

Dear editor, and dear authors,

First of all, I would like to thank the authors for taking into account previous comments and their efforts in improving especially the comprehensibility of the (numerous) aspects presented in the manuscript. Navigability has improved a lot and both the author's intentions and their scientific findings are now clearer and more digestible for an outside reader. (For remaining minor suggestions on the presentation of contents and the structure of the manuscript see the comments below.)

With this manuscript the authors are setting out to identify and attribute drivers of the interdecadal salinity changes that are observed in the Baltic Sea system. They do so from an internal process-oriented (i.e. effects due to variations in river discharge, saline inflows, and wind-induced surface mixing) and external driver (i.e. NAO and AMO climate indices) perspective. Specifically, they also set out to quantify the direct dilution effect from runoff and to provide numbers for its impact upon interdecadal salinity variations. Methodologically, they have chosen to investigate causal relationships between the target variables and potential drivers by doing a wavelet decomposition, as well as common wavelet power and wavelet coherence analysis, both on third-party observational datasets and salinity/temperature time series that were obtained from a hindcast simulation specifically conducted for the purpose of their study. The results are overall well presented, and interesting insights are generated.

However, there are still issues present that need to be addressed before I would consider the manuscript to be acceptable "as is" for publication. Please find a (probably incomplete) overview that summarizes some of the main issues next.

(For the complete set of aspects see the purple highlighted parts in the evaluated version of the manuscript and the corresponding detailed comments that are attached as a separate file. Minor wording suggestions using red color are also done in the annotated PDF.)

*Abstract:* This section of the paper is very nice and clear. I do not have any comments, except from: It would be very helpful if the discussion/conclusion sections could be improved to have a similar level of clarity/readability than the abstract.

*Introduction:* This section of the paper has considerably improved. However, the research questions that are discussed in the remainder of the manuscript are incompletely summarized here. Also there is still somewhat a too strong emphasis on runoff as a driver for Baltic Sea salinity variations, while other aspects are important and are also studied in the manuscript. The balance should be improved, please.

*Datasets:* This section is overall in a good shape. Some references could be added, as well as some details about why certain corrections were applied to the time series (i.e. the generalized additive mixed models for decadal variation evaluation, or the interpolation of the barotropic inflow time series), and thus why some decisions were made. I personally would order/group the subsections a bit differently, i.e. I might first present the purely observational series (ICES observations, climate indices, major Baltic inflows) and then those time series that are used also to force the hindcast experiment (atmospheric reconstruction and river runoff). The current choice seems a bit arbitrary to me and makes

digestion of information a bit difficult.

*Methods:* The methods are overall described in a way that would enable to reproduce the results that are presented. However, some specific issues include:

There might be a misconception (at my personal or the author's side) about how to correctly interpret the negative values for the "explained variance" metric that is used for the validation of the modeled time series. At several places of the manuscript the term "anticorrelation" is used. However, if my understanding of the metric is correct, negative values can't be interpreted as "anticorrelations" of these underlying time series (a detailed investigation on the magnitude of the single time series variances would be necessary to allow for this interpretation). In my understanding, the metric that is used is not strictly bounded on the negative side and becomes even hard to interpret for values close to zero. Either, further details should be provided, or the term "anticorrelation" should be dropped here.

For the salinity discriminated transports it is a bit difficult to follow/digest the mathematical definitions of the quantities that are used later. This could be improved by clarifying what goals should be reached by the analysis of these metrics. The reader would also better understand the decisions presented here, if a statement about why this approach is used at all would be added. Currently, such information is missing completely.

For the wavelet analysis section, it seems that many important information are present. However, this section is rather confusing, especially also because some information of similar type of those from the methods section are only found in the appendix. For the wavelet analysis, overall, the consistency of the terminology could be improved. It might make sense to define some abbreviations (this is done somewhere else, but not used consistently throughout the manuscript) for the three wavelet analysis methods in the methods section, and to use these consistently everywhere else. Also, I think the reader should be briefly prepared on how to interpret the wavelet analysis figures/results later on. This is sometimes done in the main text of the results section, but I would actually also expect these kind of information in the methods section. (For comments on the presentation of the wavelet analysis parts see also comments for the appendix section.) Furthermore, I am a bit critical about some of the choices on the pre-processing of time series for the wavelet analysis. These might even bias the results, which should at least be discussed.

The salt budget calculations are a very nice idea, but literature could be added. If I see it correctly, the derivation of important equation 16 is not presented. (I also don't understand why the exact same formulas from the method section are repeated in the online supplements.) Also, as this alpha value is a central parameter in the box model, I would expect a discussion about the choice of the numeric value here. I would be interested in seeing a plot, maybe in the appendix.

*Results:* These are overall well presented. Generally, the consistency of the wavelet analysis terminology that is used throughout the study could be improved (see also above). Also, a proper translation of each of the stated phase angles into their respective time lag would help to interpret the results that are presented. I personally would also add the contents of section 6 (which are results that somehow follow the discussion, but are not linked to it) should be rather presented in the results part of the manuscript.

*Discussion:* There are already many valuable aspects discussed here, that add scientific insights. However, I am missing a section on the impact of methodological issues on the results. Also a discussion about the limitation of the wavelet analysis (i.e. correlation is not causality). I am also missing that the results are set into context with the literature and/or the research questions presented/stated in the introduction. Also, to help with the formulation of a conclusion, it would be helpful to come up with a good summary about the differing variations in surface and bottom salinities and especially what this means for the overall investigated “Baltic Sea salinity” variations (some things are still a bit confusing here and conclusion appear a bit inconsistent). Overall, I also think that currently the overall analysis and discussion of the effect of vertical turbulent mixing on Baltic Sea salinity is a bit incomplete/too shallow. If important processes are captured by the chosen metric, i.e. the limitations, should be properly discussed and maybe already stated e.g. in the methods and results sections.

*Conclusion:* This part could be improved considerably. Only a summary of the results is presented. I would expect, however, to find especially what is actually learned from the study. On this topic, currently nothing is provided.

*Outlook:* Interesting/valuable aspects are mentioned here. Several of the sentences are not clearly formulated, though. I have highlighted these and commented them below.

*Appendix:* In my opinion, the valuable discussion about the counterintuitive numbers obtained during the model validation part of the study should not be “hidden” in the appendix, but should be placed in the main text. Either in the model validation part, or probably better in a further discussion section. For the wavelet analysis parts, the comprehensibility of the provided information in the appendix and the methods section could be improved, if there was a systematic choice on what is presented in the appendix and what in the methods section. To improve readability, in my opinion, mathematical details should go to the appendix and aspects such as steps to prepare the time series for the wavelet analysis, together with explanatory aspects about the utilized wavelet methods and how statistical tests were conducted should be rather stated in the methods section.

*References:* The list needs a little clean-up especially with respect to DOIs.

*Figures:* These are overall of high quality both in terms of scientific clarity and image resolution. For some of them the formatting of the numbers displayed at the color bars could be improved, i.e. unnecessary positions after the decimal point should be removed to improve digestion of information.

Overall, I think that the aspects presented/discussed in the manuscript are a valuable contribution to the existing literature/knowledge. I would therefore be happy to see if most/many of the remaining issues above and in the attachment would be resolved before final publication of the manuscript.