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Professor M. Jammer
Bar-Ilan University
Ramat-Gan, Israel

Dear Professor Jammer:

Thank you very much for your kind inquiry concerning my doctoral thesis. I shall attempt to answer your questions in the order that you presented them. I did my undergraduate study at the Catholic University of America in the field of chemical engineering. My graduate studies were in the field of mathematical physics at Princeton University and my thesis was done under John Wheeler.

With respect to my thesis, the principal intellectual influences were two other residents of the Princeton Graduate College at that time, Charles Misner (currently a professor at the University of Maryland specializing in gravitation theory) and Aage Peterson who was at that time Nejls Bohr's assistant and spending a year at Princeton. The basic ideas for my interpretation arose out of discussions with these two fellow students which I subsequently presented to Professor Wheeler, who encouraged me to pursue the matter further as a thesis. During the course of this pursuit I would say that perhaps the primary influences were von Neuman's book and the later chapters of Bohm's Introduction to Quantum Mechanics.

In answer to question 2, the only formal course in philosophy or psychology that I had was an introduction to epistemology at Catholic University.

With respect to question 3 on the reactions to the 1957 paper (which was a brief condensation of the ideas contained in the full document), as far as I can recollect there was very little attention given to the paper at that time. The only strong criticism of that era that I recollect was contained in a private correspondence to Professor Wheeler, from an author who shall remain unnamed, to the effect that the implied splitting of the state of an observer by my theory could not possibly be true because I (the critic) am unaware of any such splitting effect. This criticism led to the insertion of a footnote in my published paper drawing the parallel with the manifest absurdity of the Copernican system of planetary motions because I, as an observer, do not feel any such motion. The point being, of course, that if the theory itself predicts ones sensory preceptions to be what they are in fact, the weight of this criticism is somewhat diminished. The unwillingness of most physicists to accept this theory, I believe, is therefore due to the psychological distaste which the theory engenders overwhelming the inherent simplicity of the theory as a way of resolving the apparent paradoxes of quantum mechanics as conventionally conceived. Thus, the theory was not so much criticized, as far as I am aware, but simply dismissed.

Subsequent to the publication of the paper, I had informal discussions with a number of physicists concerning the subject (including Bohr and Rosenfeld in Copenhagen, in 1959, Podolski and Wigner and a number of others active in the field at a conference at Xavier University several years later). I was somewhat surprised, and a little amused, that none of these physicists had grasped one of what I considered to be the major accomplishment of the theory -- the "rigorous" deduction of the probability interpretation of quantum mechanics from wave mechanics alone. This deduction is just as "rigorous" as any of the deductions of classical statistical mechanics, since in both areas the deductions can be shown to depend upon an 'a priori' choice of a measure on the space. In classical statistical mechanics this measure is standard lebesgue measure on the phase space whereas in quantum mechanics this measure is the square of the amplitude of the coefficients of an orthonormal expansion of a wave function.

What is unique about the choice of measure and why it is forced upon one is that in both cases it is the only measure that satisfies a law of conservation of probability through the equations of motion. Thus, logically in both classical statistical mechanics and in quantum mechanics, the only possible statistical statements depend upon the existence of a unique measure which obeys this conservation principle.

One of my major points was, therefore, that the probability interpretation of quantum mechanics -- that somehow the measuring process was "magic" and subject to a separate axiom governing the collapse of the wave function -- did not have the status of an independent postulate at all, but was in fact a deduction from pure wave mechanics alone. That this point was essentially completely overlooked at that time I can now only ascribe to my failure in writing the paper. This point occurred rather far into the paper at a point where I now realize most readers would have stopped reading.

To question 4, were there any specific motives or reasons that induced me to propose my interpretation and measurement theory -- I must answer in all candor the primary motive was, of course, to obtain a thesis. However, I must also admit to a strong secondary motive to resolve what appeared to me to be inherent inconsistencies in the conventional interpretation. I was of course struck, as many before and also many since, by the apparent paradoxes raised by the unique role assumed by the measurement process in quantum mechanics as it was conventionally espoused. It seemed to me unnatural that there should be a "magic" process in which something quite drastic occurred (collapse of the wave function), while in all other times systems were assumed to obey perfectly natural continuous laws.

I thought at that time that perhaps the pursuit of this apparent difficulty would lead to a new and different theory which, while resolving the apparent paradoxes, would also lead to entirely new predictions. Unfortunately, as it turned out, the theory which I constructed resolved all the paradoxes and at the same time showed the complete equivalence with respect to any possible experimental test of my theory and that of the conventional quantum mechanics. The net result of my theory therefore is simply to give a complete and self-consistent picture (without any particular "magic" associated with measurement) that in all practical predictions will of course be identical to the predictions of the conventional formulation.

To me, therefore, the real usefulness of this picture or theory of quantum mechanics is simply as an alternative which could be acceptable to those who sense the paradoxes in the conventional formulation, and therefore save much time and effort by those who are also disturbed by the apparent inconsistencies of the conventional model. As you know, there have been a large number of attempts to construct different forms of quantum mechanics to overcome these same apparent paradoxes. To me, these other attempts appear highly tortured and unnatural. I believe that my theory is by far the simplest way out of the dilemma, since it results from what is inherently a simplification of the conventional picture, which arises by dropping one of the basic postulates -- the postulate of the discontinuous probabilistic jump in state during the process of measurement -- from the remaining very simple theory, only to recover again this very same picture as a deduction of what will <u>appear</u> to be the case to observers. I therefore believe that my formulation is by far the simplest from an axiomatic point of view. The acceptability, however, clearly is a matter of personal taste.

With respect to question 5, I cannot think of any other historical data of relevance at this time. My own history subsequent to leaving Princeton University in the summer of 1956, for what it may be worth, is as follows:

I entered the field of operations research and was employed by the Weapons Systems Evaluation Group in the Pentagon studying problems of strategic nuclear warfare until the summer of 1964. At that time, I and several of my colleagues formed a company -- Lambda Corporation -- to apply operations research and computer-modeling techniques to problems facing the United States government and also commercial enterprises. Most recently, in July of this year, I and another colleague left Lambda Corporation to form yet another enterprise, DBS Corporation -- devoted to supplying data processing services to managers and policy makers in the federal government and industry. My colleague in this enterprise, Donald Reisler, is also an ex-physicist, and indeed one who worked in the foundations of quantum mechanics. Only after working together at Lambda Corporation for some time did we discover that we had both done our theses in the field of foundations of quantum mechanics. His thesis is entitled "Einstein, Podolsky, Rosen Paradox: 'Can the Quantum Mechanical Description of Physical Reality be Considered Complete?'" Since our mutual discovery, however, realizing the pitfalls that might divert our energies, we have entered into a mutual pact not to read one another's thesis for ten years. Accordingly, we have placed a copy of each of our theses in the DBS Corporate files, to be exhumed ten years hence and read, when presumably we could afford the luxury of such a diversion.

With respect to your postscript on the possibility of getting a preprint of my paper (presumably the full document rather than the published paper) I am sorry to answer that the only copy in my possession is the aforementioned, on file with DBS Corporation. The only other copy that I am aware of is the one I sent to Professor Bryce DeWitt. I would suggest, therefore, that you inquire whether he has perhaps copied that document.

Sincerely,

Hugh Everett, III

Hugh Everett III
Chairman
DBS Corporation