Nonlinear Processes
in Geophysics

Discussions

EGU

Interactive comment on "Limiting amplitudes of fully nonlinear interfacial tides and solitons" *by* Borja Aguiar-González and Theo Gerkema

Anonymous Referee #2

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The work is devoted to a numerical analysis of the MCC-type equations describing strongly nonlinear waves in a two-layer fluid. Its novelties are in adding earth rotation (Coriolis force) and an oscillating forcing imitating a tidal current over a bottom feature. Tidal forcing is represented by an oscillating bottom hill which in most cases gives a reasonable approximation for the case of a fixed hill and periodic current. Ten variants are computed which differ in forcing velocity, layer thicknesses ratio, relative height of the hill, and the Coriolis force (latitude). Some interesting results regarding the parameters of limiting solitons, rate of their formation (in tidal periods). Some results, such as decreasing of soliton amplitude with the increase of forcing, and chande of amplitude and width of a strong limiting soliton, remain unexplained; I agree with authors that it may be due to interaction with current induced by the oscillating source.

In general, the paper deserves publication. However, some questions and notes should

be taken into account. Among them are:

1. The MCC system which is the base of the model, allows strong nonlinearity but only weak (quasi-hydrostatic) dispersion. On the other hand, stationary waves, including a soliton, realize a balance between nonlinearity and dispersion. Thus, (unlike the weakly nonlinear case), applicability of such systems for solitons cannot be taken for granted and need to be verified. It is even possible that some numerical "paradoxes" are due to this limitation (see, e.g., Ostrovsky&Grue, Phys. Fluids, 15, 2934, 2993). This circumstance should at least be mentioned.

2. The weakly nonlinear and "quasi-nonlinear" case is not quite clear for me. It should be close to the eKdV case (where the limiting solitons also exist) but the results seem somewhat different. The physic of this case should be better explained.

3. Paragraph 120. "c0 is an approximate measure of the linear long wave phase speed." – Why approximate, what is the approximation?

4. The reasoning in paragraph 325 should be made simpler and more clear.

5. Figures 8 and 13. How comes that the numerical (color) circles for soliton velocity do not go to 1 at zero amplitude limit? Is this due to some negative period-averaged current?

6. Arguments about the role of higher harmonics are unclear. First, there are no spectra shown in the paper. Second, it remains unclear how the Coriolis dispersion can enhance the table-top soliton form (paragraph 635).

In general: the work is interesting but it is overloaded with details at the expense of clear physical interpretations. If the authors agree to take the above into account, I do not insist on sending the revised paper back to me.