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Ocean Science
Discussions

Interactive comment on "Laboratory experiments on the influence of stratification and a bottom sill on seiche damping" *by* Karim Medjdoub et al.

Karim Medjdoub et al.

mvincze@general.elte.hu

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We thank the referee for reading our manuscript and for the insightful and useful comments. Below we reply to the raised issues point by point.

Comment:

A quantification of the seiche period should be possible just from the stratification and tank geometry, but that is not done. Adding this would make the paper stronger and confirm that this is the mechanism.

Response:

The evidence supporting our statement that the observed frequencies of maximum

spectral amplification coincide with internal seiche mode frequencies is presented in Fig.6b of the manuscript in a way that is, to our understanding, equivalent to calculating the seiche frequencies (periods) that the referee suggests.

The internal wave dispersion relation curves shown in the plot are calculated from the parameters of the stratification only (using the theoretical formula given in eq. 3). The observation that the peaks of the measured maximum amplification (represented here with the color scale) occur at the integer values of non-dimensional wave number L/λ (where L is the length of the tank) confirms the hypothesis that the amplification peaks are indeed coincide with frequencies associated with standing waves on the pycnocline i.e. internal seiche modes. In the revised version of the manuscript, we will make the presentation of this result clearer and emphasize its significance more.

Comment: Also, is there a delay in the seiche generation compared to the immediate IW? That would shed light on how the seiche is generated, e.g., if it draws from the surface motion or is set up by the IW.

Response:

Based on the referee's comment we have investigated this issue by taking the Wiener filtered time series of the pycncline displacement in the frequency bands of the surface oscillation and of the internal seiche mode at which the linear amplification (transfer function) was the largest. As an example, two typical filtered time series are presented in the attached Fig.1 of this rebuttal letter for the case of dominant surface wave modes m = 1 and m = 2 (experiment configuration #3). Unfortunately, however, our findings here were not entirely conclusive in terms of the delay between the two signals in any of the investigated cases.

It is to be noted, however, that marked low-frequency oscillations (and large transfer function peaks) were present in the cases of dominant surface wave modes m = 2 and m = 4 too (see Fig. 4 of the manuscript), where immediate IW generation at the obstacle is practically inhibited, as the dominant surface seiche then has an antinode above the obstacle. This observations thus suggest that the presence of immediate IWs is not necessary for the excitation of the internal seiche modes.

Comment:

It is also shown that with an obstacle, there is a faster decay than with a flat bottom. The problem I'm having here is that there are not enough quantifications of a lot of the results mentioned in the text, and the physical mechanisms are only described briefly. More details are needed throughout on what is happening for a physical point.

Response:

Indeed, the main purpose of the present manuscript was to demonstrate the fact that the presence of a bottom obstacle reaching up to the pycnocline contributes significantly to the damping of the surface seiche. This may sound trivial, but to our surprise, we found that this simple mechanism was never actually investigated in laboratory experiments before, whereas various theoretical and field observational works have been dealing with the issue in very similar geometrical settings of the ocean system, e.g. fjords.

However, unfortunately (and unintentionally) we did not cite our paper published in Experiments in Fluids, dealing with a rather similar laboratory setting to the one studied here (Vincze and Bozóki 2017, temporarily made available for the reviewer at: http://karman3.elte.hu/mvincze/pub/13_exp_fluids_obstacle.pdf). In that work we extensively discussed the mechanism of internal wave generation above the obstacle and analyzed the velocity field using the technique of particle image velocimetry (PIV). There, however, we applied oscillatory forcing at the water surface, and therefore could not explore the dynamics of damping. Thus, in the present work our focus was indeed on this particular aspect, and we did not intend to duplicate our earlier work related to the wave generation. In the revised version of the manuscript, we will certainly add this reference and will summarize its key findings in the Introduction.

Comment:

The main concern I have, however, is if this paper fits in Ocean Science. The exploration is very lab-based and theoretical, and while I really don't mind that, there are very few links to the real ocean. The concerns above requires substantial revisions linking the results to a geophysical setting to make the paper publishable in OS, but I actually think it may be better housed in a more theoretical fluid mechanics journal.

Response:

Our exploration is lab-based and theoretical indeed, and now in retrospect we also agree with the referee that a fluid mechanics journal may have been a better fit (although we note that some papers discussing laboratory experiments have already been published in OS). However, the very reason we picked OS was the fact that this setting closely resembles the geometry of the ones studied by Stigebrandt and colleagues in their works related to barotropic-to-baroclinic energy transfer in fjords (e.g. the Gullmar fjord in Sweden) which we refer to in the introduction. These works investigate a semi-enclosed basin with seiche modes on the surface, a sharp density jump at the pycnocline and a topographic obstacle that reaches precisely up to the pycnocline, see the attached Fig.2 of this response letter, taken from Stigebrandt (1999).

We agree that neither this analogy with fjords nor the fact that the profiles are typical was emphasized enough in the previous version, hence we added a paragraph discussing these aspects to the Introduction section. To our understanding, fjords are a part of the ocean system, therefore we assumed – maybe incorrectly – that this experimental demonstration of the phenomenon may be of interest for the community. In the revised version of the paper we intend to emphasize these links more in the Introduction and the Discussion.

We also thank the referee for the minor comments: we agree with all of them and we will correct the text and figures accordingly.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-114, 2020.

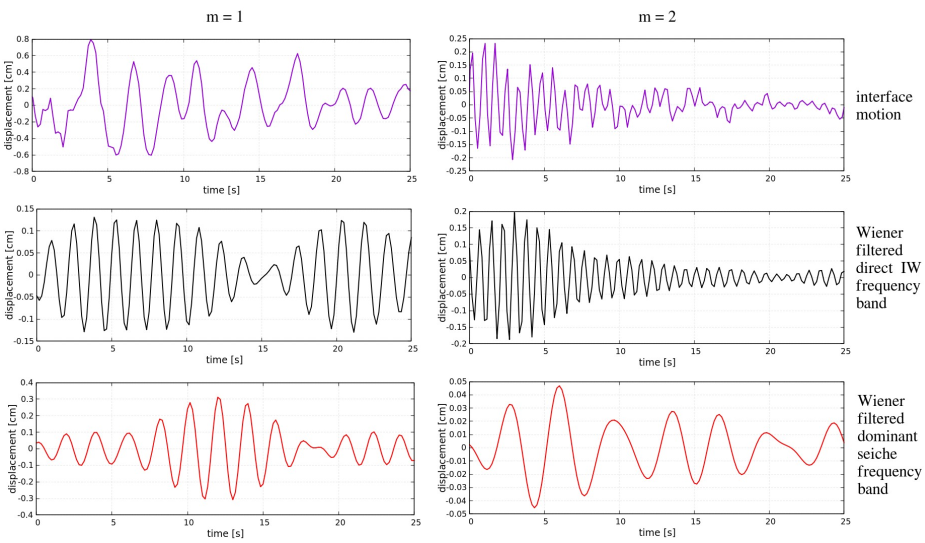

Fig. 1.

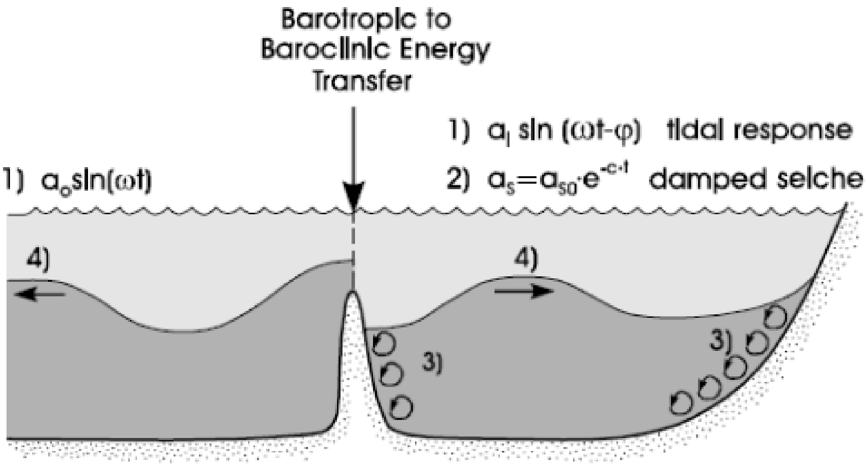
Barotropic to
Baroclinic Energy
Transfer
1) $a_i \sin(\omega t-\varphi)$ tidal response
2) $a_s=a_{s0}\cdot e^{-c\cdot t}$ damped seiche
1) $a_o \sin(\omega t)$
4)
4)
3)
3)

Fig. 2.