Review of: "Fidelity of quantum blobs"

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Potential competing interests: No potential competing interests to declare.

This is a review of the preprint ‘Fidelity of quantum blobs’ (formerly titled ‘Quantum distinguishability and symplectic topology’) by A. Henriksson. This paper overlaps quite a bit with another preprint of the author on Qeios titled ‘Quantum mechanics and symplectic topology’. (The present review thus applies to this other preprint too.)

The primary aim of this paper is to define an appropriate notion of ‘fidelity’ (analogous to that found in quantum mechanics) within the ‘quantum blob’ picture advocated by e.g. M. de Gosson as an approximation to quantum mechanics (in its phase space formulation). To get such a definition, it is proposed to rely on results coming from ‘symplectic topology’, a modern and active field of mathematics which emerged in the 1980s from investigations of the dynamical properties of classical Hamiltonian systems and from problems in harmonic analysis.

The motivation behind this ‘symplectic topology perspective’ comes from a well-known similarity between the quantum uncertainty inequalities and the foundational theorem of symplectic topology – namely Gromov’s nonsqueezing theorem, which refines Liouville theorem. In fact, Gromov’s theorem is an answer to questions raised in relation to Fefferman—Phong’s SAK principle in harmonic analysis, the SAK principle being in turn a formalization of the old heuristic idea (used e.g. by Boltzmann and by Rayleigh—Jeans) that orthogonal/distinguishable states of a system should correspond to disjoint indivisible cells in phase space. Roughly speaking, Gromov theorem implies not only that Hamilton equations preserve phase-space volumes (that is Liouville theorem), but that they also 'tend to preserve’ (rather, to not decrease too much) the areas of shadows of phase-space regions onto planes of conjugate coordinates. This implies that whatever the origin behind the quantum fuzziness between conjugate observables may be, classical dynamics suffices to explain why any initial fuzziness of a system is maintained in time. In recent years, many mathematicians (notably M. de Gosson, L. Charles and L. Polterovich, to mention a few) have explored ways how symplectic geometry/topology relate to quantum mechanics. Relations to quantum fidelity have been investigated in papers by Charles—Polterovich.

To summarize my opinion of the preprint:

(1) I think the whole project of the paper, i.e. defining ‘fidelity of quantum blobs’, is very interesting, and that it could be of great theoretical value (e.g. in harmonic analysis in relation to Fefferman—Phong’s SAK principle). Moreover, the idea that ‘fidelity between two quantum blobs’ should be related to (some measure of) their intersection certainly sounds plausible and should definitely be investigated further.

(2) Stylistically, the paper is well written and is a smooth read. Considering that the possible readership of the paper goes across many different disciplines, one appreciates the effort made by the author to explain a variety of concepts.

(3) Unfortunately, there are many inaccuracies, argumentative gaps and implicit contentions throughout the technical argument. In the end, I am not convinced the paper succeeds in defining an appropriate notion of 'fidelity' for blobs. I expect it will prove quite difficult to fix any of these issues. Therefore, much work is still needed before we can begin to assess the true potential of the ideas and intuitions put forward in the paper.

To explain my main objections, I note that the paper can be split into three main parts:

I. An introductory part, where the 'quantum blob' picture, Gromov theorem and the closely related notion of 'symplectic capacities' are discussed.
II. A central part, where (a) a precise notion of 'fidelity' between two blobs is proposed, based on an intermediary notion of 'symplectic overlapping of blobs', and (b) it is argued that this 'fidelity' can be interpreted as the probability that two blobs be mistaken for one another.
III. A final part, where a 'principle of conservation of fidelity' is posited, which implies (so the argument goes) that evolution of symplectic overlappings are governed by Schrödinger's equation.

Part (I) contains many issues in regard to the statements and the applicability of the quoted symplectic-topology results; moreover, none of these results appears to be genuinely used afterwards in the paper. For these reasons, it is unclear whether the proposed notion of 'fidelity' is well-defined or appropriate. It turns out that Part (III) is essentially independent from the first two Parts, notably from the exact definition of 'fidelity'. In fact, the rationale in Part (III) is somewhat incompatible with the 'reliance on symplectic topology' advocated in the previous Parts. Indeed, adopting this 'symplectic-topology perspective', one would expect the 'principle of conservation of fidelity' to be a theorem rather than an axiom. However, it is doubtful that the proposed definition of fidelity satisfies this principle (at least, the proof would be quite nontrivial). Moreover, some conclusions drawn in Part (III) are dubious. In the end, the coherence and tangibility of the paper are challenged.

I now detail my main objections.

(1) There are numerous issues with the interpretation and with the use made of symplectic-topology results in Part (I).

a. Contrary to what the paper claims, Liouville theorem does not imply Hamilton equations; only the converse is true. Similarly, Liouville theorem does not imply that any volume-preserving flow is a solution to Hamilton equations, since such solutions are further constrained by Gromov theorem. In other words, it is better to see Gromov theorem as a refinement of Liouville theorem than as a generalization of it. (These misconceptions only impact discussions about the physical significance of the ideas put forward in the paper.)
b. In the setting put forward in part (I), complex-valued momenta p are allowed, while the positions q are kept real-valued. In other words, letting Q denote the configuration space of the system, we are considering the complexified phase space $M := T^* Q \otimes \mathbb{C}$. However, most of the symplectic results quoted in the paper are proved in the literature for the real phase space $T^* Q$. It is far from clear that the known symplectic results extend (verbatim) to the complexified context. For instance, as written, Eq (7) does not define (the boundary of) a symplectic ball: fixing the q's,

the resulting locus of the p's is a complex hypersurface, which is therefore (generically) noncompact and unbounded. (As a result, it is unclear whether 'fidelity' is well-defined i.e. a finite number.) The most evident solution to this, i.e. replacing in (7) the terms $(p_k - b_k)^2$ by $|p_k - b_k|^2$, also fails in general to produce a symplectic sphere. Hence, allowing complex p's does not lead to the quantum blobs being symplectic balls, and so Gromov theorem does not apply.

c. It seems that the reason for allowing complex-valued p is the author's will that the 'symplectic capacities' be complex-valued. (By the way, 'symplectic capacity' is used in a somewhat different sense in the symplectic literature.) This seems motivated by the contention that symplectic capacities are to quantum blobs what quantum amplitudes are to quantum states, hence should be complex-valued like quantum amplitudes are. No argument is given in the paper to back this contention, which is thus an implicit axiom. (Let's note, for instance, that symplectic capacities have dimensions of action, which is not the case of quantum amplitudes. This is easily solved by expressing capacities in 'Planck units', but it shows the nontriviality of the contention.) Besides, the complex-valued symplectic capacities are defined as 'complex-valued areas', which is not a self-evident mathematical notion, nor one to which the standard theorems of symplectic topology readily applies.

(2) Here are some elements regarding the proposed definitions of 'symplectic overlapping' and 'fidelity' in part (II).

a. Adopting the 'symplectic-topology perspective', one would expect the notion of 'quantum blob' to be a symplectic invariant i.e. any symplectically diffeomorphic image of a quantum blob is a quantum blob (much like any unitary image of a quantum state is a state (ignoring superselection rules)). Consequently, one would expect a notion of 'fidelity for quantum blobs' to be invariant under all symplectic maps (much like the quantum fidelity between two states is unitary invariant).
   Now, the proposed definition of 'symplectic overlapping' depends on the implicit choice of canonical coordinates (q,p): it is defined as the sum of the areas of the projections of the intersection of two blobs onto each of the conjugate pairs (q_k, p_k). Considering (among other things) just how complicated the intersection of two blobs can be, it is very unlikely that this 'symplectic overlapping' is invariant under change of canonical coordinates, i.e. under symplectic maps. (In fact, for real-valued phase-space and relying e.g. on work by Abbondandolo--Matveyev, one sees that the symplectic overlapping of the standard symplectic ball with itself is already not invariant under linear non-orthogonal symplectic maps.)
b. 'Fidelity' is a priori defined as some function F of the symplectic overlapping. F is required to satisfy some axioms that would allow to interpret fidelity as the probability of being unable to distinguish between two blobs. However, perhaps the most important of Kolmogorov axioms of probability is missing from the discussion, namely the 'additivity over mutually exclusive events'. What should be understood as 'mutually exclusive events' in the context of quantum blobs is unclear. However, considering how symplectic overlapping is defined, proving this last Kolmogorov axiom should be impossible for most choices of F, most probably for the choice of F made in the paper too.
c. 'Fidelity' is defined as the modulus squared of the symplectic overlapping. The reason behind this specific choice seems to be an analogy with Born's rule in QM, based again on the implicit contention that the symplectic overlapping is appropriately viewed as the analogue of the quantum amplitude.

(3) Here are some troubles with Part (III).

a. The ‘principle of conservation of fidelity’ is posited. Although the specific choice of F is used in Eq (17) to derive the ‘Schrödinger equation for symplectic overlapping’, the precise definition of the symplectic overlapping is not explicitly used anywhere (see (c) below for how this precise definition may even hinder the derivation). Hence, the conclusions of Part (III) seem to hold regardless of what is ‘the thing fidelity is the modulus squared of’. This is somewhat an abnegation of the whole project of relying on symplectic topology.
b. A Hamiltonian function H on phase space is supposedly derived from positing this conservation principle. Again, this can be seen as an abnegation of the symplectic topology perspective: there is no need to try defining symplectically invariant notions, since the conservation principle singles out one specific, canonical, almost cosmological symplectic flow on phase space.
c. Of course, it would be tremendously interesting if the principle of conservation of fidelity were sufficient to infer the existence of such a canonical Hamiltonian function on phase space. However, no justification is given for the claim that H is a phase-space function. This claim is even doubtful, since the existence ofH is inferred from an evolution equation for (some measure of) the intersection of two quantum blobs; this intersection extends over some region of phase space, hence can hardly specify a function neither at some nor at every point of phase space. This point is perhaps made clearest by looking at Eq (30): at which phase-space point should H be evaluated? (The existence of some – possibly time-dependent – real number $H = H(\Omega)$ in Eq (30) directly follows from the principle of conservation of fidelity, since then only the complex phase of Ω can evolve. So, what is really at stake in Eq (30) is to understand how H depends on Ω, i.e. on the blobs chosen.)