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Ocean Science Discussions

Interactive comment on "Winter stratification phenomena and its consequences in the Gulf of Finland, Baltic Sea" *by* Taavi Liblik et al.

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Comment: This manuscript documents the formation of wintertime haline stratification in the Gulf of Finland due to freshwater transport and discusses its implications for early plankton bloom dynamics. The authors combined water column temperature, salinity and fluorescence data from two along-Gulf transects in winters 2011–2012 and 2014, cross Gulf measurements of surface T-S collected with a Ferrybox system and 10 years of GETM-modelled mixed-layer depths. Altogether this is a powerful dataset that allowed to a thorough documentation and description of an interesting phenomenon, which has implications for (usually disregarded) winter primary production in the area. Overall, my view on the manuscript is quite positive and I would be happy to see it published. Still there are several formal issues that need to be addressed before pub-

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Discussion paper

lication. I also feel that the description of some aspects of the dynamics of the system could be described in more depth. I develop this further below but, for example, the general seasonal wind patterns in the area, and how they relate to expected advection patterns are poorly discussed in my view. The authors have nice model simulations and a set of references to better describe the advection dynamics of the system in response to changing winds. I would suggest to develop this aspect a bit more. I feel that if the authors could condensate this information together with their own conclusions in a schematic figure that would help a lot and make the manuscript more shinny and visual.

Reply: Thank you for the review and helpful comments! We have addressed all of the points you have highlighted in the revised version of the manuscript. Action: As we explain below, the seasonal wind pattern and its role is now dealt in the paper. We added thematic figure (Fig. 12 in the manuscript), which explains the UML distributions in the case of westerly winds or easterly winds dominating. We have addressed all the detailed comments below.

Comment: Line 179. Be careful with the positioning of parenthesis for references.

Reply: We fixed it. Action: Done

Comment: Wind pattern. The wind pattern (Fig. 6) is strikingly similar for the three years shown here, with strong westerly winds until January and weaker more variable winds after that. Is this the typical seasonal pattern in the region? I think this is a very important point for your message that is not very well developed in the manuscript. You focus more on interannual variation and the links to NAO, but what are the expected seasonal variations of wind forcing during the studied period. Is this transition from strong westerlies to weak variable winds over winter a persistent pattern? Then this is very important for the onset of wintertime stratification. Could you develop this a bit more please?

Reply: Yes, we agree. This is a very good point you made. Thank you! Yes, it is part

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of the annual cycle of wind. The cycle is not as persistent as it looks from the three selected years, but it exists. There is a period from October/November to January when there are more westerly storms and after that, when atmospheric high pressure systems sustain, it calms down. The timing and magnitude vary from year to year though.

Action: We have added a new figure showing the annual cycle of along gulf wind stress with references to it in the section 3.2 (Results), Discussion, Conclusions, and also in the Abstract.

Comment: Figure 7 and lines 212-227 I like Figure 7, it is quite illustrative, but the only really new information displayed in this figure is the mixed-layer depth. Consequently, some of the information provided in lines 212-227 becomes somewhat repetitive. As the paper has a long number of display items I would suggest to show the MLD already in Figure 6.

Reply: Yes, we agree with the comment.

Action: We removed the text in 212-227 and added MLD to the figures 5 and 6. We made three minor changes in the previous two sections, just to mention the upper mixed layer depth there. Otherwise, we think the section was repetitive, as you noted.

Comment: Figure 8. Could you highlight in the caption the location of the starting point of the transects (x = 0 km)?

Reply: Yes, that is a good idea.

Action: We added, “the starting point of the transect (x = 0 km) is in the Bay of Tallinn at 59.500° N and 24.752° E.“

Comment: Line 237 “Spreading from the east to west”. This information is not really contained in the Figure. In my view it is a bit confusing to include it in the middle of this sentence which is, otherwise, a pure description of the information that is being displayed.

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Reply: Yes, we agree.

Action: We removed “Spreading from the east to west”, but added next sentence to explain the freshwater origin: “Since the main sources of freshwater are in the east, the water must have flown westward along the northern coast.“

Comment: Line 245 Which year are you talking about? Also I am curious about the fact that the onset of haline and, more importantly, thermal stratification seems to have taken place early than in the previous years. Is this related to variability in wind forcing?

Reply: We talked about years 2014 and 2016. We added an explaining sentence. The earlier onset in 2016 is related to the wind forcing. One can see it in figure 4c. The westerlies eased off earlier in 2016 compared to other two years. We added a sentence about it to the manuscript.

Action: we changed “Fresher water first appeared in the northern part in the first half of January both in 2014 and 2016. The onset of haline stratification took a place slightly earlier in 2016. This is associated with the wind forcing, westerlies eased off already at the end of December (Fig. 4c).“

Comment: Figure 11 The x-labels are placed in a strange way in this figure. Do the ticks correspond to the 1st of January of each year? Why is the label to the right of the tick? The color scale for MLD in panel b) is reversed with respect to Figure 10. This confused me.

Reply: We agree and fixed the issues.

Action: We solved the problem with ticks and we put the color scale the same way as in the previous figure.

Comment: Lines 280-294. I think this part is very interesting but needs to be improved. From Figure 11 it is a bit hard to compare the timing of stratification on-set in the different years. I would try to rethink this figure a bit and find a better way to make your point. Also the winter NAO index is an important element here. I would add this

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information to the figure somehow.

Reply: Thank you for this recommendation.

Action: We added another subplot, where one can see detailed time-series of UML in different years. We also added the Dec-Feb mean NAO index to the second subplot.

Comment: Line 305. In my view “vertical movements of the pycnocline” due to upwelling, internal waves, etc, are transient and have a mostly reversible effect on buoyancy fluxes unless part of their energy is irreversibly lost to turbulent mixing. I would avoid mentioning them or explain better what you mean.

Reply: We agree.

Action: We removed “vertical movements of the pycnocline”.

Comment: Lines 326-328. “The western border of the phenomenon is around 23âŮęE, i.e. at the entrance area to the gulf between Hiiumaa Island and the Finnish coast. Vertical mixing dominates over lateral buoyancy fluxes, and shallow stratification is not a common feature in the Baltic Proper.” I find this quite sharp boundary intriguing. Could you add some reference for this or develop a bit more this subject? Why is this change in regime, is the Baltic proper much more wind exposed so that haline stratification is completely eroded? Or is it that some dynamical process precludes the advection of freshwater out of the Gulf?

Reply: Yes, this can be mentioned here. We think the feature can occur in the Gulf of Finland because of the two factors: the high riverine input and elongated shape. The phenomenon vanishes in the area, where the extension of the gulf (at the entrance of the gulf) gets wider. Likewise, it is simply located far from the main freshwater sources (Neva river and others).

Action: We added the following text to the manuscript: “The absence of the phenomenon in the Baltic Proper can be explained by the long distance from the main rivers and due to the topography. The riverine input per unit area in the Gulf of Finland

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is 7-8 times larger than in the Baltic Proper (Leppäranta and Myrberg, 2009). One can note that the wintertime stratification phenomenon vanishes at the wider entrance area to the Gulf of Finland. Thus, likely the elongated and narrow shape of the gulf accounts to the formation of the stratification besides high freshwater input."

Comment: Lines 343–345. This sentence needs a reference.

Reply: We agree.

Action: We added Smetacek and Passow (1990) and Fennel (1990).

Comment: Figure 12. I don't like this figure very much. There is very few data available for interpolation. Why don't you use a scatter plot of biomass (with a color/size code) superimposed to a salinity contour plot? This would maybe make your point stronger.

Reply: We agree, this figure can be better designed.

Action: We have changed the figure according to your recommendation.

Comment: Color scale. In the contour figures you use a highly non linear colormap which strengthens low values a lot. I feel that sometimes the use of such a colorscale can be misleading, as it attracts the attention of the reader to this very low values, and sometimes this is not the most relevant aspect. I would suggest that the authors re-think a bit this choice for certain figures.

Reply: We agree.

Action: We have changed the color scales of figs. 5, 6, 11, 12 (according to the first submission numbering).

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