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Reviewer's report on Ph.D. Thesis presented by Jakub Janarek entitled "Quantum boomerang effect in disordered ultra-cold atomic gases"

The dissertation of Jakub Janarek presents results of his study on the quantum boomerang effect. In this effect, moving particles slow down due to collisions with a random potential, stop for a moment and eventually slowly return to the initial position. The boomerang effect is associated with the Anderson localization.

In Sec. 1, the author explains the boomerang effect from different perspectives. The explanation of this effect usually refers to the time-reversal invariance. The author's achievement, presented in Sec. 2, is a demonstration that this symmetry is not necessary. Precisely, the author shows with numerical simulations, that in a specific system without this symmetry the boomerang effect occurs. The numerical analysis is completed with extensive and challenging analytical considerations, in between with the help of the Berezinski diagrammatic technique. The essence of the method is to count how many times a particle passes each spatial point, taking into account the direction of the particle's movement. Different physical situations are represented by diagrams, which have to be classified and included in the calculation. For several pages, the author explains the method, nicely illustrating each essential step. Unfortunately, the method allows calculating the center of mass position only at short instances of time.

In Secs. 3 and 4, the author investigates the boomerang effect again, but for interacting atoms. The study is inspired by experiments on atomic gases cooled to temperatures of the order of 100 nK. At such temperature, the gas forms a Bose-Einstein condensate whose dynamics can be studied approximately by the Gross-Pitaevskii equation. The author uses this equation in Sec. 3 to show that the boomerang effect is only partial, i.e. the wave packet returns to a position only close to the initial one. The author suggests that the interactions induce dephasing, and therefore decoherence. The results of this chapter are published in Physical Review A

In Sec. 4, the author continues his study of interacting atoms but uses the full many-body theory. For weak interactions, the author points out corrections to the approximate results presented in the previous section. He examines strongly correlated systems - with infinitely strong interactions



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(the so-called Tonks-Girardeau regime) and strong interactions. He used exact correspondence between strongly interacting bosons and weakly interacting fermions in the latter case.

I read the dissertation with great interest. However, I will start my evaluation with some critical remarks, which do not alter my overall positive recommendation.

Equations (1.17) and the description below, including (1.21) are incoherent – the Author denoted with a single letter P , distribution of the local potential height with correlations between locations separated by r .

I attach this remark together with a list of minor comments as the attachment 1.

In my opinion, the connection between the model used in Sec. 3 and 4 and the physics of the 1D interacting gas could be improved. The interaction strength g , as defined via Eqns. (3.9) and (3.7) is relevant parameter, but only in the 3D system. One should redefine g after Eq. (3.12). Otherwise, the formula (3.27) does not mean energy (units wouldn't match). I would use the Lieb parameter (introduced in 4.6) or its modification to classify the interaction regime. It is because the 1D systems are very specific, i.e. strong interaction regime can be achieved by diluting the gas. In the context of experiments, it is safer to consider quasi-one-dimensional systems, i.e. 3D but squeezed by a tight harmonic trap in the Z and Y directions. Then the oscillatory length of this trap affects the parameters of the effective 1D model. In particular, conditions (4.4) would change, as the scattering length may have a different meaning.

I do not fully agree with the interpretation of eigenvalues of the reduced density matrix presented in Eq. (4.40). It may happen that in each realisation, we have a perfect condensate, but at a random location. Such a situation takes place for an attractive 1D gas without any disorder. In that case, a bright soliton is considered as a ground state. But strictly speaking, it is not – the actual ground state is translationally invariant, and it has slightly smaller energy than the bright soliton. This actual ground state would also have small eigenvalues of the reduced density matrix. But then, by a tiny perturbation, one can break the translation symmetry, reveal bright soliton, and a perfect condensate would emerge. The scenario presented in Sec. 4 might fall into the same category. A wavepacket eventually stops in each realisation but at a random location. It would be interesting to look at the reduced density matrices in single realisations to check how coherent the is the atomic cloud. If the final average state is a mixture of condensates, then the GPE may remain a good description of each realisation.

By comparing Figs 3.9 and 3.10 one already expects that E_b will be a good parameter. The subsequent discussion, during which the *nonlinear energy* is introduced is not very clear. In fact, one could use E_b on the X -axis of Fig. 3.13 or just the interaction energy at t_b .

I am very impressed by the methods used by the author. Sometimes dissertations are based on a single theoretical technique. In this case, the author used a whole arsenal of methods, often very advanced, like the Berezinski diagrammatic technique, the matrix-product states, or the time-

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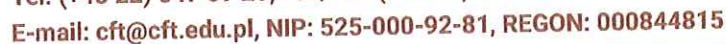
evolving block decimation method (TEBD). The dissertation, despite being focused on a single problem, shows the skills of the author and his knowledge in the broad areas of theoretical physics. He uses the techniques typical for condense matter theory, as well as the ones from quantum optics and atomic physics. I wonder if the author wrote TEBD by himself or used a ready library? I have a question concerning Sec. 4, devoted to 5 atoms: why the author didn't use the exact diagonalization method?

The thesis presents a single research problem but explored at multiple levels. I guess that for the author, the repetition of Figure 1.3 in successive scenarios might have been tedious. But finally, the dissertation is a thorough and detailed analysis of the problem and a valuable source of information. I believe that this is how a dissertation should be written. For me, the results are interesting and inspiring. To the extensive list of the possible extensions of this thesis included in the conclusion of the dissertation, I would add the study of the quantum-to-classical transition. In particular, particle losses are inevitable in any experiment and will undoubtedly affect the results. It would also be interesting to consider a noise, which would preserve some symmetries. I'm also interested in applying extensions of the GPE equation better suited to strong interactions.

I conclude that the presented dissertation meets the requirements expected for Ph.D. theses, stated in Polish regulations. The Candidate has clearly demonstrated his qualifications and original ideas in physics. Therefore, I recommend admission of Jakub Janarek to the subsequent stages of the procedure, including the public defense.

Krzysztof Pastorek

Attachment 1: List of misprints and minor comments.



1. Eq. (1.8) and above – the Heaviside theta is not precisely defined. In Eq. (1.8) – shouldn't be there ϵ instead of Hamiltonian in the exponent?
2. Eq. (1.18) : There is missing (n) .
3. Below Eq. (1.17): $V(r_i)$, but before it was depending on x . Then in the paragraph P depends on the distance r , whereas in equation it depends on the potential height V .
4. Eq. (1.17) and the description below, including (1.21) are incoherent. Eq. (1.21) defines the distribution P of the potential height at a given position. Then, the function P is used to define the distribution between the relative potential heights at locations separated by the distance r .
5. Eq. (1.30) appears a bit ad hoc. It uses the spatial correlation function for V , but in the momentum space. It wasn't introduced before.
6. Eq. (1.37): k_{ϵ} is not defined.
7. Eq. (1.37): One should justify steps between (1.37), (1.38), and (1.39). In particular, the self-energy is temporarily assumed to be momentum independent. Why?
8. Page 11, article in "an on-dimensional system".
9. Eq. (1.73) - why it is assumed that position and momentum are uncorrelated, i.e. $\langle xp + px, \rangle \approx 2\langle x \rangle \langle p \rangle$. It should be justified.
10. Page 21, "this two ingredients" -> "these two ingredients".
11. Caption of Fig. 1.6. Why $1/\Pi_0^2$ is proportional to $\langle p^2(t) \rangle$. It should be justified.
12. Above Eq. (2.15) there are formulas for two contributions to the density of states. Are they in fact contributions to the state-density per length? There is problem with units.
13. Eq. (2.3): The group velocities in the following texts are always positive, so there is missing sign in this definition.
14. Subsec. 2.4 and 2.5: It would be helpful to plot additional figure, showing complex plane with integration contours, branch cuts, poles and excluded region.
15. Page 50: The average product of the retarded and advanced Green functions "includes all possible paths between points x' and x ." One should add also "and between x'' and x ."



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16. Eq. (2.63) is not fully consistent with Fig. 2.10. It is not clear if x_i may refer to a scattering event coming from the advanced Green function (as in Fig.) or it refers only to the retarded Green function (as in Eq.).
17. Caption of Fig. (2.12) - there should be subscripts not superscripts, i.e v_- instead of v^- .
18. Page 68: The acronym DMPK is not defined.
19. Page 70: Misprint 0,008 $\rightarrow$ 0.008.
20. Page 81: "The GPE was successfully used in many experiments ..." $\rightarrow$ "The GPE was successfully used to explain many experimental results ...".
21. Page 91, above Eq. (3.24), "(see section 1.3, Eq. (1.77))" $\rightarrow$ Ref. [3]" (in Sec. 1.3 there is also no explanation of the long-time scaling, but the reference [3] is cited in Sec. 1.3).
22. Page 91: "brak time" $\rightarrow$ "break time".
23. Page 104: Diagram (3.47). Although this is in appendix, still the diagram with new elements should be explained.
24. Page 111: In the formula for D_l there should be $(M-1)!$ instead of $(M+1)!$
25. Page 113: Eq. (4.18). The second $\tilde{\Gamma}$ should have the label $l+2$ instead of $l+1$.
26. Page 114: The sentence above Eq. (4.22), starting with "The total phase imprinting the initial velocity.." is not clear.
27. Page 122: The asymptotic position of center of mass, $\langle x \rangle_\infty$ was obtained with the Eq. (4.41) or (4.42)? The latter is more accurate, according to the discussion after Eq. (4.43).
28. Page 131: "(..) non-interacting will return (..)" $\rightarrow$ "(..) non-interacting case will return (..)".
29. Page 133: "(..) entanglement averaged over the possible bipartitions.". Why such average entropy is used instead of the one defined in Eq. (4.46)?
30. Page 134: "(..) agree within several percent (..)" $\rightarrow$ "In case of weak interactions, (..) agree within several percent (..)".
31. Page 135: "In the simulations, the total number of particles is not very high (..)". I suggest to cite the number of particles, "In the simulations, we used $N=5$ particles (..)"

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