We sincerely thank the reviewer for the comments, which helped us to substantially improve our manuscript. Please find the comments (black) and our reply (green) below.

The paper describes measurements of NEE, and the two respiratory fluxes on polygon tundra in the Siberian Arctic. They authors show that flux differences persist at the micro scale between the rim and the centre of the polygon. Although the work is generally okay, I think that there is somewhat of a missed opportunity here to use the eddy covariance data that are available for this site. As the authors say, the observations are well within the footprint of the EC system, so I am left wondering why these are not used to compare chamber NEE, or split to obtain EC GPP and Reco (line 116-119). Can the authors explain why they do not use this data? Was it not available, or did it give different results (then it should certainly be used!).

By the time the manuscript was submitted, the EC dataset was not available. In the meantime the EC data were published (Holl et al., 2018) and are compared to the chamber data in the revised manuscript.

Other comments

L 33. Please be a little more precise. The Hugelius paper mentions 1300 Pg with an uncertainty range of 1100 to 1500 Pg.

Since we only refer to the organic carbon content in the uppermost three meters of permafrost affected soils (not the total organic carbon in the permafrost region) the number given by Hugelius et al. (2014) is 1035 ± 150 Pg. We slightly modified the beginning of the sentence to 'About 1,000 Pg, which considers the uncertainty range.'

L38. A more up to date reference about Arctic Amplification would be good. SWIPA 2017 would be appropriate.

We fully agree and added the suggested as well as another reference (Taylor et al., 2013).

L43. It would be appropriate to cite here Parmentier, et al., (2011). Also because it is a site in the Siberian Arctic, as discussed below in l 44-55.

We have added this reference here.

L 66. It may be better to refer to different sensitivity, rather than to "react", which is a result of the sensitivity.

We substantially revised the introduction and the mentioned sentence was re-written. Furthermore, we now use "respond" instead of "react" throughout the manuscript.

L242. Fixing the Q10 is not necessarily the correct approach here. While it is difficult to estimate Rbase separately, just fixing it does not solve. It is important here to introduce the sensitivity to the definition of the Q10 as well as resulting uncertainty.

We have tried intensely to run the respiration models with a variable Q_{10} value. However, we decided to proceed with a fixed Q_{10} value because parameter estimation during fitting in MatLab did not converge to reasonable values for Q_{10} (around 1.5). We attribute this result to the relatively low number of samples available for fitting (about 150 samples per fitting) and to a tendency of the algorithm to overfit. The range of typical Q_{10} values of (soil) respiration has been shown to be rather

narrow across different biomes with 1.4 ± 0.1 (Mahecha et al., 2010). Moreover, following Runkle et al., (2013), for our site, Q_{10} has been estimated to lie within this range with 1.5 ± 0.3 by Runkle et al. (2013) using eddy covariance data. We saw the availability of a site-specific Q_{10} as an opportunity to proceed with a less complex model. In an effort to avoid overfitting and emphasize parsimony we used prior process knowledge to reduce model complexity.

L275. This is really where I would have expected the use of the eddy covariance data.

We have now compared the modelled NEE chamber data with the eddy covariance data and the comparison showed good correlation. However, the modelled chamber NEE tended to underestimate the highest and lowest NEE in comparison to modelled EC NEE. Possible reasons for this bias is part of the discussion section.

L350 and Fig 6. I am not particularly impressed by the model-data comparison. It looks as if the fluxes are severely overestimated. Can the authors not provide a simple 1:1 scatterplot to show how well the model does?

We replaced figures 6 and 7 with 1:1 scatterplots.

L 280 and further. This section is very descriptive and basically repeats the graphics. It may be useful to see if and how far this can be reduced and made more concise. It does not really read nicely.

This comment is similar to those from the other two reviews. Therefore, we substantially revised the results section focusing on the most important results.

Table 2 could include the Parmentier paper mentioned earlier.

We have changed this table and put a focus solely on CO_2 fluxes from either polygon rim or center microsites. Therefore, we have decided to not include the CO_2 fluxes reported from Parmentier et al. (2011) as it presents CO_2 fluxes of the polygonal tundra but not individual fluxes of rims and centres.

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