Dear Prof Stoy

Please find our response to reviewers comments. We are grateful for the reviewers comments and we agree to substantial changes to the ms and the model in response to the valuable input from the two reviewers. Our response to the reviewer's comments are detailed point by point below.

Yours sincerely

Response to reviewers comments

RC1 Anonymous referee #1

<…>

I also recommend the language check by the native English speaker.

Thank you for this recommendation, which has prompted further thorough investigation of the text by the manuscript's one co-author who is also a native speaker of English. The text will be significantly reworked for clarity and grammatical precision in a number of places.

<…>

Specific comments

Methods

- Page 3, row 117: "Day time emissions" could be expressed more specifically. In the previous sentence you say that total field measurement time varied from 4 to 10 hours. It is not totally clear, did you visit each lake ones, and spent in average 6 hours there? More useful would be to know what is "daytime measurement" 10-16 (6 hours)? 12-18 (six hours)? You could say e.g. all measurement were carried out between 9am and 6 pm. Or did you visit the same lake several times?

No, we visited all lakes one time. We will clarify it as you have suggested.

- Page 3, row 117: I would put CH4 first, it is bit confusing that you mention CO2 before CH4, the whole manuscript is anyway about methane. You could also write here that CO2 was measured as a background information.

We will do it.

- Page 4, row 149: The whole idea of comparing West Siberia lakes with lakes in South Eastern Australia seems rather strange. Same "West Siberian climate is more similar to the Australian autumn." Do you mean West Siberian summers resemble Australian autumns?

Yes, we try to say, that weather conditions during summers in Western Siberia are the same as during Australian autumns. There are a lot of cool windy cloudy days and almost no calm days with zero cloudiness. We will try to clarify it in the paper text.

- Page 4, row 159: Do you mean 10 cm above sediment?

Yes, sorry for mistake.

- Page 4, rows 159-160: Please add here all the abbreviations you use later in the text.

We will do it.

- Page 5, row 170: You could specify trace metals, the correlation between CH4 and Cu comes out of the blue for the reader later in the Results section, because Cu has not been mentioned previously.

We will do it.

- Page 5, row 190: You could shortly explain here which are the "well-known controls". E.g. the first two sentences from Appendix A (Page 15, row 558-561) could be here instead of being in the Appendix.

We will add this information.
- Page 6, row 226: I would put this paragraph first or at least somewhere in the beginning of the section. Now there are lengthy explanations before you actually say the main point of the chapter.
This paragraph will be replaced.

Results
- Page 6, row 243-244: You have not explained the abbreviations EC or Eh previously.
We will fix it: EC is electrical conductivity, Eh is oxidation-reduction potential.

- Page 7, row 266-267: I would delete the last sentence. I don't think it is necessary to tell were you are going to discuss the results.
We will remove this sentence.

- Page 7, row 268: This is rather surprising considering that there are significant relationships in the simple regression analysis. I'm not familiar with Statistica, but the in the softwater I have used, multiple regression gives the similar result as the single one. If only one variable shows significant relationship, the multiple regression procedure includes only that variable. I think you could explain the multiple regression method in more detail.
We just wanted to say that we cannot find significant relationships with two or more independent variables. We will correct this sentence.

Discussion
- Page 8, row 321: According to the Table 2 there are considerable pH differences between MT and ST lake populations, with significantly higher pH in ST lakes. In the Figure B1 you present the optimum pH for the methane production. Although you say in the appendix that you have observed high methane production rates in the very low pH, the reader anyhow sees the Figure and the pH difference between lake populations. I think you should discuss the influence of pH differences between the lake populations in this section.
We agree with the referee, that we should discuss influence of pH differences between the lake populations more consistently. Strictly speaking, we have already pointed out that there are lakes with low pH in ST and for these lakes model shows relatively good performance. That's why poor model performance for MT lakes cannot be explained by pH differences between zones. There are some literature data that lakes with different pH have different pH optimums for methanogenesis (Dunfield et al., 1993; Segers, 1998), but it can explain not more than 20-30% difference and not 10 times, as we see in the model. Therefore we suppose that at the moment pH differences presented good enough in the model.

- Page 8, row 330: add: Net primary production (NPP)
We will fix it, thank you.

- Page 9, row 348: I'm wondering, if the both wetlands are acidic, why are there considerable pH differences between MT and ST lakes?
Only for these two wetlands data about MMPR are available. It doesn't reflect the general variability of pH values in wetlands of these regions. In general, MT lakes statistically are more acidic than ST lakes because in MT almost all lakes are linked to the wetlands and nearly all wetlands in MT are acidic. In ST variability of lake trophic state is much higher. Some ST lakes have ground water supply, and hence are less acidic. This high ST lakes trophic state variability (in comparison with MT) can be explained by more pronounced relief and higher level of nutrient supply.

- Page 10, row 379: This could be explained in more detail, since Cu was the only element correlating with CH4 in your data. I think you could already in the Introduction tell a little about what is known about controls and inhibitors of methanotrophy and methanogenesis.
We agree to add some information about elements, controlling main microbial processes of the methane cycle to the Introduction. Detailed explanation of possible Cu influence will be added too. Unfortunately, we don't have any data to check the importance of this explanation, but we can declare it as a hypothesis.

- Page 12, row 497: What do you mean CO2 and CH4 fluxes almost the same? Methane fluxes are about 100 times higher according to Table 4? I think you mean that there is very little variation between MT lakes in both CO2 and CH4 fluxes?
Yes, we just want to say that variation between MT lakes in both CO2 and CH4 fluxes is very little and not higher than errors of flux measurements. We will correct this phase.

- Table 2 does not include all the water quality variables that you measured. It would be nice to know how e.g. Cu varies, because it correlates with CH4.
We will add this information. It was not included only because resulting table was very big.

- Table 6: There are empty rows in the first "Reference" column. What are the numbers presented in the next columns?
It is because information about more than one lake was borrowed from several papers (namely Juutinen et al., 2009, Repo et al., 2007 and Bastviken et al., 2008). We will try to remove repeating climate information to show the reader that information in two or three rows was borrowed from one source.

RC1 Anonymous referee #2

Summary
<…>
My major concern is the idea of using the new dynamic process-based model to improve precision on model CH4 emission among biogeochemical attributes. Because, measurements of spatiotemporal CH4 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 CH4 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.
We agree with referee, that obtained data have several gaps. But we think that these gaps should not lead to overinterpretation of the model. We believe that this model is not a fantasy because we try to rely on basic principles and well-known dependencies. Partly we try to check how we can explain methane variability using parameters from literature. We do not use any calibration or selection of parameters and use average values where it was possible. We think that this approach can show us possible gaps in our knowledge about methane cycle in shallow boreal lakes.

Specific comments
Introduction
Page 1, row 37-41: New and important manuscripts had published recently about CH4 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), Saunois et al. (2016) and Verpoorter et al. (2014). Besides, according to the new assessment of Saunois et al. (2016), lakes emit a range of 37 to 112 Tg CH4 per year; so, you can include this current estimation in the text.
Thank you for very useful references, we will use it in the paper text.

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

Page 2, row 44-46: Could you go deeply in this statement? I recommend Nisbet et al. (2014) and Saunois et al. (2016) literature to improve this idea.
We did not know about these new papers and will use these papers about lakes as hot-spots.

Page 2, row 48: I suggest to include hot-topic references on this point, and even include temperature dependence on CH4 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.
We will do it, thank you very much.

Page 2, row 49-50: I can't find in Juutinen et al. (2009) manuscript this statement. They even pointed that CH4 oxidation was large in a shallow Lake Kevätön before spring overturn (when they talk about their CH4 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 Williamnson et al. 1999).
We will change it. Probably we lost sense rewording the phrase.

Page 2, row 53-61: About spatial CH4 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.
Thank you for useful references, we will use it in the paper text.

Page 2, row 63: You need a better connection to link in previous paragraphs about CH4 dynamics in lakes and the regional study.
We will try to provide it and try to put more attention to fact that there is lack of papers about spatial methane flux differences on the regional scale. And it is especially interesting in Western Siberia, where almost whole region is covered by numerous lakes.

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.
We will use it.

Material and Methods
Page 3, row 102-111:
Tables
Table 1. Could you give a range of these values instead means?
Yes, we will do it.

Why data are from 1979 to 2007?
We will add more fresh data.

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.
We will mention this information in the text.

Table 2. If you measure different sections and or sites, you can give a range and/or the variability in each data reported.
We think that it would lead to the overload of the table and is not informative. But if referee thinks it would be better for paper, we will do it.

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.
We determine trophic state according to (Wetzel, 2001) based on phosphorus and sulfate concentration. Sediment information will be added.

Page 3, row 116-117: What is the advantage to use a "rubber" boat to prevent any influence on the lake vegetation and sediment?
Of course, it is not important from what material boat was made. We will remove word "rubber".

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?
CH_4 samples stored in salt boiled water. CO_2 samples were analyzes in 1-4 hours after sampling, we did not store it. Before the beginning we checked the intensity of leakages. We will add this information.

You may indicate the number of measurements per sampling point for each gas.
We will add this information. We have 4 samples for each flux calculation, each sample was analyzed in three replicates.

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_4 "known", where gas sample was stored.
We will do it.

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).
We will add this discussion.

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.
We agree and will rewrite these sentences.

Page 4, row 159-161: Please check the sentence meaning.
We think everything is correct except "10 cm below sediment depth". It should be "above".

Page 4, row 161-163: Which was the device used to collect the water samples?
Long plastic tube. We will add it.

Page 5, row 170: What were the trace metals measured?
We will add this information.

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 O2 in CH4 oxidation (aerobic I think)? Why is primary production minimal in this model?
We will add these calculations to the model. We included simple model of oxygen production. After calculations we can say it does not change the result, because our shallow lakes were saturated with oxygen. But from general principles it was better to do.

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 CH4 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).
We will add new data about sediment water pH, obtained in 2015. They are sparse but it is better than nothing.

Page 5-6, row 208-210: Maybe you can refine this values with the studies made in sediments by Flury et al. (2015).
Thank you very much for this reference, it proofs previous estimates.

<u>Results and Discussion</u>
Page 7, row 258: is sample size enough to use two-sample Kolmogorov-Smirnov test?
We don't find a limit for this test.

Page 7, row 292: Please, add the references.
All references are given further in this paragraph after row 292. First sentence just announces it.

Page 8, row 328-331: This statement is out of the scope since you didn't study plant productivity. NPP is not described in the text.
Yes, but we don't use it in the paper for any calculations, it is a qualitative statement. We just wanted to proof our idea with this reference.

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 CH4 ebullition pathway? Additionally, please indicate the similarity order between your ebullition data and Repo et al. (2007).
We will do it.

Page 9, row 444: This sentence is confuse, please rephrase it.
We will do 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.
We will try to include these high-level ideas to the paper text.