A second review of

'Incidence and reflection of internal waves and wave-induced currents at a jump in buoyancy frequency',

by J. P. McHugh

The scope and aims of the article are the same as those of the original submission. This second version is much better in my view, with the paper properly balanced towards the analysis and results of section 6, and a better discussion in section 7. The paper is fairly easy to follow, and is generally well-written and illustrated, although some minor mathematical and typographical errors remain.

My only major concern relates to the apparent inconsistency of the amplitude equations (see below). Since the author has made many changes to the text since the original submission, there is also a list of smaller comments (below), some of which should certainly be addressed. However, I can now imagine this paper being published sooner rather than later.

Nature of asymptotic scaling

Upon first reading, a major concern would be whether or not the coupled amplitude equations (and the associated numerical simulations) 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 behaviour 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, with $O(1)$ group speed term and $O(\epsilon)$ dispersive terms and $O(\alpha^2/\epsilon)$ nonlinear terms retained in the amplitude equations, and $O(\epsilon)$ terms retained in the boundary conditions. This is often justifiable when there are simulations of the amplitude equations alongside simulations of the full nonlinear equations, and convincing agreeement is demonstrated in an appropriate regime. This is not done here. Neither is the reduced system shown to satisfy approximate conservation of energy (say).

I am guessing that a comparison with full nonlinear solutions is out of the scope of this study (although some are mentioned in the discussion), so perhaps the author can reassure me with something about conservation of energy. What I have in mind is simply calculations of domain-integrated energy and energy fluxes in and out of the domain for the numerical simulations of section 6, simply to show that there are no spurious sources or sinks of energy in the reduced system. This would give me much more confidence that the dynamics of the mean flows near the interface are realisable. This would be particularly valuable for cases such as figure 4, where it is not at all clear that energy is approximately conserved (compare 4(a) with 4(c)).

I raised this general issue in my original review, and the author replied that he had added some comments about the mixing of scales in the second paragraph of section 6 – although none are apparent. I think that a better explanation of what is going on is required. To me, one would start by noting that we have the minimal model that retains the three small processes of possible interest: envelope translation, dispersion, and nonlinear mean-flow interaction. To make these asymptotically consistent, one would ideally move into a frame of reference translating with the group speed, introduce a super-slow time variable $T = \epsilon^2 t$, and choose $\alpha^2 = \epsilon$, giving an asymptotically consistent model with all terms in balance. However, this is impossible here because of the three different group speeds in the problem. This seems to be the crux of why the model is asymptotically consistent, but I can't see this stated anywhere in the paper.

To me, the introduction of the variable $\tau = \epsilon t$ just obscures what is going on, because the real evolution of interest is happening on the even slower time scale $T = \epsilon^2 t$. That is, τ just accounts for the envelope propagation, whilst T accounts for the dispersion and nonlinear mean-flow interaction. This is consistent with your numerical results, which are shown for $N_1\tau$ ranging from about -80 to $+80$. Given that $\epsilon = 0.025$, this corresponds to $N_1 T$ ranging from -2 to $+2$.

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?

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?

3. At what point is the buoyancy frequency taken to be piecewise constant? On line 152 there is reference to buoyancy frequencies of 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 $\mathrm{d}N^2/\mathrm{d}z$.

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

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.

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.

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 are themselves of $O(\alpha)$), so that they have absolute magnitude $O(\epsilon\alpha^3)$? 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).

   (b) The scaling of the mean flow is not done consistently. In (26), $\overline{u}$ has no scaling, but on line 260 we can infer that it scales like α^2. This factor of α^2 has then been magically divided out in (27), (28), (29), (30), (31), etc.

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

9. 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).

10. 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).

    (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: $U = 2(k^2 + n_1^2)^{3/2}/k^3$.

11. Section 6 is rather intimidating at four pages in length 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.

12. 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 convention as figure 4) in the captions to figures 5, 9, 10, etc.

13. Line 603: you say that the disturbance velocity 'appears 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 $|R|$.

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

15. 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?

Minor errors, typos, etc.

1. Line 25: has $\rightarrow$ have.
2. Line 628: it's.
3. Line 746: completed.
4. Line 780: Schodinger.
5. Line 781: with with.