

Interactive comment on “Interpreting eddy covariance data from heterogeneous Siberian tundra: land cover-specific methane fluxes and spatial representativeness” by Juha-Pekka Tuovinen et al.

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We thank the referee for the comments. We respond to each of them in the following.

Comment:

The manuscript by Tuovinen et al. aims to use a detailed footprint analysis of a flux tower in Northeast Siberia to identify sensor location bias, while making use of high resolution satellite imagery. I think this study is interesting but, like the other referee, I think this paper could be a lot more effective. I agree with much of what is said in the

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other review, but I have a few additional comments.

Reply:

Please see also our response to Referee 1.

Comment:

First of all: if sensor location bias is your main goal, this can be done a lot easier through an analysis of the land cover classification from the satellite image. The heterogeneity of the landscape is already captured in there, and by taking the average of a hypothetical footprint area at random points within the satellite image, it can be clarified which locations resemble the larger area the most. A rough estimate of the footprint would be required but this would be a lot simpler than the approach in the paper.

Reply:

Estimation of the sensor location bias is included in one of the three main aims of the manuscript; these aims are reported in the Introduction (p.4, l.18-). We agree that land cover maps provide useful information that can guide the selection of an EC site location. However, the actual representativeness of EC measurements against an areal average cannot be evaluated without considering the unequal weighting of the surface elements in each measurement.

Comment:

As for the footprint area itself: the choice of the Korman and Meixner model is curious. There have been many advances in footprint analysis since, see for example Kljun et al (2015) who described a two-dimensional footprint model that already gives the footprint contribution for each part of the footprint. The model is freely available at <http://footprint.kljun.net> for Matlab, R and python.

Reply:

Despite recent advances in footprint modelling, the Korman and Meixner model is still

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commonly used since it is based on a valid micrometeorological theory of surface layer turbulence and involves a minimum number of additional assumptions for practicality. It is especially suitable for smooth surfaces, such as tundra. Furthermore, all the input data are directly obtained from the EC measurements, whereas an estimate of the mixing height is required if the Kljun et al. model is used. We will add discussion about the uncertainties related to footprint modelling.

Comment:

If the authors had used this model, they could have simplified a large part of the methods, which would really help with the readability of the paper. At present, the long list of equations makes it hard to follow what the direction of the paper is, especially since many of them are not referred to later on. Like the author referee said, we don't need textbook knowledge. Sections 2.1 and 2.2 should be drastically shortened to only those equations directly relevant to the paper.

Reply:

The choice of the footprint model employed has nothing to do with the equations presented in the manuscript. The equations in Sections 2.2.1 and 2.2.2 are totally independent of the Kormann and Meixner model and would be exactly the same for any other model. Using the Kljun et al. model would not simplify the methods a bit, nor would it make any practical difference in terms of computational complexity. Both models provide the footprint field (variable f in the text) for each 30-min averaging period, which data are then processed as described in the manuscript. As for the equations, two of them are not referred to later: Eq. (6) was included for completeness and can be removed, and Eq. (12) is an essential part of the methods. To improve the focus of the manuscript, we will remove Sect. 2.2.1 and include a shortened version, together with the related results, as an appendix.

Comment:

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Also, I don't understand why there are not more flux chamber measurements in the area, but only from bare soil. The spatial heterogeneity of methane fluxes can be much better identified from direct measurements rather than the inverse method presented here.

Reply:

Essentially the same question was raised by Referee 1, so we repeat our reply: The reviewer is right that the chamber measurements cited in the manuscript were not limited to bare soil. We also agree that such data would be useful for validating the present results. The reason for not using the chamber data more extensively is that the experimental design was incomplete: the number of chamber plots was modest, and the reach from the EC mast was limited due to the use of an online gas analyser. Moreover, the chamber plots did not fully correspond to the land cover classification that was subsequently developed and used in the present study. Thus it is not possible to use these data for a proper validation of the flux decomposition. The bare soil data were introduced to provide support for the surprisingly high uptakes rates observed. However, we can report here that the overall pattern the chamber data depicts is consistent: wet fens appear as strong CH₄ emitters (two plots, 32 observations at each plot, means of 0.56 and 3.8 microg/m²/s) and dry fens as moderate emitters (four plots, 31/32 observations, mean 0.25 microg/m²/s).

Comment:

Please, when reporting EC methane flux measurements, use nmol/m²/s rather than micrograms.

Reply:

(kilo)gram is an SI unit, so microgram/m²/s is a valid unit for mass flux density.

Comment:

Finally, the authors should put their research in a better context compared to existing

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literature. The discussion is very limited, and previous studies that have had detailed footprint analyses or emphasized the spatial variation of methane fluxes are either ignored or referenced too briefly. See for example Matthes et al (2014) and Maruschak et al (2016) for two prime examples, but also the paper by Parmentier et al. (2011) which you do cite but without mentioning that it also dealt with methane flux heterogeneity. There are many more, and this needs to be recognized.

Reply:

Quite a few papers dealing with footprint analysis and methane fluxes are mentioned in the Introduction and Sect. 3.2, but we will enhance the discussion in this respect and include the suggested references.

Other comments:

Comment:

Page 2, line 27: it would make more sense to mention only natural emissions, since this paper focuses on that. This number of 560 Tg/yr is anthropogenic plus natural.

Reply:

The sentence in question refers to the importance of CH₄ as a greenhouse gas, i.e. to climatic influence that is independent of the origin of emissions. We will add the proportion of natural emissions (40%) to the sentence.

Comment:

Page 3, line 13-15: this has been known for decades, and should not be presented as something new. See for example Christensen, 1993; Torn and Chapin, 1993; Whiting and Chanton, 1992.

Reply:

Our intention was not to imply that this is a new finding. We will rephrase this sentence

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and consider the suggested literature (thanks for pointing these out).

Comment:

Page 5, line 27-28: a table with vegetation descriptions for each class would be helpful.

Reply:

We will add such a table.

Comment:

Page 6, line 9: have you considered NDWI as another wetness index?

Reply:

We have an accurate classification of water areas and we used the topography-based TWI to predict the location of potentially wet soil, which is relevant to CH₄ production. We did not test NDWI.

Comment:

Page 10, line 23: is this normalization necessary? This paragraph seems like a complex way of simply saying that you have some unknown data in your average. In any case, at 1.4 km from the tower the effect would be negligible.

Reply:

For many calculations it is indeed necessary. Without this normalization we would underestimate the footprint-weighted averages, if the weights do not add up to 1. As shown in Fig. 3, the effect is significant especially in stable conditions.

Comment:

Page 12, line 23: which Eriophorum species? Not all of them are high emitters of methane, like Eriophorum vaginatum.

Reply:

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We will add details to the revised version.

Comment:

Page 15, line 6-7: “the areal coverage of the LCC within the study area”? what do you mean? The LCC map?

Reply:

There is a typo; it should read ‘LCCs’.

Comment:

Page 17, line 27: this feels like cheating. The tower is not representative for the larger region, so you reduce the region. Please don’t.

Reply:

We object this kind of language in scientific correspondence. Furthermore, the comment misses the point of the discussion in this paragraph. We show that the spatial representativeness of EC measurements (sensor location bias) depends on the scale. In contrast to what the reviewer states, the tower actually is representative also on a larger scale (“the overestimation of EC measurements of the CH₄ flux averaged over the study area is not statistically significant ($p > 0.05$)”, p.17, l.26-27), and referring to our calculations, we simply point out the scale that would minimize the bias. We believe it would have been fully acceptable if we had originally chosen a radius of 1 km rather than 1.4 km, as there is no standard procedure for defining the study area, which potentially results in a significant and unknown measurement bias. In stark contrast to “cheating”, we seek for objective criteria for a consistent definition. We will rephrase these sentences to make our point clearer.

Comment:

Page 18, line 2: there’s no doubt that lakes are a significant source of methane in the Arctic. Please rewrite this sentence to remove the suggestion that it may not be (e.g.

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‘Nevertheless, Arctic lakes and ponds emit significant amounts of CH₄ in general).

Reply:

We agree. The grammatical error will be corrected by removing ‘may’.

Comment:

Page 31, figure 1. Please don’t use a continuous color map for land cover. Use discrete colors.

Reply:

While the colour maps used in Figs. 1c-d are continuous (for continuous variables), this is not true for Fig. 1a that shows the discrete land cover classes.

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