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.

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.

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?).

Specific comments:

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

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.

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"

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.

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

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).

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.

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.

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.

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

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).

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.

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?

Line 466: Shouldn't it be TN here?