Referee's report on manuscript # NPG 2016-71

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

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The manuscript is devoted to a well studied aspect of water wave dynamics - weakly nonlinear evolution of a narrow band wavetrain. The work contains two novel elements: the focus of the consideration is on waves with weak vorticity, which has not been a subject of a dedicated study before; the analysis and results are formulated in terms of Lagrangian variables. The main result is the derivation of the nonlinear Schrödinger equation for the wavetrain envelope in the Lagrangian variables and an analysis of a few examples on its basis. The derivation is sound. The results are discussed from the rogue wave perspective. Although the work does present new results, seems to be correct and is clearly written, I cannot recommend its publication yet. However, it has a potential to be turned into a much better paper. To this end a revision is needed. The specific points to address are outlined below.

The main points:

(i) The motivation: From the provided literature review it is not clear why this particular study is needed: ***What are the specific questions that the authors want to clarify? Why these questions might be of interest and for what segments of the scholar community?*** Wavetrain modulations upon arbitrary vertically sheared currents were thoroughly studied by Benney and his group. If the Benney asymptotic expansion becomes invalid for the range of small values of vorticity the present work is focussing upon, then it has to be shown and explained what is wrong with the Benney expansion. The same question applies to Jonsson (1976) results.

The dependence of the cubic nonlinearity on vorticity in Jonsson (1976) and the works by Benney 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 Jonsson (1976) and/or the works by Benney group as the starting point. This should be made clear. 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. 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 expansion in ε, although in contrast with the Lagrangian approach only the leading order vorticity will be constant. In this context the most intriguing question is concerned with one of the 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 give 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. 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.

It follows from the works by Benney and his group that the transverse instability is much stronger than the longitudional one, therefore, the studies of strictly longitudional instabilities are of 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.

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.

(ii) The NLS: 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)? 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 a series in ε, the authors choose not to do this. They effectively use the zero order approximation where the difference between the Eulerian and Lagrangian descriptions vanishes. Then the rationale for using the Lagrangian approach apparently disappears.

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, Guŭrin *Second-order Lagrangian description of tri-dimensional gravity wave interactions*, JFM 772, 165-196, (2015) and references therein).

If the authors do not want to go through this straightforward but quite time consuming path 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 a 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.

The obtained NLS is presented in an "optical"form (with space rather than time chosen as the propagation variable), which is a somewhat strange choice for a hydrodynamic work. Dependence on t in this context means dependence on the 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.

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

(iii) Rogue waves: 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 focussing NLS there is modulational instability and that such a NLS admits breather solutions is known for about thirty years.

The term "rogue wave"is used in the manuscript as synonymous with the term breather, just because the latter satisfy the rogue wave amplitude 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.

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.

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.