1 (i \neq j) \tag{10}$$

The difference in what is conditioned on ensures consistency.

What would CFS say? The only salient differences between our imaginary case and their paired qutrit case are that (a) the combinatorics are simpler in the imaginary case, and (b) the correlations are superquantum. As Liang, Spekkens and Wiseman point out, quantum systems can approximate our wizard's boxes more closely than classical systems with shared randomness can, but the quantum approximation will still fall short of what the boxes can manage. If CFS's argument works for the qutrit case, it should certainly work here. If it does, the reasoning would need to go something like this. Suppose Alice opens Drawer #1 and finds green light. Mirroring CFS's language, we would say

> It follows that opening drawer #1 on Bob's box has the outcome GREEN with certainty, and opening drawer #2 or drawer #3 has the outcome RED with certainty.

Now we add the locality condition:

> A system property of a box in space–time region *x* cannot depend on events outside the light cone centered at *x*.

The phrase "system property" might better be replaced with "intrinsic property." In any case, CFS would say that if certainty is *objective* probability one, then the certainty about Bob's box amounts to Bob's box possessing an intrinsic property– one that guarantees what would happen if Bob were to open the relevant drawer. But if that's so, we end up with instruction sets and the contradiction we described above.

The problem with this argument should be clear by now. It's consistent to say that the correlations are objective, and that they ground objective unit conditional probabilities. But when Alice opens drawer #1 and sees the green light, she does not infer that drawer #1 on Bob's box has some property that guarantees green light. She infers something much weaker: if Bob really did open drawer #1, he found green light. This is what she can say "with certainty." If he *did not* open drawer #1, she makes no inference about what would have happened if he had, let alone to some intrinsic property. Indeed, Alice will allow that had Bob opened drawer #1, both he and she might have found red light. Repeat that: Alice makes no counterfactual claims about her *own* result in this non-actual case—let alone about Bob's.

The objective probability that goes with Alice's credence is

$$p(B_G|a_1 b_1 A_G) = 1 \tag{11}$$

Alice's credence seems to be of this form

$$Cr(B_G|b_1) \tag{12}$$

This is harmless if properly understood. It is not a commitment to a counterfactual, nor to an instruction set. It is better thought of as the abbreviated version of

$$Cr_{a1AG}(B_G|b_1) = 1 \tag{13}$$

where the measure "Cr_{a1AG}" represents conditionalizing on a_1 and A_G.

5.2. Certainty and the world

There is no contradiction in sight. But if there were, subjective probability would not help. Subjective certainty about something that would be contradictory if it were just plain true is still incoherent. Put another way: ask the subjectivist *what* she is subjectively certain of. If the answer does not produce incoherence, then the objectivist will be free to say: "*That* is what I claim has an objective probability of one."

There is a related point that bears on a remark CFS make toward the end of the paper. We imagine a scientist who makes repeated *z*-spin measurements on a qubit. Theory plus considerable experience convince him that he will always find spin up. CFS consider two hypothetical questions: should the scientist be surprised? And must there be something in nature, independent of his belief that accounts for the repeated outcome?

We can agree with CFS that the first question should be answered "No." If all of the scientist's prior experience leads him to be certain about these outcomes, it would be surprising if he were surprised. But what CFS say about the second question is curious. Since the scientist has thoroughly consulted experience, prior beliefs, theory, previous outcomes… in coming to his certainty, CFS ask:

> Why would he want any further explanation? What could be added to his belief of certainty? He has consulted the world *in every way he can* to reach this belief; the world offers no further stamp of approval for his belief beyond all the factors that he has already considered. (p. 270)

The reply seems to blur two issues. The scientist is certainly reasonable in his belief, and for just the reason CFS give: he has consulted the world in every way he could. But there is something obvious that the world can add by way of a stamp of approval: what he believes can be true. Indeed, that is normally the point of beliefs: to get things right about the world. If I am certain of something, then what I'm certain of is that the world is a certain way; the direction of fit is from belief to world. What the scientist

[6] There is a general principle here which, as a first approximation, we can state this way: if $\forall x(Fx \rightarrow Gx)$ is a lawful, counterfactual-supporting relation among events, then in any particular case c, $p(Gc|Fc)=1$. Offhand it is not clear what any exceptions would be like.

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is certain of is that in cases like this, the world behaves in a certain lawful way.

CFS are right, of course, when they insist that there need not be some feature of the world that mirrors *degree* of a belief. My subjective probability that Marcus is in the next room may be 90%. Depending on the evidence and my prior beliefs, that degree of confidence may be reasonable or it may not, but there need be no larger pattern at issue. In cases like this, there is no need to invoke objective probability.

In other cases, however, talk of objective probabilities is entirely natural. That does not undermine the point of subjective probabilities, but it gives them a particular way of getting their grip. The Principal Principle does not say that all sensible subjective probabilities must mirror objective probabilities; the claim is much more modest. It is that *when* there are objective probabilities, those who know them and are rational will set their degrees of belief accordingly. For coins made in a certain way and tossed in a certain way, a rational person's credence will give equal weight to heads and tails. That credence, the objectivist will say, mirrors something about the world.

Mirrors what? There's more than one possible answer, but we can illustrate a minimalist one by reverting to the quantum case. Suppose we accept quantum theory. Then we believe that if we could see the events of the world in detail, they would confirm quantum theory. This does not mean that every experiment would confirm quantum theory, nor that our expectations would never be disappointed. But it does mean that overall, the way events proceed would be an excellent fit for the theory. We do not need to claim that this is all quantum theory says. However, for an objectivism broadly in the mold of Lewis-style accounts, all we need to claim is that it says at least this much. That is still a claim about the world.

6. Quantum states

Our imaginary case was theory-free. It assumed that there is no special set of terms or concepts required to tell someone how to make the boxes, and so we have the analog of objective state preparation. The total "theory" of the boxes consists of three claims: that the strict correlations are as we have described them, that the correlations are non-signaling, and that the boxes are causally local. For all this says, there could be a more detailed theory, and the boxes could be a special case of something more general. The point will remain the same: the correlations do not force the objectivist into a choice between consistency and locality. The objectivist can say that what happens locally is undetermined, that outcomes are correlated as described, and that the correlations provide for objective probabilities. When we add the probabilistic version of the no-signaling stipulation, all the probabilities are determined, with marginals of 1/2 for the results of opening the various drawers.

How might quantum theory be different? One obvious difference is that talk of quantum states is inseparable from quantum mechanics. The story of the boxes is entirely innocent of such notions as "wizard-box states." But while this is true, the only way it could create problems for the objectivist is if objectivism about quantum probability requires some particular problematic view of quantum states. In fact, it does not. It does not even require that all states assignments be understood in the same way.

It is certainly true that well-behaved sets of quantum probabilities go with quantum states in at least this sense: we can find at least one density operator that yields the probabilities *via* the Born rule. Depending on the set of probabilities, it may be that the density operator that captures them is uniquely determined. But even in this case, objectivists do not have to agree that there is a uniquely correct state assignment. Not all state assignments serve the same purpose.

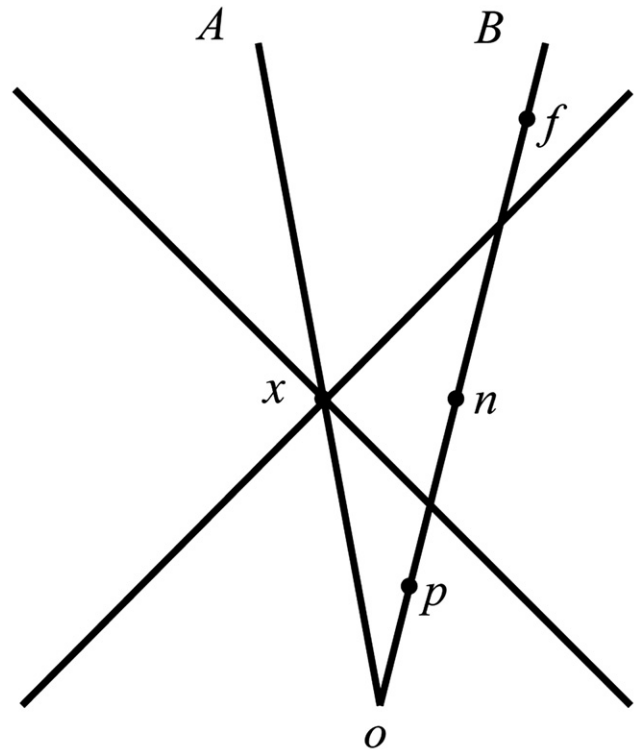

Fig. 4. Space–time diagram.

To see this, consider the singlet state. Alice and Bob share a singlet pair. Alice measures *z*-spin at *x* and finds +1. She is interested in what Bob may or may not have been up to at *n*, outside her light cone: (Fig. 4).

What should Alice say if she accepts indeterminism, causal locality and objectivism?

By now it should be clear what she *would not* say. She would not say that if Bob *were to* measure *z*-spin, he *would* find –1. Instead, she will say that if Bob *did* measure z-spin, he *did* find –1. But if he did not, she will say nothing about what he would have found if he had.

Alice will also accept a unit conditional probability: the probability that Bob finds –1, conditional on his measuring *z*-spin, is one. She can represent the information that this probability carries with it by assigning the state $|z->$ to Bob's qubit. But that does not call for saying that $|z->$ is the only state assignment anyone could appropriately make. In fact, suppose that Bob did not measure *z*-spin at all, but measured spin in direction d, skew to *z*, and got the result –1. Bob will assign the state $|d->$. Do he and Alice disagree?

Not at all. They are making use of different information. By assigning the state $|z->$, Alice is not attributing an intrinsic property to Bob's qubit. The assignment serves a bookkeeping function. She uses $|z->$ to locate this case in the proper set: cases where if Bob actually did measure *z*-spin, the result was -1. This state assignment is not associated with a particular point on the trajectory of Bob's qubit. The correlations are indifferent to whether Bob's measurement is at *p*, at *n* or at *f*. Alice's state assignment does, of course, go with a subjective probability—a willingness to bet conditionally, if you like. Should she find out that Bob actually measured *z*-spin, she will be certain that the result was -1. But Alice, objectivist that she is, will add that this credence reflects something about the world: the objective probability that Bob found result -1, conditional on Alice and Bob both measuring z-spin and Alice getting result +1, is one. This objective probability, in turn, is a reflection of a lawful, counterfactual-supporting generalization.

Of course, nothing in this state assignment requires that Bob actually did measure z-spin. If he measured spin in some other direction d, Alice will agree that the appropriate state for Bob to assign is $|d+>$ or $|d->$, depending on the result. In fact, she will agree that if she only knew what Bob knows, she would use his state assignment; he's the one who actually interacted with his qubit. And though probabilities other than zero and one are not our main focus, we can easily extrapolate to what Alice would say. Given the result of her z-spin measurement, the probability that Bob got the result +1, conditional on his having measured d-spin, is $\sin^2(\theta_{zd})$, where θ_{zd} is half of the angle between z and d. For -1, of course, it is $\cos^2(\theta_{zd})$.

Alice does, indeed, have degrees of belief. As an objectivist, she picks them because she believes that if quantum theory is correct, the pattern of events in the world has a certain character. Her credences reflect her view of what she takes the objective probabilities to be, and she and Bob can both be right about all this. Indeed, if quantum theory is correct, they both are.

6.1. States and counterfactuals

Is this all there is to state assignments? The objectivist does not have to say yes. Some state assignments support counterfactuals, and some do not. An obvious example of one that does is preparing a qubit with spin +1 in the z direction. (Here we assume, *pace* CFS, that objective state preparations are possible.) In light of our earlier discussion, we will assume that we really can prepare states such as $|z+>$. But when we do, we can make counterfactual claims: were this qubit measured again for spin in direction z, the result would be +1.

We routinely make such counterfactual assumptions about quantum processes. We assume that certain micro-switches would behave in certain ways if we used them appropriately; we assume that a medical laser produces light that will reliably behave as needed, and that were we to use the device, it would do what we intend it to do. Readers can no doubt provide their own examples. Cases of this sort do not have to do with tendentious assumptions about systems outside our lightcones; they are quotidian beliefs about gadgetry that we have come to take for granted. Of course, when the computer user or the laser surgeon makes such counterfactual assumptions, she probably would not couch them in the language of quantum states, but this goes with the points made earlier. We know how to do certain things. Some of them we knew how to do before quantum theory were invented, and we redescribed them in quantum terms. Others we might well never have learned but for quantum theory. But the counterfactuals and empirical generalizations are more resilient than the theory we use to describe them. *If* quantum theory is correct, we know how to describe the technology in its terms. The quantum descriptions generate appropriate probabilities—including unit probabilities. If quantum theory is in some way flawed, capturing the probabilities for these now-mundane cases will be a constraint on the acceptability of potential successor theories.

As for interpreting quantum theory itself, spelling out exactly what counts as a state preparation and what does not – and when counterfactuals can be asserted and when they cannot – is beyond the scope of a reply to CFS. It seems plausible that whether we attribute counterfactuals has to do with what we actually interact with, but that vague remark is hardly an account. For now, we can simply say: not all state assignments go with counterfactuals, but some do. *When* they do, however, it seems likely that the counterfactual comes first and the state assignment reflects it. Whatever the details, the tendency to think that all state assignments serve the same purpose gets in the way of clear thinking about quantum states.

7. Concluding remarks

Here is the main point in brief: supposing quantum theory is correct, there are counterfactual-supporting lawful generalizations such as if a pair of qubits prepared in the singlet state are both subjected to spin measurements in direction d, then the results will sum to zero. Generalizations like this ground conditional probabilities of one. But they can hold without instruction sets that fix individual outcomes, and without causal signals passing amidst the pair. This means that when Alice makes her probability-one claim about Bob's qubit, she does not need to infer pre-existing properties nor attribute counterfactuals. On the contrary, if she wants to square her objectivism with causal locality, those are exactly the things she should not do. Being an objectivist calls for less than CFS assume.

That said, I would not suggest for a moment that their argument was simply foolish. On the contrary: the temptation to think that EPR-type cases sanction counterfactual inferences is remarkably hard to resist. My own thinking about exactly these sorts of cases was turned around by having to reflect on CFS's argument. And even though we still do not agree about quantum probability, I believe that the kind of view gestured at here should be less uncongenial to them than the one they oppose in their paper.

There is one issue as yet unspoken: the varieties of objectivism assumed here takes for granted something notoriously problematic: there are such things as measurements, and they have definite results. More generally, there are things that happen in the quantum world, in spite of the fact that the standard dynamics makes this seem mysterious. I'm happy to agree with Fuchs that the quantum world:

> ..is sensitive to our touch. It has a kind of "Zing!" that makes it fly off in ways that were not imaginable classically." (Fuchs, 2002, pp. 8–9)

How that works is not something I can say. But if there are patterns in the events that betoken the "Zing!" then the objectivist may not need much more.

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