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Biogeosciences Discussions

Interactive comment on "Environmental factors regulating winter CO₂ flux in snow-covered boreal forest soil, interior Alaska" *by* Y. Kim and Y. Kodama

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Anonymous Referee #2

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I have responded in blue-colored sentences upon the suggestions of Referee #2. The ambient pressure has no direct effect on winter soil CO2 flux; however, ambient temperature directly influenced by this pressure is a significant key in regulating winter soil carbon emission in the boreal black spruce forest soil of central Alaska, as shown in Figure 10. Thus, ambient temperature will be parameterized in the terrestrial ecosystem carbon models for the contribution of winter soil carbon in northern high-latitude

hemisphere.

Kim and Kadama estimated winter soil CO2 efflux from a boreal forest ecosystem in Alaska's interior. Such winter analyses, though not unheard of, are relatively rare given challenges associated with measuring winter fluxes in extremely colds climates. The approach is generally sound and many (but not all) of the results/conclusions well-founded; however, I question whether the limited scope and depth of the work as presented advance the science in a way that warrants publication. Primary limitations of the manuscript are:

1) The contribution of this research to the existing body of science is very incremental and lacks context with recently published papers on the topic. I'm not one to think that every study must be novel to be published, but the authors should better describe the contribution of this paper in the context of recent studies. The authors do not cite any papers published following 2007. This neglects important recent advances in arctic/boreal C cycling science. An ISI Web of Science search of "arctic and soil CO2" and "arctic and soil respiration" yielded 108 and 120 citations, with many titles that are clearly relevant to the current study.

»» I searched the key words "winter CO2 flux," "winter soil respiration," and "boreal forest," and added "arctic"âĂŤthese have yielded only 17 and 7 papers since 2007, respectively, from the ISI Web. I have newly cited the following references on winter CO2 flux published since 2007 in the manuscript.

Björkman, M.P., Morgner, E., Cooper, E.J., Elberling, B., Klemedtsson, L., and Björk, R.G.: Winter carbon dioxide effluxes from Arctic ecosystem: An overview and comparison of methodologies, Global Biogeochem. Cycles, 24, GB3010, doi:10.1029/2009GB003667, 2010.

Bowling, D.R., Massman, W.J., Schaeffer, S.M., Burns, S.P., Monson, R.K., and Williams, M.W.: Biological and physical influences on the carbon isotope content of CO2 in a subalpine forest snowpack Niwot Ridge, Colorado, Biogeochemistry, 95, 27-

59, 2009.

Liptzin, D., Williams, M.W., Helmig, D., Seok, B., Filippa, G., Chowanski, K., and Hueber, J.: Process-level controls on CO2 fluxes from a seasonally snow-covered subalpine meadow soil, Niwot Ridge, Colorado, Biogeochemistry, 95, 151-166, 2009.

Seok, B., Helmig, D., Williams, M.W., Liptzin, D., Chowanski, K., and Hueber, J.: An automated system for continuous measurements of trace gas fluxes through snow: an evaluation of the gas diffusion method at a subalpine forest site, Niwot Ridge, Colorado, Biogeochemistry, 95, 95-113, 2009.

2) The sample size is n=1, conducted over a single season. I appreciate the challenges associated with conducting measurements in extremely cold temperatures (which is why we have minimal soil respiration measurements during winter at our site), but the authors need to make a more convincing case for this n=1 sample size and for sampling over only a single season. Certainly, there's an eddy-covariance flux tower analogy here; that is, n = 1 is adequate in the context of eddy-flux data, but most studies of soil respiration employ multiple replicates and eddy-flux studies now generally require multiple years of data to warrant publication. Are the data generated in the present study so novel that less than 1 year of data from one sensor array is sufficient for publication?

»» This study was based on winter research of 2004/5 (Kim et al., 2007) using chamber and profile methods. I have attempted to obtain multi-year winter CO2 flux data; however, the operation of sensors, periodic snow-survey, and unexpected breaks in power supply have proven quite difficult during harsher and much colder conditions.

»»The measuring array consisting of 4 sensors has some merits in the seasonally snow-covered black spruce forest soil of interior Alaska. First, CO2 concentration in snowpack air was determined by a passive measurement method that does not require withdrawal of air from the snowpack and therefore avoids inducing artificial snowpack ventilation. Second, average concentration over 30 minutes at each sensor was

stored to the data-logger (n=48 per sensor per day). Third, only meteorological data was monitored by the tower, though ecosystem carbon flux-measurements such as ecosystem respiration (Re), net primary productivity (NPP), and gross primary productivity (GPP) were not, due to instrument trouble caused by frost. Finally, continuous CO2 flux-measurement through the snowpack in the black spruce forest of central Alaska contributes significantly to the ecosystem carbon models under the extreme cold environment.

3) The emphasis of this study is on now very well-established physical drivers of soil respiration (e.g., temperature), rather than on novel and/or robust analyses. To the latter, the causal relationship with atmospheric pressure that the authors attempt to establish is questionable. With this in mind, I'm not certain that breaking down soil C flux by pressure phases is an appropriate way to present the data. The authors find no differences in soil C flux among atm pressure "treatments" (Pg 1138, Lns 11-16), suggesting that this categorization is not necessary. And, though it is useful to confirm that temperature is a driver, the results would be more informative if the authors had parsed the data by phenophases and/or snow depth (e.g., snow accumulation phase, snow melt phase, etc), rather than arbitrarily by Patm. The authors mention that a flux tower is nearby – a much more interesting comparison would be of soil respiration and Re (using flux tower data).

»» Change in atmospheric pressure led to variations in atmospheric temperature and soil temperature. Change in winter CO2 flux was dependent on change in temperature. Although CO2 fluxes do not show significant difference under high pressure (HP), low pressure (LP), and intermediate pressure (IP), the fluxes are regulated by temperatures in air and soil. As suggested by Referee #2, I have plotted the relationship between daily mean winter CO2 flux and snow depth, now shown in Figure S1.

»» I inserted the following sentences in Pg 1138, Ln 21, as suggested by Referee #2.

The equations are $y=0.0004x + 0.21$ ($R2=0.004$; $p=0.029$) under HP, LP, and IP, and y=0.0029x + 0.29 (R^2=0.80; p=0.869) under MP, respectively, based on a one-way ANOVA with a 95% confidence level. Winter CO_2 emission is constrained by snow-melting water during MP, indicating that the snow-melting water has filled the soil pore space. On the other hand, winter CO_2 emission shows much weaker relation to the change in snow depth before the snow-thawing, suggesting winter CO_2 emission is not related to the snow depth.

I have not yet inserted Figure S1 in the text, pending the approval of Referee 2.

4) Separate soil C flux models for the different atm pressure "treatments" (shown in figure 10) are not statistically justified. Because soil respiration does not differ among HP, IP, and LP atm pressure "treatments" (Pg 1138, Lns 11-16), only a single model is necessary for these.

»» I have calculated the correlation coefficients (R^2) among the variables (e.g., atmospheric pressure, atmospheric temperature, and soil temperature) and winter CO_2 emission during each phase (i.e., HP, IP, LP, and MP), as described in 3). For example, atmospheric temperature accounted for 80% of winter CO_2 emission during HP, as shown in Figure 10 (a). Thus atmospheric temperature during HP phase plays a much more significant key in regulating winter CO_2 emission than during other phases in snow-covered black spruce forest soils of interior Alaska.

5) Generally, the paper could be much better written and more concise. There are too many figures (10!). For example, figures 4 and 5, are not necessary – these essentially present raw data (e.g., temperature) that are used to derive other results.

»» As suggested by the Referee, I can delete Figures 4 and 5. However, Figure 4 provides important information regarding atmospheric temperature's control by atmospheric pressure.

»»In Figure 5, rising and falling CO_2 concentrations at three depths seems to be similar to the variation of atmospheric pressure. However, winter CO_2 flux is not directly

modulated by atmospheric pressure.

Specific points: Pg 1134. Ln 13. Which eddy-covariance tower is nearby and did the authors consider comparing their soil C flux data with Re obtained from the eddy-flux tower?

»» Ecosystem carbon flux measurements have not been obtained by eddy-covariance tower due to measuring instrument frost trouble during the winter period. These instruments could not calculate the contribution of soil C flux on the Re. In fact, I need these ecosystem carbon data from the tower for contribution of winter carbon flux. Because cold temperatures in Fairbanks reach 40°C below zero during the winter, there is risk of damage to the expensive instruments. Even so, there have been attempts to monitor the data.

Pg 1136. Lns 9-13. How sensitive are soil C flux values to the assumed tortuosity?

»»This is on Pg 1135 Lns 9-13, not Pg 1136. The tortuosity is a function of snow porosity that depends on the snow density. As I estimated in the text, the range of assumed tortuosity was from 0.74 to 0.92, indicating that the tortuosity shows effects from 8 % before snow-melting period to 26 % during snow-melting period

Pg 1136. Ln 23. What is the value of measuring soil moisture and temperature data outside the window of soil C flux measurements? And, DOY > 365 does not make sense.

»»I plotted soil moisture and temperature in Figure 2 based on when frozen soil starts to melt. »»Regarding DOY, I disagree. Scientists regularly use DOY>365 for continuous measurements. However, if Referee #2 still wishes to correct the DOY>365, I can re-plot the figures in the text.

Pg 1137, Lns 10-19. I don't find this justification for partitioning fluxes by atmospheric pressure convincing. The categories themselves seem arbitrarily selected and temperatures do not vary considerably, for example, between HP (-22.1) and IP (-21.5).

»»When I categorized the four atmospheric pressures, the difference in average air temperature between HP and IP was not significant. The reason is why atmospheric temperatures were simply averaged. However, atmospheric temperature during HP better regulated winter carbon emission than during IP, as suggested in Figures 7(c) and 10 (a and b).

Pg 1138, Lns 11-16. Soil CO_2 flux doesn't even differ among atmospheric pressure treatments (except during snowmelt), so why partition fluxes this way?

»»Although there are no significant differences among soil carbon emissions under HP, IP, and LP phases, atmospheric temperature modulated by atmospheric pressure is one of the significant environmental factors in regulating soil carbon emission, indicating that there are different correlation coefficients under each phase, as shown in Figure 10. That is, atmospheric temperature is an important factor in determining winter soil carbon emission in the snow-covered boreal black spruce forest soil of interior Alaska.

Pg 1138, Lns 22-27. I don't follow this logic concerning snow depth and temperature.

»»As shown in Figure S1, soil temperature is not related to snow depth in the central Alaska black spruce forest before the snow-melting period. I plotted winter CO_2 fluxes and snow depth on the winter of 2004/5, showing a much lower relationship (R^2=0.013) at the same site. The snow depth in the black spruce forest of central Alaska is not a significant factor in modulating winter soil carbon emission contrary to tundra (Oechel et al., 1997), alpine (Brooks et al., 1996; Monson et al., 2006), and temperate (Kim and Tanaka, 2002; Takagi et al., 2005) regions.

Pg 1139. Given points on this page, I'm not convinced that both figures 6 and 7 are necessary.

»» Figures 6 and 7 imply external, qualitative aspects for the quantitative understanding on the important environmental factors in modulating winter carbon flux. Thus I wish to

keep the Figures 6 and 7 in the text.

Pg 1140, Lns 10-15. Not sure why different models are needed if they all have statistically common soil respiration rates.

»»I have described air temperature as a most significant factor in controlling winter carbon emission, as shown in Figure 7 b and c. This relationship has been used by many scientists and is important for the parameterization of the winter contribution of ecosystem carbon models in high-latitude hemisphere.

Pg 1141. Paragraph beginning at Ln 19. This paragraph is cumbersome to read and not well assembled.

»»I have changed to the following sentence in the text as pointed out by Referee #2.

"Figure 10 shows the percentage (%) of the correlation coefficient among atmospheric pressure, atmospheric temperature, soil temperature, and winter CO_2 flux under each

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C1697/2012/bgd-9-C1697-2012-supplement.pdf

Interactive comment on Biogeosciences Discuss., 9, 1129, 2012.

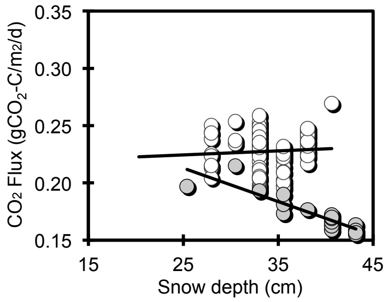

Figure S1. Relationship between daily average CO_2 fluxes and snow depths under HP, LP, and IP (open circles), and MP (solid circles).

Fig. 1. Relationship between daily average CO2 fluxes and snow depths under HP. LP. and IP (open circles), and MP (solid circles)