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Discussions

Interactive comment on "Spatio-temporal structure of Baltic free sea level oscillations in barotropic and baroclinic conditions from hydrodynamic modelling" *by* Eugeny A. Zakharchuk et al.

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We appreciate the Referee valuable remarks and recommendations 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 1 major comment

The description of the background and the research problem is professional. I would only recommend to (i) include reference to a generic overview of physical oceanography of the Baltic Sea (Leppäranta and Myrberg, 2009) already in the Introduction, (ii) mention an early but deep and still actual overview of the problem [Samuelsson, M., Stigebrandt, A. Main characteristics of the long-term sea level variability in the Baltic Sea. Tellus A, 48(5), 672-683, doi: 10.1034/j.1600-0870.1996.t01-4-00006.x, 1996], and (iii) discuss the results for the baroclinic calculations, if applicable, in the light of some recent advances towards better understanding of low-frequency baroclinic oscillations of the Baltic Sea [Kurkin, A. et al. Spatial distribution of energy of subinertial baroclinic motions in the Baltic Sea, Frontiers in Earth Science, 8, 184, doi: 10.3389/feart.2020.00184, 2020], with recommended focus on material on lines 398–407.

Reply: Thank you. We included the references to the Leppäranta and Myrberg, (2009); and Samuelsson and Stigebrandt, (1996) overviews as recommnded (see page1). However, Kurkin, et al,(2020) does not investigate the water level variability in baroclinic waves, therefor the reference to this publication would not be fully correct.

The hydrodynamic model INMOM is professional and up-to-date (although not much used as the input for international research publications). The idea is to spin up the system for a certain time interval under the impact of a strong atmospheric forcing, and then let the system run at its own, with the goal to detect the maximum number of self-oscillations over two years. To do so, the authors implement several simplifications that are not fully realistic but eventually help to detect various kinds of self-oscillations. In essence, the system of detected oscillations should be invariant with respect to the initial disturbance; however, this feature should be at least shortly discussed (and at best with some supporting evidence from other parts of the World).

Reply: 1. Thank you for this feedback. We agree with the Reviewer that in our definition

of task, simulated free sea level oscillations should be invariant relative to initial perturbation. Moreover, the agreement of our results with the results of cited publications proves this statement. In spite of difference of forcing conditions used in different studies, the resulting spectra of free barotropic oscillations are quite similar. Unfortunately, we have not found publications for other World, where the baroclinic modes of free oscillations were investigated using numerical experiments based on hydrodynamic model and were not able to discuss this question.

Also, in this context it would be important to explain why in both the barotropic and baroclinic implementations, the Baltic Sea was considered a fully enclosed basin, with no water exchange with the North Sea as stated on lines 175–177. It is also important to comment shortly on possible differences with runs that would resolve water exchange between the North and Baltic Sea. Also, it remains unclear whether river water input and ice conditions were also neglected in both the barotropic and baroclinic implementations (line 177).

Reply: We introduce requested details to clarify this remark (see section 2.3). 1."This assumption aimed to exclude the effect of external barotropic and baroclinic oscillations coming from the North Sea". 2."River water input and ice conditions were also neglected in both numerical experiments."

On lines 182–183 it is said that “Setting the turbulent viscosity to zero for the vertical components and to the minimum values for the horizontal components allows the damping of the simulated sea level fluctuations to be reduced.” Please comment whether you did so and, if yes, how the modelled spectra relate to the actual spectra of motions. Again, I guess, the results are qualitatively invariant with respect to the particular set of settings, and stronger damping would simply render some self-oscillation patterns undetectable.

Reply. We agree with the remark and edited the text for better reading: "In order to be able to characterize the free oscillations with better spectral resolution and in larger

spectral range, the sea level dumping factors have to be reduced. In both numerical experiments, the dumping effect was reduced due to 1) setting the coefficients of vertical turbulent viscosity and of bottom friction to zero and 2) setting the coefficient of horizontal turbulent viscosity to the minimum values."

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-110/os-2020-110-AC1-supplement.pdf

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