Response to reviewers

We would like to thank both reviewers for their insightful comments and for their time. There were many editorial and typographic errors in the original manuscript which the reviewers pointed out and have been fixed, in the future closer attention to detail before submission will be paid. Reviewer 1's comment asking us to consider potential energy has helped strengthen the results in the Geostrophic State section and helped show the effect that the initially leftward propagating wave has on the geostrophic state. This new result is extremely relevant to physical experiments and would not have been noticed without this comment. Reviewer 2's comments about the nonlinear effects helped us develop a new section devoted to these effects. The new results indicate significant nonlinear effects in both the geostrophic state and the ejected waves for moderate to high Rossby numbers. They also provide a clearer connection to traditional nonlinear wave theory. We would like to note that significant changes to both the structure and details of the manuscript have been made and as such listing every change would be extremely difficult. In the response to each reviewer comment we have tried to indicate where the appropriate change has been made in the new manuscript. All of the comments provided were greatly appreciated and addressing them has significantly strengthened the manuscript.

1 Reviewer 1

1.1 Major Comments

1. **I am surprised that no mention is made of the available potential energy anywhere in this manuscript. Since it provides the initial energy source for the mass adjustment, its budget should also be considered. The available potential energy is easily calculated, as is the fraction that is converted to kinetic energy and radiated away as waves. Moreover, it would provide a nice metric for normalizing the kinetic energy and would facilitate comparison with others' results.**
   We computed the available potential energy for the various cases considered using both a sorted and far upstream density profile, with both choices giving essentially identical results. To more closely match the results from Kuo and Polvani, we elected to calculate the difference in total potential energy from the initial state. The potential energy was calculated for the 'primary' cases provided in Table 1. For many of the cases the potential energy did not provide any additional information compared to the kinetic energy in either the geostrophic state or the ejected waves, however this was not the case for low Rossby number simulations. In these cases the kinetic energy in the geostrophic state is dominated by the spanwise velocities. This drowns out any other structure that appears, and hence the potential energy provides more information about

the geostrophic state in these cases. A potential energy space-time plot was especially useful for identifying the leftward propagating wave. We have elected to replace Figure 5 with the corresponding ΔPE and ΔKE and mention in the text that all the cases oscillate at the inertial frequency. We have elected to normalize all the kinetic energies by the maximum of the corresponding case (or a relevant case for the figure) so as to highlight the relative differences and similarities between the cases.

2. **My second comment pertains to the organization of the manuscript, namely the results section. Cases where the Rossby number is varied by varying rotation rates and perturbation widths are first presented. This is then followed by another section titled 'Rossby number variation'. This seems somewhat repetitious. Why not present the former two as subsections of the Rossby number variation section? It would be much easier to follow the discussion of the adjusted states for the various cases considered if the Rossby numbers were given alongside the corresponding w and rotation rate values. This would facilitate comparison with results from earlier studies.**

   The section naming convention and section order has been changed to improve the organization of the paper. Under the heading results there are now subsections: 3.1 Non-rotating case, 3.2 General evolution, 3.3 The geostrophic state, 3.4 Rossby number transition and 3.5 Nonlinear and polarity effects.

3. **In section 3.3, the most energetic stationary state is associated with w = w0 which corresponds to Ro = 1=2. One might argue that values of w that yield a Rossby number of O(1), i.e. $w = 0.5w_0, w_0$ will yield geostrophic states with the most kinetic energy. This was demonstrated in Lelong and Sundermeyer (JPO 2005) in their numerical simulations of nonlinear geostrophic adjustment. This is what Figure 5a shows though the Figure is so small that it is hard to discern all the curves, i.e. where is the curve corresponding to $w = 0.25w_0$?. I assume that the KE is given as a fraction of the initial (available potential) energy though I could not find this explictily stated in the text or the Figure caption. If this is the case, the fraction of KE in the adjusted state agrees with the results of Lelong and Sundermeyer (see their Figure 8). Concerning the energetics of nonlinear geostrophic adjustment, another reference is Boss and Thompson (JPO 1994).**

   A discussion of the Boss and Thompson (JPO 1994) paper has been added to the Introduction, and the results of this paper were used to inform the rewrite of the Results section. The general observation is that linear theory gets the order 1 story of energetics correct, but that nonlinear effects are clearly visible in either the wavetrain or the geostrophic (depending on Rossby number). The Lelong and Sundermeyer (JPO 2005) paper proved tougher to connect to the present work due to differences in model set-up, and overall aim. For example, our entire tank would span 5 grid points in these authors' simulations. Moreover the constant buoyancy frequency limit is well known to be pathological for horizontally propagating internal waves (because the Dubreil Jacotin Long equation governing solitary waves in the nonrotating limit linearizes). We have discussed some of these issues in the Conclusions, and have come up with an avenue for future work that could bridge the gap between the two studies.

1.2 Minor Comments

1. **A schematic of the initial condition for the 2 perturbation polarities would be helpful. This would help define the undefined parameters H_1, H_2 and H_0. We can guess what they represent but they nonetheless need to be defined.**
   A schematic figure has been added to the Methods section to clearly illustrate the definition of the various parameters. We thank the reviewer for the suggestion.

2. **The units used are not consistent. I would suggest sticking to mks units (i.e. do not give some parameters in meters and others in centimeters or millimeters, e.g. on line 130 and in other places too). Also, please reconcile ρ_0 as defined on line 106 (dimensional) with its (nondimensional) value of 1 given on line 141. Units? There is also a missing factor of ρ_0 in the expression for the initial density expression (line 131).**
   The units were all changed to SI. The density expression was corrected for a typo.

3. **There are numerous typos throughout the manuscript, e.g. 'preformed' instead of 'per-formed' on lines 113, 128, 134, 157 and probably elsewhere. There are also several run-on sentences (e.g. between lines 105-110, lines 88-90, lines 171-172, lines 207-209, line 325 etc.) and misspellings (e.g. 'leftover' instead of 'left over', 'hight' instead of 'height', spanws).**
   Spelling mistakes were corrected and a better job editing will be done. A particular issue was the use of auto-correct which corrected technically correct words like 'spanwise' to grammatically correct nonsense such as 'spawns'. Sometimes this occurred after our attempt to correct the sentences and we apologize for the annoyance.

4. **Figures are too small. They should span the width of the text. There are no colorbars on the color contour plots. Captions need to convey more information.**
   Figure sizes were increased in our .tex file, however during the post-processing process the figure sizes might change. A note will be made with the resubmission to increase figure sizes in the final copy provided to

the reviewers. The colorbars were not provided since we are scaling by the maximum value of kinetic energy for the plots, and hence all panels should range from 0-1 (unless otherwise stated in the caption). The captions were expanded to provide more information.

2 Reviewer 2

2.1 Specific Comments

1. (a) **In the Introduction, you mention the related studies of Grimshaw and Helfrich (2008) and Helfrich (2007). However, these studies are not mentioned in sections 3 or 4 (results and discussion). Is there any agreement (or disagreement) with their results? If so, this should be discussed at the relevant points.**
   The agreement with these authors' results is predominantly qualitative, since their description of the ejected waves as a localized wave-packet works as a good descriptor for the dynamics that are observed in the ejected wave train, certainly much better than the results based on shallow water theory. A mention of this has been added to the conclusions. A detailed comparison between weakly nonlinear, weakly dispersive-strongly nonlinear, and the full stratified equations is a worthwhile, but independent project.

   (b) **There has been a lot of other work on nonlinear geostrophic adjustment in continuously stratified fluids, albeit perhaps for slightly different systems (e.g., hydrostatic, cylindrical geometry, etc.). Comparison with some of these results would be appropriate. For example, does the theory of *Zeitlin et al. (2003, JFM 491, Nonlinear theory of geostrophic adjustment. Part 2. Two-layer and continuously stratified primitive equations)* help to understand the pulsating localised state?**
   We have elected to neglect linearly stratified fluids because we are concentrating on a literature that culminates in Grimshaw and Helfrich 2013, and this literature considers quasi-two layer and two layer fluids. Moreover, linear stratification is highly specialized in the sense that the Dubreil-Jacotin-Long (DJL) equation linearizes in this case and solitary wave solutions are possible. This is problematic in the high Rossby wave limit since the theoretical results on the Ostrovsky equation and the related literature generally build on the solitary wave solution and attempt to ascertain such waves' fate in the presence of rotation. A discussion of the Zeitlin et al papers, which are primarily concerned with multiple scale asymptotic analysis and hence do not directly inform the present simulations, has been added to the introduction.

(c) **You say on p.3 that 'numerous papers have used a variety of methods such as asymptotic expansions and numerical integration to solve this linear problem'. So the main novelty of this paper is apparently to investigate new or different nonlinear behaviour. Some of the behaviour described is certainly nonlinear; for example, at the start of section 3.2, you discuss how a bore is replaced by a wave packet. But how much of the behaviour is essentially linear, and how much is intrinsically nonlinear? I am thinking of things like the pulsations of the localised state, the timing of the wave emissions, and the final steady geostrophic state. I think this should be made clear, by making comparisons with companion linear calculations. These could be numerical calculations, or perhaps analytical calculations for some aspects (such as the final steady state geostrophic linear solution).**

A new section devoted to investigating the non-linearity of the simulations was added. The appropriately scaled horizontal profiles of vertically integrated KE of several different rotation modified cases were compared against the non-rotating case. Were the results to be linear all these profiles should collapse to the same curve. This was found not to be the case, with polarity changes yielding the greatest difference. These results show that there are fairly sizeable non-linear effects in both the geostrophic state (for low Rossby number) and in the ejected waves (for high Rossby number). For lower Rossby number the energy in the ejected wave train is less and hence linearity is a better approximation . Similarly, for low rotation rates, the geostrophic state decreases in amplitude and hence is better approximated by linear theory.

2. (a) **I think it would be good practice to give the three values of f, w and Ro together, whenever possible, rather than expecting readers to continually be turning to Table 1 (which should nevertheless be retained). For example, on line 16 of p.6 you might write ..base case with $w = w_0$ and $f = f_0$ (so Ro = 1), ...; the same extra information on Ro would be needed on line 22 of p.15. The three parameters values should be added to the captions of Figures 2, 5, 9, 10 and 12 (which otherwise have no information about parameters).**

   All three values have been added to all the captions and in text.

   (b) **The two different initial conditions are generally referred to as positive or negative polarity, but on two or three occasions as an initial wave of elevation or depression. I think it would be best if consistent language was used throughout.**

   The language has been made consistent.

3. **p.3, lines 19,20: is there a reference for the assertion about the non-rotating adjustment?**
   A new discussion summarizing the present understanding of this case has been added to the Results section.

4. **p.4 (and elsewhere): in many places, references at the end of sentences should be bracketed. Examples include on lines 4, 5 and 10 of p.4. (An example of correct usage is in lines 1516 of p.6).**
   The references have been fixed.

5. **p.7, line 12: you say that there is 'accuracy scaling with the number of grid points', but this is the case for almost all functioning numerical codes. So this statement should be removed or clarified.**
   The sentence has been changed to 'order of accuracy', and this corrected is assertion is only true for spectral methods.

6. **p.7, line 22: 'the density difference was set at 1% '. This detail takes us somewhat by surprise, since the density profile has yet to be defined (other than the mention of a pycnocline on p.4).**
   The density difference of 1% was chosen to match the experiments of Grimshaw and Helfrich 2013, a sentence has been added to clarify this.

7. **pp.68: in (1)(3), it seems that dimensional variables are used, except for $\bar{\rho}$ and ρ', which are dimensionless according to line 9 of p.6 and line 13 of p.8. However, in the first equation on p.8, $\bar{\rho}$ takes the same dimensions as ρ. So presumably the equation should start with $\rho(x; z; t = 0)/\rho_0$.**
   There was a typo in the density equations, it has since been corrected.

8. **p.8, first equation: it would be nice if $\bar{\rho}$ decreases as z increases (assuming that z points upwards).**
   There was a typo in the density equations, it has been corrected and a stable profile results.

9. **p.8: you refer to 'the dimensionless parameter' here, but you said on p.6 that there were 'two dimensionless numbers which are dynamically important'. So maybe say 'The resulting values of Ro are shown in Table 1', or 'The resulting values of this dimensionless parameter are shown in Table 1'.**
   This sentence has been changed.

10. **p.8, lines 1314: I don't believe that H_1 and H_2 have been defined, although it's easy to guess their meaning.**
    A schematic figure has been added which should provide a clear pictorial definition of these parameters.

11. **pp.7, 8: when defining α on line 3 of p.7, you say that 'H_0 is the height of the undisturbed fluid column'. However, at the bottom of p.8 you then take $H_0 = 0.3$ m, rather than the tank depth of 0.4 m. What is happening here?**
    A schematic figure has been added which should provide a clear pictorial definition of these parameters.

12. **p.9 onwards: presumably $x = 0$ corresponds to the left-hand wall?**
    A sentence has been added to clarify this.

13. **p.10, paragraph 2: how closely do the observed structures resemble the modulated wave packet of Grimshaw et al.? Is this a rather superficial resemblance, or are there explicit predictions to compare against?**
    The ejected waves quite closely resemble the modulated wave-packet of Grimshaw et al., however in contrast to their predictions, even at late times the packet does not separate from the tail. In general it is not possible to directly quantitatively compare with their results since they do not model the geostrophic state, and since their theory neglects the leftward propagating wave which reflects off the left wall and influences the dynamics. For these reasons we leave our comparison with their results to a qualitative one.

14. **p.11, lines 1718: you say that there is 'evidence of waves being continuously ejected from the geostrophic state, even for late times in the simulation'. I can't see this evidence in Figures 3a,b, where little wave ejection is visible after about t = 15.** The line has been removed.

15. **p.12, lines 12: it's almost impossible to see the generation from the fourth pulse in Fgure 4e.**
    The figures have been increased in size in the manuscript and a note will be made for resubmission.

16. **p.28, caption to Figure 5: presumably the colours represent different initial values of w_0 (or Rossby numbers), rather than different 'initial rotation rates' (whatever they may be)?**
    This figure has been replaced with a figure of KE and PE, the caption has been corrected.

17. (a) **You say that the geostrophic state is 'shown as the blue dashed line in Fig. 4'. What does this mean? Is it the centre of the geostrophic state (on the basis of kinetic energy)? Is it determined by eye, or is there an algorithm? How does the geostrophic state appear in the height field?**
    We have replaced the following figure with one showing PE and KE

within the geostrophic state which as been defined as twice the distance from the peak in kinetic energy to the left-hand wall.

(b) **I think that Fig. 4 may well be better without the blue dashed line, which obscures the geostrophic state. Could it be replaced by an arrow or a dash at the top of each panel?** The line has been removed since we no longer use the figure that it refers to.

(c) **The kinetic energy plots are a bit confusing. The text (p.12, line 20) talks about proportions, but the numbers shown in the plots certainly don't everywhere add up to 1. Presumably these are just raw kinetic energies, in which case wouldn't it be better to show proportions, or use some other normalisation (particularly in panel (a))?**
Unless otherwise stated in the caption, the kinetic energies are scaled by their maximum so as to highlight the geometrical structure between cases. Figure 5 was scaled by the maximum of all the scales to show the relative energy in each case.

(d) **On a related point, why are we just looking at kinetic energy, rather than total energy? It seems as if you are trying to get a measure of the size of the geostrophic state, in which case wouldn't it make more sense to calculate total energy? This might mask the oscillations in figures 6ce, but it might make a cleaner conclusion.**
We have replaced this Figure with a new figure that shows both the change in kinetic energy and the potential energy. These were calculated separately to relate to the commonly used ratio $\Delta KE/\Delta PE$ (though we cannot calculate it directly in this case). In general kinetic energy is used as it provides both information about the structure and velocity.

(e) **As noted in point 1(c), the whole discussion in this section should be grounded by a discussion of what happens in the linear case, when presumably some things are known (either for single-layer shallow water, or perhaps modes in a continuously stratified system). For example, is the drift in the centering of the geostrophic state as w changes understood linearly? What is known about the K.E. (and P.E.) for the linear case?**
As mentioned in the response to point 1(c) we have devoted a new section to the effects of nonlinearity and have considered a 'nearly linear' case. Furthermore for our smooth initial conditions the peak in kinetic energy always occurs where the isopycnals transition. Thus they should scale linearly with disturbance width. In regards to the energetics, according to Boss and Thompson, 1994 the linear estimate of the kinetic energy should be a very good approximation to the nonlinear case.

18. **p.13, line 16: 'We can also see that the oscillations of the geostrophic state remain quite evident for later times'. Really? Maybe this is visible in figure 7b, not in figure 7a (no oscillations visible after $t = 20$) or figure 7c (only one oscillation shown).**
The figures have been enlarged to help identify this feature, and the new figures 7 and 9 clearly show the oscillations.

19. **p.13, line 24: why is Ro = 1 picked out as being a 'critical Rossby number'? There doesn't appear to be any kind of sharp transition in the dynamics there. This is picked up again on p.14 (lines 810), where it is said there is a 'change in the dynamics'. However, this looks like part of a gradual change from higher Rossby number to small Rossby number, and there seems to be no basis for singling out Ro = 1 as a critical number.**
Though there may not be a discrete change across Ro=1, there is a shift in the dynamics, as highlighted in Figure 10. If we classify the location of the ejection of the wave packet as a metric for this transition, there is a shift when we pass Ro=1. The reviewer's point is well taken, though, and we have worked to temper the language around this point.

20. **p.14, 15: you make the point that the results here are fundamentally different to those based on KdV. However, this is entirely to be expected since, as far as I understand, the KdV analysis is for the non-rotating problem. I think this issue could be clarified.**
In the non-rotating case, the waves generated can be, at least qualitatively, described by KdV theory, one would then logically expect that the rotating version of this problem could be modeled by a rotation modified KdV theory (like the rKdV equation). The sentence has been changed accordingly.

21. **p.15, lines 614: this entire paragraph seems to be saying that the observed behaviour is almost linear. The degree of nonlinearity should be discussed explicitly. What is new here? As you move on to p.16 and talk about differences between positive polarity and negative polarity solutions, the results are clearly nonlinear.**
This paragraph has been removed and a new section devoted to nonlinearity has been added.

22. **pp.15, 16: you set up the idea that the results agree with those of Stastna et al. rather than Grimshaw et al. So please add a comment as to what is missing in the work of Grimshaw et al. For example, is that for a different asymptotic regime or physical configuration?**
The primary feature that is missing from the model in Grimshaw et al. is the lack of nonlinear dispersion. This could be added to the model by considering more terms in the asymptotic expansion. The reality is that

these effects are quite small and this has been stated at a number of points in the article. The model equation also neglects the geostrophic state which is inherently created during any initialization. Thus the advantage of our numerical simulations is that they capture all the effects in one convenient package. These comments have been added in the conclusion section.

23. **p.16, line 20: why is it clear that the 'positive polarity case is strongly affected by the boundaries'?**
    This line has been removed.

24. **Figure 12: could you add the predictions of linear theory to this graph? It would be nice to have a quantification of the importance of nonlinearity in each case.**

    We have added the results of a case with 1/200 of the base case's amplitude to the new Figure 11. This new figure shows quite clearly the manner in which nonlinear effects manifest themselves, and in particular the dominant effect of polarity. A more detailed discussion is provided in the new section on nonlinearity.