Author Comments to Referee 2
Juutinen et al., Variation in CO_2 and CH_4 Fluxes Among Land Cover Types in Heterogeneous Arctic Tundra in Northeastern Siberia

We thank both reviewers for their time, thorough reviews, and valuable comments. We have edited the manuscript according to the comments and suggestions. Following the suggestion by Reviewer 1, we checked the CH_4 consumption data from the barren tundra. We found an error in the case of the most negative CH_4 flux and recalculated the flux in two other cases. The effect on the mean seasonal value was small and did not change the landscape-level CH_4 estimate much. The substantial CH_4 consumption rate in the barren tundra is real and was detected also by the EC-measurements (this study and Tuovinen et al. 2019). We revised the text following the suggestions and emphasized the role of barren tundra as CH_4 sink in this landscape. In Tiksi, the largest consumption of atmospheric CH_4 occurs in barren that is composed of sand and small rocks. Even though the CH_4 consumption rates were large, the pattern and values agree with those measured in a few other circumpolar polar deserts and barrens, which we show in a review table (Table 4). To emphasize the CH_4 sink function found in this landscape, we appended the introduction, material and methods, results and discussion at suitable places. Reviewer 2 pointed out that showing results of temporal CH_4 flux dynamics did not serve to answer the questions set in the study. We have streamlined the text accordingly and edited Figures 5 and 6. Both reviewers criticized our sloppy utilization of the DCA analysis results, which we acknowledge. We think that the DCA summarizes many features of the landscape and we are opting to include it. Nevertheless, we have put effort to linking the DCA graph and the text. Please find below our point-by-point answers to each comment (**AR:** blue type).

Sincerely,

Sari Juutinen on behalf of all authors

****

Reviewer 2

Summary:

For their study, the authors performed closed chamber measurements of CO2 (in 2014) and CH4 (between 2012 and 2019) fluxes in different land cover types (LCTs) in Northeastern Siberia during the growing season along with supporting meteorological measurements. Upscaling of the chamber data and comparison with eddy covariance (EC) measurements revealed the importance to distinguish between different land cover types when estimating tundra C exchange on a larger spatial scale: Mainly driven by differences in vegetation coverage and soil wetness, tundra wetlands contributed disproportionately much to the total CO2 uptake and CH4 emission regarding their spatial extent. Drier tundra landcover types instead offset the CH4 emissions through significant consumption of CH4.

Major comments:

The questions addressed in the study are well within the scope of BG. The study does not really comprise any new ideas or concepts, however publishing greenhouse gas flux data and additional measurements from the still data-scarce Arctic region is valuable in itself. From my point of view (and as the authors state themselves) the small number of replicates per LCT does not allow for a precise quantitative evaluation of greenhouse gas emission depending on the LCT. I expect that assuming that a single plot per LCT (as for example in 2014 for bog and dwarf-shrub tundra, see Table 2) is representative for the whole LCT, might introduce high uncertainty into the upscaled data product. For example different microtopography types within a bog (small hummocks, hollows,…) might already show very different exchange rates of greenhouse gases. The study clearly focusses on the spatial aspect, however, many more temporal replicates were performed. The design of the measurements therefore does not match the aim of the analyses very well. Regarding this issue it is nearly surprising to me, how well the upscaled chamber measurements match the EC measurements (at least from a qualitative point of view) (Figure 7). The main conclusion that different land cover types should be distinguished for upscaling is not new but the proof of its importance, given in the paper, is still useful also regarding possible future changes in the distribution of different LCTs due to climate change.

A new aspect is added to the study by the multivariate analysis that investigates the relationship between gas fluxes and environmental variables. However, this analysis seems a bit redundant to me in this context because it does not add any information to the results or conclusions presented in the paper. Furthermore, the DCA ordination diagram (Figure 3) is only described in a rather technical manner. In my opinion the multivariate analysis should either be removed from the paper or it should be described, analyzed and interpreted in more detail.

In general more information is included in the manuscript than is needed to answer the research questions (e.g. also the temporal differences between CH4 fluxes within the growing season). This sometimes makes the manuscript hard to follow. In my opinion it would be better to focus on the data that is relevant for the study aim.

Throughout the manuscript words are sometimes written out although an abbreviation had been introduced earlier. Adding an overview table that contains all the abbreviations would be helpful also because there are quite some abbreviations used in the manuscript.

AR: We thank for the comments and suggestions. We streamlined the text to focus on the spatial aspects. We admit the low number of spatial samples that is a result of compromising for determining the light response of net exchange of CO_2, which requires temporal replication. Also, the measurement campaigns had time constraints. We specified in the text that ‘bog’ means here the drier tundra peatland that is distinguished from the fens by more abundant dwarf shrubs and lower WT position. In this case, there is no pool and hummock variation. This does not remove the fact that replication per LCT is low. We decided to show the temporal aspect of CH4 fluxes only to reveal the data behind the mean values. However, we redrew Figures 5 and 6 to focus on the spatial aspect and edited the text consequently. In addition, we have elaborated the interpretation of the DCA-analysis in results and discussions. The usage of abbreviations was checked throughout the text, but no table was included, because their number is moderate.

Minor comments:

l. 78: The word “act” is missing and “-s”

AR: Revised

l. 86: I don’t understand the meaning of the word “enhances” in this context

AR: “Enhances” replaced by “improves”

ll. 86, 87: if only the eddy covariance method is meant with “micrometeorological measurements”, I would mention this explicitly.

AR: Changed as suggested

l. 96: In ll. 87, 88 it is mentioned that flux estimates using the eddy covariance technique might be biased in a highly heterogeneous environment like the study area. Is it then reasonable to compare the chamber measurements to the eddy covariance measurements to assess the spatial representativeness of the chamber method? It is certainly helpful to compare chamber and EC measurements but the way the reasoning is expressed here it seems a bit contradictory. Maybe you could just rephrase your reason for comparing the chamber fluxes with eddy covariance measurements.

AR: The possible EC bias is taken into account in the EC vs chamber comparison by weighting the LCT-specific chamber-based fluxes by LCT proportions (Fig. 7). These proportions represent the relative contribution of each LCT as estimated from the EC footprint climatology and LCT map, and thus this weighting improves the comparability of the chamber- and EC-based fluxes. We rephrased the text.

l. 106: At several point in the manuscript, when referring to a figure, I would add the relevant part of the figure to the reference. For example in this line I would refer explicitly to Figure 1a instead of just Figure 1.

AR: We have edited the figure references throughout the text.

l. 117: I cannot see this from Figure 1 and would therefore only refer to Table 1.

AR: Done

l. 123: I would also refer to Figure 1 d-h here.

AR: Done

l. 157: “…over 5 °C…” – is that the definition of the growing season?

AR: Not necessary in the Arctic, but that value was used just to indicate conditions.

ll. 176 – 179: Since the analyses are based on little replicates it would be interesting, how many measurements had to be discarded. Maybe this information could be added to Table 2, if the numbers do not already give only the valid flux measurements.

AR: Only valid data included

ll. 229 – 238: How exactly was the “light response of Pg and NEE” determined? How exactly did you determine the value of Pgmax and Pg800?

AR: Text has been edited.

l. 238: What do you mean with “collar means”? Are these temporal means over all the measurements performed at one collar?

AR: Mean of observations per each collar, temporal means. Text has been edited.

l. 254: A bracket is missing after “…360°”

AR: Corrected

l. 275: I would refer only to Figure 2b here.

AR: Corrected

l. 276: "2011-2019"

AR: Corrected

l. 282: The reference should be to Figure 2 c-d.

AR: Corrected

l. 291: a “T” for temperature is missing after “…soil surface…”

AR: Corrected

l. 297: The sentence structure does not make sense.

AR: Corrected

ll. 301, 302: Why is the strong correlation of ER with axis 2 not mentioned?

AR: Not very strong, but it is included in the text now

l. 313: What is the meaning of these Eigenvalues?

AR: The eigenvalue is a measure of the strength of an axis, the amount of variation along an axis, and ideally the importance of an ecological gradient. We report in the text the variances explained by each axis in the vegetation data. Eigenvalues can be removed.

ll. 313, 314: I would rather add the information that “…axis 1 and 2 explain cumulatively 63% of the variation…” to the main text than keeping it in the figure caption.

AR: Edited as suggested and values are removed from the caption and the axis statistics are in the text.

l. 335: According to Figure 4 there is no significant linear relationship between CH4 fluxes and WT...

AR: Not linear but CH_4 flux was related to WT. Wording changed and correlation replaced by related.

l. 345: Is the standard error the same as standard deviation? In Figure 6 standard deviation is used and in Table 3, standard error.

AR: No, it is different. Both were edited and it is standard error in the table and figures.

l. 364: the "4" in "CH4" should be made into a subscript

AR: Corrected

ll. 370, 371: I would say "…comprised…of…" or "…contributed…to…"

AR: Edited

l. 377: I would explicitly refer to Figure 7 b-d.

AR: Edited

ll. 379, 380: Which wind sector do the percentages refer to?

AR: Those are averages. Text edited accordingly.

l. 382: I would refer to Figure 7f.

AR: Edited

l. 392: "…exchange of CO2, photosynthesis, and CH4 flux,…"

AR: Edited, now stands "…NEE, Pg, and CH_4 emissions…"

l. 401: "…wind direction sectors (a)),…". Which years are included for Figure 7 f)? Only 2014 or all years of CH4 flux measurements?

AR: The legend edited, and it is also mentioned that only year 2014 data were used.

l. 409: What are the "collar-specific estimates"?

AR: Estimates were calculated for each collar. Text edited.

l. 418: Does the "bog" not count as a wetland type?

AR: Right, reworded in the text. Graminoid types had large CO2 uptake capacity while the wet graminoid types, i.e., fens, emitted CH4.

l. 422: "%" is missing. Is it 9 or 10%? At other points of the manuscript you write that it is 10%.

AR: The value is recalculated and corrected; 8%.

l. 435: "not" instead of "neither"

AR: Corrected

l. 473: Better to also refer to Figure 6.

AR: Edited

l. 475: I cannot see this from Figure 3.

AR: Text edited but still citing the Fig. 3.

l. 476: How was the soil organic matter content inferred? The data is not shown anywhere.

AR: Right, the soil OM data was published in Mikola et al. (2018), now included in the text.

l. 497: Why do you expect "an overestimation of the emissions from the wet fens"?

AR: Reworded and the fact that EC saw even higher consumption for the northern sector is included as a more relevant fact here.

Comments to Figures and Tables:

Figure 1b):

I would be nice to either give a closer view of the map so that it can be seen in which LCTs the chamber measurements were performed or (which would be even nicer) mark the EC footprint (impact area) on the map. Is the "stony" LCT the same that is referred to as "barren" in the text? It would be helpful if the same wording was used for the LCTs throughout the paper.

AR: The figure legend is edited as suggested, thanks for noting it. We considered the close-view map, but the land-cover classification is too coarse in a sense that the chamber locations would not appear properly in a close-view image. That is because the used full-lambda schedule segmentation is region-based and the pixels are merged with the help of spectral (mean pixel value in the segment), textural (SD of pixel values in the segment), shape (areal complexity of the segment) and size information, which we weighted by 0.7, 0.7, 0.3 and 0.3, respectively. The average size of the segment (i.e. pixel: segment ratio) was set to 50 (i.e. 200 m2). The EC footprint (cumulative 90%) is about the area shown in the map and we added a reference to Tuovinen et al. (2019) where the climatology is presented.

Figure 2:

Maybe the use of different symbols for the years would be easier to distinguish for color-blinds. In figure 2f the different lines are hard to tell apart, especially where they are overlapping. Which line is for dry fen, which one for meadow?

AR: Grey-scale colors and different line types were applied. Meadow and dry fen had similar thaw depths; however, the lines were edited for clarity.

Figure 5: Differences between the different months are shown in the figure but not discussed in the text and they do not contribute to the study results. The temporal aspect is interesting but maybe beyond the scope of the study. Figure 6 would be sufficient to answer the research question. Furthermore, the data from different months do not really show an annual course of the CH4 exchange since the data was collected in different years with different meteorological conditions.

AR: That's true. We removed the monthly data.

Figure 6c): It would be helpful if the markers had different colors for the different LCTs.

AR: The figure was edited; temporal data are removed and the spatial data are appended. LCTs are marked with symbols.

Figure 7a): Why is there a vertical line around 50% for the northern wind sector?

AR. Thanks for noticing. There's something odd compared to the original figure, likely produced by the pdf conversion.