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Interactive comment on "Environmental controls on ecosystem-scale cold season methane and carbon dioxide fluxes in an Arctic tundra ecosystem" *by* Dean Howard et al.

Anonymous Referee #1

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The manuscript by Howard et al. reports two years of ecosystem-scale methane fluxes in an Alaskan tundra ecosystem measured with the aerodynamic gradient approach. They find large contributions of wintertime CH4 emissions to annual emissions and pronounced differences in flux magnitudes between the two years. They propose that deep and near-surface soil temperature dynamics cause these differences in CH4 emissions with warm deep soil temperature boosting methanogenesis and cold near-surface temperatures inhibiting methanotroph activity. The manuscript aims to improve our current understanding of methane emissions in tundra regions. It addresses an important topic and contributes methane flux measurements for a region, where such measurements are still rare. The authors discuss an interesting hypothesis about the origin of the observed interannual differences in methane emissions. However, I have two major comments regarding the manuscript that the authors should consider addressing. First, I am not sure if an ecosystem-scale flux measurement approach only is enough to support the authors' hypothesis. Flux measurements represent an important tool to derive greenhouse gas budgets and to characterise temporal greenhouse gas flux dynamics. However, soil profiles of methane concentrations would be needed to support the authors' hypothesis. Such measurements can provide information on where in the soil profile methane is produced and where it is consumed. High methane concentration in deeper soil layers would provide additional evidence for substantial methane production during winter conditions. Without this information, the authors' conclusions remain speculative. Second, the authors use the aerodynamic gradient approach to calculate methane fluxes between surface and atmosphere. How does this approach compare to the widely used eddy covariance method? What are the limitations of this approach? The manuscript mainly focusses on wintertime fluxes. Stable atmospheric conditions are characteristic for tundra ecosystems in the winter when snow cover is present. How would stability affect accuracy of the aerodynamic gradient approach? Equation 1 shows the stability-dependent similarity functions. How sensitive are calculated fluxes to these terms and is the effect the same for winter and summer? For eddy covariance measurements, a friction velocity threshold is usually applied to filter for period of low turbulence. Would such a threshold filter also apply to the aerodynamic gradient approach? The authors could consider quantifying these uncertainties and discussing potential implications on methane emission estimates.

Other comments

Line 13: Is there any evidence in the literature that sub-zero soil temperatures allow sufficient methane production to explain winter emission rates in this study?

Line 62: The authors mention here soil pore space methane and carbon dioxide concentrations, but these data are not presented in the manuscript.

Line 82: Should the unit rather be kg C m-2?

Line 91: Was only a zero calibration applied or also a span calibration?

Line 93-93: Could these concentration differences be used to derived uncertainties for the flux estimates? How much would a methane concentration bias of 0.001 μmol mol$-$1 affect methane emissions?

Line 120-121: Did the authors also account for potential effects of snow cover on displacement height?

Line 131: Which approach was used to gap-fill? Which function was used withing the R package?

Line 140: Here, and throughout the manuscript, comparisons could be supported by statistical test if possible (see for example t-test for snow depth).

Line 144: What is the response time of soil temperatures at 100 cm? How long does it take for a temperature pulse to propagate through the soil profile (see line 291)? Could it be that 100 cm winter soil temperature contains information from previous seasons?

Line 183: The soil respiration losses of about 0.5 kg C m-2 yr-1 seem very high to me (see also comparison with other tundra sites in the manuscript). Is there any particular reason why such high losses could be expected?

Line 185: The authors argue that methane production occurs deeper in the soil profile? Wouldn't it then be more intuitive to use deeper soil temperature time series to define transition seasons?

Fig. 2: Are methane emissions of 0.8 mg C m-2 h-1 reasonable for soil temperature below 0C? Methane production rates in the soil then must be at least of similar magnitude (i.e., in the absence of methane consumption in upper soil layers).

Line 324: The regression tree approach should be explained in the Methods section.

Line 340-341: Was the performance of the model equally good for winter and summer periods?

Line 360-377: Could these studies quantitatively support the temperature threshold at -2.4C?

Line 432: It is true that snow accumulation might increase in the tundra in a warming climate. However, melt periods during the cold winter period may become more frequent and lead to snow-free conditions during the winter. This could then lead to colder soil temperatures.

Line 437-439: The authors could discuss literature on wintertime methane concentration soil profiles if such studies exist.

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