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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Lake thermal structure drives inter-annual variability in summer anoxia dynamics in a eutrophic lake over 37 years" *by* Robert Ladwig et al.

Anonymous Referee #1

Received and published: 24 August 2020

Thank you very much for the opportunity to review this manuscript describing the internal (physical and biological) and external factors leading to inter-annual variability in the extent anoxia in Lake Mendota. This study uses a combination of three very different types of models to evaluate these various factors. I found this paper very interesting, very well written, and may be very useful to the scientific community. I applaud the authors in using this multi-model approach. However, I think two of the three models have serious flaws that need to be addressed prior to publication.

My main concern is that one of the main takeaways from this paper (internal productivity has very limited effect on interannual differences in anoxia) may not be true. It may

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be true that physical mixing drives the overall extent of anoxia (baseline), but I think it is too early to say interannual variability in productivity has little affect. I think two of the models need to be reevaluated prior to making those conclusions:

GLM-AED2. GLM-AED2 simulated the annual progression of anoxia very well, and simulated the importance of stratification driving not only the average changes in DO depletion but also much of the interannual variability in DO associated with stratification. But the model did not capture the interannual variability in surface productivity that may drive the other interannual variability in DO. It clearly could not reproduce the interannual variability in AF. This model had an R2 of only 0.08 and a negative NSE. Part of the problem may be that the model is trying to simulate two very different lakes (one without spiny water fleas and one with them) - all with one set of coefficients (that may not even represent the lake in the first place). Without simulating the big biological change, I am not sure you can get there with this model.

Suggestion: Use GLM-AED2 to only simulate one of the periods, either prior to or after the change in biology. If this does not improve the overall ability to predict AF, then the phytoplankton parameters may have to be adjusted. Without being able to predict most of the variability in AF, I really don't see its use in this paper.

Regression model. I think there are four flaws in the approach used here: 1. Not including loading and in-lake variables that would potentially describe interannual variability in productivity. 2) Including modeling results in a regression analysis. Given that the model does not simulate AF, it appears that using modeling results in the regression may just add noise to the regression or reinforce parameters that are in the model. 3) Using one correlation and one regression to simulate two very different types of lakes, and 4) Using way too many variables in a single multiple regression equation. Even though it appears based on stepwise regression all of the variables are significant, I think it is way over parameterized. Several studies have shown that with regressions using very few observations, many variables can look significant – with each variable coming in to describe one or a few unique observations. A good rule of thumb is to

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keep only 1 variable in a multiple regression for each 8-10 observations. So for this regression with 37 (and actually only 28 monitored years) observations, there should only be maybe 3 independent variables.

Suggestions: 1) include variables like actual loading rather than concentrations, include variables that describe inlake productivity (total phosphorus, chlorophyll, Secchi). I am not sure what GPP actually represents. If GPP does describe the changes in chlorophyll, it should be stated. I also do not think it is a good idea to include things describing DO (like maximum height of anoxia) when you are trying to predict AF (this can get to circular reasoning) 2) Only use the 28 actual observations in the correlations and regressions. 3) Look at the correlations for each part of the record (different biological conditions) separately. 4) Stick to correlations and not use regressions. Or if you do look at regressions start simple and add variables only significant when you consider the change in AIC.

My other main concern is that the deductive model seems to say that it is the inlake productivity that is driving the interannual variability in AF, and the other models seem to be saying it is driven by physics and sediment oxygen demand. Maybe with further analysis the models will come to more similar conclusions. If I am wrong with this interpretation, it should be explained better.

Specific Comments: 1. Line-125. Very little information is given on the actual loading. Can these estimates be compared with others?

2. Line 128 – It says here to look at Weng et al. 2020 for a description of the loading regression, but when I look at that paper, I don't see any more than they used a regression, with no statistics either for the monitored sites or the watershed modeling.

3. Line 136 – You mention other data earlier years, who collected that?

4. Line 159 – See comments above about mixing real observations with modeled data.

5. Line 190 – There are lots and lots of parameters in AED, how did you narrow it down

to the ones to start with, you need to start somewhere?

6. Line 215-Can you expect to capture interannual variability in productivity without having the phytoplankton simulate things specific to Lake Mendota?

7. Line 260 – My bet is that anoxia does occur under the ice, but you can't get that from one measurement during the winter.

8. Line 267 – Loads would be better than concentrations. Concentrations generally do not vary much from year to year. If you did really use loads, you should state that. But you should describe this better.

9. Line 273 – See comments above.

10. Line 278- Since Gross primary productivity (GPP) is your only inlake productivity term, you should describe this in more detail. If this is directly related to chlorophyll, maybe this addresses some of my concerns.

11. Line 281 – Consider dropping this whole paragraph.

12. Line 306 – The major conclusion of the deductive model says that water column respiration controls oxygen depletion, yet everything else seems to point to physics. Am I missing something here?? Is water column respiration the cause and physics drives the variability in this? More explanation is needed.

13. Line 322 – Please give the stats for DO. This is really what matters in this paper, especially in the part that varies from year to year.

14. Line 333 – Reorder this paragraph to put the peaks later when you talk about summer.

15. Line 345 – This paragraph could probably be deleted.

16. Line 370 – It says the model captured annual anoxia events. Yes it described the annual development, but right now it does not seem to have any interannual capabili-

ties??

17. Line 374 – See above.

18. Discussion – Need to tie all three model results together better. Right now two say physics and one says productivity.

19. Line 394 – Although I completely agree with you, I am not sure where this comes from given the model results.

20. Line 420 – Again I agree with you, but other than one variable in seven in the regression, I don't know where this comes from. Need to describe this variables importance.

21. Line 425 – Maybe the lack of relations is due to using loading concentrations rather than actual loads. This is what I think the methods say.

22. Line 433 – Is it loads or concentrations. If it is concentrations, that wouldn't surprise me at all. It is not the annual variations in concentrations that drive things, it is the difference in loads.

23. Line 440 - This could be an important point, maybe there is so much oxygen consumption in the bottom, that it dwarfs any water column consumption. But this disagrees with findings of the other models.

24. Line 445. The apparent changes caused by the Spiny water flea may be totally confounding any correlations, regressions, and your GLM-AED2 modeling. You may have to stick to one of the periods to really describe the effects of physics vs internal. Or have two different models.

25. Line 472. Rather than implementing a different type of dynamic model, maybe better capturing change in productivity and clarity, will help in describing the physics.

26. Line 481 – you didn't calibrate the biological parameters, so this should be rewritten.

27. Line 497 – Rather than thinking the deductive model is biased, maybe it is the only approach capturing the effects of the biology.

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