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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 #3

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This paper discusses the derivation and then numerical solutions of a fully-nonlinear, weakly-dispersive model for internal tides and solitary-like waves in two-layer stratifications. The model is an extension of the Miyata-Choi-Camassa theory to include rotation and variable topography. While the effects of rotation have previously been studied, the inclusion of variable topography, and forcing of the internal tide by moving topography is new. The authors find that increasing forcing (measured by the maximum speed of the oscillating topography) leads to a maximum amplitude of the radiated internal tide and that further increasing the forcing results in a reduction in radiated amplitude. This is interesting and counter-intuitive result is attributed to the generation of higher harmonics with increasing forcing. Overall the paper contains useful (e.g. the derivation of the model) and interesting results and will be of some interest to the community. However, there are issues with work as presented that need to be addressed. These

are addressed in the comments below.

1. I am not convinced that the model requires the introduction of a moving topography. The authors claim they need to do this to avoid "nonlinearities in the barotropic flow" (line 46). However, they impose a rigid lid and in doing so they can replace A(t) in their equation (48) with Q(t) and set h_t = 0. (integrate (46) with h_t = 0.) Here Q(t) is a specified, externally imposed barotropic flux. Perhaps this will complicate the equations, but it is possible.

2. In doing what is suggested above, the radiated tides will then be subject to advection by the imposed barotropic flow. This may change the results significantly, especially since they are imposing barotropic flows of order 1m/s in total depths of 100m and the tides and internal waves have speeds of this order. It would certainly call into question the near equivalence of the moving topography and correct barotropic forcing reference frames.

3. The authors discuss a "quasi-nonlinear" version of the model (see line37). However, they never explicitly show the resulting equations, or the precise terms in (41) and (42) that are ignored in this approximation. Further, they never make much of a case as to why one should even explore this aspect. What precisely is learned from this part of the work? How does one connect it to other, mathematically (e.g. asymptotically) consistent models such as the weakly-nonlinear version of (49)-(53) (e.g., the Gerkema and Zimmerman (1995) model). I don't see the value of this part of the analysis.

4. I found the discussion of the numerical experiments very difficult to follow. I was forced to repeatedly go back and forth between Table on and the figures. This was also compounded by the use of dimensional variables. I think that they could simplify the discussion if things are discussed in terms of the governing nondimensional parameters. For example, variations of the reduced gravity g' can be subsumed into a variable relating the timescale of the forcing to the propagation timescale H/c_0, where c_0 is the linear long wave phase speed. There are of course, other choices, but use of

non-dimensional variables should lead to a more compact discussion and comparison of the cases.

5. The authors claim that the appearance of the saturation in the amplitude of the radiated tide with forcing strength is due to emergence of higher harmonics. While this could be true they never demonstrate it. Furthermore, the emergence of higher harmonic is an indication that the radiated internal tide is itself nonlinear. They might consider that the increased nonlinearity of the radiated tide itself is important. For example, Gerkema and Zimmerman (1995) and Li and Farmer (2011, JPO) discuss the role of weakly-nonlinear internal tide solutions as have Helfrich and Grimshaw (2008) for the fully-nonlinear case considered here. To simply say that higher harmonics is the cause of the maximal response seems to miss the deeper issue. Also, they never show that the same maximal amplitude appears in the full set (49)-(53).

6. Figures 8 and 13 should include the dispersion curves from the Miyata-Choi-Camassa model. After all, this paper is supposed to be about the fully nonlinear waves. Also, some (most?) of the disagreement that is found is likely due to the fact that the solitary waves are propagating on a variable background field (the internal tide). This could be accounted for in the comparison. Note that if the barotropic forcing were included as prescribed time-dependent flux Q(t), then the advection of the solitary waves by the changing barotropic flow would be significant since wave speeds are in the range of 1m/s.

7. The sentence starting on line 625 regarding soliton speeds with rotation is misleading. The soliton speeds are only very weak affected by rotation. However rotation has a large effect on the speed of the internal tide from which the solitons emerge and on which they subsequently propagate (cˆ2 = c_0ˆ2 + fˆ2/kˆ2 in the linear limit).

8. Line 637. The authors never showed that the saturation occurs in the full set of euqations, nor did they demonstrate how it affects the resulting soliton amplitudes.

9. I suggest that the authors remove the linear and quasi-nonlinear results and devote

more effort into exploring the behavior of the fully-nonlinear model. After all, “fully nonlinear” is part of the title and the new aspect of the paper. The linear problem has been well covered in the literature and the connection of the “quasi-nonlin ear” reduction with existing weakly nonlinear and now the fully-nonlinear model is not obvious.

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