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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

Anonymous Referee #1

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General Comments:

There is lot of uncertainty in the response of CO2 soil fluxes to changes in climate, in particular to winter conditions and snowpack at high Northern Latitudes. Therefore, the wintertime flux measurements reported in this paper are of high value for advancing this research. They also are one of the few reports that are based on continuous wintertime observations.

A noteworthy advantage of the measurements presented here is that CO2 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

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ventilation. While flux measurements based on vertical gas concentration gradient measurements in the snowpack are a commonly used approach, this measurement is based on a number of simplifications and assumptions. Most researchers are aware of these shortcomings and present their results in the light of the rather large uncertainty of this measurement. It is regrettable that the Kim and Kodama manuscript does not provide any error/uncertainty estimates whatsoever.

Flux data are broken up in segments of four different classes of which three are based on atmospheric pressure levels. These analyses eventually come to the conclusion that "atmospheric temperature, modulated by the pressure, is a significant factor in determining winter CO2 flux in the seasonally snow-covered boreal forest soil of interior Alaska". This is by no means surprising as numerous other previous studies have shown the sensitivity of the subniveal CO2 flux on soil temperature. The presentation in this paper does not provide a convincing case that indeed the fluxes are modulated by pressure. Most likely it is simply the subniveal temperature that is the determining variable. Pressure fluctuations do, however, exert a strong influence on the formation of gas gradients in the snowpack. It is striking that this manuscript neglects new findings and literature published in this field over the past five years. For instance, the work by (Seok et al. 2009) shows data examples on the effects of pressure fluctuations/wind pumping on the snowpack gas gradients. The Seok et al. paper also presents a quantitative description of this dependency and an approach to correct the CO2 flux determination for the wind pumping effect. It is regrettable that the Kim and Kodama manuscript does not address the need to consider this effect.

In summary, my recommendation is to first correct the data for wind pumping effects and then analyze the dependency of the CO2 flux on the nice soil temperature data that were collected. Another relationship that would be of interest to investigate more closely is dependency on soil humidity. It would also be of interest to more thoroughly examine the evolution of the CO2 flux over the portion of the snow-covered season that was captured with these measurements.

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Other specific comments:

1130: I suggest mentioning in the abstract that this research was conducted at a permafrost site.

1130/11: The error margins given for the data are misleading. These values are ways smaller than a realistic estimate of the measurement uncertainty that is adherent to this type experiment.

1130/14: Explain/be more clear about what is meant by 'correlate at levels of xx%".

1132/5: I suggest to also mentioning the conceptual model of (Liptzin et al. 2009) and to discuss the findings of this study in the light of the cited literature.

1133/Fig 1: Please provide some explanation of where the gas is actually measured? What is the cuvette volume? How does interstitial air get there?

1134/16: Fig. 2 has no scale for the snow accumulation rate (black data).

1134/2: Give data averaging interval for the precision determination.

1134/19: The statement 'was . . . higher than . . . at the lowest' is confusing.

1135/4: Probably what is meant is that gradients between 10 to 20 and 20 to 30 cm were similar?

1136/14: Is indeed wind speed affecting the flux, or do the authors refer to the gas concentration gradient (again, see (Seok et al. 2009))?

1136/17: What is probably meant with this sentence is that wind speed has an effect on the CO2 flux, not vice versa?

1137/29 – Fig 5: The data and discussion would be easier to follow if below surface measurements (in the soil) would be labeled with negative depths (i.e. -5 cm).

1142/8: This statement is misleading. This result is not representative for the 'snow-covered period', but instead for the 'experimental period', which according to the infor-

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mation provided in the paper, only covered a fraction of the snow season.

1142/14-15: This estimation is highly speculative. It, for instance, neglects changes in the soil gas fluxes that have been seen in other studies during the snow melting phase (Liptzin et al. 2009), (Filippa et al. 2009).

References

Filippa, G., M. Freppaz, M.W. Williams, D. Helmig, D. Liptzin, B. Seok, B. Hall and K. Chowanski 2009. Winter and summer nitrous oxide and nitrogen oxides fluxes from a seasonally snow-covered subalpine meadow at Niwot Ridge, Colorado. Biogeochemistry. 95:131-149.

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

Seok, B., D. Helmig, M.W. Williams, D. Liptzin, K. Chowanski and J. Hueber 2009. 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.

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