

## ***Interactive comment on “Revisiting factors controlling methane emissions from high-arctic tundra” by M. Mastepanov et al.***

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### General comments

This manuscript presents very interesting data on inter-annual variability of CH<sub>4</sub> and CO<sub>2</sub> fluxes from a high-arctic wet tundra site. Multi-annual datasets of CH<sub>4</sub> from high-arctic tundra are still scarce, and thus the presented data is highly valuable for improving our understanding of the complex network of environmental factors and processes that control CH<sub>4</sub> fluxes from permafrost-affected soils of tundra ecosystems. Such understanding is crucial for appropriately assessing possible future responses and feedbacks of the arctic carbon cycle to climatic changes. The manuscript shows that the temporal CH<sub>4</sub> flux dynamics and the likely driving factors are considerably different in the studied permafrost-affected ecosystem compared to other wetlands that were

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previously studied and thus demonstrates that our knowledge about the arctic carbon cycle is still rather limited. The authors describe their measurements methods and results in high detail and extensively discuss their findings. Thereby, they share many interesting thoughts about possible biogeochemical and soil-physical mechanisms that could explain the observed temporal dynamics of CH<sub>4</sub> fluxes in the studied (and other) permafrost-affected wetland(s).

However, I think that the manuscript's analytical scrutiny needs to be improved. I would recommend starting this article with a set of more defined research questions or hypotheses and then arranging the data analysis and discussion so that these questions/hypotheses are addressed step by step (as was also suggested by Nigel Roulet). Now, the manuscript suffers from a very general statement of the objectives of the presented study (“...obtain and analyse a multi-annual dataset.”) and a somewhat lengthy discussion appearing partly too speculative. In this process of straightening the data analysis and discussion, the manuscript can be considerably shortened to become more concise. Besides compressing the text, also some figures can be omitted (e.g., Fig. 6 and Fig. 7; maybe Fig. 11). On the other hand, I think that the authors should include other published studies on CH<sub>4</sub> fluxes from permafrost-affected landscapes in their discussion (e.g., Wille et al., 2008, *Global Change Biology* 14(6), 1395–1408; Parmentier et al., 2011, *J. Geophys. Res.*, 116, G03016; Sturtevant et al., 2012, *Biogeosciences*, 9, 1423–1440). Wille et al. (2008) and Sturtevant et al. (2012) report CH<sub>4</sub> fluxes during autumn refreezing of soils but do not report autumn pulses of CH<sub>4</sub> and CO<sub>2</sub> emissions. A list of further specific comments is given below.

The English writing has to be significantly improved before re-submission. There are many orthography (especially comma placement), word order and style issues that need to be corrected. Comments on these language issues are provided in the list of technical comments below. However, I cannot give in this review a complete list of all the errors as they are unfortunately too many. I strongly recommend that the manuscript should be given to a native speaker for proof-reading.

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I recommend the manuscript of Mastepanov et al. for publication after major revisions addressing the problems raised in my and the others' reviews.

Specific comments:

Page 15854, line 3: "High latitudes" are typically defined in a much broader way. In many studies, everything closer to the pole than 60° is taken as "high latitudes". In this sense, it cannot be said that CH<sub>4</sub> emission studies from high latitudes are scarce since there are many boreal peatland CH<sub>4</sub> studies. Maybe just write "...carried out in the Arctic..."

Page 15855, line 16: "What kind of "atmospheric data"? Please be more specific.

Page 15857, lines 1ff: Was the chamber equipped with a pressure equilibration vent? Hutchinson and Mosier (1981), Soil Sci. Soc. Am. J., 45, 311–316.

Page 15857, line 10 to page 15858, line 7: I like this detailed description of data availability and non-availability. It gives a good impression on what this dataset actually looks like. I think that it would be good to additionally provide information on the reasons of equipment failures. This could be valuable information for scientists conducting or planning to conduct similar investigations under harsh Arctic weather conditions.

Page 15858, lines 8-13: Was the linear regression applied to all five minutes of CH<sub>4</sub> and CO<sub>2</sub> concentrations measurements? How was the nonlinearity in the concentration-over-time data dealt with, which is at least for the CO<sub>2</sub> data to be expected if five minutes of data are used. Was nonlinearity in the gas concentration-over-time data a problem?

Page 15858, lines 14-17: Please give some more details on this approach to estimate ebullition fluxes. This is not standard and should be described in more detail. How are ebullition events counted? Do you use a specific algorithm for this?

Page 15858, lines 20-22: What do you mean with "visual integration"? Why gaps were not filled?

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Page 15859, line 27: It is not clear for me what you mean with "< 1 °C precision" in this context.

Page 15860, line 1: Please specify if the start or the end of the zero curtain period is the starting point for the freezing period.

Page 15860, lines 2-3: Can you estimate when the time point of freezing of the entire active layer is reached, e.g., from your soil temperature measurements? Are the automated soil temperature measurements continued over the winter?

Page 15860, line 4: "I find the term "Post-season" too vague. Which season? Thaw season? Growing season?"

Page 15861, lines 13-15: Please clearly distinguish "soil thaw depth" and "active layer depth". The active layer depth is the depth of the layer that in parts of the year is frozen and in parts of the year unfrozen. The development of soil thaw depth over one thaw season does not mean that the active layer depth increases over the season. The active layer depth can only be determined over at least two years as the mean maximum soil thaw depth of several (minimum 2) years.

Page 15863, line 9: Do you use the term "ecosystem production" (NEP) synonymously with "net ecosystem exchange" (NEE)?

Page 15864, line 8: It does not become clear what you mean with "real" ebullition event.

Page 15865, line 4: Soil does not "melt", it "thaws".

Page 15866, lines 15-16: Peat and vegetation have never "negative weight". They may have some buoyancy but this is not equal to negative weight, I would say.

Page 15866, line 26: Instead of "permafrost melt" better: "lowering of the permafrost table".

Page 15866, line 28: "thawing" not "melting"

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Page 15869, lines 10-11: Here, a comparison with other published CH<sub>4</sub> flux studies from high-latitude ecosystems would be appropriate; e.g., Wille et al., 2008, *Global Change Biology* 14(6), 1395–1408; Parmentier et al., 2011, *J. Geophys. Res.*, 116, G03016; Sturtevant et al., 2012, *Biogeosciences*, 9, 1423–1440 (see also general comments).

Page 15871, lines 11-23: Again, please compare your results also to CH<sub>4</sub> flux studies from Arctic ecosystems.

Page 15871, line 24 to page 15872, line 11: In this paragraph, you should also discuss the potential of biased flux measurements during times of low atmospheric turbulence (often in nighttime) as discussed by e.g.; Schneider et al., 2009, *J. Geophys. Res.* – *Biogeosciences*, 114, G03005; Juszczak et al., 2012, *Polish Journal of Environmental Studies*, 21(3), 643-658 ; Lai, et al., 2012, *Biogeosciences*, 9, 3305-3322.

Page 15871, lines 20-21: But later you elaborate on the hypothesis that the autumn pulses are due to gas bubble releases. How does this fit together?

Page 15871, lines 21-22: What are these physical reasons? This is not clear to me without further explanation.

Page 15871, line 23: The aerenchymatous transport is not through the vessels of the vascular plants; thus it is no vascular transport.

Page 15873, lines 11-12: Why not fitting a model that represents gross photosynthesis and ecosystem respiration in one model equation? From the automatic chamber system, there should be enough data points available to constrain such a model. Examples would be: Lasslop et al., 2010, *Global Change Biology*, 16(1), 187–208; Lund et al., 2012, *J. Geophys. Res.*, 117, G02001; Parmentier et al., 2011, *J. Geophys. Res.*, 116, G03016.

Page 15874, lines 5-8: It does not become clear to me how your study shows that there is no strong “CH<sub>4</sub> flux regulation by methanotrophic activity”. Please expand.

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Page 15874, lines 18-19: How can we know if this is "regularity" or stochasticity?

Page 15877, line 7: “hypothesis” instead of “theory”

Page 15878, lines 11-21: I am not sure if I totally understand Figure 11. The figure suggests that the columns and the arrows are comparable quantities. But in the text you write that the CH<sub>4</sub> storages indicated by the columns are just scaled according to the maximum peak emission of the corresponding year. I do not see how peak emissions and storages are physically/mathematically transferred in each other. Without an attempt for quantification of the storage sizes, comparison with storage changes due to emission events is questionable.

Page 15880, line 2: Photosynthesis rates vary strongly over the day with strong diurnal variability of shortwave radiation even under polar day conditions. Please adjust your reasoning here.

Page 15899: Figure 11. This figure needs an expanded caption that better explains the presented ideas. It is also important to indicate that the storages are actually not measured or calculated storages but re-scaled peak emission values.

Technical comments:

Page 15854, line 3: “carried out” instead of “carried on”.

Page 15854: lines 8-9: Awkward sentence; I suggest: “The start of the growing season and the increase in CH<sub>4</sub> fluxes were. . .”

Page 15854, line 16: I suggest “cumulative” instead of “accumulated” (to better distinguish between a physical accumulation and a mathematical summation).

Page 15854, line 22: Remove comma after “gases”.

Page 15854, lines 25-26: I suggest re-writing like: “. . .conventionally known factors controlling methane emissions are. . .”

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Page 15855, lines 10-11: I suggest “. . .major questions regarding our . . .”

Page 15855, line 12: better “strengths” instead of “strength”

Page 15855, line 16: Place a comma after “methane”. There are many occasions like this in the text where you should place a comma after an introductory phrase or clause. Please check the whole text with respect to this issue. I will not list all places here.

Page 15855, line 17: Place a comma before “and”. There are many occasions like this in the text where you should place a comma before a coordinating conjunction (and, or, but, nor, yet, for, so) that separates two independent clauses. Please check the whole text with respect to this issue. I will not list all places here.

Page 15855, line 20: Hyphenate “long-term”.

Page 15856: Remove “characteristics”.

Page 15856, line 23: Insert “m”: “0.6 m x 0.6 m” to be mathematically consistent (equals 0.36 m<sup>2</sup>).

Page 15857, line 7: Insert the definite article “The” before “active”. There are many occasions like this in the text where the usage of definite and indefinite articles is not optimal. Please check the whole text with respect to this issue. I will not list all places here.

Page 15858, line 12: “based on” instead of “based of”. Please check the usage of prepositions throughout the text.

Page 15859, line 4: “a reference” instead of “the reference. . .”

Page 15859, line 8: “differential” instead of “differential”

Page 15859, line 11: “widely used” instead of “wide used”

Page 15859, lines 24-27: I suggest using “zero curtain period” instead of just “zero curtain”.

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Page 15860, line 18: “altogether”: better: “for the average of these three months”.

Page 15860, line 19: Remove “for”.

Page 15861: I suggest: “. . .was lower than in any other year. . .”

Page 15861, line 25: Remove comma before “and”: These two clauses are not independent.

Page 15863, line 5: “Afterwards” instead of “After that”.

Page 15864, line 21: I suggest “time scale” instead of “proxy”.

Page 15865, line 7: Remove comma before “whether”.

Page 15866, lines 14-18: This sentence is awkward and needs considerable re-writing.

Page 15866, line 19: Remove comma before “than”.

Page 15866, line 29: “permafrost table” instead of “upper permafrost bound”

Page 15867, line 28: “. . .shifted with respect to calendar time. . .”

Page 15868, line 25: Place a comma after “however”. Check throughout the manuscript.

Page 15870, line 5: “remainder” instead of “reminder”.

Page 15870, line 9: Remove “in some sense”, very vague wording. . .

Page 15870, lines 10-15: These sentences are difficult to understand. Please try to write these in a way that is easier to understand.

Page 15870, line 24: I suggest “knowledge” instead of “wisdom”.

Page 15871, line 8: Remove comma before “that”. Check throughout the manuscript.

Page 15873, line 6: “collars” instead of “corners”

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Page 15873, line 7: Remove comma before “although”. Check throughout the manuscript.

Page 15873, line 13: Remove comma after “season”.

Page 15874, line 2: Remove comma before “if”. Check throughout the manuscript.

Page 15874, line 4: Please try to write more specific. “progress differently” is quite vague: How different?

Page 15877, lines 18 and 26: “interchange” does not really fit. I think.

Page 15879, line 16: This text line seems to be mixed up.

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Interactive comment on Biogeosciences Discuss., 9, 15853, 2012.