Nonlin. Processes Geophys. Discuss.,
doi:10.5194/npg-2015-73-RC2, 2016

Nonlinear Processes in Geophysics
Discussions
Open Access

Interactive comment on "The fully nonlinear stratified geostrophic adjustment problem" *by* A. Coutino and M. Stastna

Anonymous Referee #2

Received and published: 24 March 2016

General comments

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 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.

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),

Printer-friendly version

Discussion paper

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.

There are also several bad things about this article. In some places (e.g., section 3.2), the results are set into the context of related studies. However, this is generally not the case, particularly with regard to what extent the results can be understood or interpreted using linear theory. That is, how much of the behaviour is intrinsically nonlinear, and how much is essentially linear? I also suspect that some of the behaviour reported (e.g., the pulsations of the geostrophic state) could be understood in part using existing analytical results. The quality of the text is mediocre or poor in places, with parts reading like a PhD thesis rather than a carefully written research paper. There was also inadequate proofreading prior to submission – how can phrases such as 'unduly bore' and a 'spawns instability' (should be a spanwise instability) make it into a submitted version?

Overall, I imagine that the manuscript will be suitable for publication in NPG after moderate revisions.

Specific comments

1. Context.

   (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

Printer-friendly version

Discussion paper

points.

(b) There has been alot 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?

(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).

Note: the only time that I see mention of varying the amplitude of the initial disturbance (second paragraph of p.15), you say that there 'was no discernible difference to any of the fields within the waves generated once they had been scaled by the factor that the amplitude was reduced', which sounds like a linear phenomenon.

2. General notation.

(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).

(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.

3. p.3, lines 19,20: is there a reference for the assertion about the non-rotating adjustment?

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 15–16 of p.6).

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.

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).

7. pp.6–8: in (1)–(3), it seems that dimensional variables are used, except for $\overline{\rho}$ 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, $\overline{\rho}$ takes the same dimensions as ρ. So presumably the equation should start with $\rho(x, z, t = 0)/\rho_0$.

8. p.8, first equation: it would be nice if $\overline{\rho}$ decreases as z increases (assuming that z points upwards).

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'.

10. p.8, lines 13–14: I don't believe that H_1 and H_2 have been defined, although it's easy to guess their meaning.

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?

12. p.9 onwards: presumably $x = 0$ corresponds to the left-hand wall?

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?

14. p.11, lines 17–18: 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 $t = 15$.

15. p.12, lines 1–2: it's almost impossible to see the generation from the fourth pulse in Fgure 4e.

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)?

17. Section 3.3: this section was lazily written.

    (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?

    (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?

    (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))?

    (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 6c–e, but it might make a cleaner conclusion.

    (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?

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 not in figure 7a (no oscillations visible after $t = 20$) or figure 7c (only one oscillation shown).

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 8–10), 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.

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.

21. p.15, lines 6–14: 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.

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?

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

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.

Printer-friendly version

Discussion paper

Technical corrections

1. There are many issues with punctuation in this article – particularly missing or incorrect usage of commas and semicolons. Here are some examples:
   (a) p.3, line 25: incorrect usage of a semi-colon.
   (b) p.5, line 13: missing punctuation between 'our domain' and 'this will'.
   (c) p.9, line 25: missing punctuation between 'discussion here' and 'however this'.
   (d) p.11, lines 12–14: both missing punctuation and a spelling mistake in this sentence!
   (e) p.11, line 29: should be colon (not a comma) between 'dominant features' and 'the geostrophic state'.
   (f) p.12, line 21: another example of poor punctuation.
   (g) p.13, lines 20-22: another sentence with particularly poor punctuation. The second comma (after ejected) should be something else, and the semi-colon should be a comma.

   I am stopping pointing out errors with commas and semi-colons from hereon!

2. Final line of abstract: repeated 'allows for'.
3. p.3, line 6: why are the references out of order?
4. p.3, line 22: dependant.
5. p.4, lines 13–17: this long sentence should be reworded.
6. p.5, line 19: spurious 'though'? Otherwise it's hard to make sense of this sentence.

7. On several occasions, ‘performed’ is written as ‘preformed’ (p.6 line 18, p.7 line 9, p.9 line 7).

8. p.8, line 17: presumably you mean $\times 10^{-6}$ rather than e^{-6} (too much time spent on a computer).

9. p.9, line 10: hight.

10. I suspect that ‘rank ordered’ should always be hyphenated, as it is on p.4 but not elsewhere.

11. caption to figure 7: ‘the second row corresponds to the same case as the first columns but the aspect ration has been scaled....’: presumably ‘first row’ and ‘aspect ratio’?

12. p.14, line 20: repeated ‘is the’.

13. p.15, lines 13–14: how can the authors not spot ‘spawns variation’ and ‘spawns instability’, which presumably should be ‘spanwise variation’ and ‘spanwise instability’?

14. p.15, line 7: ‘From the cases that were run there was no....’ could be shortened to ‘There was no...’.

15. p.15, lines 23–25: it isn’t necessary to repeat the information about the changed resolution (16384 and 1.59cm), which was given before.

16. p.16, line 7: ‘results ... is’ should be ‘results ... are’.

17. p.17, line 10: ‘cases exhibits’?

18. p.17, line 16: ‘..examined at the differences..’ should be ‘..examined the differences..’.

19. p.17, line 26: ‘unduly bore’ should be ‘undular bore’.

20. p.17, line 19: ‘corresponds an initial wave’ should be ‘corresponds to an initial wave’.

Interactive comment on Nonlin. Processes Geophys. Discuss., doi:10.5194/npg-2015-73, 2016.