Nonlin. Processes Geophys. Discuss.,
doi:10.5194/npg-2016-1-RC1, 2016

Nonlinear Processes in Geophysics
Discussions

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

Anonymous Referee #1

Received and published: 16 February 2016

I expected a paper like that a few years earlier, but it happens only now. Sooner or later a semi-analytical baroclinic tidal model for unlimited wave amplitudes should appear. Important is the range of its applicability is wider than just evolutionary stage on free propagating interfacial waves. Unlike the CC theory the presented here model incorporates also the generation stage of internal tides. Ideologically, this approach is similar to Miyata's first theories, but what I can see now is that the model starts with the very beginning of large amplitude internal waves production, when most of the model just fail to work, and I appreciate this fact.

Being a fan of such kind of analytical stuff I just would like to pay some attention to a few specific points that deserve a closer look. Hydrodynamically wise horizontal motions of bottom topography forth and back produce not necessary the same waves as oscillating tidal currents interacting with a motionless sill. Peter Baines did similar

experiments and received some critical feedback on this point, but he had no choice trying to reproduce internal tides in laboratory conditions. The authors acknowledge the fact that moving bottom is not the same as a steering tide, line 45-50. They started Section 3 with this statement (lines 291-294) and admit in lines 299-301 that the result could be different in both cases, e.g. tide moving over motionless topography, or generation of internal waves by moving bottom. The difference does really exist. However, making progress we should accept different approaches, so I do not think there is a great difference between two cases, specifically beyond the bottom topography where the "Galilean transformation" (line 299) can be taken into account. However, I really do not understand the reasoning expressed in lines 338-340 about similarity of two coordinate systems with referencing Fig 2. Maybe it is my problem, but I expect some readers can have the same issue. Can the authors justify their point better?

I would also appreciate some sort of revision that would make the paper more oceanographically oriented. Specifically, the parameters of the topography, tidal flow, rotation, etc., - what specific area of the World Ocean the authors have in their mind? Where the effects like that can happen? In terms of the generation mechanism even the Luzon Strait which generates probably the largest internal solitary waves ever recorded shows nearly linear mechanism of internal tide generation over two sills with the Froude number «1. In light of that, I would appreciate any hint on what area of the World Ocean area is targeted? The parameters are described in Figure 2 (see also lines 355-356, Table 1) with h1=30m, h2=70m, and tidal flow 1.2m/sec. Is there any particular object in the World Ocean which is a prototype of that (has I missed something)?

Mathematical procedures are more or less clear, and I trust the authors applied their expansion procedure correctly; I can not raise a red flag at any point. However, there are still a few minor points. The integration through the layers 1 and 2, eqns (19)-(24) looks fine, but I can not say I fully understand Subsection 2.3. In my opinion it is a bit short in explanation of "6 equations and 11 unknowns" although I accept the expansion with respect to delta (depth/wavelength) does can make sense. Some more

details would be necessary to add for better explanation of integral averaging in line 199, as well.

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