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Interactive comment on "Impacts of temperature and soil characteristics on methane production and oxidation in Arctic polygonal tundra" *by* Jianqiu Zheng et al.

Anonymous Referee #1

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General Comments

This manuscript focuses on an important problem: the fate of the vast arctic carbon stores. It is unknown how much of this carbon will be released to that atmosphere as methane. However, we do know that emissions will be highly contingent on processes of methanogenesis and methane oxidation. How these processes will proceed in the Arctic is not entirely clear. This manuscript takes a sensible approach in proposing hypotheses that are based on better-known temperate systems. The hypotheses are then evaluated in the context of arctic soils.

In testing the hypotheses, the first surprising result was that methane oxidation rates

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did not seem to be largest near the surface (where oxygen is most abundant). Instead, these rates were largest where methane concentrations were highest. In this way, arctic soils may differ from lower-latitude soils. This manuscript also made important comparisons between the temperature sensitivities of methane oxidation and production. Understanding these temperature sensitivities is an essential step toward understanding how methane emissions will change under a warming climate.

Overall, I think that this manuscript has the potential to be an understandable, interesting, and useful contribution to the literature. However, as it currently stands, there are some weaknesses in the methods, and the conclusions are not entirely justified. Here are a few major points:

1. It does not seem that the microcosms were controlled for soil water content. This could be a major problem: the classic understanding of methanogenesis is that there is an optimum soil moisture for methane oxidation (e.g., Zhuang et al. 2004, Global Biogeochemical Cycles, 18, GB3010). Wouldn't soil water variation confound the results? Note that soil water can vary both across samples and, through evaporation, over time in a single sample.

2. A more rigorous statistical analysis would make the results more compelling. What are the p-values of the different fits in Figure 2? Are there any patterns in the residuals?

3. Regarding hypothesis 2, the bit about production exceeding consumption is not very compelling. Doesn't production have to exceed consumption? Otherwise, wouldn't concentrations would eventually go negative? Of course, consumption can exceed production if atmospheric methane is being consumed, but I don't think the authors meant to go in that direction.

4. The text reads as if the experiment isolated the gross rates of methane production and methane consumption. However, I was not convinced that this was the case. As far as I could tell, only the net rate was evaluated. It was not clear what effect this mismatch would have on the conclusions.

5. Finally, there are numerous points (listed below) that require clarification.

Specific comments

P2, L29-30: The presence of a CH4 gradient, by itself, does not suggest that methane oxidation is being underestimated.

P3, L6: “rapid”: Be more specific. Are you talking about diurnal variability, day-to-day variability, seasonal variability, something else?

Section 2.3.1: I am confused as to the number of microcosms. Is it 5x9x3 = 135? (5 soil layers x 9 replicates x 3 temperatures)? Please clarify.

Section 2.3.2: Again, I am confused as to the number of replicates. Line 3 says three replicates, line 5 says nine replicates. Also, this section is called “methane oxidation potential assay”, but there are still both methanogenesis and methanotrophy going on (at least as far as I can tell). Is the argument that the effects of methanogenesis are negligible? The results would be more convincing if you explicitly make this argument.

Section 2.5: Several points need clarification. The text states that B_methanotrophs and B_methanogens were “estimated”, but it does not say how they were estimated. Please clarify. The text states that Vmax,oxi and Vmeasure,pro were obtained from incubations, but does not provide details. Explain how this is done. Were all incubations at all temperatures used, or was only a subset? Also, for any given incubation, how do you separate out production and consumption (since both are presumably happening in all incubations)? What is the justification for assuming that Roxi=Rpro? Finally, the text states that initial CH4 and O2 measured concentrations were used, but don’t you need a time series of these to estimate the parameters?

Section 3.2.1: Why is there apparently negligible production from the HCP permafrost soil, incubated under anoxic conditions?

P13, L23-26: These sentences are a direct description of results obtained in this study. They belong in the “Results” section.

Discussion: I am wondering if you could include a few sentences that explicitly describe how your results will effect the development of mechanistic methane models.

Technical corrections

P2, L27: “huge” is too imprecise

P2, L29: “deeper” than what?

P3, L7 and L24: Why is it a nonlinear response to temperature “fluctuations”? Isn’t it a nonlinear response to temperature? (That is, I think you should omit the word “fluctuations”.)

P3, L25: Respond more “rapidly” or more “strongly”?

P13, L4: “disparately” is the wrong word here.

P13, L23-24: What is meant by “temperature profile”?

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