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Interactive comment on "Geographic and seasonal variation of dissolved methane and aerobic methane oxidation in Alaskan lakes" *by* K. Martinez-Cruz et al.

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General

This manuscript presents results of estimate of methane oxidation in over 30 Alaskan lakes. This is the first such study for this region, and, to my knowledge, is the methane oxidation study involving data from the largest lake set so far. The study is significant for the area of greenhouse gas dynamics in lacustrine ecosystems since it presents the new field method for determining methane oxidation potential, and achieves clear conclusions on key factors controlling methane oxidation in Arctic tundra. The manuscript is well written, and the main conclusions are unambiguously stated. These are the

strong points.

The weak points are two in my view.

1) The authors sampled 7 yedoma lakes and 23 non-yedoma lakes. So, the reliability of statistics on these two sets is different. Are there any estimates on the sufficient n for the statistical estimates accuracy needed? This is especially relevant for yedoma lakes.

2) The authors admit (p.4228, str.15-30) that measuring methane and oxygen concentrations at 0.5 and 1 m depths they likely underestimate the maximal methane oxidation rate in a lake that is typically located in the thermocline. Thus, the authors should precise that they assess methane oxidation rates *in the surface layer*, that is not a good proxy for larger depths and lake as a whole. Therefore, I strongly recommend to look through the text and modify it accordingly, replacing "MO in a lake" by "MO in a surface lake layer" etc. The title of the manuscript should be rewritten as well, e.g.: "Geographic and seasonal variation of dissolved methane and aerobic methane oxidation in the surface layer of Alaskan lakes". Otherwise the authors would have to exclude deep lakes from their analysis where the thermocline is well-developed in summer.

Specific comments

p. 4222, 5-10, It would be useful to indicate if there was a connection between lakes' depths and RWCS. E.g., were deeper lakes more stratified in general?

p. 4223, 6, Remove one "potential"

p. 4223, 15-20, An interpretation of lag phase is relevant

p. 4225, 29-30, This requires more quantitative estimates.

p. 4226, Title, May be, "Limiting factors of MO rates" is better?

p. 4226, 23-26, "is most likely linked to the higher dissolved CH_4 concentration" sounds strangely, since due to eq. (1) it is straightforward to check the contribution of both CH_4

and DO into reduction of potential MO.

p. 4227, Any discussion on maximal MO potential (r_{max}) for yedoma lakes is missing

p. 4243, Fig.6, a, Horizontal axis should have a label k_{S-O_2}

p. 4227, 16-25, $\Delta r/\Delta K_S$ – is that a ratio of two values or just a notation for Δr for a given ΔK_S? In the former case this ratio cannot be expressed in %, and in the latter please denote it as $\Delta r(\Delta K_S)$, i.e. Δr as a function of ΔK_S.

p. 4241, Caption, Replace "3-d" by "3-day"

p. 4242, Figure 5a consists of two small plots. Please enlarge them

p. 4243, Fig.6, I'm totally confused with this Figure. If Δr is a deviation of r from its value at a mean K'_{S-CH_4}, Δr must be 0 when $K'_{S-CH4} = 1$. Moreover, increasing K'_{S-CH_4} above 1 we must get negative Δr (decrease below r corresponding to mean K'_{S-CH_4}). Please clarify what are the values Δr and how they are calculated. Moreover, the authors use $\Delta r'$ notation, whereas I can't find it in the text.

Interactive comment on Biogeosciences Discuss., 12, 4213, 2015.