9 January 2015

Referee comments on Sepulveda-Jauregui et al. submitted to Biogeosciences Discussions

General comments

This is a great contribution on GHG emissions from northern lakes. I really appreciated reading the manuscript and learned a lot. It is well written, the science is of great value and the methods used seem appropriate. Please see the comments listed below, which adds to a series of suggestions (ideas and technical corrections) noted throughout the pdf document and that I am not reproducing here.

Specific comments

1. p. 13253, line 10: Bastviken et al. 2008 estimated the importance of methane oxidation in 3 lakes, and Thauer et al. 2008 is a paper on methanogeny (maybe this paper suggest oxidation rates? if so, on how many lakes was this done??). I suggest you tone down your sentence, as this cannot be generalised! It depends on so many things and it could be anything from 0 to 100%.

2. p. 13258, line 14: Basically you assume there is NO change in the gas exchange coefficient (boundary layer) over day/week/year… This is an assumption that needs to be discussed and acknowledged. I am convinced there are large variations in turbulence; the wind is certainly not that constant (see below), and heat exchange is likely quite variable, at least over a day cycle. Heat exchange also affects gas exchange (see for ex. Tedford et al. 2014). One thing to note is that Kling et al. 1992 was using monthly wind speed averages calculated from daily averages! This is artificially generating constancy. Another thing is that Kling et al. mentioned that this value of 200 μm is likely overestimated... thus flux would be conservative. This will influence the relative importance of diffusion. Wind speed changing by a factor of 2 (regularly observed) can generate very large changes in flux. If for example wind is higher at night when heat exchange generates turbulence (so during a period when production/consumption ratio differs), the final outcome could differ.

3. p. 13258, line 25: So I understand that you do not consider the autumnal storage flux, even though your lakes seem stratified as described below (p. 13269), but this is acknowledged in the discussion. However, calculations of spring storage flux would be more accurate when comparing late autumn water column mass to late winter mass. I understand there are field logistic constraints, but I think the consequences of your assumptions need to be acknowledged at some point, like at p. 13273, line 1: In addition to the fact that you did not consider summer storage in the hypolimnion released in autumn (as you explain), could this range of values for spring storage flux be underestimated if the starting point to calculate storage is summer (when concentrations are higher) instead of late autumn prior to ice formation (true starting

point for storage; when concentrations could be lower after venting part of the summer production)? Does this make any sense?

4. p. 13263, line 8, p. 13265, line 13, and p. 13270, line 7: Dystrophy needs to be better defined since it turns to be a 'controlling' factor. And I think lakes should be classified as dystrophic OR UO, O, M or E, but not as both. Dystrophy is defined by the low productivity despite high nutrients, because of high DOC that is limiting light to primary producers. As it is, you seem to define dystrophy solely by the richness in DOC. You mention the higher nutrients and higher PP (approximated by the Chla) in dystrophic Yedoma vs NY lakes, but dystrophy should be defined as above. Are you considering other primary producers than plankton (Chla) here? If you consider macrophytes (floating Sphagnum?) in your characterization of primary production, it needs to be clarified.

5. p. 13266, line 7, and p. 13278, lines 7-11: Is this relationship with area holding within each category (Yedoma and Non-Yedoma)? i.e. is it only related to the fact that Y lakes are smaller? Would this hold true considering the same argument as discussed above, that Y lakes have a thaw bulb and that most emissions come from talik thus lake size does not really matter? The relationship should hold for Y category to make this argument stronger: do size really matter or it's only a question of Y vs NY?

6. Discussion about results given at p. 13267, line 5 and p. 13268, line 19: Can you discuss why there is no significant difference in storage flux between yedoma and non-yedoma lakes? Winter CH_4 production in Y lakes is greatly supressed? Can you discuss why yedoma lakes do not store CO_2? Water column CO_2 reduction by methanogens? Was there O_2 left in the water column during the winter?

7. p. 13269, line 19: I think it's necessary to specify the profile shape for Dolly Varden, i.e. an increase from 10 to 12 mg/L from surface to 10m (deep chla maximum? do you know the Chla at this depth?) and then it lowers again to approx 9 mg/L. The way you present this here makes us think there is an increase in DO toward the bottom waters, but this would seem strange to have large contribution of benthic photosynthesis at depth for such a deep lake. We assume (it's written ND) that DOC (TOC) is low for this lake as it was not classified as dystrophic... I wonder how you classified some lakes as dystrophic without TOC, with the eye? (brown color)

8. p. 13273, line 19: Are your temperature measurements appropriate to explore such dependency between CH_4 flux and temperature? What did you use in your statistical analyses: bottom or surface or an average water column T? Did you only use your sub data set where 2 thermistors were placed year-round (to calculate an year average) or you used the whole lake data set with only 2 profiles over a complete year? This is possibly another factor to consider concerning the absence of relationship. Would the

sediment T (where methanogens are located) be more appropriate than water column T?

9. p. 13277, line 23: Indeed, a multivariate statistical analysis, eliminating covariance, would seem more appropriate.

10. I get a feeling of redundancy as I read the discussion, which may come from the fact that you gave too much information in the result section.

11. p. 13277, line 28: Which means the system would be nutrient-limited, not C-limited?

    p. 13280, line 14: Is water column primary production truly an OC contribution or a priming effect, if the system is not C-limited? Can you estimate the C-stock provided by planktonic growth and compare it to thaw bulb C-stock (on a m_{-2} basis)?

END OF REVIEW