Review of

“Simulations and observation of nonlinear internal waves on the continental shelf: Korteweg-de Vries and extended Korteweg-de Vries solutions”

by K. O’Driscoll and M. Levine

The author’s justification for not considering rotational effects is not correct and is inadequate. A two-dimensional model just means the fields are functions of two spatial variables, say x and z. There can still be flow in the third (lateral) direction. It is well known that rotation does affect internal solitary waves on time scales of the order of an inertial period. In particular energy is radiated behind the internal solitary wave in the form of long inertia-gravity waves. Given enough time the leading ISW can disappear and the trailing IG waves can steepen and form new ISWs. See, for example, Grimshaw et al (2014) or Lamb & Warn-Varnas (2015). In addition rotation can inhibit an internal tide from steepening and forming ISWs (e.g., Helfrich & Grimshaw 2008). So this begs the question of why the effects of rotation were not considered when making comparisons with the CMO data. Why isn’t the Ostrovsky equation being used?

As a previous reviewer pointed out, refering to large amplitude ISW solutions of the eKdV equation as “tanh” waves is not recommended. There is an analytical expression for these waves in terms of tanh functions however the waves only look like a tanh function when they are very long with a flat-crest and none of the waves in this paper are remotely close to this shape. For this reason an alternative form of the solitary wave solutions, written in terms of a sech function, is usually used.

I think the paper needs considerable revision. The shoaling case with the sloping pycnocline is interesting and the comparisons with CMO observations is a useful addition to the literature however rotational effects should be included or a justification for not including them should be provided. An estimate could be made of the distance over which rotation would affect the waves.

1. Page 4, line 1. This sentence implies that the nonlinear terms are not important before the waves shoal which is not true
2. Line 5, line 16: “For the model cases .....” makes no sence
3. Page 5 equations 1 and 2. These equations don’t fit in the sentence properly. Also, I suggest just giving the eKdV equation and saying that when $\alpha_1 = 0$ the equation reduced to the KdV equation.
4. Page 5, lines 20–21: “Nonlinear transformation of the internal tide leads to the generation of nonlinear waves”. This doesn’t make sense. You are saying a nonlinear wave leads to the generation of nonlinear waves. You mean

something like “Nonlinear steepening of the internal tide leads to the generation of a packet of short nonlinear waves which ...”. On a similar vein, on page 8, lines 9 it is stated that ’the internal tide steepens and rapidly becomes nonlinear’. The internal tide is nonlinear from the start — otherwise it wouldn’t be steepening! Following this what is meant by “the internal tide becomes more nonlinear”? What is the measure of the nonlinearity?

5. Page 6, lines 19–20. This sentence is not very clear. Perhaps “... we investigate two cases with a constant sloping bottom, one with a horizontal interface and one with a sloping interface, ...”

6. Page 7. Equation (4). Here a shoaling term that is not present in equations (1) and (2) is included. The first two equations should include the shoaling term. The shoaling term is not ever mentioned. I also suggest giving expressions for the nonlinear and dispersive parameters to readers can see, for example, how α changes sign, $\alpha_1 \neq 0$ and that $\beta > 0$.

7. Page 7. Please mention the numerical method used to solve the equations.

8. Page 7 line 15: “We chose *the* starting layer thickness*es* at $l = 0$ *to be* ... with *a* bottom ...’

9. Page 7, line 18: “ ... at *a* water depth ...”

10. Page 9, line 3: Please define what is meant by the leading face? Since the internal tide is sinuosoidal this is ambiguous. Do you mean the front of the crest or the front of the trough?

11. Page 9, line 6: Do you mean the slope decreases rapidly? Above you say the leading face steepens but the steepening slows down, then you say the rate of change of the slope changes sign? I find this confusing.

12. Page 9, line 7. Where is the second shock-like front?

13. Page 9, lines 10–13. I don’t see what the difference in the magnitudes of the nonlinear and dispersive terms has to do with the phase speed of the waves. Aren’t waves propagating with phase speed less than c simply because they are in the wave trough where $\eta < 0$? In the following paragraph mention of the wave alternating between being more nonlinear and dispersive is made. This does not make sense to me and this whole paragraph seems unhelpful. The nonlinear and dispersive terms vary across a given wave, changing sign at different locations in some instances. It does not seem helpful to say that at one value of x where $\eta_{xxx} = 0$ and $\eta\eta_x \neq 0$ the wave is more nonlinear than

dispersive. The wave should be viewed as a whole. I trecommend removing this whole discussion along with Figures 2(a) and 4 and accompanying discussion.

14. Page 10, lines 5: The KdV or eKdV equation are not subject to Kelvin-Helmholtz instabilities.

15. Page 11, line 10: what is the 'back-face' of the wave? The back of the crest or the back of the trough?

16. Page 11, lines 12–13. "the number of solitary-like waves seems to have been reduced to the leading two waves". On what basis were some waves judged to be solitary-like and other waves not?

17. Page 11, lines 19–20: The instability must have something to do with the numerical scheme. The dispersion parameter may get small. It is never 0.

18. Page 12, lines 4. Here χ is defined. If use of χ is retained it should be defined when first used.

19. It would be useful to plot values of α_1 when solutions of the eKdV equation are being discussed.

20. Page 19, line 9: When you say 'using CMO site parameters' are you using a continuous stratification? How do the parameters for the observed (i.e., continuous) stratification compare with those for the two-layer approximation? I am not convinced that a two-layer approximation is appropriate even if they waves are mode-one. A figure showing the observed stratification should be provided and justification for the choice $h_1 = 25$ m should be given.

21. Figures: The figures should be improved. For example in Figure 1(a) the ticks along the x-axis should be the same in all panels like they are in figure 2(a) (and I think ticks at 20 km intervals is better than at 30 km intervals). Many figures are of poor quality but perhaps I got low resolution versions of them? In Figure 7 it is hard to distinguish the two curves in the upper left panel. In this figure intervals of 11 along the bottom axis is odd. How about going by 10?