Reviewer #2

We appreciated for the comments and suggestions that significantly improve the quality of the manuscript. We have addressed referee 2's comments point by point and will make changes in the revised manuscript, which are detailed below.

1. 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. 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.

   Author response*: Yes, this manuscript only focuses on CO_2 emissions. We simulated reservoir surface CO_2 emissions for the former mature forest area where most observational data (including fluxes measured from EC tower and floating chamber) are available. This manuscript does not intend to estimate the whole carbon budget for the boreal reservoir. We do in the future plan to the model to include methane but the description of the core carbon is already more than enough material for a single manuscript.*

MAJOR COMMENTS:

2. 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?

   Author response*： FAQ-DNDC includes a water column carbon sub-model and thermal dynamic and water stratification sub-model. It replaces many prescribed parameters used in FF-DNDC. Compared to FF-DNDC, the new model, FAQ-DNDC, has a relatively complete lake carbon cycle including DOC, DIC, POC dynamics. There are many improvements over Kim e al 2016. For example, one of the problems identified in Kim et al (2016) was the sensitivity of the modelled emissions to sedimentation of new production. In Kim et al. (2016) the sedimentation of new production was not known, hence the different guesses at this value. In FAQ-DNDC there is no need to make this guess since new production in now estimated. Modifications for Forest-DNDC in FAQ-DNDC described in section 2.1.3 are different with FF-DNDC (Kim et al., 2016).*

   Reference:

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., 2016, 327, 1-17

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

   ***Author response*：** *We used DIC-pH to calculate pCO2. This approach could be more accurate compared to the pH-alkalinity approach, as pCO2 might be overestimated from pH and alkalinity in acidic freshwaters (Abril et al., 2015). In the case of EM-1 the underlying geology is igneous rock so source of carbonate is very low. This would be something that needs to be considered in sedimentary catchments with more buffered systems.*

   Reference:

Abril G, Bouillon S, Darchambeau F, Teodoru CR, Marwick TR, Tamooh F, et al. Technical Note: Large overestimation of pCO2 calculated from pH and alkalinity in acidic, organic-rich freshwaters. Biogeosci. 2015; 12: 67-78

4. 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…).

   ***Author response*：** *Agree. We restructured this section as proposed by the reviewer.*

5. 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.

   ***Author response**: We do recognize that it is an interesting question about the contribution of allochthonous and autochthonous organic matter to CO_2 emissions in aquatic ecosystems. However, the environmental problem that we are facing is huge amounts of organic carbon (including soils and vegetation biomass) were buried due to the creation of the reservoir. Therefore, this study focuses on when and how this buried carbon emits to the atmosphere. Secondly, we do not distinguish autochthonous organic matter (POC) deposited at the bottom of the reservoir and flooded soil organic matter or sediment organic carbon. Like* *littering in the terrestrial ecosystem, POC deposited to the sediment directly entering into sediment carbon dynamics.*

The sensitivity analysis on the amount of the flooded organic matter has been done. It is the removal of the fraction of tree biomass that largely affects the amount of flooded organic matter in the site. The amount of allochthonous DOC or POC definitely influence carbon processing in the reservoir. From the simulations that we did, it is a linear response. More DOC input, more CO_2 emissions. When creating a reservoir, it should not influence the water flow and DOC concentration of upstream. To simulate the variance of DOC concentration in the inflow, this requires a landscape hydrological model and a terrestrial ecosystem model with a detailed DOC module. Therefore, the amount of allochthonous carbon input is beyond the scope of our study.

6. -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.

   ***Author response**: Indeed, this is a 1-D model that typically simulate mean conditions like mean water depth and mean amount of flooded soil organic matter. Due to its 1-D limitation, it is not realistic to compare the modeled results (mean depth ranging 5 to 11 m) to the measurements (the vertical profile ranging up to 30m, as shown in Demarty et al., 2011). Because of the spatial heterogeneity of CO_2 fluxes in terms of pre-flooded landscape (Teodoru et al., 2011), we actually do not have enough profile data for the pre-forested site.*

   Primary production and benthic respiration were empirically estimated rather than measured in the previous studies (Teodoru et al., 2011). We prefer not to compare models to models.

   Reference:

   Demarty M, Bastien J, Tremblay A. Annual follow-up of gross diffusive carbon dioxide and methane emissions from a boreal reservoir and two nearby lakes in Quebec, Canada. Biogeosci. 2011; 8: 41-53.

   Teodoru CR, Prairie YT, del Giorgio PA. Spatial heterogeneity of surface CO2 fluxes in a newly created Eastmain-1 reservoir in northern Québec, Canada. Ecosystems 2011; 14: 28-46.

7. -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.

***Author response**: Did as suggested and necessary explanation on the discrepancy has been added in the text. Reviewer 3 has the similar comment (R3C18).*

In this study, we tested the model performance for the reservoir eddy covariance tower site where mature forests dominated prior to the flooding. Spatial heterogeneity in surface CO_2 fluxes that was linked to the pre-flood landscape types has been reported for the Eastmain-1 reservoir (Teodoru et al., 2011). Thus, the simulated mean water pCO_2 has a systematic offset compared to the measured pCO_2 in the generation station – i.e. they represent different water.

Reference:

Teodoru CR, Prairie YT, del Giorgio PA. Spatial heterogeneity of surface CO2 fluxes in a newly created Eastmain-1 reservoir in northern Québec, Canada. Ecosystems 2011; 14: 28-46.

8. -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.

   ***Author response**: It would be very interesting to investigating their contributions to CO_2 emissions. However, this study aims to investigating the flooding effects on post-flooded reservoir surface CO_2 emissions. The model calculates the carbon fluxes across the sediment–water interface, which contributes to CO_2 emissions by direct DIC fluxes and indirect DOC fluxes.*

   Unfortunately, the model does not separate the autochthonous and allochthonoud sources of CO_2 in the water column, as each modeled water layer has one pool for DIC and DOC, respectively.

   We also think that the Figure 5 could show the relative contribution of sediment carbon sources (including flooded terrestrial organic carbon and settled organic carbon from the water column) to total CO_2 emissions over time.

9. -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.

   ***Author response**: The sensitivity analyses has been done for two parameters (Rw and fo2) and two climate variables (air temperature and wind speed). We have listed the reason why we chose these four parameters in revised section 2.4. Please also see our response to R1 C3 and R3C4 (R: Reviewer, C: Comment).*

The effects of air temperature and wind speed on thermal dynamics (like vertical temperature, duration of ice cover) have been reported by Wang et al., 2016. Because of the scope (the effects of flooding on reservoir CO_2 emissions) of this study, the sensitivity analysis focuses on CO_2 emissions and fluxes across sediment–water interface (Figure 5). Since we do not have direct measurements in PP, respiration, CO_2 production in the sediment, we feel it's not suitable for such an analysis.

Reference:

Wang W, Roulet NT, Strachan IB, Tremblay A. Modeling surface energy fluxes and thermal dynamics of a seasonally ice-covered hydroelectric reservoir. Sci. Total Environ.550: 793-805, 2016.

Detailed comments

10. P1L24: "“engineering” reservoir lifetime (100 years)" could be replace vy the widely-used life-time analysis

    Author response: *Did as suggested.*

11. P1-L27: oxygen effects?

    Author response: *We rephrased the phrase. The term of "partitioning coefficient of decomposition production" was used in the revised manuscript.*

12. -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 aquatic ecosystems.

    Author response: *Did as suggested.*

13. -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.

    Author response: *We assumed that soils directly became "sediments" once the flooding events occurred.*

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

    Author response: *We now list the difference in inputs in the text.*

15. -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 thereafterâ˘A˘ Ti.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.

Author response: *While the EM-1 data is quite extensive for a boreal reservoir it is a fairly inadequate data set to evaluate a model or reservoir emissions when the lifetime of a reservoir is considered. Further the observational data does not account for the spatial and temporal scales involved EC data provides a high temporal resolution carbon flux data over several m^2 of a 600 km^2 reservoir, while field campaigns with floating chambers can only provide sporadic information on a very small footprint. So while the observations are probably the most for any boreal reservoir the uncertainties in observation to model comparison are large.*

The reason why we developed the process-based model is not only to check if a relatively simple model is able to reproduce the emissions but also to enhance our understanding on mechanisms. Although empirical studies (Teodoru et al., 2012) examined similar hypotheses, they lack the ability to examine process level explanations. What we use a process-based model to simulate the carbon cycle change in response to flooding and environmental inputs over 100 years. We got different results and made different conclusions than the empirical studies, hence e think the hypotheses are reasonable. FAQ-DNDC is a structured hypothesis of how we think, based on the literature, a reservoir's C cycles operates.

Reference:

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.

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

    Author response: *We re-wrote the sentence.*

17. -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?

    Author response: *We added the information about the tree removal in the site description. Some trees were removed in the first winter after the inundation by controlling icepack elevation through dam operations. We think that clear-cutting before flooding would help mitigating CO_2 emissions.*

18. -P11-L18: what does dr stands for?

    Author response: *dr is the revised Willmott index. We added the notation when it firstly appears. This was also addressed in response to reviewer 1.*

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

    Author response: *We did as suggested. See our response to major comment 8 above.*

20. -P12-L25-26: "Both increasing and decreasing wind speeds enhanced annual CO2emissions only by 1 and 1% over 100 years, respectively." Unclear sentence, should be rephrased.

    Author response*: We rephrased the sentence. Here we mean "Changing wind speeds by 20% enhanced annual* CO_2 *emission by up to 1% over the simulation period."*

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

    Author response: *Did as suggested.*

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

    Author response: *We mentioned the FF-DNDC study in our introduction and discussed the difference between two models. Please see our short comments by Weifeng Wang and our response to the comment 2.*

23. -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.

    Author response*: Yes, the continuous carbon loss across the simulation period is attributed to the flooded organic matter. We have revised the sentence to make our conclusion clearer.*