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)

I have twice acted as a reviewer of this paper in various forms when it was submitted to a different journal. Both times I found the manuscripts unacceptable for publication primarily because the derivation of the nonlinear Schroedinger equation (NLS) was nothing new and the results did not well explain how the discontinuous jump of the wave-induced mean flow-induced depended upon the wave and background parameters. My detailed reviews pointed out several deficiencies in 1) Theory, 2) Presentation and Interpretation of Results and 3) Notation.

In this latest submission to Nonlinear Processes in Geophysics, I see that few of my major concerns have been addressed. Fundamental flaws remain in all three areas. As such I find the resubmitted paper unacceptable for publication.

Just as the author did not take time to consider my review, so will I simply repeat the comments of my earlier review with references to equations and page numbers changed accordingly.

1. Theory

   (a) Three pages (pp 278-280) are needlessly devoted to the derivation of the wave-induced mean flow. He says he follows Acheson (1976) and Scinocca and Shepherd (1992), but does nothing of the kind. The former gives a contorted derivation based on wave action and the latter gives a derivation based on Hamiltonian fluid dynamics.

   As I stated before, Its derivation is well-established (Section 3.4.5 of the textbook "Internal Gravity Waves" by Sutherland (2010)) and the derivation of the author is contorted besides.

   Equation (26) confusingly gives $\bar{u}$ as if it is order α^0, but is then said to scale as α_0 in (32) and finally it is revealed that $\bar{u}$ is in fact order α^2 in (37). This should have been self-evident from the start, if the author had begun with the horizontal momentum equation as done, for example by Dosser and Sutherland (2011), which had previously been cited by the author but is not referenced here.

   Rather than starting with horizontal momentum to derive the mean horizontal flow, he starts with vorticity (27-28) and in (29-30) erroneously switches to a hybrid co-ordinate system involving both z and $\zeta = \epsilon z$. If he is performing a multi-scale analysis he does not say so much. He then converts from vorticity to buoyancy and then gives results in (40-41) below in terms of I and R, which work with vertical velocity, not buoyancy. (Incidentally, it is only implied that R in (18) represents vertical velocity.)

   One of key results, (38) is is not related back to well-established results for the pseudomomentum of Scinocca and Shepherd or the two other representations of the wave-induced mean flow given in the textbook of Sutherland (2010).

(b) Despite all the confusing manipulations, it is evident that there is a theoretical error in the author's paper. In (15) the author defines a translating horizontal co-ordinate, $\xi = x - c_p t$, that moves horizontally at the horizontal phase speed. And yet, the author arrives at (38), which is the the mean flow found by others in a stationary frame. If the author's theory was rigorous, the two results should have differed by a factor $-c_p$.

(c) These issues are relatively minor. But the major theoretical flaw, which I mentioned in my previous two reviews and which persists in this paper, is that the interface conditions are incorrect. The conditions (61-62) have been truncated at order α^2 and so neglect the weakly nonlinear effect of the wave-induced mean flow acting back upon the waves. But the NLS equations (50), (53) and (54) are order α^3 (equivalently $\epsilon\alpha^2$, since $\alpha = \epsilon$) and so do include this effect.

The author should be aware this is a flaw. In my earlier reviews I suggested the author refer to Sutherland (1996) which examined transmission and reflection of small and moderately large wavepackets from strong to weak stratification. (That this relevant paper continues not to be referenced at all is strange oversight.) The transmission and reflection coefficients were clearly shown to be a function of amplitude through the fully nonlinear simulations presented in that study.

(d) One last point: it would give the reader more context and confidence in the results if the author related them to existing work where possible. For example, regarding (5.6) it would would be good to compare the NLS equation in the case $R = 0$ to the result of Sutherland (2006).

2. Presentation and Interpretation of Results

(a) Despite raising this concern in past reviews, most of the figures continue to show snapshots at three times instead of time series and contour plots.

I think the numerical results of the author are incorrect and time series might convince me otherwise. If the author made contour plots and time series of the control simulations with uniform stratification (Figs 3,7,13), as best as I can tell they would not look like the corresponding plots given by Sutherland (2006). (Incidentally, the author makes not mention of Sutherland (2006) regarding modulational stability and instability. Despite comments in previous reviews, the author continues to cite Whitham (1974), who said nothing specifically about modulational stability of internal waves as the text implies (eg p 286, second line).)

In particular:

-contrary to the author's implication through Fig 3, moderately large wavepackets with $n_1/k = 1/\sqrt{2}$, do narrow and peak, though not as dramatically as modulationally unstable wavepackets (Sutherland (2006), Figure 3e);

- the peak in the case $n_1/k = 0.4$ (Fig 7) should move to the rear instead of

the front of the wavepacket (Sutherland (2006), Figure 3d);
- the wavepacket in Fig 13a doesn't look like it has a truncated cosine amplitude envelope at the outset.

(b) The author insists that ϵ and α (the nondimensional amplitude) must be equal. Even if assumption constitutes part of the rigorous derivation of the NLS equation, the equation itself describes small amplitude dispersive waves simply by initializing with small amplitude waves, making the weakly nonlinear terms, $|I|^2 I$ etc, negligibly small.

If the author's results truly require the wavepacket width to be set by the amplitude instead of being independent quantities, then the application of the results to realistic circumstances are drawn into question.

3. Notation

(a) The paper continues to be exceptionally difficult to read and interpret. The author introduces several equations and variables which are unnecessary and sometimes described in a confusing manner. An example is given my comments, 1(a), above.

Here are few more, though not all, examples I could mention:

-Blatant typo on 3rd line of abstract and missing article right at the beginning of the first line of Introduction. If the author shows so little care spell-checking and proofreading, how can the reader have confidence in more complicated results of the paper?

-It is incorrect to say n_1 is the vertical wavenumber: as it is given in (16), $-n_1$ is the vertical wavenumber.

-What is the practical reason for moving in a frame with the horizontal phase speed? It makes all interpretation of the wave-induced mean flow in text and figures confusing.

-The origin of the rescaling given by U in (68) is unclear, if it is not incorrect. For example, the textbook of Sutherland (2010) suggests the wave-induced mean flow should increase as $\sec\Theta = \sqrt{k^2 + {n_1}^2}/k$.

-It makes no sense to have $\mathcal{R} = -1$ in equation (73).

(b) Also, I have great difficulty backing out physical variables from the control variables given by the author. The wavepacket width is supposed to be represented by ϵ, but its definition in (67) uses Q instead of ϵ for a proxy of wavepacket width.

Then, when the author presents results in Figure 3, etc, no mention is given of the value of ϵ or Q, or when the simulation began for that matter (Figure 13 makes me think it is not the time of the top row of plots).

If the reader cannot test this work against previous studies and if the reader does not have enough information to adapt the work to reality, then it serves no purpose being published in any journal.

I wish to reiterate that I raised most of the above concerns in my most recent review of the paper submitted to another journal. But few of the concerns have been addressed here. To my mind this shows lack for consideration for the time spent by reviewers to provide constructive criticism. And it shows lack of respect for Nonlinear Processes in Geophysics, which demands the same quality as the journal to which this paper was submitted previously.