The revised version of the manuscript has addressed some of the points I raised in my report. Although I still stand by every word of that report I do not feel that I should force the authors to do something they clearly don't want to do. I see my duty as a reviewer (apart from ensuring that the results are correct) is to help authors and readers in clarifying what has been done, how this relates to the existing knowledge and to provide recommendations to the authors on what else could be done to increase the impact and significance of the findings. Once all that has been made clear, it is up to the authors to decide what they want, since it is their work.

To formulate the comments to the revised manuscript it is helpful first to return to the main points of the discussion. Here are the key point verbatim:

Reviewer 1:
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 focusing upon, then it has to be shown and explained what is wrong with the Benney expansion. The same question applies to Jonhson (1976) results. The dependence of the cubic nonlinearity on vorticity in Jonhson (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 Jonhson (1976) and/or the works by Benney 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.**

Authors:
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**.

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

The present review: The following points should be made clear:

(i) The weak vorticity shear currents are in fact *weak currents* in the appropriately chosen reference frame. In linear setting the effect of weak currents on dispersion relation to leading order in current-to-phase velocity ratio is captured by the Stewart and Joy formula (R. H. Stewart and J. W. Joy., Deep-Sea Res., 21:1039–1049, 1974.). In the NLS context this correction enters the equation additively and yields the term with beta (in the author's notation). Of course, if we apply this literally will get the beta not dependent on the horizontal coordinate. This result is implicit in the NLS derivation by Johnson (1976).

(ii) However, it is pretty obvious that in the Stewart and Joy formula we can allow the current to depend on slow time or horizontal coordinate. Correspondingly, in the standard (Johnson or Benney) derivations we can take an epsilion square weak current and allow it to depend on epsilon square slow time or space scale.

Thus, the derived NLS with the non-uniform distribution of the vorticity is implicitly contained in the known results. This point should be made clear in the text of the manuscript. On the other hand, this has never been explicitly stated and in this sense is novel. Perhaps this alternative derivation with emphasis on weak vorticity currents might be of some independent interest.

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 Benney and his group and Johnson we derived the evolutional equation with variable coefficients.
The present review: If you allow the weak shear current to depend either on T_2 or X_2 the NLS will get the term with beta dependent either on T_2 or X_2. I reiterate that it has not been explicitly demonstrated.

An important issue of the discussion was about the implications of the derived NLS, about its meaning.

Reviewer 1: In contrast to the NLS in Euler variables where we know that the equation describes evolution of the envelope amplitude in the 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.
The present review: This is not an answer; of course, they are labels. But how do we specify these labels? How can we express the quantites we are interested in in terms of physically well defined variables In this case a common convention that variables (a,b) are the coordinates at the initial moment does not apply. Hence, there is an issue which should be recognized. This does not mean that the result itself is meaningless, absolutely not. But one has to formulate a way of expressing the results in well defined variables.
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.
The present review: I did not see a new family of solutions and I disagree with the author's claim.
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 and position of a parcel on the free surface *Y(a,0,t),* found in terms of series, provide implicit function *Y(a,0,t)* which can be easily plotted for a typical solution, say, a breather. This plot has to be 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.

The present review: To derive NLS or its generalizations in the Eulerian description one has to find the first few terms (up to the cubic) of an asymptotic expansion in epsilon. This has been done by a number of authors. Hence, the surface elevation up to cubic terms in epsilon does exist (at least in implicit form) in the literature. The authors are saying that they are interested in the leading order only. In my view the interesting things will appear in the second and third orders. However, it is up to the authors to decide how far they are prepared to go in this manuscript and what they want to present.

Conclusion: If the authors accommodate my comments above, that is they make clear to readers what has been done, I would recommend the publication of this work in the NPG. The English also needs poloshing prior to the publication.