

Biogeosciences Discuss., community comment CC1
<https://doi.org/10.5194/bg-2020-488-CC1>, 2021
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Comment on bg-2020-488: Pestiaux, L., Schoenmakers, E., Thomson, L., Macfarlane, A., Griffin, S., Steel, J.

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Community 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-CC1>, 2021

Pestiaux, L., Schoenmakers, E., Thomson, L., Macfarlane, A., Griffin, S., Steel, J.

Overall summary of the paper:

The study aimed to quantify the carbon dioxide (CO₂) and methane (CH₄) fluxes on boreal mires in southern Finland. It also aimed to identify the environmental factors controlling these ecosystem-atmosphere exchanges and which might be responsible for seasonal and inter-annual variability of carbon fluxes. Lastly, the study investigated if the CO₂ and CH₄ fluxes could help detect the heterogeneity of the surface. The study is innovative as it uses long-term data (six years of data from May to September, representing the growing seasons) measured by eddy covariance (EC) techniques.

The results of the CO₂ and CH₄ fluxes in the study site were similar to other boreal bogs. The variation in fluxes exchanges were driven by air and peat temperatures and the water table depth was a factor driving the atmosphere-ecosystem exchanges in dry years. Lastly, there was no relationship between CO₂ and CH₄ fluxes and the surface heterogeneity of the site. This was due, in part, to the uncertainty of the models used. This study will hopefully introduce further research of peat fluxes exchanges using EC techniques and will allow a better estimation and interpretations of the estimates.

General comments:

The different conclusions and results drawn from the study are valuable to our

understanding of peat bogs dynamics. However, given the length of the paper and the amount of detail contained therein, it becomes difficult for the reader to identify the most valuable information and differentiate between this and the other findings included. We suggest that the authors could clarify the main findings they want to share with the readers and make these very apparent (e.g. a clear introductory sentence at the beginning of the section and paragraphs).

Dates and periods of data collection

The paper would benefit from clarification of the exact periods from which data were collected, since the terms 'annual' and 'growing season' are used interchangeably in the paper. This can be confusing, since with the former, we would expect to see 12 months of data, and the latter, only a subset of the year. This information could be specified in:

- lines 24 (what do the authors mean by "the study represents a complete series"?),
- line 29 (did the authors consider data collected in winter? What do the authors mean when they say that the contribution of October-December CO₂ and CH₄ fluxes was 'not negligible?'),
- line 62 where 'annual' is used interchangeably with 'growing season'.
- Line 304, "The importance of the non-growing season fluxes was also analyzed" meaning that annual data was indeed collected; again, reducing clarity on exactly when the work was undertaken.

Comments on the Method section

Line 110: We enjoyed the details to which the authors described the study sites. These detailed information enable the reader to understand better the environment in which the study was conducted.

L120: Figure 1.b could be expanded to match the size of the photo and a more detailed map of the Siikaneva-2 site with the location of the EC tower could be added. We understood that some data were gap filled with a closely situated site, Siikaneva-1. It would be valuable for the reader to have an idea of the location of Siikaneva-1 and be able to see the similarity in environmental conditions between these two sites. Are these sites similar enough to use the data interchangeably? A close-up of the map showing the

replicates of the study (line 157-158) as well as the different land cover would improve the method section.

Unless the information presented in line 255, in section 3.1 (results) are information from data collected by the authors, we suggest the section (Environmental conditions) should be moved to the method section as these are background information.

Some information found in the Discussion and Results sections should be explicitly set out in the method and should not be stated at the end of the paper. Line 304 ("*the importance of the non-growing season fluxes was also analyzed*"), should be stated in the Method section.

Line 254: Section 3 is called "Results and discussion". This is confusing as there is another "Discussion" section later (on Line 406). It would be clearer for the readers to have well-delimited and defined section enabling them to locate themselves in the paper.

Comments on the figures

The authors present many figures which make it hard for the readers to understand what the most important results and main messages are. On a general note, it is easier for the reader to have the whole figure on one page and avoid the graph being cut (for example, Line 375).

Line 140. Figure 2: The surface energy balance closure (SEBC) should be defined in the figure description or in-text. Whilst the formula is written (which is great), the variables are not defined.

Line 145. Table 1: We do not understand why the authors

separate the periods May-September and June-August. More details on why the authors want the readers to notice the differences would be valuable (added in the caption or in the text where the figure is referenced).

Line 165. Figure 3: The FPR abbreviation could be spelled out clearly in the caption.

Line 285. Table 4: As said earlier, the notations in Table 4 such as (0.68...0.78) could be clarified (at least the first time it is used in the abstract).

Line 314. Figure 6: We enjoyed the format of Figure 6 and the fact that the authors highlighted some part with the part shaded in grey. The figure could be formatted slightly bigger to allow for more precision in scales, particularly the x axis. When the figure is too small, it is difficult to determine the variability per month.

Line 329. Figure 8: The x axis is represented by the number of the days in the years. We think these values are not good indicators of annual peaks. We suggest months and dates as values in the x axis; this will make it easier for the reader to interpret the figure.

Comments line by line

Line 0: We suggest the title could be more explicit. The authors could add emphasis on the difference this study has compared to others regarding the technique used such as the EC technique (I.e., add 'using eddy covariance technique'). We also suggest the authors could add information about the investigation of methane balance in the title.

Line 28: The authors introduce "(6.4...8.5)" to represent a range of data. This is done on multiple occasion (Table 4. in Line 285). To improve the readability of the paper, a clear explanation of what this annotation means as well as stating what the average is (I.e., 7.1) could be added at the beginning of the paper.

Line 59-60: The authors specify "certain" challenges in identifying typical bogs. These challenges could be stated clearly, and more information could be added on the reasons the authors chose to study bogs in Siikaneva-2 site.

Line 65: The author specifies that the widespread in these numbers is 'attributed to' site specific and external factors. What are the implications of such assumptions? It would be informative for the reader to have references for the sentence in Line 25.

L75: The author stated that the water table level is an important driver for methane being held in the oxic zone before it reaches that atmosphere. Explanation on why this mechanism is important is needed.

L88: We noticed that the word "ebullition" was written twice in that specific sentence.

L133: Why are the CH₄ fluxes at Relative Signal Strength (RSSI) < 20 excluded from analysis? What are the implications of this exclusion? Explanation of why this part was excluded would benefit those less familiar with RSSI.

Line 135: The sentence starting with "Interestingly...". How important is this to methods? This sentence seems not to have its place in the method, and we wonder if it should not be included in the discussion section instead?

Line 148: Why were these specific depths chosen for the measurement of the peat temperature? More references and/or explanations could be provided.

Line 152-154: It seems that a large part of the data was taken from other sites (also Lines 162-163). We wondered to what extent the gap-filling is consistent with the other data.

Line 163-164: There appears to be a lot of uncertainty for the measure of LAI. It might be useful for the author to provide further information on gap-filling or discussions of these measurements.

Line 158: Further clarifications (and potentially a visual representation) of the replicated in the study should be added. We are not sure about the working out of the replicated and the total number (how can three replicates lead to 18 in total?). Do you have only one site (Siikaneva-1)?

Line 159: Explanation as to why LAI was measured twice a month throughout the growing season, unclear on why this number was chosen.

Line 180: The sentence does not read well, and we wonder if there is not a verb missing.

Line 189: The author states that the footprint lengths need 'careful calculation'. This description of the mindful calculation seems unnecessary.

Line 205: What is meant by 'high instantaneous' z0 values? Definition needed.

Line 211: We suggest that the definition or explanation of footprint nodes could be added.

Line 246: The author mentions a 'clearly superior performance'. It would be useful to offer a quantification, by how much?

Line 250: The authors could clearly define what they mean by 'short gaps' and 'long gaps'.

Line 366: The author mentions that "the data of 2011 is shown separately". It would be useful to provide more information in the methods section, and potentially in the results section, as to why that is the case.

Line 387: Again, "Given these considerations, the seasonal cumulative values presented in Table 5 should be taken with caution as they contain a large proportion of gap-filled data." Please explain further why gap-filled data is not an issue.

Line 410: the use of friction velocity seems to be unreliable in this case, so an explanation of why it was used in this study would be welcome

Line 412: In the sentence, "implies the presence of some other factors degrading the performance of EC technique", we wondered what other factors the authors meant. We suggest that the authors write clearly if the factors are unknown as this would make it clearer for the reader and future researchers.

Line 465: We notice that the word "limits" was in the sentence and seemed out of place. Is that a typing mistake?

Line 506: The rhetorical question "what might cause such a peak in Ch4...?" may not be necessary as it could add confusion to the reader.