Answers to referee #1. Referee comments in *italics* and our answers in normal text.

> *The manuscript investigates the sensitivity of the salinity of the Baltic Sea to changes in the freshwater forcing and the salinity at the open boundaries in a model setup comprising the Baltic Sea and the North Sea. As mentioned by the authors, several studies have already been performed on the sensitivity to freshwater forcing. However, considering the salinity at the open boundary is a novel approach and also the idea of approximating the salinity of the Baltic Sea by a Taylor polynomial depending on the effects of the two forcings under consideration is new. Finally, the study adds novel insights to the water exchange between the North Sea and the Baltic Sea. All in all, the study meets the quality and scope of Ocean Science.*

Thanks for a detailed and constructive review. We have answered the comments below, and described how we have improved the manuscript according to the comments where relevant. We refer to the changes by line numbers in the marked-up-file:

General comments

1. *The study could provide a bit more background / context information, e.g., about typical variations of freshwater forcing and boundary salinities, about how they are expected to evolve in the future, whether there is any kind of interdependence etc… . Why did you choose specifically those two parameters? Aren't other parameters like wind patterns / sea level rise more important for the salinity in the Baltic Sea than the boundary salinity in the North Sea (the importance of sea level rise is at least mentioned at the end of the text)? What exactly is the use of the Taylor polynomial? To explore the parameter space without having to run simulations for every combination of parameters? Are there alternative approaches and if so, why did you choose this particular approach? A final assessment of how this study advances the existing knowledge at the end of the manuscript would also be great.*

We have added information about the magnitude and variability of river runoff (line 44) and about the spread in projections of future Baltic Sea salinities (line 29) in the introduction. We have also added text in the introduction (lines 63-71) on other forcing factors, e.g. zonal winds and sea level, and argue why a study on the influence of North-East Atlantic salinities is important. We have rewritten the last paragraph in the introduction (lines 73-85) to make it clearer what this study contributes with relative to earlier studies. The intended use and importance of the polynomial will be explained in the introduction (lines 79-80), the summary and discussion (lines 393-395), and in the abstract (lines 18-20). The intended use of the polynomial is mainly interpolation and to some degree extrapolation of the Baltic Sea salinity changes based on changes of the two input variables, making it easier to asses salinity changes without having to run the simulations.

2. *There could be a few more references. Others have already looked at, for instance, the impact of runoff on inflows via changed sea level gradients; maybe, there are also more studies on the water exchange between the North Sea and the Baltic Sea or about the salinity at the North Sea boundary. See suggestions in the attached pdf.*

We have gone through the suggested additional citations below, and have also added additional citations in the introduction.

3. *The model validation could be a bit more comprehensive. For instance, a validation of transports in the entrance area of the Baltic Sea would be great as they are important for the study. The salinities in the central Baltic Sea look quite good (with some exceptions mentioned in the detailed comments) but they might be "right for the wrong reason". In addition, possible inaccuracies introduced by the Taylor polynomial approach, namely by the short averaging period / nonstationarity of the time series in this period, the discrete differences, and the truncation of the polynomial, are only discussed very briefly (in Figure 7) and might deserve a few more sentences.*

To the authors knowledge there are no very good estimates of transports in the entrance area that could be used for validation, so time series of salinities are the best means we have for validation the model with respect to salinity dynamics. We have extended the text in this section somewhat (lines 207-210 and 213-215). The non-stationarity of the system is discussed in further detail below.

In case the suggested modifications lead to a too long manuscript, less important parts might be moved to a supplementary file.

With the present modifications we do not see a reason for moving text to a supplementary file.

Suggestions for additional references

Thanks for the suggestions. Below we have answered to each of these if we have included them and if not, then why not. A general comment to this is that our paper is not meant to be a review paper, so we only want to include the references of relevance for our study.

Some studies dealing with the connection of precipitation / runoff, inflows, and sea level:

- *H. Schinke, W. Matthäus, Continental Shelf Research 18 (1998) 67-97*

Focus on major inflows and how they have changed over time (mainly over season) due to weather and fresh water forcing. We do not see that this adds to the background of this manuscript.

- *https://doi.org/10.1029/2023GL103853 ; Barghorn, L., Meier, H. E. M., & Radtke, H. (2023). Changes in seasonality of saltwater inflows caused exceptional warming trends in the western Baltic Sea. Geophysical Research Letters, 50(12), e2023GL103853.*

Focus on seasonality of inflows and how this influences Baltic Sea temperatures. We do not focus on Baltic Sea temperatures in this paper and have not included the citation.

- *https://doi.org/10.1111/j.1600-0870.2006.00157.x : Hünicke, B., & Zorita, E. (2006). Influence of temperature and precipitation on decadal Baltic Sea level variations in the 20th century. Tellus A: Dynamic Meteorology and Oceanography, 58(1), 141-153.*

Focus on sea levels and how they depend on pressure, temperature and precipitation. The relevance to this manuscript is not obvious and the citation is not included.

- *https://www.tandfonline.com/doi/abs/10.1111/j.1600-0870.2007.00277.x :Hünicke, B., & Zorita, E. (2008). Trends in the amplitude of Baltic Sea level annual cycle. Tellus A: Dynamic Meteorology and Oceanography, 60(1), 154-164.*

Focus on seasonal sea level differences. We do mainly focus on annual mean salinities and the forcing factors for the annual sea level cycle are therefore not of relevance for this publication.

Water exchange between the North Sea and the Baltic Sea:

- *https://doi.org/10.1016/j.ocemod.2020.101585 : Haid, V., Stanev, E. V., Pein, J., Staneva, J., & Chen, W. (2020). Secondary circulation in shallow ocean straits: observations and numerical modeling of the Danish Straits. Ocean Modelling, 148, 101585.*

Focus on high-resolution flows (100 m) and what is gained extra relative to 500 m resolution. Citation has been added in the end of the discussion of modeling of strait flows (line 477).

- *Bertil Håkansson, Geophysica (2022), 57 (1), 3–22*

Study of barotropic flows in and out of the Baltic Sea. We are not sure how the focus on barotropic exchanges adds to our study.

- *https://www.tandfonline.com/doi/abs/10.3402/tellusa.v48i2.12063 : SAYIN, E., & Krauß, W. (1996). A numerical study of the water exchange through the Danish Straits. Tellus A, 48(2), 324-341.*

Old coarse resolution study. The relevance for this study is unclear and the citation is not included.

- *https://doi.org/10.5194/os-21-913-2025 : Jahanmard, V., Ellmann, A., & Delpeche-Ellmann, N. (2025). Quantification of Baltic sea water budget components using dynamic topography. Ocean Science, 21(3), 913-930.*

Focus on batrotropic flows, and only for a short period without MBI. Not relevant for our study.

- *https://doi.org/10.1007/s10236-024-01626-7 : Pham, N. T., Staneva, J., Bonaduce, A., Stanev, E. V., & Grayek, S. (2024). Interannual sea level variability in the North and Baltic seas and net flux through the Danish straits. Ocean Dynamics, 74(8), 669-684.*

Focus on sea level variability in Baltic and North Sea and barotropic flows and how they depend on runoff, precipitation, etc. Mainly focus on seasonal changes and therefore not of large relevance for this work.

Changes in the North Sea:

- *https://link.springer.com/book/10.1007/978-3-319-39745-0 (e.g., chapters 3.2.3 and 6.3 and references therein) : Quante, M., & Colijn, F. (2016). North Sea region climate change assessment (p. 528). Springer Nature.*

North Sea climate change assessment. Future salinity changes in the North Sea are relevant for the choice of perturbation range for the citation has therefore been added in section 2.4 (line 236).

Specific comments

Line 17: Maybe a last sentence about the significance / implications of the results in the abstract?

We have added a sentence at the end of the abstract "Besides providing new understanding of the processes that govern the Baltic Sea salinity sensitivity to freshwater forcing, the results of this study provide a means of quickly assessing the importance of North-East Atlantic salinities and Baltic Sea freshwater forcing for Baltic Sea salinities."

Line 20: salinity is not everywhere below 12 psu in the Baltic Sea (although in most parts this is true); it should be mentioned that not the average low salinity but also the strong horizontal and vertical salinity gradients are challenging for the ecosystems

We have changed this to "mostly less than 12 psu" (line 24). We are not including a sentence on gradients since we are not focusing on gradients in the present study.

Lines 22-24: Additional information to that study could be useful. How strong were the salinity decrease and temperature increase in that study? Which other "anthropogenic" factors were they compared to?

We have added information on scenarios and examples of pressures (lines 27-29).

Line 26: rather cite original studies (Meier et al. 2017; https://doi.org/10.1007/s00382-016-3333-y and maybe also Meier et al. 2021; https://doi.org/10.1038/s43247-021-00115-9) instead of the review Meier et al. 2022

We have changed the citation to Meier et al. 2017.

line 37: Average / typical depths of the mixed layer / permanent halocline and the seasonal thermocline could be given

Has been done (lines 41-42).

line 39: According to the reference Mohrholz 2018 (table 3 therein), about half of the salt transport into the Baltic Sea is baroclinic. Hence, the statement in the next sentence that the main transport is sustained by small barotropic inflows, should also be reconsidered.

We have modified "mainly" and "the main part" to "to a large degree" and "a large part", and mention the baroclinic exchanges (lines 46-49). We have not been able to read where the number for baroclinic exchange in Table 3 in Mohrholz 2018 comes from.

Lines 54-55: How is the representation of inflows improved? Compared to predecessors of the model that was used or compared to other models?

Good point. Compared to previous versions of the same model with the same resolution. This part has been removed from the rewritten paragraph.

Line 72: Are SB and RP really defined relative to their unperturbed states (i.e., as some kind of Delta SB, Delta RP)? For me, the Taylor expansion (specifically, the terms (SB – SB_0), (RP – RP_0) and so on) rather looks as if they are defined in an absolute way.

That is correct. SB and RP are absolute values, except that RP is normalized with the unperturbed state. Text has been corrected (lines 97-99).

Line 75: Would you expect an interaction between the two forcing terms? In one way or both ways? It's not obvious that / how they should interact.

Since it is a non-linear system, they may interact. For example, the combined effects of a fresher boundary and increased freshwater input may either reinforce each other or dampen each other. It is not obvious and that is the reason to use a model to investigate this. We have expanded somewhat on the discussion of this when discussing Fig. 6 (lines 279-282). Actually, the interaction term is quite small, but we were not able to predict this a-priori. Note that this is a modification to the original manuscript, where we wrote that the interaction term was relatively important.

Lines 86ff: How did you choose h and k? How do they compare to, e.g., interannual variations of SB and RP? How large is the uncertainty they cause in the discrete differences in equations 3-7?

We have removed this text since it is more or less repeated in section 2.4. However, we have expanded on the reasoning for the choice of these values for h and k in that section: "These choices for size of perturbations were somewhat arbitrarily chosen to yield a span containing the long-term variability and expected effects of climate change. For river runoff, 20% is in the upper end of the expected increase in runoff by 2100 (e.g. Saraiva et al. 2019). For boundary salinity 0.5 psu represents rather well the expected variability in changes at the outer boundaries of the North Sea (e.g. Quante and Colijn 2016). However, the variability in projected salinity changes between different GCMs is rather in the range of 1-2 psu, so in hindsight we could have chosen a larger perturbation value." (lines 232-237).

Line 91: The considered time span (1990-2017) is quite short given the pronounced multidecadal variability of the system. This might add some uncertainty to the results

This may to some degree be true. However, given that the response time of the Baltic Sea is about 30 years, it will almost be in equilibrium with forcing on longer time scales. Since the multidecadal forcing is equal in all runs, the difference between the runs will therefore mainly be caused by the perturbations we impose on the forcing. Note also, e.g. in Fig. 4, that the difference to the CTL run increases most in the first 30 years and then levels out after that, i.e. in the period we analyze. This means that, yes, the polynomial surface may look slightly different if calculated with different center/equilibrium points at other phases of the multidecadal variability, but given that our check points give reasonable results, we expect that the polynomial surface is a reasonable approximation to the model representation of the Baltic Sea equilibrium state response to changes in forcing.

Line 140: What is meant by "turbulent mixing of the inflowing water masses to the region"? How can a water mass be mixed to a region? Do you mean it's mixed to the water masses that are present in that region?

The outflowing water masses are a result of mixing between the water masses flowing into the region. This is basically what we have written but we have tried to make it clearer (lines 166-167).

Lines 150ff: You say "The upper envelope ranges between the surface and 250 m and uses 43 levels". How does that match with terrain-following coordinates? Do you always have 43 levels if the water depth is less than 250 meters and 43+13 if it's more than 250 meters (which is rarely the case in the Baltic Sea)? Are the depth levels otherwise *equidistant at a certain grid point? You wrote in the introduction that this model has an improved representation of inflows (see an earlier comment of mine). How does this selection of coordinates improve the representation of inflows? Is it better than, for example, adaptive vertical coordinates (https://doi.org/10.1016/j.ocemod.2011.04.007)?*

Having 43 levels in the upper envelope does not always mean that there are 43 vertical levels in a given water column, it also depends on how much smoothing has been done when constructing the envelopes. If the envelope surface outcrops the real bottom some layers will be removed (the extreme case of having totally flat envelopes would, for instance, yield a standard Z-coordinate). In MEs coordinates smoothing is done to the envelope surfaces where there is steep topography. The level of smoothing is based on a series of horizontal pressure gradient error (HPGE) tests done to detect where the strongest spurious currents develop. The smoothing reduces the spurious currents that are due to the HPGE. Smoothing of envelopes improve the main shortcomings (spurious mixing and currents) of a standard terrain-following coordinate system and makes MEs more general and adaptable. All this is explained in Bruciaferri et al (2018). Compared to Z-coordinates MEs improves the inflows because it doesn't have the staircase -like representation of the bottom that leads to artificial (numerical) entrainment of the gravity current. We have not made a rigorous intercomparison with an adaptive coordinate system (like Hofmeister et al (2011)) so we refrain from commenting on that. However, compared to models presented in Baltic Sea Model Intercomparison Project (Gröger et al., 2022) we see that our MEs configuration performs really well.

Lines 162, 163: you write "we ran the model three cycles repeating the same 10 years period using atmospheric, runoff and open boundary forcing from 1961–1970 so that model dynamics reached near-equlibrium level". Earlier in lines 90, 91 you wrote "we use runs over the period 1961-2017, with t1 = 1990 and t2 = 2017, which gives a thirty-year long spin-up". Now, is the spin up period 1961-1990 or was the spin up run for 30 years in the three cycles and then the actual runs were started from 1961? Or do you have one general spin up period and then another one for the perturbations? Maybe, a small schematic could facilitate the understanding of the experimental design.

We have 30 years of spin up before the actual runs start in 1961. We have removed the text in Section 2.1 mentioning the spin up from the start of the run to the averaging period. But yes, we do have a spin up (from 1961 to 1989) after imposing the forcing changes and until we do the averaging. We have added text in section 3.1 lines 253 – 255 to explain this.

Line 183: What about the strong Major Baltic Inflow in January 1993?

The text has been changed to 1993 (line 213).

Lines 199ff: I don't fully understand the choice of the additional experiments to test the validity of the polynomial. RP++SB++ makes sense but why isn't there any equivalent negative experiment RP-- SB--? How did you choose experiments 8 and 9? And shouldn't the number of "validation" experiments be a bit larger than three (although that would be computationally demanding)?

It would be good to have many more validation experiments, but these were the ones we could afford in the present project. We chose to have one extreme and two less extreme experiments with focus on increasing runoff, which we expect is the most relevant case for the future.

Lines 213ff: It could be interesting to also analyze the surface mixed layer and deep-water layer in the Baltic Sea separately for the different experiments. But maybe that's beyond the scope of the study.

Agreed, but we think the focus on mean salinity is sufficient for the present manuscript.

Line 229: Is it really clear from the figure that the response to the perturbations is linear in h? For me, the contours are too coarse to tell

That is a good point. We have modified Figure 6 and added more contours, see also below.

Line 232: How can you compare the values if they have different units?

That is another good point. We have removed the text, and rely on the improved figure.

Lines 245, 246: You write that (SB_0 + 2h, RP_0 + 2.5k) is quite a large extrapolation. However, if I get it right, the range in figure 6 is even larger or not (at least, the salinity range is larger than that in the previous figure)?

We understand the confusion, and have increased the ranges for RP in Fig. 6 and included points in the figure corresponding to each of the runs, see below. It also turns out that we did an error when calculating the polynomial salinity for the most extreme case in Fig 7. The actual correspondence is much better now, see below.

Line 269: "the net outflow, seen as Q at low salinities" – isn't it particularly seen at s = 0?

Yes. Has been corrected (line 319).

Line 276, 277: "At the sill transect, the influence of fresh water input on inflows is seen to be larger for low saline inflows than for high-saline inflows." Is it because low saline inflows contain a larger portion of fresh water which makes the impact of changes in freshwater input larger? Or is there a different explanation?

We do not understand this question. However, we are not sure about our own statement either, and have removed the sentence.

Lines 281, 282: "Changing boundary salinities are mainly affecting the salinities of inflows and outflows at the northern Kattegat boundary, but do cause less changes to the inflows and outflows at the sill transect." This is really difficult to see in the figures which is why I would suggest to have separate panels / inlets with only the maxima of the curves and their dependencies (see figure comments).

The resolution of these figures is not too good. We will have higher resolution pictures in the final version and there it will be clearer that the black, red and blue dotted and dashed lines are closer to the solid lines at the sill than at the northern transect.

Lines 284-286: Could there also be a small effect due to the fact that the transect across the sills is not closed? See discussion of figure 2 in Radtke et al. 2020

We are not sure what is meant here. The sill transect is closed in the sense that water cannot enter/exit the Baltic Sea without passing this transect.

Line 288: How realistic is Gamma = 1?

Thanks for catching this. We used Gamma = 0 (line 338), i.e. that the local fresh water is exported through the northern Kattegat transect. We think this is realistic, since the local fresh water is added to the surface layer in Kattegat which is mainly exported through the northern transect.

> *Lines 300-310: Those results sound quite interesting. Do you get similar results directly from your simulations when comparing periods with low and high freshwater input (of course, there could be confounding factors like changing wind fields; but it would to some extent provide some validation)? Could you compute the overturning streamfunction or something like that in the Kattegat?*

On could say that we have calculated the overturning stream function at the northern Kattegat transect. One could do that also at other transects, but that would be beyond the focus of this paper. One could also look into different periods of varying forcing, but then we get into the time scales of the processes causing the response. The focus in this paper is on steady state/ long term response, but it would be interesting to also check the time scales in future work.

> *Lines 315ff: I have the impression that figure 12 needs some more detailed explanation and interpretation. Can you say a few words on why the maximum of the curve is shifted to the negative side while the tails of the distribution look relatively similar (i.e., all in all, there seems to be some skew involved)? What implications does this have for the actual in- and outflows? Do I see it right that for large inflows the blue and red curves overlap? What does that mean?*

We think we have explained this by adding the black dashed lines which are the shifted curves. The change is seen mostly where the pdf is steep. At high in- and outflows the pdf is flat and is therefore not influenced much by a lateral shift, in the same way as it is not influenced much near the maximum. We have added a sentence to extend somewhat on the interpretation (lines 375-377).

> *Lines 329ff: Could you make a rough calculation of how much more / less barotropic flow across the sills can be expected for a few centimeters of change in sea level gradient as shown in figure 13 (you might use equation 3 from Mohrholz 2018 as in https://doi.org/10.1029/2023GL103853)?*

It is a rather complex calculation since it is dependent on the instantaneous volume flux. If the volume flux is 100 000 m^3/s, an additional 4677 m^3/s (RP+ case) would require 2 cm extra using a friction coefficient 2 10^{-11} s^2/m^5 for the Belt Sea. For smaller volume fluxes the required extra sea level difference is smaller and for larger volume fluxes the required extra sea level difference is larger. Now, the quadratic equation is not the truth, and this discussion would be rather extensive, so we prefer to not include this rough estimate in the manuscript.

> *Lines 349, 350: "When increasing precipitation and runoff to the Baltic Sea, 54% of the increased net fresh water input were exported as increased outflows through the sill transect, whereas 46% resulted in decreased inflows." I'm not sure this is correct (or I get it wrong). Your figure 11 shows volume fluxes as far as I understand, not freshwater fluxes. Also, the decreased inflow is (to some extent) a result of the increased outflow due to the recirculation you show in the figure while your statement sounds as if they are independent of each other.*

You are right, the wording is inaccurate and can be interpreted as if 54% of the freshwater is exported. We have tried to improve the text to this: "When increasing precipitation and runoff to the Baltic Sea, the net outflow though the entrance will increase with about the same amount.

This can result in either increased outflows and/or decreased inflows. In our model study, 54% of the runoff increase resulted in increased outflows of volume through the sill transect, whereas 46% resulted in decreased inflows of volume." (lines 406-409).

> *Lines 352-354: "With the large-amplitude fluctuations in in- and outflows taking place in the inflow region, such a more or less constant net change to the barotropic flows contributes almost equally to increased outflows and decreased inflows." Wouldn't those modifications also be there if the amplitude of the fluctuations between in- and outflows was smaller? Or what's the message here?*

Take a look at Fig. 12. If the spread of inflows would be 5000 m3/s the distribution would be narrow and centered around -15700 m3/s. In that case the change would be entirely on the negative side of the distribution, and 100% of the change would be caused by increasing outflows (since there would be almost no inflows). With increasing spread, more and more of the change ends up on the inflow side and in the extreme case with much larger spread than mean flow 50% of the change would be on each side of the distribution.

> *Line 357: Shouldn't it be the "net outflow of volume"?*

Yes. Has been corrected.

> *Line 363: How do you get from equation 22 to 23? I suppose you are employing the Knudsen relations?*

That is true and has been made clearer in the text (line 421).

> *Line 366: Where do you take S_0 = 33.5 psu from? If I search the document for "33.5", I don't find it anywhere else. Is it from the TEF analysis (same question for the 16 psu in line 380)?*

That is calculated from the TEF analysis for the CTR run, which has now been specified in the text (lines 426-427). Where the 16 psu come from is unclear, since the TEF analysis gives 17.6. We have adjusted the figures to this inflow salinity (lines 441-442).

> *Line 371: At which steps in the calculations are diffusive fluxes neglected? How important are they? I think they are mentioned for the first time here*

The diffusive fluxes across the transects are not considered in the TEF analysis, which we assume is a good approximation since these transects are influenced by rather strong advective fluxes. This has been made clearer in the text (line 432).

> *Lines 383-388: How do your results compare to other estimates like those of Radtke et al. 2020 or Meier et al. 2023? In the same paragraph, how exactly do you differentiate between inflows and inflow salinities? With inflow, do you mean the inflow volume which is sensitive to changes in the freshwater forcing due to the change in sea level gradient that you described before? It's important to be precise here because the term "inflow" is often used for both the volume and the salt import.*

We have specified this better and also included a comparison with the results of Radtke et al. and Meier et al.: "These results do give a new picture of the factors influencing Baltic Sea salinity sensitivity to freshwater forcing. For example, Radtke et al. (2020) find that only about 25% of the salinity sensitivity is caused by direct dilution, and Meier et al. (2020), although they find that both direct dilution and changing inflows are important for low-frequency salinity variability, they do not separate the influences of inflow volume and inflow salinity, and they do not quantify the various contributions. It is also worth mentioning that both of these studies focus on the low-frequency variability rather than the steady stated change that we are approaching." (lines 445 – 455).

Line 391: How did the other studies explain their results then?

We have changed the text to this: "These studies have explained the sensitivity in terms of other processes, including geostrophic control in the Arkona basin (Meier and Kaufer 2003), and geostrophic control of the outflowing Kattegat water combined with assumptions on how this relates to inflow salinities as well as reduced inflow volumes with increased freshwater input (Stigebrandt and Gustafsson 2003). The present results provide a simpler framework for describing the salinity sensitivity, although work is still needed to understand why the fractions of North Sea and Baltic Sea water remain relatively constant, when freshwater forcing changes." (lines 458-463).

Lines 405-406: You mention that the model (of course) cannot properly resolve the Danish straits. Could you briefly mention in the model description of the methods section how you modified the bathymetry in the Danish straits to make sure that transports are realistic?

The bathymetry is based on the same bathymetry used in Hordoir et al. (2019), so the modifications to the bathymetry in the straits are described there.

Technical comments

We have adjusted the manuscript according to these comments, including publishing data behind the figures and adding acknowledgments.

Comments to figures

Figure 1: The nonlinear axis scaling might be pointed out in the figure caption. In the caption, it should be "orange lines" instead of "orange line". In addition, basins mentioned in the paper should be labelled in the map such that readers from other regions understand where, for instance, the "Gulf of Bothnia" is.

We have added a note on the non-linear scale, corrected "orange lines" and labelled model basins.

Figure 2: Over which period where the modeled profiles averaged?

The period is 1970 – 2017, which has be written in the Figure captions.

Figure 3: Units are missing in the depths of the stations given in the titles of the panels. Are the stations BY15 and BY31 really only 150 m deep in your model (80 m would also be quite shallow for BY5)? How were "surface water" and "bottom water" defined? Did you correct for a possible seasonal sampling bias (as, for example, in Radtke et al. 2020)? Do you have an idea why modeled bottom salinities at station BY15 are quite off at the end of the period? It looks as if the strong MBI in 2014 / 2015 was not captured that well.

We have added the depts of the stations in the captions and describe in the text why we have not used bottom values for the "bottom water". Basically, this is because the selected depths are more representative for the water volume below the halocline (lines 207-210). Note also that we have changed the last station from BY31 to BY38 since most data from station BY31 have been removed from the SHARK data base since we produced the first version of the plot, for reasons that we do not control.

> *Figure 4: The curves look very smooth. Is it really annual means or were they additionally smoothed? In addition, most curves don't look as if they reach a steady state in the last decades. Wasn't this a prerequisite for the Taylor expansion? (you mention it later in lines 284ff.)*

This is annual means of the volume averaged salinities without additional smoothing. With regard to steady state, the CTL run is not totally in equilibrium and the perturbation runs are therefore also not in steady state. It is more important that the difference between the runs approaches a steady state. This is not totally the case and will never be so, but we are happy with the degree of response we get during our 57-year runs.

> *Figure 6: Resolution is too low (also check resolution of other figures; they are not as bad but don't seem to be sufficient either). Also, I'm not sure whether I fully understand how the figure is composed. Do I get it right that you compute the Taylor coefficients (eq 3-7 plus reference salinity), then plug them into equation 2 and then vary SB and RP in equation 2 to explore how the salinity changes? Then this should maybe be reflected in the labeling of the x- and y-axes by labeling them "SB – SB_0" and "RP-RP_0" or so. Finally, is there a reason for the diverging colorbar? And if so, why is it centered around 8? Wouldn't it make more sense to center it around the reference salinity?*

We have improved Figure 6 with improved resolution and new labels. We have also added the points corresponding to each run to make it clearer to what degree the figure represents the actual simulations. Finally, we have removed the colorbar, since the values are given in the contour plot.

> *Figure 7: Axis labels are very small (maybe also check the other figures). You might also add a 1:1 line to better see deviations from the perfect correspondence. Is it RP + 2k? Or RP_0 + 2.5k? (and also SB_0 + 2h)*

We have improved the figure. When doing so we discovered an error in the calculation of the most extreme run, which is now situated closer to the 1:1 line.

> *Figures 8 and 9: Salinity units at the x-axis missing. Maybe, there could be a separate panel / inlet showing only the maxima (i.e., the points where outflow changes to inflow) – could be interesting to see how the x- and y-values of the maxima depend on the perturbation factors. What's the resolution of your salt axis (is it large enough to properly resolve differences in s between the maxima?)?*

We have added the salinity unit. We think we can avoid the use of additional panels since the figure will be in higher resolution making it easier to see the difference between the curves. The resolution is 1 psu, which to some degree makes it difficult to see the exact point that separates inflows from outflows. That point is not that important for any of the conclusions though.

> *Figure 12: y-label missing. In addition, although it's mentioned in the text, the caption should mention that the figure refers to the sill transect*

The y-label has been added and the text modified.

Answers to referee #2. Referee comments in *italics* and our answers in normal text.

Review of manuscript "Response of a semi-enclosed sea to perturbed freshwater and open ocean salinity forcing" by Arneborg et al. 2025

This study uses high-resolution model sensitivity experiments to examine how the Baltic Sea's steady-state salinity responds to variations in freshwater forcing and salinity at the boundary of North Sea. From these experiments, the authors constructed a second-order polynomial that relates the basin-mean steady-state salinity to changes in freshwater forcing and boundary salinity. Results show that the Baltic Sea's response to freshwater forcing is large and non-linear, whereas its response to boundary salinity is more linear but less significant. The authors also analyze the impact of freshwater forcing and boundary salinity changes on the freshwater volume fluxes in and out of Baltic Sea driven by circulation changes.

Overall, the manuscript presents some important results, but it needs some revising and reorganizing before it can be considered for publication. The detailed comments are provided below:

Thanks for a detailed and constructive review. We have answered the comments below, and described how we have improved the manuscript according to the comments where relevant. We refer to the changes by line numbers in the marked-up-file:

1. *The study examines the sensitivity of Baltic Sea salinity to net freshwater input into both the Baltic and the North Sea, as well as to variations in salinity at the North Sea boundary. However, it is not clear which specific boundary of the North Sea is used for prescribing or evaluating the boundary salinity. Could you clarify this, and indicate the location on Figure 1? Additionally, it is very likely that changes in the net freshwater forcing would influence the boundary salinity in the North Sea itself. If so, how do you separate the individual impacts of the freshwater forcing and boundary salinity within your analysis?*

We mean the open boundary salinities and have made this clearer in the text (lines 230-231). It has also been made clearer that the boundary salinity change is intended to represent changes in Nort-East Atlantic salinities rather than salinities in the North Sea (e.g. line 20, lines 64-71, and lines 393-395).

The separation between boundary salinity and fresh water forcing is done by construction in the experiments. When we raise the runoff in the North Sea we freshen it a little bit. Whatever, presumably small, effect that has on our experiments counts as a runoff effect, not as an open boundary salinity effect. So, they are naturally separated by the experiment

The influence of fresh-water forcing on salinities in the North Sea are seen in Fig. 5 for the RP+ and RP- cases. At the boundaries the influence is almost zero. This may partly be because the boundaries are specified so, and in reality there may be an influence caused by recirculation of outflowing water into the domain, which we do not capture with a one-way coupled model. The focus in this study is on the changes in the Baltic Sea, and we have placed the open boundary further away from the Baltic Sea than most previous studies. The inflows to the Baltic Sea do include influences of outflowing fresh water to the North Sea, so we believe that we do have a good separation between impacts of freshwater forcing and boundary salinities at the boundary of the model.

2. *The figure captions are vaguely written. For example, in Fig.2, it is not clear during which period the observed salinity profiles are compared with the hindcast profiles? Are these averaged over a period?*

The time period has now been included in the captions. The solid line is the temporal mean and the dashed lines are the 5th and 95th percentiles as described in the caption.

3. *Figure 2: I suggest plotting the observed salinity profiles in thin lines rather than dots. The multiple grey dots at each depth level*

We tend to think that is nice to see the observational points as in the present plot.

4. *Figure 3: Please report quantitative statistics for these comparisons. How do the mean and standard deviation of salinity differ between the observations and the hindcast experiments? It would be helpful to list metrics such as bias, root-mean-square error (RMSE), and correlation coefficients.*

From the figure, it appears that the bias in the hindcast salinity increases with depth. To better illustrate this, you could include a vertical profile of correlation coefficients (with statistical significance indicated) to show how model–observation agreement varies with depth.

We could add these metrics, but we do not see the value of doing so for this paper. If doing an intercomparison study or optimizing a model, such metrics would make sense, but here we do not see how the metrics would help the reader to decide whether the model describes the necessary processes or not. We don't know about any relation between, e.g., RMSE for salinity and the model sensitivity to boundary salinity changes. To help the reader, we do instead refer to the Baltic Sea Model Intercomparison project, adding a sentence: "Generally, the model salinities compare as well with observations as those of the better models in the Baltic Sea Model Intercomparison Project (Gröger et al., 2022) and the model is therefore well suited for the sensitivity study which is the focus of this work." (lines 213-215).

5. *Figure 4 needs more explanation. Note that the salinity trends are different RP- and RP-SB- runs as compared to the CTL and all other runs. Can this be explained? When you say yearly salinities, do you mean the mean salinity values averaged over each year? The caption is not clear.*

We have added more explanation to describe what the figure shows, including "when the freshwater forcing decreases (blue lines), the salinities increase" (lines 249-250). We write "yearly mean salinities averaged over the whole Baltic Sea volume inside the sill transect (Fig. 1)" in the text and "Yearly mean salinities for the Baltic Sea averaged over the volume inside the sill transect" in the captions.

6. *Figure 5: How do these salinity anomaly maps for different runs compare with the CTL run?*

The hindcast run (CTL) would only show zeros. We have added "(CTL)" (line 259) to make it clearer that CTL is the hincast run.

7. *Figure 6 caption is too vague. Specify the units.*

Caption has been improved.

8. *Figures 8 and 9: Over what period have these volume and salt fluxes been integrated? Is there no inflow of salt into the Baltic sea, given all positive values of F in Fig. 9?*

The period is 1990 – 2017 which has now been included also in the caption. There are inflows of about 250000 kg/s (maximum value of F) on average for the CTL case. Have we written somewhere that there are no inflows?

9. *Figure 11: In the caption, you need to mention that these are the differences in fluxes between the RP+ and RP- experiments.*

We have added the following to the caption: "The fluxes are based on the differences between runs RP+ and RP- but normalized to represent an increase in Qf of 100 m3/s."

10. *I recommend moving the equations and the calculations estimating percentage changes in salinity due to variations in inflows, outflows, and related terms from the Summary section to the Results section. The final section should focus primarily on synthesizing and highlighting the main findings.*

We prefer not to move these calculations and discussion. The main results of the study are the polynomial and the total exchange flow analysis results based on the 3D model calculations. The calculations in the discussion part of Section 4 are more speculative with a number of underlying assumptions, and are meant more as a suggested simplified interpretation of the Baltic Sea sensitivity to the investigated forcing factors than a firm result of the simulations.

11. *The manuscript would be strengthened if the authors placed their results in better context with previous studies. In the Introduction, the authors state that their work differs from earlier studies because the model includes the North Sea domain. Could the authors elaborate on what difference this inclusion makes for the analysis and interpretation of salinity variability in the Baltic Sea? How do the results compare with those from previous studies that did not include the North Sea*

We have extended the text comparing our results with earlier studies in Section 4 (lines 450-463). We will not focus too much on the importance of including the North Sea domain, but we have written in the introduction that this is important in order to study the influence of North Atlantic salinities on the Baltic Sea, which has been investigated to a very limited degree in the past (lines 65-71).