Review of: "Quantum distinguishability and symplectic topology"

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Potential competing interests: No potential competing interests to declare.

Let us summarise the main findings in this manuscript. The author investigates the issue of distinguishability of quantum blobs in terms of quantum fidelity. It is based on the notion of symplectic capacity, the fidelity being given by the absolute square of the complex-valued overlap between the symplectic capacities of a pair of states, the symplectic capacity being bounded from below by the Gromov width of h/2 which generalises the Liouville thorem for volume preserving evolution in classical mechanics, by constraining the shape of the flow. The Schrodinger equation is shown to follow for closed Hamiltonian systems from the requirement of conservation of fidelity.

The manuscript is overall clearly written. However, I would note that similar ideas have also been mentioned earlier. I would like the author to state clearly in what way the manuscript goes beyond existing literature. Most of the observations stated in the abstract of the manuscript have already been made in Maurice de Gosson's papers (See for instance https://arxiv.org/pdf/0808.2774.pdf and more recent ones, by Maurice de Gosson).

Below I list a few points:

1. The author complexifies the momenta p but not the coordinates q . This seems a strange thing to do in the context of symplectic geometry, breaking the symmetry which appears natural in the problem. Even in quantum physics, the momentum representation is as good as the coordinate representation. This asymmetry in the treatment seems to go against the spirit of symplectic geometry.

2. Let us consider the following statements made in the manuscript.

'The fuzziness for non-commuting observables, such as e.g. qi and pi, will be interpreted to correspond to the impossibility of squeezing the projected area of the quantum blob onto the (qi, pi) plane to a value smaller than the Gromov width h/2. For commuting pairs, e.g. qi and qj, there is no fuzziness since there is no restriction on the smallness of the projected area. The area can be made arbitrarily small.'

'The key character of the quantum Hamiltonian flow, contrasting its classical approximation, is thus the constraint on the shape of the flow as encoded in the indeterminacy relation. This is in direct contradiction with the Liouville theorem. The Liouville theorem state that any initial region on the phase space can deform continuously in any conceivable way as long as its volume does not change [40]. Thus, according to the Liouville theorem, it is possible to deform the arbitrary state ξ in such a way that the symplectic capacity onto some given subset of conjugate pairs is smaller than the Gromov width h/2, as long as it is balanced by an increase in the symplectic capacity of another subset of conjugate pairs, keeping the volume invariant. Thus, classical mechanics, whose dynamics on the phase space are governed by the Liouville theorem, violate the indeterminacy relation.'

It is clearly stated (even in elementary classical texts like Goldstein) that there are Poincare Cartan Integral Invariants, which pose stronger constraints than the Liouville theorem, which only states the preservation of phase space volume. Equivalently, in. modern language, the preservation of ω is a stronger constraint than the Liouville theorem, which only requires that the symplectic volume $\omega \wedge \omega \wedge \omega .... \wedge \omega$ (n times) is preserved. Even in classical statistical mechanics one can envisage states which have a spread in phase space. Constraints coming from symplectic capacity still apply in that context. The suggestion that we are dealing with a purely quantum phenomenon, does not appear to be well supported.

3. I am somewhat puzzled by the author's use of the word symplectic topology. The notion of symplectic capacity should be considered as global symplectic geometry (local symplectic geometry is trivial by Darboux theorem). In contrast, notions like fixed points of a symplectomorphism and Arnold's conjecture may be rightly termed as symplectic topology.

I would like the author to consider the above mentioned points in the revised version of the manuscript.

Regards,

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