

Interactive comment on “Scaling and balancing carbon dioxide fluxes in a heterogeneous tundra ecosystem of the Lena River Delta” by Norman Rößger et al.

Anonymous Referee #1

Received and published: 12 March 2019

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

C1

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.

Specific comments P2 L3: the meaning of “exposed land area” is unclear. 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”. 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. 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? P3 L24: Clarifying the term “microforms” when it is first used and explaining it in relation to the “heterogeneous surface” could also be beneficial. P3 L28-35: The first and second objectives seem to overlap a bit. 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 differ-

C2

ent 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. 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. 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)? P6 L23: Why was 1m resolution used here when the classification available was of a much higher resolution? 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 was the distribution of NDVI/leaf area index for the vegetation in the tower footprint compared to that of the rest of the study area to ensure the model is representative? 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. P11 L23-24: Although they're mentioned here no ground-truthing methods or results are presented in the text. P11 L37- P12 L2: Why is this described here rather than in the methods and results section? 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

C3

to compare the fit statistics like this. 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 only chamber measurements to their results. P13 L22-25: A correlation between P_{max} and NDVI is mentioned here but doesn't appear to be included in any of the figures or results. 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 P_{max} and other parameters aren't provided in the methods for equation 2 and 3. P16 L30-33: Again a relationship between the flux parameters and NDVI doesn't appear to be presented in the text or figures. 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. Figure 6,7: Why are not units given for the $\alpha_{1,2}$ axes. 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.

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". P1 L16: Suggest rewording "flux signal associated therewith could extensively be decomposed" P1 L18-19: Suggest rewording "unveiled a differing seasonality" P1 L25-26: Suggest rewording "approved the reliability" P1 L36-38: Suggest restructuring the final sentence of the abstract. P2 L6: Suggest rewording "historical carbon sink function". P2 L11: Suggest rephrasing "The arctic north of 60° N" P2 L25: Should "ties" read "uncertainties"? P2 L27: Suggest rephrasing "reduction of these discrepant uncertainties". P2 L33, P3 L17 and elsewhere: "aggravates" doesn't seem to fit in this context P3 L21: typo "and the study" -> "and to study" P4 L31: typo "an quarter" -> "a quarter" P5 L11-12: Suggest rephrasing "a very high spatial information density" P5 L13: Suggest listing the software used to

C4

carry out the classification here. 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). P6 L8-9: Suggest simplifying or rephrasing “respiration multiplies/divides when the temperature rises/drops” P8 L3-5: Suggest simplifying the sentence listing the air temperatures and precipitation rates. P8 L18: The phrasing of “a dominating respiration” is awkward. P8 L24-26: Suggest rephrasing “ripening phase”, “verged on full maturity” and “colouration and shedding of leaves”. P8 L34-L35: Referring to the classifications by the more descriptive names given earlier rather than a numbering scheme would make the results easier to follow. P9 L16: Suggest rephrasing “featured most of the significant differences between each other” P9 L19: Suggest rewriting/simplifying “On account of both the coinciding variables of explanatory variables and explained variable” P9 L25: Suggest rephrasing “less good” P10 L16: Suggest rephrasing “despite methane’s minor percentage of roughly 3% in the entire greenhouse gas exchange”. 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. P14 L11: Suggest rephrasing “mutual start” P15 L17: “proofs” -> “proves” Figures 1-9: The axis and legends provided seem small even when printed on a full page.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-10>, 2019.