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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 #2

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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 cold 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

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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 The authors do not cite any papers published following 2007.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.

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?

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).

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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.

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.

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?

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

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.

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).

Pg 1138, Lns 11-16. Soil CO₂ flux doesn't even differ among atmospheric pressure treatments (except during snowmelt), so why partition fluxes this way?

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

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

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

Pg 1141. Paragraph beginning at Ln 19. This paragraph is cumbersome to read and

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not well assembled.

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