

22 June, 1972

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Dear Belinfante,

I found your three letters of the 22nd May on my recent return from a journey abroad. This is the only reason for the delay of this reply, because with regard to the nature of my reply I have not the slightest hesitation. Let me tell you right at the start that I find it very encouraging to see that such a critically minded and sharp thinker, as I know you are, has arrived independently at essentially the same conclusions that I would have reached on these vexed matters. I shall now take the points of your letter in order, and give you whatever comments I can to each of them.

1. I agree with your mild criticism of the Italian physicists whose method is, as you say, not entirely rigorous and also rather complicated. In fact this was the motivation for me to give the simplified and I think also more strictly correct exposition of their argument, which you quote. In this respect I may call your attention to a very good paper by Jasselette and Voisin of which I enclose a reprint. I may add that in the course of recent work in collaboration with Prigogine and George on the theory of large quantal systems, we have arrived at a still simpler formal presentation of the argument. This paper is in the last stage of printing, and I hope to be able to send you a reprint in the near future.

2. What you tell me about Wigner identifying his "consciousness" with the recording process amazes me. If such a statement should be taken literally, then it is just, as you say, a misuse of the word "consciousness". However, I suspect that he really means more than that, and somehow

implies that this recording process is not entirely describable by quantum mechanics. This opinion, I maintain, is simply wrong, but I do not want to make any exegesis of Wigner's obscure thinking. With regard to Everett neither I nor even Niels Bohr could have any patience with him, when he visited us in Copenhagen more than 12 years ago in order to sell the hopelessly wrong ideas he had been encouraged, most unwisely, by Wheeler to develop. He was undescribably stupid and could not understand the simplest things in quantum mechanics. To enlarge upon the remark of Shimony which you quote, I would suggest that Occam's Razor could be most profitably used to rid us of Everett or at least of his writings.

3. Ballantine, whom I had the honour to meet at your old place, Vancouver, last April, looked to me as a rejuvenation of Everett himself, just as bumptious and probably no less stupid. I was giving a general lecture on the foundations of quantum mechanics at the request of Opechowski and at the end Ballantine came to me and said, "I am very embarrassed because I expected that I would strongly disagree with you and I find that what you said is in agreement with my views."

4. Since I have not read Ballantine's paper I do not know what his views areaabout the Einstein-Podolsky-Rosen paradox. However, I am not prepared to concede to you that Bohr's reply was not decisive. Bohr was certainly not prepared to say, as you suggest, that quantum theory did not try to be complete in Einstein's sense. He rather said that it is nobody's business to decide a priori what an element of reality should be; this is something that we have to learn from nature and quantum theory is a complete answer to that question, which we have learned by interrogating nature about the properties of atomic systems. This is, in my view, as clear and sharp a definition of completeness as one may wish.

5. I thought originally that every school boy would know that the concept of probability implied in its very

definition that it was concerned with ensembles, until I discovered to my horror that even eminent people (e.g. Monod in his rather slap-shod and superficial book "le Hasard et la Nécessité") talk about probabilities of single events. Bohr, of course, had been taught the theory of probability in a competent way and in my experience, in all discussions that I have witnessed, it was taken for granted that the statements of quantum mechanics express properties of ensembles of atomic systems characterized by a given structure. I even remember a discussion in connection with the E P R paradox in which Bohr stated with great emphasis that as soon as the measurement of the position of a particle had been performed, one had irrevocably lost the possibility of ever making any statement about the momentum of that particle at the time of the measurement. This, he pointed out, does not mean that quantum mechanics is incomplete, since a measurement of momentum can always be performed if desired upon another particle of the same kind placed in the same circumstances. Not only therefore is it futile to speak of two Copenhagen schools; but it is even wrong to speak of one Copenhagen school; there has never been any such thing and I hope there will never be. The only distinction, which is unfortunately necessary, is that between physicists who understand quantum mechanics and those who do not.

6. You than raise the point whether one should call the statements of quantum mechanics objective. Your proposal to call them "objectively justifiable" is, I think, excellent for didactic purposes, and I shall perhaps on occasion borrow it from you, but I do not think you are right to go on and say that one could do without reducing the state vector, which means physically without carrying the measurement to its completion by recording a permanent mark of its result. You should leave such a heresy to Everett.

7. Now we come to another heresy which much to my surprise such a fine thinker as d'Espagnat is propagating. I cannot imagine from where he can have got the impression that Bohr should ever have expressed such a crazy idea as that he

ascribes to him. I suspect that he must have mixed up the correct statement (which, of course, concerns not what an atomic system does in such and such circumstances, but what we can unambiguously say that it does) with the quite different and in fact incompatible mode of description of classical physics (which pretends to give statements about the system's behaviour independently of any observation).

S. Concerning the point you raise about prediction and what you call postdiction and what I would prefer to call retrodiction, I can be very brief, since I completely agree with your very elegant statement of the question. Bohr never had occasion to discuss retrodiction in his writings, but from conversations I know that he looked upon the matter precisely as you state it. Incidentally, I may tell you that in the work mentioned before together with Prigogine and George we are also able to give a very simple formal representation of the difference between prediction and retrodiction. I am glad you found a direct error in d'Espagnat's considerations, because that will help to kill this nonsense. Finally, let me tell you how much I admire your patience with those people, and how much I appreciate the service you are rendering, especially the younger generation, by putting these matters straight.

With kind wishes,

Yours,

L. Rosenfeld

/EM