

Reviewer #1

We appreciated for the comments and suggestions that significantly improve the quality of the manuscript. We have addressed referee 1's comments point by point and will make changes in the revised manuscript, which are detailed below.

1. The paper presents a model, developed specifically for calculation of CO₂ emissions from hydroelectric reservoirs. To the best of my knowledge of the literature available so far, this is the first mechanistic model of CO₂ emissions applied to and validated at a concrete operating reservoir. The model demonstrated generally fair agreement to observations. This work is a substantial step towards process-based modelling assessment of GHG efflux from either existing or planned hydroelectric stations. The perspectives for the development of this work via including methane dynamics and more process-based approaches to simulate C transformations are clear and promising. I have no doubts that the manuscript is worthy to publish in Biogeosciences.

Author response: *We thank the referee 1 for his/her positive overall evaluation of our study.*

2. I have a number of specific comments, especially in the model description section. They are mostly related to justification of model formulations chosen, but also to processes, that are omitted. For instance, the vertical bubble transport of gases and the CO₂ flux from turbine flow, are not included, and there is no discussion what it might imply for the model performance. I would also like to see the details of vertical diffusion of DIC between hypolimnion and epilimnion, given there is usually a huge DIC gradient there (BTW, is it the case for Eastmain-1 reservoir?), so that the vertical CO₂ flux from bottom waters to surface is controlled by diffusivity coefficient. What is the value for this coefficient used?

Author response: *We appreciate reviewer 1's comments and suggestions on model formulation. Yes, we tested the model using data collected from the Eastmain-1 reservoir.*

The bubble emission pathway is important and will be considered in our next manuscript that focuses on methane and oxygen dynamics in the water column and sediment. The bubbles typically are composed of gases of CO₂, CH₄, and N₂. As the CO₂ production in the sediment may not be increased, the lack of bubble emission pathway will not have significant impacts on total CO₂ emissions.

Degassing from the turbine flow is beyond the scope of the current study, but discussion on the issue is provided in the revised manuscript. Our partners in this research, Hydro-Quebec, measure the gas concentration from the outflow of the turbines so there are good empirical observations. One could, in principle, develop an empirical relation between flow through the powerhouse and gas emitted, but the actual flows are difficult to obtain because they are proprietary information as it can influence the price of electricity.

In the model, vertical exchange of DIC between hypolimnion and epilimnion is controlled by water mixing (convection), as diffusion is very inefficient. For example, the epilimnion deepening during the summer bring DIC from hypolimnion to the epilimnion. We added

sentences to describe how the model mix the water and its solute (e.g., DOC). We published a paper this year that describes the convective mixing in this model (Wang et al. 2016).

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.

3. The paper lacks comprehensive explanation on the choice of parameters the model sensitivity was studied in respect to. What parameters entering model formulae for biogeochemical processes can be considered as firmly established, and what are loosely defined? Of course, this is a difficult task for such distinction to be made, if even possible so far, but anyway some discussion on this topic should be provided.

Author response: *The information about why only chose these four parameters has been added in the revised manuscript. The similar issue has been addressed by the other two reviewers (see R2C5, R2C9 and R3C4; R: reviewer, C: comment) in terms of different perspectives.*

Briefly speaking, we are interested to know how flooding terrestrial organic carbon influence post-flooded reservoir CO₂ emissions, which is the main purpose of this model and this study. The sensitivity analysis for the parameter of aboveground biomass removal shows the amount of flooded organic carbon significantly and positively influence CO₂ emissions. For the oxygen effect parameter (in the revised manuscript, we changed it to “partitioning coefficient of decomposition production”), we interested to understand if the lack of oxygen cycle in the model significantly affected the simulated emissions. The results indicate that incorporating oxygen cycle would improve the quality of the output and we are now in the process of adding this to the model as we add in methane. Further, we want to know how and by what mechanisms the environmental factors (air temperature and wind speed) influenced the carbon emissions using the process-based model, as these two climate variables have more significant influence on thermal dynamics and carbon emissions.

We have now categorized all physical and biological parameters (Table 1) as hardware parameters. Model sensitivity for most existing parameters in their original model have been investigated in previous studies (e.g., Zhang et al., 2002).

Reference:

Zhang, Y., Li, C., Trettin, C. C., Li, H., and Sun, G.: An integrated model of soil, hydrology, and vegetation for carbon dynamics in wetland ecosystems, Global Biogeochem. Cycles, 16, 1061, 10.1029/2001GB001838, 2002.

Minor comments:

4. P1L27: what does it mean "positively enhance"? Did you mean simply "enhance"?

Author response: We have deleted “positively”.

5. P1L28: seeming a contradiction: isn't CO₂ flux the same as CO₂ emission?

Author response: CO₂ flux across air-water interface is the same as CO₂ emission. We rephrased the sentence to avoid the confusion.

6. P1L30–31: do you mean, larger wind speed makes open water period shorter? Please, make it clearer

Author response: Yes. Higher wind speed leads to larger heat loss (higher latent and sensible heat fluxes to the atmosphere), resulting in a shorter open water period (Wang et al., 2016). We rephrased the sentence to make it clearer.

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.

7. P2L20: ...provide...with...

Author response: Did as suggested.

8. P2L27: air temperature? Wind speed.

Author response: Yes, air temperature. We specified it in the revision.

9. P2L27: Wind speed.

Author response: Did as suggested.

10. P3L22: assess

Author response: Did as suggested.

11. P3L31 climate data inputs

Author response: Did as suggested.

12. P4L2: I would say: verified, calibrated

Author response: Did as suggested.

13. P4L3: simulates

Author response: Did as suggested.

14. P4L5: temperature

Author response: *Did as suggested.*

15. P4L7: not all of them, as methane dynamics is not included

Author response: *Yes, you are right. None of models can simulate all processes. DNDC actually is able to produce methane in the sediment through simulating soil redox chemistry. We are in the process of attempting to develop a methane module for FAQ-DNDC. Please also see our response to comment 2, R2C1, and R3C7 about the methane module and its potential impacts on the current study.*

16. P4L8: I would say: ... are implemented as follows.

Author response: *Did as suggested.*

17. P4L13: ... are simulate:...

Author response: *Did as suggested.*

18. P4L18: Please, precise, how do you mix between epilimnion and hypolimnion using SIWAS.

Author response: *We added the description about water and its solute mixing in section 2.1.2 Thermal dynamic and water mixing. Please see our response to the comment 2.*

19. P4L21: The sentence seems incomplete

Author response: *We rephrased the sentence.*

20. P4L23: How do you specify DIC and DOC concentration in atmospheric precipitation? Are there observations data on this concentration?

Author response: *For parameterizing these two carbon input variables from the atmospheric precipitation, we conducted literature search and consulted experts (Dr. Tim Moore, McGill University) to select 0.6 and 2.0 mg/L for DIC and DOC, respectively.*

21. P4L24: However, snow is made up by atmospheric precipitation, which includes DOC and DIC. Have you estimated, how much of annual atmospheric DOC&DIC input is contained in solid precipitation?

Author response: *We do not estimate the amount of atmospheric DOC or DIC input stored in solid precipitation (snow). To our best knowledge, few studies focus on DOC/DIC concentration in solid precipitation. We think the atmospheric deposition inputs (either DOC or DIC) has insignificant effects on total carbon dynamics compared to inputs of carbon from inflow in our study reservoir.*

22. P4L31-32: I would replace all these numeric values by symbols, and provide a table with values standing behind these symbols. Thus, you underline that these parameters should be, strictly speaking, reservoir-dependent, and not constants like physical constants. This also applies to other biogeochemical parameters used in your paper below, so I do not return to this question there.

Author response: We did as suggested. The table would be like below:

Equations	b_0	b_1	b_2	b_3	b_4
1 GPP	0.80	-0.67	0.75	1.33	-0.77
1 PR	0.67	-0.94	0.77	1.28	-0.64
3	1.58	4.97	-	-	-
6	-0.453	0.71	-0.087	-	-
11	2.51	1.48	0.39	-	-
12	1911.1	-118.11	3.4527	0.4132	-
16	-17.0	0.06	-	-	-

23. P4L31-32: I don't understand how do you avoid using atmospheric radiation fluxes in calculating GPP. I understand that you try to minimize the input atmospheric data and cite the other work in eqs. (1) and (2), but would you please provide a comment, how is radiation regime is implicitly included here (as it should be).

Author response: We estimated GPP and PR using regression models other than biogeochemical equations. The FAQ-DNDC model itself calculates radiation and heat fluxes. We will investigate if biogeochemical photosynthesis models are better to be used for aquatic photosynthesis in the future but we have to keep the input requirements to a minimum since data is extremely scarce for the northern boreal regions we are interested in.

24. P5L1 : Please, specify, what are the definitions tou use for mixing depth and sunlight depth.

Author response: We revised the manuscript to have the detailed description of these two variables. Mixing depth indicates the depth of epilimnon, while the sunlight depth is the depth of the water that is exposed to certain intensity ($>0.03 \text{ W/m}^2$) for irradiation in one layer) of sunlight.

25. P5L4 : Not clear, is this ratio is assumed in the inflows of a reservoir, or it is a fixed ratio inside a reservoir?

Author response: This fixed ratio is only for exudation of GPP (like root exudation of terrestrial plants). We modified the sentence to make it clearer.

26. P5L13: In physical equations I recommend to avoid using multi-latter notations, like ESD. Consider replacing by single-letter symbol, like D (for diameter).

Author response: Did as suggested. We changed multi-letter notation to single-letter symbol for non-common variables.

27. P5L20: What is the value for Q10?

Author response: *Q10 = 1.5 listed in the table 2. We added its value in the text and removed it from the table.*

28. L5P23: I doubt if it is correct to call it Fick's law, as it there is not a gradient of concentration, but a difference of concentration across a phase boundary. Please, check.

Author response: *We rephrased our sentence to avoid this confusion.*

29. L5P24 There is one more emission pathway for CO₂ in reservoirs -- that is through turbines. If the turbines are located deep, they extract water from hypolimnion with much higher CO₂ concentration, compared to epilimnion. Do you take it in to account? Or you can provide estimates arguing it is insignificant for that particular reservoir you simulate in the paper?

Author response: *Degassing is an important pathway of greenhouse gases for hydroelectric reservoirs. Unfortunately, we do not have direct measurement. So, we do not take it into account in this study yet. The carbon budget estimation using the process-based model is interesting, but we need to consider the effects of flooding on different pre-flooded landscapes (forests, peatlands, and lakes) to giving a whole picture. See our response 2.*

30. P5L25: better to call it "piston velocity", as "usual" diffusion coefficient is measured in m**2/s

Author response: *Did as suggested. We revised our expression for equations 7-9.*

31. P5L25: what is less than z_{mix}?

Author response: *Considering the daily time-step, CO₂ diffusion coefficient in number should be less than mixing depth.*

32. P5L26: Solubility

Author response: *Did as suggested. We simplified the equations about air–water gas exchange in terms of Henry's law in the revised manuscript.*

33. P6L2: please make the brackets higher

Author response: *Did as suggested.*

34. P6L4: it is a reference piston velocity at Schmidt number = 600.

Author response: *We revised our related text.*

35. P6L9: please, find a single-latter notation

Author response: *Did as suggested.*

36. P6L14: As this is a physical formula, I would prefer if you provide a combination of physical constants providing this value.

Author response: *The number of 83333.3 is for unit conversion other than physical constants. We listed it as unit conversion coefficient in the revised manuscript.*

37. P6L23: what is a difference between transmission and diffusion?

Author response: *We revised it to “light transmission and heat transfer”.*

38. P7L5: I wonder, what is the minimal vertical diffusion coefficient you use in the model. During stratified periods, metalimnion (thermocline) is almost laminar in lakes and reservoirs, and vertical diffusion is almost molecular, and hence very inefficient. But, given a typically huge concentration gradient of CO₂ in metalimnion, the upward CO₂ flux should be very sensitive there to diffusivity.

Author response: *In the model, vertical diffusion of DIC/DOC between hypolimnion and epilimnion is controlled by water mixing, as diffusion is very inefficient. For example, the epilimnion deepening during the summer bring DIC from hypolimnion to the epilimnion. We added sentences to describe how the model mix the water and its solute (e.g., DOC). See our response to the comment 2.*

39. P7L6: What do you mean by stirring here? TKE equation includes only production by buoyancy and shear.

Author response: *Stirring is induced by wind, which is more important than shearing. The surface mixing algorithm follows the DYRESM model (Imberger and Patterson, 1981).*

40. P7L14: Exist

Author response: *Did as suggested.*

41. P7L24: is that correct that woods are immediately added to litter? What is the typical time for stems fall onto sediments? what do you mean by this: "... water depth is equal to mean water depth"?

Author response: *It is might not quite right to have all living biomass immediately added. There is not a lot known about plant mortality in flooded and partially flooded conditions. There is no question that wood would decompose slowly. The wood is added to litter but the slowly decomposing pool. In the model, we did not assume that terrestrial plants die immediately, while there is a time lag by assuming that trees die when reservoir water depth*

reaches the mean water depth. We simulated the water filling process (from November 2005 to June 2006). A more difficult issue to deal with is how the main woody parts of trees is dealt with physical when a reservoir is created. If the trees are frozen into the ice cover the volume of reservoir can be managed to mechanically remove the trees. The logs then float to the surface and can be salvaged over the next few years before they sink. However, in his form of FAQ-DNDC we do treat the wood as recalcitrant litter and if wood were removed it would have to be treated as an additional loss.

42. P8L3: I would not start sentences with abbreviations or notations

Author response: *We revised the sentence to avoid this.*

43. P8L4: I expect high CH₄ concentrations there leading to bubble formation, so that bubbles transport CH₄ and CO₂ directly to the atmosphere. Do you think this pathway for CO₂ from sediments is insignificant?

Author response: *Yes, bubbles may contain CH₄ and CO₂. In this study, we do not separate the emissions pathways as we are developing the methane dynamic sub-model (oxidation in the water column and emission pathways). We think the lack of this pathway for CO₂ may not significantly influence the total amount of CO₂ emissions through the water surface. The CO₂ production in the sediment does not change. Also see our response to the comment 2.*

44. P8L7: d² C/dz²

Author response: *Thank you for correcting.*

45. P8L9: "diffusion coefficient" can't be "a sum of processes". Please rephrase

Author response: *Did as suggested.*

46. P8L12: What is the nature of "turbulence" expected here in porous soil?

Author response: *To our knowledge, in porous sediment, the bottom reservoir water may influence the top porewater. In the thermal module, this has been neglected as the influence is slight compared to the whole water column. However, for the carbon cycle, the turbulent influence cannot be neglected. We used D_{tur} to simulate the effects of water flow on porewater carbon diffusion. .*

47. **P8L16: i-th**

Author response: *Did as suggested.*

48. P8L25: I would expect this coefficient representing not only effective diffusion on the soil side, but also at the water side

Author response: *This diffusion coefficient only works on the diffusion across water-sediment interface. On either soil or water side, they have different diffusion coefficient.*

49. P9L5: megawatt-hours units are used to quantify the total energy produced for a specified period, 1 day, 1 yr, etc. If this period is not specified, they use megawatt.

Author response: *We indicate the installed capacity. We re-wrote the sentence.*

50. P9L7: OK, does it mean that roughly 32% of annual atmospheric DIC and DOC are gained by reservoir in form of snow?

Author response: *Here just shows the climate in our study region so that there is no any implications for the carbon cycle. We assume that no DOC or DIC exists in snow in this study.*

51. P9L20: As the eddy covariance system was deployed at the island, are there estimates how much of measured EC CO₂ flux originated from the island?

Author response: *The reservoirs fluxes are only for when the towers 'sees' the water surface. The land sectors from the island were not included in our analysis when they were in the footprint. We have added a sentence to the methods to make this clear.*

52. P9L26: was the depth of water uptake by generation station taken into account, to compare CO₂ measured and simulated?

Author response: *Unfortunately, we do not have such information about the depth of intake hole in the dam. We assume that the outflow represents the mean water conditions, as FAQ-DNDC is a one-dimensional model.*

53. P9L31 : in river?

Author response: *yes, in river. This is calculated for river POC input.*

54. P10 L29 two?

Author response: *Actually, we used three methods: root mean square error (RMSE), refined Willmott index (dr), and Pearson correlation coefficient (r).*

55. P11L3: I would expect here a rationale, why only these two parameters from a large number of biogeochemical parameters used in the model, were selected for sensitivity runs.

Author response: *We have revised the paragraph to explain why these four parameters were selected. Please also see our response to the comment 3.*

56. P11L13: I wonder, if you could make up a budget of CO₂ in the reservoir, based on your model? I.e. calculate the contribution of all internal/external sources and sinks of CO₂ into CO₂ emission?

Author response: Definitely yes, the model can be used to estimate the carbon budget of the reservoir. However, the current study focuses on model development, calibration, and testing. Because of the spatial heterogeneity of CO₂ emissions, a completed carbon budget has to include the effects of flooding on different pre-flooded landscapes. This would make the model development paper lose its focus. However, a carbon budget is the subject of a subsequent paper.

57. P11L18 : What is d_r?

Author response: *d_r* is the revised Willmott index. We added the notation in the methods part in the revised manuscript. This is also been mentioned by reviewer 2.

58. P12L13: significant?

Author response: yes, it means significant. We revised “great” to “significant”.

59. I couldn't find in the model description section, was terrestrially-derived DOC , DIC and POC attributed to inflows only or groundwater discharge was explicitly taken into account as well?

Author response: Yes, terrestrially-derived DOC, DIC, and POC attributed to inflows, whereas groundwater discharge was not taken into account. The reservoirs we study in northern Quebec are located on the Canadian Shield. It is the topography created in this landscape along with the generally low permeability of the igneous rock of the Shield that makes the region suitable for reservoir creation. Groundwater could be an important component in other geographical regions but we have not included it in our study because it is not an issue.

60. P13L13 : declines

Author response: Did as suggested.

61. P13L25-26: You don't include the possible role of CO₂ ebullition

Author response: Bubbles will be considered in our next manuscript, but we think it will not significantly increase CO₂ emissions. See our response to comments 43 and 2.

62. P14L16 Occur

Author response: Did as suggested.

63. P14L31: what do you mean by water vertical movement here? Is it convection, that is directly affected by thermal dynamics?

Author response: Water vertical movement indicates spring/autumn turnover, summer stratification. We rephrased the sentence to avoid the confusion.

64. P15L16: I don't see a link between summer CO₂ flux and diurnal variation. The reader could look into cited literature, of course, but could you be more clear here, please?

Author response: *Here we were arguing the second reason (high GPP in summer due to high water temperature) for lower summer CO₂ fluxes. Higher GPP at daytime than at nighttime leads to lower CO₂ emissions. We rephrased the sentence to make our point clearer.*

65. P15L19: what does it mean: dissolved CO₂ ... can be filled up ...?

Author response: *Here we wanted to say that dissolved CO₂ (a component of DIC) in the epilimion increased with epilimion deepening (mixing the upper layer of hypolimion). We re-wrote the sentence*

66. P16L2: What aspects? Please precise

Author response: *Basically, we do not consider carbonate equilibria in the current model formulation. We rephrased the sentence.*

67. P16L17: Typo?

Author response: *should be “remaining”.*

68. Figure 1, Are there radiation fluxes?

Author response: *FAQ-DNDC does not require radiation fluxes as inputs, but calculates the radiation fluxes (short-wave and long-wave radiation, heat fluxes) by its inner algorithms based on latitude and an average cloud cover parameter. Since these inputs variables are seldom available as standard observations we continue to use DNDC’s computed radiation fluxes. This approach has its advantages for general use but is a disadvantage if actual observations were available.*

69. I suggest to rearrange this figure: 1) make two columns of plots 2) increase the vertical scales, as now both measured and simulated time series are at the bottom of plots and hardly discernable

Author response: *Did as suggested. We also changed the symbols to open triangles for the clarity. This is also addressed by reviewer 3.*