Reviewer #3

We appreciated for the comments and suggestions that significantly improve the quality of the manuscript. We have addressed referee 3's comments point by point and will make changes in the revised manuscript, which are detailed below.

1. The authors represent a modelling approach to quantify CO2 emissions by integrating a terrestrial and an aquatic model. They applied their model framework to assess the effects of reservoir creation and the following CO2 emission on carbon dynamics. In my opinion this work represents an important step towards a more complete understanding of the carbon cycle. It stresses the importance to integrate terrestrial and aquatic systems to fully map the different components of the carbon cycle, which is especially importance with respect to climate change. Although I see several issues that should be solved first, I recommend publishing the manuscript in Biogeosciences after revision.

MAJOR COMMENTS:

2. A) One concern is that to me it seems that the authors did the calibration of their model and the validation not with independent data. It should be made more clear which data have been used for calibration (P1 L16 'using the measurements : : : in a _600 km2 boreal hydroelectric reservoir, Eastmain-1') and for validation ('We then evaluated the model performance against observed CO2 fluxes data from an eddy covariance tower in the middle of the EM-1 reservoir').

   Author response**：** *We recognize the importance of not evaluating the model on data that was used for calibration, and we did not – we used independent data set to calibrate and validate the model. In our study, we calibrated the model using the mean concentrations of DOC and DIC in the water column and tested the model performance by comparing the outputs against estimates of water column respiration, POC sedimentation, and CO_2 fluxes across air-water interface. We do not spilt the CO_2 flux data and use part for calibrations because the EC flux data was spotty (only 23% of the measurements from the reservoir were retained – data was rejected because the wind direction was not from the open water surface and/or the conditions were not acceptable such as u* were too small). We only used observations in our evaluation. To our knowledge, there is no standard gap filling techniques for air-water gas fluxes. Unlike terrestrial ecosystems such as forests and grassland, reservoirs or lakes can store dissolved CO_2 and the instantaneous gas exchanges are only controlled not only by metrological factors (e.g., wind) but also the carbon supply (i.e., DIC concentration) from the water column to the surface, and the supply of DIC to the water column. DOC can be easily mineralized. Therefore, we chose these two variables to calibrate the model.*

3. (B) For comparison of observed and simulated data, or rather expected and simulated data I'd like to see more statistical tests to show if the differences are significant or not. Statements as 'reasonably well' (P1, L22) or 'greatly influence' (P12, L28) are not sufficient.

Author response*: We did use three statistical index or methods to show the model performance. See P10L29–34. We will add the notation of each statistical index in the text and emphasize them in the abstract. For the statistical analysis, we mentioned in P11L12 that if the mean change over 10% of the base run the model was considered to be sensitive to the parameters tested. This has also been pointed out by reviewer 2.*

4. (C) The conducted sensitivity analysis only includes Rw, FO2, air temperature and wind speed. It would have been interesting to see how sensitive the model reacts on the most important parameters controlling the processes in the water column, like decomposition rates of POC and DOC.

   Author response: *The sensitivity issue were also raised by the other two reviewers (see our response R1C3 and R2C9, R: reviewer C: comment). The decomposition rates of POC and DOC listed in the table 2 is the decomposition rate at 20 °C, and the real decomposition rates for organic carbon pools in the water column change with temperature. We kept parameter values as same as in their previous model. In our study, we wanted to keep it focus on our major aim, which is the flooding effects on reservoir CO_2 emissions.*

5. (D) It did not appear clear to me, how much of the model developments has been originally done by the authors and how much of their framework relies on the work of others. This should be stated clearly and possibly also indicated in an overview figure as Figure 1.

   Author response*: First, we contributed the linkage among the three models. For example, modifications for the water column carbon sub-model listed in section 2.1.1 was to incorporate dynamic thermal and water stratification module.*

   Second, we modified related components since it is possible to have more dynamic processes through coupling hydrodynamic component and the carbon cycle. Take GPP calculation as an example, the old version of lake carbon model calculated GPP using an empirical function of total phosphorus (constant input value) so there was no dynamics of GPP. We also modified production to include the influence of water temperature (calculated in the thermal module), mixing depth (from the thermal module), chlorophyll-a concentration (newly calculated using a function of POC concentration).

   Third, we modify a terrestrial soil biogeochemical model to simulate sediment carbon dynamics (section 2.1.3). Many process have to be added in the new sediment module. Overall, our new model can dynamically simulate reservoir carbon processing and flooding effects, while few models are able to.

   We revised the manuscript to make sure that our direct contributions are clear in the overall description of our model.

6. (E) I see some potential for improvement in the discussion.- P 13 LL14: The empirical model shows a decline period of 12 to 15 years. Another study (not including water column processes) estimates the period to be several decades. The author’s model (including water column processes) estimated a period of only 3 years. It should be more clearly discussed

that this inconsistency (a) shows the importance of including the water column processes and (b) shows that the implementation in the model can still be improved.

Author response: *This is a good suggestion. We, as we responded to comments by the first two reviewers, discussed more about the discrepancy between empirical study (Teodoru et al., 2012), previous modelling study (Kim et al., 2016), and ours.*

References:

Kim, Y., Roulet, N. T., Li, C., Frolking, S., Strachan, I. B., Peng, C., Teodoru, C. R., Prairie, Y. T., and Tremblay, A.: Simulating carbon dioxide exchange in boreal ecosystems flooded by reservoirs, Ecol. Model., 327, 1-17, http://dx.doi.org/10.1016/j.ecolmodel.2016.01.006, 2016.

Teodoru, C. R., Bastien, J., Bonneville, M.-C., del Giorgio, P. A., Demarty, M., Garneau, M., Hélie, J.-F., Pelletier, L., Prairie, Y. T., Roulet, N. T., Strachan, I. B., and Tremblay, A.: The net carbon footprint of a newly created boreal hydroelectric reservoir, Global Biogeochem. Cycles, 26, GB2016, 10.1029/2011GB004187, 2012.

7. The authors state (P13 L25) that they ‘did not incorporate methane production’. Please explain why do you think that ‘if methane production is included, the reservoir would probably need more time than reported here’.

   Author response: *The development of a methane module requires the incorporation of the cycling of oxygen, the modification of methane production in DNDC, the oxidation of methane in the water column, and the inclusion of convective and ebullition transport. These developments are substantial and beyond the scope of this paper (we have been developing a methane module for several months now and expecting to be publishing this work in the autumn). The statement in the manuscript is an inference: we are speculating that a portion of the methane produced in the sediments will be transported through bubbling. The larger of upward carbon fluxes of DOC, DIC, and CH_4, the longer the sediment loses carbon. We revised the paragraph to make the logic and words more precise.*

Minor comments:

8. (A) It would be helpful to show the sequence of processes calculated in the model. I assume this could be added to Figure 1.

   Author response*: Did as suggested.*

9. (B) Some specific questions: Why is GPP depending on DOC (Equ.1)? How did the authors convert form biomass to carbon (P5 LL5)?

   Author response*: The equations 1 and 2 are from Carignan et al. (2000). Their study was conducted for Canadian Shield lakes where our reservoir is located “… a negative effect of DOC on both GP and R. This effect may be due in part to a metabolic inhibition of photosynthesis and respiration by DOC… ” is asserted by Carignan et al., 2000.*

Generally, a coefficient of 0.5 is used for conversion of biomass to carbon in the model. We checked all units for the equations newly added to the model.

Reference:

Carignan, R., Planas, D., and Vis, C.: Planktonic production and respiration in oligotrophic Shield lakes, Limnol. Oceanogr., 45, 189-199, 10.4319/lo.2000.45.1.0189, 2000.

Figures:

10. -The figures only partly support the message of the manuscript (e.g. Figure 4 and Figure 5).

    Author response: *We deal with the reviewer's comments below by each figure.*

11. Figure 1 is not clear enough to show the important processes and fluxes in the model. Some arrows seem to come from nowhere (e.g. the solid line arrow 'Incident solar radiation', or one solid black line going down from the 'Passive humus pool'). Please clarify.

    Author response: *We revised the figure 1 to make it clearer.*

12. -In Figure 2 one cannot distinguish the temporal patterns the authors refer to in the text, especially in (c), (d) and (e).

    Author response: *We restructured the figure following the comment (two columns) by referee 1 and changed the symbols (from closed cycle to open triangle) to make it readable.*

13. Figure 3. The model's reaction seems relatively constant over the years, which could be a sign that important processes might be ignored or their implementation should be improved. Please discuss.

    Author response: *The modeled annual emissions are quite close to EC observations and the camber-based observations, with the exception year one chamber fluxes. The trends of the measurements and model output are decreasing over time. However, the standard error in the measurements is large so it is not a highly constrained test except that the simulated fluxes are the same order of magnitude as the observations and they have the same general trend. The large errors in observations is a reality.*

14. Figure 4. I can only partly agree with the conclusions the authors draw from the data shown in this figure, especially for the sinking rate which seems to be poorly reproduced. Why do the observed data don't have any variance?

    Author response: *From the literature (Teodoru et al., 2011), we do not find the variance of reported values. We speculate that the variance of the measurements should be larger than our modeled because the field measurements are typically instantaneous.*

For the sinking rate, the annual means should not have significant difference, as the simulated mean is within the standard deviation of measured mean. Unfortunately, there is only one year data available.

Teodoru, C. R., Prairie, Y. T., and del Giorgio, P. A.: Spatial heterogeneity of surface CO_2 fluxes in a newly created Eastmain-1 reservoir in northern Québec, Canada, Ecosystems, 14, 28-46, 10.1007/s10021-010-9393-7, 2011.

15. Figure 5. The figure does not support the statement that the 'annual CO2 emissions and benthic dissolved CO2 fluxes showed great sensitivity to Rw'. There seems to be some sensitivity but I would not call it 'great'. And besides I'd like to see more statistics on the significance of differences in general.

    Author response: *We defined that the model is sensitive to the tested parameter if the modeled results over 10% of the base run.*

16. Figure 7. What does this figure show? It doesn't fit to the caption.

    Author response: *We have revised the caption. It is "Figure 7: Simulated sediment carbon burial efficiency (= [F_{DOC} + F_{DIC}] / F_{POC}) under concurrent climate and hydro-thermal regime. If the net flux is from sediments to the overlying water column the burial efficiency is more than 1.".*

17. Figure captions should include more units. E.g. caption for Figure 5: 'The curves were smoothed using a moving average filter with a span of 5.' I assume 5 years. And in the caption for Figure 6 'The curves were smoothed using a moving average filter with a span of 5.' Here I assume days.

    Author response: *Yes, it should be 5 days. We added this information to the caption.*

18. I would rather keep Figure S1 in the main part and remove others (e.g. Figure 2 and Figure 7) to the supplementary.

    Author response: *Reviewer 2 had the similar comment (see R2C7). We completely agree move figure S1 to the main part and add necessary explanation on the discrepancy between modelled and observed. However, figure 2 is to test the model performance for simulating CO_2 emissions. Figure 7 is our major finding by using the model to see how long the terrestrial organic carbon is going to influence reservoir carbon cycle.*

(D) Tables:

19. Table 1. 'Thickness of mineral soil' is assumed to be 80cm. Please indicate/explain the basis of this assumption? Especially because it strongly influences the soil organic C in mineral soil and therewith the size of this carbon pool. What are the numbers in parenthesis for 'soil organic C (based on Paré et al 2011)?

Author response: *For terrestrial ecosystems, scientists typically assume 1m soil depth. Since we get the organic layer depth (20cm) in our studies area from the literature, we assume that the thickness of mineral soil layer is 80 cm. SOC content in mineral soil is not affected by the thickness.*

20. Table 2: On which data are the assumptions based?

    Author response: *We assume no trees will survive in submerged conditions. The fO2 is assumed based on DNDC model calculation for wetlands.*

21. (E) There are some language issues and typos in the manuscript (e.g. caption Figure 6 'celerity' should be 'clarity'; P2 L29 'in a seasonally ice-covered lakes'; P9 L5 'can generates').

    Author response: *Thanks. We will double check the typos.*