Editor's comments  
The authors performed the work on dynamic modeling of organic carbon fates in lake ecosystems. Review reports are positive for this manuscript but it needs revisions before final acceptance. Three questions are mainly raised by the reviewers, first regarding the ground water inflow and outflow, reliable prediction from the model and the lack of sufficient information on how temperature effect on respiration and other processes are implemented in the model, authors are informed to address these questions. In view of the comments and reports of reviewers, your manuscript has been evaluated and you are informed to resubmit the manuscript after proper and thorough revisions in accordance with the comments of editor/reviewers. Review reports are appended below. Thank you for your submission in Ecological Modelling.  
Santanu Ray  
Associate Editor  
Ecological Modelling   
  
  
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.        
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?   
3.      Page 22 - Figure 3 is repeated.  
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).    
5.      Lines 34-45 - Nice comparison to Hanson's results.  This is very useful.    
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.     
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?  
  
  
  
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.   
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.    
Other comments:   
-       In table 4, relative standard deviation could be more informative measure for the goodness of fit instead of RMSE.  
-       Q10 is confused with Qoutflow in Equation 1.5.   
-       Does model account for variations over water column for instance for oxygen and temperature? Would such variations affect the results of the simulations?   
-       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?   
-       Does results in Table 5 extracted from simulations? Please mention it in the caption.   
  
Ali Ebrahimi (MIT)