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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.

Anonymous Referee #1

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Review on the manuscript titled 'Interpreting eddy covariance data from heterogeneous Siberian tundra: land cover-specific methane fluxes and spatial representativeness', submitted for publication in Biogeosciences by J.-P. Tuovinen et al. in April 2018.

The authors present a study that aims at decomposing the flux signals captured by an eddy-covariance flux system into flux signatures for individual land cover classes (LCCs) within the heterogeneous terrain surrounding the tower. In a first step, the landscape is mapped at highest resolution based on remote sensing datasets supported by ground-trothing, yielding gridded maps of LCC, elevation, NDVI and wetness. Based on this information, the surface is subsequently categorised into 4 major groups, which are expected to feature significantly different CH4 flux signatures. Using about 8 weeks of flux data from a Siberian tundra site, Tiksi, in combination with an analytic footprint model, the authors then derive mean CH4 flux rates for each LCC group. Finally, the study evaluates the larger-scale representativeness of eddy flux measurements at the Tiksi site using the so-called sensor location bias as a metric, which compares the fractions of LCC groups within the tower footprint to those within the larger region. Here, in spite of significantly different flux signatures between groups, they find no systematic biases between study regions of different sizes.

The scientific objective behind this study is certainly relevant to the eddy-covariance (EC) community, and to all those using eddy-covariance datasets. While the EC-technique features many advantages, such as non-destructive, continuous measurements at high temporal resolution, the interpretation of results is often hampered by the fact that EC systems integrate signals from larger, often heterogeneous areas. Particularly in case of highly variable flux signatures within the landscape of interest, it is moreover often unclear how well the data from the chosen tower position represents the characteristics of the larger area. Therefore, an approach to decompose this integrated signal into separate flux signatures that represent major surface types within the field of view of the sensor could open new possibilities to data users, e.g. for modelers who this way could calibrate their frameworks for individual land cover types. Unfortunately, the presented approach falls short of this ambitious goal. The results are compromised by missing important components and over-simplifications, while the text itself is unbalanced regarding the level of detail in different sub-sections.

MAJOR COMMENTS 1.) constant flux rates: I was extremely surprised when reading the results section to find that temporal variability in flux rates is ignored, instead a constant flux rate is assigned to each of the chosen LCC groups. This constitutes an over-simplification of the tundra ecosystem in the first place, but also limits the presented approach to extremely basic overall findings. Considering that fluxes over a period of 8 weeks from the late growing season of an Arctic ecosystem are used, besides short and mid-term climate variability the fluxes will be influenced by slowly varying conditions such as e.g. thaw depth or soil temperatures. Accordingly, it cannot be assumed that even mean flux levels for moving windows remain constant over such a long period. Even more important in this context, I would strongly assume that the different LCC groups will show a different trend in phenology in this part of the season, i.e. some may be subject to earlier senescence, and also some of them may react more strongly to environmental stress such as water limitation, or first nights with freezing temperatures. This implies that the differences/ratios between flux rates from LCC groups will not be constant over time. So what is the value of providing just a single mean flux rate per LCC group? Would the differences still be significant if short-term temporal variability, and shifts in flux rate ratios, would be taken into account? Regarding the applicability of the results, under the given circumstances the output of the flux decomposition is of no value for any other purpose than investigating a potential sensor location bias (and even here the impact is limited). To reach a broader audience, temporally varying fluxes per biome with functional links to environmental controls would have to be provided. Summarising this item, it is obvious that the chosen approach is based on a strong simplification of the actual flux patterns. This should be discussed in detail in a revised version of the manuscript. This discussion needs to be supplemented by a demonstration how variable measured net flux rates are over time, broken up into the chosen LCC groups. If it cannot be proven that the ratios between flux rates from different groups remain largely constant over time, the approach cannot be applied as is. In general, I strongly urge the authors to consider extending their approach, so temporal variability in flux rates can be considered.

2.) uncertainty estimates: The manuscript misses to even discuss some essential sources of uncertainty that influence the given approach, and those few aspects that are treated (e.g. uncertainties in maps) are only covered qualitatively. Even very easy components, such as e.g. assigning an uncertainty to the input flux rates from the EC system, which is then projected onto the modelled LCC flux rates, is missing. Most importantly, there is no uncertainty estimate for the footprint approach. It is obvious that any source weight function can only be an approximation of the actual field of view of the sensor, as many footprint validation studies have shown in the past. In this study, however, footprint simulations are treated as a given fact. There are uncertainties in all the input parameters used to feed the footprint model, there are uncertainties associated with parameterizations/assumptions inherent to the footprint model, and there are uncertainties related to the methodology (e.g. horizontal homogeneity, stationary flow, and so on). For a modified version of this study, the authors need to provide a convincing concept to constrain the uncertainties in computed source weight functions, and how these influence the results obtained by the flux decomposition approach. In addition, the uncertainty concept should, as mentioned above, also involve the flux data uncertainty, and also the uncertainties inherent to the maps used in this study should be involved, and quantified.

3.) Validation of flux rates: In Section 3.2, the authors include a good paragraph (p.16 ll.20ff) that supports the negative flux rates found for bare soil. As part of the line of argumentation, chamber measurements from a previous study are cited. I think it's fair to assume that this study did not only measure those 32 data points cited here for bare soil, but also other components within the Tiksi landscape. Why are those not used? Having flux chamber results for the different LCC groups would be the best way to validate that the flux composition actually produced realistic results. Also, the Tiksi flux tower has been running for several years now - why restrict this study to just 8 weeks? Why not use more data, so the database is more representative, and can also resolve temporal variability? Why not split the dataset into training and validation sets, so any finding can actually be evaluated?

4.) scope of this study: With the limitations of the chosen concept (constant fluxes) as mentioned above, the authors should clearly restrict the scope of the study to an estimate to constrain sensor location bias. I do not see any other application of their method besides this (I would be glad to get convinced otherwise, e.g. by a thorough discussion ..). I do not think they can claim to provide land cover specific CH4 flux rates, since they present one set of mean flux rates for a single period of time, nothing more. They also do not interpret EC data, since obviously there's no temporal variability, no links to environmental controls, no interpretation why certain LCC groups show different fluxes than others. What is being provided here is an extremely simplified approach to estimate flux rates per LCC group, and check if, given these flux rates, the net fluxes represent the emissions from a larger area (aka sensor location bias). Since there is also no discussion which aspects influence the performance of this approach (e.g. length scale of variability in terrain features, differences in flux rates between LCC groups, footprint variability, etc), there is no way of telling if this approach could be applied at other sites as well.

5.) a thorough discussion is simply missing! What is the implication of the findings? How could the presented approach be used? Where are the weaknesses, which factors limit the interpretation of the findings?

These are the main points of criticism. More general and technical comments are listed below.

Summarising, I think there is a lot of potential in developing solid schemes that facilitate the decomposition of eddy-covariance flux rates into signal for individual landscape components within the tower footprint. The presented approach, however, provides only a very minor first step towards this direction. The only contribution towards an improved evaluation of eddy-covariance data I currently see is that, given very strong simplifications regarding signal variability, a sensor location bias can be roughly approximated. In case such a bias is detected, however, the output of this scheme wouldn't help to overcome it. Under this light, the overall content of this paper is quite slim, particularly if many unnecessary sections providing textbook knowledge are removed. Summarising, in the present form that manuscript is far from being ready to be accepted for publication. Still, I believe the chosen topic is very relevant, and therefore I hope that the authors are willing to fix the issues raised in the comments above (and below).

GENERAL COMMENTS 1.) The introduction is well structured overall, and the 3 different sets of objectives are clearly formulated. The paragraph on methane (starting p.2 l.25) is rather confusing, though, and should be revised.

2.) Section 2.2 needs a complete overhaul. Many sections, e.g. most parts of 2.2.1, are textbook knowledge, and do not need to be shown in detail herein. Section 2.2.2 is much too detailed for what actually needs to be described. You project a source weight function on gridded maps, and accumulate the weights of individual cells, sorted by categories, nothing more. Overall, this whole section is much too long. I suggest to revise it to the following structure: - 2.2.3 should be moved to the front - 2.2.2 should be shortened, and simplified, coming next - 2.2.1 should be discarded entirely - 2.2.4 should be moved as part of the results section

3.) In Section 2.3, the ordering of the information should be revised. Many pieces of information given in 2.3.2 were needed to interpret the text in 2.3.1, for example.

4.) Results Section 3.1: The first part on general footprint characteristics should be removed (P1, P2). After all, what you basically state here is the obvious fact that footprint areas grow with stable stratification. The authors may move Table 3 to the appendix, and refer to it in the main text in case any reader wants to see the details, but this is clearly not part of the main story. The center part, highlighting the heterogeneity of surface characteristics within the footprint, reads well (P3-P5). The last part (P6+) should be revised - it is informative to describe a sensor location bias using the different surface characteristics, but the current format is confusing, using too many versions of a reference area (also Table 4 should be reduced).

4.) p.16 l.10ff: I don't think it makes much sense to compare the Tiksi flux rates against values from other sites without also comparing environmental conditions, and the measurement approaches.

5.) results section 3.3: It is confusing, and actually not understandable, why so many different reference areas have been used to compare the footprint LCC composition to. This actually leaves the impression that the authors were searching for a nice configuration that can demonstrate that the EC measurements are actually well representative (e.g. p.17 l.27 ‘ the sensor location bias could be minimized by reducing the radius to 800– 1000 m’). What is the value in such an exercise? People who are interested in using EC data want to know how well they represent a LARGE area.

TECHNICAL COMMENTS p.3 l. 16: I don’t see a connection between spatial heterogeneity and the need for long-term measurements . . .??

Section 2.1.1: A bit too brief. Soil types could be mentioned, and it should be mentioned that vegetation is given in a different subsection.

Section 2.1.2: The outline of the QC is too short. What exactly was done regarding instationarity, for example? How were unphysical outliers defined? And how were the gaps treated in the end?

Section 2.1.3: I suggest moving the definition of PCTs into a table. It should be mentioned that the dominant vegetation, and other characteristics, are described later in 2.1.4

p.6 l.16: The authors should decide if they want to use LCC or PCT as a term for this classification. Using both is very confusing!

Fig.4: the lower 3 panels are not necessary , since they show the same patterns as above, only normalized against the black dashed line

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