

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-5

Anonymous Referee #2

Referee comment on "Variation in CO₂ and CH₄ fluxes among land cover types in heterogeneous Arctic tundra in northeastern Siberia" by Sari Juutinen et al.,
Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-5-RC2>, 2022

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.

Minor comments:

I. 78: The word "act" is missing and "-s"

I. 86: I don't understand the meaning of the word "enhances" in this context

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

I. 96: In II. 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.

I. 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.

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

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

I. 157: "...over 5 °C..." – is that the definition of the growing season?

II. 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.

II. 229 – 238: How exactly was the "light response of Pg and NEE" determined? How exactly did you determine the value of Pg_{max} and Pg_{800} ?

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

I. 254: A bracket is missing after "...360°"

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

I. 276: "2011-2019"

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

I. 291: a "T" for temperature is missing after "...soil surface..."

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

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

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

II. 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.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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.

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?

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.

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

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