

Interactive comment on “Integrating aquatic and terrestrial biogeochemical model to predict effects of reservoir creation on CO₂ emissions” by Weifeng Wang et al.

Anonymous Referee #2

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This paper deals with the modeling of CO₂ emissions from the boreal hydroelectric Eastmain-1 reservoir. Eastmain is the most studied boreal reservoir so far in terms of greenhouse gas emissions and therefore the existing database allows the development of process-based models.

GENERAL COMMENTS: The manuscript is topical for the readership of Biogeosciences and such model and its potential development towards methane emissions is of first importance for aquatic biogeochemists working on carbon cycle. The manuscript focuses only on CO₂ emissions and could be significantly improved by exploring in details the main processes of the carbon cycle (see major comments), especially when field data exists to compare with the model.

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MAJOR COMMENTS:

-The model is a combination of Kim et al., 2016 (Ecological Modeling) and Wang et al., 2016 (Science of the total environment) models. It should be clearly said in the introduction of the model description and more importantly, a comparison with Kim et al. should be given in details since the CO₂ emissions are simulated in both papers over 2006-2009 and validated with the same dataset (TBL and EC tower). Is there any improvement with the addition of the water column model? Was the Forest-DNDC model modified compared to the version of Kim et al. 2016?

-The calculations of pCO₂ are simplified and it does not take into account carbonate equilibrium. What is the advantage compare to the classical calculations?

-The organization of the section 2 (material and methods) could be improved. It should start with the site description and being followed by a section with a summary of relevant work conducted on this site and used in the publication (in situ measurement and modeling). It is currently spread over the model description, site description, model tests and calibration as list of parameters or values. It would help the reader also to better identify the recent improvement of the model resulting from the addition of the water column. If done, the model description, tests, calibration and validation should be clearer and to the point. The section 2.1.2 is very short and implies to read Wang et al. (2016). A few key equations would be very useful for the reader. The sections 2.3 and 2.4 should be divided in shorter and more focused sections (initial conditions, inputs from watershed, meteorology...).

- In the section 2.1.3, the reader expects a content related to the degradation of allochthonous and/or autochthonous organic matter deposited at the bottom of the reservoir whereas it is mostly about the degradation of the flooded organic matter (soil, vegetation...). This section should probably be divided in two distinct sections to improve the readability. Some sensitivity analysis should be performed on the amount of the flooded organic matter and on the amount of allochthonous DOC/POC.

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-The model is a 1-DV model and no vertical profile of modeled variables is shown. Such typical figures are missing to evaluate if how processes are well reproduced by the model or if the model gives “only” a good average value for the “bulk” water column. It would be nice to see data from Teodoru et al (2011) (pelagic and benthic respiration, primary production, benthic respiration) and Demarty et al (2011) (vertical profiles) for instance being used for comparison with the model.

-I would recommended to put the monitoring of the pCO₂ in the generation station (supplemental) in the main document since it is the best way to have the average concentration over the whole water column. It also offers the possibility of computing downstream emission.

-A discussion about the pool of carbon fueling emissions would be very interesting: What are the relative contributions of the pelagic respiration, the autochthonous and allochthonous organic matter and the flooded organic matter to the CO₂ emissions? Those elements could reinforce the section 4.1 where all sources are listed but no information is given about the main source for the first years and after a few decades..

-the section 4.2 is basically about the sensitivity of the model to temperature change on CO₂ emissions. I would be very informative to provide illustrations of temperature change on both the physics (vertical stratification, duration of ice cover. . .) and on biogeochemical processes (respiration, PP in the water column, CO₂ production in the soils and overlying sediments. . .). Currently, this section does not provide any quantitative answer to the tested effect.

DETAILED COMMENTS: -P(age)1-L(ine)24: ““engineering” reservoir lifetime (100 years)” could be replace vy the widely-used life-time analysis

-P1-L27: oxygen effects?

-P2-L9-10: Many papers by JJ Cole, Carpenter and their teams or the synthesis by Duarte and Prairie (2005) would be more relevant for the prevalence of heterotrophy in

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aquatic ecosystems.

-P2-L11 " water-saturated sediments where the organic matters (e.g., plant biomass, litter, and soil organic matter)": Sediments are different from the flooded organic matter.

-P3L14: what are those "minimum inputs" compare to the listed "sophisticated" models? This should be discussed later on in the manuscript.

-P3-L23-26: "Based on limited empirical data, we test the hypothesis that the boreal reservoir will be a net source of CO₂ to the 25 atmosphere. We further hypothesize that the exchanges will be the largest in the first one to two decades and will then show little secular change thereafter—i.e. year-to-year variability around a fairly constant mean" The Eastmain database is not a limited database: 6 years of EC, several field campaigns with floating chamber, DOC, pCO₂, respiration, Chloa to cite a few. . . And the two hypotheses here are not hypothesis since those results are well know (Teodoru et al., 2012). The challenge was rather to check if a simple model is able to reproduce the emissions.

-P4-L21: Is the sentence a title for a section?

-Page 10 Line 20-23: There is no explanation about the tree removal. Was it really done before flooding? If yes, this should be in the site description. Is it a theoretical hypothesis for the evaluation of the role of tree trunk organic matter on emissions and the evaluation of mitigation options?

-P11-L18: what does dr stands for?

-P11-L26-27: This should be extended as noted is the general comments.

-P12-L25-26: "Both increasing and decreasing wind speeds enhanced annual CO₂ emissions 25 only by 1 and 1% over 100 years, respectively." Unclear sentence, should be rephrased.

-P12-L29: "grater" . . . greater

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-P13-L17: more information about the pelagic processes is needed since this is where the improvement over Kim et al. (2016) are.

-P13-L20: “Our simulations also show that sediment organic C keeps loosing over the simulation period” needs to be rewritten taking into account that this is very probably the pool of flooded organic matter that loose C instead of the sediment which might accumulate C even if at very low rate.

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