

21 June 2015

Response to S. Zimov (Referee)

We thank the reviewer for their comments on our manuscript “Thermokarst-lake methanogenesis along a complete talik profile.” Based on their comments and suggestions, we have revised our manuscript in an effort to improve it and address their concerns. Below is our response to each of their comments (reproduced in bold).

During the investigation, the authors assess methane production in yedoma permafrost at +3°C. The emission is several times higher than emission measured earlier in the middle of the lake. I see no any contradiction in the values and trust the incubation results. Even so the lake is young, but the results show that taberal sediments lost almost all labile carbon. Now, methane production from fresh thawed sediments in the lake (temperature is about 0°C) is not high. I believe, when the sediments will warm to +3°C methane production will be the same as authors have gotten in the incubation. I guess that methane emission from the lake surface was underestimated. Methane bubbles could accumulated in the sediment up to 10% of their volume. They release usually during moving a cyclone of very low pressure. Such event may happen not each year.

We thank the referee for their comment concerning our comparisons of CH₄ production potentials measured in our incubations versus CH₄ emissions measured at Vault Lake. As we note in our discussion, the referee is correct in suggesting *in situ* CH₄ emissions from thermokarst lakes differ from CH₄ production potentials measured in incubations due to differences in CH₄ production rates due to sediment temperatures and *in situ* CH₄ consumption, dissolution, and entrapment within a thermokarst-lake system. The referee also suggests that lake sediments may store large quantities of CH₄ that are released during rare extreme-low pressure events. This implies that common methods of ebullition ice-bubble surveys combined with bubble-trap measurements, which are the basis of the emission estimate by Sepulveda-Jauregui et al. (2015), are unlikely to capture these temporally rare, but potentially large emission events from lakes. Thus, the difference between true lake emission and laboratory incubation production potentials may be more similar than reported here. We revised our Discussion section to include this possibility.

We would like to thank the reviewer for the time and thought they put into their comments, which have helped us improve our manuscript. We hope that our revised manuscript will be considered suitable for publication in *Biogeosciences*.

21 June 2015

Response to Anonymous Referee #2

We thank the reviewer for their comments on our manuscript “Thermokarst-lake methanogenesis along a complete talik profile.” Based on their comments and suggestions, we have revised our manuscript in an effort to improve it and address their concerns. Below is our response to each of their comments (reproduced in bold).

Something for the authors to think of is if parts of the method can be written shorter? For example; the measurements of the magnetic susceptibility is very detailed (and rather long) described. It is however not clear to the reader why these measurements are important. It is mentioned in the results, but the discussion is not based on this data and no conclusions are drawn from these results?

We have simplified the methods section by removing our descriptions of calculating wet bulk density using magnetic susceptibility and repeated details for our computing software in the statistics section.

What is the role of allochthonous vs. autochthonous C sources (briefly discussed on L8 – p4881)? Fig. 6 show that CH₄ production normalized per unit Corg also is the highest in the surface sediments (which consists of both allochthonous vs. autochthonous C). Is this only due to that recently deposited is more labile to methanogens and/or is there also a priming/fertilizing effect? What would for example happen if for example autochthonous C was mixed into the incubations of permafrost soils? The authors further touch this at L16-24 (p4884) where they discuss if the high CH₄ potentials in Vault Lake is due that the sediment is a mix of biolabile OM, Holocene aged OC and in lake primary produced C.

We thank the referee for bringing up the interesting question as to whether autochthonous C may provide a priming or fertilizing effect to decomposition of allochthonous C. We are presently conducting additional incubation experiments exploring this hypothesis and will present results in a follow-up manuscript. In the mean time, we have added to our revised manuscript's Discussion section, this hypothesis of a potential priming effect whereby the autochthonous C inputs to sediments stimulate co-metabolism of more recalcitrant allochthonous C.

L9 p4876: Mean depth is not a result of this study and already mentioned in section 2.1

The sentence describing the independent bathymetric mapping has been removed from the results section and the unpublished data have been cited in reference to the maximum and mean lake depth in section 2.1.

L25 p4878: Maybe put brackets around “R”?

Brackets have been added around “R” as suggested by the referee.

L25 p4884: This section is hard to follow, especially since it refers to the next section.

We thank the referee for pointing this out and have edited this paragraph to improve clarity.

We thank the reviewer for the time and thought they put into their comments, which have helped us improve our manuscript. We hope that our revised manuscript will be considered suitable for publication in *Biogeosciences*.