Author Comments to the Editor.

Dear Prof. Grimshaw,

The revised manuscript is downloaded. I was unable to provide a pdf file with comparison between the versions using LatexDifference, that program only works on the text comparisons. I don't know what I need to change in equations to make them doable for the LatexDifference. If required I could supply TEX files of the initial and the revised manuscripts. The major comment from the Referee 1 was mainly concerned on the proper references to classical and some recent papers. Appropriate references were added to the revised paper. The major comment from the Referee 2 was concerned with the question on stability of solutions for the KdV type equation with quartic and higher nonlinearities. This question was addressed in the revised version and additional figures for stable and unstable solutions were provided.

The minor comments from both referees addressed some misprints, which are corrected in the revision.

Sincerely,
Oleg Derzho

Author Reply to Referee 2.

Dear Referee 2, Thanks for the fruitful comments. All minor comments and appropriate references to "2+4 nonlinearity" and damped solitary waves are incorporated in the revised version. Major comment on stability has been discussed. Actually the revised version included stability analysis for the considered waves based on the result of Bona et al. 1987. The main result is that some 2 +3+4 waves are stable but some do unstable. All these issues are examined in the revised version.
Sincerely,
Dr.Oleg Derzho

Author Reply to Referee 1.

Dear Prof. Stastna,

Thanks for your comments. **My point to point replies are written in bold for better readability.**

General remarks.

1. The presentation varies between full disclosure of equations, to wide gaps in logic and some very odd, and at times hilarious, linguistic mis-steps.

   Words such as hilarious and odd, wide logic gaps are not appropriate according to the Editorial Policy.

2. As near as I can understand the context of the results presented (and the presentation of context is pretty poor in this manuscript) it is that for a linear stratification the well known Dubreil-Jacotin-Long (DJL) equation that governs fully nonlinear solitary waves linearizes and no solitary wave solutions are possible. This does not mean that the stratified Euler equations to which the DJL is equivalent linearize in this case, but it does mean the nonlinearity needs to be addressed by other means (see Grimshaw and Zengxin, JFM 229), for an example which shows why the KdV is not the relevant equation here.

   It is correct that DJL equation for exactly linear stratification is linear even if wave amplitude is not small. Grimshaw and Zengxin (JFM, 1991) derived the forced Korteweg-de Vries equation to describe resonant flow over topography. When the fluid is uniformly and weakly stratified the quadratic nonlinear term is absent. That case requires an alternative theory which was examined in the above mentioned paper. It is crucial to note that the case considered in the submitted manuscript implies that
   a) the rigid lower boundary is flat, so flow over topography is irrelevant to the present manuscript;
   b) stratification is essentially nonlinear as stated on page 2, eq.(1). So nonlinearity is present and this nonlinearity is responsible for the multiscaling. It is the core point of the paper.

3. The author also does not provide any of the context. I have provided in the above paragraph and indeed presents the DJL equation as his own past work.

   I certainly do not present DJL equation as own past work. The paper clearly states that on page 3 lines 7 and 8. I will add the appropriate references on pages 1 and 2 to avoid any misunderstanding. Additionally I would add more references on the subject of the paper as suggested.

4. The stratification used for the primary example in Figure 2 has a largest departure from the linear density profile on the order of 5e-6 (or 5e-4 when scaled by the top to bottom density difference). This strikes me as linear for all intents and purposes, and certainly to the extent that field measurements could discern. The author makes no effort to explain how broad of a range of stratifications his theory applies to.

The presented theory is asymptotic so the range of stratifications the theory applies to is given in the model assumptions. Please look at Eq. (1). Stratification is essentially nonlinear as stated on page 2, Eq.(1). Effects of nonlinearity associated with nonlinear stratification lead the multiscaling. It is the main point as anticipated above.

Variation of stratification due to the fine structure mentioned in Eq. (15) is $2\alpha\sigma^2 \sim 5 * 10^{-4}$ as noticed by the referee. From this figure the referee stroked the profile "as linear for all intents and purposes, and certainly to the extent that field measurements could discern". To this point, I would again mention that the paper considers asymptotic produre and some numbers are immaterial and presented for illustrational purposes. The major point of any asymptotic theory is the definition of scales. The value of σ defines wave speed according to the equation in line 10, page 2. The value of δ defines the horizontal length scale once we fix δ/μ^2. For illustration $\sigma = 0.01$ and $\delta = \sigma$ were taken in the manuscript. For example, if $\sigma = 0.04$ and $\delta = 0.25$ the result of the manuscript remains the same, just wave speed is doubled and physical horizontal length scale is increased in 5 times.

5. When I put the stratification used to produce Figure 2 into my DJL solver I do not get multi-scale solitary waves, but a small solitary wave of depression. I am not discounting the possibility of the multi-scale solitary wave, but it is troubling that the variational method more naturally converges to a different wave.

As I understand your DJL solver, it is not designed to account for free surface and the nonlinearity associated with it. Thus the comparison with the presented model is not correct. Moreover, I just have to notice that any numerical matters related to the specific numerical method with its specific convergence are beyond the scope of the present manuscript. However, I definitely mention the spectral method implemented in your group that is designed to be applicable to a broader set of stratifications even with fine structure.

6. I do want to note that I like the characterization in terms of the polynomial the author provides, but the presentation needs to make the method reproducible by the reader (at present I have no idea how P_N is computed and the 1968 Mathematical Handbook the author quotes for the result is not useful for providing this vital information).

In the current manuscript only polynomial formula for stratification is considered, it directly leads to nonlinearities in the polynomial form. To justify the model for a more complicated form of stratification the Weierstrass approximation theorem (1885) provides a theoretical foundation for the presented approach. The Weierstrass approximation theorem states that every continuous function defined on a closed interval [a,b] can be uniformly approximated as closely as desired by a polynomial function. (for recent accounts on the topic look at Hazewinkel, Michiel, ed. (2001), "Stone-Weierstrass theorem", Encyclopedia of Mathematics, Springer, ISBN 978-1-55608-010-4). I did not construct approximations in the paper, just mentioned that it is a mathematically correct procedure. Such construction is beyond the scope of the present manuscript.

7. The reference list is 40% self-citation, which would be fine for a strong result, but seems like a poor choice for what looks like a mathematical oddity at best.

   The list of references will be extended in the revised version. Nonetheless, this "oddity" was discussed in your own paper. The major point is that the present asymptotic model predicted multi humped solitary waves back in 1990 and the original result was cited only once in 2011 in NPG, nothing appeared before and after. In order to demonstrate priority, the author decided to submit to NPG as to an open access journal for wide international audience.

Specific detailed comments.

page 1

"Lines 10 sentence is meant to say the opposite of what it actually says"

Line 10 probably reads unclear. A capillary ripple superimposed on gravity wave is one of such examples. Going to change as follows.
If the wavelength of the disturbance is too small AND COULD DISPLAY, for instance, capillary dispersion, multiscaled solitary waves are possible as shown by Benjamin (1992).

Line 12: How can a similar effect be observed due to viscosity. Viscosity means energy is not conserved and hence solitary waves cannot exist.

I did not mean stationary solitary waves in that sentence. I meant the situation in the vicinity of the breaking point where singularity resolves through the generation of an oscillatory zone, weak dissipation defines the scales in that zone as originally described by Benjamin & Lighthill 1954. Multiscaling in Introduction means that several scales could be observed due to different competing physical mechanisms contrary to the one physical nonlinearity described later on in the paper.

Line 23: again I think the sentence states the opposite of what it actually means to state.

In the sentence appeared in lines 23-25 the word "Neither" should be replaced with "Either"   The additional letter "N" was a sad typo. Sorry about that.

The sentence now reads as follows.
However, either specific nonlinearity in terms of power series in wave amplitude necessary to reveal a two humped structure or regions of density profiles at which such structures exist were not presented.

page 2:
The equation (number 2) is the DJL equation, why not explicitly state this?

I will state this for sure. However eq.(2) is written readily for asymptotic theoretical modelling using the proposed assumptions. For DJL in its full form an application of asymptotic approach is impossible.

What is the reason for keeping the free surface? It seems like an unnecessary complication.

The reason why the free surface is kept is that I presented the effect of the multiscaling on the surface, i.e. predicted scale and height of the surface displacement. Free surface also affects nonlinearity in the solitary wave. To this end, direct comparison with DJL numerical models between solid boundaries is incorrect as mentioned before.

Line 20: "searched" is not the correct verb here; perhaps "sought"? **Agree, will change**.

Line 20. Equation (7) and similar expressions; please use \cos in latex.
Agree, will change.

page 3:

Line 20: So the whole set up is a perturbation of the linearly stratified case? Seems restrictive.

Do you mean the perturbation as a flow that returns to a linearly stratified state when the wave has passed through? It is not the case since undisturbed stratification is nonlinear as stated in eq. (1). However, undisturbed stratification is supposed to be only slightly nonlinear. Yes, it is a restriction. But this restriction introduces a small parameter necessary to construct a tractable asymptotic model without

restriction of small wave amplitude. And the presented approach generalises the seminal work by Benney and Ko (1978)

Then the solvability condition is expressed in terms of a polynomial P_N which is only given implicitly? An example or two here is essential.

In the revised paper I will present the amplitude equation in exact form for the specific stratification given in eq. (15)

page 4: "tree-humped" **Sorry for the typo that slipped through the spell checker.**

Figure 1 is hard to make out, but I guess the ordinal is alpha (written as "alfa"), the definition of which only appears after Figure 1 is discussed. Or is this the delta of equation (1)? In any event, a clearer exposition is needed.

Alpha is a constant as defined in eq. (15) I need to mention that below the eq.(15. The value of δ defines the horizontal length scale once we fix δ/μ^2. For illustration $\sigma = 0.01$ and $\delta = \sigma$ were taken in the manuscript. Moreover, Lamb and Wan (1998) and Damphy et al. (2011) numerically discussed the case of 2 pycnoclines stratification. The present study (along with the earlier paper by the author) explicitly shows multiscaling for the case of monotonic Brunt-Vaisala frequency, see eq. (15).

The figure contains a typo and will be corrected in the revision. Sorry about that.

page 5 and 6: The Conclusions are really barebones. Is it possible to suggest how these waves could be generated; would flow over topography do it?

Generation of waves by topography is an essentially transient phenomenon and to this end lies beyond the validity and the scope of the present study. Seminal paper by Benney and Ko (1978) among others stated that any initial disturbance will be split into solitary waves and continuous spectrum. Solitary waves propagate until viscosity effects become apparent. As derived, for example, by Grimshaw and Yi (1991) uneven topography does not produce nonlinear terms in the forced KdV type equation. The present paper shows that multiscaling is the interplay of various nonlinear terms in the KdV type equation.

Sincerely,
Dr. Oleg Derzho