

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2020-488

Anonymous Referee #2

Referee comment on "Carbon balance of a Finnish bog: temporal variability and limiting factors based on 6 years of eddy-covariance data" by Pavel Alekseychik et al.,
Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-488-RC2>, 2021

I was excited to read this paper given the huge amounts of data and the relatively long measurement period (6 growing seasons! Both CO₂ and CH₄ measurements), as well as having really all the important environmental variables and fluxes measured concurrently. I think that the conclusions are more or less supported by the results in a logical way and given what I know about the site from reading other papers (Korrensalo, et al....). However, as a peatland expert who knows a lot about C fluxes and modeling but doesn't use EC techniques, the results section manuscript was incredibly challenging to read and to follow. Fortunately, the discussion section mostly redeemed it; the authors did a nice job of integrating the results of this study with earlier studies at this site and across northern peatlands.

The challenge was that I was not convinced on the appropriateness of the modeling and the subsequent analysis of the model parameters. This is partly a result of the framing; I thought that these parameters were simply used for gap-filling (e.g. Table 5) but instead these made up the bulk of the results. An analysis of the modelling parameters used in the flux calculations comprised the whole of Sections 3.2, 3.3, 3.4, 3.5. This was problematic because the explanation of the modelling was insufficient and unclear and the justification for the approach, both theoretical and practically, was quite weak. First, the parameters are not even named (defined) or explained (Section 2.4). Then, it is not clear how these parameter values were determined. Or rather, it was clear until I read the results section and looked at Figure 7 (which shows something different than Table 4), 10, and 11 which shows that these parameters are dynamic over time, then I was completely lost. What, how and why the modeling was done in this way must be clear. This analysis comprises the bulk of the results section so the explanation needs to be clear, include the relationship with time, and can take some space.

Furthermore, I was not convinced that the model structure is completely appropriate until I read the explanation in the discussion and got a refresher about other results from Siikanen; there is no justification for the use of these models for this site other than an earlier study used similar methods. It isn't apparent that the authors tested alternative model structures for either Re or CH₄ that might include other known controls on CH₄ flux (like water table). From the discussion, a bit more insight emerges as to why the authors chose these particular models for the C flux parameters but this needs to be justified in

the model description section with references to the earlier studies from this site. Finally, Table 1 shows that many years had only a small amount of data meeting the QC criteria. I'm a bit concerned about the circularity of using modelled fluxes (including the gap-filled data) that have been modelled given prescribed controls (with insufficient justification for the use of the models) to look at the controls of the fluxes, given that these fluxes were modelled using temperature. A more rigorous analysis is justified.

Specific comments:

Introduction: I really disliked that the background for this manuscript relied only on information and background from other EC measurements in peatlands, particularly from bogs. Linking fluxes to sub-surfaces controls was originally done using chamber measurements; these have really laid the foundation for understanding environmental controls on fluxes using EC, including at this site, and some did this more than 10 years ago.

75: Why are all these referred to as "potential" drivers? These are known drivers, at least at other sites.

162: Wasn't 2011 hot and dry? Is this appropriate?

254: This is really only the result section given that there is a later discussion section

295: Dome?

Section 3.3: could use figure references. How and where was this non-growing season flux determine?

Figure 7: this is confusing (see main points above) given also Table 4.

350-353: Confusing

Table 5: Why not add indicate the error here? Especially because of disclaimer on line 359?

385: 30-40% of what?

4.1: where is this shown?

433: interannual difference controlled by temperature, but how is this related to and dependent on the model used here?

441: Not shown in Fig. 13

481: other studies have shown differently (e.g. King et al., 1997).

507-8: Unclear what this paragraph is referring to? Maybe include some references to figures and tables.

555: include a figure reference here.

560: Could this be related to the gap-filling or modeling methods?

Data availability: Given that this is 2021 and there are many opportunities for data

publishing and indeed this is generally required, contacting the author for data is really not an acceptable route for data availability.

Additional references to consider:

Chadburn, S. E., Aalto, T., Aurela, M., Baldocchi, D., Biasi, C., Boike, J., ... & Westermann, S. (2020). Modeled microbial dynamics explain the apparent temperature sensitivity of wetland methane emissions. *Global Biogeochemical Cycles*, 34(11), e2020GB006678.

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King, J. Y., & Reeburgh, W. S. (2002). A pulse-labeling experiment to determine the contribution of recent plant photosynthates to net methane emission in arctic wet sedge tundra. *Soil Biology and Biochemistry*, 34(2), 173-180.

Rößger, N., Wille, C., Veh, G., Boike, J., & Kutzbach, L. (2019). Scaling and balancing methane fluxes in a heterogeneous tundra ecosystem of the Lena River Delta. *Agricultural and Forest Meteorology*, 266, 243-255.

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