being done today, especially work on providing dynamics to modal interpretations and on exploring the relations between modal interpretations and the formal structure of Hilbert space. It is extremely encouraging to see the various strands of interpretations, which had for so long a time lost contact with one another, being brought together with such clarity and precision.

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Reference

Albert, D [1992]: *Quantum Mechanics and Experience*, Cambridge, MA: Harvard University Press.

The Many Faces of Non-Locality: Dickson on the Quantum Correlations

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1 Introduction

Philosophers of physics, it seems, have been barking up the wrong tree. Michael Dickson believes that many of the questions that have traditionally plagued the philosophy of quantum mechanics cannot be solved. These are metaphysical questions about the nature of the quantum world such as: 'is quantum reality non-local?'; 'is it indeterministic?'; 'are there definite outcomes to measurement procedures?'; 'is there causation in the EPR–Bohm experiment?'. The questions that we *should* have been asking, and which do have answers, are more specific questions about the models that physicists employ to represent quantum phenomena: 'in what way is Bohm's theory deterministic?'; 'are modal interpretations stochastic?'; 'are collapse theories non-local?'; 'is there action at a distance in the Everett-style interpretations', and so on.

This attitude to the philosophy of quantum mechanics may be called *deflationism*. I am in favour of deflationism. It is a needed corrective to the excessive metaphysical speculation that quantum mechanics has been prone to. But I don't think Dickson goes far enough. At a crucial step in his argument, in discussing non-locality, he surrenders the position. He derives conclusions that are general, and model-independent. I would like to keep him squarely in the deflationary camp, and I offer some arguments that show that these general and model-independent conclusions are not inevitable.

2 Some quantum theories

Quantum Chance and Non-Locality aims to provide an analysis of probability and action-at-a-distance as they appear in the different interpretations and theories of quantum mechanics. These include: collapse theories (such as the orthodox—Copenhagen—interpretation, and the Continuous Spontaneous Localization—CSL—approach); the Everett relative state formulation and its many-worlds and many-minds interpretations; modal interpretations (such as the Kochen–Healey–Dieks and Dieks–Vermaas); and the Bohm theory (in its minimal and non-minimal versions).

Dickson lays out a useful classification. He employs two tools: the *eigenstate–eigenvalue link* ('e/e link') and a definition of *determinism*. The *e/e link* says that an observable A has a value in a system in state W if and only if $Prob_W(\boldsymbol{P}^{A}{}_{a}) = 1$, where $\boldsymbol{P}^{A}{}_{a}$ is the projector that corresponds to the a eigenvalue in the spectral decomposition of the operator that represents A. In other words, the *e/e link* lays out a necessary and sufficient condition on the value-definiteness of observables.

A theory is deterministic, according to Dickson, if there is only one future state of the universe that is consistent with each possible present state. (Different present states may correspond to one and the same future state.) Dickson then shows how collapse theories obey the *e/e link* but violate *determinism*; the Bohm theory violates the *e/e link* but obeys *determinism*; modal interpretations violate both; and Everett-style interpretations obey both. The rest of the book is a discussion of these interpretations.

3 Chance

The first part of Dickson's book is an instructive guide to the advantages and disadvantages of these interpretations *vis-à-vis* quantum probability. There is no clear winner: all interpretations are seen as worthy and interesting in their own right. But overall the modal interpretation and the Bohm theory fare rather well, a little better perhaps than their *e/e link*-obeying competitors. Here Dickson may be just a little unfair: the Continuous Spontaneous Localization theory, for instance, has had more successful physical applications than, say, the modal interpretation. And the relative state formulation (without any additional many-worlds or many-minds interpretation) has the advantage of neutrality—it describes formally the relative properties of entangled systems, and this minimal description is accepted by every interpretation.

Dickson also defends modal interpretations against the criticism that they are not grounded in physical intuitions. I found this defence unconvincing. The point of the criticism is this: Bohm's theory and the CSL approach have as their starting point and motivating force a revision in the physics—both begin by asking us to review our physical intuitions about the properties and dynamics of quantum systems. It is not surprising that these theories provide effective tools for building applications to concrete physical systems such as harmonic oscillators, particles in potential wells; and more complex ones such as neutron interference experiments, tunnelling, the hydrogen atom, etc.[1] By contrast the modal interpretations' starting point is a radical revision in semantics (the abolition of the *e/e link*), coupled with an unashamedly conservative attitude to the physics. The advocates of this interpretation are just now (thirty years after the interpretation's birth!) beginning to concern themselves with, for example, providing a *dynamics* (and it is to Dickson's credit that he has been at the forefront of this exciting research—much of it in collaboration with Guido Bacciagaluppi).

So the objection is not that modal approaches fail to offer any interpretational insight, nor is it that they do not fit the facts; the objection is rather that as a programme of empirical research, modal interpretations seem impoverished—it is difficult even to begin to imagine what kind of experiments we could possibly design to test, extend and develop the interpretations in any particular direction (other than the kind of experiments that we may design to extend, develop or test Bohm's theory itself, which Jeffrey Bub has shown can be construed very abstractly as a type of modal interpretation). To use Lakatosian terminology, the modal interpretation does not seem fit as a

[1] For physical applications of the Bohm theory, see, for instance, P. Holland ([1993]) and J. T. Cushing ([1994]). For applications of the Continuous Spontaneous Localization approach, see I. Percival ([1999]) which I review, with Adam Brocklehurst, in [2000].

research programme in empirical science, never mind a *progressive* one. It may be a programme in the logical analysis of established science, but not in physics.

To the extent that this is an objection, Dickson tries to meet it by suggesting that the set of well-defined properties that modal interpretations postulate to be well defined is relative to the physical situation at hand. Different physical entities will be, in specific circumstances, endowed with certain properties and not others. What properties they possess will depend as much on the circumstances as on the nature of the entities. Consider a gas in a chamber, which lacks the property of surface tension; under different circumstances (i.e. a lower temperature) the same (liquefied) gas may none the less possess that property. Why not expect quantum particles to be similar? Dickson shows that modal interpretations are able to accommodate this possibility: for any one system, the set of properties that these interpretations take to be definite can be made to vary with changes in the physical situation.

But is this more than an empty truism? How could *any* interpretation of quantum mechanics fail to accommodate this possibility? Wouldn't any 'interpretation' which did immediately conflict with quantum mechanics itself—if indeed quantum mechanics is empirically adequate? The claim that in modal interpretations the set of well-defined properties is relative to physical situations does not constitute a physical application in itself—it merely allows for the possibility of one. The objection asks for *actual* applications of modal interpretations; it isn't convincing to be told that applications are *possible*.

4 Non-locality

The second part of Dickson's book concentrates on the EPR–Bohm correlations. The crucial Chapters 6 and 7 of the book contain the main results. A distinction is there made between static and dynamic models of the EPR–Bohm experiment. The former calculate the probabilities for outcomes of measurements on each wing of the experiment conditional on the (complete) state of the pair of particles as they are emitted at the source. The latter calculate the probabilities conditional on the complete state of the pair at the time of measurement. (Alternatively: both models calculate the probabilities conditional on the complete state of the pair at the time of measurement, but the static models assume that the state of the pair does not evolve in time, while the dynamic models assume that it does.)

4.1 Static models

The main result reviewed in Chapter 6 is the well-known fact that the condition of (static) factorizability entails (two-time) determinism. It is worth recalling what these conditions are here; in simpler notation than Dickson's, they are (here x_1 is the outcome of the measurement on particle *1*, x_2 is the outcome of measurement on particle *2*, and *S* is the complete state of the particle pair at the source):

Factorizability (FACT): Prob $(x_1/x_2 \,\&\, S) = Prob\ (x_1/S)$,

Two-Time Determinism (2-DET): $\forall i = 1,2$, *Prob* $(x_I/S) = 1$ *or* 0,

In words, (FACT) states that the measurement outcomes are probabilistically independent, conditional on the state. (2-DET) states that the conditional probability of any outcome on either of the wings, given the state, is one or zero. I won't show it here, but is easy to prove that (FACT) and (2-DET) are equivalent in the case of the perfect EPR–Bohm anti-correlations. This equivalence result, which is central to Dickson's subsequent discussion of non-locality, is essentially a special case of a theorem by Arthur Fine.[2] Fine shows a special case of the following in the more general case of quantum correlations between the outcomes of measurements by polarizers set in arbitrary directions: if these correlations have a (FACT) model then they have a (2-DET) model, and vice versa. In the special case of perfect anti-correlations, however, a (FACT) model is already a (2-DET) model, so in that special case, (FACT) and (2-DET) are equivalent.

Dickson reads the equivalence result as implying that any stochastic theory must be non-local, in the sense of involving action-at-a-distance between spatially separated events. This is because he understands the factorizability condition (FACT) to be a condition of locality. (And therefore he takes the Bell inequalities to lay out conditions that local theories must satisfy). The equivalence shows that no theory that violates (2-DET), i.e. no genuinely stochastic theory, can obey (FACT). Dickson concludes that no stochastic model of the EPR–Bohm correlations can be local.

This a very strong conclusion about all stochastic theories in general, and it does not depend upon the details of any particular model. Dickson's willingness to accept it is strangely at odds with the overall aim of his book—to show that only those claims about chance and non-locality that are made in the context of particular models can be warranted. Fine himself drew precisely the opposite conclusion from his theorem: he concluded that local stochastic theories are not ruled out because (FACT) is not a plausible condition of physical locality. While Dickson takes the equivalence of (FACT) and (2-DET) to constitute a *reductio* of locality (for all stochastic theories), Fine's own, more *deflationary*, attitude is that we should never in principle foreclose the issue of whether local, non-factorizable, stochastic theories exist; and, in a conjectural spirit, he has proposed a

[2] Fine ([1982a]). The original result is in Suppes and Zanotti (1976).

detailed non-factorizable stochastic model of the EPR–Bohm correlations, the *synchronization model*, which he has claimed to be local.[3]

Whatever the merits of Fine's *synchronization model*, I think that it is better to leave the issue of non-locality open and unprejudiced, and to separate carefully the notion of physical locality and the formal condition of factorizability. The connection does not hold in general, for instance for a single particle, as is shown by the following example. Consider a non-Markovian process, which may well be taken to describe the evolution in the state of a (physically possible) particle. The present state of a stochastic non-Markovian process does not screen off the history of previous states from the future state of the particle; and factorizability is violated (S_f is the future state of the particle; S_p is its present state; S_h is the history of its previous states.): $Prob(S_f/S_p \& S_h) \neq Prob(S_f/S_p)$. But this is not a reason to deny that the evolution of the particle has indeed been local. Physical locality does not in general entail factorizability.

It is for this reason that, in my view, Dickson is mistaken in adopting the identification of factorizability with physical locality, and this has serious consequences for the rest of the book. It is unclear to me, however, how deep the source of the equivocation is. At one point in the book (p. 174), Dickson explicitly refers to (FACT) as a *probabilistic* condition, 'because the notion of independence that [it] employ[s] is probabilistic independence'. It is difficult to figure out from the text exactly what the contrast is supposed to be with, but one possibility is with 'physical independence'. If so, Dickson would be in conceptual agreement with Fine, for there is no reason to suppose that physical and probabilistic independence overlap; and the dispute would turn merely on an unfortunate terminological choice. However, in at least one occasion, which I shall now discuss, Dickson adopts an assumption that is only consistent with the contentious identification of factorizability with physical locality.

4.2 Dynamic models

In order to describe determinism in a dynamical setting we need not one but two different conditions: one for the outcomes given the initial state of the pair; and another for the evolution of the (complete) state. Dickson calls the first *deterministic results*, and the latter *deterministic transitions*. In my own simplified notation (where x_i are the outcome events, S_0 is the state of the pair at the source, and S_t is the state at the time of measurement):

Deterministic Results (DET-Res): $\forall i=1,2 \; Prob\,(x_i \,/\, S_0) = 0 \text{ or } 1.$

[3] Fine ([1982b]).

Deterministic Transitions (DET-Trans): Prob (S_t / S_0) = 0 or 1.

(DET-Res) says that the state S_0 ascribes probability one or zero to every possible outcome. (DET-Trans) says that the conditional probability of any future state of the pairs, conditional on the initial state of the particles being S_t, is one or zero. These conditions jointly suffice to determine whether a model is fully deterministic. In addition they serve to characterize models that are deterministic in the evolution of the complete state, but indeterministic in the generation of outcomes (i.e. models that obey (DET-Trans) but not (DET-Res)). These models fully determine the complete state of the pair of particles at all times, including the measurement times; but instead of determining outcomes with probability one, these models merely determine a probability distribution over the possible outcomes. I call them *inherently stochastic models*.

Dickson none the less maintains that an additional condition is required, namely a condition of state determination:

State Determination (S-Det): Given that the time of the occurrence of the outcome for particle i is t, the complete state at t, S_t, completely determines the outcome.

This condition assumes full determinism of outcomes, and is, in my view, highly controversial. Dickson seems to think that the condition is reasonably innocuous. He writes (p. 150):

> This condition might appear strong at first—for example, it might appear to be an assumption of determinism—but in fact it will turn out to be trivial on my definition of the [states S]. (They will turn out to be complete states of regions of space-time including the region occupied by the detector.)

The idea is to let the complete states *S(t)* of the pair of entangled particles represent not just the state of the pair at time *t* but also the state of a whole space-like slice that includes at least also the measuring devices at time *t*. (S-Det) then follows as part of the definition of the complete state *S*: if the measurement is made, and the outcome recorded at *t*, the state S_t must fix the outcome by definition—for the outcome is included in the state.

But why should the complete states of quantum particles be taken to represent entire regions of space-time? After all, quantum mechanics has always been thought of as a theory of *matter*, not of *space-time*. Dickson offers very little to justify this choice.[4] There is however one obvious reason

4 A vague hint is offered that this choice may render the individual states of each particle (described by space-like hypersurfaces *within* the backwards light-cones of each measurement) formally covariant. But why accept space-time symmetry considerations at this point, when the pertinence of these considerations is precisely what is at issue?

for taking the state to represent a slice of spacetime that contains the measurement devices: this is the choice that makes (S-Det) analytically true. If the state is meant to *contain* the outcome, then there is no way that the state can fail to determine the outcome; and (S-Det) holds.[5]

But note that in an EPR–Bohm set-up where particles evolve independently of each other, (S-Det) implies that the composite state of the entangled pair at the source renders the outcomes on the wings stochastically independent from each other. To see this, take particle *A*'s individual state at the source to be $S^a{}_0$, and particle B's to be $S^b{}_0$. In quantum mechanics, at least, these states are (almost always) uniquely determined by partial tracing from the complete state S_0 of the pair at the source. They evolve into S^a_t and S^b_{t}, where *t*, *t'* are the times of measurements on *A*, *B*, and we assume that the evolution is independent in the sense that: $Prob(S^a{}_t \,/\, S^a{}_0 \,\&\, S^b{}_t) = Prob\,(S^a{}_t \,/\, S^a{}_0) = Prob\,(S^a{}_t \,/\, S_0)$ and similarly for *B*. (The first equality is Dickson's own condition of independent evolution for dynamic models of the EPR; the second equality follows if, as is almost always the case in quantum mechanics, S_0 uniquely determines $S^a{}_0$ and $S^b{}_0$.) By a local version of (S-Det), $S^a{}_t$ on its own determines the outcome x_1 of the measurement on *A*, while $S^b{}_t$, on its own determines the outcome x_2 of the measurement on *B*. It follows, from the probabilistic independence of $S^a{}_t$ and $S^b{}_t$, that $Prob\,(x_1 \,/\, x_2 \,\&\, S_0) = Prob\,(x_1 \,/\, S_0)$. In other words, in such a set up, and assuming (S-Det), factorizability holds.

Now, the condition of independent evolution *may* be taken, and is indeed so taken by Dickson, to express a condition of *physical* locality, and therefore, if (S-Det) is analytic, factorizability follows from the only empirical assumption of physical locality. Thus, (S-Det) *is* highly controversial, and certainly so for stochastic theories; for it has a highly controversial consequence, namely that physical locality requires factorizability. Anyone who does not believe that this consequence of (S-Det) is true for genuinely stochastic theories will not believe that (S-Det) is true in those theories.

What kind of models might violate (S-Det)? Consider *inherently stochastic models* where the complete states refer only to the properties of point-particles. By definition these models satisfy (DET-Trans): the complete initial state of the pair of particles, S_0, determines with probability one the subsequent states, including the states of the pair at the times of the measurements, S_t and $S_{t'}$. If (S-Det) were true of these models then the states S_t and $S_{t'}$ would determine the outcomes of the measurements with

[5] A version of Dickson's choice is defended in Butterfield ([1989]). Butterfield also derives (FACT) by assuming that the complete states refer to whole regions of spacetime. But he does not assume that these regions include the outcomes. Besides, Butterfield's explicit agenda is to find a justification for (FACT) in general, so that the violation of (FACT) by quantum mechanics turns out to be surprising, and at odds with the rest of physics. This is not Dickson's agenda.

probability one. It follows that if (S-Det) were true of these models, (DET-Res) would also be true, for the initial state at time t_0 would determine with probability one the outcomes at times t and t'. But (DET-Res) is false in these models by definition. Therefore (S-Det) must be false in *inherently stochastic models*, as long as the complete state of a quantum particle is taken to refer to nothing but the particle.

Are *inherently stochastic models* non-local? Dickson's answer must be yes, necessarily. For he thinks that any empirically adequate, stochastic model is non-local. But on the dynamical definition of locality that he himself offers us, they aren't necessarily so. For if (S-Det) fails, as it does in *inherently stochastic models*, the individual states of the particles may well factorise conditional on the state at the source, in the way required for independent evolution, while the outcomes of measurements on such particles may not factorise at all. Only if (S-Det) is true does the factorization of outcomes follow from the factorization of states. And recall that the derivation of Bell's inequalities, which are violated by experiment, requires the *outcomes* to factorise. So in these models the *states* may factorise, conditional on the state at the source, and yet the models may be empirically adequate as the measurement outcomes may fail to factorise conditional on that state, and hence may well reproduce the quantum statistics.

5 Independent evolution

There is a further, deeper problem. I have here been adopting Dickson's own definition of independent evolution. The definition *assumes* that the physical evolution of two particles is independent of each other, *if and only if* their evolved states factorise conditional on their initial states. But recall my counterexample to the identification of locality with factorizability: a non-Markovian process where the future *states* of a system are not screened off from its history by the system's present *state*. This counterexample applies to the factorizability of *states*! The notion of independent physical evolution has indeed much to recommend itself as a notion of locality. But unfortunately Dickson again chooses to represent this notion formally in terms of a factorizability condition. This choice is bound to fail: there is in general no necessary connection between physical locality and probabilistic independence.

Never mind. Ignore the formal representation of physical independence as factorizability. The intuition behind the notion of physically independent evolution is still clear. The evolution of particle A is determined by the initial state of the particle A and nothing else; in particular it is not physically influenced by any of the states of particle B. And vice versa: the evolution of particle B is physically independent of particle A's. Are *inherently stochastic models* of the EPR–Bohm experiment non-local in *this* sense? Well, maybe they are; but maybe they are not. Nothing can settle this issue in advance. We have to look at the details of the models themselves. For instance, it does not seem possible to rule out a dynamic extension of Fine's *synchronization model* that would be local in the required sense: every particle is ascribed a state, and there is no presumption in Fine's model that the *evolution* of that state is in any way *physically* influenced by the any of the states of the distant particle.

What consequences does this have for Dickson's analysis of non-locality in the different interpretations of quantum mechanics? Often it doesn't make a difference for, as it happens in most cases, when factorizability fails, we also have reasons to think that the evolution of one particle is not independent of the other's. In a couple of cases, however, it matters what locality really is. In discussing Dennis Dieks' response to the charge of non-locality for modal interpretations, for example, Dickson offers an argument that is based on the failure of physical independent evolution, not factorizability. This may be a case where there is genuine physical non-locality even if there isn't a failure of factorizability.

More importantly, perhaps, it isn't clear to me that what rules out locality in the Continuous Spontaneous Localization models of the EPR–Bohm experiment is anything other than the failure of the formal condition of factorizability. It is not immediate that in these models there is also a failure of independent physical evolution. So this may be a case where the model is really local, but Dickson's analysis makes it look non-local. These issues are subtle and require more attention that I can give them here. But I hope that these two examples at least show that the debate over the relation between locality and factorizability is important and can not be avoided.

6 Conclusion

Quantum Chance and Non-Locality is full of interesting distinctions and definitions, and subtle arguments. It is a thought provoking book that advances our knowledge of the subject. No philosopher of physics can afford to ignore it. But there are no general lessons that one can safely derive about the nature of the quantum world. Not even the lesson—merely a conditional—that Dickson is inclined to derive, namely: that if quantum processes are genuinely stochastic, there is non-locality. This conditional statement is not true in general. It requires independent assumptions—and these assumptions are neither necessary nor likely. In the absence of detailed physical models, it is simply not possible to settle these general questions. True *deflationism* indeed!

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References

Brockelhurst, A. and Suárez, M. [2000]: Review of I. Percival, *Quantum State Diffusion*, *British Journal for the Philosophy of Science*, **51**, 3, pp. 527–530.

Butterfield, J. [1989]: 'A Space-time Approach to the Bell Inequality', in J. T. Cushing and E. McMullin [1989], pp. 114–44.

Cushing, J. T. [1994]: *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*, Chicago: University of Chicago Press.

Cushing, J. T. and McMullin, E. [1989]: *Philosophical Consequences of Quantum Theory*, Notre Dame, IN: University of Notre Dame Press.

Dickson, M. [1998]: *Quantum Chance and Non-Locality: Probability and Non-Locality in the Interpretations of Quantum Mechanics*, Cambridge: Cambridge University Press.

Fine, A. [1982a]: 'Hidden Variables, Joint Probability and the Bell Inequalities', *Physics Review Letters*, **48**, 5, pp. 291–95.

Fine, A. [1982b]: 'Some Local Models for Correlation Experiments', *Synthese*, **50**, pp. 279–94.

Holland, P. [1993]: *The Quantum Theory of Motion*, Cambridge: Cambridge University Press.

Percival, I. [1999]: *Quantum State Diffusion*, Cambridge: Cambridge University Press.

Suppes, P. and Zanotti, M. [1976]: 'On the Determinism of Hidden Variable Theories with Strict Correlation and Conditional Statistical Independence of Observables', in Suppes, P. and Zanotti, M. [1996].

Suppes, P. and Zanotti, M. [1996]. *Foundations of Probability with Applications*, Cambridge: Cambridge University Press.