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.

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.

Thank you for this general comment. We have substantially shortened the results section and removed repetitions to highlight the general differences of individual CO_2 between rim and center. The section presenting environmental details was shortened and revised accordingly. We furthermore added a comparison between the measured chamber data and EC data from the same study site (Holl et al., 2018). The comparison of CO_2 fluxes from this study with other chamber studies (Table 2) was substantially revised by focusing on chamber CO_2 fluxes from polygonal tundra. Furthermore, Figure 8 was improved by adding estimates of soil C and scaling the size of the arrows based on the CO_2 fluxes. Throughout the figures the colors of the single fluxes were synchronized.

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.

We have revised this sentence to "The measured fluxes on the microscale (1 m – 10 m) were used to model the NEE, GPP, R_{eco}, R_H, R_A and net ecosystem production (NPP) to determine cumulative growing season fluxes".

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

To the best of our knowledge this is the first time that the differing response of R_A and R_H CO_2 fluxes to hydrological conditions have been examined in permafrost systems. We have revised this sentence accordingly.

Line 31: 'lad' should be led

Changed accordingly.

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

We concluded the abstract with a summary of the implications of the current study.

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.

The introduction was substantially revised to consider to a greater extend changes in soil moisture in permafrost regions after warming and its effects on CO_2 fluxes in arctic ecosystems.

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)

The respective literature is cited here.

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.

Thank you for this important comment. In this part of the introduction we wanted to address the imbalance between the number of studies on CO_2 fluxes from Russian and Alaskan tundra ecosystems. However, this part of the introduction was deleted. The introduction now focus on partitioning CO_2 fluxes and the impact of environmental parameter on the individual fluxes rather than on CO_2 budgets.

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

Since we made substantial changes within this paragraph the suggested reference does not fit anymore.

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.

Thank you for this suggestion. We have made changes in the two sections between lines 44-67 focusing now on moisture effects on CO_2 fluxes. This includes a substantial reduction of the discussion on the total flux magnitude.

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

We have revised the wording in the paragraph. Furthermore, we added values of total inorganic C (TIC) content in the last paragraph of section 4.1 as these values (just 0.2% TIC in all depths at rim and center) show the minor importance of TIC here.

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

A sentence on the importance of flux partitioning was added and embedded into the section about temperature and moisture impacts on CO_2 fluxes.

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).

Thank you for this helpful comment. Substantial changes were made in this part of the introduction to point out the importance of warming and changes of soil moisture on the individual CO_2 fluxes. Here we have added the suggested research (Chivers et al. 2009, Zona et al. 2012, Natali et al. 2015, Mauritz et al. 2017)

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

In the discussion section root/shoot respiration and R_h to R_{eco} is considered and the respective citation is now also considered in this part of the introduction.

Study Site

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

Deleted.

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.

The discussion about methods to partition R_{eco} is included now into the method section 3.4 as suggested.

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

The clipping and trenching method is related to considerable disturbances to the ecosystem. It was shown for other ecosystems that the additional decomposition of dead roots after clipping and trenching, can lead to an overestimation of R_H (Subke et al., 2006). Therefore, we compared CO_2 fluxes from measurement plots that were trenched in 2014 with those that were trenched in 2015 to see if differences in R_H fluxes could be measured. We assumed that an additional decomposition of residual roots from plots trenched in 2014 would have ceased in 2015, one year after the treatment (following Diaz-Pines et al., 2010). The results have shown no significant differences between the plots that were trenched and clipped in different years. We have revised the respective section for clarity.

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?

The start time of the measurement was in some cases manually adjusted to remove irregularities of the flux curve. The start and end times were written down manually and were therefore partwise not identical to the real start of the flux measurement. We have revised this sentence and substituted 'adjusting the time series' with 'adjusting the flux calculation window'.

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

Yes.

Results

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

The figure panels were specified accordingly.

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.

To clearly distinguish between general seasonal descriptions and microsite comparison, we have placed the description of the soil temperature at the rim and center at the beginning of the next paragraph.

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

We have added a comparison with precipitation data between 2003 and 2010.

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.

The complete figure was revised (see response below) and smaller adjustments were made to the text.

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.

We have shortened this paragraph and removed parts of the results as they are not central to the chamber flux 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?

We have revised this sentence as the wording was not sufficient. The reduction of the total soil C content with depth was observed at both the center and rim, but more pronounced at the rim. Here the soil C content was half as much compared to the surface after 5 cm soil depth, while the C content at the center halved after 20 cm.

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.

We have revised the description of chamber flux results substantially and removed specific values. Instead, we put a focus on the description of the differences between the microsites and the seasonality of the single fluxes.

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

This is definitely one of the questions arising from the correlation of respiration fluxes with the water table at the polygon center. However, we haven't found a correlation between soil moisture and respiration fluxes at the polygon rim. Due to its elevation and the fast run-off of melt and precipitation water, the moisture regime at the polygon rim is completely different compared to the center. For instance, we discussed a 'recycling' of respired CO_2 due to its slow diffusion through the moss layer (caused by a submersion of mosses), as possible reason for a correlation between R_A fluxes and water table fluctuations at the polygon center. However, the moss layer at the polygon rim is not water-saturated and therefore respired CO_2 can diffuse much faster through the moss layer than at the center. Furthermore, the moisture differences at the polygon rim are rather small, with a range between 28 and 34 % VWC, which might be not enough to cause differences in respiration fluxes. We added this to the discussion section 5.3.

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

We have added references to the equations in brackets.

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?

It is a standard deviation of the daily averaged means and displays the temporal variation of the fitting parameter. We have revised the wording to clarify it.

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?

Thank you for this comment. Although P_{max} *is strongly reduced at the onset of polar night the steep decrease in* P_{max} *at the polygon center is likely caused by plant senescence. Runkle et al. (2013) related the decrease of* P_{max} *at the end of August to plant senescence and we think that this factor leads to the convergence of the patterns between the two microsites. As discussed in section 5.2, the* P_{max} *at the polygon rim seems to be less affected by plant senescence, most probably due to the resilience of mosses, which are dominant at this site.*

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?

Yes, we agree with the reviewer's interpretation. The higher sensitivity of R_{eco} fluxes at the polygon center to air/surface temperature is likely due to the sensitivity of R_A to changes in these temperature rather than changes in soil temperatures. At the polygon rim it is the other way round (the soil temperature describes the R_{eco} fluxes better than the surface temperature). This makes sense if the different contributions of R_H on R_{eco} fluxes are considered with contributions of >50% at the rim and <50% at the center. Giving this contribution, the R_{eco} fluxes at the center are stronger affected by surface/air temperature as the fluxes are mainly driven by R_A, while at the rim the fluxes are mainly driven by R_H and are therefore stronger affected by soil temperature. However, this holds not true for the modelled R_H fluxes as the R_H fluxes from the polygon center are better described by air than by soil temperature. Therefore, we cannot fully explain why the respiration fluxes are best described by air/surface temperatures at the polygon center. Both the soil temperature at polygon center and rim were measured at an adjacent polygon rim and center. The water table at the adjacent polygon center was permanently above the soil surface, while this was not the case at the polygon center where the flux measurements were conducted (see Fig 2). Therefore, there are most likely differences in soil temperatures in the upper soil layers between the polygon centers, which could lead to an attenuated fitting of the soil temperature with R_H fluxes at the center.

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

We do both a comparison of means and, later in the manuscript (section 4.4), a comparison of cumulative fluxes.

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?

We have streamlined this section and put a focus on seasonal cumulative fluxes (section 4.4 and Fig. 8). However, in Table 1 we still show the mean values and ranges of the modelled fluxes as we think that especially the ranges are in particular cases of interest to the reader.

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.

We have reduced the section 4.3 and adjusted 4.4 to show the differences of each flux component and their impact on differences in NEE fluxes between the microsites. As we discuss the impact of the single flux components on the net CO_2 fluxes and their drivers intensely in the next section, we haven't expanded the mentioned section here.

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).

We revised this paragraph by highlighting the general differences between polygon rim and center.

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

We have added an additional sentence to highlight the differences in R_{eco} fluxes at the two sites in the results section 4.2.

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

This paragraph was revised substantially. We now focus on the comparison of CO_2 fluxes from this study with other studies considering polygon rim and center microsites.

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.

We have changed the comparison of CO_2 fluxes from this study with other studies substantially. All the known chamber flux studies from polygon rims and centers are included (based on Virkkala et al., 2018).

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.

We have changed the title of this section to 'Factors controlling CO_2 fluxes'

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

Changed accordingly.

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

We have harmonized the terminology throughout the complete manuscript and only use NEE.

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?

We agree, that these trends are not convincing when considering the complete measurement period. However, by zooming into the fluxes during September, the trends are much clearer with a significant increase of NEE at the polygon rim and a slight decline of NEE at the polygon center. We discuss in section 5.2 that this increase might be assigned to the dense moss cover at the polygon rim, which might show low photosynthesis rates due to light stress during times of high PAR and desiccation (Murray et al., 1993, Zona et al., 2011). This interpretation is in accordance with the observation of rising NEE at the polygon rim when the drier period ended and PAR values were decreasing towards the end of the season (see Fig. 2).

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

We didn't discuss the mentioned studies since they haven't estimated R_H fluxes over the growing season under in situ *conditions. However, the wording was misleading and was revised to '(…) a few studies have estimated R_H fluxes from arctic tundra ecosystems over a growing season under* in situ *conditions'.*

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

We have revised the wording to '(…) differences in R_H fluxes between these estimates and those presented in 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?

Yes, an uptake of CO_2 from the water column by plants could serve as an explanation for the relationship between water table fluctuations and R_A fluxes. The diffusion velocity through water-saturated soils is distinctly slower compared to well-aerated soils (Frank et al., 1996). Therefore, it seems plausible that a 'recycle' process as described by Liebner et al. (2011) gains more importance and lead to lower release of CO_2 by R_A. This process would affect R_{eco}, not only R_A fluxes. However, the relationship between R_H fluxes and water table fluctuations might be missed due to the absence of photosynthetic active biomass in the measurement plots. We have revised this paragraph in section 5.3 substantially to clearly explain this effect on respiration fluxes.

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

Yes, while we have made substantial changes of the introduction with changing the focus from CO_2 budgets towards a focus on the impact of environmental and vegetation factors on single CO_2 fluxes, we have also added a sentence about the necessity of studying the impact of hydrological regimes on R_A fluxes.

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.

Yes, it might be possible that just the release of CO_2 by R_A is driven by WT and not the R_A flux itself. Therefore, we revised this sentence accordingly. However, we think that there is a lower release of CO_2 by R_A from the polygon center compared to the rim. Although the integrated fluxes are almost the same one has to consider the differences in GPP between the sites as photosynthesis is the source of R_A. The GPP:R_A ratio at the polygon center is twice as high as at rim (10.5 and 5.1, respectively), which shows that about half as much CO_2 is released by R_A at the center compared to the rim at similar GPP rates. These findings are added in section 5.3 to illustrate the difference in R_A fluxes from rim and center. Furthermore, the GPP and R_A fluxes at the rim are linearly correlated ($R^2 = 0.48$, $p < 0.05$), with higher R_A during times of high GPP. This trend was not observed at the center ($R^2 = 0.01$, $p > 0.05$). This indicates that there certainly is a lower release of CO_2 by R_A 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.

Thank you for this comment. We have made substantial changes to the comparison between CO_2 fluxes from the current and previous studies on chamber CO_2 fluxes from polygonal tundra sites. According to Virkkala et al., (2018), CO_2 fluxes on the chamber scale (1 – 10 m) from polygonal tundra were only reported from Barrow and the Lena River Delta. We have also emphasized this fact in the introduction.

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.

Changed.

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.

We have revised the figure. We adjusted the temporal frequency to daily means instead of half-hourly means. Furthermore, we have added lines at y=0 if necessary and gave a negative scale for the panel with thaw depths. The precipitation data are now presented in an own panel.

At line 829 'rim an center' has a typo, fix to 'rim and center'

Changed.

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.

We have added the labels. Furthermore, we have changed the colors of the single fluxes to synchronize them with the colors of the single fluxes of Fig. 8.

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.

Thank you for this comment. We have removed the regression line for insignificant fluxes and added a vertical line at 0 cm. Unfortunately, we were not able to control for plot-level variation as the R_A fluxes were calculated from fluxes from different measurement plots and not measured directly. Furthermore, there are no estimates of biomass for each plot. The analysis might pick up seasonal variation in radiation and temperature, but we estimate that the effect on the regression itself is low as the water table reacts rather slow to variation of both temperature and light. Furthermore, we discuss the different behavior of GPP and R_A fluxes to changes of the mentioned parameter in the section 5.3.

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

The values for the initial canopy quantum efficiency α were obtained from modelled fluxes of the Eddy Covariance measurement system (Holl et al., 2018). The footprint of the EC system contains both polygon rims and centers. Therefore, the same value of the α parameter was used for both microsites. We have added this in section 3.6.

Figure 6 & 7: move to supplement.

According to the suggestions of reviewer #1, the presentation of these data were revised substantially but still included in the main part of the manuscript.

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 ~ 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.

The size of the arrows were scaled to the size of the flux and legend items for the water table and the thaw depth were added. Furthermore, we have added estimates of SOC for both microsites. Estimates *of aboveground biomass for both microsites are lacking. There are estimates for a polygon rim and center from Samoylov Island from the literature (see Zhang et al., 2012), but they differ distinctly from what we have found at the study site (e.g. total aboveground biomass is higher at the rim than at the center). Therefore, we have decided not to include these estimates here.*

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