Response to referee #2:

Thank you for your comments. My manuscript has now been significantly improved as a result of your comments, the comments of the other referees, and also my own additional work.

Detailed responses to you specific questions and comments are provided below. In most cases I have altered the manuscript as a result of your remarks:

1. *...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 have added a significant discussion of the character of the equations in the results section. Separating the amplitude into magnitude and phase is particularly revealing, and shows several important results.

   The conclusion section was originally intended as just a quick reminder of the most important results. Now I have expanded the conclusions to be more of a short review of the paper and also added some thoughts that extend beyond the hard results of the equations.

2. *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 .... 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 behavior was described using mathematical analysis of the coupled equations.*

   I do not agree that this study is 'incremental', which implies that I am merely confirming previous results and have no significant new results. There are several important new results here. Most of all is the evolution of the interaction mean flow to smaller and smaller scales. This strictly nonlinear phenomena has not been reported previously.

I do agree that there was little analysis of the amplitude equations in my original submission and I have now worked hard to rectify this, as I mention above.

3. *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.*

   It is true that the mean flow that I find here is identical to the form we found previously in Grimshaw and McHugh (2013). I now acknowledge this in a forthcoming revision, and also have removed some of the derivation of the mean flow at the insistence of another referee. We did show in the previous paper that the mean flow will be discontinous, although we did not produce the NLS equations, and that is new in this paper.

4. *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.*

   I have made a significant effort to reword the introduction, and include comments where my work is making a contribution.

5. *you imply that internal waves are always modulationally unstable, but this isn't the case.*

   This paragraph has been reworded and no longer makes that unintentional incorrect inference.

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

   I agree that it is a bit strange, but on the other hand my definition of buoyancy seems to be most common and matches our previous work in Grimshaw and McHugh (2013). To more easily relate to this previous work, I have decided not to change the definition.

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

The background density is $\tilde{\rho}$ and I have not assumed $\tilde{\rho}(0) = 0$. But I have used the Boussinesq approximation in deriving (14). The Boussinesq approximation uses the reference density ρ_0 in the inertia terms and hence ρ_0 appears in the dynamic interfacial condition when I substitude for pressure. I have added a brief comment here to clarify this.

8. *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 more 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.*

   While I am sympathetic to this suggestion, I prefer not to make this change in order to not conflict with other work using the same variables.

9. *in (16), are k and n_1 both implicitly taken to be positive?*

   Yes, I have chosen all wavenumbers to be positive. I have now reworded this paragraph to clarify. This was also suggested by another referee.

10. *is there a reference for the reflection and transmission coefficients (24) and (25)?*

    I have added several references here.

11. *in deriving (31), are you taking $\bar{w} = 0$?*

    Yes, $\bar{w} = 0$. This is a consquence of horizontal periodicity. The revised paper has a much different and reduced discussion of the mean flow, to comply with another referee. However I now note that $\bar{w} = 0$.

12. *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?*

    Yes, this arbitrary function is set to zero. The justification is that there is no mean flow before arrival of the wave packet. Again this section is much different, and this particular integration is not shown explicitly.

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

Indeed equation (38) has appeared elsewhere and what follows has not appeared before (thank you for pointing that out). This section is much different now and this issue has been addressed.

14. *should equation (38) be read as 'leading order part of* $\bar{u}$*' equals' . . . 'leading order part of* $\hat{b}$*'?*

    Yes, your interpretation is correct. Again, this section has been rewritten.

15. *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?*

    The nonlinear terms in the interfacial conditions have the potential to do two things: 1) they could (but don't) contribute to the primary harmonic at this order, and 2) they could create higher harmonics (2 n_1, 3 n_1, etc. . .). This paragraph is discussing these higher harmonics, rather than the primary harmonic effect. I have reworded here and after equation (46) for clarification.

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

    I have included this extra step, as you suggest.

17. *it could be made clear that the choice* $\epsilon = \alpha$ *is being made hereon?*

    I have reworded this paragraph to address this problem, also suggested by another referee.

18. *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 comfortable with this mixing of orders? Under what circumstances might this be justified?*

    In the second paragraph of section 6 I discuss the appearance of ϵ in the final amplitude equations, and point out that it can be removed by choosing a different scaling for τ and ζ. The equations are the same except ϵ does not appear. Hence really there is no mixing of scales.

But as long all terms $O(\epsilon)$ are retained, the such a mixing of scales is completely rigorous. I have added a few comments here to clarify this.

19. *there's very little about the structure or properties fo the derived coupled amplitude equations. For example, are there any conserved properties? Should we expect modulational instabilitieis in the coupled system?*

    As I mention above, I have added a signficant discussion of the properties of the coupled equations as you suggest.

20. *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?*

    I agree that these details are missing and have added them in the new version.

21. *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?*

    It is apparent in (40) that both I and R increase the strength of the mean flow. My comments here were trying to further explain this by pointing out that the waves are reflected in a manner where the sense of the horizontal component of group velocity doesn't change. Since the sense of the mean flow matches the sense of the group velocity, and the incident and reflected mean flows are additive. I have reworded this somewhat now.

22. *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'.*

    You are correct about the signs of c_g' in the two layers for figure 8. The sign of c_g' in the upper layer is such that defocussing occurs. The figures are correct but the text was not correct here. I have changed the wording here and elsewhere to clarify the amplitude equation that I am refering to when I discuss c_g'.

23. *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?*

    I have expanded the Conclusions section, as I mentioned above. Several questions arose in the direct numerical simulations that have been resolved here. Notably, the numerical simulations showed that the mean flow that is localized at the interface does not disappear as expected. At the time I thought this was due to wave breaking in the upper layer. But as a result of this present study and also the results of Grimshaw and McHugh (2013), I now see that the flow has to allowed to be discontinous somehow, and this was not the case in the numerical simulations. I now discuss this in the Conclusions, and also make some comments about previous observations.