Review of: "Neural Quantum Superposition and the Change of Mind"

Lasse Bjørn Kristensen[1]

1 University of Toronto

Potential competing interests: No potential competing interests to declare.

I found the paper to contain an interesting perspective on the applications of quantum-like ideas to complicated mental processes like decision making, and definitely see the value in these kinds of explorations, regardless of the difficult question of whether the underlying physical process is itself quantum. That said, I had some difficulties recognizing the quantum physics I know in the math and concepts of the paper, and have some recommendations for things to look at if the author want to directly invoke quantum mechanics (and not just a quantum-like model). In order of the appearances in the text:

Conceptual comments:

1. In the introduction, it is implied that the total probability is not a conserved 100% in quantum mechanics (described as the “sure thing” principle). I don't know of any interpretations of quantum mechanics where this is not the case. Indeed, this is the reason for the normalization requirement and that the author makes use of later in the manuscript.
2. In the section on the dual-slit experiment, it is implied that destructive interference is equivalent to “independent slits”. It seems to me that this is exactly the most importantly correlated regions - outputs from independent slits can easily add, but subtraction like in destructive interference is only possible for waves, so it is exactly in the destructive interference that the wave-nature of the electron is most clearly revealed.
3. I am not sure if “entanglement” is the right word to use for the overlap term. It is usually used to talk about correlations between degrees of freedom (like the number of electrons in each slit). Since the number of terms in a superposition is to some extent a choice made by the person writing down the equation (I could split \Psi_A in two if I wanted), this “entanglement between terms” necessarily has a weaker physical interpretation I think.
4. I like the way that decision is related to time evolution, partly by an inherent time evolution driven by the (diagonal) \omega operator and partially by an of-diagonal operator that mixes \Psi_A and \Psi_B (i.e., the perturbation part). The fact that measurement collapse is not accounted for is a little concerning, but I could see it as fruit for further thought.

Mathematical comments:

1. In Eq. 6a, it seems like the overlap between \Psi_A and \Psi_B is assumed to be one. For normalized elements in a Hilbert space, this actually implies that they are co-linear. In other words, \Psi_A must be equal to \Psi_B up to a phase. Since it seems like they are both real, the conclusion must be that \Psi_A = \Psi_B. This does not seem to be

the intent of the paper, and seems to cause inconsistencies in what follows.

2. The resulting normalization-condition in Eq. 6b indirectly inherits this interpretation. Not adding a separate symbol for the cross term gives 1 = alpha^2 + beta^2 + 2 alpha beta → alpha + beta = 1 → beta = 1 - alpha. You only have one independent degree of freedom, alpha, and If indeed the overlap is perfect and \Psi_A = \Psi_B I would argue that it is to some extent fictitious.
3. Based on your statement that alpha, beta and gamma are independent, you start from an initial state where gamma = 1 and alpha=beta=0. While this is a very reasonable starting point, I would argue that it is incompatible with the grounding in quantum theory of Eq. 6. After all, the expression for gamma then gives "2 alpha beta = 1 and alpha=beta=0", which doesn't seem consistent.
4. From at least a basic QM point of view, the full overlap assumption from Eq. 6 actually conflicts with your time evolution. If the states \Psi_A and \Psi_B are both eigenstates of the operator omega driving time evolution with different eigenvalues/energies \omega_A, \omega_B, then they are necessarily orthogonal (overlap=0) by linear algebra considerations. If Eq. 7 is desired, it therefore seems to have to arise from a theory that is different from basic quantum mechanics.
5. The choice of \delta_A and \delta_B seemed a bit unmotivated by me, and I found the math that follows hard to keep track of. Sometimes alpha^2 + beta^2 is zero (e.g., Eq. 8) and sometimes its not (e.g., Eq. 8a).
6. I cannot follow how the off-diagonals in Eq. 11 just completely disappear. Maybe the intended symbol is not an equality but arrows indicating multiple transformations?

I think there are probably ways to bring the formal math to more closely align with (my understanding of) quantum theory. If the author is interested in discussing this more, feel free to reach out - the comments above are in no way meant with any animosity, just a desire to assign the right label (quantum or not) to the proposed theory.