Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-40-RC1, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Ocean Science
Discussions

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

Anonymous Referee #1

Received and published: 5 July 2020

General comments

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 publication. 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.

Specific comments

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

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?

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.

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

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.

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?

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.

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 information to the figure somehow.

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.

Lines 326-328. "The western border of the phenomenon is around 23°E, 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?

Lines 343–345. This sentence needs a reference.

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.

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.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-40, 2020.