

This paper by Skeeter et al. looked at vegetation and environmental conditions influencing greenhouse gas exchange in a drained lake basin in the Western Canadian Arctic. I enjoyed the opportunity to review this paper and I thank the authors for what is a well written paper overall (but with some tweaks needed). Given the lack of studies outside of the Barrow Peninsula, is a worthy addition to the literature.

I will add however, I apologise, I am not an expert on the eddy covariance data cleaning, gapfilling and analyses and I am therefore unable to comment fully on those sections.

Introduction: I find this section well written if rather short. I think a little more context could be given for the reader. You could include more information from non-DLB work but still relevant arctic tundra literature.

[Responses]

We added more context on why thermokarst landscapes are important (spatial extent, significant soil carbon storage) to the first paragraph [Lines 32-34]. Further we added another paragraph on Arctic carbon budgets [Lines 40-49]. Including a review of pan-arctic NEE chamber studies by Virkkala et al. 2017.

Line 54: NEE should be ER – GPP.

[Responses]

We corrected the equation to $NEE = ER - GPP$

Line 85: Can you include somewhere the dominant species found in each vegetation class. It would be interesting and useful to know what sort of sedge dominated the sedge class – is it *Carex aquatilis* or *Eriophorum angustifolium* for example?

[Responses]

We added a sentence to mention the specific species [Lines 90-91]. This information was partly available in Table 1. We made the table more detailed to include the dominant species present within each class/subclass where known/applicable.

Line 100: Completely an aside but COOL!

- Yes, it was an amazing thing to see.

Section 2.3: I would remove any mention of N₂O – you don't present the data, so it is unnecessary.

[Responses]

We removed mentions of N₂O throughout the manuscript.

Line 150: Can you be more explicit with how many collars were used? 2 per site – 10 sites total? I know the main focus of this paper is not the collars, but I'm not sure if a replicate of 2 per vegetation type over an 11-day period is very representative.

[Responses]

We updated the text to be more specific about numbers of replications per vegetation type [Lines 176-177]. There were 19 collars and 10 sites. The bare ground site only had 1 collar, thus making 2 per sites for the rest. The number of collars that could be shipped in via helicopter were limited and there was a high amount of heterogeneity in soil and vegetation characteristics within the basin. The chamber study was designed to better understand the relationship between soil properties and carbon loss in a situation where permafrost had aggraded within the lake bed to potentially protect 'old' carbon from mineralization and accumulate 'new' carbon since the lake drained. We expected saturated soils (where there were wet sedges) to have higher organic carbon accumulation and be dominated by anaerobic respiration processes, which was interesting to us, therefore we chose two different wet sites populated by sedges. We also focused on different statures of will (low, tall and dense) as we expected different amounts of snow accumulation and different impacts on permafrost at these sites.

Line 152: How soon after installation were the collars fluxed for the first time?

[Responses]

Collars were installed on July 11th and first set of measurements was taken on July 12th, so about 24h.

Line 154: Why not use a clear chamber so you could get GPP then cover with a dark sleeve in order to get ER?

[Responses]

We appreciate the comment. The chosen chambers were not designed for NEE measurements. Although measurements of GPP would have been informative, logistics limited us to use the existing collars, and the number of measurements we could make.

Section 2.4 (and subsequently Appendix A): Unfortunately, I do not have the expertise in these methods so I do not feel comfortable commenting on it in a reviewer context.

Section 3: I think it would be better to separate this section out into Results and Discussion rather than combine them. As it stands, it's quite hard to follow.

We have added a discussion (section 4) and rewrote the results to solely contain the objectively retrieved data, so we hope the manuscript to be more straightforward and easier to follow with separate "Results" and "Discussion" sections.

Line 241: You only mentioned thaw depth twice? Why not measure it on each day chambers were used?

[Responses]

Thaw depth tends to increase over time but at different rates at different locations within the basin as a result of varying soil and surface properties. We measured thaw depth at the start and end of the measurement period to highlight these differences rather than develop a variable that could be related to the fluxes. In past studies we found that day to day variations in respiration correlate best with near surface soil temperature and moisture rather than thaw depth while spatial variations in average fluxes can sometimes correlate to max thaw depth. In the revised manuscript we refer to thaw depth more and use it to compare Illisarvik to Katyk Line 384

Section 3.1: I think this section needs an overhaul unfortunately. Many of the sentences do not make sense in their current format. For example: Lines 254-257: 'NEE was greater than (ie. Less carbon uptake) 255 EC observations of from four wetter, sedge dominated DTLB, where peak season NEE was -2.5 g C-CO₂ m⁻² d⁻¹, ER (1.5 g C-CO₂ m⁻² d⁻¹) was lower than at Illisarvik while GPP (4.0 g C-CO₂ m⁻² d⁻¹) was slightly higher (Zona et al., 2010).'

[Responses]

The entire section has been rewritten and the references to other work were moved out from the results to the discussion section.

I also think it might be useful to separate the EC results and the chamber results into subsections. By referring your measured values to other studies in the results, it makes it hard to follow for the reader.

[Responses]

We agree and have separated the EC and chamber results into Sections 3.1 and 3.2

Further, by combining the results, there is a lack of discussing the results (for example, it feels like only section 3.4 is really doing this). It sadly reads as a lot of results statements and then suddenly we are at the conclusion.

[Responses]

As mentioned above, in the revised manuscript, we added a separate "discussion" section (Section 4).

Line 273: Why compare methane to ER here?

[Responses]

We thought it is relevant to contextualize the differences. Methane emissions are far more spatially variable than ecosystem respiration. We revised the sentence to get rid of the comparison of the magnitude because that is less relevant, but left the point about spatial variability being enhanced [Line 316]. We think it is an important finding to show that NME is more influenced by spatial heterogeneity than ER.

Line 286: Although discrepancies do occur between upscaling chamber measurements and EC measurements – some studies have done it successfully and I think it would be good to include here as a caveat;

- **Budishev et al. 2014: Evaluation of a plot-scale methane emission model using eddy covariance observations and footprint modelling. Biogeosciences 11. 4651-4664**
- **Zhang et al. 2012: Upscaling methane fluxes from closed chambers to eddy covariance based on a permafrost integrated model. Global Change Biology, 18, 1428-1440.**
- **Davidson et al. 2017. Upscaling CH₄ fluxes using high-resolution imagery in Arctic Tundra Ecosystems. Remote Sensing, 9, 1227; doi:10.3390/rs9121227**
- **Sachs et al. 2010 Environmental controls on CH₄ emissions from polygonal tundra on the microsite scale in the Lena river delta, Siberia. Global Change Biology, 16, 3096-3110**

[Responses]

We reviewed this literature and added it to the newly separated “discussion” section [Lines 417-418]. We also decided to use the footprint weighted upscaling method discussed in Budishev et al. (2014) for the chamber upscaling [Line 207-208], but it did not make an appreciable difference in the upscaled chamber ER or NME.

I think more discussion of the results in the context of other GHG literature from other tundra ecosites would be useful. Although this study is focused on drained lake basins, the results are comparable to wet-sedge dominated tundra landscapes. I feel this would be a good addition and strengthen what is already a useful paper.

[Responses]

We added section 4.1 where we discuss NEE and NME observations at Illisarvik relative to natural shrub vs. sedge-dominated DTLB to highlight the differences among these environments rather than attempt to fully contrast Illisarvik to a myriad of arctic tundra types/sites. These comparisons are always challenging given different years, time periods within a year, instrumentation, and data presentation. However, we believe we make a strong argument that shrub vs. sedge-dominated DTLB have the potential to differ and Illisarvik differs in particular from all other DTLB in its low methane emissions. (Table 4). We now further highlight the important implications of vegetation succession on CO₂ and CH₄ fluxes at our site in the discussion section 4.3 “Future Trajectories”. This is one of the key messages associated with DTLBs – they undergo relatively rapid vegetation change over a number of decades that will influence their C budgets.

Figure 2a: Could you change the colour of the T_a line? Red on orange is difficult to read.

[Responses]

Agreed. We have changed the bar colour, setting the orange to grey to make it easier to distinguish.

Figure 3: I will leave this up to the author’s discretion, but I wonder if this figure (and in fact, all figures) would benefit from having a plain white background. I find all the lines distracting. Especially when other lines are being used to annotate.

[Responses]

We agree the grid is distracting for Figure 3 and removed it, but left grids in all the other figures.

Figure 4c: Please use another three colours for Shrub. It is confusing that they are the same colour as VPD on the left-hand panel.

[Responses]

We changed the colour scheme to address the concern.

Figure 4 and 5: I think these figures would benefit from having a title for each panel – it was not clear to me initially the difference between Figure 5a and b. I think just putting VWC above left hand panel and T_s above right hand panel, this would make it much clearer.

[Responses]

We added subtitles to all panels in Figures 4 and 5.

Tables: Caption should go above the Tables.

[Responses]

Captions were all moved above Tables.

I think a table including the dominant vegetation species for each class would be super useful for the reader.

[Responses]

We updated Table 1 to be more specific and included additional information where known/applicable.