Biogeosciences Discuss., 12, 19781-19817, 2015

Authors Response to Referee Comments

Reviewer #1 (W. Eugster)

Comment 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 sufficent 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/m2 and 12.48 gC/m2, which means ±0.01 gC/m2. This accuracy is unrealistic, so please round to 1 decimal (or provide some evidence that the uncertainty is really as good as ±0.01 gC/m2).

Responses:

➔ We have altered the abstract text to include the term thermokarst (line 27) as follows "*Thermokarst and permafrost disturbances, especially active layer detachments and retrogressive thaw slumps, are present across the Fosheim Peninsula, Ellesmere Island, Canada*." We have also included the keyword thermokarst in the introduction (lines 76-79): "*These disturbances are usually linked to thermokarst and affect soil temperature, water quality and soil nutrients (Mackay, 1970, Lamoureux and Lafrenière, 2009, Lantz et al., 2009, Kokelj and Lewkowicz, 1998, Kokelj and Lewkowicz, 1999).*"

➔ The increase in frequency and magnitude refers to multiple publications (Lewkowicz, 1990; Lewkowicz and Harris, 2005) analyzing permafrost disturbances on the Fosheim Peninsula (line 589-592). We now mention this in the discussion: "*The frequency and magnitude of these land surface disturbances appear to be increasing across the Fosheim Peninsula (and elsewhere in the Arctic) as a result of the warming climate, thus exacerbating these impacts (Lewkowicz, 1990; Lewkowicz and Harris, 2005b; Lantz and Kokelj, 2008).*" In addition, one of the authors (GHRH) has observed a substantial increase in the frequency and magnitude of disturbance since the early 1990s in this study area. This has been confirmed by WH Pollard, who visits this region annually (pers. comm., 2014).

➔ Values have been rounded to 3.8 g C m^{-2} and 12.5 g C m^{-2} (line 36) (over the measurement period).

Comment 2: Units
You present EC fluxes in gC/m2, but chamber fluxes in µmol/m2/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 µmol/m3, 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.

Response: When individual (e.g. hourly) values are compared (as in Figure 6), we use µmol m^{-2} s^{-1}. However, to summarize our EC measurements over the entire season (and component periods), we chose g C m^{-2} day^{-1}. This has been consistently applied across the manuscript and is commonly done in the literature.

Comment 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 extraced 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.

Response: We now mention the hydrologic impacts (on downstream water quality) of these systems as studied in the Canadian High Arctic in the introduction (lines 76-79) and in the discussion (lines 585-589). As our site is within the High Arctic, we added references that are located within this region.
In the introduction, we state "*These disturbances are usually linked to thermokarst and affect soil temperature, water quality and soil nutrients (Mackay, 1970, Lamoureux and Lafrenière, 2009, Lantz et al., 2009, Kokelj and Lewkowicz, 1998, Kokelj and Lewkowicz, 1999)*".
In the discussion, we mention "*Although the landscape area directly impacted by disturbance at this time is minimal, indirect impacts such as the lateral export of dissolved and particulate organic matter (hence, carbon) through streams and the hydrologic network are also important (Lamoureux and Lafrenière, 2009, Kokelj and Lewkowicz, 1998, Kokelj and Lewkowicz, 1999).*"

Comment 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 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?).

Response:

➔ We have added in details regarding the site logistics (lines 618-621): "*Due to*

the dynamic nature of these disturbances, the site could not be left alone as personnel were needed monitor the slide edge location and adjust the equipment as needed. Leaving the site unmanned would have put the equipment at risk."
We mention the annual trends found at other Arctic sites while acknowledging the limitations of our study (lines 621-630), as periods outside of our measurement period may also be important, and could be explored in future studies.

Comment 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.

Response: Thank you for this suggestion. We have followed the suggestion and incorporated the Appendix material 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".

Response: This sentence (lines 64-67) has been reworded as follows
"*Measurement difficulties and uncertainty regarding carbon storage in cryoturbated soils may result in an underestimation of current estimates, by as much as a factor of two (Hugelius et al., 2013)*."

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

Response: Thank you for this reference. We have incorporated these findings (lines 104-105) into this paragraph as follows: "*Variability may be explained by nutrient availability, substrate quantity and soil organic matter (Mbufong et al., 2014)*."

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

Response: This section has been modified to reflect the light conditions over 24 hour daylight as follows (lines 125-128)
➔ "*Further, with 24 hours of daylight during which the sun remains relatively high above the horizon, the usual partitioning methods for EC measurements into component fluxes (Reichstein et al., 2012) are not applicable, as they rely on nighttime measurements, or measurement during low light 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?

Response: We have replaced "isolate" with "partition" and "pure fluxes" with "component fluxes" (lines 246-248). This sentence now reads:
➔ "*By solving a set of linear equations (Eq. 3 and Eq. 4), we are able to partition the component fluxes of CO_2 (Fig. 2) from the disturbed tundra (NEE_d) and from the undisturbed tundra (NEE_c) from both towers (T1 and T2)*"

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

Response: Soil surface was defined as the ground surface in any border of the collar, which was installed for level conditions across the site.
We have inserted the following sentence (lines 286-287): "*As moss cover was minimal and discontinuous, the location of the ground surface could be easily identified as the upper surface of the soil.*"

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?

Response: We used a vented non-steady state chamber, and text has been updated (lines 295-296) to reflect this system as follows: "*A non-steady state vented portable chamber system similar to Jassal et al.(2005) was used to measure fluxes from each collar using transparent and opaque chambers."*

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?

Response: We have corrected for humidity. The manuscript has been updated with corrected values throughout.

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

Response: The equation was rewritten as suggested.

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

Response: The maximum wind speed was determined from the raw 20 Hz spike, which is now reflected in the text (lines 395-397) as follows: "*There was a significant windstorm beginning on 22 July and that lasted 24 hours, with wind speeds (as determined from the raw 20 Hz spikes) of up to 21 m s^{-1}.*"

19794/19: rounding to 1 decimal seems more appropriate

Response: We have rounded to 1 decimal place.

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.

Response: We have removed the reference to Fig. 4.

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).

Response: Thank you for spotting this error. Orientation of the photo has been checked and corrected. The view is towards the south and the caption has been updated accordingly.

Fig. 6: can you add uncertainty bars to the chamber flux measuremens? 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.

Response: We have decided not to include uncertainty bars on Fig. 7 as we felt it would overwhelm the plot. Error associated with the data measured using the chamber system can be found in Fig. 8 and Table 2. We have added error bars to Fig. 6 and this information can also be found in Table 1.

Anonymous Referee #2

Comment

This is a well thought out and executed study, and well written. However, I'm a bit concerned about the actual eddy covariance NEP data because the authors used LICOR 7500 open path CO2 sensors. As far as I can tell, no attempt was made to correct for the well-documented heating issues that these open path 7500 sensors have, espe- cially in places where temperatures are cold (i.e., their site). The heating issues result in exaggerated CO2 uptake rates, which can be particularly important in regions where NEP is already extremely low. There are publications that advice how to best deal with this issue (e.g., Burba GG, McDermitt DK, Grelle A, Anderson DJ, Xu L (2008) Addressing the influence of instrument surface heat exchange on the measurements of CO2 flux from open-path gas analyzers. Global Change Biology, 14, 1854–1876). Can the authors please address this prior to the final publication of this article. It's very important!

Response: Thank you for this valuable comment – we acknowledge that under cold conditions instrument surface heat may critically affect the ability to measure CO2

fluxes with an open-path sensor, in particular with an upright mounted sensor (Burba et al., 2008). The EddyPro® manual summarizes "When CO_2 and H_2O molar densities are measured with the LI-7500 in cold environments (low temperatures below -10 °C), a correction should be applied to account for the additional instrument-related sensible heat flux, due to instrument surface heating/cooling."
(https://www.licor.com/env/help/eddypro6/Content/Calculating_Off-season_Uptake_Correction.html).

We did not apply this correction because it would lead to unrealistic values ('overcorrection') for the following three reasons:

- Our measurements were made during July, with 24 hour sunlight. Our average air temperature was +10ºC. Temperatures never dropped below 2°C, and reached as high as 16°C. Hence, ambient temperatures did not fall within the critical range of < -10ºC, where fluxes are affected, mentioned in Burba et al. (2010).
- In addition, our sensors (Li-7500) were mounted at an angle of 30º to minimize issues associated with this heating and reduce pooling of moisture on the windows. The correction cannot be employed with a tilted sensor. Burba et al. (2008) note that the correction "… assumes that the instrument is mounted in a near-vertical orientation". Previous work by our colleague Prof. A. Black (UBC, pers. comm.) has shown that a tilted sensor does not cause measurement differences between open-path and closed-path systems. A comparison between fluxes measured with a tilted Li-7500 sensor and a closed path system as function of temperature can be found in the appendix to this response.
- In our current approach (i.e. without correction) closed chamber measurements and the EC approach match well (RMSE = 0.6 µmol m^{-2} s^{-1}). If we would correct fluxes according to Burba et al. (2008) and assume the sensor was mounted upright, we found that the correlation between chamber measurements and EC measurements becomes worse (RMSE = 1.4 µmol m^{-2} s^{-1}). To us, this is a strong argument that the correction would be incorrectly applied (assuming a vertically mounted sensor) and would not improve our dataset.

Additional supporting evidence

Demonstration that a tilted sensor mounting of the Li-7500 sensor causes no sensor disagreement to a closed-path system between 2ºC and 16ºC.

To investigate whether there are any systematical differences between measuring CO_2 fluxes with a tilted open-path (OP, Li-7500) vs. a closed-path (CP) analyzer in the range of the currently observed temperatures, we used a dataset sampled by the UBC Biometeorology group (Prof. A. Black, pers. comm.) and provided to us. This dataset was recorded a forest clear-cut in Saskatchewan, Canada (Fluxnet site HJP02, Zha et al. 2009, Figure 1) at a comparable height to our systems. We used data from June – December 2004 when both a Li-7500 open path analyzer tilted by about 30º (same tilt we used in our measurements) and a closed path

analyzer (Li-7000, Fluxnet Canada standard) were operated simultaneously. Air temperatures during this time covered the range from -15 to +20ºC.

Figure 1: Tilted open-path and inlet for closed-path system at the HJP02 Fluxnet Canada site (Photo by Zoran Nesic, UBC)

The half-hourly differences between F_{CO2} measured by the tilted OP and F_{CO2} measured by the CP were binned by air temperature (2 K bins). Figure 2 shows that for the range between 0 and 20ºC, the systems are not systematically different, while below 0ºC the OP instrument starts to systematically underestimate fluxes, which indeed becomes a major issue < -10ºC - presumably due to the sensor heating effect. Temperatures during our measurements for the current study in the High Arctic were always between 2ºC and 16ºC, with an average of 10ºC, so we do not expect that our fluxes were compromised with a tilted sensor mounting of the Li-7500.

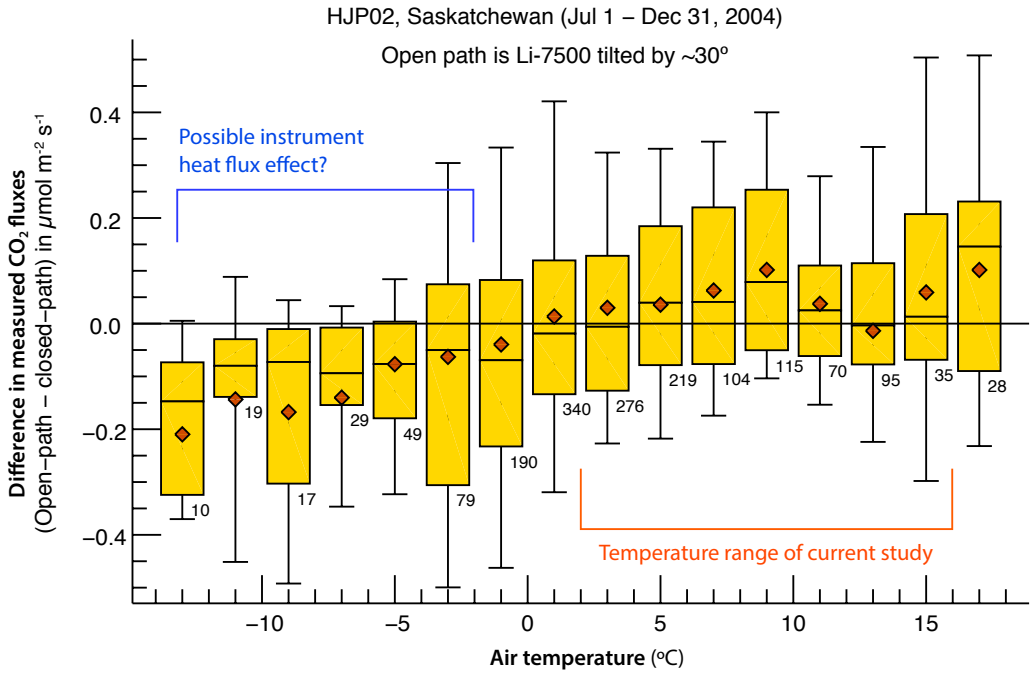

Figure 2: Difference between CO_2 fluxes determined by open path and closed-path system from July 1, to December 31, 2004 at the Canada Fluxnet Site HJP02. Diamonds are mean values in each bin, the boxes cover the 25% and 75% percentiles, with the horizontal bar being the median. The whiskers show the 5 and 95% percentiles. Numbers are the number of half hourly values in each bin (when both systems provided high quality data).

Anonymous Referee #3

Comment 1:

I am little bit confused with the general structure of the introduction (particularly from line 90 to line 126), where some interesting things (e.g., the importance for NEE measurements or the influence of tundra vegetation) should be before detailing the objectives, whereas some information (such as the design developed in this study or the use of static chamber) should be placed in the materials and methods section. Also, it would be nice to know which mechanisms are proposed to hypothesize that RTS will impact the carbon balance (lines 80-82).

Response: We have modified the introduction so that the objectives follow the introduction of the importance of measurements. The introduction section ends with "*In this study, we analyze the impacts of RTS on CO_2 exchange in a high Arctic tundra ecosystem. Our main research objective was to examine how growing season NEE and its component fluxes vary between a RTS and undisturbed tundra.*" (lines 132-134). We have moved details regarding the design of the EC system to the methods section (line 162-167): "*An appropriate sampling design was necessary to quantify the CO_2 fluxes between land surface and atmosphere simultaneously from disturbed and undisturbed sites in close proximity (Hollinger and Richardson, 2005). We used a dual eddy covariance approach, which was advantageous over a single eddy covariance tower as we were able to measure fluxes simultaneously from disturbed tundra and the surrounding undisturbed tundra (Fig. 1; Fig. 2).*"

Comment 2:

Finally, is it possible to go further in the discussion to put into perspectives quantitative estimates of such landscape disturbance? I know that this is probably tough since the authors may not have good estimates of disturbance areas (just based on personal communication) and there are gaps for fall, winter, and spring seasons.

Response: We have provided an estimate of landscape disturbance across the study location (personal communication: 585). As details of the rates of landscape change are minimal at this site, we have included an additional paper, which seeks to quantify permafrost disturbance across the Mackenzie Delta region of Canada, where increasing rates of thaw are discussed (Lantz and Kokelj, 2008). While not quantitative, one of the authors (GHRH) has seen the increased frequency and magnitude of these disturbances during nearly annual flights over the area and visits to sites on the Fosheim Peninsula since 1989. Other colleagues, in particular A. Lewkowicz and W. Pollard have also conducted permafrost research on the Fosheim Peninsula, and have told us they also clearly see an increase in these

disturbances since the late 1980s. Providing a robust estimate of the change in number and area of these disturbances would be an excellent research project.

We have addressed the following minor comments as follows:

Abstract
Line 29: Please precise the country (Canada)

Response: Canada has been added.

Introduction
Line 52: "ground", do you mean soil or top soil? Precise please.

Response: Line has been clarified to reflect organic carbon as follows: "*The northern permafrost zone carbon inventory estimates the quantity of organic carbon stored in the top 3m of frozen and unfrozen soils in northern circumpolar permafrost regions …* "

Line 80: Remove "(Lantz et al., 2009)"

Response: This reference has been removed.

Line 88: Use either "retrogressive thaw slump" or "RTS" in the text.

Response: RTS have replaced the term retrogressive thaw slump throughout the text, after the term has been defined on line 80.

Line 130: Same than for the Abstract, please add "Canada"

Response: Canada has been specified (now line 139).

Results
Lines 307-308: Can you indicated the DOY corresponding to the periods that you evoked ("end of July", "middle of July") to be consistent with the Figure 2.

Response: Three periods have been identified and DOY have been added as follows "*During the measurement period (Table 1) T_a increased from 10.5ºC in the early season (DOY=175-185) to 12.2°C during the peak of the growing season (DOY=186-202) and then decreased to 7.2°C by the end of July (DOY=203-210)*." (Now lines 358-360).

Lines 321-322: This explanation for the choice of the three periods should appeared before (in 3.1).

Response: The description and explanation of the three distinct periods has been

moved to section 3.1 (lines 355-358).

Discussion
Lines 410-411: The inter-annual variability is controlled by temperature. In which direction?

Response: This sentence has been rewritten for clarity as follows "*Arctic sites show significant inter-annual variability, which is controlled by temperature; increased temperatures may result in enhanced emissions …*" (lines 539-541).

References

Burba, G. G., McDermitt, D. K., Grelle, A., Anderson, D. J., & Xu, L. (2008). Addressing the influence of instrument surface heat exchange on the measurements of CO_2 flux from open-path gas analyzers. Global Change Biology, 14(8), 1854-1876.

Burba, G., D. McDermitt, J. Hupp, D. Anderson, & R. Eckles. (2010). Solution for Minimizing Surface Heating Effect for Fast Open-path CO_2 Flux Measurements in Cold Environments. American Geophysical Union Fall Meeting, San Francisco, California, 13-17 December.

Lantz, T. C., and S. V. Kokelj (2008). Increasing rates of retrogressive thaw slump activity in the Mackenzie Delta region, N.W.T., Canada, Geophys. Res. Lett., 35, L06502, doi:10.1029/2007GL032433.

Lewkowicz, A. G. (1990). Morphology, frequency and magnitude of active-layer detachment slides, Fosheim Peninsula, Ellesmere Island, N.W.T., Nordicana, 54, 111–118, 1990.

Lewkowicz, A. G. and Harris, C. (2005). Frequency and magnitude of active-layer detachment failures in discontinuous and continuous permafrost, northern Canada, Permafr. Periglac. Process., 16(1), 115–130..

Zha, T., Barr, A.G., Black, T.A., McCaughey, J.H., Bhatti, J.S., Hawthorne, I., Krishnan, P., Kidston, J., Saigusa, N., Shashkov, A., & Nesic, Z. (2009). Carbon sequestration in boreal jack pine stands following harvesting. Global Change Biology 15(6): 1475-1487.