Review Essay

Quantum measurement: on this side of paradox[1]

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Quantum Measurement: Beyond Paradox, Richard A. Healey and Geoffrey Hellman, editors, Minnesota Studies in the Philosophy of Science, XVII, University of Minnesota Press, Minneapolis, London, 1998.

1. "In an earlier era of 'natural philosophy', physics and philosophy of physics were quite inseparably intertwined, but in the modern age of proliferating specialization, fruitful communication across the disciplines has become the exception rather than rule. We would like to think that the workshop[2] and this volume are symptomatic of an ongoing process of reunification, one which can pave the way toward exceptional progress in this fundamental and highly challenging area, and others as well" — the editors Richard A. Healey and Geoffrey Hellman write in the Preface. The main topic of the ten articles in this volume, Quantum Measurement, indeed, requires both physical and philosophical considerations.

It is a widespread view that "Interpretation" of QM belongs to metaphysics, and it is entirely outside of the scope of a normal physical theory. I disagree with such a view because of several reasons. First of all, there is no such a sharp demarcation between physics and metaphysics. Only the most naïve physicist can believe that a physical theory is completely free of metaphysical assumptions, and, on the other hand, only the most obscure "metaphysical" specula-

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[1] Forthcoming in Daniel Greenberger, Wolfgang Reiter, Anton Zeilinger (eds.): *Epistemological and Experimental Perspectives on Quantum Physics*, Kluwer, Dordrecht, 2000.

[2] Workshop on quantum measurement held by the Center for Philosophy of Science at the University of Minnesota in May 1995. (L. E. Sz.)

tion can ignore the huge human experience accumulated in physics and other sciences. The second reason why one cannot separate QM from its Interpretation is that there is no physical theory without interpretation. Different interpretations of QM yield different theories about what is the world like. The fact of overall empirical underdetermination of scientific theories does not entitle us to think that there are no empirically testable differences among the various interpretations. The analysis of "Quantum Measurement" is important not for its own sake, but to test the consistency of alternative interpretations of quantum theory.

2. Let me first sum up the basic problem. Denote ψ_0 the quantum state of a system on which we perform a measurement of an observable $\hat{A}$. Let χ_0 be the initial state of the measuring apparatus. So, the initial state of the coupled system is $\psi_0 \otimes \chi_0$. During the measurement process the initial state evolves into a final state related to the initial state by a unitary transformation. If the initial state of the system is an eigenstate of observable $\hat{A}$ with eigenvalue a, then the final state is of the form

$$U\left(|a\rangle \otimes \chi_0\right) = \Phi_a \tag{1}$$

where it is assumed that the final state of the coupled system Φ_a corresponds to pointer position "a" in the sense that the probability of position "a" is $p_{\Psi_a}(\text{position "}a\text{"}) = 1$.

Now, let the initial state of the object-system be $\psi_0 = \sum_a c_a |a\rangle$, which is not an eigenstate of $\hat{A}$. Then the final state of "object + apparatus" will be

$$U\left(\left(\sum_a c_a |a\rangle\right) \otimes \chi_0\right) = \sum_a c_a \Phi_a \tag{2}$$

This final state is a coherent superposition of states corresponding to macroscopically distinct pointer positions. The probability, in the final state, that the pointer has position "a" is equal to $|c_a|^2$, just the same as the probability in the initial state ψ_0 that the observable $\hat{A}$ had the value a.

3. According to the two usual interpretations of QM *state,* we distinguish two major branches of interpretations of QM[3]:

(A) The *statistical interpretation*, according to which a pure state (and hence also a general state) provides a description of certain statistical properties of an abstract *ensemble* of similarly prepared systems, but need not provide a complete description of an individual system.

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[3] A good review on these two interpretations is provided in Ballentine (1970) and (1990).

(B) Interpretations which assert that *a pure state ψ provides a complete and exhaustive description of an individual system.* A dynamical variable represented by the operator $\hat{A}$ has value a if and only if $\hat{A}\psi = a\psi$.

These two interpretations actually yield two different versions of QM, which I will refer to as QM(A) and QM(B).

The issue of quantum measurement is no problem for A-theorists. According to statistical interpretation, the coherent state (2) does not mean that the apparatus "has no definite pointer position" at the end of an *individual* measurement process. For statistical interpretation assigns state (2) to an abstract ensemble of similarly initiated "object + apparatus" systems. In other words, according to QM(A), at the end of an individual run of the measurement the pointer may have a definite position at "a ", even if the statistical features of the whole ensemble are widely different from those characterized by the "eigenstate" Φ_a. If, as a particular case, the final state of the ensemble is Φ_a, then each member of the ensemble has pointer position "a " (with probability 1).

On the contrary, the final state (2) raises serious contradictions for QM(B). According to QM(B) the pointer must not have a position at all. And this claim is prima facie conflicting with the definiteness of macroscopically distinct configurations of the measuring apparatus, which we commonly experience and appeal to in the very laboratory practice of testing QM itself.

4. The collection of articles begins with Anthony Leggett's review on the conflict between QM(B) and "our common-sense, realistic conceptualization of the everyday world". On the basis of an analysis of the two-slit experiment he formulates the standard view on a coherent superposition as follows:

> When a microscopic system belongs to an ensemble where the correct QM description is by a linear superposition of probability amplitudes for two different states..., then [in general] it is not true that one or other of these two states has been realized. (Q1)

Leggett applies his Q1 not only for the measurement processes but he takes it as a general rule of QM and asks whether we are to reaffirm Q1, with "microscopic" replaced by "macroscopic". With the exception of the "relative-state" interpretation, he says, almost all the currently marked interpretations of the QM formalism answer this question in the negative. In Leggett's terminology "the currently marked" interpretations are the ones of type (B). He writes:

> One way out of this dilemma is to refuse to interpret the QM formalism in any way at all, that is, to deny that the assertion (Q1) has any meaning either at the microscopic or even at the macroscopic level. This is essentially the point of view taken by adherents of the full-blooded "statistical" interpretation of QM, which is a logical development of the Copenhagen approach. ... I personally find this (non)interpretation internally consistent but extremely uncongenial...

I don't really understand Leggett's "distaste" for QM(A), for his conclusion is actually the same as that of the adherents of the statistical interpretation. Namely, that 1) the contradiction between QM(B) and macroscopic definiteness is "irresolvable", and 2) "QM as presently conceived is not the whole truth about the world". This sounds a denial of QM(B), rather than QM(A).

Of course, I cannot comment everything written in the ten extremely meaty articles. So, I must skip Leggett's "Macroscopic Quantum Coherence" experiment and Abner Shimony's convincing comments on it, by which he sharpened the conclusions drawn from the expected violation of the "temporal version" of Clauser-Horne-Shimony-Holt inequality.

5. I want to continue with a few remarks on the vocabulary of conceptions, which I find sometimes loose throughout the book. In the first place, it is confusing that the terms 'to be in a quantum state', 'event', 'the state of the world/affairs', 'property', 'actualization of a potentiality', 'a variable takes value', 'property ascription', 'value assignment', etc. are frequently used as functional synonyms. The complete clarification of these notions and the relations among them would require a long and far-reaching analysis. I would like to mention only that, for an A-theorist, 'to be in a quantum state' is not an event. For a quantum state, even if we assign it to one individual system (in a "propensity" sense), is an abstract entity by which we describe the *probability* distributions over different sets of *events*.

For an A-theorist, since there is no such a correspondence between quantum states and physical events, a *superposition* of two states is definitely *not a disjunction* of events or alternatives. But it is not clear why a superposition is identified with a disjunction of "alternatives" in a B-theory. (See Leggett's analysis of the double-slit experiment, for instance.)

I don't believe that we can explain or understand anything with respect to the measurement problem by introducing such ambiguous terms like 'actual and potential properties' or 'sharp and non-sharp values'. On the one hand, these concepts are not needed for QM(A), and on the other they are entirely meaningless within the framework of the "complete and exhaustive" QM(B). The phrase 'definite outcome' is another vague term. How can an outcome event be other than definite? I wonder how I should instruct a laboratory assistant to assort the outcomes of the successive runs of a measurement into the classes "definite" and "not definite".

Finally I would like to mention that the 'universal validity' of QM only means that *the laws of QM* apply to all physical systems and processes, including microscopic and macroscopic objects, measuring interactions and human brains. But, universal validity does not imply interpretation (B). The laws of QM(A) do describe the collection of probability distributions, do describe how these probability distributions evolve in time and how they change under different interactions. *These laws are universally valid.* But this universal validity does not imply that they must provide a detailed, "complete and exhaustive" description of physical processes.

6. QM(A), which regards quantum states as being descriptive of abstract ensembles of similarly prepared systems, is completely open with respect to hidden variables (HV). If there existed a non-contradictory version of QM(B) theory, it would be a particular HV theory for QM(A), as Bacciagaluppi and Hemmo rightly pointed out (page 106) in connection with the modal interpretations. The B-quantum-states would play the role of hidden variables. The Schrödinger-equation would provide the "hidden" background mechanism. And the unknown B-quantum-states of the measuring apparatus and other components of the environment would appear as the epistemic source of randomness, making QM(B) a stochastic HV theory.

Beyond the problem of the violation of Bell-type inequalities, which is common in all HV theories, the main obstacle to QM(B) is, of course, the "measurement problem". I can see no explanation of why we should insist, of all candidates, on QM(B). Nevertheless, the challenge of finding resolution for the measurement problem in QM(B) is the main core of the book.

7. Decoherence theorists have shown that interactions with the environment and similar random effects can rapidly destroy interference between macroscopically distinct measurement outcomes. More precisely, the fluctuations can destroy the off-diagonal elements of the density matrix describing the system after the measurement. "This has given rise to hope, if not belief, that appeal to such environmentally induced decoherence is all that is required to solve the quantum measurement problem." — Healey writes on page 56. "I believe such hope is misplaced", he adds, and sketches several difficulties in the details of the decoherence program. There is, however, a more straightforward argument against it: The decoherence theory works very well in QM(A), where there is no measurement problem. Where we do encounter the measurement problem, in QM(B), the whole decoherence story is meaningless. For what kind of relief a B-theorist can feel when he knows that the pure state of the system after the measurement means *to the A-theorist* (for some particular observables) the same *probability* distributions as if the system's state was a suitable incoherent mixture?

Decoherence is a natural phenomenon from the A-theoretic point of view, explaining why it is so that some measurement interactions yield to coherent superposition (for example the spin recombination experiments with single-crystal neutron interferometer), while some others yield to an incoherent mixture (when the spectrometer is not a high-precision device).

8. The erosion of QM(B) begins with the "modal" interpretations, which is the subject of five chapters of the volume, written by such famous experts of the topic like Healey, Bacciagaluppi and Hemmo, Vermaas, Dieks and Dickson. Many of these papers contain an introduction providing a review on the various versions of modal interpretation. So, the reader, like me who is ignorant of the details of these different approaches, can find a nice guide to the field.

All versions of modal interpretation start with rejecting the eigenvalue-eigenstate link, by assuming that, at least in some particular processes, there is a privileged circle of quantities possessing ("sharp") value, even if the initial state of the object system was not an eigenstate of the operators in question.

Without here entering on the details of the different versions of modal interpretation, I would like to deal with the question in what sense we can "resolve" the measurement problem in this way. In point **6** we have already clarified the relation of QM(A) and QM(B). If we do not identify A-quantum states with B-quantum states, then the two theories can coexist in the sense that QM(B) can be a stochastic HV model of QM(A). Since QM(B) turns out to be contradictory, on the modal interpretations we explicitly give up interpretation (B), and go back to QM(A). And then a new stochastic HV model is constructed for QM(A). Consider, for instance, how this happens within Healey's modal interpretation. He writes (page 71):

> The quantum state of a system does not describe its properties; instead, it specifies the chance that future measurement-type interactions between that system and another system will produce a dynamical state in the latter that includes one dynamical property from a set of properties, each member of which may be taken to indicate a different result of the measurement.

This is nothing but interpretation (A). Beyond this statistical description of the system, provided by QM, he construes a new model that is, in my reading, actually a stochastic HV theory. Since now the quantum state does not provide a complete and exhaustive description of an individual system, *additional* state (hidden parameter) is introduced, called "dynamical state". It is a collection of "system representatives", in the sense that if the system consists of subsystems then the dynamical state consists of the system's system representative together with the subsystems' system representatives. A system representative is a vector (subspace) in the system's Hilbert space. In spite of the prima facie similarity, it is emphasized that (page 72)

> ... the dynamical state and quantum state are conceptually distinct: the former records intrinsic, categorical properties of the system, while the later specifies relational, probabilistic dispositions involving not only the system but also other systems correlated with it, or with which it is to interact.

The hidden dynamics governing what will happen in an individual measurement consists of two parts. The system representative of the composed (object + apparatus) system evolves according to the Schrödinger equation, while — if I have not misunderstood it — the subsystems' system representatives do not follow the Schrödinger equation, but they make — again, in my reading — a random jump into one of the terms in the unique biorthogonal decomposition of the composed system's system representative, with probability $|c|^2$, where c is the corresponding coefficient in the decomposition.

9. I don't want to mention here the hotly discussed problem whether the above scheme can indeed describe the real measurement processes. What concerns us here is the fact that the measurement problem disappeared as soon as we returned to the statistical interpretation of quantum state. All the other details of the modal interpretation belong to the program of elaborating a stochastic HV theory.

In other words, the real test of modal interpretation is not whether it solves the measurement problem, since there is no measurement problem in that context. The real question is whether it stands, as an HV theory, the test of No-Hidden-Variables theorems. Bacciagaluppi and Hemmo are dealing with this question in their chapter and conclude that it doesn't. It turns out that in a Bell-EPR situation the modal interpretation violates the Bell inequalities, and their discussion of Healey's proposal shows that it exhibits both outcome and parameter dependence.

This non-local feature of modal interpretation is not surprising, because explaining what could be behind QM, which is a stochastic model about the world, it provides a stochastic HV theory which is nothing else but a copy of QM. Moreover, if the above cited interpretation of system representatives is taken seriously, and they indeed correspond to "intrinsic, categorical properties of the system", then the whole measurement problem reappears in terms of the system representatives.

10. In some presentations, it seems fundamental what modal interpretation claims about the "possessed properties". Three chapters, the ones by Vermaas, Dieks and Dickson, are centered on the property ascription problem. The modal interpretation of QM assigns "definite" values to a limited set of magnitudes. Different authors develop different policies for finding the largest possible set of "definite-valued" magnitudes without contradictions. This is no place to expound the different approaches to this problem. (Vermaas' paper provides an exhaustive review on this issue.) My concern is only to clarify what is the contribution of this value/property ascription claim to the resolution of the measurement problem.

It's hard to see what such a contribution would be. According to Healey's above cited interpretation of quantum state (in point 8.), modal interpretation accepts the statistical interpretation of quantum states, by which the original measurement problem disappears. Also in the modal interpretation, the time-evolution of the quantum state settles only the probabilities of the possible measurement outcomes, but it does not determine the value of a magnitude assigned to the system in an individual measurement. In other words, the quantum state of the coupled object+apparatus system does not single out the outcome of the measurement.

In what sense, then, modal interpretation uses the term "definite-valued" magnitude? We can find a clear answer to this question in Dickson's chapter (page 161): Let W be the state operator of the system, and denote A_W a set of projectors[4] representing a corresponding collection of properties/magnitudes. The elements of A_W posses "definite value" if quantum probabilities $\{Tr(WP)\}_{P\in A_W}$ are representable in a classical, Kolmogorovian probability space. The motivation behind this definition is understandable: it follows from the Pitowsky (1989) theorem that probabilities admit Kolmogorovian representation if and only if they are weighted averages of the corresponding classical two-valued truth-functions. And this is a necessary condition of the ignorance interpretation of the probabilities in question.

We are all biased by our personal views, I know, and I am personally discontented with this approach. Because it insinuates that 1) the set of propertiesA_W thus obtained is relevant and has a privileged ontological status, in contrast with those which are not contained in A_W, 2) as if type-$Tr(WP)$ quantities would have no ignorance interpretation in general, and 3) this all would be a special phenomenon of QM, in contrast with classical physics. But this is not the case at all; the misunderstanding is rooted in the misinterpretation of $Tr(WP)$ as probability. For there is no, in general, such an event (occurrence of a property, state of affairs, actualization of a potentiality, realization of a value, etc.) which would happen with relative frequency equal to $Tr(WP)$. The QM $Tr(WP)$ is not the (absolute) probability of a real event, but it is a *conditional* probability $p(A\,|\,a)$, which means the probability of the outcome-event A, given that the measurement-preparation a has happened. Even in a *classical* probabilistic theory, no doubt admitting ignorance interpretation, a collection of conditional probabilities belonging to different conditions, as it happens, may or may not violate the rules of Kolmogorovian probability theory, if we try to use them as *absolute* probabilities. This is not a phenomenon characterizing the quantum systems only, and definitely not a reason for jumping to conclusions with respect to the ontological status of magnitudes represented by the corresponding projectors.

Probably, QM is not the whole truth about the world. But nothing prevents us to believe that there are ontologically relevant properties of a system, and there are, perhaps hidden, physical quantities describing these properties, and each of these quantities possesses a "definite" value in any moment of time. Since the outcome of an individual quantum measurement supervenes on the ontologically relevant quantities, it is "definite", too.

The only difficulty with this picture is the non-locality problem appearing in the EPR-Bell situation. However, as I mentioned in point 9, non-locality is a problem also for the modal interpretation.

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[4] Due to the spectral theorem one can extend this definition to many other self-adjoint operators which are not projectors.

11. The ten chapters cover a wide range of the various approaches to the problem of quantum measurement. There is, however, an aspect of the problem the detailed discussion of which is missing, although it is of considerable importance from philosophical point of view. Unruh formulates this problem in his chapter as follows:

> Measurements are performed by means of measuring apparatuses. As aspects of the physical world, such measuring apparatuses should themselves be describable by QM. But it is difficult to have a system in which at the same time a concept is an axiomatic feature of the theory *and* one describable by the theory.

To put it in a wider context of endophysics[5]: Is it possible to formulate a theory exhaustively describing a certain region of the universe, without any reference to something exterior to that region? Do the laws governing a system differ when you're inside the system from those you see when you look at the system from the outside? More particularly, can we formulate QM or other physical theory without any reference to directly observable (macroscopic) phenomena? These are real philosophical questions arising in a natural way with respect to the measurement problem, and lurking behind the critical scrutiny by Bub, Clifton and Monton in their paper on David Albert's Bare Theory, and by Elby in his chapter on Zurek's "existential interpretation".

References

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[5] Cf. Kampis and Weibel (1993); Breuer (1995)