Review

Reviewed Work(s): The Interpretation of Quantum Mechanics by Michael Audi

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BOOK REVIEWS

MICHAEL AUDI. ***The interpretation of quantum mechanics.*** Chicago: University of Chicago Press, 1973. xiv + 200 pp. $11.50.

Audi's main aim in this book is to show that if indeterminism is genuinely accepted, all the philosophical problems of quantum mechanics become tractable (pp. *ix*, 142, etc.). This sounds promising. For surely indeterminism would be a small price to pay to avoid the interpretation problems which have plagued the quantum theory since its inception. And, of course, nearly everyone thinks that quantum mechanics is in some sense indeterministic.

To evaluate Audi's claim we need to know what it is we must genuinely accept. According to Audi, a deterministic theory must satisfy three criteria: (*i*) given the values of its state variables for some initial period, the theory must logically determine a unique set of values for any other period (p. 61); (*ii*) given a certain degree of detailed information about an isolated system at a particular time, it must be possible to obtain the same degree of detailed information about it at all other times from the equations of the theory (p. 62); and (*iii*) the theory must be, in principle, deterministic with respect to any extension or perturbed state of a previously isolated system (*ibid.*). The first of these criteria demands that deterministic theories have "determinate" equations of motion, *i.e.*, equations which yield a unique solution given the appropriate initial conditions. The second, amounts to the demand that deterministic theories *not* incorporate a superposition principle (p. 64). And the third, amounts to the demand that the eigenvalue problem corresponding to a new "perturbed" Hamiltonian function, $H + \delta H$, have an exact solution if the eigenvalue problem corresponding to H has an exact solution (p. 68).

Now, it is Audi's contention that the quantum theory satisfies (*i*), but not (*ii*) and (*iii*). It satisfies (*i*) since the Schrödinger equation is a first-order partial differential equation in time. But it fails to satisfy (*ii*) since an isolated quantum-mechanical system can start in an eigenstate and evolve into a superposition state. And it fails to satisfy (*iii*) since whether or not the eigenvalue problem corresponding to $H + \delta H$ has an exact solution depends on the nature of the physical system and the perturbation.

This reasoning, however, is open to objection. First, it is not obvious that the quantum theory satisfies (*i*). Certainly some theorists, *e.g.*, Wigner, have argued that quantum-mechanical systems evolve in two distinct ways: deterministically *in the absence of measurement* and indeterministically when measurements are performed. (For Audi on measurement, see below.) Second, point (*ii*) involves our already having adopted a particular (partial) interpretation of quantum mechanics, since the point rests on our identifying physically possible states with eigenstates (cf. p. 64). Third, since the Schrödinger equation for an unperturbed system may or may not have a unique solution (roughly, it depends on whether or not the conditions of the Cauchy-Koweleska theorem obtain) point (*iii*) adds nothing new.

Taking these points into account and remembering that we cannot at this stage of the argument make an appeal to any particular interpretation, determinism *à la* Audi amounts to the following: A theory is deterministic just if given the complete state description of a system at any time (i.e., a description which is "maximally informative") the theory yields its complete state description at any other time. (This is certainly not as precise as it might be, but it will suffice for our purposes.) If this is a reasonable reconstruction of what Audi is saying, his main claim is this: If we accept that quantum mechanics is not deterministic in the sense just indicated, all the interpretation problems become tractable.

(It is perhaps worth noting here that determinism is frequently and, I believe, correctly taken to involve not only "determinateness of evolutions" as in the above but also "determinateness of quantities", *i.e.*, the idea that a deterministic theory must always yield precise or "sharp" values for all its magnitudes. Further note that there are strong arguments to show that quantum mechanics also fails to satisfy this component of determinism. However, since Audi does not consider it and since, as we will see, on his preferred interpretation quantum mechanics does *not* fail in this regard, let us ignore it and take determinism to involve only determinateness of evolutions.)

As restated, Audi's main claim is false. The reason for this is that there are a host of interpretations of quantum mechanics which are compatible with non-determinate evolutions. If states are taken to be clusters of magnitude values, the quantum theory almost always fails to single out a particular state of a system at a time, given its state at some other time. If states are identified with eigenstates, determinism fails since quantum mechanics incorporates a superposition principle. If states are identified with state-vectors, indeterminism still reigns provided there is a reduction ("collapse") of the state-vector during measurement. And so on.

So, if one reads Audi's book as an attempt to establish the claim that the failure of determinism in the quantum-mechanical realm explains the puzzling features of quantum mechanics, it will be found singularly unconvincing. Fortunately, however, there is a more interesting and charitable way of reading it, namely as a sustained defence of an indeterministic particle interpretation of quantum mechanics. Read this way, we see Audi as arguing that alternative interpretations are either physically or conceptually inadequate and that well-known objections to particle interpretations can in fact be satisfactorily handled.

Audi considers three alternative interpretations: the Copenhagen interpretation, "the hidden variable interpretation," and the logical interpretation (which has it that we should forgo classical logic). To all intents and purposes, he dismisses the logical interpretation on the grounds that a rejection of classical logic is unwarranted given an indeterministic particle interpretation. So this dismissal depends on his providing a satisfactory particle interpretation. His argument against hidden variable theories rests on what appear to me to be some rather dubious claims concerning simplicity, initial plausibility, and extendibility of prediction. So I won't consider it further. More interesting is his argument against the Copenhagen interpretation.

Audi claims to attack this interpretation "on its central theses rather than by refuting disreputable philosophy . . . often associated with, but inessential to the interpretation" (p. *x*). In a nutshell, the argument goes as follows. Contrary to certain philosophers, not all the terms figuring in a theory are theory dependent. There is progress in science and scientific theories can be compared. So some terms are "meaning invariant." Furthermore, we have certain behavioral indicators which we can appeal to in order to determine whether or not a term which figures in more than one theory is meaning invariant with respect to these theories. But if we make use of these indicators we will see that when Bohr and others use the term 'particle' they are using it in the same way as, *e.g.*, Newton did. That is, 'particle' is meaning invariant between classical and quantum mechanics. But for something to be a particle it must possess simultaneous position and momentum (p. 30). So when Bohr talks about particles he is talking about things which have simultaneous position and momentum. Whence much of what Bohr and others say about particles is just confused.

Now, although Audi takes this argument very seriously (he calls it a "genuine" objection to the Copenhagen interpretation) it seems to me to have the status of a *reductio ad absurdum*. But of what? I suggest that what has gone wrong is that Audi himself is relying on disreputable philosophy. It seems to me that both Newton and Bohr could refer to the same thing and yet have different, and possibly incorrect, theories about particles. (This is a point which has been made recently by Putnam in [2].) We can hold this view if we hold a causal theory of reference. Newton and Bohr refer to the same thing since they both refer to that thing, whatever it is, which was singled out (by a not necessarily correct definite description) by the "introducer" of the term 'particle'. That they both refer to the same object is guaranteed, according to the causal theory, because both Newton and Bohr come to possess the term 'particle' via albeit distinct "chains of communication" which lead back to the same "introducing event." (For more details, see Putnam's [2], [3], and [4].) On the present theory, we can account for progress in science *and* make sense of what Bohr is saying. But, if the causal theory or something like it is correct, Audi's objection to the Copenhagen interpretation fails. At the very least, we must conclude that Audi has a lot more philosophical work to do to make his objection good.

Of course, I am not suggesting that all is well with the Copenhagen interpretation. It isn't: for a start, there's the so called quantum-mechanical measurement problem. So let us turn to Audi's positive view.

On indeterministic particle interpretations, particles have definite positions and momenta at all times, *i.e.*, they have definite paths; but the relevant forces do not determine these paths uniquely (p. 142). But matters aren't this simple. For, as has been emphasized time and time again, such interpretations seem unable to handle interference and diffraction effects.

Landé, who more than anyone has attempted to develop a satisfactory particle interpretation, deals with this problem by appealing the "Duane selection rule," which states that a body with a period l changes its momentum parallel to l only in increments of h/l. It is then argued that interference arises due to the fact that the body (e.g. crystal), not the particle, is spread out in space. Now, as Landé is well aware, this by itself does not solve the interpretation problem. We still have "quantum jumps." But Landé does not leave the matter here. He attempts, I believe unsuccessfully, (cf. [5]) to show that the Duane selection rule (as well the Planck and Wilson-Sommerfeld rules) can be derived without recourse to any of the "dualistic" aspects of the Copenhagen interpretation, i.e., wave-particle duality, complementarily, etc.

How does Audi approach this matter? After mentioning Duane (p. 105), Audi goes on to give an account of diffraction which makes no use of the Duane selection rule. He manages to show that there is only a discrete set of possible scattering angles, but to do this he has to assume that the incident beam of particles is "properly represented" (p. 108) by a *plain wave function* (p. 107). But this is surely something he cannot assume given his interpretation. So, it would seem that Audi's defence of the particle interpretation is even weaker than Landé's.

Audi usually refers to his view as a particle interpretation, but on occasion he suggests that what he is proposing is a statistical interpretation. For example he dismisses the so-called quantum-mechanical measurement problem on the grounds that "the quantum-mechanical description (applies) to statistical ensembles" (p. xii). But this view will only satisfy those who are willing to accept that quantum mechanics is *incomplete.* Furthermore, given such incompleteness, it would seem reasonable, contrary to Audi, to look for hidden variables (cf. [1]). (Of course, statistical interpretations, like standard particle interpretations, are plagued by the diffraction/interference problem mentioned earlier.)

These considerations convince me, Audi's book notwithstanding, that the interpretation problem for quantum mechanics is still very much with us. However, even if there are no answers to be found in the book, it does raise in a forceful way a number of interesting questions (only some of which I have considered here). Particularly commendable is Audi's avoidance of something which has been catching on lately, namely sophistical solutions to physical problems. Nevertheless, I should also add, some readers will find the tone of this book rather offensive. Surely Audi is overdoing it when he says that "much of the literature on quantum mechanics is unreadable and irrelevant" (p. *xiv*). It seems to me that the debate concerning the interpretation of quantum mechanics is one of the high spots of recent philosophical research. And given Audi's failures, it would seem to be far from irrelevant. *Andrew Lugg, University of Ottawa.*

REFERENCES

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[3] Putnam, H. "Meaning and Reference," *Journal of Philosophy*, 70 (1973), 699-711.
[4] Putnam, H. "Language and Reality." Unpublished.
[5] Shimony, A. "Review of Landé's 'New Foundations of Quantum Mechanics'," *Physics Today*, 19.9 (1966), 85-91.

RAIMO TUOMELA. **Theoretical concepts.** New York and Vienna: Springer Verlag, 1973. xiv + 254 pp. np.

Mario Bunge, in the General Preface preceding each volume published in the *Library of Exact Philosophy*, claims that "the more exactly we proceed in handling genuine philosophical problems, the narrower should become the gap between the humanities on the one hand, and mathematics and science on the other." Tuomela's volume, the tenth in the *Library*, is more likely to disconfirm than to confirm Bunge's claim.

The more's the pity. Tuomela's book, while sometimes annoying in its insistence on an exact and technical approach to its problems, and while sometimes less helpful than it ought to be to the reader whose technical equipment is limited, is an important defense of critical scientific realism, and especially of the methodological, epistemological, semantical, and logical indispensability of theoretical concepts. (Their ontological indispensability is adumbrated, but not defended.) Yet it will speak only to those already persuaded of the merits of exact philosophy, for it uses a formidable technical apparatus—drawing on proof theory, model theory, information theory, graph-theoretical semantics, second-order logics, Hintikka's distributive normal forms for first-order logics, and Hintikka's logic of induction. All of these formalisms are obviously well under the author's control and are used with a fairly light touch, but there can be little doubt that this work will not bridge the gap between scientists and humanists. It will not even bridge the gap between those philosophers of science who are disinclined to use formal techniques and those who employ them freely. The more's the pity.

In this review, I shall make no attempt to go into the wealth of technical results which Tuomela achieves, though a number of them are novel and of considerable importance. Rather, I shall provide an overview of his central argument and highlight a few problems regarding the relation of his technical results to his informal claims. For it is these problems which bothered me most in coming to grips with the book.

The argument. There is widespread agreement that the progress of modern science is due in large part to the use of theories, but there is serious disagreement as to the nature of specifically theoretical concepts, their function(s) in theories, and their importance for the progress of science. In particular, empiricist and instrumentalist philosophers and scientists have held that theories are tools for deriving and/or organizing statements of empirical laws, and that their primary uses are to systematize our experience and to make predictions. In line with this view, many thinkers have argued that the whole power of modern science is to be found in its empirical laws which, they allege, may be stated and systematized without the use of any specifically theoretical concepts or vocabulary. Such views are not currently popular, to be sure, but important philosophers and scientists still subscribe to various versions of them. Tuomela, in contrast, holds that "the goals of science cannot really be achieved by purely empirical laws, but *only* by introducing and employing theories containing so called theoretical terms (such as 'electron', 'gene', 'refractory goal response') which are not explicitly definable on the basis of observational concepts" (pp. 1-2). Such a point of view is commonplace at present, yet Tuomela is right when he complains "that relatively few really good and compelling arguments have been presented to support this view" (p. 2) and to controvert instrumentalism. His main purpose in this book is to provide sound and explicit arguments for the realist position just indicated. On the way he investigates a variety of current problems concerning the analysis and interpretation of theoretical terms and theoretical systems by means of partially formal techniques.

Tuomela fragments his large problem into a series of subproblems. The unwary reader will lose track of the larger problem from time to time, for the subproblems