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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Incidence and reflection of internal waves and wave-induced currents at a jump in buoyancy frequency" *by* J. P. McHugh

Anonymous Referee #2

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Overview

This paper examines the evolution of internal gravity waves incident upon a discontinuity in the (otherwise uniform) buoyancy frequency. The waves are horizontally periodic, but propagate vertically towards the discontinuity as a small amplitude wave packet. Amplitude equations are derived the incident waves, the reflected waves, and the transmitted waves, along with an expression for the horizontal mean flow change, which itself appears as a coupling coefficient in the amplitude equations. These equations are solved numerically for various parameters, and the evolution of the wave packets and mean flow change are examined.

In general, the paper is well written and illustrated, and is fairly easy to follow. It deals with a problem of relevance in the atmosphere, and, as far as I can tell, is more-or-less

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technically correct. On the negative side, I'm not sure that the coupled equations and numerical simulations have been analysed with a great deal of thought, and I didn't gain much insight from the study. This state of affairs is summed up by the two paragraphs of Section 7 (i.e., the conclusions), which are particularly weak.

I would also regard this study as rather incremental (although the author may be able to clarify this). The first 12 pages of the study are standard background material, which appears elsewhere in the literature as far as I can see. The new material looks to be section 5 (derivation of the coupled equations for I, T and R) and then the numerical simulations (totalling 12 pages). There is little discussion of the structure or properties of these coupled equations, so the bulk of the paper is really the numerical simulations of section 6. These are interesting, but I was disappointed that little of the simulated behaviour was described using mathematical analysis of the coupled equations.

I think that more work should be done in that area in particular, along with something to address the more specific comments below.

Specific comments

Introduction: it wasn't clear to me from the Introduction (or elsewhere) how the present study differs from previous work (e.g., Grimshaw and McHugh). I think this should be clarified. I think that further similarities or differences could be clarified throughout the manuscript – for example, equations (40-42) here look very similar to (34,35) of Grimshaw and McHugh.

Introduction: it's nice to see a good deal of background information relating to other work. However, I thought this material could have been presented and ordered rather better, with the context of the present study in mind.

p.272, second paragraph: you imply that internal waves are always modulationally unstable, but this isn't the case.

p.274: I thought it strange that positive b (buoyancy) corresponds to anomalously heavy fluid, rather than the other way round (i.e., $b \to -b$).

p.276: in deriving equation (14), you seem to have used $\tilde{\rho}(0) = 0$. Presumably then $\tilde{\rho}$ isn't the background density, but rather the deviation of the background density from the reference density ρ_0.

p.276: I thought that the use of ξ and ζ was not optimal, since they are hard to distinguish (at least in the fonts used by NPG). Wouldn't χ (which looks like x) be a better choice than ξ for the quasi-horizontal coordinate? If so, you would need to change the character for vorticity later on.

p.276: in (16), are k and n_1 both implicitly taken to be positive?

p.278: is there a reference for the reflection and transmission coefficients (24) and (25)?

p.278: in deriving (31), are you taking $\overline{w} = 0$?

p.279: in (34), where is the arbitrary function of ζ and τ that should be on the RHS after integration? Has it been set to zero for some good reason?

Section 4: you say that this section follows previous work. I am guessing that equation (38) has been derived elsewhere, but afer that it is not so clear. Can you be more explicit about what is new, and what has been derived before?

p.279: should equation (38) be read as 'leading order part of $\overline{u}$' equals '...leading order part of $\hat{b}$'?

p.281: wouldn't the first paragraph of p.277 (explaining why nonlinear terms don't contribute to the evolution of the primary waves) be better placed immediately after equation (46), i.e., where this information is actually used?

p.281: an intermediate step from (43-46) to (47) would be helpful. For example, the $O(\alpha^2)$ term could be written in terms of $\hat{w}$, $\hat{b}$, $\hat{u}$ and $\overline{u}$, and then given in the final form of (47) once the substitution from the primary harmonic is made.

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p.281/282: it could be made that clear that the choice epsilon=alpha is being made from hereon?

p.282: you talk about 'leading-order results', but your equations (50,53,54) and boundary conditions (63,64) contain both $O(1)$ and $O(\epsilon)$ terms. Are you uncomfortable with this mixing of orders? Under what circumstances might this be justified?

p.283: there's very little about the structure or properties of the derived coupled amplitude equations. For example, are there any conserved properties? Should we expect modulational instabilities in the coupled system?

p.284: the details of the numerical methods are inadequate – how can others possibly reproduce your results with so few details? What is the domain size? How many grid points are used? What is the time step?

p.287, line 11: you make a link between the sign of the horizontal component of the group velocity and the mean flow change. Should this be apparent in your formulae (39-41) for the mean flow change, or do you have something else in mind?

p.288, line 7: you talk about the coefficient of the dispersion term being negative. Do you mean the coefficient in the I and R equation, or the coefficient in the T equation, or both? I think this may also lead to some confusion in the results. As far as I can see, for the case with $n_1/k =$0.4, $c_g' > 0$ in the lower layer but $c_g' < 0$ in the upper layer. So the upper layer would be defocussing. This is consistent with Figure 8, but inconsistent with the text, which says that 'the tendency to focus continues in both layers'.

p.292: the Conclusions were rather short and inadequate. For example, how do these results compare with the numerical simulations of wave packet propagation that you mentioned in the Introduction (McHugh, 2008)? Can you make a case for why this approach (coupled amplitude equations) is better than numerical simulations? Is the structure of the mean flow change (as found in section 6) at all reminiscent of any observations near the tropopause?

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Technical corrections

p.270, Line 4: buoyancy.

p.270, Line 20: is $\rightarrow$ are.

p.272, line 14: Gibbon.

p.275, equation (14): should be $+\eta/2!$.

p.276, line 3: period $\rightarrow$ periodic.

p.280, equation (39): $n_- \rightarrow n_1$.

p.287, line 20: completed $\rightarrow$ completely.

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