Referee report SciPost Physics: Beyond Gross-Pitaevskii equation for 1D gas: quasiparticles and solitons by J. Kopycinski, et al.

In the present work the authors construct a beyond mean-field hydrodynamic approach named the Lieb-Liniger Gross-Pitaevski equation (LLGPE). The resulting equation takes the form of the usual Gross-Pitaevskii one where the corresponding nonlinearity is inferred from exact results of the well-known Lieb-Liniger model. Also, the constructed model operates in cases where the density of the gas is slowly varying in space. The model is benchmarked against solutions of the Lieb-Liniger one and in particular regarding the ground state and the elementary excitations of the Lieb-Liniger model. For the elementary excitations the main focus is placed on two branches known from the Lieb-Liniger model as particle (type-I) and hole (type-II) modes corresponding to phonons and solitons and respectively. It is shown that a linearization analysis of the LLGPE gives the phononic spectra which coincide with the exact formulas for type-I excitations of the Lieb-Liniger model. Moreover, numerical minimization leads to solitonic solutions of the LLGPE characterized by a dispersion relation being comparable with the one of type-II excitations of the Lieb-Liniger model supporting a 'solitonic' branch.

I find the present work an interesting attempt towards setting up beyond mean-field models and the consequent understanding of many-body effects. This is undoubtedly a research field of continuous interest especially corroborated by various experimental realizations. To my understanding suitable extensions of this method might find applications in different contexts. On the other hand, I believe that the manuscript requires a serious revision in order for some issues to be clarified and thus become accessible to a broad audience. Indeed, several parts need to be rephrased, explained and supported by stronger arguments while additional relevant references need to be included. Moreover, I have a number of comments regarding the presentation and explanation of the results. For these reasons I can not recommend the manuscript for publication in SciPost in its current version. However, if the authors address my suggestions convincingly by properly revising their manuscript, then their work might be significantly advanced and be suitable for publication in SiPost Physics. Below, I provide a list of mandatory (to my opinion) revisions.

1) In the abstract the meaning of the so-called type-I and type-II excitations should be briefly clarified. Which are these excitations and what are their differences? Do they refer to particle and hole excitations related to phonons and solitons respectively? Also, an example needs to be provided for the long- and short-wave structures.

2) I would strongly encourage the authors to describe in the Introduction in more detail the major results of their study. Currently, the description is very short and it is focused on the topics that will be discussed and not the actual new results. Moreover, since the topic of quantum dark solitons is a main topic of interest in the main text I believe that it deserves to be mentioned even briefly in the Introduction. Here, relevant references besides the ones by Sacha should be included such as earlier studies where some of the authors participated but also from other groups (see e.g. *Phys. Rev. A 98*, 013632 (2018), *New J. Phys. 19*, 073004 (2017)). This way, the reader can be directed to them and have an overview both for the homogeneous and for the trapped cases.

3) In a related note a mentioning on the fact that droplets in one-dimension have been investigated already by using ab-initio approaches such as the quantum monte-carlo is missing. This should be certainly rectified and relevant references in one-dimension such

as Phys. Rev. A 102, 023318 (2020), Phys. Rev. Lett. 122, 105302 (2019) and *arXiv:2108.00727* need to be included if the authors want to keep the claims regarding the Lee-Huang-Yang energy correction. Currently only references regarding higher dimensions are provided while the focus of the present analysis is in one-dimension.

4) In the Introduction, the sentence “The figures of merit are their dispersion relations and …” reads awkwardly. Please rephrase.

5) What is the physical interpretation of the pressure term given in Equation (4)? Please elaborate.

6) After equation (7) it is stated that the Gross-Pitaevskii theory is not justified for strongly repulsive atoms. I do not entirely agree with this statement. I think it should be complemented and mention that also in the weakly interaction regime the Gross-Pitaevskii theory can fail due to the presence of quantum correlations. There are several demonstrations of this fact especially regarding the dynamics of cold gases.

7) At the end of equation (10) the “,” should be replaced by “.”

8) The last terms appearing in equations (13) and (17) and representing beyond Gross-Pitaevskii corrections (if I understand correctly) as well as their role should be clearly explicated. Why these terms operate beyond a macroscopically occupied orbital and for which order/type of correlations do they account for? This is a central finding of the present work and should be clearly communicated to the reader.

9) After equation (17) it is stated that equation (13) coincides with the equation proposed in Ref. [15] in the limit of \gamma<<1. I suggest to explicitly state some more details here regarding the equation provided in Ref. [15] in order the corresponding description to be accessible to a broad audience.

10) At the beginning of Section 4 the word “apperas” should read “appears”.

11) In Section 4.1 the case of a soliton in a Tonks-Girardeau gas is discussed among others. What is the value of the interaction strength g where this strongly coupled bosonic system is approached? It is also very important that the properties of the black solitons in this regime will be mentioned. Do they remain the same as in the weakly interacting case? Please elaborate at least briefly and also provide some relevant references if any to this topic such that the interested reader can be directed.

12) On page 11, the sentence “We examine closer a relation between these excitations studying …” is not understandable to me. Please rephrase.

13) The legend of figure 5 is not displayed properly. Please fix this issue.

14) In the right panels of figure 5 the two- and three-body correlation functions are shown. It can be seen that in the non-interacting case both the two- and three-body correlations do not become zero at the center of soliton. However, in the Tonks regime there is a clear correlation hole probably due to fermionization. I wonder whether the finite value of G_2 and G_3 at the center of the soliton in the non-interacting case is related to the fact that quantum dark solitons have a filled core due to presence of correlation effects. Please comment. If this is not the case how the basic properties of quantum black solitons can be discerned within this approach. Here I mean properties such as the filled core of the dark

soliton which is a manifestation of the presence of condensate depletion and in turn related for instance to the dispersion of the dark soliton's position. These processes are already discussed in the already cited references and the ones suggested in question 3. A relevant discussion is necessary.

15) Along the same lines, can the authors comment on the dynamical evolution of usual mean-field black soliton embedded in the LLGPE approach as an initial condition?

16) In the first paragraph of page 13 it is commented that higher order correlation functions have m local minima and not a single one as the common soliton structure. Is this observation a manifestation of the fact that quantum solitons may exhibit a splitting process or a dispersive behavior due to correlations? Please clarify.

17) I would suggest to carefully check the entire manuscript for typos and grammar errors beyond the ones mentioned above. This is certainly needed and will be definitely beneficial for the reader.