

Comments from the reviewers are in black and responses from the authors are in blue.

Authors' responses to the Reviewer #3

The paper describes an interesting field experiment with interesting and relevant results, but the structure is difficult to follow, which makes the manuscript as a whole a struggle to read. The use of poorly-described acronyms persists, it seems as if the authors have invented their own language for describing the field site. The English usage is fine, but the results and discussion section badly needs restructuring for clarity. It just says things, often without support, and certainly with little internal structure to help the reader. Simply replacing this section with separate results and discussion sections – and (please) removing all extraneous information and assumptions (the list below is partial) would cut many pages from the manuscript and make it a pleasure to read and a valuable contribution to this understudied field.

The authors appreciate valuable comments from the reviewer #3. All the aspects that were pointed out have been edited. For detailed information, please see answers below.

On line 36, low air temperature does not necessarily imply C sink and in the following sentence the carbon accumulation is partly attributable to the permafrost itself making C (largely) unavailable for respiration. It is corrected as the reviewer suggested, by adding “that inhibit the mineralization of soil carbon”.

The end of the first paragraph could use references beyond those of Schuur et al. (not that these aren't good references). Some references are added, such as “(Abbott et al., 2016; Koven et al., 2011; Schuur et al., 2008, 2015)”

With respect to the statement on line 50 note <http://www.pnas.org/content/112/9/2788>.short changed. This reference is added.

One may argue with the statement ‘The influence of temperature on CO₂ cycling processes in the Arctic has been well studied (Belshe et al., 2013)’ given uncertainties with respect to permafrost. It is corrected for clarity.

With respect to the statement on line 99, note <http://iopscience.iop.org/article/10.1088/1748-9326/9/8/085004/meta> to the extent that the observations noted in this paper can be used as a surrogate for long-term ecosystem changes. This sentence has been removed to avoid confusion.

Awkward wording on line 112: As a continuation of analysis hydrological manipulation. It is corrected as the reviewer suggested, by removing “analysis” from the sentence.

The methods section is written nicely, but what constitutes WTD low? A sentence, “Plots were classified as ‘dry’ when the average WTD of the growing season was lower than -10 cm.” is added and this information was added in Table 2.

Subheader 2.2 should be plural. OK.

If the UGGA was used why are only CO₂ flux data presented? The authors focused on the drainage effects on CO₂ fluxes in this manuscript. CH₄ fluxes were measured in parallel, but the presentation of these results in combination with the CO₂ results would add too much material for a single manuscript. Thus, the CH₄ results will be presented in a separate paper.

Space between 3 and sigma on line 200. OK.

Personally I find it interesting if not a bit inefficient to parameterize a model usef for remote sensing for surface fluxes. From this standpoint, which references suggest the prescribed temperature parameters? Please use the multiplication sign or nothing at all rather than the dot which may be taken to mean the dot product. Also, how were the MODIS data used? The pixel may be too big to see relevant effects and at any rate to properly use modis the values around any given point should be averaged with it due to uncertainties in reshaping the ellipsoidal return function to a square(ish) pixel.

We took T_{opt} of 20 °C from the literature (Mahadevan *et al.*, 2008), and this value appeared reasonable for the site from plots of T_a vs. GPP.

MODIS MOD13A1 observations of EVI at a 500m, 16-day resolution were downloaded from <ftp://ladsweb.nascom.nasa.gov/allData/5/MOD13A1/2014/> (Solano *et al.*, 2010).

Observations were only used for pixels and time-periods which had been flagged as being of the best quality in both MOD13 and in the corresponding MOD09 surface reflectance observations. Both spatial and temporal gap-filling approaches were then used to create a clean and gap-free dataset at a 1 × 1 km resolution, which accounts for the issues raised by the reviewer.

From the cleaned dataset, a pixel was selected on the basis of its coordinates: the central latitude and longitude of study sites was located within the selected pixel. It was found that the time-series of EVI values at this pixel agreed well with EVI time-series from neighboring, relatively unmixed and terrestrial pixels. So as to reduce variability in parameters or outputs arising from EVI alone, and to mitigate risks associated with using pixels with high water fractions which would concurrently have dampened seasonal variability in EVI, the same EVI values were applied to generate estimates of GPP at all chamber locations.

‘Eriophorum a.’ is uncommon usage. Use instead ‘E. angustifolium’. These and other errors make me question if all of the coauthors contributed to this version of the manuscript. It is corrected throughout the manuscript.

Please quantify ‘EriophorumShrub’ and ‘CarexShrub’. An explanation may appear possibly around line 405 but it’s certainly not quantified in the text. [This information is added to Table 2 and in the text.](#)

I’m not sure that bootstrapping is the best way to estimate parameter uncertainty in this case. I recommend [noting http://www.fasebj.org/content/1/5/365.full.pdf](http://www.fasebj.org/content/1/5/365.full.pdf) (<http://www.fasebj.org/content/1/5/365.short>). [The method suggested by the reviewer would be a very good strategy for acquiring accurate uncertainty ranges of each parameter. However, the main purpose of the bootstrapping used within the context of our study was to get uncertainty ranges of interpolated fluxes instead of getting those of each parameter. For this purpose, the authors believe that bootstrapping approach was adequate.](#)

Personally I feel that the results and discussion section would read much more nicely if separated into results then discussion. Section 3.2.2 is a particularly egregious example of a section that is difficult to read in the context of a long combined results and discussion section that should be restructured for clarity. [The first version of this manuscript that was submitted for peer-review in Biogeosciences had separated Results and Discussion sections. When revising the manuscript, we decided to merge the two sections in order to avoid repetitions, which were pointed out by one of the reviewers. Although the reviewer #3 mentioned that the merged one can be a better option, we decided to stick to this manuscript structure, but restructure certain sections of ‘Results & Discussion’ as recommended by the reviewer. Accordingly, the results and discussion are not separated, but they have been substantially modified for better readability.](#)

The paragraph beginning line 355 says little and is not supported by data. [This paragraph as well as the whole Section 3.2.2. has been corrected.](#)

The paragraph on line 360 is expository and belongs in the introduction or elsewhere. [This has been corrected as well.](#)

The discussion of C:N ratios on line 364 was somewhat surprising given the topic of section 3.2.2: soil temperature and TD effects on CO₂ fluxes. [This section is substantially changed, focusing on soil temperature and TD effects on CO₂ fluxes.](#)

The paragraph beginning on line 403 is confusing in part because of the insistence on using poorly defined acronyms like ‘control_low’ which probably reflects internal dialogue about these treatments rather than something that a reader can hope to understand. A figure might help, or a table of abbreviations. [Abbreviations are added in Table 2, and some ambiguous sentences have been edited for clarity.](#)

The statement on line 426 is qualitative. [Statistical analysis results are added: “\(GPP: \$F = 11.23\$, \$P < 0.001\$, \$R_{eco}\$: \$F = 3.63\$, \$P < 0.01\$ \)”.](#)

What is a ‘first vegetation effect’ on line 428? [It is corrected to “One of the vegetation effects”.](#)

What does ‘stabilize’ mean in the context of line 444. Do plants really ever stabilize? It is changed to “E. angustifolium is fully replaced by Carex sp. and shrubs” and “resistant to disturbances” in the line #424 and #429.

The statement on line 445 doesn’t make sense. Note also this notion of ‘stabilization on line 449 which continues to not make sense. This paragraph has been substantially changed as well.

Regarding litter added to the soil on line 474, can you be sure? What if shrubs have higher leaf area index? It is corrected to “the proportion of litter added to the soil will decrease accordingly”.

The statement on line 504 doesn’t make sense. It is corrected to “the replacement of E. angustifolium by Carex sp., more aerobic conditions, and increased soil surface temperature all weakened CO₂ uptake and increased CO₂ emission”.

From the conclusion, the results are nice and simple to follow. It’s a shame that the results and discussion section doesn’t reflect this. The first couple paragraphs should be the beginning of a restructured discussion section and the last paragraph of the conclusions should serve as a succinct conclusions section. The results and discussion are substantially changed for clarity.

Table 1 is nice. OK.

What does anything in Tables 4 and 5 mean? The authors believe that Table 4 includes important information of this manuscript. Thus, it is left as it is. Table 5 is moved to Supplementary information.

Red and green should not be used simultaneously if avoidable (and it’s certainly avoidable) in figures 4, 6, 9, and S1. The color palette is changed as the reviewer suggested.

Figure 9 isn’t particularly revealing, it may make sense to study the relationship between changes in air pressure and flux. This plot is changed with X-axis with changes in air pressure and Y-axis with NEE.

References

Solano, R., Didan, K., Jacobson, A., and Huete, A.: MODIS Vegetation Indices (MOD13) C5 User’s Guide, Terrestrial Biophysics and Remote Sensing Lab, The University of Arizona, available at: <http://www.ctahr.hawaii.edu/grem/modis-ug.pdf>, 2010.