Reply to comments of referee 1

Manuscript: Scaling and balancing carbon dioxide fluxes in a heterogeneous tundra ecosystem of the Lena River Delta

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Colour legend: Referee's comments are written in red
Our responses are written in blue

General comments

The authors of "Scaling and balancing carbon dioxide fluxes in a heterogeneous tundra ecosystem of the Lena River Delta" have collected what appears to be a robust eddy flux data set from a remote location in the Siberian arctic. They conduct a modeling analysis in order to downscale the fluxes and distinguish between the signals associated with the two types of vegetation cover present in the towers footprint. They then use the resulting model to calculate a robust greenhouse gas budget. Overall the data, analysis and questions seem to be relevant and of high quality. However, I have a number of questions and suggestions. A more thorough exploration of literature focused on similar questions and utilizing similar methods could improve the introduction and framing of the manuscript. I also have questions regarding why the authors chose to fit parameters at a daily time step rather than explicitly represent leaf area index or NDVI in the model and why they chose to make comparisons to the parameters and fit statistics of Shaver et al 2007 who utilized a distinctly different model. Moreover, chamber flux measurements, ground truthing of the vegetation classification used in processing the fluxes, methane flux measurements and a correlation between the model parameters and NDVI are mentioned in the discussion but do not appear to be included in the text of the methods or results. Finally the phrasing and word choice at times makes the manuscript challenging to read and interpret. The results and overall analysis seem to be of high quality but the manuscript itself needs changes to clarify some ambiguities and better guide the reader.

We sincerely thank the reviewer for providing his helpful comments, which we implement in order to improve the quality of our manuscript.

Specific comments

P2 L3: the meaning of "exposed land area" is unclear.

The term "exposed land area" was adopted from the original source (Zhang et al., 2000), where it had been repeatedly used. We suppose that this term was chosen in order to distinguish it from sub-sea permafrost. We changed the term to "land area".

P2 L35: The meaning of "flux variability" is unclear. I'm assuming it refers to variability across space rather than say across time. Also, I would not consider "vegetation composition and structure" to be "environmental controls".

The text has been modified as suggested.

P2 L38 – P3 L5: Could this list of the drawbacks of chamber measurements be simplified? A lot of space is dedicated to it and it seems ancillary. Although the authors decompose eddy flux data to make inference about different microforms within a single site I'd argue they do not directly compare eddy flux and chamber measurements and attribute differences between the carbon budgets obtained to any of these particular drawbacks.

We did not pursue the aim to discuss and attribute differences in carbon budgets obtained with both measurement approaches. We rather introduced the common approach for achieving our objective (i.e. investigating micro-scale fluxes). However, we did not choose this approach, and substantiated this decision with the elucidated drawbacks. As we put that in the introduction, we deem that it is appropriate to synthetise the drawbacks of chamber measurements in the form of a little review, not least to also highlight our approach, where we circumvented these problems (i.e. using the advantages of eddy covariance measurements on the mesoscale whilst resolving the pronounced variability on the microscale).

We shortened the text in order to clarify these thoughts. Furthermore, we largely reduced the similar critique of chamber-derived fluxes in section 4.2.

P3 L10: Clarifying the meaning of "heterogeneous" when it is first used or replacing it with a more precise term could be beneficial. Does it refer exclusively to heterogeneity in vegetation cover?

As most flux-contributing areas of the flood plain are covered with vegetation, and we used vegetation classes to capture its heterogeneity, we mostly refer to the varying vegetation composition, when mentioning the term "heterogeneous" in our study. In the introduction, where we elucidate the effects of a heterogeneous

landscape on observed flux dynamics, however, we refer to a heterogeneous surface in general, i.e. an irregular pattern of individual land cover components, regardless of vegetation cover. So we modified the text accordingly.

P3 L24: Clarifying the term "microforms" when it is first used and explaining it in relation to the "heterogeneous surface" could also be beneficial.

The term has been clarified as suggested.

P3 L28-35: The first and second objectives seem to overlap a bit.

The objectives have been clarified to reflect this thought.

P3 L28-35: Given that the goals of this study relate to better understanding the impact of heterogeneity in the landscape on fluxes, text should be added to the introduction which better makes the case that the patchy nature of tundra ecosystems contributes to the uncertainties in tundra flux budgets and explores issues related to scaling flux measurements across the landscape. A number of articles have addressed questions similar to those asked here, albeit using different methodology, but the text is not framed in relation to many of them (e.g. Shaver et al 2007, Loranty et al 2011 Ecosystems, Kade et al 2012 in journal of geophysical letters, Shaver et al 2013 Philosophical transaction of the Royal Society, Sweet et al 2015 in Global Change Biology). In particular, Shaver et al 2007 is mentioned in the introduction, but its main conclusion that leaf area index explains a large proportion of the variability in CO2 flux across vegetation types isn't explored in detail.

We changed the text as suggested by emphasising the need for considering the surface heterogeneity, and included useful classification parameters, of which the vegetation and its scale-dependent issues was discussed. We also added the references and the main conclusion of Shaver et al. (2007) as suggested.

P3 L28-35: The introduction could also benefit from discussing the impacts of climate on tundra vegetation and the tundra landscape. Permafrost carbon is an important player in determining the future carbon budget of tundra, but not the only player. This seems especially important given that this article focuses on the impact of vegetation and the physical characteristics of the landscape on fluxes.

We have extended the introduction by impacts of climate change on tundra vegetation as suggested.

P5 L9-36: Why is an evaluation of the classifications accuracy using either independent ground-based measurements or digitization not presented in the text (some uncertainty metrics appear in table 1 but aren't mentioned in the methods)?

Due to the thematic overlap of the present manuscript with Rößger et al. (2019) plus the intention to keep the manuscript short, we referred to (the open-access article) Rößger et al. (2019) for further information regarding the classification routine (including the uncertainty estimation). As suggested, we added some information and a reference in the caption of Table 1.

P6 L23: Why was 1m resolution used here when the classification available was of a much higher resolution?

For accelerating the computation of the Ω values, we chose a resolution of 1 m for both the source weight function as well as the vegetation map. We argue that this resolution is sufficient to resolve the vegetation heterogeneity at our site.

P6 L36 - P7 L24: Could this method have been simplified by including information about NDVI/leaf area index or using a smooth function to describe the change in these parameters over time thereby removing the need to estimate so many parameters at a daily time step? Using information about leaf area index might also make the parameterization more generalizable (see Shaver et al 2007, 2013). Along these same lines given that the model doesn't include NDVI or leaf area index, the distribution of NDVI/leaf area index for the vegetation in the tower footprint was compared to that of the rest of the study area to ensure the model is representative?

Our model procedure surely offers the potential to be simplified, e.g. by including measured vegetation parameters such as LAI values. In fact, we conducted multiple measurements in the footprint with a LAI-2200C Plant Canopy Analyzer. Unfortunately the results were not satisfying, as we found that this type of measurement (with an upward-pointing sensor) was not applicable for vegetation with very little biomass such as sedges, mosses, lichen, which there is a lot of at our tundra site. The LAI measurement strongly depended on the position of the device. Consequently, the LAI values largely varied when sampling the vegetation at the same spot. Therefore, it was not possible to create a discrete time series of objective LAI measurements over the growing season.

LAI values are widespread and hence generalizable. However, P_{max} and α values are also widely used in the scientific community (Mbufong et al., 2014). So for modelling the biomass, we used these two parameters, whose values and seasonal courses could successfully be validated (section 4. 2.). By calculating replacement functions for α

and P_{max}, we used some kind of smooth functions. However, this rather served a gap-filling purpose. Our goal was to consider the high spatial flux variability; hence, we had to utilise a preferably high temporal resolution: we chose a daily timestamp and a spatial resolution of 1 m. In contrast, online available satellite-based NDVI values are provided for intervals of 8 days (or longer in case of technical issues as with MODIS Aqua in 2015) and 250 m pixel size. Nevertheless, NDVI values were used to add confidence to the obtained P_{max} values given the coupling of both parameters. Due to their coarse resolution (and more problems, see p.13 l.28), their confirmative power should not be overestimated.

P10 L30-L35: The river terrace carbon flux budgets and decomposition of the methane fluxes don't appear to be described in any detail in the methods section.

The eddy covariance system on the river terrace has been mentioned on p.4 l.24; further details are available in the given reference (Holl et al., 2019). A brief description of the river terrace site and three further references are provided on p.4 l.1.

We have added some information on the methane flux decomposition in the methods section as suggested.

P11 L23-24: Although they're mentioned here no ground-truthing methods or results are presented in the text.

We assume an appropriate accuracy for our surface classification due comprehensive field surveys (of vegetation and also altitude, soil moisture, moss characteristics, thaw depth) in the course of extensive field work. This gained knowledge on the local conditions largely helped us to create a proper vegetation map based on the high-res orthomosaic. An objective evaluation of this vegetation map was carried out through its comparison with the manually surveyed vegetation map, as a result of which we obtained the classification uncertainties (Table 1).

We added this information in the text in order to respond to the critique.

P11 L37- P12 L2: Why is this described here rather than in the methods and results section?

We agree, this paragraph contains aspects of methods and results. However, similar to the paper of Rößger et al. (2019) that has a similar structure, this paragraph is a part of the validation of the decomposed fluxes, which we think is best placed in the discussion, as we discuss the uncertainty our results. A note on the mounted camera

is placed in the methods. And placing this paragraph in the results would have an adverse effect on the flow of the text

P12 L12: Shaver et al 2007 use a different model, which includes leaf area index and compares chamber flux data from a number of plots in Alaska and Sweden. I don't think it is valid to compare the fit statistics like this.

Our intention was to put our fit statistics in context, and Shaver et al. (2007) was the best study we found for this purpose. However, we agree that this comparison is not valid, and besides, not absolutely necessary. We hence removed that sentence.

P12 L21-35: These chamber flux measurements and the sampling design used isn't described in the methods or results section. Do five measurements refer to five individual discontinuous measurements? Moreover, the critique of chamber-derived fluxes seems ancillary to the question of validating the model especially given that the authors seem to be comparing an extremely small number of respiration chamber measurements to their results.

These chamber measurements play a very minor part in our study. So we invoked them only in the discussion, where they slightly but nicely contribute to the flux validation. More details on the measurements (than given in discussion) in the methods or results section seem unnecessary and off-topic. However, in the text, we included the information of the correct assumption that the 5 measurements were individual and discontinuous.

As suggested, the critique of chamber-derived fluxes has been removed.

P13 L22-25: A correlation between Pmax and NDVI is mentioned here but doesn't appear to be included in any of the figures or results.

For the validation of the obtained P_{max} values, we at first mentioned the relationship between P_{max} and NDVI, and then noted that we found higher NDVI and P_{max} values in the same year; thus, our data is in accordance with that relationship. This is not a very strong argument for the reliability of our fitting parameters, but, again, nicely adds to our set of successful validation measures.

This limited argumentative power is the reason why we did not decide on a figure, in which the seasonal courses of P_{max} values for 2014 and 2015 are plotted together with the seasonal NDVI courses (which are freely available at the stated source of ORNL). However, the fact that the P_{max} values were greater in 2015 can be inferred from Figure 6 and Figure 7.

We replaced the word "correlation" with "relationship", which better outlines the simple P_{max}-NDVI ratio we discussed.

P13 L33-L39: Is this comparison valid? Again the model used in shaver et al 2007 represents the canopy in a different way than equation 2 which is effectively a big leaf model (see Rastetter 1992). Also, the units of Pmax and other parameters aren't provided in the methods for equation 2 and 3.

Our GPP model and the GPP model of Shaver et al. (2007) create light-response curves that include a parameter that describes the initial slope of that curve. However, both models have different parameterisations, and as long as the question on the comparability of their fitting values remains unanswered, the comparison of their fitting parameter values is critical. Therefore, we compared our ratio of $\alpha_{shrub}/\alpha_{sedge} = 1.05$ with Shaver's ratio of $\alpha_{shrub}/\alpha_{sedge} = 1.27$. We argue that comparing ratios instead of absolute values is eligible despite the different parameterisations.

P16 L30-33: Again a relationship between the flux parameters and NDVI doesn't appear to be presented in the text or figures.

As mentioned above we conducted only a simple (greater or lesser) relationship between P_{max} and NDVI. A quantitative analysis of this relationship was not out goal. We raised that issue in the conclusion, as investigating this relationship may be a worthwhile objective of further studies. We delivered P_{max} values and the corresponding (pretty gappy) NDVI values are freely available online.

Figure 2: Putting imagery in the background of this figure and including an inset map showing the site and its extent in regional context could be beneficial for the reader.
Samoylov Island is situated in the Lena River Delta, whose location is displayed in Figure 1. Within this delta, Samoylov is located in the southern central part. This information is given in Rößger et al. (2019), which is referenced for further details regarding the site description. The extent of the flood plain on Samoylov Island is displayed in a small inset in Figure 2. Besides, Samoylov has gained some fame within the scientific community so that many pictures of this Island can easily be found online.

We tried to design a narrow figure, which fits into one text column of the journal in order to save space. If this is not possible (according to the typesetter in case of a manuscript acceptance), we are happy to add more information on the geographical setting.

Figure 6, 7: Why are not units given for the α 1,2 axes.

α is the initial quantum yield and is defined as the ratio of carbon dioxide net uptake (μmol m^{-2} s^{-1}) and photosynthetic photon flux density (μmol m^{-2} s^{-1}); therefore, it has no units. This is in accordance with e.g. Eckhardt et al. (2019).

Figure 9: It's not clear why a scatter plot is used here if the goal is to show the differences between the two vegetation types. Maybe two bar or box plots would fit better.

We included two boxplots as suggested.

Technical corrections

P2 L13-16: Suggest simplifying/rephrasing this sentence, in particular, the phrasing of "However, due to ambiguous results and large confidence intervals, it currently remains unclear".

The text has been changed.

P1 L16: Suggest rewording "flux signal associated therewith could extensively be decomposed"

The phrasing has been changed as suggested.

P1 L18-19: Suggest rewording "unveiled a differing seasonality"

The wording has been altered as suggested.

P1 L25-26: Suggest rewording "approved the reliability"

The phrasing has been changed as suggested.

P1 L 36-38: Suggest restructuring the final sentence of the abstract.

This sentence has been modified as suggested.

P2 L6: Suggest re-wording "historical carbon sink function".

The sentence has been changed as suggested.

P2 L11: Suggest rephrasing “The arctic north of 60 ○ N”

The phrasing has been modified.

P2 L25: Should “ties” read “uncertainties”?

Yes, it reads “uncertainties” in our original manuscript. It somehow got changed.

P2 L27: Suggest rephrasing “reduction of these discrepant uncertainties”.

The phrasing has been altered.

P2 L33, P3 L17 and elsewhere: “aggravates” doesn’t seem to fit in this context

The word has been replaced.

P3 L21: typo “and the study” -> “and to study”

This is not a typo, but we slightly changed that sentence.

P4 L31: typo “an quarter” -> “a quarter”

We have corrected that typo.

P5 L11-12: Suggest rephrasing “a very high spatial information density”

We rephrased that part as suggested.

P5 L13: Suggest listing the software used to carry out the classification here.

The software was ArcMap v10.2.

P5 L16: Suggest using “shrubs” in place of bushes, as this is more consistent with language used in other literature (see: CAVM Team. 2003. Circumpolar Arctic Vegetation Map).

The suggestion has been adopted. We accordingly changed the legend in figure 2.

P6 L8-9: Suggest simplifying or rephrasing “respiration multiplies/divides when the temperature rises/drops”

We agree, the word structure of “a/b” is somewhat unusual, but we argue that this structure is simple and embedded in a short and straightforward sentence. Furthermore, we used a similar structure in the second sentence in the caption of Table 3, where the referee did not make a remark.

P8 L3-5: Suggest simplifying the sentence listing the air temperatures and precipitation rates.

We struggled to convert the already simple sentences into an even simpler version.

P8 L18: The phrasing of “a dominating respiration” is awkward.

The phrasing has been changed.

P8 L24-26: Suggest rephrasing “ripening phase”, “verged on full maturity” and “colouration and shedding of leaves”.

We have modified the text as suggested.

P8 L34-L35: Referring to the classifications by the more descriptive names given earlier rather than a numbing scheme would make the results easier to follow.

The names given earlier are meant to allow a consistent listing of surface classes in the legend of figure 2. We agree that the descriptive names are basically easier to follow than the class numbering scheme. However, the names are only generalisations and hence not very precise. For example, a “sedges class” suggests that there are only sedges in that class, although there are also small shrubs (and mosses and lichen) in that class. Based on this thought plus the fact that we used only two vegetation classes, we argue that our vegetation references (that are less descriptive but more precise) in the text are appropriate.

P9 L16: Suggest rephrasing “featured most of the significant differences between each other”

The phrasing has been changed.

P9 L19: Suggest rewriting/simplifying “On account of both the coinciding varibailites of explanatory variables and explained variable”

The sentence has been restructured.

P9 L25: Suggest rephrasing “less good”

The sentence has been modified.

P10 L16: Suggest rephrasing “despite methane’s minor percentage of roughly 3% in the entire greenhouse gas exchange”.

We struggle to identify a need to rephrase that sentence.

P12 L24: Suggest rewriting “mean carbon dioxide flux with a standard deviation of 2.1 +- 0.9” to make it clear that the first number therein is the mean.

We think that this is pretty clear. If a reader erroneously assumes that the first number refers to the standard deviation, he would inevitably notice while continue reading that his thought was wrong as there is certainly no standard deviation of a standard deviation given in the text.

P14 L11: Suggest rephrasing “mutual start”

The phrasing has been changed as suggested.

P15 L17: “proofs” -> “proves”

The word has been changed.

Figures 1-9: The axis and legends provided seem small even when printed on a full page.

We are happy to adjust the layout of the figures in consultation with the typesetter in case of an approval of our manuscript.

References

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