Response to reviewers

We have revised the manuscript in its entirety, focusing on the aspects that differ from the existing literature, and in particular the nonlinear aspects of the problem. We have expanded the theoretical discussion in a number of places, including a comparison with the predictions of linear dispersive wave theory. The two sets of reviews have been divergent from the beginning, and we have endeavoured to balance the point of view we have, with those of the two reviewers. As an example, while Reviewer 2 finds no value in the discussion of the KdV equation, the discussion of the role of polarity was included as a response to an earlier comment by Reviewer 1 which (in our opinion) has improved the stand alone nature of the presentation. Detailed responses are provided below with our responses in bold.

Reviewer 1

We appreciate the many constructive suggestions the Reviewer makes and have included the results of linear theory earlier on in the discussion. We have attempted to focus the discussion on the nonlinear aspects of the behaviour (which are surprisingly varied), building on a polished (hopefully) version of the section on nonlinearity and polarity. Detailed responses can be found below (though we note that much of the text has changed).

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 x 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 po-

larity (although the text there could be polished up). I also (still) wonder how much of the behaviour in sections 3.23.4 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.

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

2. p.3, line 22: we dont yet know the tank geometry, so the significance of a leftward wave is lost.
   The wording has been changed.

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?
   The wording has been changed.

4. p.4, line 21: f is not the rotation rate, but either twice the rotation rate, or the Coriolis parameter.
   This has been fixed throughout the manuscript.

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

6. p.5, line 3: presumably four times longer, rather than four times larger?
   The wording has been changed.

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.
   This has been moved.

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

9. p.4, 6: on p.4 you say that , 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 on around line

13 p.6, you seem to confuse H_0 with H_1. This should all be clarified.
This has been corrected throughout the section.

10. p.6, line 10: 1e6 is a bit careless (and incorrect as written, I think).
This has been fixed.

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?
This has been more explicitly stated.

12. p.7, line 5: The→the.
This has been changed.

13. p.7, line 11: maybe dispersion coefficient r_{01} ... and nonlinear coefficient r_{10}?
This has been changed.

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

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

16. p.10, line 11: as we increase the initial width, the shape of... would be better (so we dont have to translate width of the initialization to the previously established terminology of initial width).
This has been corrected.

17. p.10, 11: as in the original manuscript, its still hard to see wave emission in fig 6(e), and to a lesser extent in fig 6(d).
This figure is no longer included.

18. p.11, line 6: presumably the extent or location of the geostrophic region is defined as given.
The calculation of the geostrophic region has been more explicitly stated.

19. p.11, line 7/8: we cannot 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)!
This has been corrected.

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.
The wording has been changed.

21. p.12, caption to Fig.8: should be scaled by the...
The wording has been changed.

22. p.12, line 4: patters→patterns.
The wording has been changed.

23. p.13, line 6: obvious pair of typos with superscripts to be corrected.
This has been 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?
More linear theory has been added to frame the discussion of the nonlinear effects. Furthermore we have added lines indicating the theoretical wave front location according to linear theory to the new figure 3. We have also expanded the theoretical discussion to include rotating theory as well, so as to provide a link with the well studied Klein Gordon equation.

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.
This section has been removed.

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

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: its 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).
(a) **This has been changed.**
(b) **The sentence has been changed.**
(c) **The wording has been changed.**
(d) **This has been changed.**
(e) **This has been changed.**
(f) **The wording has been changed.**

28. Will the URLs be removed from the final reference list?
Unfortunately, this appears to be due to the NPG bibliography style, note of this will be sent with submission.

29. caption to Table 1: obvious typo with superscript.
This has been fixed.

Reviewer 2

We appreciate the reviewer's time, and many of the suggestions provided are excellent. We certainly apologize for the transposition of the author list that led to the absence of the discussion of Lelong and Sundermeyer (JPO 2005), and have included extensive discussion of this paper in the revisions. We have focussed the revised manuscript on evidence of nonlinear effects, providing linear theory early on both in terms of links to mathematical physics (through the Klein Gordon equation) and the predictions of linear dispersive wave theory. We follow this up with a detailed comparison with Lelong and Sundermeyer (finding many points of agreement), though it should be noted that this is a secondary goal of our work. As per the reviewer's suggestion the Froude number is discussed, and since this is essentially an inviscid process the confusing discussion of the Reynolds number has been removed. We have confirmed that the simulations are Reynolds number independent. However, we do disagree with the reviewer rather fundamentally on the role of numerical simulations. Numerical simulations are not only useful when they are compared against model theories. This is certainly useful, and one of us has worked on aspects of this problem in the past. Good numerical simulations allow for a virtual laboratory in which 'what if' questions can be asked. Here resolution matters, and while we did not dwell on this in the manuscript, the numerical set up in the Lelong and Sundermeyer paper cannot answer questions about the radiating wavetrain in any situation apart from that of a very broad initial condition which yields only small amplitude, long waves (even here the periodic boundary conditions are difficult for us to interpret). Such a situation is precisely the opposite of what one finds in the laboratory experiments that provided the major motivation for our work. Perhaps our presentation led to the big picture being missed, and we hope the revisions highlight the novel aspects of what we have done.

We provide detailed responses below (though we note that much of the text has changed).

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 concerned 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 concerned to looking at kinetic energy Hovmoller 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.

Many of the responses to the general comments can be found in the preamble. As a point of fact, the KdV equation was (and continues to be) utilized immediately after its definition to explain the importance of disturbance polarity in the type of response (solitary wave train versus undular bore) for the nonrotating case. While this is not 'solving the equation' it is, nevertheless, useful. The variations in the Rossby number are, in our opinion, much easier to understand in the revised version which provides detailed comparisons with the work of Lelong and Sundermeyer. In terms of what we have learned in the study, we are quite clear about the fact that the primary contribution is to catalogue the nonlinear effects both in the wave train and the geostrophic state. Detailed comparison to previous work centers on the papers by Kuo and Polvani and Lelong and Sundermeyer, with direct referencing of relevant figures in the latter. The vertical structure of the inertial oscillations is also considered something that is not possible to do using shallow water theory, or boundary conditions that are periodic in the vertical.

1. Abstract, line 7: How do variations in the Rossby number demonstrate

the presence of two wave trains?
The wording has been changed.

2. Page 3, line 32: Since the set-up is on experimental scales, the at ocean bottom should be at bottom.
The sign has been changed.

3. Page 4, As in the previous version, the definition of → on line 7 is still not consistent with the expression for → given on Page 6 (top of page).
This has been made consistent.

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

5. 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?
This has been removed.

6. 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?
The definition of B has been provided. The reason for including the KdV equation has been discussed above. The DJL equation is discussed briefly to provide context. In the revised manuscript linear rotating theory is discussed as well, to provide a link with the well studied Klein Gordon equation.

7. Page 8: Please state the polarity of your base case.
A statement has been added.

8. 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).
The wording has been changed.

9. Page 9, Sentence starting on line 19 does not make sense.
The wording has been changed.

10. Page 9, lines 28-30 run-on sentence.
The sentence has been changed.

11. 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 ΔPE can be 0? What's wrong with that? The difficulty in computing ΔPE/ΔKE may come from ΔKE vanishing at t = 0 but typically, one is interested in the

value of this ratio at large time.

There was an error in the original ratio, it has since been fixed. The typical measure (seen in Kuo and Polvani 6.b) is ΔKE/ΔPE which is undefined at ΔPE=0.