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

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A note upfront from the submitting person: This review was prepared by Daniela Baumann and Manuela Altermatt, both master students in geography at the University of Zurich. The review was part of an exercise during a second semester master level seminar on "the biogeochemistry of plant-soil systems in a changing world", which I organize. We would like to highlight that the depth of scientific knowledge and technical understanding of these reviewers represents that of master students. We enjoyed discussing the manuscript in the seminar, and hope that our comments will be helpful for the authors.

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Murase et al. describe several measurements and experiments that were performed to study the potential methane oxidation rate of the wetland soils of a taiga-tundra ecotone in Siberia. Arctic wetlands form one of the largest sources of atmospheric methane. Climate change may alter methane dynamics, which will potentially lead to increased methane emissions. This study is relevant as it helps to better understand these dynamics. Murase et al. collected peat samples from three different vegetation types and tested whether methane oxidation is temperature dependent, what effect nutrient and charcoal addition have on methane oxidation and examined the depth profile of potential methane oxidation under different conditions. They found that as soon as conditions become favorable, methane oxidation occurs immediately, even in deep frozen layers. Furthermore, temperature dependence of methane oxidation was confirmed and plants' role in methane oxidation was found to be negligible. As temperatures rise in high-latitudes as a result of climate change and conditions become favorable, more methane will therefore be oxidized by methanotrophs.

Murase et al. do a good job at highlighting the importance of their study in the context of climate change. Prior research was considered to outline current knowledge. The poorly understood role of plant-associated methane oxidation was identified as a research gap. The lab methods they used are mostly described in detail, ensuring reproducibility (e.g. section 2.3 "methane oxidation potential of soil samples").

Despite the fact that the relevance of the topic in general and the study area (i.e. arctic wetlands) were convincingly outlined, we found that it remained somewhat unclear what novelty this study offered and how the study was better or more relevant than studies that have previously been conducted. We suggest that the authors explain in a bit more detail how the study, the experiments and the findings are new and why they are important.

Although a gap in the literature was identified, no research questions are explicitly stated in the introduction. In addition, despite mentioning a hypothesis in the introduction, the study does not focus enough on it but rather entails various other experiments

and aspects. Perhaps listing the research questions and hypotheses in the introduction may improve the structure of the paper, especially considering the at times unclear relationship between research questions, hypotheses, methods, results and discussion. For example, the research gap regarding the role of plant-associated methane oxidation in methane dynamics is identified. We assume that the inhibitor experiment is conducted to understand more about these dynamics. However, due to the lack of a specific research question on this topic, the purpose of the inhibitor experiment remains unclear.

Certain aspects of the experimental design are not clear to us. For example, it is not stated how many samples were collected in total (n=?). Why were different layers sampled in different years? (0-10 cm in 2013 and deeper layers in 2015). Why were certain experiments conducted in specific years and/or only in one year? (e.g. dissolved oxygen only done in 2014 and inhibitor experiment in 2014 and 2015). In addition, we were surprised that the methane flux measurement in 2014 lasted 10-15 min while in 2015 it was 18-19 hrs (p.8, line 1). Is this a unit mistake (hrs instead of min)? If not, please explain. As previously mentioned, the entire inhibitor experiment is not clear; why was it done? How was it done? We suggest that section 3.4 be either revised and explained in more detail or deleted. Considering that the charcoal addition experiment is conducted to test the hypothesis, it should be explained more profoundly in a separate section. Moreover, in section 2.3, only the addition of charcoal is mentioned, while the results section 3.1 suggests that nutrient addition and higher concentrations were also tested. Then, in the discussion, it becomes clear that these nutrients and charcoal are transferred from the sea or forest fire, which we think should also be mentioned in the introduction to make clear why this experiment is being done. Separate sections for this experiment in both the method and results could make it easier to follow. Furthermore, the statistical analysis section (p. 8) seems incomplete as well: It is stated that a one-way ANOVA and a Wilcoxon's test were performed. The caption of figure 5 (p. 27) however, indicates that Tukey's honestly significant difference test was also done. We suggest that all statistical analyses conducted be included in section 2.5 (p.

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8).

We do not fully understand Figure 6. Why is flux 1 without CH2F2 and flux 2 is with or without? What exactly is being compared? What does this graph show? Furthermore, the results of the vertical profile (p.9, line 15) and the inhibitor experiment (section 3.4) show high variability, which is not addressed or explained in the text. What influence does this have on the significance of the results?

In general, we found that the paper is rather short, especially the results section. We would encourage the authors to explain the methods and results in more detail and to engage more in the discussion of the results.

While this study is well embedded in the context of climate change, the implications of the results on methane dynamics is not discussed i.e. what is the relevance of these findings for the taiga-tundra ecotone under future climatic conditions? Furthermore, in the conclusion it is mentioned that future studies should focus on the ecology of methane oxidizing bacteria. To avoid confusion, it might be helpful to elaborate on how researchers should do this.

There were several statements that were unclear to us, which we list below:

âĂć P.3, line 12: “the potential methane oxidation rate is typically one order of magnitude higher than that of methane production (Segers, 1998)”. Segers in his paper says however that “potential rates of both methane production and methane consumption vary over three orders of magnitude”. He therefore describes the relationship between methane production and consumption as nondirectional and three orders of magnitude, while Murase et al. cite Segers by saying there is a directional relationship with one order of magnitude. Also, Le Mer & Rogers (2001) state that 60 to more than 90% of CH4 in the anaerobic zones of wetlands is reoxidized in their aerobic zones. We are therefore unsure about the accuracy of the cited statement on p.3. âĂć P.3, line 25: what are polygon depressions and polygon rims? âĂć P.7, line 10: We are unsure as to how “potential atmospheric deposition” fits into this sentence. âĂć P.9, line 15: It

is stated that there was no dissolved oxygen found below 10 cm. In the same paragraph, it is mentioned that immediate methane oxidation was observed in the frozen layer at a depth of 30-40cm. Is this not a contradiction? Does this refer to anaerobic methane oxidation? Please elaborate. âĂć P. 10, line 1 & Figure 5: What is meant with threshold temperature for methane oxidation? âĂć P.11, line 22: Why is methane oxidation compared to carbon respiration? The sentence is long and confusing, maybe splitting it and explaining in more detail could help. âĂć P.12, line 4: What is meant with "potential over the soil depth was estimated at 15°C"? âĂć P.12, line 11: What is meant with "did not affect the methane flux with any rates"? âĂć P 27, figures 5 a, b, c: Why was "ppmv" instead of "ppm" used as a unit?

We suggest proofreading by an English native speaker. Language issues sometimes led to confusion. Below we list some things we found:

âĂć P.2, line 3: "could cause a change in methane dynamics" âĂć P. 3, line 8: "by climatic changes" âĂć P.3, line 11: the result of âĂć P.3, line 19: o "In many studies the potential methane. . . " o "determined using an incubation experiment" âĂć P.3, line 20: In which the collected samples were incubated under high âĂć P.5, line 2: to make sentence more clear → "it is not clear if the measured methane oxidation represents the actual potential of the collected samples or if/whether the methanotrophic. . ." âĂć P.6, line 15, "wooden stick" âĂć P.7, line 6: threshold âĂć P. 7, line 10: "atmospheric deposition" âĂć P. 9, line 3: "of all the" âĂć P.9, line 11: "a lower rate than" âĂć P.10, line 12: "treatment" âĂć P.10, line 16: "in northeastern Siberia" âĂć P.11, line 8: "plotting" âĂć P. 11, line 6: "did not affect methane oxidation" âĂć P.11, line 12: "throughout" âĂć P.11, line 16: "under unfavorable conditions" âĂć P.11, line 23: "mortal" âĂć P.12, line 2: "oxidation" âĂć P.12, line 13: "in both vegetation types" âĂć P. 12, line 15: "deeper layer" âĂć P. 13, line 1: for example "The lack of an effect of the added inhibitor" âĂć P.13, line 8: "in northeastern Siberia" âĂć P. 13, line 9: "under unfavorable conditions" âĂć P. 13, line 16: "in northeastern Siberia"

Some figures are incorrectly labelled in the text: âĂć P. 10, line 2: Fig 5C instead of 4C

âĂć P. 10, line 9: Fig 6 instead of 5 âĂć P. 11, line 25: Fig 5C instead of 4C

References:

Le Mer, J. and Roger, P., 2001. Production, oxidation, emission and consumption of methane by soils: a review. European journal of soil biology, 37(1), pp.25-50. Segers, R. (1998). Methane production and methane consumption: a review of processes underlying wetland methane fluxes. Biogeochemistry, 41(1), 23-51.

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