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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 and for stating that "a revised version of this paper might be suitable for publication in OS". Below we address the raised issues point by point.

Comment:

1. The experiments do not address the generation of seiches over the continental shelf, where the domain is "semi-infinite. The seiche generation mechanisms discussed rely on quantization of the wavelength along the axis of the tank, or between the obstacle and the ends of the tank. Clearly, if the domain is semi-infinite the seiche generation

mechanisms will be modified. A discussion is required about this. Indeed, the authors have overlooked the study by Davies, Xing and Willmott (2009) Ocean Dynamics, 9, 863.

Response:

We thank the reviewer for the suggested reference, that we will cite and briefly summarize in the updated version of the manuscript. As far as the surface seiche modes are concerned, the main difference between the closed and semi enclosed basins (i.e. gulfs) separated by a sill from the "semi infinite" open ocean is that in the latter case a node must be present at the obstacle (as also discussed by Davies, Xing and Willmott), similarly to the odd (i.e. antisymmetric) modes of our setting, in which the obstacle is situated in the midpoint of the tank. As for the internal seiche generation, a semi enclosed setting would have seiche modes between the end of the domain and the sill (as in our experiment) but no such standing waves could develop on the semi-infinite side. These differences indeed have to and will be mentioned when linking our setting to a natural setting of a gulf or fjord with an associated sill.

Comment:

2. The role of topography in seiche generation leaves for questions than answers. Why this shape of topography? Why is it always at a fixed point in the flume? From an oceanographic perspective it would be more interesting to have a representation of the continental shelf and slope. As it stands, the experiments discussed in this paper have at best tenuous relevance to the ocean.

Response:

We agree that this situation is not typical for the open ocean. However, this setting (a quasi-two layer stratification with an interface at sill depth) is often considered as a "quite good approximation of real fjord stratification because particularly dense water that occasionally refills the basin is trapped by the sill" (Stigebrandt, 1999), see also the sketch attached to this response letter (Fig.1) taken from that reference.

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.

Comment:

3. The way the seiches are generated looks rather crude with the configuration of six foam bumpers. I am not convinced that you can accurately deform the free surface into the prescribed waveforms. Why not fabricate a solid material (planiform) with a surface that represents a linear external standing wave as characterised by the along channel modal number m?

Response:

It is indeed true, that the surface cannot be deformed to prescribed waveforms with our excitation method, but our intention was not the excitation of pure modes. As indicated by eq. (1) the oscillation of the surface that actually develops in the system at the $x = 0$ location (left sidewall) is always a sum of various modes. Our goal was to acquire the natural frequencies and the frequency-dependent damping coefficients from the data by fitting the formula. Therefore, theoretically, any random initial surface shape could have provided the same information, since the Fourier components can be considered independent of each other in the linear approximation. Our "quasi pure" initiation method was applied because of practical reasons. As mentioned in the manuscript, even with this crude initiation a two-term sum ($N = 2$ in eq. 1), i.e. the combination of two eigenmodes was found to be sufficient to account for > 90% of the observed

variance in all cases, making the regression (the acquisition of eigenfrequencies and the corresponding damping coefficients) much easier. We will modify the text under eq. (1) accordingly, to emphasize this aspect in the updated version of the manuscript.

Comment:

4. The presentation of the results in the paper is sloppy. Please include a figure of the side elevation of the tank showing the two layer fluid, the depths H1, H_2, h, L, delta rho etc. The "golden rule" is that each mathematical symbol MUST be defined when it is first introduced in the paper. The authors appear to be unaware of this rule!

Response:

We thank the referee for the suggestion about the figure. We will add such a sketch to Fig.1 in the updated version of the manuscript. Although, it appears from the manuscript that, in accordance with the "golden rule" each symbol is defined in the text upon its first appearance (even if in a "sloppy" manner, for which we are sorry). However, these can still be rather inconvenient to find, we will therefore add a table to section 2 enlisting all the symbols that appear in the manuscript, alongside their definitions.

Comment:

5. Why is there a problem with the m=5 standing wave? Using a more refined way of setting up the initial free surface displacement may well resolve this problem.

Response:

Indeed, a surface oscillation with an $m = 5$ dominant standing wave mode could surely be excited with a more refined wave maker. However, as mentioned in our reply to comment 3, we did not intend to excite pure eigenmodes, and we could in fact observe $m = 5$ mode as a harmonic (albeit not as a dominant mode).

Comment:

6. Figure 3, and elsewhere. A colour scale is required.

Response:

A color scale has been added.

Comment:

7. The analysis of time series of the interfaces was only conducted near the end walls of the flume. Why not at other locations.

Response:

This position was selected to ensure that all standing wave modes have antinode – i.e. maximum amplitude – at the measurement location. Due to the boundary conditions the endwalls are the only such locations in the tank. A temporal spectrum taken here (and only here) has the same Fourier amplitudes as the total (space- and time-dependent) spectrum. Whereas (as an extreme case), at the midpoint of the tank, where all odd modes (m = 1,3,5,. . .) have nodes, one would get zero amplitudes for all these components in the local spectrum. Thus, the endwall appeared to be the best location for the analysis.

Comment:

8. The paper has not been thoroughly proof read which is off putting for the referees.

Response:

We are terribly sorry and we are very grateful for the referee for listing the mistakes. These (and more) will be corrected in the updated version of the manuscript.

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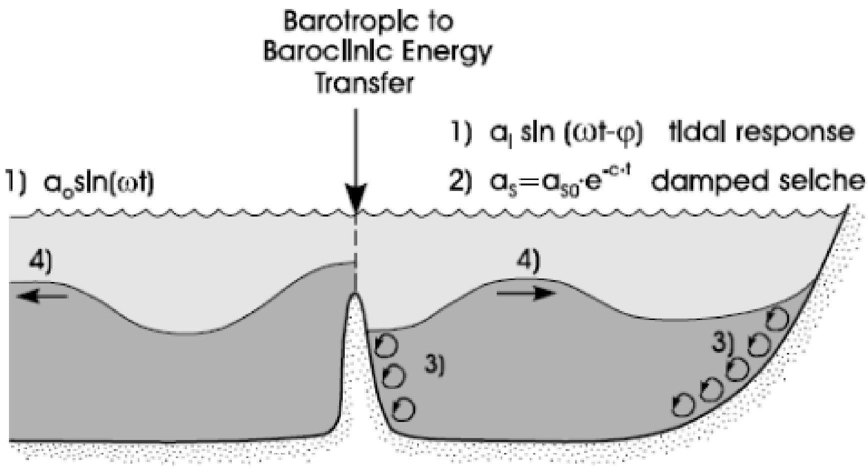

Fig. 1.