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Discussions

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

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General comments

The exchange of fluids of different densities through a contraction subject to tidal motion – or, in general, (long) barotropic waves – is an interesting problem, which have received some attention before and which may be relevant in a number of places. As such I find the manuscript a significant contribution to the ongoing debate on problems related to hydraulic control.

I have three points of criticism, which I think need to be addressed before the manuscript is ready for publication. The first is that the manuscript lacks a discussion of places and/or situations in which the findings would be of use. The authors have made an effort to compute the effects of tidal modulation for a wide range of the parameter γ and so make the results broadly available. It shouldn't be too hard for the authors to find examples of real exchange flows that are modified by tidal currents or other barotropic, transient motions in connection with which the their results can be discussed.

Second, one of the primary assumptions, that of hydrostatic pressure, is usually fulfilled in natural situations. But for the present calculations it may be violated for a considerable part of the parameter space, which I will explain in more detail below. This is in part connected to my first point of criticism because the contraction that is subject to the investigations has dimensions of roughly half a meter by half a meter (width and depth), and so it is far removed from the real situations that the authors are trying to mimic ultimately. This isn't a big problem because the scaling, used in Figures 5 to 8, make the results generally accessible. However, the circumstance implies that the findings are not as applicable as it may seem. The authors should discuss this and point out the limitations.

Third, the resonance in the model, which causes some weird peaks in the exchange flux and which ends up contributing considerably to the authors' discussion, raises doubt about the validity of the results. This is particularly so because the authors eventually decide to employ a choice of parameters for which the resonance is not so apparent. Therefore it is not clear whether the results are better and/or explain phenomena not accounted for by the other studies cited by the authors (Helfrich, 1995 and Phu, 2001). The authors clearly state that they "do not investigate the source of resonance in this paper'' (page 2008, line 8). I think, however, that they need to do this in order to show that their approach is valid.

Specific comments

On p. 2001, l. 5 it says that "it can be shown that tidal variations may exceed the frequency over which the quasi-steady solution is valid.'' It would be nice to see the details of this, and it would aid a discussion of the relevance and applications of the

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investigations.

As mentioned above, the momentum equations (3–4) are based on the assumption of hydrostatic pressure. A measure of when this is fulfilled is a wave length to depth ratio of 10 or more (see any text book on small-amplitude, linear waves). Using the authors' symbols this is equivalent to $T\sqrt{g/H} > 10$. Rewriting γ I find that $\gamma = T\sqrt{g/H} \times H/l \times \sqrt{g'/g}$. Taking the numbers that the authors end up using ($g'/g = 10^{-3}$, $H/l \approx 1$), the results should hold for $\gamma \leq 0.3$, which is about the upper half of the parameter space investigated by the authors. For the other half the hydrostatic assumption doesn't hold, implying that the motion induced by the barotropic ("tidal") waves is confined to the upper part of the water column. Therefore the exchange due to the wave modulation is reduced, and the solution curves in Figure 5 should approach zero more quickly. There should also be a noticable effect on the data shown in Figures 6 to 8, although focus here is on a shorter range of γ. In realistic situations, in which H/l is more like 10^{-2} or less, the valid range of γ is larger, but the authors need at least to be aware of this problem and discuss it adequately. In fact, the barotropic wave travelling through the computational domain shown in Figure 3 has an apparent wave length of roughly the length of the domain, which is near the lower limit of the validity of the hydrostatic assumption.

Section 2.2, some of the details of the open boundary conditions, which play an important role in the experiments, are described and discussed in the submitted manuscript by Nycander et al. (2006), and so at this point I'm unable to comment much on them.

Figure 4, the barotropic flux at the left hand boundary is always positive. Does that mean that there is a net barotropic flow toward the left, i.e. the non-perturbed situation is not a pure exchange flow? This isn't clear.

Technical corrections

Units are lacking in several places. The ones that I have spotted are the colorbars of Figure 2 and 3 (which I assume show velocity, but this isn't indicated anywhere), L and

g' on p. 2008, l. 5, and the legend of Figure 5. Also, it is not clear whether the height and flux amplitudes of Figures 6 to 8 are dimensionless or not. In case of the latter, units are also lacking here.

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