

Answers to Referee #1

In response to **comments 1, 2 and 5** of the reviewer, we want to clarify that the numerical simulations did not aim to reproduce the mean circulation of the lake and its interaction with the basin-scale wave field. Rather, it was our objective to complement the field data on the analysis of the structure of the seiches measured. In the revised version now we explain that the structure of the internal standing waves depends only on the bathymetry and on the stratification. So, the internal seiches can be analyzed without considering the mean circulation. In fact, some previous authors have based the analysis of the internal wave patterns on unforced models (Salvadé et al., 1988; Guyennon et al., 2014) (commented on P4 L30 – P5 L2).

On the other hand, from all the possible standing waves, the internal seiches that are excited depend on the wind forcing, which is why we have forced the POM model with the wind prior to and during the campaigns. According to Sakai et al. (2011), wind forcing events persisting for some fraction of the wave period excite wave modes and this excitement can persist for several periods. For the purpose of this work, a 5-day spin-up period of the model was found to be enough for the standing waves to develop, although a longer spin-up period might be needed to accurately describe the circulation of the lake. Lorrai et al. (2011) also used a spin-up period of 5 days of the 3D hydrostatic Boussinesq model to study the seiching dynamics of a medium-size lake. Note that if the POM model had not reproduced the modes studied in this paper, the surface elevation filtered around the periods of the observed oscillations would not have presented a coherent structure throughout the lake as it is demonstrated in the Results section (P5 L3 –L10).

Regarding **comment 3**, the model was initialized with the temperature and salinity profiles measured in the field so the upper mixed layer was already introduced on the initialization. Some previous published studies, e.g. Lorrai et al. (2011) used the same initialization scheme. A clearer sentence has been added to the revised text (P4 L10).

For the analysis of the horizontal structure of the internal seiches, no transition from sigma levels to z-levels was required (**comment 4**) because the study is based only on the structure of the surface layer. Avoiding interpolation between different vertical levels is, in fact, an advantage of the method (P5 L19 – L24). It is true, however, that for the analysis of the vertical structure of the waves, such a transition is made but only at a deep station in the center of the lake where no interpolation problems arise.

The noise in some of the filtered surface elevations in the southern part of the lake (mentioned by the reviewer in **comment 6**) is unfortunately highlighted by the contour levels chosen for a better visualization of the horizontal structure of the wave over the whole lake.

We acknowledge and appreciate the reviewer for his comments which helped to clarify important aspects in the manuscript and improve it as a result.

Answers to Referee #2

We regret the reviewer's complaint about the language. The paper had been reviewed by a native speaker. Now it has been reviewed by another native reviewer who has more experience with scientific texts.

After reading the reviewer's comments, we realized that when we presented the results we were distracted by the historical development of the work and didn't present the results in a logical way. The 2006 data were analyzed prior to the 2013 campaign. At that time, we found that while the observational and numerical results matched well, the vertical structure of the waves could not be assessed based only on the bottom velocities. That's why we never tried to publish the results but we presented them at a conference (Forcat 2013). After the 2013 campaign, we realized that the data recorded within the water column could help to assess the vertical modes and so we analyzed them. Unfortunately, we presented the results starting from 2013 and, because the 2006 results had not been published, we incorporated them in the paper, but we should not have mentioned Forcat (2013) which only confuses the reader.

After considering the reviewer's General comments, **we have changed the structure of the Results and Discussion sections.** The results are presented starting in 2006, and the plots have been modified accordingly. We have added a small introduction to the Results section that mentions the main characteristics of the stratification and the wind conditions which are described previously in the Materials and Methods section. The plot with the characteristic density profiles has been complemented with the corresponding temperature and salinity profiles. Results of the 36 h mode from 2006 are now presented (Fig. 6a)).

The Discussion section has been organized according to the horizontal and the vertical structure of the internal waves. In it we comment on the features that favor the excitation of the different modes and the reasons for the differences observed between 2006 and 2013 (P10 L11 – L15; P11 L10 – L23). Finally, although the fluxes related to the internal seiches cannot be assessed using these data, based on our results, a brief discussion of the relevance of the evolution of the internal seiche field for the Aral Sea and other shallow lakes in the current climate scenario has been added (P10 L15 – L21; P11 L23 – P12 L6; P12 L18 – L24).

As mentioned in the previous version of the manuscript, differences of morphometry and stratification make it difficult to compare the observed sub-inertial internal waves in the Aral Sea with those studied in other lakes. Therefore, after reading the reviewer's comment, we removed the last paragraph of the Discussion section and added the references given there to the Introduction section. More recent works have now been added to the Discussion section.

The reviewer's specific comments have also been addressed in the new version of the manuscript. In our opinion the reference to the Defant/Mortimer model does not contradict to works by early researchers treating the lake as multilayer systems after mentioning Mortimer (1979). Note that the work of Heaps (1961) had already been mentioned in the first manuscript after the reference to Mortimer's work.

We acknowledge and appreciate the reviewer's constructive comments. We believe they have helped considerably to improve the manuscript.