Response to the editor:

Dear Roger,

I have now completed the revisions to my paper. I have changed the discussion of the derivation, as I outline below, and clarified my arguments in favor of the linear interfacial conditions. However, plots of the integral of $\bar{u}$ show that as the packet transits the interface, this integral is not perfectly constant. The error here is due to the lack of nonlinear effects at the interface, as suggested by referee #2. I spent a significant effort attempting to include the nonlinear interfacial effects, but this is extremely complicated and will have to wait. So I have retained the linear version of the interfacial equations and argue that they are approximate, as you suggested.

Also, I discovered that the plots of mean flow in my previous submission in the upper layer had the wrong scale and the mean flow appeared to be weaker in the upper layer than they should have. The results were correct but the graphics were wrong. I have now corrected this problem.

Detailed responses to your remarks are below:

1. *...has raised serious concerns over the scaling of the amplitude equations...*

   Due to the continued criticism of the scaling that I was using, I have now changed this aspect of the paper and use the usual NLS scaling that you mention. The final NLS equations are the same except without the confusion of mixing of orders. Really only the derivation is different and admittedly better than my previous submission. I am using an approach similar to that of Schrira (1981), which includes all slow scales at first. This approach is in fact helpful when treating the interfacial conditions.

2. *...the structure suggests that the choice $\alpha \approx \epsilon$ should be made, and you have put $\alpha/\epsilon = 4$. These small parameters can be chosen independently as you have done, but the balance $\alpha \approx \epsilon$ must hold.*

   I agree that dispersion and nonlinearity must be approximately in balance. But the actual numerical value of ϵ that results in this balance is not well-defined. The value for ϵ, which measures how slowly the wave amplitude is changing, is usually the inverse of the length of the packet, and it is the 'length' that requires definition. There are several obvious choices. My choice is to use the overall length, which with my choice of packet shape (raised cosine) is well-defined. Another choice might be the Full Width at Half Maximum (FWHM). Sutherland (2006) used a Gaussian packet shape, $e^{-z^2/2\sigma^2}$ and defined the packet length to be $1/\sigma$, which seems appropriate but in fact is a much smaller length than my choice. If the two packet shapes have the same FWHM, then my value for ϵ is $4\sqrt{2\, ln2}$ times Sutherland's value. This means that with my definition of ϵ, $\alpha/\epsilon \approx 4$. The behavior of the wave packet in the simulations agree with this choice. Furthermore, with $n_1/k = 0.4$ and $\alpha = \epsilon = 0.1$, the simulations show that dispersion dominates and nonlinearity is unimportant.

I was tempted to change the definition so that the values of ϵ and α are equal and nonlinearity and dispersion are in balance, however there is no obvious choice with the raised cosine, so I have maintained my definition and have added a discussion of the numerical value of ϵ.

3. *...Indeed there is also the claim that...your uniformly stratified case is incorrect.*

   The previous work by Sutherland (2006) had an extra term, that being the third derivative, I_{zzz}, as you mention. I added this term and repeated some calculations to match the results shown by the referee. This required substantial work as the third derivative term results in a numerical method that is more unstable, however eventually I succeeded in getting it to work. As expected, the results in a single layer match what the referee shows in his results for the case where $n_1/k = 1/\sqrt{2}$. I show a plot of this result in my response to the referee.

   However, since the nonlinear terms appear at $O(\alpha^3)$, then this third order term does not appear until the next order and therefore need not be included. When written in slow variables, this third-order term has ϵ for a coefficient. However when reverted back to original variables the ϵ disappears, as for all of the linear terms. But this does not mean they are all of the same order. I have not included the third-order term in my results and do not feel it should be included for this scaling.

   The issue is really only important for the case where $n_1/k = 1/\sqrt{2}$, which is dispersion-free without the third derivative term. I have greatly reduced the discussion of this dispersion-free case. You suggested that I omit it entirely, but I use this case to make several points, and so I retain some of the discussion but I have added a comment on the dispersion term used by Sutherland.

With regard to two-layer simulations of the referee, I would point out that importantly, he is not allowing slip at the interface, which must occur in inviscid flow. He would likely argue that he has artificial damping that acts like viscosity and eliminates the slip. However he then has a thin boundary layer that is governed completely by his artificial damping! This is a critical issue with this problem. If the nonlinear theory that allows slip does not match the DNS that do not allow slip, then the least confidence has to be directed at the DNS. I do not agree with the referee that his simulations are the 'right' answer.

4. *Turning to the interface issue...it is not clear to me that (14) written is correct...*

   I have expanded my discussion of the interfacial conditions extensively. Indeed there are interactions with second-harmonics that appear, as you suggested. However all of these nonlinear terms are still at $O(\alpha^2)$, raising the question of their importance. When the Schrodinger equation is written in terms of slow variables, and $\epsilon = \alpha$, then there are no small parameters present, and all terms are in balance, as they must be. There are an infinite series of other terms that are traditionally neglected due to the presence of a small parameter taken to some power (including the I_{zzz} term of Sutherland (2006)). But the interfacial conditions written in terms of slow variables do have the small parameters present, in particular the α^2 for all nonlinear terms, and by the same argument used for the Schrodinger equations, these terms can be safely neglected.

   On the other hand, the vertical integral of $\bar{u}$ is not perfectly constant, suggesting that nonlinear terms be retained. When the Schrodinger equation and the interfacial conditions are written in terms of original variables x, z, t rather than slow variables, then the nonlinear terms appear with the coefficient α^2 in both Schrodinger equation and interfacial conditions, an appealing consistency. I attempted to include the nonlinear terms in the interfacial conditions, but they turn out to be extremely complicated, and so this will have to wait for a sequel. In the present manuscript I clarify these issues and then proceed with the linear interfacial conditions, arguing that these are a reasonable approximation. In the end, as I mention above, the plots of the integral of $\bar{u}$ show that there is an error with these linear interfacial conditions.

5. *...you need to check that the vertical integral of $\bar{u}$ is indeed preserved across the interface...*

   I have done this and included a plot in the manuscript. Overall this plot shows that $\bar{u}$ is approximately constant, as discussed above.

I feel that I have successfully addressed the concerns of all referees, and this paper is ready for publication.

Response to referee #1:

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

As a result of a referee comment, I discovered that the plots of mean flow in the upper layer had the wrong scale in my previous submission and appeared to be weaker than they should have. The results were correct but the graphics were wrong. I have now corrected this problem.

Detailed responses to you specific questions and comments are provided below. You will find that I agree now with some of your comments, but disagree with others.

1. *...the interface conditions are truncated at an amplitude-order, inconsistent with the rest of the weakly nonlinear analysis.*

   As a result of your concerns and the concerns of others, I have changed the discussion of the derivation of the equations. My new derivation is more like that of Shrira where all slow scales are retained up to the order where nonlinear terms first appear.

   As I now show, when the Schrodinger equations are written in terms of the slow variables, if $\alpha = \epsilon$ then there are no small parameters present and all terms are in balance. The corresponding interfacial conditions show that the nonlinear terms written in terms of the slow variables have α^2 as a coefficient. This suggests that the nonlinear terms in the interfacial conditions do not need to be included. It is this argument that I have tried to make previously. The decision to delete a term is generally made when equations are written in slow variables: after all, there are an infinite set of linear dispersion terms that seem to be the same order when written in original variables, but clearly should not be included.

   On the other hand, plots of the integral of $\bar{u}$ show that this quantity is not perfectly constant, as it should be to conserve momentum. The error, while relatively small, is indeed due to the nonlinear contributions in the interfacial conditions. It is this fact that has convinced me that the nonlinear terms should in some way be retained, even though the weakly nonlinear theory suggests otherwise. I now agree with you on this particular issue.

However, as I now discuss, these nonlinear terms have great complexity and it was not possible to include them in a timely manner for this paper. Hence I have not included them and argue that this is at least a reasonable approximation, based on the above. Indeed, the same plots of $\bar{u}$ that indicate that these terms should be retained also show that their inclusion is a minor adjustment in the reflection and transmission coefficient. Hence my overall results will remain intact.

2. *...I have simply run a series of fully nonlinear numerical simulations using parameters provided by the author in his different scenarios.*

   Your single-layer simulations have $n_1/k = 1/\sqrt{2}$. This is a special case where the primary dispersion term has a coefficient of zero, as I had discussed previously. But a fully nonlinear simulation has all dispersions contributions, not just the I_{zz} term, which accounts for the difference between your single layer results and my previous results.

   Sutherland (2006) included a third derivative term I_{zzz}, which strictly speaking is higher order for this problem, as is evident when the equations are written in terms of slow variables. However for this special case where $n_1/k = 1/\sqrt{2}$, this extra term seems to provide approximately the correct dispersion. I repeated this case with this third derivative term included, and results are shown in the figure. These results seem to match your figure quite well, and this confirms that the difference between the two approaches for this special case is due to this third derivative term.

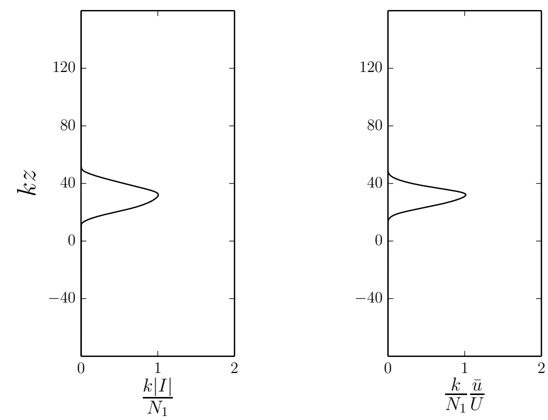
   For other parameter values the third-derivative term appears to be quite unimportant, although causes significant numerical difficulties. Hence this third derivative term is not included here. Furthermore, I have deleted my discussion of the special case $n_1/k = 1/\sqrt{2}$. I really only used this case to show how well the numerical methods work.

   You also show two-layer simulations of this same special case, and mention results for $n_1/k = 0.4$. However, as shown previously in Grimshaw and McHugh (2013) and here, the mean flow is discontinuous at the interface. Thus any full simulation of a two-layer inviscid flow must allow slip at the interface. I attribute any difference in the results with $n_1/k = 0.4$ to the fact that you are not allowing such slip. This is a critical issue and a strength of my results, as slip is allowed. A direct numerical simulation that allows slip is a challenging endeavor.

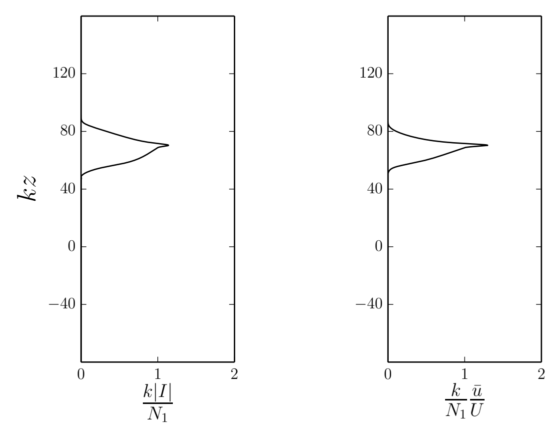
An alternative simulation is viscous, which has a very thin boundary layer at the interface, introducing a different set of challenges. I am presently working on both types of simulations, but such results will not be available in time for a comparison any time soon.

3. *. . . As is well established in the literature, momentum conservation requires the vertical integral of* $\bar{\mathrm{u}}$ *to be contant over time . . .*

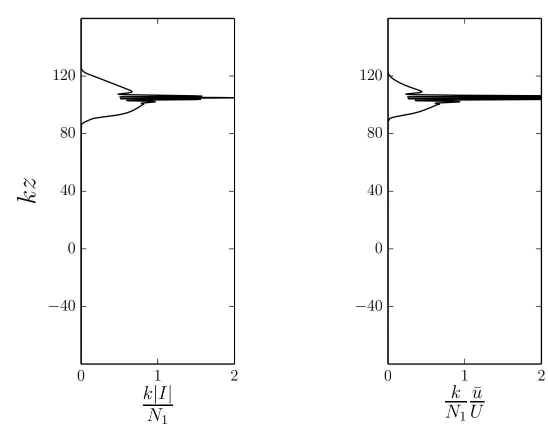
   I agree as mentioned above and I have included a plot in the manuscript. Overall this plot shows that $\bar{u}$ is approximately constant, as discussed above.



(a) $N_1\tau \approx -75$



(b) $N_1\tau \approx 0$



(c) $N_1\tau \approx +75$

Figure 1: Vertical profiles of the wave magnitudes A (left panel) and mean flow (right panel) at three times in one layers. The parameter values are $n_1/k = 1/\sqrt{2}$, $N_2/N_1 = 1$, $\epsilon = 0.025$, and $\alpha = 0.1$. The third order derivative is included.

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 the editor, and also my own additional work.

As a result of a referee comment, I discovered that the plots of mean flow in the upper layer had the wrong scale in my previous submission and appeared to be weaker than they should have. The results were correct but the graphics were wrong. I have now corrected this problem.

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. *. . . a major concern would be whether or not the coupled amplitude equations (and the associated numerical algorithms) actually capture the dynamics of the full nonlinear system, in an appropriate weakly nonlinear regime. If an asymptotic analysis is carried out consistently (all terms and boundary conditions treated to the same order) then I generally expect that the reduced system is physically sound, meaning that (i) it will give the correct behavior in a certain weakly nonlinear regime, (ii) appropriate conservation laws (e.g., energy, momentum) won't be violated. Here, there is a mixing of orders . . .*

   As a result of your concerns and the concerns of others, I have changed the discussion of the derivation of the equations. My new derivation is more like that of Shrira (1981) where all slow scales are retained up to the order where nonlinear terms first appear. The final nonlinear Schrodinger equations are basically the same as before, except now there is no confusion over the mixing of orders. In the end I write them in terms of original variables instead of slow variables, as do previous authors. I have not shown all of the details, but the essentials are here and show that my system of equations is sound in a rigourous manner. As such my approach corresponds to your item (i) above.

   My previous derivation was an attempt to get these same results without introducing for the intermediate slow scales. However the new derivation is admittedly clearer and has helped me explain the final form of the interfacial conditions.

2. *I am guessing that a comparison with full nonlinear solutions is out of the scope of this study...*

The mean flow in the inviscid case is discontinuous at the interface, hence a full numerical solution would have to allow slip. This makes such a simulation very challenging and, yes, outside the present scope. An alternative simulation would be to include viscosity, but then there would exist a very thin viscous layer that would require resolution and would add different features to the problem. This would be an interesting result, but not quite a direct confirmation of the inviscid results given here, which is what I think you were suggesting.

3. *...perhaps the author can reassure me with something about conservation of energy. What I have in mind is simply calculations of domain-integrated energy ...*

   Other referees also have suggested that I show such results, so I have added time histories of $\bar{u}$. In the present theory the integral of $\bar{u}$ is proportional to the integral of the sum of the amplitudes squared, e. g. the kinetic energy. Using a periodic box that extends vertically outside the limits of the wave packet, it can be shown that the vertical integral of $\bar{u}$ must be constant with time. However my results show that there is an error when the packet transits the interface. This means that momentum is not perfectly conserved. I now discuss this in several locations. Without the interface, this integral is conserved quite accurately. Hence the error must be caused by the interfacial conditions, as suggested by another referee. These linear interfacial conditions are creating transmitted and reflected waves that are somewhat too large or small, depending on the wave parameters. The correction for this issue is to include nonlinear terms in the interfacial conditions, which has proven to be very complicated. Since the error is relatively small, I have retained only the linear interfacial conditions. Note that the overall dynamics will be the same with more accurate interfacial conditions, and my results are still approximately correct.

In response to your detailed comments,

1. *Line 42: you say that Grimshaw and McHugh (2013) treat 'unsteady and steady flow', and then a few lines later (line 47) say that you now include the 'temporal evolution'. What is the distinction between unsteady flow and temporal evolution?*

The unsteady results in Grimshaw and McHugh (2013) are expressions for the mean flow in terms of the wave packet shape, in particular at large times after the waves have interacted with the interface. That paper then uses those unsteady results to evaluate the steady case. The evolution of a particular wave packet was not considered in Grimshaw and McHugh (2013). I have reworded this section to avoid confusion.

2. *Introduction: wouldn't the paragraph starting at line 85 be better immediately after that ending at line 75, since they both talk about stability issues?*

   I have made this change.

3. *At what point is the buoyancy frequency taken to be a piecewise constant? On line 152 there is reference to buoyancy frequencies N_1 and N_2 (rather implying two constants, since otherwise one would just have $N(z)$), but on the next page there are terms in dN^2/dz.*

   The equations in section 2 apply to any vertical profile $N(z)$. I have changed the wording here by moving the mention of N_1 and N_2 to the end of this section and adding a comment about the derivatives of N^2.

4. *I believe there to be a sign error in (13) - it should be $-\eta_x$? - but (14) is nevertheless correct.*

   I have corrected this typo.

5. *Can you please include a reference (if possible) around line 213, to justify the stated amplitude of the higher harmonics that arise due to modulation.*

   I have rewrittent this paragraph and added a reference to Thorpe (1968) and Grimshaw (1976).

6. *Line 244: in my original review, I asked 'is there a reference for the reflection and transmission coefficients'? The author said that he 'added several references here', but none are evident near line 244. Since these coefficients have surely been given elsewhere, it would be nice to show awareness of this and confirm consistency with previous results! I guess this counts as good scholarship.*

   I apologise for not including that reference originally. In fact I have not been able to find explicit reference to these formulas in the context of internal waves. However exactly these same formulas appear in optics, going back to Fresnel in 1823. I now make reference to a book by Born that discusses this optics example.

7. *I was confused by some of the terminology relating to the asymptotics: (a) on line 213 you say that the $O(\epsilon\alpha^2)$ (nonlinear modulational) terms are small and need not be included. Do you mean that these terms are of $O(\epsilon\alpha^2)$ relative to the leading order terms (which themselves are of $O(\alpha)$? I would like to see this clarified because there are terms of absolute magnitude $O(\epsilon\alpha^2)$ (or relative magnitude $O(\alpha^2)$) in (31), (32), and (34).*

   The higher harmonics will appear at $O(\epsilon\alpha^2)$ relative to the the leading order $O(\alpha)$ terms. But when such higher harmonics are combined with first-order primary harmonic to contribute to the evolution equation for the primary harmonic, that term will be $O(\epsilon\alpha^3)$, and hence negligible. I have reworded this section for clarification.

8. *I was confused by some of the terminology relating to the asymptotics: (b) The scaling of the mean flow is not done consistently. In (26) $\bar{u}$ has no scaling, but on line 260 we can infer that it scales like α^2. This factore of α^2 has then been magically divided out in (27), (28), (29), (30), (31), etc.*

   This was an oversight. I have now included α^2 in (26) for the mean quantities, and further discussed the scaling.

9. *Line 299: of course, there are terms $O(\epsilon^4)$ in (35), so not all term s higher than quadratic in small parameters have been dropped-but we know what you mean.*

   I have adjusted this equation for consistency.

10. *Equations (46) and (48): should $(u\eta)_x$ and $(u\eta)_\xi$ be $u\eta_x$ and $u\eta_\xi$? This has corrected itself by (50).*

    Equations (46) and (48) are both correct as written. If I use $u\eta_x$ on the left of (46) say, then I would need to add $w_z\eta$ on the right. I have used continuity to combine these two terms.

11. *There were several issues with dimensions. (a) In (19) and (20), I has dimensions of speed, assuming that α is dimensionless. However, I is dimensionless in (56).*

Equation (56) has been corrected to kI/N_1, instead of I.

12. *There were several issues with dimensions. (b) In line 443, a factor $N_1^2/c_p\sigma^2$ is introduced, which has dimensions of 1 over speed. However, in line 446 this factor is written as U (rather misleading for a quantity with units of 1 over speed!), and then on line (57) U appears as a dimensionless quantity! Indeed, I find that*

$$\frac{N_1^2}{c_p\sigma^2} = \frac{(k^2+n_1^2)^{3/2}}{N_1 k^2},$$

which is not even close to the expression you give: $U = 2(k^2+n_1^2)^{3/2}/k^3$.

The wording in my previous submission was 'Define U to be this factor, which after rescaling becomes,'. So I rescaled $N_1^2/c_p\sigma^2$ by multiplying by k/N_1, which gives

$$U = \frac{k}{N_1}\frac{2N_1^2}{c_p\sigma^2} = \frac{2(k^2+n_1^2)^{3/2}}{k^3},$$

exactly the factor I had previously. I have reworded this section to clarify this. I now define U in it's dimensionless form so there are no units.

13. *Section 6 is rather imtimidating at four pages with no subsections. To aid the reader demarcate the various ideas, I would suggest splitting into section 6.1 (set-up, lines 393-464), section 6.2 (properties of equations, lines 465-511), section 6.3 (non-dispersive evolution, lines 512-611), section 6.4 (dispersive evolutions, lines 612-711), section 6.5 (mean flow comparisons, lines 712-744), and section 6.6 (rigid lid cases, lines 745-769), or similar.*

This suggestion is helpful. I have included these subheadings in the manner that you describe. Note that the discussion on evolution without dispersion is now reduced as a result of the comments of other referees.

14. *Figure 4 (etc.): I thought that the labelling could have been better. In particular, what is shown is the wave magnitude (as stated in the caption), but the axes are labelled $k|I|/N_1$. This is fine for figure 3, but not for figures 4 and 5 (for example) where $|R|$ and $|T|$ are also shown.*

The explanation of the graphical styles (lines 539-542) would also be better in the caption to figure 4, with appropriate comment (i.e., same convection as figure 4) in the captions to figures 5,9,10, etc.

I now show A as a placeholder for $|I|$, $|R|$, and $|T|$ in the labels where appropriate, and I have expanded the captions as you suggest.

15. *Line 603: you say that the disturbance velocity 'appeas to be discontinuous' in figure 5. I wouldn't be able to make such a judgement by eye from a plot of* $|I|$, $|T|$, *and* $|R|$.

    I have reworded this sentence.

16. *Line 786: the interface is a region of strong shear even without viscosity!*

    Yes, I agree. But with viscosity the slip at the interface would be converted into a region with even stronger shear. Again I have reworded here.

17. *Conclusions. It is nice to see a better discussion of these new results with those of McHugh (2008) and McHugh et al (2008a). For the former, what was the value (or values) of* n_1/k*? For the latter, perhaps the observed waves were strongly nonlinear, and thus in a completely different regime to this weakly nonlinear study?*

    For McHugh (2008), $n_1/k = 0.37$, close to the case here with $n_1/k = 0.4$. For McHugh et al (2008a), yes, those waves were likely very large amplitude and hence strongly nonlinear.