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Interactive comment on "Integrating aquatic and terrestrial biogeochemical model to predict effects of reservoir creation on CO_2 emissions" *by* Weifeng Wang et al.

Anonymous Referee #3

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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

Printer-friendly version

Discussion paper

(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 CO_2 fluxes data from an eddy covariance tower in the middle of the EM-1 reservoir').

(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.

(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.

(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.

(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.

- 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’.

Minor comments

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

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

(C) Figures:

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

- 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.

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

- 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.

- 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?

- 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.

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

- 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.

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

(D) Tables:

- 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)?

- Table 2: On which data are the assumptions based?

(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').

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