

Response to Short Comment 1 by Katharina Höflich

We would like to thank Mrs Höflich for her constructive and helpful comments! We give our response as:

Short comment – Authors' response where needed – Changes to manuscript.

5. *Are the results sufficient to support the interpretations and conclusions?*

very likely; however, the structure of the paper is not very easily grasped; therefore, it is difficult to decide if everything is consistent and conclusive

Indeed the structure of our paper deviates from the standard because we use two techniques (Wavelet coherence / box model for estimation of direct runoff effect) and do not want to skip any of them. If we kept to the classical structure (methods, results, discussion), this would mean alternating between the two (methods wavelet – methods runoff effect – results wavelet – results runoff effect – discussion wavelet – discussion runoff effect) and this might be even harder to follow for the reader. So, we prefer to keep the existing structure.

6. *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists?*

not in every aspect, e.g. please provide more details on the significance analysis of the signals appearing in the wavelet decomposition/coherence analysis; have you considered the papers by Douglas Maraun (e.g. 10.5194/npg-11-505-2004) here?

Thank you for pointing at this reference here. Indeed we follow the suggestion giving in this article by complementing the analysis of common wavelet power (CWP, called WCS in their article) by that of wavelet coherence (called WCO in Maraun and Kuhrts 2004). In the significance analysis for common wavelet power, we use the Monte-Carlo approach of randomizing only one of the two time series, the explanatory one, and not both. This overcomes the problem discussed in the quoted paper that peaks in the common wavelet power caused by one of the two time series only are considered significant.

We will refer to the suggested publication and point this out more clearly.

7. *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?*

proper credit to related work is only given fairly; in my opinion the overall literature work for the paper could be improved; there is a strong focus on papers that focus mostly on runoff as a driver; however, as the authors claim to explain the investigated interdecadal salinity changes and they also directly investigate on variations in saline inflows/transport and wind-induced mixing, their literature work should be more substantial on what is actually known about drivers behind salinity variations; spontaneously, I can think of works by Schimanke (e.g. 10.1175/JCLI-D-15-0443.1) and e.g. Gustafsson and Andersson (10.1029/2000JC000593); there is also work focusing on improving the conceptual understanding of major Baltic inflows and ultimately Baltic Sea salinity, but unfortunately these are published only as conference talks (e.g. 10.5281/zenodo.3536232 and 10.5281/zenodo.3567086)

Thanks a lot for these suggestions!

We will improve the introduction section and include the suggested references.

8. *Does the title clearly reflect the contents of the paper?*

not really; the title suggests an analysis of variations in a hindcast simulation only, however, variations in observational records are also studied; furthermore, there are not really many satisfying explanations given, at least not in a conclusive and nicely summarized way; generally I think the manuscript would benefit from focusing on fewer aspects, that should then be tackled more deeply

Even though we do not reach a final explanation yet, we think that the results we have are already worth publishing to support future research on the subject and reach “satisfying explanations”. The hindcast simulation with its completeness in time is the major ingredient for the analyses, but it is clear that we also have to take observational data into account.

We change the title to “Investigating drivers of interdecadal salinity changes in the Baltic Sea in a 1850-2008 hindcast simulation” to make clear we do not end up with a final explanation.

10. Is the overall presentation well structured and clear?

mostly; however, in my opinion the authors should better decide on what their actual research questions are; they open many boxes, but do not properly "close" them; the manuscript appears very unfocused to me

The manuscript focuses on two things: (a) using wavelet coherence analysis to show that both runoff and inflows are possible drivers of the 30-year oscillations while AMO and NAO are not and (b) showing that the direct-dilution effect of river runoff is small.

We will add a table summarizing the main findings of the wavelet analysis to better concentrate the results that are scattered across the sub-sections now.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? for better focus, e.g. - leave out Figure 3, climate indices analysis, ... - extend "direct dilution effect" discussion/analysis, ..

We think the result that wavelet analysis excludes the climate indexes as drivers of the 30-year oscillations is valuable, we want to keep it.

We move Figure 3 to Appendix D and extend the discussion of the direct dilution effect results.