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Are there quantum jumps?

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*when was Zeh?
1993
No quantum jumps
No particles*

If we have to go on with these damned quantum jumps, then I'm sorry that I ever got involved. E. Schrödinger.

1. Introduction

I have borrowed the title of a characteristic paper by Schrödinger (Schrödinger, 1952). In it he contrasts the smooth evolution of the Schrödinger wavefunction with the erratic behaviour of the picture by which the wavefunction is usually supplemented, or 'interpreted', in the minds of most physicists. He objects in particular to the notion of 'stationary states', and above all to 'quantum jumping' between those states. He regards these concepts as hangovers from the old Bohr quantum theory, of 1913, and entirely unmotivated by anything in the mathematics of the new theory of 1926. He would like to regard the wavefunction itself as the complete picture, and completely determined by the Schrödinger equation, and so evolving smoothly with-

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out 'quantum jumps'. Nor would he have 'particles' in the picture. At an early stage, he had tried to replace 'particles' by wavepackets (Schrödinger, 1926). But wavepackets diffuse. And the paper of 1952 ends, rather lamely, with the admission that Schrödinger does not see how, for the present, to account for particle tracks in track chambers.... nor, more generally, for the definiteness, the particularity, of the world of experience, as compared with the indefiniteness, the waviness, of the wavefunction. It is the problem that he had had (Schrödinger, 1935a) with his cat. He thought that she could not be both dead and alive. But the wavefunction showed no such commitment, superposing the possibilities. Either the wavefunction, as given by the Schrodinger equation, is not everything, or is not right.

Of these two possibilities, that the wavefunction is not everything, or not right, the first is developed especially in the de Broglie Bohm 'pilot wave' picture. Absurdly, such theories are known as 'hidden variable' theories. Absurdly, for there it is not in the wavefunction that one finds an image of the visible world, and the results of experiments, but in the complementary 'hidden' (!) variables. Of course the extra variables are not confined to the visible 'macroscopic' scale. For no sharp definition of such a scale could be made. The 'microscopic' aspect of the complementary variables is indeed hidden from us. But to admit things not visible to the gross creatures that we are is, in my opinion, to show a decent humility, and not just a lamentable addiction to metaphysics. In any case, the most hidden of all variables, in the pilot wave picture, is the wavefunction, which manifests itself to us only by its influence on the complementary variables.

If, with Schrödinger, we reject extra variables, then we must allow that his equation is not always right. I do not know that he contemplated this conclusion, but it seems to me inescapable. Anyway it is the line that I will follow here. The idea of a small change in the mathematics of the wavefunction, one that would little affect small systems, but would become important in large systems, like cats and other scientific instruments, has often been entertained. It seems to me that a recent idea (Ghirardi, Rimini, and Weber, 1985), a specific form of spontaneous wavefunction collapse, is particularly simple and effective. I will present it below. Then I will consider what light it throws on another of Schrödinger's preoccupations. He was one of those who reacted most vigorously (Schrödinger, 1935a, 1935b, 1936) to the famous paper of Einstein, Podolsky, and Rosen. As regards what he called 'quantum entanglement', and the resulting EPR correlations, he 'would not call that *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought'.

2. Ghirardi, Rimini, and Weber

The proposal of Ghirardi, Rimini, and Weber, is formulated for nonrelativistic Schrödinger quantum mechanics. The idea is that while a wavefunction

$$(1) \quad \psi(t, \vec{r}_1, \vec{r}_2, \dots, \vec{r}_N)$$

spectively. It can be interpreted as supplementing (21,22) by giving the probabilities for jumps in the two systems as t' and t'' are advanced independently from independent starting points. It does not depend on t' or t'' except through the 2-time wavefunction ψ (and any time dependent external fields in Hamiltonians A and B). The relative time translation invariance of the theory is then manifest.

The reformulation (A22,A23) of the theory can also be used to calculate the statistics of jumps in one system separately, disregarding what happens in the other. The result, (A24,A25), makes no reference to the second system. Events in one system, considered separately, allow no inference about events in the other, nor about external fields at work in the other,... nor even about the very existence of the other system. There are no 'messages' in one system from the other. The inexplicable correlations of quantum mechanics do not give rise to signalling between noninteracting systems. Of course however there may be correlations (e.g. those of EPRB) and if something about the second system is given (e.g. that it is the other side of an EPRB setup...) and something about the overall state (e.g. that it is the EPRB singlet state...) then inferences from events in one system (e.g. 'yes' from the 'up' counter) to events in the other (e.g. 'yes' from the 'down' counter) are possible.

5. Conclusion

I think that Schrödinger could hardly have found very compelling the GRW theory as expounded here...with the arbitrariness of the jump function, and the elusiveness

of the new physical constants. But he might have seen in it a hint of something good to come. He would have liked, I think, that the theory is completely determined by the equations, which do not have to be talked away from time to time. He would have liked the complete absence of particles from the theory, and yet the emergence of 'particle tracks', and more generally of the 'particularity' of the world, on the macroscopic level. He might not have liked the GRW jumps, but he would have disliked them less than the old quantum jumps of his time. And he would not have been at all disturbed by their indeterminism. For as early as 1922, following his teacher Exner, he was expecting the fundamental laws to be statistical in character: '...once we have discarded our rooted predilection for absolute Causality, we shall succeed in overcoming the difficulties...' (Schrödinger, 1957).

For myself, I see the GRW model as a very nice illustration of how quantum mechanics, to become rational, requires only a change which is very small (on some measures!). And I am particularly struck by the fact that the model is as Lorentz invariant as it could be in the nonrelativistic version. It takes away the ground of my fear that any exact formulation of quantum mechanics must conflict with fundamental Lorentz invariance.