Review of the manuscript “An assessment of the variability in temperature and salinity of the Baltic Sea from a simulation with data assimilation for the period 1990 to 2020” written by Ye Liu, Lars Axell, and Jun She, submitted to Ocean Science journal.

The study presents a new dataset of the physical parameters of the Baltic Sea. The dataset is covering the period 1990-2020 (30 years). It is produced using a regional circulation model for the Baltic Sea (NEMO-Nordic setup) with a data assimilation (DA) technique called the local Singular Evolutive Interpolated Kalman filter (LSEIK), assimilating both in-situ T/S profiles and satellite products. To estimate the performance of the above-mentioned model setup, the study provides some validation of the results against in-situ T/S profiles, sea surface elevation from tide gauges, SST from a satellite product, and mixed layer depth (MLD) derived from in-situ observations. The validation is based on well-known quality metrics. In addition, the study investigates seasonal, inter-annual, and multidecadal dynamics of the water temperature and salinity produced by the authors’ model setup at different vertical layers (surface, 60m, 100m, and bottom). By comparing model runs with and without DA, the study shows that DA improves the model quality in the Baltic Sea region. Despite some persisting uncertainties, the authors are convinced that the dataset demonstrates exemplary performance and can be utilized for further research. The study also identifies a warming trend across the Baltic Sea, with the temperatures at intermediate depths increasing faster than at the surface. As for salinity, the study highlights opposite trends in SSS across the Baltic Sea, with slight freshening in the northern part of the sea and slight salinization in the southern part. At the same time, the authors observed a notable positive trend in salinity below the halocline in the Baltic Propper. Based on the comparison with other studies and products, the authors conclude that the temperature and salinity trends are comparable to the estimates provided earlier by the community.

The study is well-structured and generally well-written. However, I have some comments on the general scientific questions it tackles. If the study's objective is solely to present and validate the new dataset, which can be inferred neither from the title nor the chosen journal, the other journal is probably the better choice (e.g., Geoscientific Model Development from the same publisher). To be published in the current journal, the authors need, in my opinion, to invest more work into the paper. First of all, I would appreciate a more detailed comparison with, e.g., the current publicly available CMEMS Baltic Sea physics reanalysis (https://doi.org/10.48670/moi-00013) covering the period from 1993 to 2023, which is the same duration as for the presented reanalysis, or possibly with other state-of-the-art Baltic Sea reanalyses (one could include it to the validation part). This analysis would demonstrate the added value of the new reanalysis described in the study.

The current analysis of temperature and salinity variability and its discussion seem to be a continuation of the reanalysis validation. In my opinion, the study, in its current form, does not deliver any new insights into T and S variability in the Baltic Sea. I think the interesting part is an investigation of T and S variability at different depths, not just at the surface, but this part needs to be improved, e.g., authors could elaborate more on the causes of the strong trends in T and S at the intermediate depths in the Baltic Propper (was it just the inflow activity or something else). In addition, some analyses and results are also currently questionable (for a detailed explanation, see the comments below). Therefore, I suggest the authors undertake a major revision and resubmit the revised manuscript. Below, I provide a few detailed comments on the manuscript:

L21: "*its significant impact on the Nordic climate change*": I would reformulate the sentence since the Baltic Sea is considered to be significantly driven by external forcing, not the other way round (see, e.g., Stigebrandt & Gustafsson, 2003).

L23-24: "*The Baltic Sea exchanges with the North Sea through the Danish Strait in the transition zone.*": Danish Strait**S**. I would also reformulate this sentence, e.g., *The water exchange between the Baltic and North Seas happens through the shallow and narrow Danish Straits.*

L27: *"(Matthäus et al. 2008)."*: I think a reference to newer research could be added, e.g., (Mohrholz, 2018).

L31: *"The Baltic Sea exhibits various features over time due to local changes in climate and forcing"*: How do the changes in climate differ from the changes in forcing? The sentence does not sound right to me; please reformulate it.

L36-37: *"a comprehensive climate assessment directly from the observations is still lacking due to the spatiotemporal coverage limitations, especially in the deep Baltic Sea."*: I think most observations are focused on the deep Baltic Sea, where most monitoring stations are located. Therefore, I would assume assessing the climate impact on the coastal areas is more challenging.

L92: "*The spin-up run (1 January 1975 – 31 December 1989)*": Could you please say a few words about the spin-up duration? I would say it is rather short.

L119: "*It should be note*": It should be note**D.**

L138-139: "*of 3 ^{o}C or 3 PSU, respectively"*: Please comment on the thresholds. I would assume the one for salinity should be lower since its variability is lower than that of temperature.

Eq.5: Are you sure it should be -0.005 under exponent, not +0.005? Now, it looks like the error variances are decreasing after 70m, which contradicts the text.

L157: "*oN"*: Typographical error.

L170: *"low-resolution":* In the ICES dataset, they are called "high-resolution CTD data."

L173: *"the SHARK and ICES data center".* Why only those two? There is also, e.g., the IOW dataset for the Baltic Sea (https://odin2.io-warnemuende.de/).

L177: *"increases significantly with time."* This conclusion contradicts Fig.2, which shows a substantial gap in 2012.

Eq.7: Add "="

Eq.12: You use the standard deviation of the observations to calculate the cost function. It might affect your estimates if the observations are, for example, seasonally biased or if there are only a few profiles available. It may be better to use the model standard deviation instead. I assume the model should capture the variability more or less correctly.

L240-241: *"The salinity shows a stronger stratification at the eastern Gotland Basin (BY15) compared to the Bornholm Basin (BY5)."* Based on Fig.4, I would say the opposite is the case since the vertical salinity gradient seems stronger at BY5 compared with BY15. Please check.

L242: "*Compare*": Compare**D.**

L246: *"As shown in Fig. 3, error of DA simulation is very small":* Shouldn't you refer to Fig.4 instead?

L315-316: *"using a seawater density criterion to define the MLD as the depth at which the seawater density deviates by 0.03 kg m-3 from the surface value (Chrysagi et al., 2021)."* I suggest adding the original publication on this method: https://doi.org/10.1029/2004JC002378. In addition, please clarify whether you used in-situ or potential density. To remove the effects of pressure, the density should be potential. But I think the difference should be very minor in the Baltic Sea.

L322-323: *"The MLD was shallower in the far ends of the elongated Baltic Sea, including the Bothnian Bay, the Arkona Basin, and the Gulf of Finland, compared to the MLD in the central Baltic Sea":* Is it only due to the different depths?

L345: "*intermedia*": intermedia**TE**.

L349: *"well mixing"*: wrong use of English language.

L352: *"This pattern indicates a clear temporal variation of the T/S trends in the Baltic Sea.":* I would add a reference here.

L378-379: *"the tendency for increased salinity was more pronounced at deeper waters, as evidenced in both the Baltic Proper and the Bornholm Basin.":* Please add additional analysis on that topic.

L381: *"notable, significant":* a general comment. I would estimate the statistical significance of the trends presented in the paper and discuss them accordingly.

L427: "*On data assimilation scheme, please write*": please write.

L431: *"Fig. 3"*: maybe Fig. 4?

L436: *"Fig. 4"*: maybe Fig. 3?

L465: *"Fig. 3"*: maybe Fig. 2?

L474: "which well match with observed values,": English language

L479: "to accurate representation": English language

L480: *"data is"*: data are

L493: *"which is helpful to constrain the bottom simulation":* I didn't understand what the bottom simulation is.

Table 1: The decrease of salinity at intermediate depths looks pretty suspicious. This means that, generally, the stratification is unstable, which cannot be accurate. The same problem is visible in Fig.9. One possible explanation could be that the shallow transition region was included in the averaging at the surface but not at the intermediate depth. To avoid that and account for the different conditions in different Baltic Sea regions, I recommend providing the analysis for different basins of the Baltic Sea separately (the HELCOM definition can be applied here). In addition, I would calculate trends at all available depths and provide the vertical profiles of trends rather than focusing on specific vertical layers. If the authors still want to focus on particular depths, they should, in my opinion, account for area differences when averaging data. Are trends statistically significant?

Figure 3: A typo in the figure caption. There is no panel C in this figure. I suggest adding colorbar labels as well.

Figure 4: I suggest adding captions to the plots (station name for the profiles and an indication of whether the simulation was with or without DA for 2D plots). In addition, I would make colorbars equally spaced to facilitate the discussion of stratification, etc., in the text.

Figure 5: Please add labels.

Figure 7: Labels as well.

Figure 8: Also, colorbar labels. In addition, instead of "size of the mixed layer depth," I would say "mixed layer thickness."

Figure 9: Same as in Table 1. In addition, I would add an indication of seasonal variability on that kind of plot (e.g., +- sigma). Why is the salinity in [g kg^{-1}] in this figure? I suggest replacing the label with PSU since, in the model, the EOS-80 equation of state was used.

Figure 10: Trends significance and some discussion on observed results.

Figure 11: Same as in Figure 10.

Figure 12: Please indicate what error bars mean. This Figure also showcases the importance of the basin approach. I believe the seasonal signal in the bottom temperature is caused by disproportionally more shallow areas that enter into bottom values averages compared to the deeper regions of 60m and 100m, which, in my opinion, could lead to false conclusions.

Literature:

Stigebrandt, A., & Gustafsson, B. G. (2003). Response of the Baltic Sea to climate change—Theory and observations. Journal of Sea Research, 49(4), 243-256. https://doi.org/10.1016/S1385-1101(03)00021-2

Mohrholz, V. (2018). Major Baltic Inflow Statistics – Revised. Frontiers in Marine Science, 5, 385391. https://doi.org/10.3389/fmars.2018.00384

Montégut, B., Madec, G., Fischer, A. S., Lazar, A., & Iudicone, D. (2004). Mixed layer depth over the global ocean: An examination of profile data and a profile-based climatology. Journal of Geophysical Research: Oceans, 109(C12). https://doi.org/10.1029/2004JC002378