

Authors Responses

_____ 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 have added seasonal trends of the MLD to the revised manuscript on Figure 4.

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 (now Fig. 5) 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 have therefore revised the text to make this distinction clearer, softened the wording throughout the section, and now describe the ecological consequences as potential mechanisms rather than direct outcomes. We have also added relevant citations and clarified that phytoplankton productivity, oxygen conditions, nutrient cycling, and cyanobacterial blooms are controlled by several interacting factors, not by MLD alone.

The infographic and its caption have also been revised to emphasize that the figure illustrates **potential** relationships between MLD trends and ecological responses.

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 thank the reviewer for this important comment. We agree that the original wording did not explain the purpose of the PEA diagnostic clearly enough and could give the impression that we intended to diagnose the literal energy source of vertical mixing. This was not our intention. In the revised manuscript, we clarify that the below-MLD PEA is used as a diagnostic of the stratification barrier beneath the diagnosed mixed layer, not as a statement that mixing is driven from below. In a surface-forced system, further deepening of the mixed layer requires entrainment of denser water from below the MLD; therefore, the stratification immediately beneath and below the mixed layer is directly relevant for interpreting MLD variability.

To test whether our interpretation depends on the chosen vertical integration range, we repeated the trend calculation using three related diagnostics: below-MLD normalized PEA, full-depth normalized PEA, and above-MLD normalized PEA. These results are shown in Fig. R1. The below-MLD diagnostic and the full-depth diagnostic show very similar spatial trend patterns and comparable magnitudes, especially in the Baltic Proper and the major deep basins. In contrast, the above-MLD contribution is weaker and mostly opposite in sign. This indicates that the full-depth PEA trend is mainly controlled by the stratification changes below the mixed layer, while the already mixed upper layer contributes much less to the dominant trend pattern.

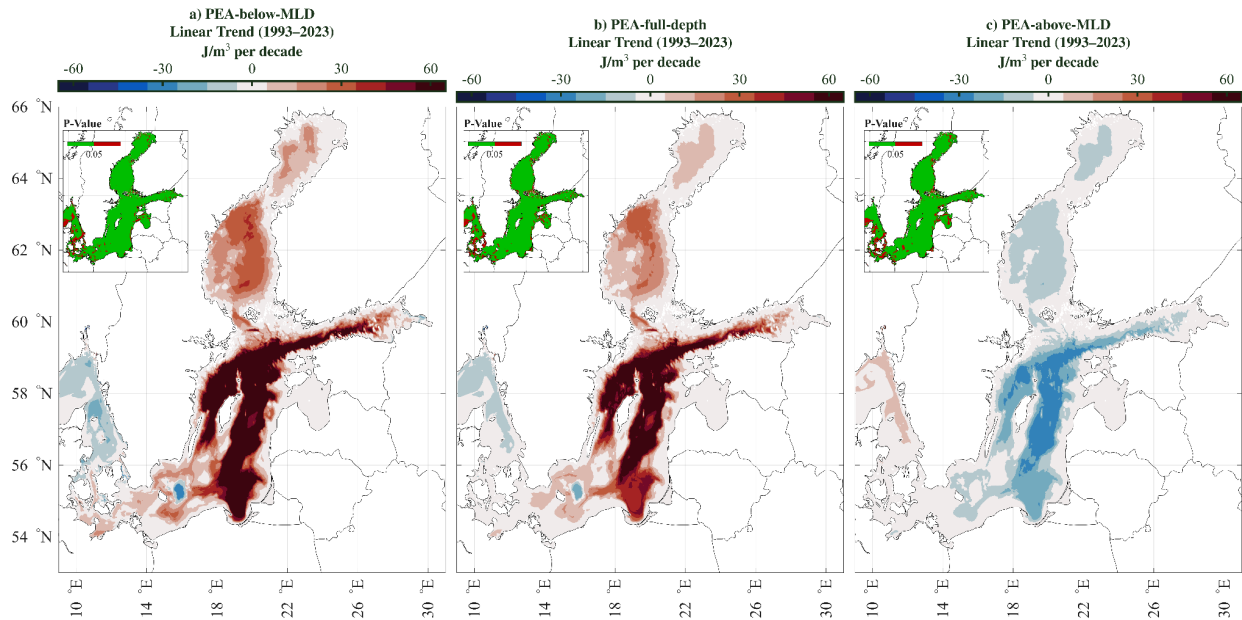


Figure R1. Additional check of PEA trend diagnostics used to evaluate the reviewer’s concern. Linear trends are shown for (a) full-depth normalized PEA, (b) above-MLD normalized PEA contribution, and (c) below-MLD normalized PEA contribution over 1993–2023.

We therefore retained the below-MLD normalized PEA as the main diagnostic because it is most directly connected to the question addressed in the manuscript: why and where the mixed layer becomes more or less resistant to further deepening. The full-depth diagnostic is physically meaningful as a measure of total water-column stratification, but it is less specific to the interpretation of MLD trends. The sensitivity test confirms that using the below-MLD diagnostic does not change the main interpretation relative to the full-depth PEA.

We did not adopt a fixed 100 m integration depth because this would not be physically consistent across the Baltic Sea. Large parts of the basin are shallower than 100 m, and the MLD, halocline depth, and bottom depth vary substantially between sub-basins. A fixed-depth integration would therefore combine different local regimes and would not consistently represent the density barrier controlling mixed-layer

evolution. It would answer a different question, namely the energy anomaly associated with mixing to a prescribed depth, rather than the local stratification constraint on the diagnosed mixed layer.

Accordingly, we revised the manuscript text to define the below-MLD PEA more explicitly and to state that it is used as an index of the stratification barrier beneath the mixed layer. We also clarified that the diagnostic should not be interpreted as a complete mechanical energy budget for mixing, but as a physically motivated stratification measure for interpreting spatial trends in MLD.

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 have revised Fig. 3 using a discrete colormap to improve the readability of regional trends and corrected the PEA units. We have also included the spatial distribution of p-values as embedded subfigures.

RC2: 'Comment on sp-2025-7', Anonymous Referee #2, 06 May 2026

Review of the manuscript “Long term trends and variability of the mixed layer depth in the Baltic Sea”.

General comments

This manuscript reports on studies of long-term trends in MLD in the Baltic Sea, and also discusses the implications for the ecosystem. It is a nice and fairly well-written manuscript that is easy to read. The figures are also well done, though I have a request for either an additional figure or a remake of one of the figures (the MLD trend; see my specific comments below). I recommend the paper to be published after minor corrections.

Specific comments

l. 9: “...reanalysis data set...”

Has been corrected

l. 20: “...more stratified which...”

Has been corrected

l. 49: “...Baltic proper...” (...and many other places in the manuscript. Many people write proper with Upper-case P, but grammatically it should be lower-case p, as proper is not part of the name.

Example: France proper means mainland France, i.e. not including French Guiana.

We thank the reviewer for this comment. We agree that "proper" is normally written in lower case when used as a general adjective referring to the central or mainland part of a geographical region, excluding peripheral regions, bays, or sea areas. However, in the present manuscript, "Baltic Proper" is used as a formal geographical name for a Baltic Sea subregion, not as a descriptive phrase. For example, Rolff et al. (2022) use "Baltic Sea proper" in the article title in the grammatical sense, while the term “Baltic Proper” is used consistently with a capital "P" within the main text when referring to the recognized Baltic Sea subregion.

The capitalized form "Baltic Proper" is used in established marine nomenclature, including the Marine Regions Geographic Identifier (MRGID), as suggested in FAO (2019), and is also widely used in Baltic Sea oceanographic literature. We therefore retain "Baltic Proper" with a capital "P" throughout the manuscript for consistency with accepted regional naming conventions.

MRGID: <https://marineregions.org/mrgid.php>, last accessed: 11 May 2026.

FAO. 2019. Aquatic Sciences and Fisheries Information System. Geographic Authority List. ASFIS-7 (Rev. 3). Rome. Licence: CC BY-NC-SA 3.0 IGO.

l. 87-89: Perhaps you could rewrite this paragraph a bit, to emphasize that the validation was done outside of this paper, and also comment on the result of the validation (e.g. that most validation stations

are in open water and that the really deep MLD often seem to be underestimated by the model. The trends that you calculate could still be okay, as I see it.

Has been rewritten as suggested

I. 90: "The annual cycle..."

Has been corrected

I. 91: "...for the whole reanalysis period..."

Has been corrected

I. 94: "...10-20m during the period..."

Has been corrected

I. 95: "...80 m deep during the reanalysis period..."

Has been corrected

I. 96: "...until the end o summer..."

Has been corrected

I. 97: "...for the reanalysis period..."

Has been corrected

I. 98: "...comparison of the MLD product..."

Has been corrected

I. 104: "...more energy is required..."

Has been corrected

I. 107: As far as I can see, the variable names "SA" and "CT" are not used anywhere else, so I suggest to simply leave them out (but of course keep the words "Absolute Salinity" and "Conservative Temperature")

Has been corrected

I. 113: "where g is..." (lower-case "w")

Has been corrected

I. 117: What do you mean by "streaming statistics"?

We have deleted this technical detail of calculating trends spatial distributions from big data.

I. 123: "...derived from the high-resolution..."

Has been corrected

I. 121-133: This figure caption is too long, in fact I think there has been a mistake here. Everything except the first sentence should be in the main text, referring to the figure.

We have corrected the figure caption according to the suggestion

I. 147: Error in figure heading for subplot “f”: Should be Total precipitation.

Has been corrected

I. 151-152: I think there is a mistake here. The areas shaded in red must be the regions with insignificant trends, right? And the areas shaded in green are the regions with significant trends?

Has been corrected

I.167: “...seasonal fluctuations...” (“s”)

Has been corrected

I. 173-174: Judging from the figure I would say Oct-Mar and Jun-Aug, respectively.

Has been corrected

I. 255: How can you see a “trend toward stronger stratification” in figure 3? Which sub-panel?

We have changed the reference to figure 3c, indicating a sub-panel for the PEA trends.

The discrete colormaps are used to enhance trend visibility.

I. 119: I would suggest changing the section heading to simply Results, and add a new section heading called Discussion between I. 229 and I. 230. Or keep it as it is; just a suggestion...

We considered separating the sections into distinct "Results" and "Discussion" chapters. However, we decided to retain the combined "Results and Discussion" format because the manuscript is strongly process-oriented, where the interpretation of the observed spatial and temporal MLD patterns is closely linked to the presented results themselves.

In particular, many of the findings require immediate physical interpretation in the context of Baltic Sea stratification, sea-ice decline, buoyancy forcing, and regional mixing processes with corresponding references. Separating the discussion into an independent section would lead to substantial repetition of the same patterns and mechanisms, making the manuscript more redundant and interrupting the continuity of the scientific narrative.

The current structure allows observational results and their mechanistic interpretation to be presented together in a coherent way, which we believe improves readability and helps maintain a clear connection between the identified trends and their underlying drivers. This format is also commonly used in oceanographic and climate-related studies where interpretation is tightly coupled to spatial analysis and process understanding.

I. 266, 276, 279: You discuss a trend toward a shallower MLD during winter, but figure 3 a) shows the MLD trend for the year as a whole, as I understand it. If the winter trend is most important, perhaps fig 3 a) can be remade to show that instead of the annual trend? Or do you need both for the discussion?

We have added seasonal trends to revised manuscript as figure 4 and have rewritten text accordingly.

Once more we thank both reviewers for the constructive suggestions.