

## Comments to the Author,

### Review of “Variability in methane emissions from West Siberia’s shallow boreal lakes”

#### Summary

The manuscript describes a study of CH<sub>4</sub> emissions from boreal shallow lakes (14 lakes), with distinct limnological characteristics across two taiga zones in west Siberia. Authors used static chamber and bubble traps to estimate the total CH<sub>4</sub> emissions. The aim of the study was to compare the magnitude and variability of CH<sub>4</sub> emissions between lake at different zones, and among lakes. To achieve this, CH<sub>4</sub> emissions and environmental controls were used in a new dynamic process-based model. The main idea to use this model is the fact that CH<sub>4</sub> emissions are not predictable, uniform nor spatiotemporal distributed. Therefore, self-organized critically theory (model theorem) can help to assess the spatiotemporal heterogeneity of CH<sub>4</sub> emissions.

The study of CH<sub>4</sub> emissions from lakes is a topic of broad scientific interest as lakes represent important sources of this gas to the atmosphere. Moreover, nowadays lakes represent an important uncertain in the global CH<sub>4</sub> budget and more information is needed to improve the current estimations. The value of this manuscript is that it shows an overview across lakes located in an area scarcely studied but with important quantity of lakes; jointly with the possibility to use dynamic process-based model to improve the knowledge of the CH<sub>4</sub> cycling in lakes. Hence, the manuscript is a potential contribution to Biogeosciences. However, there are some aspects in the manuscript that could be improved to enhance the value of the acquired information.

My major concern is the idea of using the new dynamic process-based model to improve precision on model CH<sub>4</sub> emission among biogeochemical attributes. Because, measurements of spatiotemporal CH<sub>4</sub> emissions and biogeochemical parameters were scarcely done. As Patrick Crill pointed “data without models are chaos, but models without data are fantasy” (mentioned in Nisbet et al. 2014), therefore, poor measurements promote data inconsistency and inability to extrapolate estimations accurately. In this manuscript, the justification to use this model is very subjective, since some parameters were poorly measured and/or taken from literature (e.g. dissolved CH<sub>4</sub> concentration in water surface, ebullition traps, physicochemical sediment information). I would ask them to present a better explanation for the use of that model and the scope of it. Because, as it stands, it makes me think that the lack of actual data collected from the field has influenced the poor performance for individual lakes in the middle taiga region.

## Specific comments

### Introduction

Page 1, row 37-41: New and important manuscripts had published recently about CH<sub>4</sub> emissions of small ponds and boreal lakes, and lake distribution that can be included in the references: Holgerson and Raymond (2016), Wik et al. (2016b), Saunio et al. (2016) and Verpoorter et al. (2014). Besides, according to the new assessment of Saunio et al. (2016), lakes emit a range of 37 to 112 Tg CH<sub>4</sub> per year; so, you can include this current estimation in the text.

Pages 1-2, row 41-44: It should be the first part of this paragraph to follow from general to specific ideas.

Page 2, row 44-46: Could you go deeply in this statement? I recommend Nisbet et al. (2014) and Saunio et al. (2016) literature to improve this idea.

Page 2, row 48: I suggest to include hot-topic references on this point, and even include temperature dependence on CH<sub>4</sub> production in lake sediment assessments. For example: Schulz et al. (1997), Marotta et al. (2014), Yvon-Durocher et al. (2014). Maybe you can remove Kotsyurbenko et al (2001), since it is a study of reactor sludge and competition between methanogens and sulfate reducers bacteria.

Page 2, row 49-50: I can't find in Juutinen et al. (2009) manuscript this statement. They even pointed that CH<sub>4</sub> oxidation was large in a shallow Lake Kevätön before spring-overturn (when they talk about their CH<sub>4</sub> budget). Martinez-Cruz et al. (2015) found a very active methanotrophy in water column of shallow lakes from an Alaska North-South transect. From those lakes and others reported in Sepulveda-Jauregui et al. (2015), 10 shallow lakes presented stratification during summer. Which is a common pattern in ecosystems rich in DOC (see Williamson et al. 1999).

Page 2, row 53-61: About spatial CH<sub>4</sub> emission variability and factors that controls them. I recommend to check new and hot-topic literature. For example: Wik et al. (2016a), Schilder et al. (2016), DelSontro et al. (2011, 2015 and 2016), Hofman et al. (2013), West et al. (2015), Natchimuthu et al. (2015) among others.

Page 2, row 63: You need a better connection to link in previous paragraphs about CH<sub>4</sub> dynamics in lakes and the regional study.

Page 2, row 82: I would recommend to include some studies previous mentioned from DelSontro's group since they have interesting approaches to study bubbling variability.

## Material and Methods

Page 3, row 102-111:

Tables

Table 1. Could you give a range of these values instead means? Why data are from 1979 to 2007? Additionally, I think there are few information on this table, therefore, I recommend to include more climatic characteristics, or just mentioned it in the text and avoid a poor Table information.

Table 2. If you measure different sections and or sites, you can give a range and/or the variability in each data reported.

Text

How do you define humic lake? This information is missing here or in Table 2. Moreover, I cannot see how you determine trophic state mentioned for some lakes, moreover, in others lakes I don't have idea of the trophic state and the method used to determine it. Finally, sediment information needs to be acknowledged.

Page 3, row 116-117: What is the advantage to use a "rubber" boat to prevent any influence on the lake vegetation and sediment?

Page 3-4, row 120-132: How do you store the syringes? I mean there is a strong possibility of leaks and permeability with these syringes type. Did you have a control to check this problem? Additionally, how do you divide the syringes for CH<sub>4</sub> and CO<sub>2</sub> measurements? You may indicate the number of measurements per sampling point for each gas.

Page 4, row 133-135: It is very confusing to me, please organize the idea and include more information. For example, headspace volume and water volume, concentration of CH<sub>4</sub> "known", where gas sample was stored.

Page 4, row 141-143: I am not convinced of this statement, since shallow lakes and ponds in boreal regions has been presented stratification (e.g. Bouchard et al. 2015, Sepulveda-Jauregui et al. 2015). Additionally, you need to discuss deeply about single daytime measurements and possible bias in the flux estimates. Bastviken's and co-researchers are working nicely in this topic (Wik et al. 2016a, Schilder et al. 2016, Peixoto et al. 2016, Natchimuthu et al. 2015, Natchimuthu et al. 2014, among others).

Page 4, row 148-151: I think, a cite is not reliable to support such statement of comparing between Australian with Siberian lakes. Moreover, you can't justify your statement of "no store flux in your lakes", when your study covers only single day measurement in summer. What happen in spring turnover? If you have humic lakes and well protected by forest, then they may present a stratification period in warm summers.

Page 4, row 159-161: Please check the sentence meaning.

Page 4, row 161-163: Which was the device used to collect the water samples?

Page 5, row 170: What were the trace metals measured?

Page 5, row 191: Figure 2 and Model Structure. Oxygen production in the model is not considered (A2 equation and discussed in Page 17, row 657-658), however, you could explain better the reason and avoid like this statement: "no data about solar radiation is available". Why is not important O<sub>2</sub> in CH<sub>4</sub> oxidation (aerobic I think)? Why is primary production minimal in this model?

Page 5, row 202-203: You didn't measure pH in sediments and therefore you are overinterpreting with the water pH results. Therefore, it could influence in the idea to use pH in the model. I pointed this because, sediments contains important quantities of pH regulators, so, pH in sediments is commonly higher than pH in the water column (in acidic lakes). For example, in studies of CH<sub>4</sub> cycling in an acidic bog lake in Germany (divided in four sections), pH in sediments from the acidic section was ranged from 5.9 to 6.0 in the first 20 cm, while pH in the water column was ranged from 4.2 to 4.6 (Casper et al. 2003).

Page 5-6, row 208-210: Maybe you can refine this values with the studies made in sediments by Flury et al. (2015).

## **Results and Discussion**

Page 7, row 258: is sample size enough to use two-sample Kolmogorov-Smirnov test?

Page 7, row 292: Please, add the references.

Page 8, row 228-331: This statement is out of the scope since you didn't study plant productivity. NPP is not described in the text.

Page 9, row 337-343: Ebullition traps were used 80% in ST and 30% MT of the lakes, so, you need to acknowledge that your data contains important uncertainties. As mentioned above, please review Bastviken's research and DelSontro's research about the spatial variability and distribution of the ebullition (even you see Anthony et al. 2010, Anthony and Anthony et al. 2013). Are your traps enough to be representative of the CH<sub>4</sub> ebullition pathway? Additionally, please indicate the similarity order between your ebullition data and Repo et al. (2007).

Page 9, row 444: This sentence is confuse, please rephrase it.

Page 10, row 384-409: Flury et al. (2015) study can enhance the idea in this discussion section.

Page 12, row 486: West et al. (2015) and DelSontro et al. (2016) studies can enhance the idea in this sentence.

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