

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 CH<sub>4</sub> production potentials measured in our incubations versus CH<sub>4</sub> emissions measured at Vault Lake. As we note in our discussion, the referee is correct in suggesting *in situ* CH<sub>4</sub> emissions from thermokarst lakes differ from CH<sub>4</sub> production potentials measured in incubations due to differences in CH<sub>4</sub> production rates due to sediment temperatures and *in situ* CH<sub>4</sub> consumption, dissolution, and entrapment within a thermokarst-lake system. The referee also suggests that lake sediments may store large quantities of CH<sub>4</sub> 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*.