Comment on bg-2022-4

Anonymous Referee #3

Referee comment on "Hydrodynamic and biochemical impacts on the development of hypoxia in the Louisiana–Texas shelf – Part 2: statistical modeling and hypoxia prediction" by Yanda Ou et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-4-RC3, 2022

Review of "Hydrodynamic and Biochemical Impacts on the Development of Hypoxia in the Louisiana–Texas Shelf Part II: Statistical Modeling and Hypoxia Prediction"

Summary

The manuscript employs a novel approach in applying numerical and statistical modeling techniques to more accurately forecast hypoxia area on the Louisiana-Texas shelf in the Gulf of Mexico. After selecting a set of predictors that are well correlated with hypoxic area in the Gulf, a long-term ROMS numerical simulation of this study area (2007-2020) is used to train an ensemble of statistical models using both generalized linear and generalized additive modeling techniques. The most promising techniques are then applied to global model outputs and USGS forcings to develop an accurate forecast over a later time period (2019-2020).

Overall, the manuscript describes a highly applicable and useful approach to rapidly forecast hypoxic conditions using a statistical ensemble. This approach appears to offer multiple benefits to past forecasts, and would serve as a helpful template for other coastal areas as well. The paper utilizes a limited number of explanatory variables to achieve a good fit, and I think that the predictors they use are appropriate and highly applicable to hypoxic area estimates. I've tried to include many notes to summarize these points, but this is not an exhaustive list.

Major comments

General:

There is a fair amount of general awkward phrasing and minor grammatical and spelling errors, but I don't find that they hinder my own understanding of the content.

Introduction:

I think that this section could be broken up into three sections as opposed to the 2 paragraphs it has now. Currently, only one sentence discusses the ecological/societal consequences of hypoxia in this region, and the authors immediately begin discussing the predictive capabilities of previous forecasting efforts. In my opinion, there could be more motivation in the first paragraph that illustrates why hypoxia forecasts are important and useful, and the benefits that environmental managers and others could gain from an accurate forecast. Otherwise, this reads a bit more like an interesting scientific modeling exercise done for its own sake. The second paragraph could then focus on past efforts to create a forecasting system, while the final paragraph could talk about some of the shortcomings that this model ensemble will address.

Methods:

I have some minor questions about the equations described for the hydrodynamic-related predictors section, but I don't think that they are likely to alter the conclusions of the paper in a meaningful way.

Discussion:

Would suggest renaming this section as "results" since a discussion section is typically what is described in the conclusions section here.

Conclusions:

I think that the paper would benefit from a more comprehensive conclusion that reiterated some of the broader implications and benefits that could come from this hybrid ensemble approach. The final two sentences are really just devoted to saying that this is the first of its kind, which again reinforces some of the issues I mention in the introduction related to this being a pure modeling exercise.

Specific Comments

Line 15: It may benefit the reader to include a percentage value in comparison to the low RMSE value of 3204 square kilometers, which may be quite large in other coastal systems.

Line 20: Suggest removing the words "by far". Because this model is the first to do this, the modifier "by far" suggests that no other groups are anywhere near this operational capability. I'm not sure if this is the intent, maybe this is meant instead to say that this ensemble model has the highest performance skill "by far".

Line 25: Suggest changing to "shelf-wide" here and elsewhere in the paper

Line 30: I've seen "destruction" of hypoxia used more often than "deconstruction" in the literature, suggest making this change

Line 41-43: Awkward phrasing, cut out "however" from sentence

Line 46-47: Suggest rephrasing as "An additional Bayesian model applied to summer bottom DO predictions accounts for May total nitrogen…"

Line 49-52: Suggest rewording as "Mechanistic prediction methods have also been applied by Laurent and Fennel (2019) to develop a weighted mean forecast that is calibrated using May nitrate loads and three-dimensional hindcast simulations over the period 1985-2018. Once calibrated, the model only requires May nitrate loads as an input to produce the seasonal forecast for a given year."

Line 55: Suggest changing "shortages" to "drawbacks"

Line 55-59: Remove periods before points 2 and 3, otherwise you can remove the colon and break them all up into single sentences. Point 2 could also be reworded slightly, reads awkwardly now. Change "year-to-year" to "interannual"

Line 61-62: Suggest rewording to something like "Here we aimed to provide a new technique in HA prediction that considers both stratification and biochemical effects, and accurately produces daily forecasts of HA based on selected predictors' own forecasts."

Line 65-67: Hypoxic volume really hasn't been mentioned up to this point in the manuscript, and here you say that it will be neglected because HA is a better predictor anyway. Would suggest removing these sentences altogether.

Line 71-77: I understand that some of the data used for model evaluation are described in the companion paper, but this section seems to be much more focused on derived model inputs (e.g. reanalyses and model outputs). Suggest changing the title of this section to reflect this better.

Line 87: Suggest changing to "… the amount of energy per volume required to homogenize the entire water column"

Line 95: Change "… are other two factors influencing" to "are two other factors that influence"

Line 95-96: Could be worth mentioning that the effect of tidal mixing on stratification is neglected in this study site, since it's included as an additional term in the Simpson 1981 paper.

Line 98: The first term on the right-hand side of this equation is negative in Simpson et al. (1978), but it seems like the way that this has been defined (reversing the position of water density and depth-integrated water density), that this may actually be referencing the equation of Simpson 1981. Equation 1 in Simpson 1981 also does not have "h" in the first right-hand side term, but I'm unsure if this is an error on Simpson's part since it appears in the 1978 paper. Suggest changing the reference and/or modifying the equation (may be easier just to change the reference rather than redo calculations/figures).

Line 110-111: Suggest referencing figure 1a here as was done in lines 90-92.

Line 126: Suggest changing "… estimated for the following" to "estimated by"

Line 128: I am having trouble understanding why this equation does not match what is shown in equation 2.27 of Monteith and Unsworth (2014). It looks as if some simplification occurred such that the denominator of the exponential (T-T', where T'=36K in Monteith and Unsworth) was incorporated into the numerator in the manuscript. However, when I plot the two curves against each other I find that they are unequal, and the gap increases with increasing temperatures. At 20 degrees C, for example, this is equal to vapor pressure difference of approximately 23 Pa. Is this a relatively minor difference, or is this likely to strongly affect the correlation found when combined with W^3?

Line 142-143: Here I would also suggest pointing the reader to figure 1a as was done in lines 90-92.

Line 145-146: Suggest changing phrasing to "However, global forecast model systems like HYCOM do not currently include biochemical fields."

Line 156: Suggest removing this sentence and adding the correlation metric to the sentence that describes it first from lines 153-155. This earlier sentence could then read "… calculated as 19 days ($R^2=0.8157$, Figure A2a)."

Line 158: Is there a reference for this decomposition rate coefficient, or has this described in more detail in the companion manuscript?

Line 163-165: I would suggest immediately describing these variables as PEA_heat, PEA_wind, and DCP_temp, rather than defining them here again.

Line 166: Can you better define what it means when you state that "multicollinearity may become a problem"? Maybe adding a short technical detail on the ramifications of this would be helpful to the reader.

Line 169-170: Are all the grid cells the same size for this model domain? Is this described in more detail in the companion paper?

Line 188: Change "rest" to "remaining"

Line 190-191: Change "is chosen randomly" to "are chosen randomly" and "is grouped into" to "are grouped into"

Line 192: Suggest changing to "split at intervals of 5000 km^2"

Line 272: Some awkward phrasing "… which impose more threatens to the shelf ecosystem."

Line 299: Misspelling of "procedure"

Line 332-333: Suggest change to "… tends to underestimate HA peak estimates (like those seen at samples 310 and 920)"

Line 351-352: What daily data are referred to here, the outputs derived from HYCOM or the nitrate and nitrite loadings from USGS?

Line 378-381: These two sentences are a bit repetitive and could be combined. I'm also not entirely clear about whether HYCOM is expected to integrate USGS runoff in the future. Is the use of daily estimates part of long-term plans for HYCOM simulations?

Line 399: Some awkward phrasing, suggest changing to "… HA forecast capable of explaining up to 80% of the total variability"

Line 404: "… on HYCOM,s"