Biogeosciences Discuss., 11, C2481–C2500, 2014
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Biogeosciences
Discussions
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Interactive comment on "Effect of ablation ring and soil temperature on 3-yr spring CO_2 efflux along the trans-Alaska pipeline, Alaska" *by* Y. Kim

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Received and published: 15 June 2014

Point-by-point response to Referee #1 and #2's comments

I appreciate the invaluable comments from the Reviewers regarding the improvement of this manuscript by careful revision.

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Biogeosciences Discuss., 11, 3615-3652, 2014 (doi:10.5194/bgd-11-3615-2014)

"Effects of ablation ring and soil temperature on 3-yr spring CO2 efflux along the Dalton Highway, Alaska" by Kim

For clarity, see Reviewer #1 (yellow) and Reviewer #2 (green) in the corrected pdf file

(bgd-11-3615-2014-R#1 and R#2.pdf).

Also, I corrected the manuscript by the Native English speaker of the University of Alaska Fairbanks as suggested by Reviewer #2.

âĂČ Response to Referee #1's comments

Effects of ablation ring and soil temperature on 3-yr spring CO2 efflux along the Dalton Highway, Alaska

Y. Kim

Thank you for your invaluable comments on my manuscript.

First, the reason I observed soil CO2 efflux surrounding tree trunks in the spring season was to better understand ablation ring effects and the subsequently increased soil temperature in the snow-disappeared soils of coniferous forests and tussock tundra. Ablation rings were found in nearly all white and black spruce forests and across tussock tundra in the early spring. I have many photos available upon request.

Considering these effects, I measured efflux surrounding the trunks of coniferous forest trees and tussock tundraâĂŤthat is, in snow-disappeared soilâĂŤto better understand the efflux between exposed and snow-covered soils in same site. In addition, efflux measurement was investigated for differences between four-directional CO2 emissions from white spruce stems. Despite a narrower range for soil temperature, soil CO2 efflux increased greatly, showing its highest Q10 value, as I described in the manuscript.

For winter and spring, I also focused on the effects from snow depth and snow crust on soil CO2 efflux, across boreal forest and tundra sites.

For all these reasons, I focused on the effects of ablation ring and subsequent soil temperatures through this investigation of winter and spring CO2 efflux, which took place along the unpaved 660-km haul road (total running distance: ca. 3800 km/year). Although the manual chamber system places some constraints on the temporal variability of soil CO2 efflux at some points, I observed the efflux measurements at the same points for each site during winter and spring, 2010-2012.

Responses to comments 1. The paper does not really focus on ablation rings as the title implies, nor are the other main objectives of the paper adequately addressed. One objective is to evaluate the environmental controls on spring respiration rates, but only soil temperature and snow depth are considered.

»> As mentioned above, the paper focuses on the change in soil CO2 efflux from the effects of ablation ring, as well as the subsequently increasing soil temperature in exposed soil just after snow disappearance. As expected, it is not easy to match up the timing of field observations, especially over a long-distance trip. In addition, I found higher emission of soil-originated CO2 surrounding the tree trunks and tussock tundra of exposed soil, resulting from ablation rings and the subsequently increasing soil temperature in exposed soils, in comparison to snow-covered soils in each site.

2. What is the temporal pattern of soil respiration and temperature and the timing of snowmelt and the formation of ablation rings. Do respiration rates at the different sites diverge after snowmelt or once the soils exceed 0C?

»> As mentioned, I did not observe temporal variability of soil efflux, temperature, timing of snowmelt, nor the formation of ablation rings. Although the manual chamber system offered simplicity and efficiency for covering a wide range and easily estimating spatial CO2 efflux, this system cannot conduct measurements at frequent periods, due to the disturbance of snow and soil surface after flux measurement. Further, it is difficult to observe temporal variability of soil CO2 efflux at the same point at each site using a manual chamber system. As a result, it is necessary to monitor additional year-round soil CO2 efflux using the FD (forced diffusion chamber) system described by Risk et al. (2011), as noted in the text.

»> I have monitored air and soil temperature as well as snowmelt timing/formation of ablation rings with a time-lapse (four-hour interval) camera at each site from 2010 to

now, as described in Lines 15-18 of page 3624.

»> Despite different snowmelt timing at different sites, I was able to measure CO2 effluxes in exposed soil of >0 °C just after snowmelt and in snow-covered soil of <0 °C, at each site. The magnitude of soil CO2 efflux depends on the presence (or not) of snowpack, as shown in Figures 3-4 and 6-7, and is controlled by soil temperature.

3. What accounts for the inter-annual differences in respiration patterns and rates? Why is 2010 much lower than other years?

»> This is due to snowpack in 2010, which disappeared in areas surrounding tree trunks and tussock much later than in 2011 and 2012, suggesting a difference in soil temperature, which also results in inter-annual differences in CO2 efflux, as described in Lines 15-18 of page 3624.

»> Spring soil CO2 efflux in 2010 was much lower than in other years, due to relatively early observation, as described in Line 15, page 3619. This suggests that snowpack was still relatively deeper compared to 2011 and 2012, and that soil temperature was much lower, as shown in Figures 3 and 7.

4. Spring soil respiration is highly heterogeneous, how does this compare to heterogeneity in the summer?

»> I observed summer soil CO2 effluxes during 2006-2010 within a 25 × 25 m plot (5-m interval: 36 points) at each site for spatial variability (Kim et al., 2013). During summer, boreal forest and tundra sites require 36 sampling points to generate an experimental mean falling within ±20 % of the overall mean at 95 % and 90 % confidence levels, respectively.

»> During winter and spring seasons, it is difficult to observe the spatial variability of soil CO2 efflux, due to disturbance of the snowpack and soft soil surface by CO2 efflux measurement. However, after minimal flux measurement, I was able to compare spring efflux for 2010-2012 with summer mean efflux at each site for spatiotemporal heterogeneity.

5. The methods need to be substantially improved in order to understand how data was collected.

»> I revised '2.1 sampling description and methods' as suggested by R#1. In particular, I deleted 'Line 3 to 6 of page 3620,' moved 'Line 10 to 15 of page 3620' to 'Line 11 of page 3631,' and added 'Line 29 of page 3620' to 'Line 22 to 25 of page 3623', all for better understanding of the methodology.

o soil moisture measurements described in the methods are not reported.

»> Although I had a probe for soil moisture, I could not measure soil moisture during the winter and spring seasons. This is due to no use of longer probe (8 cm) within the frozen subsurface soil.

»> I deleted the text relating to soil moisture in 'Lines 4 to 11 of page 3623' as suggested by R#1 for readability, as follows.

< Lines 4 to 11 of page 3623> Soil temperature at 5 cm below the surface, in conjunction with soil CO2 efflux- measurement, was measured at each site with a portable thermometer (Model 8402-20, Cole-Palmer, USA). For additional measurements of soil temperature and moisture, hourly temperatures at depths of 5, 10, and 20 cm and at 1.3m above ground (HOBO data logger U-12 and sensor TMC6-HD, Onsetcomp, USA) were monitored at each site. Monitoring of soil moisture at depths of 5 and 20 cm (THLog data logger and sensor HH2, Delta-T Devices, UK) was conducted at intervals of 1 h.

o replication of respiration measurements is unclear

»> I did not observe replicated CO2 efflux for the entire observation, as chamber bases were inserted into the soil at least one day before the prevention of disturbance. However, at the white spruce sites in 2011, I did measure replicated CO2 efflux on April 25 and May 1 (GC), and on April 27 and May 2 at (TZ). Specifically, at site GC, CO2 efflux

was 13.9 and 13.0 gCO2-C/m2/day south (60 cm) of the stem on April 27 and May 2, respectively, suggesting similar CO2 effluxes.

o how many tree trunk or ablation ring areas were surveyed?

»> I surveyed 1-3 trunks in the boreal forest for four-directional CO2 efflux measurements, and 8-15 points in tussock and inter-tussock over tundra, due to the limited number of chamber bases and deeper snowpack. Figure 7 shows mean CO2 efflux at each site.

o P. 3622 line 2: this sentence is confusing, were bases only used in certain circumstances?

»> The chamber base was used in exposed soil and on the hardened snow surface. I corrected the sentence in Lines 1-5 of page 3622, as suggested by R#1, as follows.

<Line 1-5 of page 3622> To prevent contamination and disturbance, bases were not used due to the soft snow surface at boreal sites during winter and spring seasons (Kim et al., 2007, 2013). Bases were also used to measure winter CO2 efflux when the snow surface was hardened by sublimation at the tundra sites.

o P. 3624 line 1: soil CO2 flux was estimated with profile measurements? I thought all the measurements were chamber based

»> I corrected the sentences of 'Line 25 of page 3623' to 'Line 5 of page 3624,' as follows. I did not measure the soil CO2 profile throughout the entire observation, though snowpack CO2 profiles between trees and surrounding white spruce stems were performed during the winter season for soil-originated CO2 transport.

<Line 25 of page 3623 to Line 5 of page 3624> Furthermore, during the winter season, snowpack CO2 concentration gradients in snowpack between trees and near tree wells were 2.52 to 4.78 ppm cm−1 and 0.93 to 1.20 ppm cm−1 using a stainless steel-made probe (0.4 cm OD; 0.2 cm ID; 80 cm long) with connecting tubing, tri-way stopcock, and syringe at sub-surface and bottom snowpack depths, respectively. This suggests that

lower CO2 gradient near tree trunk results in faster CO2 transport from the soil through snowpack to the atmosphere than in snowpack between trees. This demonstrates that the air-snow-soil interface surrounding the tree trunk is much thinner than in forest opening areas.

o there is no description of how respiration measurements were scaled to calculate a spring contribution to annual CO2 loss

»> I described spring contributions to white spruce forest sites' loss in 'Lines 8-18 of page 3621,' and added to 'Lines 10-15 of page 3620.'

o what does fig 7 show? Are these site averaged fluxes and soil temperatures?

»> Yes. Figure 7 shows the likely latitudinal distribution of CO2 efflux for soil temperature at 5-cm depth for whole sites throughout the three-year flux measurements.

o what were the ANOVA comparisons used for?

»> This indicates the significance between soil CO2 efflux and soil temperature at the 95 % confidence level, as shown in Table 3.

o What is the value of reporting CV?

»> Coefficient of variation (CV, %) is found by dividing average by standard deviation, and is meant to quantify the spatial variation of obtained data (Kim et al., 2013).

o It is difficult to keep track of the sites based on the acronyms

»> I fully recognize the tracking of sites based on acronyms throughout the manuscript may be difficult. However, I have also explained the sites in 'Sampling descriptions and methods (Line 7 to 13 of page 3619)' and in Figure 1.

âĔŸA ′c The purpose of the temperature response functions is unclear. What do the different temperature response functions represent?

»> I have used the same temperature function (Line 17 of page 3622 to Line 4 of

page 3623) used by many scientists, as cited in the manuscript. Basically, the effect of the ablation ring results in a subsequent change in soil temperature between snow-disappeared and snow-covered soils at each site. Thus, despite different characteristics for each site, I suggest throughout the manuscript that soil CO2 efflux is regulated by soil temperature.

o If the temperature responses are dramatically different between sites (as Fig 3 suggests), then is it really appropriate to apply the same temperature response to all sites (as per Fig 7)?

»> As described, sites where distinct ablation rings were found were white spruce, tussock tundra, and black spruce forests. I have shown average soil efflux and temperature for each site during each year in Figure 7, with the error bar showing standard deviation. Because of the constraints of observation frequency and accessibility along the trans-Alaska pipeline, I may be missing tundra spring season flux, though it is difficult to observe springtime tundra CO2 flux due to saturated soil, waterlogged by snow-melting water in coastal tundra, (in contrast to boreal forests) as shown in the time lapse camera photo that follows (CT site; May 24, 2013). Thus, Figure 7 displays latitudinal distributions of CO2 efflux and soil temperature (Kim et al., 2013).

Figure. Tundra in early spring on May 24, 2013 using the time-lapse camera, as attached file.

o Does Fig 3 really represent intrinsically different temperature sensitivity? Or merely a different range of sample temperatures? While boreal forest sites have soil temperatures at 5C and 10C, tundra sites doe not exceed 2C. Are the temperature responses substantially different between -5C and 2C?

»> I think this is due to the effect of ablation ring in spring, as mentioned in 'Sampling descriptions and methods (Line 19 of page 3620 to 7 of page 3621).' The difference in soil temperature is proportional to the magnitude of snow disappearance at each site, resulting from the effect of ablation ring . Namely, this indicates the heat capacity of white spruce, black spruce, and tussock tundra from the short wavelength of the sun during daytime. Subsequent soil temperature measured at each site depends on the extent of exposed soil, while wet soil also shows high heat capacity in spring.

»> In response to the last question, yes. Though the range of soil temperature was different for each site, the magnitude of soil CO2 efflux is followed by the extent of exposed soil, as shown in Figure 2. I also included Figure 3 to show characteristics of vegetation type. Even as shown in Figure 4, where the strength of CO2 efflux was different in four directions from the tree stem, despite the same white spruce sites, efflux displays differences in accumulated soil organic matter, soil exposed time, forest floor plants, and so on.

»> Differences in soil temperature from three ecotypes represent differences in temperature range and subsequent magnitude of CO2 efflux at each site, as shown in Figures 3 and 4. Therefore, I think the response of CO2 efflux to temperature in tundra is much more sensitive than in boreal forest.

o Fig 4: shows that the temperature-flux relationship is driven by location around the tree – this is important as it creates large spatial heterogeneity. What additional value is derived from showing these two sites separately?

»> I monitored environmental factors such as temperature and soil moisture at each site, indicating seasonal differences in air temperature between both sites, as listed in Table 2. Further, temporal variation in soil moisture at both sites is also displayed differently. Soil organic carbon at GC and TZ sites was 1.69 ± 0.31 and 1.53 ± 0.22 gC/m2, respectively, indicating there is no significant difference between the sites.

o What do the Q10 values mean? Mikan et al 2002, Soil Biology and Biochemistry 34, demonstrate that the transition from frozen to thawed soil produces very different Q10 values, but that these are not truly thermodynamic temperature responses. It is a valuable point of discussion that respiration rates change extremely rapidly during the transition from frozen to thawed, and that the temperature response does not follow

predictions from the growing season.

»> I completely agree with your point about different temperature responses between frozen and thawed tundra soils through the culture experiment (Mikan et al., 2002). As a result, I have cited this reference in my manuscript for better explanation of temperature dependency below and above 0 °C, in Line 21 of page 12 as follows.

< Line 21 of page 3626> Mikan et al. (2002) demonstrated that temperature response in frozen and thawed tundra soils was displayed differently through a cultural experiment above and below 0 °C, as the unfrozen water content in frozen soil, which is a significant controlling factor, greatly affected the physiological response of soil microbes, such as extracellular and intercellular mechanisms. However, unfrozen water was also unrelated to soil organic matter quality, as well as the nutrients contained in tundra organic soils (Mikan et al., 2002). These results are beneficial to better understand the temperature response of spring CO2 efflux to below and above freezing temperatures in tundra and boreal forest soils.

âËŸA ′c The results contain too much discussion and unnecessary detail (see below for further details)

âËŸA ′c The relevance of some data is unclear, eg:

o Fig 6: shows that snow depth is important for flux at tundra sites and even at similar temperatures the presence of a snow crust suppressed flux rates. Is a comparison with and without snow-crust an accurate representation of the thawing process? Presumably the dynamics in a naturally thawed patch are different than in a patch where the snow is removed between measurements. How does this data inform temperature and ablation ring dynamics?

»> I measured CO2 efflux with and without snow crust in snow-covered tundra soils, except for the snow-disappeared tussock tundra area. These data are not related to the effect of the ablation ring and temperature response in frozen soil, as shown in

Figure 6. And actually, despite the spring season, tundra soils are mostly covered by seasonal snowpack, as shown in Figure 2. I note the conduit for CO2 emission to be tussock tundra in snow-disappeared tundra soils, which are affected by the ablation of tussock and subsequently increasing temperature, compared to snow-covered soils.

o the presence of unidentified fungal communities (fig 5)?

»> After CO2 efflux measurement in snow-disappeared, inter-tussock tundra soils, I found the presence of unidentified fungal colonies within the chamber base. I consulted Professor Lee Taylor (Institute of Arctic Biology, University of Alaska Fairbanks) for identification, though he could not identify the community since I was unable to collect samples due to disturbance. However, he offered the following by e-mail. I will have a chance to collect these samples next spring season.

Interesting! That does look like fungal growth. It could be any of thousands of species. Did you collect any of it? If so, we might be able to run a molecular analysis. A variety of snow molds are quite common in Interior Alaska; they are most noticeable right at spring breakup.

o the temperature differences between tussock tops and bottoms (fig 8&9). While very interesting, how does this relate to the rest of the data?

»> My colleague and I measured IR temperature on April 19, 2010, before the installation of soil temperature sensors on August 28, as shown in the captions for Figures 8 and 9. After IR photos, I considered this an important conduit for soil CO2 emission, and have observed spring CO2 emission from ablation and subsequently stimulated soil temperature in exposed soils.

o In figure 9 it is confusing that doy counts incrementally from the beginning, rather than starting over at 1 in each year.

»> I will make the change to an x-axis for ‘mm-yy,’ as suggested by Reviewer #1 and as follows.

âËŸA ′c The discussion needs to address the mechanisms which could be responsible for these differences.

»> I will add the mechanism for temperature difference between areas in ‘Line 3 of page 3631,’ as suggested by Reviewer #1 and as follows.

< Line 3 of page 3631> The temperature difference between the top of tussock and inter-tussock was displayed distinctly during the spring seasons of 2011 and 2012. This mechanism is identical to the ablation effect in boreal forests, as shown in Figure 2. This results from strong solar radiation in daytime in the exposed tussock top.

o Could the impact of snow depend on the time of year? In winter snow insulates, so respiration rates may depend on a combination of snow factors. On the one hand, greater snow cover in winter, which insulate from cold air temperatures. On the other, more rapid thaw, exposure to radiation and higher temperatures in spring, which enhance decomposition rates.

»> Snowpack is indeed a critical factor influencing CO2 efflux during winter and spring. Of the various snow factors, snow depth, which regulates soil temperature, is the primary key in determining soil CO2 production from microbial activity. I think that monitoring snow depth is also significant for the determination of the snow-depth threshold. For example, I have measured snow depth in temperate forest soils, with a threshold depth of 40 cm. Soil surface temperature rose suddenly to above 0 °C when snow depth was greater than 40 cm. Unfortunately, I did not monitor snow depth during 2010-2012, though I have observed snow depth at each site with a time-lapse camera since 2013.

o How long before these high flux rates decline?

»> As I did not monitor CO2 efflux at each site, I am unsure. However, this effect may be extended to mid-spring. Thus, I require additional research, such as the monitoring of CO2 efflux, as well as environmental parameters at each site, as mentioned in the manuscript (Lines 9-12 of page 3629).

o What is the mechanism for such high fluxes? Microbial stress response? Turnover/community composition change of the microbial community? Depletion of labile C?

»> Higher spring efflux was similar to summer efflux; however, I think the mechanisms for CO2 production during spring and summer may be different, despite temperature dependency. Mikan et al. (2002) suggested a temperature response to CO2 efflux in thawed tundra soils, as cited in Line 13, page 3626.

< Line 13 of page 3626> Mikan et al. (2002) found that the temperature response of CO2 efflux was related to soil organic matter (SOM) quality and soil microbial community in thawed tundra soils. Thus, higher spring efflux in white spruce forest may result from accumulated SOM quality and the decomposition of preferentially labile carbon by soil microbes in exposed soils.

âĖŸA ′c Discuss limitations of the study: o The latitudinal gradient confounds the temporal component since spring in the tundra sites will be delayed relative to the boreal forests.

»> I thoroughly agree with your suggestion. Our spring CO2 efflux observation depends greatly on road conditions and transport distance along the pipeline. In spite of the lag in spring timing between tundra and boreal forest, I observed exposed tussock tundra for every spring season, as shown in Figure 2. In fact, I even attempted to depict latitudinal gradients of CO2 efflux and environmental parameters; it is extremely difficult, however, to understand the spatiotemporal variability of CO2 efflux and environmental parameters along the pipeline without monitoring CO2 efflux at each site.

Specific points regarding the results section:

P. 3620: the site description here is very detailed and reads like results, either simplify and refer to tables, or include in results section

»> I deleted ‘Lines 3-6 of page 3620,’ which is also listed in Table 2.

»> I moved ‘Lines 10-15 of page 3620’ to ‘Line 11 of page 3631’ for estimation of winter/spring CO2 contribution.

P. 3623 line 19 – paragraph end: discussion? Or reword to make a better connection to the results P. 3625 line 9 – section end: discussion

»> I moved ‘Lines 19-22 of page 3623’ to ‘Line 5 of page 3622’ for methodology.

»> I also moved ‘Lines 22-25 of page 3623’ to ‘Line 29 of page 3620’ for better understanding of the ablation ring.

P. 3624 line 12, line 18: neither of these points is illustrated in Fig. 2

»> I deleted ‘Line 12 of page 3624’ and ‘Line 18 of page 3624’: ‘as shown in Figure 2’.

P. 3624 line 16: this statement is confusing, how does the flux data suggest a 10-17 day earlier melt? This conclusion must come from the photos and the flux data shows that timing of melt-out strongly impacts flux. Reword.

»> I rewrote ‘Lines 15-18 of page 3624’ as follows-

<Line 15 to 18 of page 3624> Because the snow-disappearance date in 2011 was approximately 10 to 17 days earlier than in both 2010 and 2012, based on the measurement of the four-hour time-lapse camera, spring CO2 efflux in exposed soils in 2011 was at least tenfold higher than in snow-covered soils.

P. 3624 line 20: I would find this easier to follow if the data was organized either by magnitude or going around the compass rose, N, E, S, W

»> I agree with the suggestion; however, the length from stem to point was quite different due to the extension of exposed soil. Further, the number of figures would be too great if the compass rose was plotted.

»> Magnitude was described in ‘Lines 25-28 of page 3624,’ and Q10 values were described in ‘Line 29 of page 3626 to Line 3 of page 3627,’ as calculated in Table 3.

»> I deleted ‘Lines 23-29 of page 3626,’ relating to the response of temperature dependence to CO2 efflux at the four directional sides of each white spruce site, at the suggestion of R#1.

P. 3625 line 9 – these values are not a ten-fold increase?

»> I corrected this range, as suggested by R#1:

»> 1.05 ± 0.057 gC m-2 day-1 to 0.13 ± 0.09 gC m-2 day-1. âĂČ

Response to Referee #2’s comments

Effects of ablation ring and soil temperature on 3-yr spring CO2 efflux along the Dalton Highway, Alaska

Y. Kim

I really appreciate your invaluable comments on my manuscript.

Kim presents measurements of soil CO2 efflux during winter and spring along a latitudinal gradient in arctic Alaska. The methods are general sound and the study addresses an important knowledge gap, arctic ecosystem CO2 dynamics outside of the growing season, which is of critical importance to global change research. For the most part the study is methodologically sound. One exception may be the extrapolation of measurements to calculate winter and spring contributions to annual C balances for each ecosystem. It is not clear how this was done, but I suspect that the methods used were no appropriate. The writing could also stand to be improved; there are many poorly worded passages and a number of confusing sections. The study will not be suitable for publication until major revisions to the writing have been made that improve grammar and clarity, and until several key issues have been clarified.

»> I conducted winter CO2 efflux for January 2010, Feb-March 2010, and March 2011, in addition to citing winter efflux from Kim et al. (2013). Thus, I think the estimation

of winter CO2 efflux is appropriate. Furthermore, I checked this manuscript with the professional native-English editor (Mr. Nate Bauer) at my research center before submission. However, I have indeed revised the manuscript again for improvements in grammar and other key issues suggested by the Reviewer #2.

Were these chamber bases permanently installed? It is my understanding that CO2 may build up in the snowpack, resulting in pulses. It would be good to discuss whether this could have affected your measurements at some point in our paper, perhaps in the methods.

»> The chamber bases were permanently installed when the soil was exposed; however, bases on the snowpack were not permanent, but temporary, before the efflux-measurements Kim et al. (2013) used. I will add responses to comments suggested by Reviewer #2.

»> I corrected the following sentence from P 3622 L1-L5. To prevent contamination and disturbance, bases were not used due to the soft snow surface at boreal sites during the winter and spring seasons (Kim et al., 2007; 2013). Bases were also indeed used to measure winter CO2 efflux when the snow surface was hardened by sublimation at the tundra sites.

P 3617 L7: what do you mean ‘terrestrial carbon is susceptible to climate changes’?

»> Terrestrial ecosystem carbon (e.g., CO2 and CH4) is susceptible to these climate change responses (Chapin et al., 2000), as in P 3617 L1-L5 in the text.

»> That is to say: changes in CO2 and CH4 effluxes in terrestrial ecosystems of northern high latitudes depend on these climate change responses in the Arctic.

The second paragraph in the introduction is rather confusing and contains a number of logical inconsistencies.

»> I mean for the second paragraph of the introduction to describe changes in winter and spring CO2 effluxes in response to changes in the seasonally snow-covered period and, subsequently, early spring onset. In particular, the recent snow disappearance timing rate was seven-fold faster than in 60-year NOAA/CMDL historical data. This may represent the stimulation of spring CO2 efflux due to prompt snow disappearance. Thus, I have focused on winter and spring CO2 efflux-measurements, as well as environmental factors, for better clarity regarding the contribution from spring CO2 efflux to annual CO2 emission.

Could R#2 please further point out what is mentioned as confusing and logically inconsistent in the second paragraph of the Introduction?

P3618 L 25: Are you looking at the effect of the pipeline, or simply establishing a north-south transect using pipeline infrastructure (i.e. the Dalton Highway) to access sites. If the latter it is not necessary to emphasize the pipeline.

»> I understand your comment; I am not noting the effect of the pipeline on CO2 efflux. »> As a result, I have changed ‘trans-Alaska pipeline’ to ‘Dalton Highway’ in the title and text, as suggested by Reviewer #2.

P3624 L 11-18: It is not clear whether these values represent differences between exposed and snow covered soils, or forest and tundra, or both. It would be good to have both. This paragraph a very unclear.

»> I rewrote the following paragraph from P2624 L11-18, as suggested by Reviewer #2. »> During the spring, average CO2 efflux in exposed and snow-covered soils of the boreal forest and tundra, as shown in Figure 2, were 1.31 ± 0.25 (CV: 20 %) and 0.17 ± 0.18 (108 %) gC/m2/day in 2010, 5.38 ± 3.67 (69 %) and 0.30 ± 0.33 (110 %) gC/m2/day in 2011, and 2.71 ± 1.77 (65 %) and 0.27 ± 0.20 (74 %) gC/m2/day in 2012, respectively. Spring CO2 efflux in exposed soil in 2011 indicated values at least ten times higher than in snow-covered soil, suggesting that the snow-disappearance date in 2011 was approximately 10 to 17 days earlier than in both 2010 and 2012 (as measured by 4-hour interval camera), as shown in Figure 2. Because the snow-disappearance date in 2011 was approximately 10 to 17 days earlier than in both 2010

and 2012, based on the measurement of the four-hour time-lapse camera, spring CO2 efflux in exposed soils in 2011 was at least tenfold higher than in snow-covered soils.

P3629 L5: What vegetation community types?

»> Fahnestock et al. (1988) measured winter CO2 efflux vegetation communities including Cappiope and Salix, upland tussock, and dry heath communities, and showed the lower relationship between winter CO2 efflux and snow depth (R2 = 0.19).

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C2481/2014/bgd-11-C2481-2014-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 11, 3615, 2014.

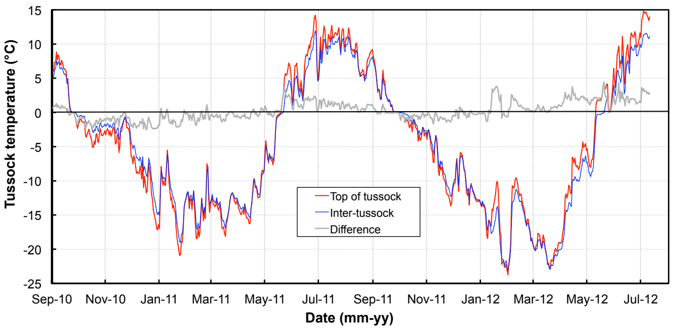

Figure 9

Fig. 1. Figure 9. Revised

Fig. 2. Tundra in early spring on May 24, 2013 using the time-lapse camera