

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2021-207

Anonymous Referee #2

Referee comment on "Temporally resolved coastal hypoxia forecasting and uncertainty assessment via Bayesian mechanistic modeling" by Alexey Katin et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-207-RC2>, 2021

This manuscript builds upon previous statistical models for hypoxia in the Gulf of Mexico to make a temporally resolved forecast. This work is potentially valuable for management of the ecosystem's fisheries and offers some important insights regarding the contributions of different sources of uncertainty to this type of forecast. The manuscript is well written and will be of interest to readers of HESS. I have one substantial concern and a number of comments for the authors to consider.

This modeling approach is impressive as it handles the different sources of uncertainty without excessive parameterization or an actual process-based model. To do accomplish this, the model relies on use of historical data for the trajectory of summer meteorology, discharge and loading. The authors select the most relevant years' summer records based on similarity of spring forcings. This selection process at line 115 is explained clearly, but more justification would be helpful as this is potentially influential for the forecast. What are the consequences if this selection step were omitted? What happens to predictive performance if 10 less relevant years are used? Does that lead to degradation of performance?

It appears that this temporally resolved forecast would, in fact, be static once the spring hydrology data are used to identify the most similar years to use for summer forcings. Could this somehow be informed by additional data from after May, perhaps using some information from previous years?

Figure 2. Are the hindcasted data for all of the years, or only those that were matched as most relevant and used for the model?

Figure 4. The the predictive intervals in the shaded region do not appear to increase over the course of the season, which is unexpected based on Figure 3. The authors provide parts of the explanation for this in lines 240-245, but I suggest adding an additional sentence that puts together the 1) disparity in uncertainty between inputs and fitted parameters and 2) change in contribution of inputs over the season.

Table 2 and Line 326. The caution about increasing uncertainties over the season is appropriate, but is not adequately captured in the figures or tables in the main text or supplement. Instead, the decrease in R2 is presented. Although the uncertainties are referenced in the text, it would be helpful to have a table or visual that shows the uncertainty for the pseudo-forecast by month (by that I mean IQR in units of HA or BWDO)

Technical Comments

Table 2. What does the bold styling indicate? It appears to be the highest R2 of the four cases, but this could be explained in the caption

Line 91 - The rationale for this correction is clear and sound. Presumably there is some uncertainty associated with both the predictor and fitted parameters of this regression. Were those carried forward into the forecast? I would expect that uncertainty to be impactful.

Line 135 - Did this occur frequently? Assigning a threshold for performance is appropriate, but consider adding some justification for why this particular threshold was selected.