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 will improve the manuscript according to the comments where relevant:

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 will add information about the magnitude and variability of river runoff and about the spread in projections of future Baltic Sea salinities in the introduction. We will also add text in the introduction 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 will rewrite the last paragraph in the introduction 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, the conclusion, and in the abstract. 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 will also add 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 not 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 will extend the text in this section somewhat. 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 the causes of forcing factors for the annual sea level cycle is 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 will be added in the end of the discussion of modeling of strait flows.

- *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 will therefore be added in section 2.4.

Specific comments

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

We will add 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 will change this toe "mostly less than 12 psu". 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 will add information on scenarios and examples of pressures.

> *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 will change 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*

Will be don.

> *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 will modify "mainly" and "the main part" to "to a large degree" and "a large part", and mention the baroclinic exchanges. 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. We will add text on this.

> *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 will be corrected.

> *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 will expand somewhat on the discussion of this when discussing Fig. 6. 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 will remove this text since it is more or less repeated in section 2.4. However, we will expand 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."

> *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 will try to make it clearer.

> *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 will remove 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 will add lines in Fig 4 showing the averaging period to make this clearer.

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

The text will be changed to 1993.

> *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 will modify Figure 6 and add more contours, see also below.

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

That is another good point. We will remove 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. Will be corrected.

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 will remove 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, 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 will add a sentence to extend somewhat on the interpretation.

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 will try to improve the text to something like 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 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. Will be corrected.

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

That is true and will be made clearer in the text.

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 will be specified in the text. Where the 16 psu come from is unclear, since the TEF analysis gives 17.6. We will adjust the figures to this inflow salinity.

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.

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 will specify 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."

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

We will change 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 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 will adjust the manuscript according to these commente, 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 will try to get the basin names into the Figure.

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

The period is 1970 – 2017, which will 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 will add 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.

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.

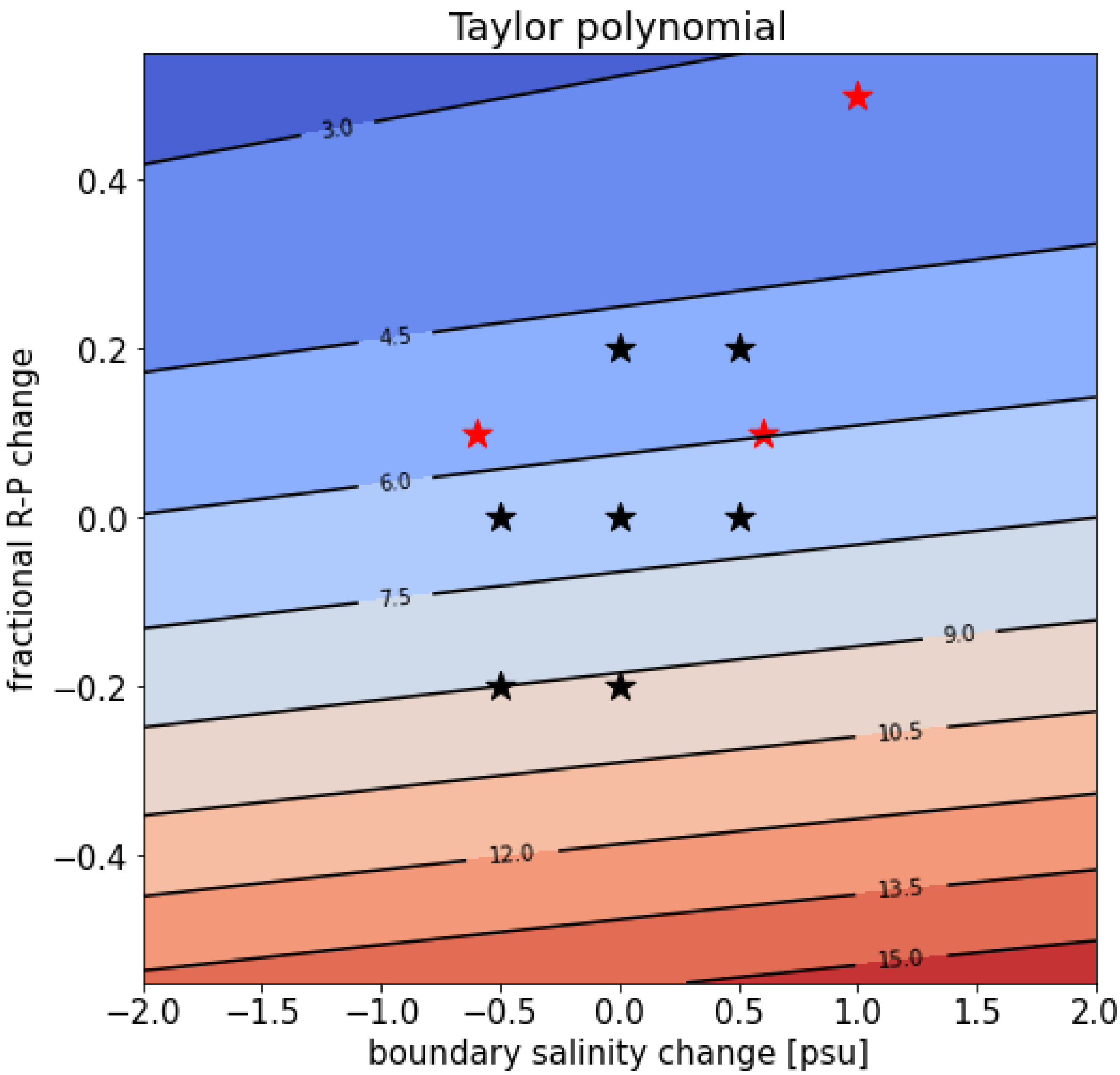
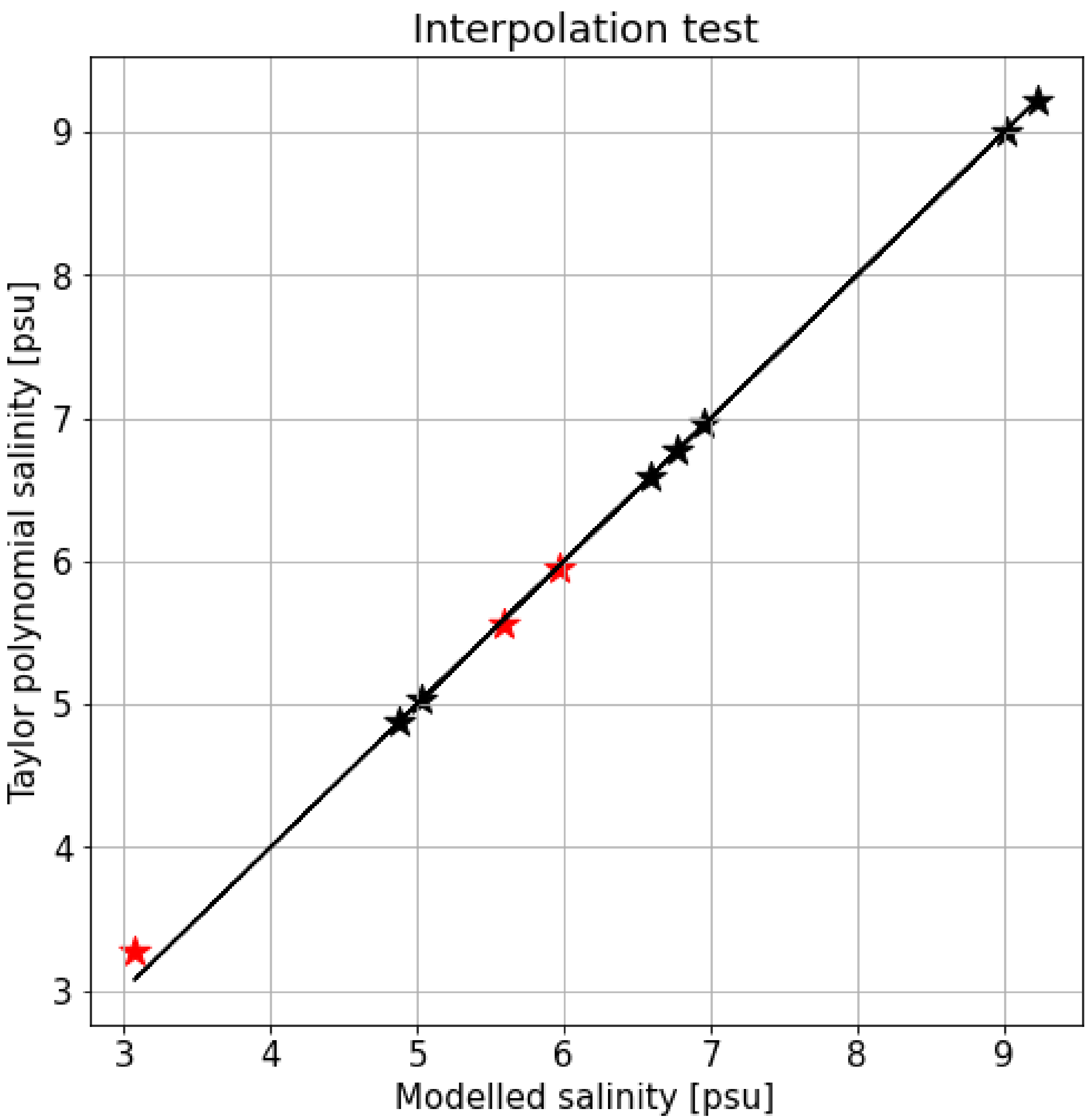


Figure 7: Axis labels are very small (maybe also check the other figures). You might also add a 1:1 line to better see deviations from the perfect correspondence. Is it RP + 2k? Or RP_0 + 2.5k? (and also SB_0 + 2h)

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 will add 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 will be added and the text modified.