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Biogeosciences Discussions

Interactive comment on "Methane oxidation potential of the arctic wetland soils of a taiga-tundra ecotone in northeastern Siberia" *by* Jun Murase et al.

Anonymous Referee #2

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Interactive comment on "Methane oxidationpotential of the arctic wetland soils of ataiga-tundra ecotone in northeastern Siberia"by Jun Murase et al.

The study by Jun Murase and coworkers addresses the aerobic methane oxidation potential of differently vegetated wetland soils along the taiga-tundra transition near Indigirka River. The climate feedback through mobilized carbon from a warming Arctic is still an important field of research. Biological methane oxidation can reduce the flux of soil derived methane substantially. Quantifying and projecting the biological methane filter is therefore important. The work by Murase et al addresses methane oxidation potentials in tundra ecosystems, however, I am concerned there is very little novelty and

gain in knowledge through this study. The complexity and heterogeneity of methane oxidation potentials in Siberian Arctic tundra including the response to temperature has been reported before (for example Liebner et al., 2009; Knoblauch et al., 2008; Osudar et al., 2016). The temperature dependence of methane oxidation potentials provided here is appreciated and as such provides some interesting new data. However, on its own this is not sufficient for a full research paper at Biogeosciences. In addition to the lack of novelty, I have some severe concerns with regard to the experimental design. First of all, different sampling strategies were applied in different years. Besides, it is not clear where exactly sampling took place, what replication was etc. To me, the collection of data of this manuscript appears somewhat random. Further, what is the scientific reasoning of coal and mineral amendment conducted here? An introduction to this is missing completely. Many controls of methane oxidation (oxygen, methane concentrations, temperature, ammonia..) have been studied before but I have never come across potential limitations associated with minerals? Further, the authors here state on no effect through CH2F2 (specific inhibitor of methane oxidation) on the process. However, the concentration applied in the field seems random. Where there any tests conducted to determine optimum concentrations? Therefore, the conclusion on no inhibitor effect lacks evidence.

Specific comments:

Abstract:

Lines 1-5: Make two sentences out of it.

Lines 19-21: Under frozen conditions, oxygen diffusion should be too low to sustain aerobic methane oxidation. This statement is too vague here because it is simply based on extrapolations but not on real measurements.

Line 22: Specify which inhibitor was used.

Introduction:

Line 13: The phrase “typically” requires more than one reference.

Line 23: Be more specific here: “..differs spatially and temporally under the influence of different conditions” contains very little information.

Page 5, line 11-12: The hypothesis “methane oxidation of the organic soil may be constrained by limited amounts of mineral nutrients including nitrogen” come out of the blue, there is no introduction to it before. Please include some background guiding to this hypothesis.

Materials and Methods:

The chapter on sample collection needs more details on replication.

Chapter 2.2: Was the soil water saturated? If so please state. If not, a DO meter makes no sense I think. At which depths was oxygen measured? Was oxygen continuously monitored?

Chapter 2.3: Was the injected concentration of methane according to in-situ concentrations? It appears a bit arbitrary.

Page 7, line 1: There is no description on the temperature dependence measurement at all. Please include.

Chapter 2.4: The inhibitor concentration seems arbitrary? Were there initial tests conducted to determine inhibitor concentration? How can the authors be sure that soil methanotrophs were fully exposed to the inhibitor?

Results:

Page 10, lines 2-4: Extrapolating methane oxidation measured at 5 °C to subzero temperatures by simple linear regression is an invalid approach. First of all oxygen diffusion is hampered at subzero temperatures. Second, enzyme kinetics are not linear. See concept of cardinal temperatures for growth.

Same page, line 8: This is not a rate, it is a flux.

Discussion:

Page 11, line 1: Lack of methane oxidation in mounds could be caused by too high methane concentrations in your incubations. The mounds are likely colonized by atmospheric oxidizers so that methane concentrations of 5000-10000 ppm would be way too high.

Same page, lines 22-24: Strange wording. English should be corrected throughout the entire manuscript.

Page 12, lines 10-14: This conclusion lacks proof. See comment before. The inhibitor concentration may simply be too low or the inhibitor was not sufficiently distributed.

Page 13, lines 1-6: I do not understand what is meant here, please clarify? The methanotroph inhibitor has no effect on uptake of oxygen?!

Conclusion:

Page 13, lines 14-17: Active methanotrophs in Arctic wetlands have already been studies. See for example Graef et al., 2011, EM Reports.

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