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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.

Anonymous Referee #2

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Overall we recommend major revisions to highlight how the microsite-scale understanding from this study can extend to a better understanding of Arctic C flux dynamics.

General: This manuscript investigates effects of small-scale polygon heterogeneity on autotrophic and heterotrophic CO2 fluxes. The primary finding is that NEE spatial heterogeneity was very large, with four times more net CO2 uptake at polygon rims compared to centers. The CO2 flux rates varied with hydrology of the two rim locations, in part because GPP was higher and Rh lower in polygon centers compared to rims. The amount of information presented in the manuscript is impressive and the full partitioning of net CO2 fluxes into autotrophic and heterotrophic components provides

insight to mechanisms of spatial CO2 flux variation. The manuscript is based on an impressive dataset and would be improved by streamlining the results and crafting a stronger narrative to highlight the implications of these results for understanding Arctic C fluxes. The results should be shortened, and repetition removed. A number of environmental details could be condensed, for example by showing daily averages that are more relevant to the scale of sampling and highlighting only the model output that adds understanding to the measured data, like relevant physiological parameters or cumulative flux estimates. The discussion should consider the implications of these small-scale dynamics for understanding Arctic CO2 fluxes. Table 2 is an attempt to provide this context however the comparison to other sites across the Arctic seems anecdotal and raises more questions than it answers. Instead, the authors might consider relating the small-scale heterogeneity to net CO2 flux dynamics measured at the scale of flux towers, commenting on the relative balance of wet/dry sites across the island, and expected future trajectories for the island/region. It might also be interesting to discuss the role of water table versus plant biomass or other physiological drivers of C balance. Figure 8 is a nice summary and could make an even greater statement about the ecosystem C balance by incorporating the soil C estimates and literature-based plant biomass. More details are provided below.

Abstract Line 21-22: 'Fluxes measured at the microscale were used to model NEE, GPP, Reco, RH, RA and NPP over the growing season.' Modeled at what scale? It's a little unclear whether the fluxes were scaled up to a larger area or to get cumulative growing season estimates.

Line 22: 'For the first time' – first time ever in all permafrost systems? Or for the Lena River Delta?

Line 31: 'lad' should be led

Line 31: It would be helpful to conclude the abstract with a few words on the implications of the work.

Introduction Since this manuscript focuses on wet vs dry microsites the introduction should guide the reader toward moisture effects on CO2 flux, and interactions between moisture and warming. As it stands, the introduction focuses overwhelmingly on warming responses, partly because there is more literature on warming effects which is in itself a useful thing to highlight.

Line 43: There may be more appropriate citations here that specifically address plant and nutrient responses. For example: (Elmendorf et al. 2012, Salmon et al. 2016)

Line 46: It would be useful to be a little more specific with this statement. There are a number of studies that suggest the annual CO2 budget of arctic tundra is a weak sink to source (Oechel et al. 2014, Celis et al. 2017, Euskirchen et al. 2017) but that there's substantial spatial variation that we don't fully understand (Belshe et al. 2013, Ueyama et al. 2013). The effects of shifting hydrology are also not well understood.

Line 47: see also (McGuire et al. 2018)

Line 59: The discussion of variation in total flux magnitude could be condensed in this paragraph. The uncertainty related to hydrologic changes should be discussed.

Line 64: specify: 'inorganic fluxes are minor in highly organic soils'

Line 66-67: state briefly why it's important that the component fluxes react differently to changing conditions

Line 85-87: This sentence is very dense and so specific that it doesn't sufficiently highlight the uncertainties. The phrasing is also a little confusing because an increase in Ra would lead to a relative decrease in Rh but not necessarily an absolute decrease in Rh. And that detail isn't necessarily essential to the introduction. It would be helpful to discuss a little more generally how warming and moisture interact and highlight some of the competing CO2 flux processes. For example: warming stimulates plant productivity and CO2 uptake while increasing moisture has been found to suppress or stimulate both GPP and Reco (Chivers et al. 2009, Zona et al. 2012, Mauritz et

al. 2017). Drainage and warmer surface soils could reduce microbial biomass (Frey et al. 2008) however the effects could vary throughout the soil profile with drainage potentially stimulating decomposition of deeper soil C (Natali et al. 2015).

Line 86-87: (Segal and Sullivan 2014) might be a helpful citation regarding the contributions of root/shoot respiration and Rh to Reco.

Study Site Line 101: delete 'of' in 'depths of down to 300 to 500m'

Methods Line 185: Heterotrophic respiration section: The discussion of trenching and isotope methods producing relatively similar estimates of Rh might be better placed here than in the introduction. The introduction can then instead focus more on the big picture and include less methodological detail. This is a useful approach for fitting and evaluating NEE and Reco chamber measurements.

Line 193-196: what exactly does this 2014-2015 trenching comparison test?

Line 216: what is meant by 'the flux curve was re-inspected to see if irregularities could be removed by adjusting the time series'? What gets adjusted?

Line 240-245: Does this mean the only flexible and estimable parameter was Rbase?

Results Throughout, specify figure panels, eg: line 280 soil temperature (figure 2a).

Line 278 – 279: This sentence is out of place since it's a rim/center comparison and the following descriptions are all seasonal. The logical flow would be nicer with a general seasonal description followed by a microsite comparison.

Line 286: how does total precip compare to longer-term means?

Line 293-296: Is this level of detail on PAR necessary? It is impossible to see this detail in the figure, and the measurements were taken every few days so the detailed diurnal variation is less important. The occurrence of polar day/night is important and was already mentioned in the methods. A figure of daily PAR might be more useful since it would presumably show the declining light conditions toward the end of the season.

This high-resolution figure could go in the supplement, if it's necessary to refer to it at some point.

Line 299-306: This information is given in the site description, and it is unclear whether it's considered a result from the study or whether this data was collected simply for greater site characterization. Collecting this information is a lot of work and the details could be retained and moved to a supplement, perhaps with depth-resolved figures or tables which provide added value to the data from this paper but are not central to the results.

Line 300: a reduction in %C with depth at both the center and rim? Is the reduction in depth similar or do they reduce by different amounts?

Line 308: Start with the larger picture to put the fluxes in context. It's much more interesting and easier to read a description of the magnitudes and patterns of NEE, GPP, Reco, Ra, Rh and differences between microsites. Which microsite has higher sink strength? How do seasonal NEE patterns differ between center and rim? How do the magnitudes of Reco and GPP compare between center and rim? Does one site have more seasonal variation than the other? The specific max or min values or periods only need to be highlighted if it serves to illustrate something important or remarkable.

Line 346: The water analysis deserves its own section. What about correlations between VWC and R fluxes on the rim?

Line 351: Remind the reader what the parameters represent or refer back to the equations.

Line 354: This sentence says that Pmax showed strong temporal variation at the polygon center (mean 250.7 +/- 101.9) what does the +/- represent? Spatial variation around the mean? Or temporal variation? Is it a range, standard error, standard deviation, confidence interval?

Line 355: This might not be the most informative comparison given the very different temporal patterns in Pmax. In Figure 5b it looks like the patterns differ between Rim and Center until mid-August and then converge. That matches the GPP pattern between the two sites, and interestingly it does not coincide with marked changes in temperature or moisture. Perhaps it does coincide with the onset of nights?

Line 364: Hm, it's interesting that center is fit better with surface temperatures. Could this be related to the low fluctuation in soil temperature and the fact that surface temperature captures some of the variation in Reco that is related to Ra?

Line 368: averaged or cumulative? Why compare means instead of cumulatives?

Line 368 -397: This section is confusing, it repeats many of the flux results described above. It is unclear what additional information is gained from this detailed description of modeled fluxes. What do we learn from the means of the modeled fluxes? Isn't the main purpose of modeling to calculate seasonal cumulative fluxes?

Line 399: The previous section can be reduced, with far less detailed description of the modeled flux fluctuations. That space can be used to expand upon this section because it's very interesting. Address each flux component in turn, and how they compare between the two sites, and what that means for the NEE of each site.

Discussion: Line 406: This is a nice study with results that are a valuable contribution in their own right. Saying 'this is the first' doesn't necessarily elevate the results. Instead the value of the results might be better emphasized by highlighting the general differences in environmental conditions and fluxes between center and rim, and the most interesting elements of the results (like the different GPP:Reco ratios).

Line 412-414: That is interesting. That should definitely be more visible in the presentation of the results.

Line 421: starting the sentence with something other than 'Solely' would be better.

Line 421-423: Out of how many studies compared? Are these all the known studies

from Polygonal tundra? Based on (Virkkala et al. 2017)? And 3/8 studies agreeing means that about half the sites show comparable Reco.

Line 430: this section is misnamed since the majority of the writing is not about environmental controls. Environmental controls are typically abiotic factors and a lot of what is discussed here are vegetation factors.

Line 454-455: lead this paragraph with Reco or Rh since they are directly related to SOM decomposition.

Line 467-468: remain consistent in terminology rather than switching between NEE and net CO2 uptake.

Line 466-469: These trends are not terribly convincing. It is possible that the eye sees declining NEE in the center because of the steep slope from June to September and a smaller decline on the rim because NEE is overall lower through the season. What is the main argument here?

Line 481: What about (Dorrepaal et al. 2009, Schuur et al. 2009, Nowinski et al. 2010, Hicks Pries et al. 2013)?

Line 481: Unclear what 'these estimates of Rh' refers to. The previously cited studies? The results of this study?

Line 515: what is meant by recycled? The CO2 is taken up from the water column by plants before it can escape into the atmosphere? Is the argument here that declining Ra and Reco with rising water table is actually the result of CO2 uptake from the water column and thus a lower flux of CO2 to the atmosphere?

Line 528 – 532: This would be a useful statement in the introduction too.

Line 541: Except that Ra might not actually be driven by WT? Because the Ra measurement might in fact be affected by CO2 recycling? And the center vs rim comparison certainly does not suggest lower Ra in wet areas.

Figures and Tables Table 2: This table is not particularly helpful since it is unclear whether this is an exhaustive summary of other locations, or how this site relates to these other studies.

Figure 1: Turn landsat website into a citation so that the link can be removed from the caption. Just to make the caption a little cleaner.

Figure 2: This figure is difficult to read because of so much overlapping data within single panels. It should be revised to highlight only the most important variables, group variables with more logic (for example why is soil temperature in the panel with precipitation and air temperature in a separate panel (c)? it might make more sense to pair air temperature with precipitation). Consider showing these data at a temporal frequency more relevant to the measurements. panel b, add a line at y=0 to make it easier to see the WT relative to the soil surface. panel d give y-axis a negative scale otherwise it doesn't really make sense. At line 829 'rim an center' has a typo, fix to 'rim and center'

Figure 3: Add label for Polygon Center on the top and Polygon Rim on the bottom to make the figure easier to read at a glance.

Figure 4: panel letters are missing? Caption is incorrect in the flux sequence. For Rh and GPP, if the regressions are non-significant then there shouldn't be a line. Add a vertical line at 0cm to make it easier to see water table above and below the surface. Was this analysis done as a mixed effects model? Including a plot random effect might strengthen some of the relationships because it would control for plot-level variation (eg: biomass differences). Is this analysis picking up seasonal fluctuation in temperature (and light?) that coincides with rainfall and higher water tables. Even if the analysis is picking up seasonal variation in light and temperature RA and GPP would be expected to behave similarly. This is interesting to discuss.

Figure 5a: why isn't there an alpha parameter for center and rim sites?

Figure 6 & 7: move to supplement.

Figure 8: Nice way to summarise results! This figure would be easier to interpret if the arrows scaled by the size of the flux. It takes quite a lot of staring at the figure before it becomes clear that NEE is $\sim$3 times greater in the center. The figure could be even bolder by including C stock estimates for the soil and plants. Consider integrating the soil C profile data. Are there plant biomass estimates from other studies on Samoylov Island? It might get complicated but if it works then that would be a really nice synthesis of the C flux and partial C budgets for the two microsites. Add a label or legend item for the permafrost table and water table.

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