

Interactive comment on “Multiscaled Solitary Waves” by Oleg G. Derzho

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This manuscript considers the topic of multi-scaled solitary waves in continuously stratified fluid. The topic is of mathematical interest, though no evidence that such waves actually occurring in nature is provided (or exists in the literature, to the best of my knowledge). The presentation varies between full disclosure of equations, to wide gaps in logic and some very odd, and at times hilarious, linguistic mis-steps. The expression “tree-humped solitary waves” that slipped through the spell checker will linger in my memory for some time.

I have worked on fully nonlinear solitary waves for a very long time. 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

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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). The present manuscript considers stratifications that are nearly linear, and uses a perturbation expansion to construct solutions. 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 am 100% OK with the author disagreeing with me, but I am not OK with nothing being said at all.

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, and 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. 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. Lamb and Wan have considered stratifications with multiple solitary waves possible for a given stratification, so 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.

Incidentally the DJL code I mention above is the same code that we reported in Dunphy et al 2011, and the main point there was not that multi-scale solitary waves actually occur in nature, but that the spectral methods we implemented allow for even something as finely balanced as one of these waves to be computed in minutes. Indeed, as appears to have been missed by the present author, the method outlined in that paper computes exact multi-scale solitary waves for a much broader set of stratifications than the present manuscript addresses. 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

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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 order for the manuscript to be publishable the author needs to provide a fair assessment of the literature and his findings. I think some of my comments above, and the detailed comments below, will provide the means to do this. In the end, I think NPG is a good place for nifty mathematical problems but the presentation and editorial standard needs to be much higher than that in the initial version of this manuscript.

Detailed comments:

page 1

Line 10: I think this sentence is meant to say the opposite of what it actually says

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

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

page 2:

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

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

Line 20: “searched” is not the correct verb here; perhaps “sought”?

Equation (7) and similar expressions; please use \cos in latex

page 3:

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

page 4: “tree-humped”

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.

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?

Interactive comment on Nonlin. Processes Geophys. Discuss., doi:10.5194/npg-2017-11, 2017.

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