

24 July, 1972

Professor Fred Belinfante
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Dear Belinfante,

Many thanks for your letter of 26 June and the accompanying manuscripts. My previous letter has already prepared you for my reactions to these papers. Concerning "Time Symmetry of Quantum Theory", of which you show me the concluding part and the appendices, I have nothing more to say except to confirm my full agreement with your position. I found your paper "Measurements in Objective Quantum Theory" very clearly written and well argued, but I would like to make a few comments enlarging on some points of my previous letter.

1) You are definitely too polite to Ballantine. I have not read his stuff, but glanced through it sufficiently to convince myself that one is justified in applying to it the famous dictum "Wäre das Wahre nur neu, wäre das Neue nur wahr". Of course, the greatest fool cannot help making true statements from time to time, but the whole tendency of his over-ambitious paper, his constantly operating with undefined pseudo-philosophical concepts, and his deficient information and lack of understanding of what he is writing about is in my view sufficient to dismiss what I regard as a misguided effort, and in any case not to recommend it, as you do, as a basic reference to the young generation. I have no objection to your referring the reader to one or two sections of the paper, which happen to be a bit better, but I would not go further than that. In particular, I strong-

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ly object to your calling Ballantine's rule a prescription known for forty years, and I object still more to your calling this rule an axiom, since it was clear from the beginning that it was only meant as an approximate or idealized procedure. I see no advantage to calling this rule F5, rather than simply reduction rule. This reminds me of the following anecdote: There once was a thesis in Copenhagen, in which the author could not mention any physical concept without ascribing a letter to it. Thus, he would speak of the velocity V, even when this letter would not occur in any formula. The old Martin Knudsen, who had to criticize the thesis, pointed this peculiarity to the candidate and remarked, "You would not normally say, "now I am going to have my lunch L"".

2) With regard to Moldauer, I had hoped for a while that he had understood the measurement problem, but unfortunately he seems to have been so eager to debunk Wigner that he has shot over the mark, and is now trying, in spite of my effort to dissuade him from doing so, to argue that there is no measurement problem, but that one gets the same result whether or not one applies the reduction. He simply forgets to take into account the evolution of the wave function in time after the interaction between the system and the instrument. Now, the crux of the problem which worries Wigner so much is that the reduction rule appears to be in contradiction with the time evolution described by Schrödinger's equation. The answer, which was of course well known to Bohr, but has been made formally quite clear by the Italians, is that the reduction rule is not an independent axiom, but essentially a thermodynamic effect, and, accordingly, only valid to the thermodynamic approximation.

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I accordingly disagree with your contention on p. 37 that the analysis of the measuring process is independent of the macroscopic character of the instrument. Your footnote 74 actually gives away the whole argument, and neither you nor anybody else can avoid committing himself to accepting the necessity of macroscopic measuring instruments. It is, of course, the registration which is of a macroscopic nature, and this registration is the absolutely essential part of the whole argument. In fact, without it the phenomenon that one is talking about is not well defined. This point was very forcefully put by Bohr in his paper in *Dialectica*.

3) Now, the reduction rule is nothing else than a formal way of expressing the idealized result of the registration. There is accordingly no choice whatsoever about the necessity of applying the reduction. In your words, the application of the reduction is mandatory. Again, your attempt to leave this point open gives away the whole argument, and, as I see it, is a serious relapse on your part into the methodological errors that you so gallantly want to kill. It seems to me that there may be a confusion in your mind between the choice to apply or not to apply the reduction rule (which in my view is just an absolutely necessary routine procedure inherent to the use of quantum mechanics), and the choice always open to us whether or not to make a definite measurement. Obviously, this last problem, the epistemological implication of which I do not want to minimize, has nothing to do with the former, which alone is the subject of your discussion.

4) From what I have just stated, it follows that your attempt to rescue something from Everett's muddle is not merely an act of compassion, but actually quite

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misguided. I hope that by calling your attention to this dangerous pitfall to convince you of the necessity to leave out the last part of your otherwise so excellent article and to be more careful about some parts of the preceding text. Just to show you how careful one ought to be on this slippery ground, let me ask you bluntly what you actually mean by such a sentence as the last one on p. 46. In my view this sentence has no well defined meaning, because it is built upon an illegitimate use of the ambiguous word "exist".

I need not assure you that I am ready to further discussion (an offer which I am certainly not prepared to make to most of the people concerned with this subject), and I am anxious to hear your reactions to my remarks.

With kind regards,

Yours sincerely,

L. Rosenfeld

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