Response to Referee # 2 comments

Kieran O'Driscoll would like to thank the reviewer for their substantial and considerate review.

Based on the general comments of Reviewer 2 (and also those of Reviewer 1):

I have a number of problems with the paper and think that is requires significant revision. My basic problem is that there doesn't appear to be much that is new here other than the application to the CMO site in the Middle Atlantic Bight and that is quite a small part of the paper.

The paper needs considerable polishing. Figures are in some cases hard to read and many dimensional values are given without units. It would probably benefit from being shortened and more focussed on the comparisons with observations however I am not convinced of the value of these simulations in that context. The authors make some comparisons of their results with those of Holloway et al from 30 years ago. Recent work has been done in this area using model equations that include rotation (e.g., Grimshaw and co-workers). The authors need to make a compelling argument for this set of simulations.

Response: Done. The article has been shortened by removing Sections 2 (Theoretical Background), 3.1.1. (Two-layer model level bottom), and Figs. 1 – 6.

Comments

1. The title highlights KdV solutions with no mention of the eKdV solutions. I think it is well established by now that cubic nonlinearity is necessary to adequately model many observed solitary waves in the ocean, so if anything the eKdV equation should be mentioned in the title. Indeed, one wonders what the benefit of even considering the KdV equation is. Comparisons of the predictions of the KdV and eKdV (or Gardner) equations, as well as the RLW equation, with fully-nonlinear numerical simulations for a two-layer stratification are discussed in Lamb and Xiao (Ocean Modelling, 2014). This seems like a relevant reference.

Done, thanks.

2. Why are rotational effects not considered? The site of the observations is at mid-latitutude where rotation is going to affect the evolution of the internal tide and the amount of energy that ultimately gets transferred to ISWs. For example in Figure 10 the linear long wave propagation speed is about 0.5 m/s so waves take about 50 hours to travel 100 km. That is lots of time for rotation to affect their evolution.

The model is two-dimensional, so the waves propagate in the horizontal x-direction only.

3. First paragraph of page 4. Nonlinear effects can become important even without shoaling, as illustrated by the authors own flat-bottomed simulations so this should be reworded.

Done: Section 2 (Theoretical Background) has been removed

4. Page 5, lines 6–8. “It was originally developed in the context of internal waves by Benney ....”

Done: Section 2 (Theoretical Background) has been removed

5. Equations 10(b) and 10(c) are both incorrect.

Done: Section 2 (Theoretical Background) has been removed

6. Page 10. The introduction to section 3 repeats material from the introduction so should be deleted.

Done

7. Page 12, line 15: For a given water depth and wave amplitude cubic ...”. Then on lines 16–18, whether or not the eKdV model is similar to the KdV model depends a lot on the wave amplitude. For a two layer stratification, whenever the interface gets displaced close to the mid-depth cubic nonlinearity becomes important (though if $h1/h2 << 1$ higher-order nonlinear may be needed).

Done: Section 3.1.1 (Two-layer model level bottom) has been removed

8. Section 3.1.1. The cases explored in this section are not well explained. All four cases have different total depths $H = h1 + h2$ and different depth ratios $h1/h2$ while from what I can understand the initial wave amplitude is the same in all cases. So both the depth ratio $h1/h2$ and the initial nonlinearity have been changed. Comparing these cases is then a bit problematic, particularly with statements to the effect that you expect one case to be more nonlinear than the other. Also, throughout ratios such as α/c and β/c are given without units. These ratios are not dimensionless. The KdV and eKdV equation have been used a lot to model internal solitary waves in the ocean. What have we learned from this set of simulations?

Done: Section 3.1.1 (Two-layer model level bottom) has been removed

9. Section 3.1.2. What is new here? The general picture of the evolution of a shoaling internal tide has already been well described. What is the new contribution from this section?

These simulations studied the development of evolving internal tide as a packet or developing wave train across the linear sloping bottom, whereas most other studies have inspected the development and advance of a single soliton across similar bottom slope.

10. Page 17, Line 5: there are no higher-order terms to prevent the development of solitary waves in the models used here.

Sentence has been removed.

11. Page 18, lines 14–15: What do you mean by 'We expect the waves to become unstable"? Do you mean your numerical solution is unstable? If so should a smaller time step be used? If a physical instability what type of instability is referred to?

No, physically unstable, ie., Kelvin-Helmholtz instability or billows.

Done, thanks.

12. Page 19, line 14: Do you mean the CMO line will be horizontal — lots of straight lines don't have constant h1.

Yes, thanks. I will add Fig S1 (to replace old Fig 2) which shows values of h1, h2 for CMO.

13. Page 21, lines 9–10. α/α_1 is not a dimensionless parameter.

Done, thanks.

14. Page 22, lines 3–8. Why is α_1 so much greater at the CMO site than in case A? Is it because h1 is so much less?

Yes, thanks. Mostly due to h1 half the value of h2. Done.

15. Page 25, line 15. Do you mean figure 16c?

Yes, thanks.

16. Page 26, 2nd paragraph. Something else that could be going on is the nonlinear evolution of inertia-gravity waves that form behind internal solitary waves due to rotation. See Grimshaw et al, JPO, 2014 or Lamb and Warn-Varnas, NPG, 2015. What about multiple packets forming each tidal period because of different generation mechanisms or multiple tidal constituents?

Thanks, done. References to these papers and alternate generation and evolution processes included.

17. Page 29, lines 8–9. The internal tide is nonlinear right from the beginning — it doesn't become nonlinear sooner as β is reduced. As β is reduced waves have to get narrower before dispersive effects become significant.

Yes, thanks. Done. Changed accordingly.

18. Page 29, line 16. I think you mean if this ratio is much larger than one

Yes, thanks. Done. Edited accordingly.

19. Page 30, line 8. What do you mean by 'the internal tide was forced with a sech2 wave. Don't you mean the simulation was initialized with a sech2 wave?

No. It is forced, since it takes a tidal period for the wave to propagate into the model domain, i.e. the sech2 wave has tidal period.

20. Figures. In general I find the font size too small in most of the figures – it is difficult to read them. In the caption for Figure 7 panels (a), (b) and (c) referred to in the text are not labeled. Figure 11 is of particularly poor quality

Done. Figures with problem font sizes have been increased in size (because of removal of Figs. 1-6). Fig. 7 relabelled, Fig. 11 removed.