Reviewer 2

Review of Cerkasova et al., Egusphere
This comprehensive study dealt with future environmental situation of the Curonian Lagoon as impacted by projected climate change affecting itself and its drainage basin by the end of the 21st century. The study is very well written, the methods are mostly appropriate (see one minor point below on flood timing and a point on standard deviations as an uncertainty metrics), the results are of high scientific value (although they partly repeat the findings from the previous study of the authors from 2023). What I particularly like in this study is its multi-dimensional character and looking at environmental situation at a holistic way. Of course, not all possible parameters are considered, but their number is higher than in the majority of comparable studies which I recall.

First, we thank the reviewer for the valuable comments and remarks. We revised our manuscript and answered the reviewers questions.

I do not like the title. It can be drawn from the title that the authors are “modelling uncertainty”. I do not think it is the case. They are modelling future environmental conditions and analysing uncertainty. The tile also suggests that the authors are “modelling the impact of uncertainty on a watershed and lagoon”. I do not think this wording is correct (is the “impact of uncertainty” really modelled here?). In summary, I suggest to rethink the title.

Authors’ response:

Noting the reviewers comments, we agree to change the title and reformulate it as:
Exploring Variability in Climate Change projections on the Nemunas River and Curonian Lagoon: coupled SWAT and SHYFEM modeling approach

The authors used a wide range of different analysis and visualisation techniques for different environmental parameters (line plots with moving averages – sometimes with trend lines and sometimes without; box plots – for entire period which does not say anything about direction of change; annual line plots with trend lines; combined annual and moving average line plots; heatmaps; changepoint detection plots). I personally think that the study would benefit from a more consistent method of presenting the results. If there are good reasons why the results for each parameter are presented in a different way, I failed to understand those. On the positive side, it is good that the Figure 10 summarises the results for different parameters in a consistent ways (could the burbot spawning period also be covered here?).

Authors’ response:
When performing the analysis, we were targeting stakeholders and the information which could be used later for the ecological evaluation of the system.
Since the graphs are hardly readable due to large dataset count, we added the trend lines where the dataset was less condense and the trend was readable in the graph. The plots with 10-year moving average were chosen for the parameters with high variability between the years and were challenging to read otherwise.
We agree that the data in a box-plot does not show the direction of change, however the direction of change is summarized in Figure 10. The numbers of the averaged water residence time are important for the ecological evaluation and we hope for higher citation of these data. However, we value the reviewer's comments and due to his concerns we have changed the boxplot picture

Fig. 4 to a line plot to be more consistent. Furthermore, we moved the boxplots to the Annexes to keep this information in a manuscript.
The figure types were chosen to best represent the variation in the projections for specific outputs. We harmonized the data representation by using the same color palette so that the reader would see consistency in between the figures. We changed Fig.7 and 8, to have the same colors for scenarios as in previous pictures.
According to burbot analysis we will add requested data to Fig. 10.
Although this might seem like a diverse visualization technique, we'd prefer to keep the current and updated figure types.

Specific comments:
Line 66 Avoid the term 'forecasting' (here and elsewhere), it is not a synonym of projections

Authors' response:

Thank you for the remark, we have revised the manuscript accordingly.

Your narrative in the Introduction would sound more convincing if you started your thread with the specific problems of the Curonian Lagoon ecosystem that require attention, in particular in the context of projected climate change. Then explain that tackling such problems requires integrated modeling frameworks. And only then start with your introduction on climate modeling uncertainty. Otherwise your first mention about „integrated modeling tools" in line 42 seems to come out of the blue.

Authors' response:

We revised the introduction based on your comments. We believe that the introduction is more convincing and clear now.

Line 100 Should be „two" models
Line 113: maybe „main outlets" instead of „two points"?
Line 118: maybe „key physical variables" instead of „all the physics"

Authors' response:

Thank you for your detailed remarks. We made the mentioned revisions in the updated version of the manuscript.

Table 1 Consider modifying Table 1 to distinguish between the actual model input data and the reference data that are used for calibration/validation. Besides, weather data seem to be missing.

Authors' response:

Missing information is added to Table 1.

Line 128: Please mention that you refer to the future conditions here. Also important to mention about the bias correction method.

Authors' response:

We added more details about the bias correction and referred to future conditions in Section 2.3.

Line 160: If your spring flood window starts on 1 Feb, then „spring" is maybe not the best term (cold season flood? snowmelt flood?), but this is not so important. More important is that, as I look at Fig. 5, it is clear that in many cases, particularly towards the end of the century, your date of peak flow occurrence happens on 1 February. I bet that in most of these cases the actual date of peak flow occurs earlier – in January, or maybe even December of the previous year! A more appropriate method for detecting trends in flood timing is by converting dates to angular values, using the circular statistics approaches (see e.g. Bloschl et al. 2017). Both the p values and trend slopes could be affected by this issue (although there is no doubt that regardless of the method, downward trends will prevail).

Authors' response:

Regarding the "spring flood" - a good observation, however this term is used by the local stakeholders. As we target them, we will keep the term, and add a clarification to the reader, why this particular period was chosen and why it is called "spring flood" (first lines in paragraph 3.1.2.).

"The high discharge of the Nemunas River and subsequent flooding of the delta region is a nearly annual event which occurs in late winter - spring season, and is referred to as "spring flood" in Lithuania. We use the same term in this study and consider the historic period of high river flows to be from 1st of February to 30th of April."

Regarding the flood timing - it is also a very good observation and suggestion! We will definitely include this in our future studies. For this paper, as we target a specific situation, we will keep the analysis and the results. The paper text is now amended to reflect this drawback and potential future work (last sentences in section 4.1).

"Moreover, the projections show that the timing of spring high flows are moved to the boundary of the analyzed period (February 1st), which indicates that the peak flow rate might occur even earlier. Although not analyzed in this paper, a follow-up study will explore these projections using more appropriate methods for detecting trends in flood timing, i.e. using the circular statistics approaches (Bloschl et al., 2017)."

Line 177: It is not clear to me why for all variables you applied Mann-Kendall trend detection, but for the burbot spawning temperature indicator you dealt with changepoints.

Authors' response:

The trend of annual temperatures shows a clear tendency for the increase, i.e. the gradual long-term change from past conditions. However, the modeling data also show some abrupt changes, especially at the beginning of the modeled time series in 90's. For the conservation purposes it is more important to detect those abrupt changes in the system and relate them to stock data, and fisheries practices. Identifying the further change points when the spawning season becomes critically short, it is possible to inform fishery managers about necessary measures for species protection. To sum up, changepoint detection is a more beneficial statistical indicator for the possible stakeholders (fishermen and relevant policy authorities). Nevertheless, to make the findings systematic, we add the trend analysis results to Fig. 10.

Line 205: It is not clear for which period the data underlying the box plots were aggregated. I can only guess it was done for the entire simulation period 1975-2100. If this is the case, I would recommend to split this long period into one or two 30-year periods. Aren't we mainly interested in climate change signals? You can than still make comparison between climate models, but not so much about their actual values, but projected changes, which in my opinion is more relevant.

Authors' response:

We already made the changes based on the previous comment. We changed the box plots to line plots, and moved the box plot to Appendix A. However, we agree with the reviewer that the figure splitted in a 30 year period could complement the analysis, therefore we added this picture to Appendix A as well.

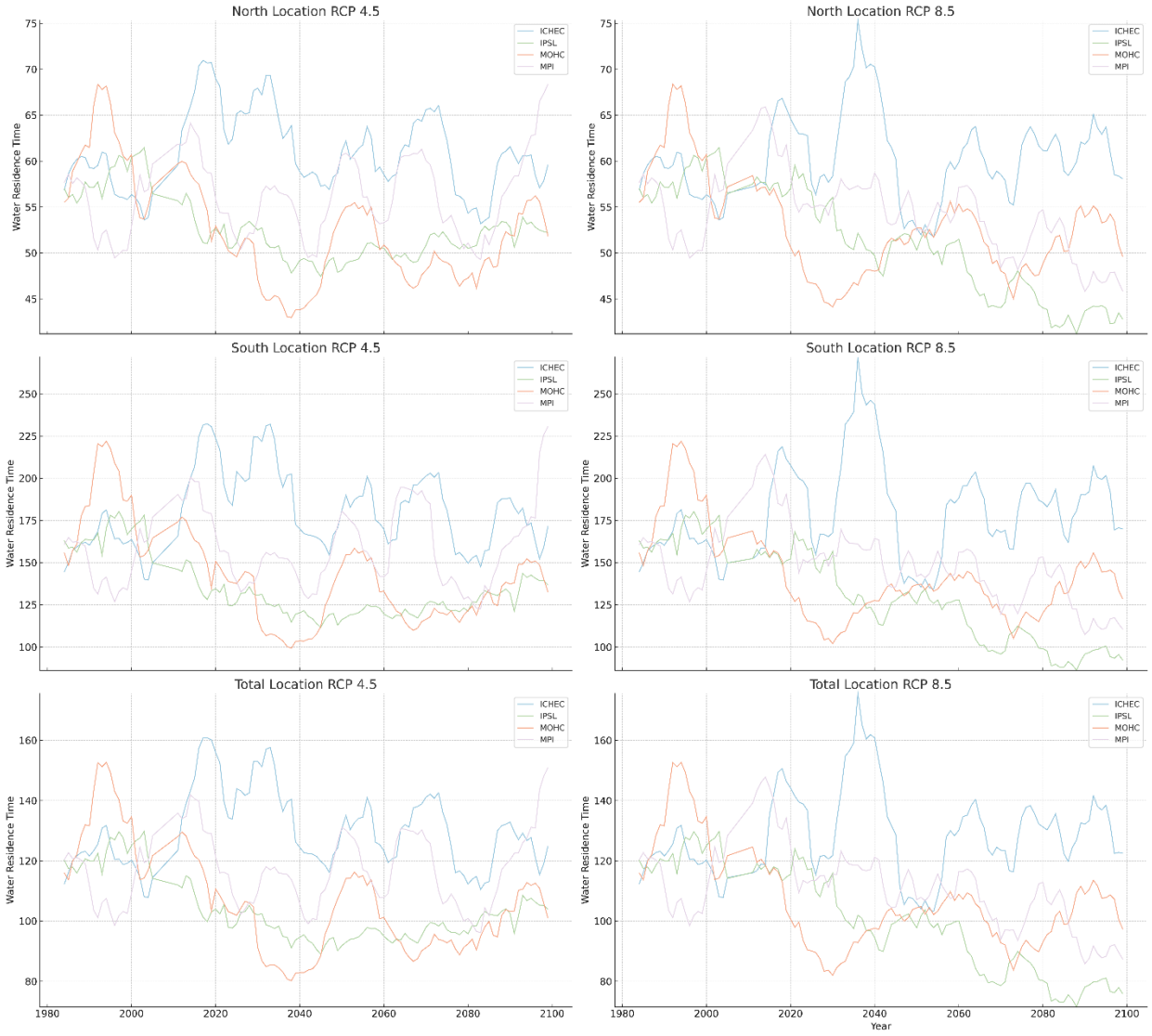
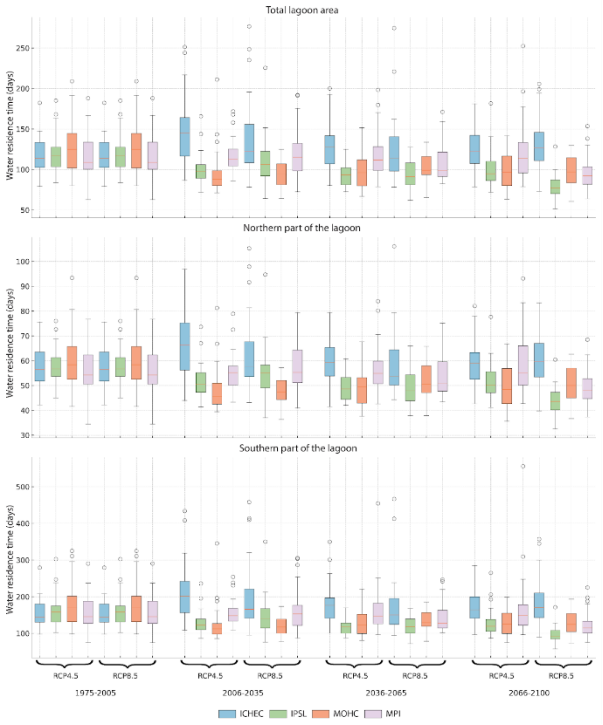


Fig. 4. Will be plotted as lines and the box plots moved to Appendixes



Line 237: I think that the sentence “TP loads could eventually fall below the targets” sounds overly optimistic, looking at Fig. 6. All trend slopes are positive for TP loads, it is just that there are some periods for which the loads could fall below the target, but there is not a single case for which the simulated values would be continuously below the target for a longer period.

Authors’ response:
That is an astute observation. We agree and rephrase the corresponding text to reflect a more in-depth analysis. Now the statement reads: “TP loads can fall below the maximum target during several brief periods, but the timing of this will depend on the actual climate scenario that unfolds.”

Fig. 7: Why in this plot you have shown the annual plot lines in addition to the moving averages?

Authors’ response:

This was done to display the underlying variability. While the moving averages help to smooth out short-term fluctuations and highlight longer-term trends, the annual plot lines provide a clear view of the year-to-year variability in the data. This combination allows for a more comprehensive understanding of both the general trend and the fluctuations that occur on an annual basis, offering a fuller picture of the data's behavior over time.

Fig. 10: Shouldn’t it be a table? In addition, Theil-Sen slope estimators are given in absolute values, which only allows to compare them between climate models, but not between environmental parameters. There exist simple methods for standardizing Theil-Sen slope (e.g. express it as an average change per decade relative to some “average” value for a given parameter).

Authors’ response:

Since the journal does not allow the colored cell tables, we call it a figure. The table with numbers only does not show a full window off the analysis, as a result, we would like to leave it as it is. The manuscript covers many parameters between the models and climate projections, therefore additional analysis (important and interesting it may be) is out of the scope of this paper. In addition, we have ideas for the future publication, where other methods will be used to evaluate results from climate simulations, including above mentioned (i.e. flood timing, splitting of the projection period, etc.).

Line 305: Section 3.3 I suggest to be more careful with the wording here regarding 'uncertainty'. The authors seem to treat standard deviation calculated from annual values of various environmental parameters as a measure of uncertainty, whereas in fact it just tells us about inter-annual variability. In literature, model spread is a common (although imperfect) metric for quantifying uncertainty. Model spread is quite nicely visible in Figs 3 and 6. For example, in Fig. 6, TN loads under RCP4.5 are characterized with relatively low and almost constant in time model spread. However, under RCP8.5, model spread is growing in time, and by the end of century becomes huge.
Standard deviation is not really a measure of climate model uncertainty – maybe just one of its facets. If standard deviations from two climate models significantly differ, it indirectly indicates that there could be an offset in future projections
One limitation of your analysis of standard deviations is that you include the entire simulation period of 125 years. It would be more meaningful if this long period was divided into shorter periods and comparison was done between them. And again, comparison of the model spread between the periods would be more informative about uncertainty than standard deviation.

Authors' response:

It is true that we did not follow Moss and Schneider, 2000; Manning et al., 2004; Mastrandrea et al., 2010 for uncertainty evaluation. To make the article more clear, we rename the section to "Variability in the projections". We reformulated this Section 3.3. and the rest of the manuscript and changed the uncertainty terms to variations or variability, where applicable.

In section 3.3 we discuss the standard deviation and variation of different models and we agree that these statistics only partly explain the uncertainty. The manuscript was prepared from the project's final results, which were carried out according to the project proposal and targeted at specific stakeholders, as mentioned before. Nevertheless, we agree that for a scientific paper it may be a bit confusing. We hope that this revised version of the text will clear this confusion.

Line 384: In your discussion about uncertainties, you should at least mention about one source that was neglected in this study, namely the regional climate models (RCMs). Your results are based on a single RCM, while different RCMs could yield different results, similar to GCMs. Where there any studies for this region which considered ensembles consisting of multiple GCM-RCM model combinations? Was RCM uncertainty component quantified?

Authors' response:

For consistency, we used the same forcing source for both models (SHYFEM and SWAT), and only the RCA4 met all the data set needs for both model setups. No other RCM provided such overlap of forsings and/or area. It is common that the Baltic Sea studies are carried out based on this model only (i.e. Huttunen et al., 2021; Rusu, 2020;Soomere, 2022; Bonaduce et al., 2019). There are studies, where different RCM were used for SWAT model (i.e. Plunge et al. 2022 and 2023), these studies are cited and are in line with our study results. This is a valuable comment, and we add a remark in the text addressing this concern.

References:

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Line 466: Shouldn't it be TN here?

Authors' response:

Yes, thank you. It is corrected. Now 544: "However, a severe discrepancy from the targeted loads of TP is projected forecasted by the middle of the century by all models and especially by MOHC"

In addition, we correct some small typing errors found after additional proofreading.