Author responses to:

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*

*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 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.*

The incubated soils were kept at their original soil water content to best represent the field conditions in the thaw season in Barrow. These microcosms were created by placing soil in serum vials sealed with butyl rubber stoppers. Therefore, no changes in soil water content are expected during incubations, and we treated soil moisture as constant in individual samples during the incubation. Soil moisture was indeed significantly different among different samples, contributing to the variations in observed differences in methanogenesis and iron reduction rates. We'll add additional discussion on soil moisture in the revised manuscript. We developed a new figure illustrating the experimental design (discussed below), which should help clarify this point.

*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?*

We have fitted the data using both linear and hyperbolic models before selecting the linear model. We will add the p-values in the result section and provide a residual plot in the supplementary material.

*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*.

We consider methane consumption exceeds production when the concentration of methane in soil column is lower than the ambient level. Methane production and consumption have different temperature sensitivity, thus the net methane production in response to warming is undetermined. We will rephrase the question to clarify in the revised manuscript.

*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.*

The experiment isolated the gross rates of methane production and potential methane consumption. Gross production was measured by incubating samples in an anoxic $N_2$ headspace, while potential gross methane consumption was measured by incubating samples in ambient air with addition of 1% $CH_4$ headspace. The new figure should make the experimental design easier to understand, and we will clarify in the method sections 2.3.1 and 2.3.2.

*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.*

The discrepancy between high $CH_4$ concentrations in deep soil and near zero surface emissions suggest $CH_4$ oxidation can be an important factor determining surface $CH_4$ flux rates. We will clarify the types of gas flux estimates or models that could be affected by this discrepancy in the revised manuscript.

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

Revision: accelerated warming.

*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.*

Yes. We started with 5x9x3 = 135 microcosms to measure $CH_4$ and $CO_2$ production. For each soil layer x temperature combination, 3 of the 9 replicates were opened to set up $CH_4$ oxidation experiments at Day 10, and additional 3 replicates were opened at Day 20. We created a new figure for the revised manuscript to better explain the workflow (see attached).

*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.*

Three replicates (about 10 g soil each) were opened to reconstruct nine methane oxidation assays (about 2 g soil each). Please see the new figure. We will clarify that methanogenesis is expected to be negligible under the fully oxic conditions of the methane oxidation potential assay.

*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?*

This simple simulation for Figure 7 was performed to illustrate the increasing ratio of methanotrophs to methanogens required for a zero net $CH_4$ emission scenario at increasing temperature. Therefore, we calculated the ratio of methanotroph biomass (*B_methanotrophs*) to methanogen biomass (*B_methanogens*) by assuming $R_{oxi}=R_{pro}$. This simulation illustrates whether the soil is going to be a $CH_4$ source or sink at *B_methanotrophs* to *B_methanogens* ratios different from these equilibrium curves. We will modify Figure 7 in the revised manuscript with clear marks of $CH_4$ source and sink: $CH_4$ sink above the plotted lines, and $CH_4$ source below the plotted lines.

$V_{max,\ oxi}$ and $V_{measure,\ pro}$ were obtained from rates measured at three temperatures in soils from the FCP transition zone, as this layer exhibited highest $CH_4$ production and consumption rates. By fitting measured rates at three different temperatures with an exponential function, we further estimated the biomass ratio in response to temperature changes. Only the initial $CH_4$ and $O_2$ concentrations are needed for assessment of methane balance in the given soil. No temporal scale is included in Figure 7. We will clarify the calculations in the revised manuscript.

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

The measured $CH_4$ concentrations from HCP permafrost were mostly below the detection limit of our gas chromatograph with flame ionization detector. We believe this is mostly due to the overall low microbial activity from the HCP permafrost, also measured as $CO_2$ production.

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

We assumed zero net $CH_4$ production to demonstrate the possible uncertainties associated with temperature increase and the sensitivity to different ratios of methane producing and consuming microbes (Figure 7). This simulation is a discussion point used to support our point that more accurate representation (and measurement) of methanotrophs and methanogens biomass is needed. We will clarify this simulation, as described above.

*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.*

We will add an additional paragraph discussing how to use these incubation results in mechanistic methane models.

*Technical corrections*
*P2, L27: "huge" is too imprecise*

We will provide a more quantitative assessment of the differences in the revised manuscript.

*P2, L29: "deeper" than what?*

We will add specific depths in the revised manuscript.

*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".)*

We will omit "fluctuations" in the revised manuscript.

*P3, L25: Respond more "rapidly" or more "strongly"?*

We will replace "rapidly" with "strongly" in the revised manuscript.

*P13, L4: "disparately" is the wrong word here.*

We will change it to "disproportionately" in the revised manuscript.

*P13, L23-24: What is meant by "temperature profile"?*

We meant "in response to temperature change". We will rewrite that sentence in the revised manuscript.