Author Comments to Referee 1

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

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Reviewer 1

General and specific comments

The manuscript "Variation in CO2 and CH4 fluxes among landcover types in heterogeneous Arctic tundra in Northeastern Siberia" by Juutinen et al. presents several years of CO2 and CH4 flux data, measured both with manual chambers as well as the eddy covariance technique. The authors combine their flux measurements with detailed investigations of site vegetation characteristics and site meteorological data, measured at an Arctic tundra site in Siberia.

This is an important study because it highlights the difficulties in determining C emissions from these heterogenous ecosystems. The study is set in an understudied region in terms of C exchange, and considering how challenging measurements in these remote regions are, I highly value the multi-year data series that are presented here. Further, there are only a few studies that report C fluxes measured with different techniques simultaneously, as is done here, and studies such as this are very much needed to improve our ability to constrain the high-latitude C budget. I also appreciate the detailed and thorough vegetation analyses performed in this study to accurately determine LAI and linking vegetation characteristics to fluxes.

I have a couple of comments that I encourage the authors to address before publication.

1) I suggest adding a few sentences discussing the possible reasons for the observed differences between manual chamber and eddy covariance estimates observed here

AR: The two estimates give similar trends. Differences in the magnitudes most likely arise from the small number of the chamber measurement plots and accuracy of the LCT map. We have speculated this in the discussion.

2) Please add a short explanation of high- vs. low affinity methane oxidation as well as of barrens for the general benefit of the reader (lines 78-80). Barren tundra surfaces can be quite different from each other (rocky surface with thin or absent organic layer/polar deserts, or eroding surfaces in more organic-rich areas, peatlands). Would be good to know which type the authors refer to here, and if CH4 uptake occurs from all barren surfaces or some ecosystem types in particular. Similarly with high-affinity methane oxidation: CH4 oxidation in high vs low CH4 environments (low- vs. high affinity methanotrophy) are important concepts for this study looking at contributions from wet vs dry tundra, so they should be adequately addressed in the introduction if mentioned.

AR: This is an important comment. We have improved the site description and the different CH_4 consumption concepts and the importance of the consumption overall in the introduction and elsewhere in the text.

3) The measured CH4 uptake rates seem rather high, especially some of the maximum values presented in Fig. 5 for lichen tundra. I consider the observed large contribution as a CH4 sink of this landcover class to the regional CH4 balance an important finding and agree it is important to highlight this in this study as the authors have done. However, I am skeptical of these very large flux rates that seem to be one order of magnitude larger than what has been reported previously (references below). Since this is a potentially important message of the manuscript, I would suggest the authors double check the slopes used for calculating manual chamber fluxes and start point gas concentrations, and afterwards re-evaluate if the reported 10% offset of CH4 emissions by CH4 consumption is accurate.

Looking at Fig. 5, the maximum CH4 uptake goes as low as -0.1 mmol CH4 m-2 h-1. If my conversion is correct, this corresponds to -39 mg m-2 d-1. This would seem like an unreasonable large flux to me, considering diffusion constraints of atmospheric CH4 into soils. I recommend the authors double-check at least these large uptake rates, as they may substantially distort the mean.

- Are these manual chamber measurements (flux calculation based on only a few data points and lower accuracy when measuring with GC) or were these fluxes measured with the LGR?

- what was the initial concentration at the start of the measurement/starting point of the selected slope? Did the authors check these concentrations were close to ambient? Otherwise, a starting concentration above ambient after chamber placements may not yield realistic flux estimates.

- what was the minimum number of points included, e.g. for manual sampling with 4 time points, were always for points used for determining the flux or even less?

- Reported EC values are in the same range. Is the closed-path eddy covariance instrument that was used reliable for low concentration (below ambient) measurements, or do these concentrations have to be taken with a grain of salt? Any issues with instrument noise for the low end of fluxes?

Compared to CH4 uptake reported from northern soils (Arctic + boreal) these values would appear one order of magnitude larger than could be expected. In lines 499-502 the authors compare their fluxes (mean: 0.02 mmol m-2 h-1, max 0.1 mmol m-2 h-1) to CH4 uptake rates determined at similar sites which were about one order of magnitude smaller (0.005-0.01 mmol from bare ground, 0.003-0.004 mmol m-2 h-1, ref D-Imperio et al. 2017), and are in the range of what has been reported from Arctic-boreal synthesis studies on CH4 fluxes from a large number of sites. I suggest comparing with some of these studies, for example the following references:

Kuhn, M. A., Varner, R. K., Bastviken, D., Crill, P., MacIntyre, S., Turetsky, M., ... & Olefeldt, D. (2021). BAWLD-CH 4: A Comprehensive Dataset of Methane Fluxes from Boreal and Arctic Ecosystems. Earth System Science Data Discussions, 1-56.

Bartlett, K. B., & Harriss, R. C. (1993). Review and assessment of methane emissions from wetlands. Chemosphere, 26(1-4), 261-320.

E.g., Bartlett&Harriss report that CH4 uptake from these ecosystems is generally < -2 mg CH m-2 d-1 on average, and the more recent synthesis by Kuhn et al. report uptake in the range of -1.1 - -0.17 mg CH4 m-2 d-1.

AR: The large consumption rates occurred in schist-like rocky surface with negligible organic matter (Fig 1 below). We checked the outlier data point (the most negative value) and found a typing error. In two other cases we recalculated the flux using a longer closure period and the effect of initial high flux after the closure evened out. We opted for the more conservative estimate in these cases. Most of the data were measured using the LGR analyzer, and we show below (and in the ms appendix) examples of chamber concentration time series (Fig. 2 below). The precision of the instrument is sufficient to detect small fluxes, and there are more than 60 data points per a closure. The starting concentrations were checked and confirmed to be ambient. The mean flux for the barren decreased after recalculating the three highest fluxes, but the consumption rate of the barren surface remains high. Consequently, the estimate of total CH_4 consumption relative to the emissions in the landscape decreased, and the resulting shares of CH_4 flux in barren and all the consuming LCTs were 6% and 8%, respectively, of the emissions. Tables and figures are corrected accordingly. Overall, the large consumption of CH_4 was evident and was also detected by the EC measurements. That is shown in Fig. 7f, and also earlier by Tuovinen et al. (2019). The sensitivity and precision of the EC instrument are sufficient to detect the small downward CH_4 fluxes, especially when averaged over a longer time period. Fig. 5 in Tuovinen et al. (2019) shows the CH_4 fluxes in different wind direction sectors.

We compiled, however, reference data into a table now included in the text (see below and Table 4) and enhanced the discussion. In this review, we focused on similar dry tundra sites, including barren and polar desert sites and relevant data from the BALDW synthesis (Kuhn et al. 2021), which are from the study of Emmerton et al. (2014), incl. five sites on Ellesmere Island. The table indicates, as already known, that the barren surfaces with limited organic matter and energy supply for the microbes tend to have large CH_4 consumption rates.

Fig. 1. (Ms Fig. A1) Examples of the barren (left) and lichen tundra (right) plots with close views. Vegetation consists of, for instance, lichens, *Dryas octopetala*, and dwarf shrubs *Vaccinium vitis-ideae.*

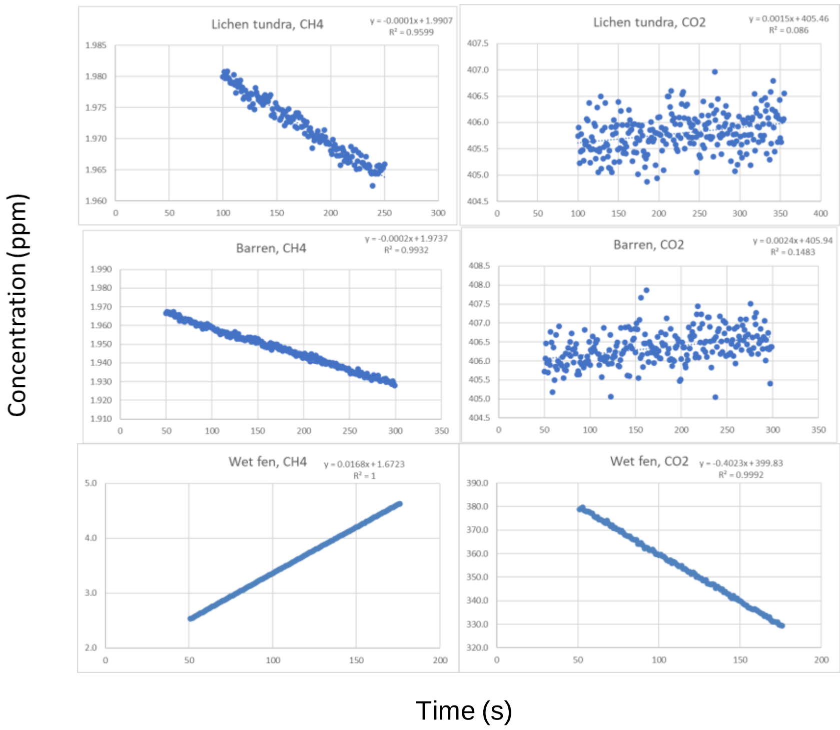

Fig. 2 (ms Fig. A2). Examples of gas concentration variations in chambers measured using the LGR analyzer. The examples represent lichen tundra, barren, and wet fen.

Table 1 (ms Table 4). Review of reported consumption rates of atmospheric CH_4 in dry mineral soil tundra incl. this study.

Location	Habitat type	Mean	Min	Max	Reference
		$(\mu mol\ m^{-2}h^{-1})$			
Narsarsuaq, Greenland	low elevation heath vegetation	-1.2	-4.0	-0.2	St Pierre et al. 2019
Narsarsuaq, Greenland	high elevation heath vegetation	-2.6	-11.9	3.6	St Pierre et al. 2019
Disko Island, Greenland	low elevation heath vegetation	-3.8	-12.1	-1.1	St Pierre et al. 2019
Disko Island, Greenland	high elevation heath vegetation	-3.5	-12.1	-1.3	St Pierre et al. 2019
Tierra del Fuego, Argentina	alpine tundra	0.5	-16.6	10.3	Sá et al. 2019
Disko Island, Greenland	dry tundra heath[1]	-4.0	-4.4	-2.5	D'Imperio et al. 2017
Disko Island, Greenland	bare ground[1]	-9.0	-15.0	-3.8	D'Imperio et al. 2017
Disko Island, Greenland	*Betula nana* and *Salix* sp. heath	-4.0			Christiansen et al. 2014
Axel Heiberg Island, CA	vegetated ice-wedge polygon		-2.7	-0.3	Lau et al. 2015
Lake Hazen, Ellesmere I., CA	polar desert[2]	-3.6	-7.0	0.0	Emmerton et al. 2014
Zackenberg Valley, Greenland	moist tundra	-3.1	-7.0	-2.0	Jørgensen et al. 2014
Zackenberg Valley, Greenland	dry tundra & barren ground	-7.0	-16.0	-4.0	Jørgensen et al. 2015
Zackenberg Valley, Greenland	tundra heath	-1.3	-6.0	0.0	Christensen et al. 2000
Okse Bay, Ellesmere I., CA	polar desert[3]	-0.5			Brummel et al. 2014
Petterson R., Ellesmere I., CA	polar desert[3]	-0.04			Brummel et al. 2014
Dome, Ellesmere I., CA	polar desert[3]	-0.5			Brummel et al. 2014

BAWLD-CH_4 Synthesis	dry tundra		-2.9	5.2	Kuhn et al. 2021
BAWLD-CH_4 Synthesis	boreal forest		-2.6	-0.5	Kuhn et al. 2021
Tiksi, RU	lichen tundra mean	-11.3	-57.9	-0.4	This study
Tiksi, RU	barren	-18.1	-57.9	-3.0	This study
Tiksi, RU	vegetated	-6.0	-34.7	-0.4	This study
Tiksi, RU	meadow	-1.0	-21.1	24.5	This study
Tiksi, RU	dwarf-shrub tundra	-0.2	-2.9	20.3	This study
Tiksi, RU	bog	-2.1	-14.8	6.6	This study

1) mean estimated from a figure, 2) min and max estimated from a figure, 3) one-three day measurement

Line edits

Introduction

L62: and warming?

AR: text edited

L78: add reference. Also, useful to add that dry tundra is often reported as CH4 neutral, not necessarily as a small sink even. A recent reference that the authors may find useful: Kuhn, M. A., Varner, R. K., Bastviken, D., Crill, P., MacIntyre, S., Turetsky, M., ... & Olefeldt, D. (2021). BAWLD-CH 4: A Comprehensive Dataset of Methane Fluxes from Boreal and Arctic Ecosystems. Earth System Science Data Discussions, 1-56.

AR: Text edited accordingly

L78-80: a short explanation of tundra barrens and high-affinity methane oxidizers would be useful in this context (see comment above).

AR: Text changed as suggested

L87-88: Please be more specific – biased towards what? Does this mean in heterogeneous environments estimates are biased towards emissions? Or biased in that sense that an integrated flux does not yield sufficient information on sink/source behaviour of individual landcover types?

AR: The EC measurement may bias the flux estimate if the surface source/sink distribution is heterogeneous. This results from the fact that, even though EC integrates spatially, the spatial integration involves non-uniform weighting of the upwind surface elements (flux footprint).

Furthermore, the EC sampling varies temporally depending on meteorological conditions (footprint climatology), which affects the resulting flux statistics. Fluxes are not systematically biased towards either emission or uptake; the bias depends on the distribution of footprints vs that of sources/sinks (see Tuovinen et al., 2019). We rephrased the text.

Methods

L110: delete “normal”

AR: That’s replaced by climate normal

L113: soil organic matter content? Additionally, please provide some information of organic layer thickness at the site in the methods text, and refer to Table 1. Based on the reported low OM content, lichen patches are located exclusively on mineral soil with very thin or no organic layer? Do the authors have any information on the lichen species that could be added?

AR: The most abundant lichen genera were added into the Table 1. Those are *Thamnolia*, *Flavocetraria*, and *Alectoria*. Information about the organic layer depths added in the text: it is negligible in lichen tundra (and barren), a few cm in the dwarf-shrub tundra, meadow, and graminoid tundra, and at least 30–40 cm in bog, dry fen, and wet fen (Mikola et al. 2018)

L170: please add specifics of vials used for storage as well as type of GC (manufacturers, volume, tested for gas tightness during storage, how long were samples stored before analysis?)

AR: The following specifications are included in the text now. “four air samples were taken from the chambers using syringes (250 mL). The sample was pumped into a 50 mL glass vial with a rubber septum. The vial was purged with the sample and the vial was over-pressurized with the last 10 mL of the sample. Methane concentrations in samples were determined at the laboratory of the Voeikov Main Geophysical Observatory using a TSVET 500-M gas chromatograph (Chromatek, Ru) with a flame ionization detector. Each measurement was accompanied by calibration using standard gas mixtures with the NOAA2004 scale. The samples were analyzed within a month. The vials were tested prior the measurements: after two weeks, the samples were still over-pressurized and CH_4 concentrations were within ±3 ppb the concentration in the standard gas”.

L173: Was the 5-minute enclosure time applied to all surfaces, and was this enclosure time sufficient to accurately determine slope for low emitting (or uptake) sites?

AR: It was enough. The analyzer has a high precision.

L178: What about non-linearity due to PAR for CO_2 measured with transparent chambers?

AR: Data were screened for variation in PPFD and rejected if the variation exceeded 100 µmol $m^{-2}s^{-1}$. Now mentioned in the text.

L174-178: Where there some general rules applied as to how many points were usually discarded at the beginning of each measurement, and how many points were included for flux calculation? How was the quality of fluxes assured (R2, RSME, other)? Please add some specifics.

AR. We added specifics of the flux determination in the text: The first data points were generally neglected when determining the slope of concentration change over time and cases with linear concentration change had coefficient of determination (R^2) > 0.9. No change in concentration meant zero flux. There were a few ebullition cases at the vehicle track measurement points that had only sparse or no vegetation cover. When determining NEE using the transparent chamber, the data were screened for variation in PPFD and rejected if the variation exceeded 100 µmol m^{-2} s^{-1}.

L262: why were different classes for graminoid tundra applied to CO2 and CH4 and not the same for both gases?

AR: That was based on assumed flux rate similarity based on moisture and vegetation characteristics.

Results

L297-298: Check sentence structure.

AR: Right, the text edited.

L330: This is indeed quite large as a mean flux for atmospheric CH4 consumption. Please see specific comments above.

AR: See our reply above. Even though the largest negative flux data point was erroneous, and we applied more conservative data selection for the other two large value, the mean is still large. The large sink is supported by the EC observations, and the EC estimate for the northern sector shows actually larger consumption of CH_4 than our chamber-based estimate.

L367-398: It would be interesting to see the time series of CO2 and Ch4 fluxes measured with the eddy covariance technique, instead of just mean numbers based on wind sector contribution. That way the reader would get a better overview of the timing of high/low fluxes or possible peaks that would help interpret the data and help understand discrepancy between chamber and EC data. This time series could be in shown as supplementary in case the authors are tight for space in the main text.

AR: We compared data measured in the same time window. We refer here to Figure 5 in Tuovinen et al. (2019) that shows the 30-min CH4 fluxes as a function of wind direction. The range of these fluxes is about -0.045 to 0.18 mmol/m2/h, the limits corresponding to the barren and wet fen values measured with the chambers.

Discussion:

L422: Do the authors mean 9% ? Early they state 10%.

AR: We checked the consistency and replaced with a revised value (that is 8% of the emissions)

L430-431: their high OM content is already mentioned in line 426.

AR: We edited the sentence

L441: soil organic matter?

AR: The sentence was edited (l. 462)

Figures and tables:

Fig. 2: add information on landcover class (wet fen, dry tundra) in figure panels c), d) and e) instead of just in the figure caption. Add info on missing thaw depths measurements (panel f) for some landcover types (e.g., too rocky under lichen cover) in figure caption.

AR: Changed as suggested

Fig. 3: please add percent explanatory power to each component axis (xx%). The DCA is very much dominated by the high CH4 fluxes from wetlands. The authors may want to consider adding a second panel to this figure, where they provide DCA only for low-emitting and uptake sites, to identify the influence of environmental settings on low fluxes. Also, is there a reason why soil temperature was not included in the figure?

AR: As suggested the percentages explained are now shown in the axis titles. We think that it is unnecessary to show only the dry tundra plots as a DCA plot, because it is already evident from the current plot, i.e. minimal vegetation and dry conditions. Soil temperature was not comprehesively measured across the plots and thus we opted not to use it.

Fig. 5: Please see my comments above regarding large uptake in lichen tundra. Additionally, consider colouring the fluxes by measurement year.

AR: The figure is revised according to the data changes. The data distribution is explained in Table 2 and therefore the symbols are unchanged.

Fig. 6: Symbols for vehicle track and bar

AR: The figure has been edited and those surfaces are included in the current version.