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Biogeosciences Discussions

Interactive comment on "Temperature and moisture effects on greenhouse gas emissions from deep active-layer boreal soils" *by* Ben Bond-Lamberty et al.

Anonymous Referee #1

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General comments

The authors have measured CO2 and CH4 emissions from mineral soils sampled from an Alaskan boreal forest within a discontinuous permafrost zone. These soils were incubated for 100 days at two temperatures and moistures in order to better understand the effect of these variables, and others like soil chemistry, on microbial respiration rates. Temperature sensitivity of respiration was higher in the drought treatment than field moisture conditions, suggesting that respiration rates in these deep active layer thickness soils may be more sensitive to changes in moisture than to temperature. Overall this is a valuable data set. This manuscript contributes to the growing body of Arctic permafrost and active layer incubation literature that will help inform temperature

sensitivities of the relatively large mineral C pool at depth, and the interactions of soil temperature with soil moisture, nutrients, and carbon quality that also influence the rate of C remineralization. There has been a lot of focus in incubation studies on the effect of inundation on CO2 and CH4 emissions, but drought is an important factor to consider as well. I do think that the paper has within it data that is relevant to the scope of BG, but I think that a more in depth analysis of the data, and a better contextualization of the data within the Arctic literature is required. The methods and concepts are not novel, but this is an understudied system and very timely work. More is needed in order to reach substantial conclusions. I think that the work would benefit from a more thorough comparison with boreal forest incubations across the Arctic.

Specific comments

N section would benefit from more Arctic-centric comparisons of N limitations and in particular of boreal forest N dynamics. Q10 can be temperature dependent, also depending on N limitation in the system.

This study raises interesting questions. In mineral soils, under woody vegetation that might be of low C quality, and slower C pool, one might expect higher temperature sensitivity. I think that these questions, even if not addressed directly by the data presented, should have been discussed more explicitly. Comparison with other Arctic woody plant systems would be instructive.

Studies have shown that moisture can have a weaker effect on temperature sensitivity early on during an incubation experiment, in the presence of more labile C. This relative to the effect on moisture on the Q-10 of cumulative respiration, reflecting slow turning over C – this could be an interesting analysis to include here, and would help to assess how

How do your results in terms of temperature and moisture sensitivity (especially under drought conditions) scale with Alaskan climate change predictions from modelers? How does it compare with deep soils incubations (mineral soils) from the Arctic, and

from boreal ecosystems?

Comments:

Abstract

Line 31-34: I cannot find discussion of this point in the rest of the text, and while important, this statement is relatively vague and there are no cited references. Since it underpins the rational for studying deep, unfrozen Arctic soils, it would be helpful to expand on this more in the manuscript.

Introduction

Lines 48-60: I think that this section would benefit from an introduction of the interactions between the specific ecosystem (upland boreal forest) you are studying, and its interaction with soil chemistry, since vegetation type is influential in terms of soil carbon quality and quantity. Woody plant biomass tends to have a higher C:N ratio relative to herbaceous dominated systems, and this tends to result in lower quality resources for microbial communities.

Lines 70-72: These are really important considerations, and it seems appropriate to discuss them more explicitly. How is the temperature and precipitation regime of the boreal forest of interior AK expected to change? There are also indirect effects of vegetation type on soil temperatures that could be discussed here.

Lines 72-74: While these are important questions, they are not really addressed in this study, and so either it might make sense to leave it out, or to discuss the particulars as they apply to this study, ie: the importance and questions related to C:N ratios.

Lines 77-80: I think a stronger argument for why deep active-layer soils can be made, and it would be helpful to clarify what are the “strong effects” of warming.

Methods

I cannot tell if C:N, %C and %N were measured at the end of the incubation. Could

these results be collated in a table in the manuscript? Otherwise the methods section appears to be detailed and well written.

Results

Line 232: In this section it would also be interesting to know the soil respiration decay rate per treatment over the course of the incubation experiment.

Line 238-240: Confusingly worded sentence.

Discussion

I don’t think that the summary of nearby respiration studies add very much to the discussion section. Perhaps if the similarities and discrepancies were more integral to the central findings of the paper or integrated differently into the discussion they would seem more meaningful here. Perhaps comparing with other boreal incubations (eg: Lee et al., 2012; Lavoie et al., 2011) would help to provide some additional context.

Line 270: There is missing punctuation after the word “results”.

Line 286-293: Perhaps the new synthesis by Schadel et al., 2016, would also be a useful comparison here.

Line 293: That soil moisture may be as important a control on microbial respiration as temperature is an important finding in recent incubation studies, and the potential to define its interaction with temperature will help modelers of soil decomposition better constrain the physical parameters of microbial respiration rates. This feels buried in the manuscript, and I think that it would improve the paper if it were highlighted better throughout the text.

Line 311-317: This section could be better explained in the context of the discussion or omitted altogether. It seems less important to defend the plausibility of relatively low temperature sensitivity, but instead to try to explain it in the context of these soil characteristics. Could low temperature sensitivity be the result of low C quality in this

deep soil environment?

Line 322-332: This section, which lays out the crux of the paper, the interaction between temperature and moisture sensitivity in driving microbial respiration is relatively vague. It would be good to describe the less-temperature-sensitive processes that would be important to consider for more stable-C metabolism. And how does moisture play a role here? Perhaps DOC becomes more limiting in the drought conditions?

Line 356: The Janssens et al., 2010, citation refers to a meta-analysis of temperate forest soils that are not nitrogen limited. There are studies focusing on Arctic N cycling that would be more appropriate, and many Arctic studies have shown that N availability can limit C mineralization rates. Is this site considered to be N limited in the deep active layer?

Line 367: Is this comparison, with North American soils, relevant to this study?

Line 383-384: Can you be more explicit in your meaning here? How do you mean that there is weakness in what can be inferred about temperature sensitivity from experiments?

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