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Author
Stapp, Henry P.

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Henry P. Stapp
Physics Division

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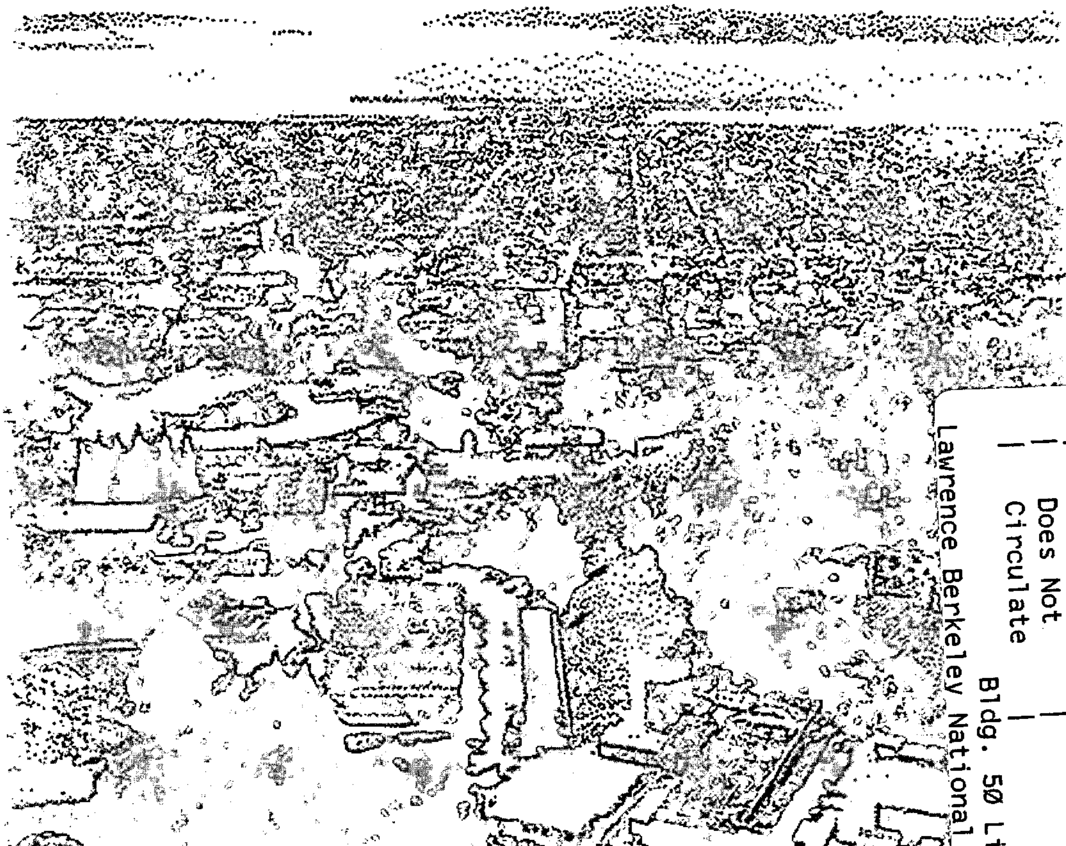

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On Quantum Theories of the Mind *

Henry P. Stapp
Lawrence Berkeley National Laboratory
University of California
Berkeley, California 94720

Abstract

Replies are given to arguments advanced in this journal that claim to show that it is to nonlinear classical mechanics rather than quantum mechanics that one must look for the physical underpinnings of consciousness.

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In a paper with the same title as this one Alwyn Scott (1996) has given reasons for rejecting the idea that quantum theory will play an important role in understanding the connection between brains and consciousness. He suggests that it is to nonlinear classical mechanics, not quantum mechanics, that we should look for the physical underpinnings of consciousness. I shall examine here all of his arguments, and show why each one fails.

He first contrasts the linearity of quantum theory with the nonlinearity of certain classical theories, and notes the complexities induced by the latter. Thus he asks: "Is not liquid water essentially different from gaseous hydrogen and oxygen?" Of course it is! And this difference is generated, according to quantum field theory, by certain nonlinearities in that theory, namely the nonlinearities in the coupled *field equations.* These field equations (or, more generally, Heisenberg equations) are the direct analogs of the coupled nonlinear equations of the corresponding classical theory, and they bring into quantum theory the analogs of the classical nonlinearities: these nonlinearities are in no way obstructed by the linearity of the *wave equation.*

To understand this point it is helpful to think of the equation of motion for a classical statistical ensemble. It is linear: the sum of two classical statistical statistical ensembles evolves into the sum of the two evolved ensembles. This linearity property is a trivial consequence of the fact that the elements of the ensembles are imaginary copies of one single physical system, in different contemplated states, and hence they do not interact with one another. Thus in classical statistical mechanics we have both the (generally) nonlinear equations for coupled *fields*, and also the (always) linear equation for a certain statistical quantity.

Similarly, in quantum field theory we have both the (generally) nonlinear field equations for the coupled *fields*, and also the (always) linear wave equation for a certain statistical quantity, the *wave function.* The fact that a group of several atoms can behave very differently from how they would behave if each one were alone is a consequence of the nonlinearity of the field equations: this nonlinearity is not blocked by the linearity of the wave equation.

This blurring of the important distinction between the completely compatible linear and nonlinear aspects of quantum theory is carried over into Scott's discussion of solitons. The nonlinear field equations make the parts of this configuration of fields hang together indefinitely, and never spread out like a wave, as could be verified by doing experiments that probe its 'togetherness' by making several measurements simultaneously at slightly separated points: the various simultaneously existing parts of the soliton never move far apart. There is no conflict between this stability of the soliton and the linearity of the quantum mechanical wave equation. The wave function for the *center-of-mass of the soliton* does eventually spread out in exactly the way that a *a statistical ensemble* consisting of the *centers of the solitons* in an ensemble of freely moving solitons (of fixed finite extension) would do: the spreading out of the *wave function* of the center-of-mass of a soliton just gives the diffusion analogous to the spreading out of a statistical ensemble of superposed *centers of mass*, due to the distribution in this ensemble of velocities of these centers of mass: the extended object itself, the soliton, does not spread out; its parts are held together by a nonlinear effect that can be attributed to the nonlinearity of the field equations.

This blurring by Scott of the important conceptual distinctions between the two very different aspects of the soliton associated with the linear and nonlinear aspects of quantum theory creates, I think, a very false impression some significant deficiency of quantum theory with regard to the manifestation of the analogs in quantum theory of nonlinear classical effects. No such deficiency exists: the atoms of hydrogen and oxygen do combine, according to quantum theory, to form water.

Failure carefully to follow through this conceptual distinction is the root of the failures of all of Scott's arguments.

Scott emphasizes the smallness of the spreading of the wave function of the center-of-mass of Steffi Graf's tennis ball. That situation involves the motion of a large massive object, the tennis ball, relative to, say, a baseline on a large tennis court.

The pertinent analogous situation in the brain involves the motion of a calcium ion from the exit of a microchannel of diameter 1 nanometer to a target trigger site for the release of a vesicle of neuro-transmitter into the synaptic cleft. The irreducible Heisenberg uncertainty in the velocity of the ion as it exits the microchannel is about 1.5 cm/sec, which is smaller than its thermal velocity by a factor of about 4×10^{-3}. The distance to the target trigger site is about 50 nanometers. So the spreading of the wave packet is of the order of 0.2 nanometers, which is of the order of the size of the ion itself, and of the target trigger site. Thus the decision as to whether the vesicle is released or not, in an individual instance, will have a large uncertainty due to the Heisenberg quantum uncertainty in the position of the calcium ion relative to the trigger site: the ion may hit the trigger site and release the vesicle, or it may miss it the trigger site and fail to release the vesicle. These two possibilities, yes or no, for the release of this vesicle by this ion continue to exist, in a superposed state, until a "reduction of the wave packet occurs". Thus, if there is a situation in which a certain particular *set of vesicles* are released, due to the relevant calcium ions having been captured at the appropriate sites, then there will be other nearby parts of the wave function of the brain in which some or all of the relevant captures do not take place—because, for this part of the wave function, the calcium ion misses the target—and hence the corresponding vesicles are not released.

This means, more generally, in a situation that corresponds to a very large number N of synaptc firings, that until a reduction occurs, all of the 2^N possible combinations of firings and no firings will be represented with comparable statistical weight in the wave function of the brain/body and its environment. Different combinations of these firings and no firings can lead to very different macroscopic behaviours of the body that is being controlled by the this brain, via the *highly nonlinear* neurodynamics of the brain. This nonlinear dynamics, which can probably be approximated reasonably well by a classical model, is, roughly speaking, what is controlling the evolution of the various individual classically describable possibilities. These various individual classically described evolutions will lead to various different classically described attractors.

But the important thing, here, is that there is, *on top of this nonlinear and extremely important classically described neuro-dynamics*, a quantum mechanical statistical effect analogous to the spreading out of the wave function of the center of Steffi Graf's tennis ball, relative to the target base line, but arising in the case of the brain, from the spreading out of the wave functions of the centers of the various presynaptic calcium ions relative to their target trigger sites. In this latter case the spreading is very significant, and it will surely cause the wave function of the whole brain to disperse into a shower of superposed possibilities. Each possibility can be expected to evolve into the neighborhood of some one of the many different attractors. These different attactors will be brain states that will evolve, in turn, into to different possible macroscopic behaviors, unless a reduction occurs.

So, although the magnitude of the effect of the spreading of the wave function of the center of Steffi Graf's tennis ball is virtually nil, the magnitude of the effect of the spreadings of the wave functions of the centers of the presynaptic calcium ions is enormous: it will cause the wave function of the person's body in its environment to disperse, if no reduction of the wave packet occurs, into a profusion of branches that represent all of the possible actions that the person is at all likely to take in the circumstance at hand. The reduction of the wave packet is, in this latter case, in contrast to the case of Steffi Graf's tennis ball, a decisive controlling factor: it selects from among all of the very different possible large-scale bodily actions generated by the nonlinear (and, let us even suppose, classically describable) neurodynamics.

In this circumstance, the question of how, and in what manner, the reduction occurs becomes crucial: this reduction plays an absolutely decisive role in the determination of what bodily behaviour we will observe to be occurring at the macroscopic scale!

The situation is just opposite, in this respect, to the one involving the position of the center of Steffi Graf's tennis ball relative to the target baseline.

In this discussion I have generated the superposed macroscopically differet possibilities by considering only the spreading out of the wave packets of the centers-of-mass of the pertinent presynaptic calcium ions relative to the target trigger sites, imagining the rest of the brain neurodynamics to be adequately approximated by the nonlinear classically describable neurodynamics of the brain. Improving upon this approximation would tend to increase the quantum uncertainty, not diminish it.

The situation, therefore, is this: elementary considerations show that the Heisenberg uncertainty principle *forces* the wave packet that represents body/brain to evolve, in the absence of any reduction, into a shower of possibilities that includes *all* of the different macroscopic behaviours that are at all likely to occur in the physical situation at hand. The dispersing of the wave packet in this way is in no way disrupted by thermal noise: it occurs quite independently of the thermal noise and of all other classical uncertainties. Nor is it dependent upon any long-range quantum coherence in the brain. In any individual situation it is therefore the reduction of the wave packet that controls which of the macroscopic behaviours will manifest. But in the pragmatic Copenhagen interpretation of quantum theory, which is the mainstream interprtation, and also in the von Neumann-Wigner interpretation, described by Wigner (1967) as the "orthodox" interpretation, the reduction of the wave packet represents a projection of the conscious knowings of the human observer onto the physicists' objective mathematical representation of reality. The consciousness of the observer is thus forced into the dynamical picture by the Heisenberg uncertainty principle, both at the pragmatic level that is the focus of the Copenhagen interpretation and at the ontological level that is described by the von Neumann-Wigner interpretation. This intrusion of consciousness into the dynamics is a macroscopic consequence of quantum theory that has nothing at all to do with the fact that quantum theory is needed also if one wants to get a precise description of the chemical reactions that are important to the small-scale dynamics: accuracy in the theoretical treatment of chemical reactions is a matter completely different from what is involved here!

Scott now lists a number of reasons for believing that quantum theory is not important in brain dynamics in a way that would relate to consciousness. However, as I shall now explain, none of these arguments has any relevance to the issue, which is this: the Heisenberg uncertainty principle forces the reduction of the wave packet that represents the body/brain to be, in any given situation, the controlling factor that specifies which macroscopic behaviour will become manifest, and this collapse is closely tied in quantum theory to our conscious experience.

Scott's first set of reasons have to do with the Born-Oppenheimer approximation in the study of the properties of molecules. The brain is, of course, far more complex than a molecule. But my argument does not depend on any approximation: I am considering, basically, general exact features of the evolution of the wave functions of the entire body/brain and its environment.

He then considers a subject he has worked on: polarons. He says the the effect of the quantum corrections is to degrade the global coherence of the classical polaron. But this "degrading" is not just some fuzzying-up of the situation: it is the very thing that is of interest and importance here. In our more complicated case of a body/brain this "degrading" becomes the separation of the wave function into branches representing various classical describable possibilities, only one of which, however, is experienced in the mind associated with this body/brain. And quantum theory itself in its present form is mute on the question of which of these possibilities is experienced. But then what is it that *undoes* this huge (in our case) degrading that the linear wave equation forces on us. It is not the classical nonlinearities, for the quantum analogs of these are already built into the quantum description, and they play there the very crucial role of creating the various classically describable possibilities that we are talking about.

His next two points concern the difficulty of maintaining "quantum coherence" in a warm wet brain. But I consider, in principle, the wave function of the entire body/brain and its relevant environment (really the whole universe) and never appeal to, require, or in any way tacitly assume or depend upon, any large-scale coherence properties.

Scott's next item is the theory for the propagation of an action potential along a nerve fiber. He points out that this propagation is well described by the classical Hodgkin-Huxley equation. But, as I have emphasized above, I assume in my model (Stapp, 1993) the adequacy of a classical description of this propagation: I consider, explicitly, only those quantum effects arising from the motions of the calcium ions in the presynaptic nerve terminals. (Fogelson and Zucker, 1985; Zucker and Fogelson, 1986). Other quantum effects can increase the effect I am considering, but are not required. A more detailed account is given in Stapp (1997).

Scott's final point is about Schroedinger's cat. He says the Schroedinger equation cannot be constructed because the cat does not conserve energy. But I am considering, in principle, the wave function of all the particle in the universe, in the manner pursued by von Neumann (1950). Within that theoretical framework there is no problem with the energy of a cat, or of a human brain.

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ERNEST ORLANDO LAWRENCE BERKELEY NATIONAL LABORATORY
ONE CYCLOTRON ROAD | BERKELEY, CALIFORNIA 94720

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