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Interactive comment on "Partitioning CO$_2$ net ecosystem exchange fluxes on the microsite scale in the Lena River Delta, Siberia" *by* Tim Eckhardt et al.

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Review of Eckhardt et al.

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!).

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.

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

L43. It would be appropriate to cite here Parmentier, et al., (2011). Âǎ Longer growing seasons do not increase net carbon uptake in the northeastern Siberian tundra. Journal of Geophysical Research - Biogeosciences. ÂǎISSNÂǎ2169-8953. Âǎ116(G04013).Âǎdoi: 10.1029/2011JG001653. Also because it is a site in the Siberian Arctic, as discussed below in l 44-55.

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

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.

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

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?

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.

Table 2 could include the Parmentier paper mentioned earlier.

C3