Response to the reviews of the preprint bg-2020-488, Alekseychik et al. 2021 "Carbon balance of a Finnish bog: temporal variability and limiting factors"

The responses are in blue.

Community comment 1 (Pestiaux, L., Schoenmakers, E., Thomson, L., Macfarlane, A., Griffin, S., Steel, J.)

Dear Team of Reviewers, thank you very much for providing an assessment of our work.

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).
   - Thank you for noting this, I will write a paragraph with a clearer summary of the findings, and add introductory sentences.

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.
- This is a valuable comment. "Inter-annual variation" in this context means all data from a single year, covering the period of interest (June-Aug or May-Sep). Growing season is conventionally defined as May-Sep, as the time when most aboveground biomass (or LAI) develops.
- "Complete series" means a 6-year record, of course accounting for all the gaps. Will be rephrased.
- Non-negligible non-growing season fluxes: see some assessment based on fragmentary winter measurements on Lines 502-505.

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.
- Thank you for these suggestions. Figure 1b will be reworked and a map showing the relative locations of the Siikaneva fen and bog sites will be added. They really are very close to each other, being separated by a little over 1 km. The vegetation and peat properties are rather similar, too, of course accounting for the inevitable fen/bog differences in species composition and hydrology.

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.
- We will consider moving the Environmental conditions into the Site description section.

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. – will be done
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.
- sorry for this inconsistency, will be corrected.

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). – Sure, will be taken care of (on the production stage if not earlier).
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. - done

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). – May-Sep is the canonical "growing season" in boreal
environment studies, but we also add June-Aug as most years have good data coverage in this interval.
Some more information will be added.

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

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. - done

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. – This is understandable, done.

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. – Please note that this plot shows the timing of the peak, but the figure and/or
its discussion must be unclear, which cause this misunderstanding. To be edited.

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.
-    using "EC" in the title sounds good, we will consider that.

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. – this notation complies with the format of
Biogeosciences

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. – will be made more specific

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. – The references in Lines
65-71 summarize the relevant factors, and it would be difficult to improve this part due to the scarcity
of the previous literature.

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. – Please note that this particular referral to the potential role of
WTD is entirely based on the literature cited in this sentence. It proved challenging to identify the
specific reaction of the CH_4 flux to WTD in this work, however.

L88: We noticed that the word "ebullition" was written twice in that specific sentence. – thanks a lot
for noticing that, corrected.

L133: Why are the CH_4 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. – This is a standard quality check for open-
path analyzers. The threshold of 20 was determined based on scatter plot of CH_4 flux vs. 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? – to be moved to Discussion

Line 148: Why were these specific depths chosen for the measurement of the peat temperature? More references and/or explanations could be provided.- These depths are quite standard. -5 cm is the shallowest depth where the moss canopy can be considered more or less closed, i.e. the measurement of its T becomes possible. -50 cm approaches the greatest depth where the annual T variation can still be detected. The 20 and 35 cm depths are simply inserted between the former two.

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. – the two sites from which the data were used are located nearby (Siikaneva-1 fen: 1.2 km, SMEAR-II: 7 km). Siikaneva fen has a very similar WTD dynamics and nearly identical meteorological record (not shown). SMEAR-II, too, has a meteorological record representative of the conditions at Siikaneva-2 bog.

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. – it would be very difficult to estimate the uncertainty on LAI. For some information on this, we invite you to consult with the cited papers by Korrensalo et al.

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)? – Please see Korrensalo et al. (2017) for details on this.

Line 159: Explanation as to why LAI was measured twice a month throughout the growing season, unclear on why this number was chosen. – Please see Korrensalo et al. (2017) for details on this.

Line 180: The sentence does not read well, and we wonder if there is not a verb missing. – I do not think that a verb is missing, but I will try to rephrase it.

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

Line 205: What is meant by 'high instantaneous' z0 values? Definition needed. – will be specified. Basically, this refers to 0.1 m >z0>3 m.

Line 211: We suggest that the definition or explanation of footprint nodes could be added. – This is a rather standard EC concept so I suggest not to include this.

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

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

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. – To highlight the data of the drought/heatwave year 2011 and avoid their overlap with the data within the -20…-15 cm WTD bin.

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. – Years with a large proportion of model data are always uncertain, as model performance, especially during long gaps, is a matter of great uncertainty.

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 – Why, the u* threshold is well defined, in both CO2 and CH4 EC fluxes, and its application in this study is perfectly justified, as in nearly all other EC studies.

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. - Those are listed in the latter part of the same sentence.

Line 465: We notice that the word "limits" was in the sentence and seemed out of place. Is that a typing mistake? – Indeed it is, thanks for noticing that!

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. – will be rephrased.