We appreciate the Referee valuable remarks, recommendations and his profound corrections and carefully addressed them in the new version of the manuscript. . Our answers on the major comments can be found in the text bellow and full answers are given in the Supplement materials.

On behalf of all authors,

Elena Zakharova

Referee 2 major comment

The oscillations studied in this paper occurred in a model where some parameters were adjusted to unrealistic values in order to reduce damping (l. 182-183). It would be interesting to see some discussion on how the results obtained relate to the sea level behaviour in the real Baltic Sea.

How much are such oscillations expected to contribute to the real sea level variability? Is there a possibility that the parameter adjustments affect the oscillation frequencies?

Reply:

1.The parameters of adjustments affect the dissipation speed and do not affect the frequencies. If we do not reduce these parameters we can't catch the oscillations of seasonal scale. *We edited corresponding phrase in the text for clarification.*

2. A comparison of tide gauge and numerical simulation spectra is shown on figure XX . The real sea level fluctuation is the superposition of forced and free oscillations of different origin. The tide gauge spectrum contains very big amount of peaks those amplitudes are significantly higher. In contrast, the spectrum of simulated free oscillations is characterised by a small number of peaks of lower amplitudes which are masked by forced oscillations. So, we decided to exclude comparison of real and simulated spectra from the discussion.

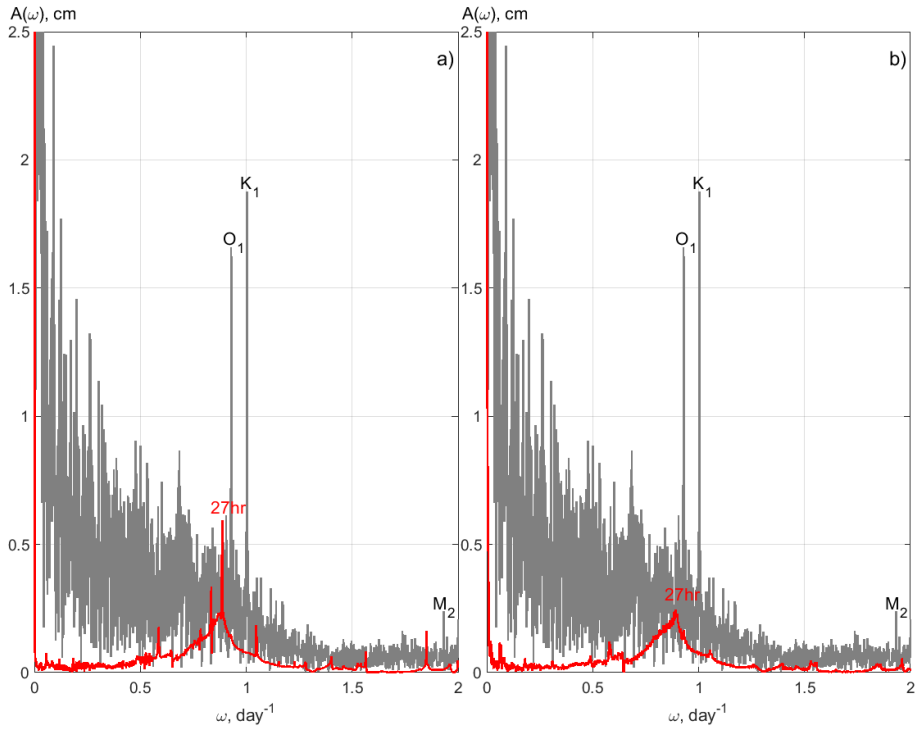

Figure XX. Amplitude spectra A(ω) of tide gauges at Helsinki station for 2009-2010 (grey line) and of simulated free oscillations in barotropic (a) and baroclinic (b) conditions (red line).

How fast would they be damped?

Reply: The answer for this question is given in the Section 2.3. "Under natural conditions, the free sea level oscillations attenuate rapidly due to the dissipative effects of vertical and horizontal viscosity, near-bottom friction, non-linear effects, and Earth's rotation (Proshutinsky 1993, Zakharchuk et al., 2004). According to a theoretical concept and previous numerical experiments (Proudman, 1953; Wübber and Kraus, 1979; Zakharchuk et al., 2004; Leppäranta and Myrberg, 2009), the relaxation of the Baltic large-scale free sea level oscillations takes several days."

Fig. 8: Why is there so much white space in these maps? The areas around the oscillation nodes are apparently excluded due to low amplitude. But why are e.g. phase speeds for the eastern Gulf of Finland missing in Fig. 8b, even if the amplitude of the oscillation should be high (Fig. 7b)?

Reply: We introduced additional phrase to the caption of corresponding figures and following phrase into the body text. "In areas where ΔF_x and ΔF_y equal to zero (white areas on fig 8), the standing wave component prevails."

In a seasonal scale in the baroclinic simulation, after all the external forcing ceases, I would assume that something happens to the temperature and salinity distribution also. Were such processes considered, and how would they affect the surface height?

Reply. Yes, we would also expect some changes in T,S fields and their potential effect on free sea level oscillations of seasonal scale. Indeed, considering that these oscillations occur only in baroclinic conditions, they can be related to spatial variability of the T and S (e.g. water density). However, an investigation of this interesting problem was out of scope of presented manuscript and will be studied in future.

l. 499-502. Most of the interannual variability in the seasonal sea level fluctuations likely originates directly from the interannual variability in the atmospheric forcing. E.g. the role of the air pressure conditions, the NAO index, etc., have been shown to explain a significant portion of the interannual variability. Thus, I suppose the contribution from the baroclinic free oscillations is minor. (Which might be mentioned.)

L. 499-502.

We agree that the interannual variability in the seasonal sea level fluctuations is apparently related to interannual variability of seasonal fluctuations of the wind and atmospheric pressure. This fact was supported by many researches (Ekman and Stigebrandt, 1990; Ekman , 1998; Plag and Tsimplis, 1999; Stramska et al, 2013; Barbosa and Donner, 2016; Cheng et al, 2018). This is also true that our estimated amplitudes of free baroclinic oscillations of seasonal scale are low (2,5 – 5,5 cm). Nevertheless, these amplitudes are of the same order as the amplitudes of annual Baltic Sea level variability (4 - 13 cm) estimated using stationary approximation from the tide gauge observations for 60 year period (Ekman, 1996). *Corresponding phrase was added to the Discussion section*. Indeed, for non-stationary process observed annual amplitudes are higher and can reach 30-40 cm (Ekman and Stigebrandt, 1990; Medvedev, 2014). In our Discussion section we put forward the idea that the role of found in our study baroclinic free oscillations under combination of specific conditions (resonance, favorable stratification), of cause, occurring not each year, might be non-negligible. And this question requires more clarification in future studies.

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l. 144-145: sigma_p is not the relation of sigma_m and sigma_tg (as stated), but sigma_er and sigma_tg.
The correction was introduced."The standard deviation of the observed (σ_{tg}) and simulated (σ_m) sea surface height.."

l. 145-146: Please give the definition of P_m. Now it remains unclear which measure should be <0.674*sigma_tg.
The phrase was corrected. "The P_m parameter allows the assessment of the NUMBER of good simulations (comparing to total number of outputs) considering their accuracy < 0.674σ_{tg}."

l. 201: ...a_k and b_k are the coefficients...
The correction was introduced.

l. 202: "mean average" => "mean" or "average"
The correction was introduced.

l. 207-211: Is the period P same as T above? If so, please use the same symbol. If not, please explain.
The correction was introduced: P was changed for T in the text and corresponding equations.

Fig. 4: The original amplitude of the displacement ranges from -50 to +100 cm in Fig.3. How does this relate to the plots in Fig. 4 starting from around +10-20 cm at every station? What is the vertical axis in these?

The Fig 3b shows the sea level under the meteorological forcing, while on the Fig4 we demonstrate the simulated FREE se level oscillations AFTER cessation of the forcing. For more details on comparison of free and real oscillations see the figure XX above.

Fig. 4: It would ease the comparison of amplitudes if all subplots had the same y axis (as they are very close to each other already).
The correction was introduced. The Y-axis scale is unified.

l. 229 and Fig. 5: What is "standard deviation of amplitudes"? If this is the standard deviation of time series, as Eq. (4) implies, then the word "amplitude" here is misleading.
The title was changed for "Sea level standard deviation of free barotropic oscillations.."

l. 230: Please check the font size of all subscripts, here and elsewhere.
The correction was introduced.

l. 233: Consider adding locations of Pärnu Bay and Rügen Island to Fig. 1.
The map on the Figure 1 was updated.

l. 235-236: "Oscillations of medium intensity can be noted as over local uplifts in the Baltic Proper as over-bottom depressions..." Please reformulate this sentence.
The sentence was rewritten. " Oscillations of medium intensity occur in area of local uplifts in the Baltic proper, in area of the Ulvö Deep, the Landsort Deep, the Northern Deep and the Gotland Deep."

l. 261: There is a 17-h period listed. However, there are no 17-h peaks in Fig. 6. This should be 16-h?
Thank you for catching this error. The text was edited. "In our experiment, the corresponding periods were 31, 27, 23, 20, 16, and 13 hours "

l. 310: Leppäranta and Myrberg, 2009
The correction was introduced.

l. 311: From Eq. (13), a range of 12-67 m/s corresponds to depths of 15-458 m. From where does the lowest limit of 15 m come? Please specify.

Again, thank you for your vigilance. We verified data used against data of (Lepparana and Myrberg, 2009). The correct range of mean depths in studied areas are 15-77 m and the range of maximal depth is 14–458 m. Necessary corrections were introduced

l. 321: "anemobaric forcing": please specify what is meant by this. Is "anemobaric" a synonym to the entire meteorological forcing described on lines 118-119?
Anemobaric forcing was changed for meteorological forcing.

l. 322: "30-35 cm in amplitude" => "in range"? In Fig. 9, the largest range of variations seems to be 35 cm. This is not amplitude, which by definition is half of the total range.
The correction was introduced: "30-35 cm in range"

l. 330: "standard deviation of the amplitudes"; see comment above.
Correction was made.

l. 336: Ellesmere Island? Please check the name, and add location to Fig. 1.
We meant Öland Island area. Correction was introduced.

l. 362: "Free oscillations of 27 h periods in the baroclinic conditions reached the maximum in the narrow zone near the southwest Finland coast". There seems to be a much more apparent maximum in the eastern Gulf of Finland in Fig. 12b, please check this sentence.
Thank you, we agree with remark. The corresponding changes were made.

l. 382-383: "Dash line on the histogram plots indicates minimum theoretical value of phase speed of baroclinic (Ci) and barotropic (Cg) gravity waves." It looks like the dash line for Ci indicates the maximum value (1.53 m/s), not minimum.
We agree with remark. The corresponding changes were made.

l. 412: May be more specific here: "significantly lower for 358-day waves and belong to the theoretical range for 89-day waves". I see this is what is meant, but "longer" and "shorter" are a bit too generic and it is hard to understand the sentence.
The text was rewritten and, we hope, now is more simple and clear. " For 358-day waves, our estimations of the phase speed are significantly lower than that of the theoretical internal gravity waves (C_i). For 89-day waves, the part of our estimations belongs to the range of phase speed of internal gravity waves."

Fig. 15. Line 388 says the Ci range is 0.08-1.53 m/s. Why is the maximum lower here?
Thank you for this remark. The figure was redrawn. The line corresponding to max value 1.53 m/s was removed as it is out of x-axis limits. The XLim was left as previously as at a larger range the details of the histogram will be invisible.

l. 467: decreases => increases
Thank you for catching this error. The correction was introduced.

l. 479: It would be helpful to mention explicitly the period of inertial oscillations in the area (about 14 hours).
The correction was introduced.