Review of "Incidence and Reflection of Internal Waves and Wave-Induced Currents at a Jump in Buoyancy Frequency"
by J. McHugh
Nonlin. Proc. Geophys. Discuss. 1, 269-315 (2014)

In my earlier review I pointed out what I considered to be several flaws in the theory presented by the author. Several points I raised were relatively minor - a matter of creating confusion for the reader rather than undermining the central premise of the work. However, I had pointed out a major theoretical flaw, which is that the interface conditions are truncated at an amplitude-order, inconsistent with the rest of the weakly nonlinear analysis. As a result, I believe the physics of the interactions between the waves and the wave-induced mean flow are not correctly represented. The author is unequivocal in his stand that the conditions are correct. It might have helped the author's argument if he had produced time series that could have been compared with simulations that have been published elsewhere. But this he has not done. Now he does show snapshots of the waves in simulations with $n_1/k = 0.4$ with uniform and step-stratification. But he does not do so in the case $n_1/k = 1/\sqrt{2}$ (the control case) and $n_1/k = 1$.

The author argues that his simulations are correct. But I argue that the equations he is solving are wrong to begin with.

I have suggested the author examine if transmission and reflection is a function of amplitude, as has been shown in earlier work by fully nonlinear simulations. The author has refused to do so stating the "nonlinear reflection does not occur at this order". This is nonsense. In a weakly nonlinear theory, weakly nonlinear effects can influence reflection. Even fully nonlinear simulations can capture linear evolution (if run with very small amplitude waves) and weakly nonlinear evolution (if run with moderately large amplitude waves). If the author's weakly nonlinear theory cannot capture aspects of what fully nonlinear simulations show when run with moderately large amplitude waves, then it is not a predictive theory.

Rather than re-argue my theoretical concerns, I have simply run a series of fully nonlinear numerical simulations using parameters provided by the author in his different scenarios. The simulations have convincingly shown me that the results of the author in the weakly nonlinear parameter regime he has examined are qualitatively incorrect. This I demonstrate below.

In trying to reproducing the author's results I found a typo in boundary condition (56) that launches the wave. The cosine part should have $\cos(2\pi\epsilon c_g\tau)$ with τ ranging from 0 to $1/(\epsilon c_g)$. (Note that I put the 2π in the cosine rather than in range of τ so that the wavepacket width is $1/\epsilon = 40$ as appears to be the case, for example, in Figure 3.)

The results of simulations run with the same parameters as those of McHugh in his Figures 3 and 4 are shown in Figure 1 below. The structure and amplitude of the induced mean flow at $N_1\tau = -75$ is the same as that of the author at this time (Fig 1a and d here, and Fig 3a and 4a, right panels in McHugh).

Clearly, even the author's control case of a wavepacket propagating in uniformly stratified fluid is incorrect. The simulations presented in this review clearly show that the wavepacket does not simply translate upward but its structure also changes. This is revealed by profiles of the wave-induced mean flow $\bar{u}$, which is related to the amplitude envelope squared (Figs 1b,c). The result is not surprising. It has previously been shown that weakly nonlinear effects modulate the envelope of a wavepacket with $m(= |n_1|) = -0.7$. For example, see the time series of $\bar{u}$ in Fig 3e (fully nonlinear simulations) and Fig 4b (weakly nonlinear equations) of Sutherland (J. Fluid Mech., v 569, pp 249-258 (2006)). If the author had compared his control simulation results with these time series, as I have suggested in the past, he should have suspected an the error and run his model with the parameters of that paper ensuring he could reproduce those results before confidently claiming his model was correct.

Proceeding to simulate the circumstance of the wave encountering enhanced stratification with $N_2 = 2N_1$ above $kz = 0$, the discrepancies persist. The structure and amplitude of the transmitted waves in Figure 1f, is significantly different from the broad disturbance above $z = 0$ in McHugh's figure 4c, (right panel). On top of the inability of McHugh's model to capture the weakly nonlinear evolution in uniformly stratified fluid, I fully believe the discrepancy is due to the author incorrectly employing interface conditions at $z = 0$ that neglect weakly nonlinear effects. This I have claimed in previous reviews, but the author has been insistent that his equations are correct. Outside of the direct comparison of results, it appears to me that the vertical integral of $\bar{u}$ in figure 4c (right panel) of McHugh does not equal the vertical integral of $\bar{u}$ in figure 4a (right panel). As is well established in the literature, momentum conservation requires the vertical integral of $\bar{u}$ to be constant over time. If the author does not have the correct interface conditions, then momentum conservation is violated, a fundamental flaw.

I have also run simulations with $n_1 = 0.4k$ and $n_1 = k$. These likewise show significant discrepancies for waves incident upon a layer of enhanced stratification with $N_2 = 2N_1$. In summary, the simulations I have run with the parameters provided by the author differ qualitatively and quantitatively from those of the author. I can be confident in the validity of my simulations, because they produced qualitatively and quantitatively similar results computed separately by solving the nonlinear Schroedinger equation that describes the weakly nonlinear evolution of internal wave packets. And they conserve momentum.

I must emphasize that at its core I do like the *idea* of the author, particularly with regards to its application to enhanced winds at the tropopause. But this is a theoretical paper and so it is paramount that the theory is correct in its fundamentals. It is not, and so I do not find the paper acceptable for publication.

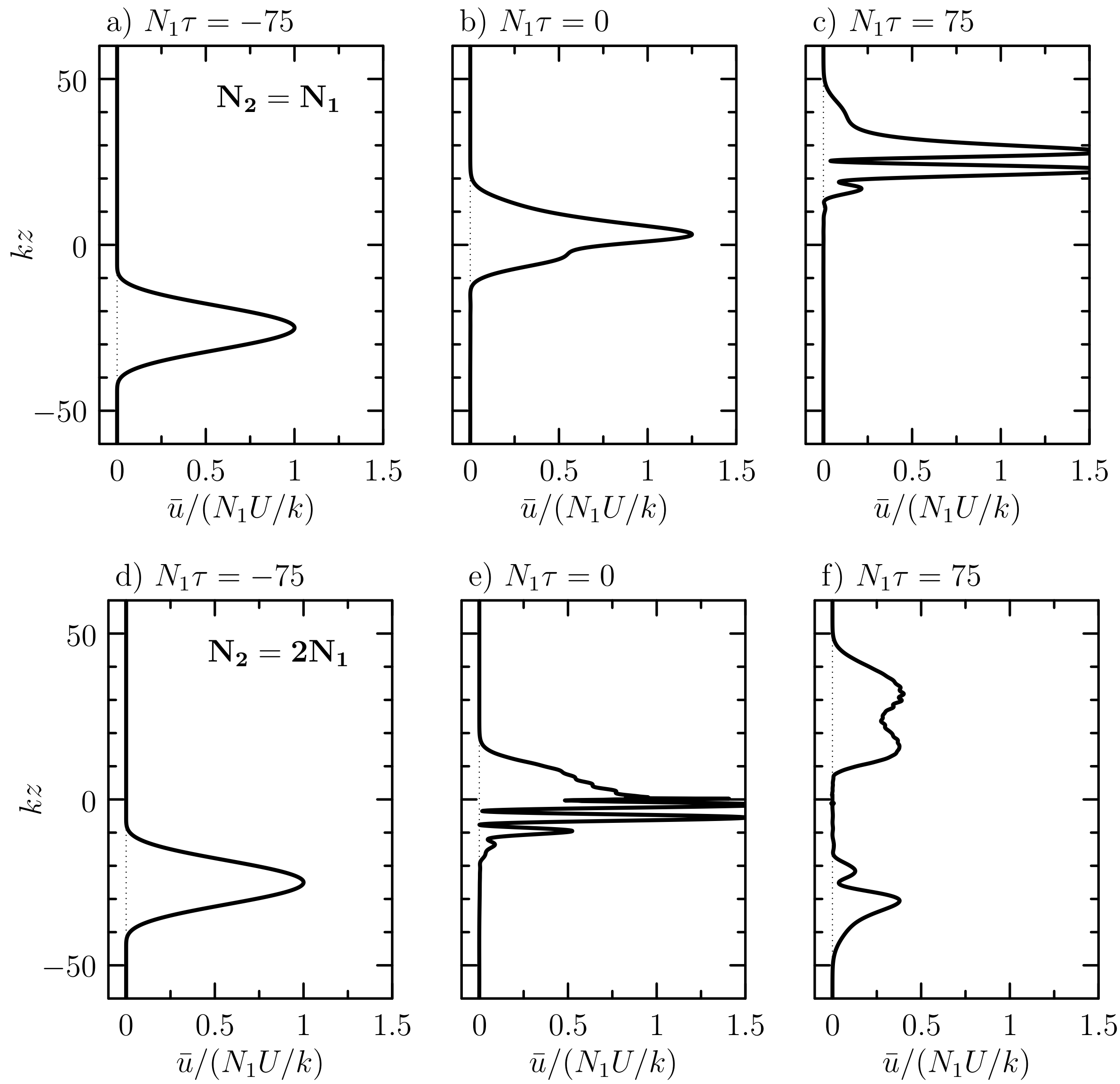

Figure 1: Profiles of the induced mean flow $\bar{u}$ for a wavepacket in uniformly stratified fluid computed at times a) $N_1\tau = -75$, b) 0 and c) = 75 and for a wavepacket in stepwise-constant stratification at times d) $N_1\tau = -75$, e) 0 and f) $\tau = 75$. In all cases $n_1 = k/\sqrt{2}$, $\alpha = 0.1$ and $\epsilon = 0.025$. The stepwise-constant stratification has $N_2 = 2N_1$ with $N = N_2$ for $kz > 0$. By construction, plots a,b,c) should correspond to the rightmost panels of Figs 3a,b,c) and plots d,e,f) should correspond to the rightmost panels of Figs 4a,b,c) in the manuscript of McHugh. That they do not, demonstrates the equations being solved by McHugh do not correctly capture the weakly nonlinear evolution of the wavepackets.