Answers on review's comments on paper

The Lagrange form of the nonlinear Schrodinger equation for low vorticity waves in deep water: rogue wave aspect

by

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<u>RESPONSE TO REVIEWER 1</u>

THE MOTIVATION:

<u>Review 1:</u>

From the provided literature review it is not clear why this particular study is needed? ***What the specific questions that the authors want to clarify? Why these questions might be of interest and for what segments of the scholar community?***

<u>Authors:</u>

We found a new family of solutions for the wave train propagation in the deep water. Their novelty is **non-uniform distribution of the vorticity**.

<u>Reviewer 1:</u>

Wavetrain modulations upon arbitrary vertically sheared currents were thoroughly studied by Benny and his group. If the Benny asymptotic expansion becomes invalid for the range of small values of vorticity the present work is focusing upon, then it has to be shown and explained what is wrong with the Benny expansion. The same question applies to Jonhson (1976) results. The dependence of the cubic nonlinearity on vorticity in Jonhson (1976) and the works by Benny is not singular. Therefore similar expansion for the small vorticity can be carried out in the Eulerian framework as well using the known results, say, by Jonhson (1976) and/or the works by Benny group as the starting point. I think what the authors are doing is a re-derivation of the NLS for weak vorticity; the results were known, although implicitly, since nobody looked specifically into this case.

<u>Authors:</u>

We study flows with the vorticity depending on both Lagrange coordinates. That corresponds to the background current depending on the variables x, y, t in the Eulerian approach, not a shear flow $U(y)$ as Johnson or Benny studies. **Our approach differs from other known ones cardinally.**

<u>Reviewer 1:</u>

Hence there is indeed a novelty here, but a comparison with the Eulerian results is necessary. In the Eulerian case vorticity can also be always presented as an explanation in *epsilon*, although in contrast with Lagrangian approach only the leading order vorticity will be constant.

Authors: Yes! But the functions of vorticity's row Ω_n ($n > 1$) depend on x, y, t, i.e. $\Omega_n = \Omega_n(x, y, t)$. They are not integrals of motion as in the Lagrangian description. So our question to the reviewer: how to set these functions? It is obvious that **the Lagrange approach is more preferable in that situation.**

Reviewer 1:

In this context the most intriguing question is concerned with one of highlights of the work: the vanishing of the cubic nonlinearity in the NLS in the Lagrangian variables for the Gerstner wave. (This result is more significant than the authors it credit for: it shows that in principle an $O(\varepsilon^2)$ shear might kill the NLS nonlinearity. The question is: what is the manifold of Eulerian shear profiles (or vorticity distributions which would zero the NLS nonlinearity? I believe it could be answered by a straightforward analysis of the known expressions for the coefficient.

Authors: **That's right.** Using the accordance principle (pages 13, 14) we can conclude that the shear flow with the vorticity equal to the vorticity of the Gerstner wave kills the nonlinearity in the NLS equation.

Reviewer 1: Also the similar question applies to the Lagrangian formulation: the vorticity distribution is arbitrary, what are other distributions for which the NLS nonlinearity vanishes? I doubt that the Gerstner is an isolated special case.

Authors: The NLS equation's nonlinearity vanishes if $\psi_{1t_1} = 0$. The Gerstner wave is **the single solution** of this equation.

Reviewer 1: It follows from the works by Benny and his group that transverse instability is much stronger than the longitudinal one, therefore, the studies of strictly longitudinal instabilities are limited interest from the viewpoint of sea applications and could be applied only to narrow wave tanks. I'd like this point to be mentioned more explicitly in the introduction. This is important since it squarely places the derived NLS into the realm of toy models. This does not mean that the results cannot be of interest or should not be published, it just means that the results might interest a different community.

Authors: We mentioned Benny's result in the introduction (lines 63-68). And suppose that it is quite enough. In our opinion the limit of longitudinal instability is

rather applicable for open sea conditions where the length to width ratio of a non-linear wave package is much less than in any restricted waters.
As for narrow tanks they are usually much more active in view of transverse instability and need special tuning to avoid this effect. The reviewer is absolutely correct in this state. So the general formulation of the model under the condition of the rigid borders could be applicable for interpretation of experimental results as well.

Reviewer 1: The original element of the work is the asymptotic derivation of the NLS in Lagrangian variables. In my view this is complementary to the existing Eulerian works and it remains unclear what new features/aspects this might reveal.

Authors: The original aspect of our study is **horizontal non-uniformity of vorticity's distribution** (lines 126-128). As a consequence, in contrast to Benny and his group and Johnson we derived the evolutional equation with variable coefficients.
THE NLS:

Reviewer 1: In contrast to the NLS in Euler variables where we know that the equation describes evolution of the envelope amplitude in the (x,t) space and how the actual elevation can be expressed as a Stokes-like series in wave amplitude up to cubic order, here the NLS in Lagrangian variables is an object which is much less straightforward to interpret. Obviously, A is the envelope amplitude, but what are the independent variables (a,b)?

Authors: Lagrangian variables are the labels of the fluid particles, nothing more over.

Reviewer 1: Their link to the standard Eulerian variables (x,y) is not known. Although, it is straightforward, at least in principle, to provide this link in terms of series in ε, the authors choose not to do this. The effectively use the zero order approximation where the difference between the Eulerian and Lagrangian description vanishes. Then the rationale for using the Lagrangian approach apparently disappears.

Authors: **That is not correct.** We derived a new family of solutions due to Lagrange approach. It is much more difficult problem to get them in Eulerian description which has not been solved yet.

Reviewer 1: I suspect (this is the most interesting point), that if the authors make transformation to return to the Euler variables, they will get a higher order NLS type equation since the transformation itself is nonlinear (see e.g. F. Nouguier, B. Chapron, C-A, Guérin Second-order Lagrangian description of tri-dimensional gravity wave interactions, JFM 772, 165-196 (2015) and references therein).

Authors: That is a special problem. We are ready to discuss it further.

Reviewer 1: If the authors do not want to go through this straightforward but quite time consuming pass I suggested above, then they can handle the comparison numerically. The Lagrangian solution yields X,Y in terms of a,b,t. Hence the surface elevation $Y(a,0,t)$ and position of a parcel on the free surface, $X(a,0,t)$, which are found in terms of series, provide implicit function $Y(a,0,t)$ which can be easily plotted for a typical $Y(X,t)$, say, a breather. This plot has to compared to the Eulerian solution with the cubic terms retained.

Authors: That is a good programme, but nobody has calculated the Eulerian solution with the cubic terms. All authors are restricted to the derivation of the NLS equation. With what solution do we have to compare our results? Or we must study our problem in Euler variables too? Besides, we are interested in rogue waves in this paper and study the leading order of the solution only. The terms of the second and cubic orders are out of our attention.

Reviewer 1: The obtained NLS is presented in an "optical" form (with space rather than time chosen as the propagation variable), which is somewhat strange choice for a hydrodynamic work. Dependence on t in this context means dependence on running variable. I do not understand why the authors choose this form and stick to it, they give no clue. They have either argue for their preference or switch to the conventional form.

Authors: In traditional hydrodynamics form (in variables a_1, a_2, t_2) it is impossible to lead our evolutional equation to the usual NLS equation. So it is used the "optical" variant of the equation. We shall switch that explanation in the conventional form.

Reviewer 1: The authors consider the NLS derivation allowing for horizontal non-uniformity, which raises a host of questions. How arbitrary the dependence on a_2 is? What does it mean? Are the a_2 dependencies of these vertical and potential parts of the Doppler correction linked to satisfy the Lagrange equations? How these dependencies can be specified?

Authors: The vorticity $\Omega_2(a_2, b)$ is arbitrary continuous differentiable bounded function. That means the boundedness of the vorticity or the derivatives of the field of the velocity. The vertical and horizontal parts of the Doppler correction don't link. It is obvious from the comparison of the equations (41) and (44).

ROGUE WAVES:

Reviewer 1: As I've already mentioned, the strong transverse instability of the wavetrains does not allow one to speak seriously about ocean applications. I found nothing new and specific adding to our understanding of rogue waves. The fact that the NLS is formulated in the Lagrangian variables and only the leading order term is used makes this equation equivalent (to this order) to the Eulerian NLS. The fact that in the focusing NLS there is modulational instability and that such NLS admits breather solutions is known for about thirty years.

Authors: Definitely we do not propose a new understanding of rogue wave formation, just the original approach of their description. And in this sense our solutions are not analogous to any known ones.

Reviewer 1: The term "rogue wave" is used in the manuscript as synonymous with the term breather, just because the latter satisfy the rogue wave criterion. Although the NLS breather solutions are indeed often used as prototypes of rogue waves, this could be done only with appropriate explicitly spelled out caveats.

Authors: The last phrase could be considered as a private opinion of the reviewer. Using the corresponding terms we just follow traditions of the scientific community.

Reviewer 1: The weakest point in the rogue wave aspect of the paper is that I don't see any new insight into the nature of rogue waves even in the framework of the chosen toy model.

Authors: The novelty of the present paper is that we proved a possibility of formation and propagation of the rogue waves at the background of the horizontally non-uniform current. One can consider a single localized vortex as an example of this solution. The waves of such type didn't study yet.

Reviewer 1: In my view the following question might be of interest in the context of rogue waves and would have an element of novelty: what is the profile and maximal height of the found Akhmediev Lagrangian breather in the Eulerian variables. To answer this question the authors have to sum up all orders of their expansion and then perform the transformation to the Eulerian variables. The results will differ from the corresponding expansion in the Eulerian variables. I re-iterate that it would be of interest to discuss this difference. I've mentioned already the simplest way to get it.

Authors: That is a good idea. But we have stress: where does the reviewer see the explicit solutions up to the third order in the Eulerian variables? And why nobody has accomplished this work in the Eulerian description? We assume that the reviewer doesn't understand the complexity of his suggestion. He proposes a big new project which could be the subject of a new paper.