Responses to reviewers

Reviewer comments in black

Author responses in blue  
  
  
Reviewer #1: In ECOMOD-18-400, the authors develop a simple dynamic model of organic carbon in lake ecosystems and apply it to five different lakes.  Although the model is simple and makes a number of assumptions, the authors show that it can be calibrated and used to successfully predict the general trends in carbon dynamics of the five lakes.  The predicted dynamics are generally smoother than the observations, but this is an expected result of using such a simplified model with missing information such as the lake exchange with groundwater.  The authors are to be commended for generally following modelling best practices, including providing information about model construction and critical assumptions, calibration and validation steps, and sensitivity analyses.  Finally, the results are interesting and potentially useful in our quest to better understand the global carbon cycle and how it will be impacted by climate change.  The predicted shift to higher respiration and  
therefore C loss from lakes is not surprising, but it is useful to see this simple model capture and support this type of prediction.  It also does a nice job of highlighting missing data that might improve this understanding and the predictions.  
  
Minor Concerns  
  
1.      I would like to have seen results showing the potential impact of the assumed 0 groundwater inflow (outflow).  This seems like a common unknown that could have large impacts on the results, especially since Trout Lake had estimates of 19% input to the lake.

We agree with the reviewer that the groundwater contribution is a large limitation of our model. While conducting this study, we found it unfeasible to incorporate groundwater into the model for the following reasons a) groundwater contributions were rarely available, b) DOC concentrations of groundwater were not available, and c) It is well known that groundwater contribution changes with drought (major evidence in the Trout Lake watershed) and with anthropogenic pumping (Lake Monona watershed). In the future research needs section of the discussion, we indicated groundwater volume and DOC concentrations as unknowns that must be addressed in future research.

Once DOC is in the system, the model does not differentiate DOC from groundwater vs. surface water. Adjusting the parameter for the proportion of total inflow as groundwater would therefore simply add more DOC to the system at concentrations of 10 g/m3, a value which was calibrated in Hanson et al. (2014) (who tested concentrations of 2-40 g/m3). If we adjusted groundwater volume, we would be assuming changes in DOC inputs directly proportional to the change in volume because we have no dynamic groundwater DOC concentrations. Adding quantities of DOC through groundwater would then complicate our ability to balance the overall budget based on the other known OC values, so we decided to maintain constant groundwater contributions of DOC.

2.      On line 199 you use the term "static parameters," which I am not familiar with.  Could you please better define this term?  My understanding is that the very nature of the term parameter is that the value is unchanging in the context of a given model, so why is "static parameter" not redundant?

We agree that “static parameters” may be confusing as a term. We therefore adjusted this phrase to “literature-based” to differentiate from calibrated parameters (revised lines 198-199).

3.      Page 22 - Figure 3 is repeated.

We removed the second figure 3. Thank you for catching this.

4.      Table 5 - Why did you choose to separate the SD from the mean?  I wonder if this would be better reported together as mean (SD).

We had no compelling reason to separate means and SD into different rows, so we adjusted Table 5 to report means and standard deviations (in parentheses) in the same rows.

5.      Lines 34-45 - Nice comparison to Hanson's results.  This is very useful.

Much appreciated comment.

6.      Line 112 - Yes, data limitations may help explain the low NSE score, but it maybe that the observed large swings in OC maybe driven by more detailed ecological processes (e.g., food web) not included in this model.

This is why we begin the discussion that we were able to recreate long-term DOC by balancing the major components of the budget. We agree, however, that such large swings may be driven by processes not in the model and that food web dynamics represent a good example. We therefore added the sentence” Additionally, food web dynamics (e.g., grazing) may also help explain large fluctuations in allochthonous inputs or poor NSE values.” (revised lines 589-591)

7.     It would be helpful in the discussion to again re-emphsize the spatial distribution of the lakes investigated.  Do the authors expect that the model would apply equally well to lakes in tropical or subtropical latitudes?  
  
We agree that the generalizability of the model should be more explicitly discussed. We therefore added to the first paragraph of the “On-going research and data needs” section of the discussion:

“Although we designed our modeling framework to be flexible across different lake ecosystems, our study contained four north-temperate lakes and one arctic lake, all of which were deep and dimictic (summer and winter stratification and spring and autumn mixing of the water column). Literature-based parameters were obtained from previous research on these lake ecosystems and may not apply in all other lake ecosystems. Future work should include additional high-latitude, tropical, or shallow lakes to test the generalizability of our model across a more diverse set of lake ecosystems than those included in this study. Nonetheless, part of our intention for including model data and code with this manuscript was so that future work can build off our model and make adjustments as more data across more diverse lake ecosystems become available.” (revised lines 551-560)  
  
Reviewer #2: The manuscript proposes a mass balance model to describe carbon cycle in lakes including   
         autochthonous and allochthonous organic carbon sources. The model is built on formulating major inflow and outflow processes of organic matter and respiration. The model is validated against several data sets from lakes with different geographical locations. Overall the manuscript is well written, the method is nicely explained, results are discussed and associated uncertainties of proposed model are described. I have some comments and recommendations that I believe would help the manuscript to be stronger and accessible for broader audiences.   
My main concern was about the predictive capability of the current model, especially under climate change scenarios. At the moment it is not clear how such model could be used to provide reliable predictions. I was wondering if it would be possible to divide the data sets to "training, test, and validation sets" and evaluate if the model could predict new datasets after training.

In our study, we did not separate the observed data into training and validation sets. This was done because we were interested in using the model to understand the drivers of DOC variability over the time series rather than using the model for prediction.

However, the reviewer raises an important concern about the predictive capabilities of the model, especially under climate change. We hope that the results of this study can be incorporated into analyses that focus on the climate change impacts on DOC cycling. While we took a broad look at DOC drivers, our model could be employed to look specifically at the impacts of changing temperature, rain events, groundwater, or land use change. Future work focused on predicting carbon cycling would employ modeling methodologies more suited for that task.

The last discussion section covers implications of our model and its findings under climate change and the last 2 sentences of the discussion now reads:

“Although our model was not designed as a predictive tool, our findings illustrate the usefulness of a dynamic mass balance model for highlighting key global change processes and interactions that ultimately influence the role of lakes in global C cycling. Improved estimates of the contribution of lakes to global C budgets should account for the source and degradability of total OC loads and consequent effects on respiration and burial.” (revised lines 628-632)

Another major concern is the lack of sufficient information on how temperature effect on respiration and other processes are implemented in the model. While it is mentioned in the text that Q10 temperature adjustment is used but it is not described how Q10 is obtained (field data?) and how does Q10 varies over different lakes. Uncertainties associated with Q10 are also not described.

This was an oversight on our part, and more specific details have been added to the manuscript. A temperature multiplier of 1.08 was used for all lakes. This equals a Q10 value just over two. As no lake specific numbers are known, this is a common approach used in the literature. We address this uncertainty when referring to uncertainty surrounding NPP estimates. (revised lines 256-257)

Other comments:   
-       In table 4, relative standard deviation could be more informative measure for the goodness of fit instead of RMSE.

RMSE is standard practice for evaluating model error. It compares the distance (spread) of the residuals between observations and predictions. Relative standard deviation compares the spread (precision) of a vector. We’re not sure how relative standard deviation could be more informative.

-       Q10 is confused with Qoutflow in Equation 1.5.

Equation 1.5 quantifies downstream OC export as a function of allochthonous DOC produced and quantity of outflow (Qoutflow), so we believe the equation is correctly written in the methods text and Table 3.

-       Does model account for variations over water column for instance for oxygen and temperature? Would such variations affect the results of the simulations?

The model uses surface water temperature and mixing depth to estimate autochthonous OC. We used mixing depth to account for changes in temperature as a function of depth in the water column; however, during the summer stratification period when most biological activity is occurring, water temperature and dissolved oxygen vary little from the water surface to the mixing depth.

-       Is it possible to validate the results of Figure 4 with field observations? If not, does a choice of input parameters would affect the results?

Figure 4 represents modeled OC fates for which we do not have corresponding field observations. Essentially, not having such field observations is part of the motivation for developing the model. The best we can do is compare the output OC fates to values from other published studies. We cover this in the “Recreating lake processes” section of the discussion.

Choice of input parameters does affect results, which is the motivation for the sensitivity analysis of the calibrated parameters (Fig. 3).

-       Does results in Table 5 extracted from simulations? Please mention it in the caption.

For clarity, we added “modeled” to the title of the table so that readers will understand the mass balances in the table were model outputs.  
  
Ali Ebrahimi (MIT)