

## ***Interactive comment on “The effect of a permafrost disturbance on growing-season carbon-dioxide fluxes in a high Arctic tundra ecosystem” by A. E. Cassidy et al.***

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The authors have placed two short-term (32-days) eddy covariance flux towers on two opposing sides of a thermokarst feature, a retrogressive thaw slump (RTS) and make a pairwise comparison of the CO<sub>2</sub> fluxes from the disturbed (RTS) versus the undisturbed (i.e. surrounding high Arctic tundra). Since eddy covariance flux footprints vary with wind direction, stability and time, they made a careful assessment of the percentage of each footprint representing disturbed and undisturbed tundra, and extracted the respective fluxes by unmixing the signal according to percentage footprint coverage. For comparison they also carried out conventional static flux chamber measurements.

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On the one side the effects of such thermokarst features on the local and regional CO<sub>2</sub> budget is only poorly known and hence this quantitative assessment provides an excellent insight into the quantitative effects during a 32-days case study in the short summer season. On the other hand side the authors pretty much use a state-of-the-art combination of eddy covariance flux measurements and footprint unmixing via a reasonable footprint simulation model, to obtain their conclusions.

This is a very carefully prepared, nicely written manuscript, and it is difficult not to become too picky because most aspects are nicely treated and covered. My comments are thus rather a suggestion for further improvement of the manuscript than real critique of concern, thus only minor revisions suggested:

## 1. Abstract

For me as a reader the keyword would have been “thermokarst”, but this does not appear in the abstract. I wonder why – it is nicely introduced in the Introduction, but I definitely would argue that looking at a retrogressive thaw slump is one form of thermokarst and should definitely be put in context already in the Abstract.

The “increase in frequency and magnitude” (line 5/19782) is never substantiated in the text (or I did not find it). The information I could find is the 0.4% of area given as a personal communication on line 1/19798. This is not sufficient to prominently place the “increase in frequency and magnitude”, so please either substantiate this in the text or remove this from the abstract. It is important that we do not sell expectations as facts in scientific papers.

Rounding the numbers: on lines 13/14 you give the uptake as 3.84 gC/m<sup>2</sup> and 12.48 gC/m<sup>2</sup>, which means  $\pm 0.01$  gC/m<sup>2</sup>. This accuracy is unrealistic, so please round to 1 decimal (or provide some evidence that the uncertainty is really as good as  $\pm 0.01$  gC/m<sup>2</sup>).

## 2. Units

You present EC fluxes in  $\text{gC}/\text{m}^2$ , but chamber fluxes in  $\mu\text{mol}/\text{m}^2/\text{s}$ . This is confusing, since the physical principle behind the measurements is the same if you use optical absorption spectroscopy: you measure the absorption of light which is proportional to  $\mu\text{mol}/\text{m}^3$ , and then convert to e.g. a mass flux. My suggestion (since at the end we expect that the two fluxes should be comparable): homogenise the reporting to either report mass fluxes or mole fluxes, but not a mixture of both.

## 3. References

I am not really into retrogressive thaw slumps, but what surprises me is that none of the effects on the aquatic systems is mentioned. Please have a look at the literature below that I extracted from the Toolik LTER publication database (without reading everything in detail) and consider whether this is not also an aspect to mention at your site: how the thaw slump and export of the silty material downriver may affect the aquatic ecosystems.

## 4. Annual effects

At the end of the discussion (lines 3–13/19799) you quickly scratch the surface of longer-term fluxes. Given that you have only 32 days of measurements it does not appear to be defensible to relate this to longer term effects, rather consider this a case study and remove that paragraph. Maybe lines 3–8 can be kept (but then it would be good to know more about “logistic constraints” – a typical constraint is that you want to go on vacation and hence don’t have the time to continue measuring, but it could

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also be something that is more informative to the reader; maybe you want to say that the systems could not have been left alone due to the activity of the RTS, and hence you ran out of personel funding to service the site? Maybe there is a justification to say that one learns most during that part of the season and not during the later part of the season anyway?).

## 5. Appendix

I find an appendix that only has 10 lines of text rather special. The aspect covered in the appendix is a component covered by the text and relevant to the story, so it is not really an appendix. Why not include that paragraph into material and methods? The style is different and needs some change though, but depending on what other reviewers say I would definitely include Fig. A1 as a normal figure into the manuscript.

## DETAILS

19783/7–8: this sentence does not sound right to me, like lacking the main verb: “Current estimates are likely an underestimation, by as much as a factor of two, **due to difficulties measuring and uncertainty** regarding carbon storage in cryoturbated soils”.

19784/13: maybe the Mbufong et al. (2014) paper has a relation to this aspect.

19786/5–8: this is not quite correct; please double-check with Eugster et al. (2005), but I think the implicit assumption you make here is not the 24 h of sunlight but that in your case (high Arctic) the sun is relatively high above the horizon even at midnight, were in the low Arctic where we measured, the conditions at night are still more night-time like despite the sun being above the horizon. I think a more clear rephrasing will help

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the readers who are not familiar with the differences between high Arctic and low Arctic conditions.

19789/14: I think that the correct term would rather be “partition” or “unmix” than “isolate”. Moreover, I am not clear what you mean with “pure fluxes”. Maybe component fluxes?

19790/16: how exactly did you define “soil surface”, namely when mosses were present?

19790/24: there are still two types of non-steady state chambers: vented ones and unvented ones. Can you be more specific which kind you used?

19791/20: not considering the humidity effects leads to substantial overestimation of photosyntheses, see Pérez-Priego et al. (2015). Why did you not correct for humidity effects? Is there an argument to not do it?

19791/26: this is backwards! Why not write  $GPP = R_e - NEE$  which looks more straight forward to the reader?

19793/23: over which period were the 21 m/s determined? Raw 20 Hz spike, or e.g. a 30-min average?

19794/19: rounding to 1 decimal seems more appropriate

19794/21: Fig. 4 does not show any correlation. Even the comparison between Fig. 4 and Fig. 3 does not allow for the assessment of correlations (this would be unpaired sample comparison, whereas correlation assumes paired samples). So either remove the reference to Fig. 4 or add a graph showing the correlation.

Fig. 1: if the view is really towards SW, then this valley rather looks like running in WSW–ENE direction. Please double-check (the Google Earth images are not good enough to allow to see this at the coordinates you specify for the site).

Fig. 6: can you add uncertainty bars to the chamber flux measurements? Also for



the EC measurements some indication on the uncertainty (either bars of symbol size) would allow for a more critical visual assessment by the reader.

Overall a really interesting and relevant paper!

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