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Ocean Science Discussions

Interactive comment on "Tidal modulation of two-layer hydraulic exchange flows" *by* L. M. Frankcombe and A. McC. Hogg

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Our thanks to the referee for their thorough review. We have made substantial changes to the manuscript in this revision, and believe these changes have greatly improved the manuscript.

Our explanation of tidal resonance in this model was questioned by both referees, and we accept that we did not sufficiently investigate this effect in the first version of this paper. Since receiving the reviews we have performed additional tests on the data and taken advice from colleagues on this issue. This has resulted in a major change to the paper, namely that, through the addition of two new figures (figure 6 and 8 in the revised manuscript) we can now determine that the effects we observe are not solely due to resonance. Instead, we show that, while the effect of resonance dominates in one particular case, in other cases the internal dynamics of the baroclinic system

dominates the response to tidal effects. This result substantially changes the paper and adds to its significance.

Referee 1 has two additional primary points in their review. We respond to these points below:

1. The referee requests a discussion of the places and/or situations in which the findings would be of use. The primary regions of interest are noted in Helfrich (1995). We have noted some of these sites in the revised Introduction, and have added a paragraph in Section 4 noting the relevant values of γ for these sites.

2. We agree that the values of wavelength and height are inconsistent with the hydrostatic approximation. As pointed out by the referee, in the geophysical applications of these results, the hydrostatic approximation is certainly valid, because of aspect ratios (i.e. H/ℓ) which are of the order of 10^{-2}–10^{-3}. To address this comment we have re-derived the governing equations in non-dimensional form. Furthermore, we note in Section 2.1 that we assume small aspect ratio.

In response to the four specific comments:

1. We have altered this sentence to say that “Helfrich (1995) showed that tidal variations may exceed the frequency over which the quasi-steady solution is valid." In addition, we have added a physical description of this process.

2. As mentioned above, we agree that these results are only valid in the low aspect ratio limit, and have modified the manuscript to reflect this.

3. The manuscript describing the boundary conditions is still under review with Ocean Modelling. The first reviews were positive and we expect the manuscript to be accepted shortly. We are happy to provide the latest draft of this paper to the editor if required.

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4. The original figure 4 was erroneous and this has now been corrected. A very small barotropic component remains, and this has been explained in the text of the revised manuscript.

Finally, regarding the technical corrections, we have changed dimensional variables to dimensionless so that all variable have no units.

Interactive comment on Ocean Sci. Discuss., 3, 1999, 2006.