Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2022-5-RC1, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

reviewer comment on bg-2022-5

Anonymous Referee #1

Referee comment on "Variation in CO_2 and CH_4 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-RC1, 2022

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

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.

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.

Line edits

Introduction

L62: and warming?

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.

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

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?

Methods

L110: delete "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?

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?)

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?

L178: What about non-linearity due to PAR for CO2 measured with transparent chambers?

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.

L262: why were different classes for graminoid tundra applied to CO2 and CH4 and not

the same for both gases?

Results

L297-298: Check sentence structure.

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

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.

Discussion:

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

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

L441: soil organic matter?

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.

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?

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

Fig. 6: Symbols for vehicle track and bar