Review of: **The Fully Nonlinear Stratified Geostrophic Adjustment Problem**

*by A. Coutino and M. Stastna*

The writing and presentation has improved on this resubmission but, unfortunately, the depth of analysis has not. Results of numerical simulations of mass adjustment for the classic dam-break problem with nonlinear high-resolution 2D numerical simulations are presented, with a focus on the dispersion characteristics of the radiating wavepacket, on the impact of the initial disturbance polarity and the role of the Rossby number. The authors are applied mathematicians, yet only 4 equations appear in the paper: the equations of motion and a KdV equation. In fact, it is not clear why the latter equation is even included since it is not solved nor used at all in the manuscript. Comparisons with previous work are confined to very broad generalizations. Comparison of simulations in different regimes are equally qualitative. We are not told why Ro=1 represents a transition in behavior. What is the impact of the Reynolds number? Why not mention the Froude number? The analysis remains primarily confined to looking at kinetic energy Hövmöller plots and isopycnal displacements. Numerical simulations can be a powerful tool when used in conjunction with some theory, but here no hint of theory is presented. What have the authors learned from this study? How does energy in the geostrophic state and radiating waves depend on the Rossby number or the nonlinearity parameter? What is the role of the Reynolds number? Why include an entire table of Reynolds numbers when the impact of dissipation is not ever discussed? In my first review, I mentioned that the authors might compare their results with Lelong and Sundermeyer (JPO 2005). Instead, they concentrated on Sundermeyer and Lelong (JPO 2005) which, I agree, has no bearing on the current study. I am sorry that I cannot recommend publication of this manuscript in its present incarnation.

**Comments:**

1. Abstract, line 7: How do variations in the Rossby number *demonstrate* the presence of two wave trains?

2. Page 3, line 32: Since the set-up is on experimental scales, the *flat ocean bottom* should be *flat bottom*.

3. Page 4, As in the previous version, the definition of $\rho$ on line 7 is still not consistent with the expression for $\rho$ given on Page 6 (top of page).

4. Page 4, line 15:

5. Page 4, top of the page, equations 1-3: These are the Navier-Stokes equations, not the Euler equations which are, by definition, inviscid.

6. Page 4, line 15: Why not state Lamb's equation 14 explicitly? Nowhere in the manuscript does an energy equation appear. Which terms are dominant in the different regimes?

7. Page 7, paragraph starting on line 5: It not clear why the paragraph on the KdV theory is included since no explicit comparison with numerical results is made. You are solving the NS equations, not the KdV or DJL equation. Do your numerical results match the solutions of either of these equations? In Eq (4), what is B?

8. Page 8: Please state the polarity of your base case.

9. Page 9, lines 10-11: Those 2 sentences can be combined: Figure 4(a) corresponds to the base case (Ro=1/2), 4(b) to $f = f_0/2$ (Ro=1) and 4(c) to $f = f_0/4$ (Ro=2).

10. Page 9, Sentence starting on line 19 does not make sense.

11. Page 9, lines 28-30 run-on sentence.

12. Page 11, lines 8-9: '... *since the potential energy may be zero since it can reach its initial starting point*'. Do you mean to say that $\Delta PE$ can be 0? What's wrong with that? The difficulty in computing $\Delta PE/\Delta KE$ may come from $\Delta KE$ vanishing at $t = 0$ but typically, one is interested in the value of this ratio at large time.

13. and so on....