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Arlington, Virginia
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Dr. Aage Petersen
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Dear Aage:

It was very good to hear from you again. Perhaps we will be able to talk together again sometime soon. There is a good chance that I will be sent to Europe in the fall on business, and I could probably take a few weeks off and come to Copenhagen. Please let me know what the best times to come are so that I can arrange things (to the extent that I am able) to be most convenient.

In the meantime, lest the discussion of my paper die completely, let me add some fuel to the fire with a number of random comments and criticisms of the "Copenhagen interpretation."

First of all, the particular difficulties with quantum mechanics that are discussed in my paper have mostly to do with the more common (at least in this country) form of quantum theory, as expressed for example by von Neumann, and not so much with the Bohr (Copenhagen) interpretation. The Bohr interpretation is to me even more unsatisfactory, and on quite different grounds. Primarily my main objections are the complete reliance on classical physics from the outset (which precludes even in principle any deduction at all of classical physics from quantum mechanics, as well as any adequate study of measuring processes), and the strange duality of adhering to a "reality" concept for macroscopic physics and denying the same for the microcosm.

Now I do not think you can dismiss my viewpoint as simply a misunderstanding of Bohr's position. I am willing to admit that Bohr's complementarity principle, which expresses limitations on the unrestricted use of classical concepts, is a valid

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principle. I even am prepared to admit that in the initial stages of formulation of quantum theory this principle was very useful in clarifying the theory and showing that it does not lead to any of the more obvious kinds of contradictions. The trouble goes much deeper than this however. I believe that the basing of quantum mechanics upon classical physics was a necessary provisional step, but that the time has come to proceed to something more fundamental.

There is a good analogy in mathematics. The complex numbers were first defined only in terms of the real numbers. However, with sufficient experience and familiarity with their properties, it became possible and indeed more natural to define them first in their own right without reference to the real numbers, and to derive from them the special case of the reals. I would suggest that the time has come to do the same for quantum mechanics -- to treat it in its own right as a fundamental theory without any dependence on classical physics, and to derive classical physics from it. While it is true that initially the classical concepts were required for its formulation, we now have sufficient familiarity to formulate it without classical physics, as in the case of the complex numbers. I am sure that you will recognize this as Bohr's own example turned against him.

The analogy goes further yet. Just as we no longer regard complex numbers as mere appendages tacked on to the reals to cover annoying inabilitys to solve certain equations, we should no longer regard quantum mechanics as a mere appendage to classical physics tacked on to cover annoying discrepancies in the behavior of microscopic systems.

Let me now mention a few more irritating features of the Copenhagen interpretation. You talk of the massiveness of macrosystems allowing one to neglect further quantum effects (in discussions of breaking the measuring chain), but never give any justification for this flatly asserted dogma. Is this an independent postulate? It most certainly does not follow

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from wave mechanics which leads to quite strange superposition states even for macrosystems when applied to any measuring processes! In fact, by the very formulation of your viewpoint you are totally incapable of any justification and must make it an independent postulate -- that macrosystems are relatively immune to quantum effects.

Another inconsistency: you vigorously state that when apparatus can be used as measuring apparatus then one cannot simultaneously give consideration to quantum effects -- but proceed blithly to apply the formula $\Delta X \Delta P \geq \frac{\hbar}{2}$ to such devices, tacitly admitting quantum effects.

You say you see no further difficulties with approximate measurements. I have yet to see any adequate account of the phenomena and would appreciate any references you can supply.

Just one final point. I am getting weary of hearing on the one hand that it is the fundamental irreversibility of the measuring process which allows the destruction of phase relations and makes possible the probability interpretation of quantum mechanics, and on the other hand that the fundamentally probabilistic processes of quantum mechanics allow truly reversible processes and for the first time make a satisfactory thermodynamics possible. As a matter of fact, there is nowhere to be found any consistent explanation of this "irreversibility" of the measuring process. It is again certainly not implied by wave mechanics, nor classical mechanics either. Another independent postulate?

I am sure that these points (by no means exhaustive) are poorly and inadequately expressed here, but hope you will think about them until we can have a full discussion. I look forward very much to renewing our always enjoyable arguments. Please give my regards to Betty.

Sincerely,

Hugh Everett, III

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HE:ng

Copy sent to Wheeler