

# Response to Referee 1 (Discussion Forum)

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

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### Anonymous Referee #1

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?

We agree that our reasoning may be too brief and, therefore, unclear. In a revised version, the following lines would be included in section 3.

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First, lines 310-312 would read:

*‘At this point we recall that the oscillation of the topography is included within the forced-MCC equations, in dimensionless form, as  $h = h(X)$ , with  $X(x, t) = x - a \cos t$ . The constant  $a$  prescribes the strength (speed amplitude) of the oscillating topography via  $U = a \sin(t)$ , the mimicked barotropic tidal flow (see (43)-(47)). For convenience in later discussions, we introduce here  $c_T$  to refer the (dimensional) speed of the oscillating topography, which follows from  $c_T = c_o a$ .*

Later in the text, lines 338-340 would read:

*‘In Fig.2, interfacial waves generated from both models are presented for various numerical experiments which differ in the strength of stratification under a fairly strong barotropic tidal flow (Gerkema 1996: gray line) and oscillating topography (forced-MCC: black line). Results over the top of the sill indicate a close correspondence between numerical solutions from Gerkema (1996) and the forced-MCC equations, suggesting only a minor impact of the non-inertial nature of our frame of reference within the parameter space of this study. These results encourage us to refer hereafter the strength (speed) of the oscillating topography as the ‘strength of the tidal flow’*

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  $h_1=30\text{m}$ ,  $h_2=70\text{m}$ , and tidal flow  $1.2\text{m/sec}$ . Is there any particular object in the World Ocean which is a prototype of that (has I missed something)?

We appreciate the interest of the reviewer in knowing whether the present results are applicable to observations in a specific region of the ocean. We have tried to find observational material to compare our findings with, but the difficulty lies in what is actually the strength of the model, namely that it covers all stages, from the creation of the internal waves over topography to the development of the solitons. The problem then is to find observational data on all these stages. We found some on table-top solitons but without the specifics of the source. We would like to continue working on this line and would appreciate it if the reviewer could suggest helpful references. For this paper, we focus on two main goals: first, to present the derivation of a new two-fluid layer model which extends MCC equations with forcing terms and Coriolis effects; and second, to use this novel fully nonlinear model to provide an overview, as generally as possible, on the conditions by which tide-generated interfacial waves may exhibit limiting amplitudes. In line with these two goals, we would add in a revised version the following text in section 5:

*‘Whilst not designed to represent a specific region of the ocean, the numerical experiments presented here allow us to investigate for the first time the conditions by which tide-generated interfacial waves may exhibit limiting amplitudes in ocean-like scenarios. With this aim we adopt a two-fluid layer system where the parameters span a broad range of values in order to make clear the qualitative features of these nonlinear processes (Table 1).*

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.

We will work on this section and add explanatory comments to clarify the text where necessary.

About the integral averaging in line 199, at the lowest order ( $\delta^0$ ) we are in the hydrostatic regime and horizontal velocities are independent of  $z$  within each layer so that  $u_i^{(0)} = \bar{u}_i^{(0)}$

$$\begin{aligned}
 \overline{u_i u_i} &= \frac{1}{\eta_i} \int dz u_i^2 &= \frac{1}{\eta_i} \int dz (u_i^{(0)} + \delta u_i^{(1)} + \dots)^2 \\
 & &= \frac{1}{\eta_i} \int dz (u_i^{(0)2} + 2\delta u_i^{(0)} u_i^{(1)} + \dots) \\
 & &= u_i^{(0)2} + O(\delta) \\
 & &= \bar{u}_i^2 + O(\delta)
 \end{aligned} \tag{1}$$

so that

$$\overline{u_i u_i} = \bar{u}_i^2 + O(\delta), \quad \overline{u_i v_i} = \bar{u}_i \bar{v}_i + O(\delta).$$