A second review of
'The fully nonlinear stratified geostrophic adjustment problem',
by A. Coutino and M. Stastna

General comments

As for the original version, this paper looks at the process of geostrophic adjustment in a stratified fluid, which is an important problem in atmosphere-ocean fluid dynamics. The study is based upon high-resolution fully nonlinear simulations of almost two-layer flow in a rotating tank at high Reynolds number, for a range of different parameters (Rossby number, width and height of initial disturbance), with the particular geometry being motivated by recent experimental results obtained at Grenoble. The nature of the ejected (nonlinear) waves and the remaining geostrophic state are discussed, along with their dependence upon the initial amplitude and polarity of the disturbance.

As for the original version, there are several good things about this article. It appears to fill in a hole in the literature for high-resolution (up to 16384 $\times$ 192) numerical simulations of nonhydrostatic nonlinear adjustment in 2D (which has otherwise been extensively studied analytically), and makes a direct link to recent experimental results. The paper thus provides a useful catalogue of results, which can be used by theoreticians to test asymptotic analyses, and which supplement (imperfect or incomplete) observations from laboratory experiments. So the results are new and significant in this sense, with the numerical simulations being of an international standard. The manuscript is well-structured, of an appropriate length, with well-prepared figures. The Abstract and Introduction are well written, and would be understandable to a wide audience.

This is a considerable improvement over the original version, with most of the original shortcomings rectified, and with a useful new section on nonlinearity and polarity (although the text there could be polished up). I also (still) wonder how much of the behaviour in sections 3.2–3.4 could be understood in broad terms using linear theory, since it has been shown (in section 3.5, e.g., Figures 11(b,c)) that the leading-order behaviour in certain cases is approximately linear. The paper would certainly be better and offer more insight if some simple theory (just based on comparisons with the linear dispersion relation for inertia-gravity waves) was deployed. One might argue that the main emphasis here is simply on presenting the novel numerical results and setting them in the context of previous studies, but a really good paper should also include appropriate theoretical explanations.

I think that the manuscript will be suitable for publication in NPG after some more minor revisions, mostly relating to the clarity of the text.

Specific comments and technical corrections

1. p.1, lines 15-22. It might be helpful to say that the 'linear problem' was first considered by Rossby, since it's not obvious until line 22 that you are indeed first talking about 'this linear problem'.

2. p.3, line 22: we don't yet know the tank geometry, so the significance of a 'leftward wave' is lost.

3. p.3, line 26: probably best to avoid the use of the word 'reflect' here – maybe reserve it for wave reflection? Perhaps '..expected to evolve according to linear theory'?

4. p.4, line 21: f is not the rotation rate, but either twice the rotation rate, or the Coriolis parameter.

5. p.5, line 2: presumably the physical experiments were by Grimshaw et al. (2013), not Grimshaw and Helfrich (2008)?

6. p.5, line 3: presumably 'four times longer', rather than 'four times larger'?

7. p.5, line 6: 'the density difference was set to 1%' would belong better at the top of p.6, where the form of the initial density is first discussed.

8. p.5: presumably the physical experiments had a free surface, whereas you have a rigid lid? This could be clarified.

9. p.4, 6: on p.4 you say that $\alpha = \eta/H_1$, where H_1 is the height of the undisturbed fluid column – although, according to figure 1, H_1 is the depth of the lower layer, or height of the undisturbed interface. Then, when defining α on around line 13 p.6, you seem to confuse H_0 with H_1. This should all be clarified.

10. p.6, line 10: $1e^{-6}$ is a bit careless (and incorrect as written, I think).

11. p.6, line 24: is this scaled by the maximum kinetic energy (in space) at fixed t, or over all space and time? Is this just for certain figures?

12. p.7, line 5: The → the.

13. p.7, line 11: maybe 'dispersion coefficient r_{01} ... and nonlinear coefficient r_{10}'?

14. p.8, start of section 3.2: should it be clear that we are now looking at negative polarity cases?

15. p.9, line 20: this sentence needs to be rewritten (unclear, and grammatically incorrect).

16. p.10, line 11: 'as we increase the *initial width*, the shape of...' would be better (so we don't have to translate 'width of the initialization' to the previously established terminology of 'initial width').

17. p.10, 11: as in the original manuscript, it's still hard to see wave emission in fig 6(e), and to a lesser extent in fig 6(d).

18. p.11, line 6: presumably the *extent* or *location* of the geostrophic region is defined as given.

19. p.11, line 7/8: 'we cannote compute $\Delta PE/\Delta KE$ since the potential energy may be zero since it can reach its initial starting point': this needs a rewrite to be clear (and correct)!

20. p.11, line 11: 'quick decay' is an odd term for something that is not tending to zero. Maybe 'rapid equilibration' would be better? Same issue on line 1 of p.12.

21. p.12, caption to Fig.8: should be 'scaled by the..'.

22. p.12, line 4: patters → patterns.

23. p.13, line 6: obvious pair of typos with superscripts to be corrected.

24. section 3.3 (and perhaps 3.2 and 3.4, too): no simple arguments are given for any of this behaviour – surely something useful could be said based on elementary theory? For example, on line p.3 of p.14, can the 'spreading of the wave packet' be interpreted in terms of the linear dispersion relation for rotating shallow-water waves (i.e., does $\partial^2\omega/\partial k^2$ increase as Ro decreases)? On p.15, you do state that the wave packet behaviour 'to leading order can be understood from the point of view of linear dispersive wave theory' – so surely you should be using this to explain the behaviour?

25. p.16, caption to Fig 10: 'above $Ro = 1.25$ case' and 'below $Ro = 0.75$ case' are misleading – better just to write $Ro = 1.25$ and $Ro = 0.75$.

26. p.14, line 4: 'crossing from below to above one Rossby number..' needs to be rewritten to be clear (and correct)!

27. The text in the new section 3.5 was hard (but not impossible) to understand, because it hadn't been written (or edited) with sufficient care. Some specific points:

    (a) p.15, line 10: 'reflecting the fact that' → 'since'?

    (b) p.16, line 7: an ambiguous sentence. 'In contrast to what was observed before' (presumably you are talking about fig 11(b)?), 'for the lower rotation case' (presumable you mean 'for this lower rotation case')?

    (c) p.16, line 15: 'Panel (a) clearly shows the energy difference that was seen in Fig.11(b)' – misleading, since it sounds like this is a different view of the same data as Fig.11(b) (which was at different parameters). You mean something like the 'same kind of energy difference between positive and negative polarity cases that was also seen in Fig.11(b).'.

    (d) caption to Figure 12: it's simply incorrect that the figure shows negative (left column) and positive (right column) – this is only in panels (c,d).

    (e) p.17, line 5: should be Figure 12(b), not 11(b).

    (f) caption to Figure 14: perhaps rewrite 'wave of elevation' and 'wave of depression' in terms of polarity (as elsewhere in text).

28. Will the URLs be removed from the final reference list?

29. caption to Table 1: obvious typo with superscript.