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_____ Anonymous Referee comment

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RC1: 'Comment on sp-2025-7', Anonymous Referee #1, 13 Mar 2026

This is my first review of a manuscript by Raudsepp et al. titled “Long term trends and variability of the mixed layer depth in the Baltic Sea” in which the authors analyze mixed layer depth from energetics perspective in the Baltic Sea and associate its long term changes with ecosystem changes. The manuscript is interesting but I do have three major points of critique. I think addressing them is straightforward, after which I think the manuscript can be considered for publication.

Major comments:

1) I think basing the manuscript on annual analysis is problematic. The physical processes that contribute to the trend are quite different between winter (generally surface forcing acts to weaken stratification) and summer (generally surface forcing acts to strengthen stratification), and as the authors say, the seasonality is strong in the region. I would ask the authors to do the analysis on seasonal basis, perhaps using JFM, AMJ, JAS, OND to align with seasonality in the Baltic Sea.

We thank the reviewer for this important comment and we will add seasonal trends of the MLD to the revised manuscript (ms).

2) The manuscript is to a large extent motivated by the changing MLD affecting the ecosystem through deoxygenation and nutrient delivery to the surface. This narrative ends up with Figure 4 suggesting differing ecosystem impacts on the eastern/western side of the Baltic Sea (arguably there is more of a coastal/open ocean divide) – although the analysis done is solely physical. My strongest problems are in fact linked to Figure 4. The impression that a casual reader gets from Figure 4 is that the oxygen concentration and primary production are going up, whereas cyanobacteria is going down in the Gulf of Riga and Gulf of Finland. I realize that the authors perhaps indicate that the increased MLD in these regions promotes such change, not that such net change has necessarily happened, but since I don't think a real reduction in e.g. cyanobacteria blooms has been reported (e.g. <https://doi.org/10.1016/j.hal.2019.101685> or <https://helcom.fi/wpcontent/uploads/2023/03/BSEFS-Cyanobacteria-biomass-1990-2021.pdf> and https://helcom.fi/wpcontent/uploads/2025/08/BSEFS-on-cyanobacteria-biomass_2000-2023.pdf), I think the infograph cuts too many corners. I would ask the authors to clarify the text, add citations (also for oxygen and primary production trends), and adjust the figure such that the reader does not get the wrong impression.

We agree with the reviewer that the original wording and Fig. 4 may overstate the connection between the diagnosed MLD trends and observed ecosystem changes. Our intention was not to present direct evidence of changes in cyanobacteria, oxygen, or primary production, but rather to discuss possible implications of changing mixing conditions for phytoplankton dynamics and oxygen conditions, in order

to place the physical analysis into a broader interdisciplinary context. We will therefore revise the text and infographic to more clearly distinguish between the physical results and their potential ecological effects.

3) It is a bit unclear how to interpret the PEA in the form the authors have decided to diagnose it. I am more familiar with the PE defined as:

$$PE = \int (g * z * \rho)$$

and PE anomaly compared to reference state, e.g. to one in which the stratification is completely mixed ($\rho_{ref}[T_{ref}, S_{ref}]$)

$$PEA = PE - PE_{ref} = \int (g * z * \rho) - \int (g * z * \rho_{ref}) = g \int (z * (\rho - \rho_{ref}))$$

Using your equation (1) seems to compute the energy needed to mix the water column below the MLD. However, in reality most of the mixing (especially in the Baltic) is driven from the surface down and thus what really happens is that MLD waters are mixed deeper down and that requires energy.

Figure 3c shows clearly that the authors diagnostic is very sensitive to the bottom depth. Basically, I would suggest doing one of the following

1) compute PEA as a difference between the given stratification and its fully mixed reference state, integrating from the surface to some fixed depth, such as 100 meters, which would reach below the halocline in most places. Alternatively, you could integrate down to the bottom and/or show the trend as percentage.

2) compute PEA as you did, but show the trend as percentage.

Personally, I think approach (1) is more interesting, as it answers the question 'how much energy do I need to increase MLD to a given depth?'.

We agree that the reviewer's concern likely arose from ambiguity in how the PEA diagnostic was described. We also agree that full-depth PEA is a standard and physically interpretable measure of total water-column stratification. However, our aim here is to interpret MLD trends rather than only total stratification. For this purpose we have implemented the stratification below the diagnosed MLD, since this represents the density barrier resisting further mixed-layer deepening. To address this we will calculate full-depth PEA and its corresponding spatial trend distribution, and compare it with the below-MLD PEA approach before deciding which diagnostic is most appropriate to retain in the manuscript. We will also revise the text to better explain the motivation for using the PEA diagnostic and to avoid similar misunderstandings.

We will not use a fixed 100 m integration depth, as this is not physically consistent across the Baltic Sea. Large parts of the basin are shallower than 100 m, while the halocline depth, MLD, and bottom depth vary substantially between sub-basins. A fixed-depth integration would therefore mix fundamentally different stratification regimes across the domain and would not directly represent the local density barrier controlling MLD evolution.

Minor:

Figure 3: I prefer showing data with discrete colormaps (at the moment it is quite hard to estimate what the trend really is in e.g. Gulf of Finland), and I would also show the non-significant P-values regions by e.g. white dots on the map. However, I leave it to the authors to decide if they want to modify the plot.

We will revise Fig. 3 using a discrete colormap to improve the readability of regional trends. We will also include the spatial distribution of p-values as embedded subfigures.