# Response to REFEREE 3 (Discussion Forum)

## Limiting amplitudes of fully nonlinear interfacial tides and solitons (npg-2016-1)

Borja Aguiar-González[1,2]* and Theo Gerkema[3]

[1]Dpto. de Física, Facultad de Ciencias del Mar, ULPGC, E-35017 Las Palmas, Spain.

[2]NIOZ Royal Netherlands Institute for Sea Research, Department of Ocean Systems Sciences and Utrecht University, P.O. Box 59, 1790 AB Den Burg, Texel, the Netherlands.

[3]NIOZ Royal Netherlands Institute for Sea Research, Department of Estuarine and Delta Systems, and Utrecht University, P.O. Box 140, 4400 AC Yerseke, the Netherlands.

## Anonymous Referee #3

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.

As the reviewer already indicates, introducing a barotropic flow in this setting complicates the equations; indeed, the barotropic flow itself would become part of the problem. The point is that one cannot impose a simple barotropic flow in a way that is consistent

*aguiar@nioz.nl

with the fully nonlinear equations; a barotropic flow would here involve higher harmonics, generated by advective terms like $UU_x$ ($U$ the barotropic flow). In other words, one would actually have to *solve* the barotropic flow from the fully nonlinear equations. Since we are not primarily interested in any intricacies of the barotropic flow, the easier road is here to avoid the problem altogether and prescribe an oscillating topography.

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.

We understand the referee's initial concern in this regard; nevertheless, we consider we have been conservative enough to restrict our study to a parameter space where a semi-equivalence between two different generation models has been tested on the generation of the linear and quasi-linear internal tides (Fig. 2 of the submitted manuscript in the 'Discussion Forum'). If a significant departure between the mimicked tidal flow and the use of an actual tidal flow would exist, it should be then noticeable in the above model-comparison, especially over the top of the oscillating topography; but this was not the case. Far from the sill, the bottom is flat and at rest so it is not expected that the 'non-inertial' frame causes any artifact once the internal tide has been generated and propagates.

Some of the main findings of our study are that quasi-linear tides become saturated as the tidal forcing is increased and that, consequently, leading solitons of the disintegrated internal tides may be also subjected to a limiting amplitude besides that predicted by eKdV and MCC theories. For completeness, and as a double-check, we have tested these findings with the weakly nonlinear model derived in Gerkema (1996), which works with an actual tidal flow over topography. We don't show in this document the full analyses but just a hint of each of them.

Fig. I shows the amplitude saturation of quasi-linear internal tides as the tidal forcing, $c_T$, is increased. As the flow becomes supercritical[1](Fr>1), a further increase of $c_T$ does not generate larger internal tides. This agrees well with our findings from the quasi-linearized version of the forced-MCC equations.

In Fig. II we solve the full set of weakly nonlinear equations derived in Gerkema (1996). Results show how the saturation amplitude of the quasi-linear internal tide, as shown in Fig. I, affects the growth of the leading solitons by also limiting its maximum am-

[1] To characterize the hydraulic state where internal waves propagate we use the Froude number calculated as $Fr = \frac{c_T}{c_p}$, where the strength of the mimicked tidal flow acting as external forcing, $c_T$, is confronted to the linear long-wave phase speed for interfacial waves, $c_p$.

plitude. The Gerkema (1996) model is built around the weakly nonlinear framework of the classical KdV theory and Klein-Gordon equations, where the amplitude saturation of solitons does not occur. However, Fig. II shows that tide-generated solitons exhibit a limiting amplitude even in the weakly nonlinear regime. Noting this it seems reasonable to argue that the limiting factor is then related to the addition of a tidal forcing.

The above results give support to conclude that findings from the forced-MCC-$f$ equations do not lie on an artifact of the oscillating topography and represent an insightful extension to the fully nonlinear frame of work where tide-generated solitons may attain limiting amplitudes even without reaching a 'table-top' shape, then also subjected to a saturation amplitude of the underlying internal tide prior to its disintegration into solitary waves.

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.

In a revised version we will explain more explicitly the distinction between the different set of equations and show in an additional Appendix how the (quasi)-linearization of the forced-MCC-$f$ equations was performed. Also, we will re-name the 'quasi-nonlinear case' as 'quasi-linear case' because we understand that the former name has led to confusion when we discussed the physical interpretations and findings.

For both the forced-MCC-$f$ and Gerkema (1996) models, the quasi-linear case involves neglecting the baroclinic interactions but retaining the nonlinear terms involving a combination of barotropic and baroclinic fields. The equations are then still linear with regard to the baroclinic fields, but the coefficients become time-dependent due to barotropic factors (which are prescribed), so that higher harmonics will be generated. Hence one should not expect the quasi-linear case to be close to the eKdV case on showing saturated interfacial waves.

The above feature is argued in our study to be the most likely factor limiting the growth of leading solitons from the already limited quasi-linear internal tide (besides the soliton saturation predicted by eKdV and MCC theories). This finding is the reason why we find insightful and valuable to start our study on fully nonlinear tide-generated solitons from the generation of the internal tide by which the formers will raise. In a revised version we will make this point more clear as we consider crucial to keep the analyses on the quasi-linear internal tides.

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 $\mathrm{H}/\mathrm{c}_0$, where $\mathrm{c}_0$ 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.

In a revised version the discussion of the results will be held using the governing nondimensional variables and the table listing the runs will be presented in a more clear and simplified manner.

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).

We actually think we conclusively demonstrated that the saturation of the amplitude is related to the generation of higher harmonics; this is the very reason why we considered the quasi-linear case in detail. After all, the presence of higher harmonics is the *only* difference between the linear and quasi-linear cases. In the purely linear case, obviously, the solution grows linearly with the forcing. But as the results in Fig. 3 (of the submitted manuscript) show, the quasi-linear case follows the linear growth as long as the barotropic currents are weak, while deviations occur for stronger currents, and then the amplitude becomes saturated. We cannot see any other connection than with the higher harmonics.

6. Figures 8 and 13 should include the dispersion curves from the Miyata-ChoiCamassa 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.

We agree. In a revised version the MCC analytical solutions will be included for discussion and comparison with the forced-MCC numerical solutions. Also, we will account for the suggestion made by the referee about the effect of the solitary waves being embedded on a variable background flow, an argument we agree with.

Additionally, in a revised version we will use the Froude number, as define above in item (2), to account for the importance of advection by the changing barotropic flow.

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 ($c^2 = c_0^2 + f^2/k^2$ in the linear limit.

We agree on this important remark that will be corrected in a revised version.

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

Regarding the demonstration of the limiting amplitudes by higher harmonics, we already provided an answer in item (5). About how this amplitude saturation affects the resulting soliton amplitudes, we believe that we have shown numerical solutions doing so. This is described on the basis of presented results, for instance, in lines 578-580 and lines 606-612 of the manuscript submitted to the 'Discussion Forum'. And it is further discussed later in lines 659-670 (Sect. 5. Summary and conclusions) within the scope of main results.

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-nonlinear' reduction with existing weakly nonlinear and now the fully-nonlinear model is not obvious.

Following the referee's request we will devote more effort in a revised version to explore the physical interpretations of the fully nonlinear model by discussing the results using the governing nondimensional parameters (as it was suggested in item (4)).

We understand that in the submitted version to the 'Discussion Forum' it was not clearly explained how the quasi-linearization of the model equations was performed and how that version differs from a weakly nonlinear set of equations. This obviously led to miss an important point of our discussion. This is a topic which we further discussed and answered above in item (3). In a revised version we will make a more clear distinction between the different set of equations.

Regarding the purely linear case, we note this is well covered in the literature. In a revised version we will make more clear that our aim on showing the linear results is only to highlight its departure with the quasi-linear case. It is the latter which presents a new and relevant feature that we investigate, i. e. the saturation amplitude of internal tides subjected to the forcing.

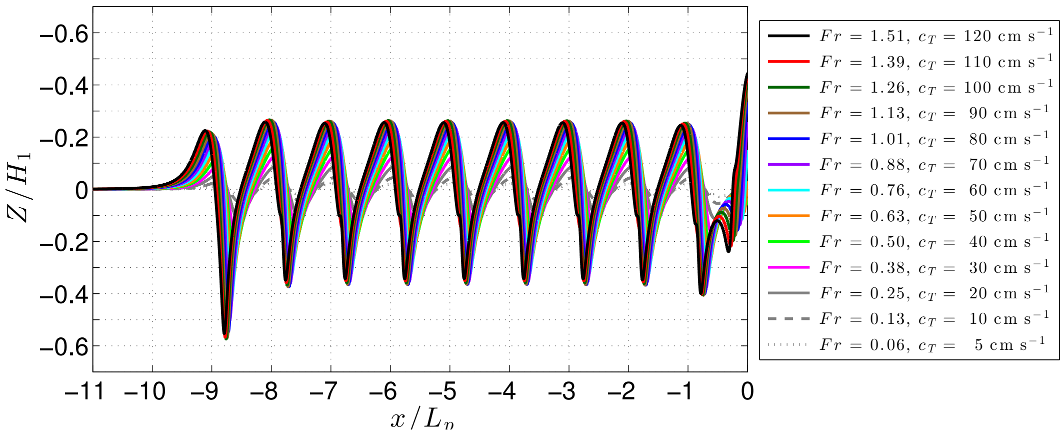


Figure I: Snapshots of the interfacial displacement of leftward propagating quasi-linear internal tides for run A1 ($H_1 = 30$ m; $L_p = 35.49$ km). The amplitude saturation is evident as the tidal forcing is increased and the flow becomes supercritical, $Fr > 1$ (see legend). The run time is 9 tidal periods. The model equations used here are a quasi-linearized version of the weakly nonlinear model in Gerkema (1996).

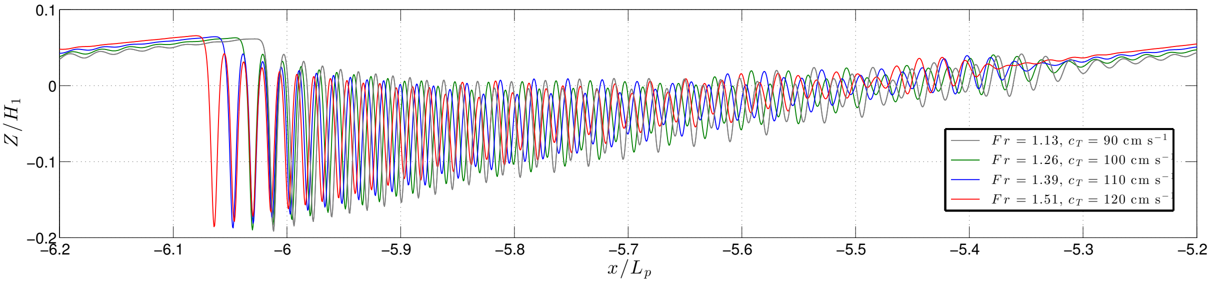


Figure II: Snapshots of the interfacial displacement of leftward propagating weakly nonlinear internal tides and solitons for run A1 ($H_1 = 30$ m; $L_p = 35.49$ km). The limiting amplitude (which is here non 'table-top' shaped) is evident as the tidal forcing is increased but the soliton amplitude becomes saturated. The run time is 9 tidal periods. These waves are generated from the weakly nonlinear generation model derived in Gerkema (1996).