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Interactive comment on “Methane dynamics in warming tundra of Northeast European Russia” by M. E. Marushchak et al.

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This manuscript presents valuable data on CH₄ fluxes from the understudied permafrost region of NE Europe. CH₄ fluxes were measured on the plot scale by closed chambers and on the landscape scale by the eddy covariance approach. The combination of these two approaches is a particular strength of this study. Furthermore, the authors present interesting data on stable carbon signatures of pore water and emitted CH₄ which allows new insights in the processes that are involved in the CH₄ emission.

Author response: We thank the reviewer for highlighting the strengths of our work presented in this paper.

An interesting scenario analysis of land cover changes due to climate warming and per-

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mafrost degradation demonstrates the potential effects of such climate-induced landscape changes on CH₄ fluxes. However, the projection of much higher CH₄ emissions due to higher temperatures on the basis of a Q10 temperature sensitivity parameterization derived from seasonal flux data appears questionable to me (see specific comment on P. 13946, l. 22-24 and P. 13956).

Author response: Based on this and other reviewer feedback, we have reconsidered the presentation of the scenarios for future CH₄ emissions, and decided to exclude the projections based on temperature response of CH₄ emission. The use of temperature response function based on data from a single season in order to predict long-term ecosystem response has indeed many uncertainties. Detailed biogeochemical modeling of future CH₄ of the study site is underway, and will be presented in a later publication.

The manuscript is generally well written; however, I found several comma and smaller orthographic errors (see list of technical comments). More importantly, the wording at several places should be improved for the sake of clarity and consistency with scientific terminology (see list of specific comments).

Author response: We thank the reviewer for a meticulous review of our paper providing detailed suggestions for improving the paper quality. We have now addressed all such suggestions.

I recommend the manuscript of Marushchak et al. for publication in Biogeosciences after careful consideration of my comments.

Author response: We thank the reviewer for his recommendation.

Specific comments:

P. 13932, l. 18: Inappropriate wording: A process can discriminate against the ¹³C isotope, which is heavier than ¹²C; however, it cannot discriminate against a high (better than “heavy”) delta-¹³C value, which is the result of the discrimination.

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Author response: The point is well taken. We have now removed the word ‘heavier’ from the line as implied.

P.13932, I. 18: I think that your statements about the reasons for the light emitted CH₄ in the abstract and later in the discussion, respectively, are not really in line with each other: In the discussion, you argue that the emitted CH₄ is light because it is transported from deeper peat layers. Here in the abstract, you argue that the light emitted CH₄ is due to the plant-mediated transport. These are two quite different statements which are both not completely clear for me:

Author response: The description of the stable isotope measurements and data has been expanded and clarified throughout the text with changes in the following paragraphs: ‘Abstract’, ‘1.0 Introduction’, ‘2.4 Isotope analysis of emitted and porewater CH₄’, ‘3.3 Isotopic signature of C-CH₄ in emission and porewater’, and ‘4 Discussion’. Seasonal variability in stable carbon isotopes of CH₄ in porewater and emission has now been highlighted in the new Figure 6. The Figure 7 with more detailed depth profiles from 2008 has been added to the MS. The reviewer’s specific questions pertaining to the isotope data and our response are given below.

To the first argument (discussion): Do you have indications for lighter CH₄ in deeper peat layers? This is often the case since there you find CH₄ that is not influenced by the ¹³C discrimination by CH₄ oxidation like in the upper peat layers (or maybe also a higher contribution of hydrogenotrophic methanogenesis to CH₄ production as opposed to acetoclastic methanogenesis), but did you sample also the deeper peat layers at your site?

Author response: Lighter CH₄ was found in the deeper peat layers than in the shallow porewater. This is seen in the data from permanently installed gas collectors at 5 cm and 30 cm and further supported by more detailed depth profiles of C isotope composition of porewater CH₄ sampled occasionally (please refer to the new Figure 7).

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To the second argument (abstract): Do you have discrimination by diffusional transport in mind? Diffusion across the rhizodermis? In the aerenchyma? Does CH₄ also get lighter by CH₄ oxidation along this plant-mediated transport?

Author response: Our data suggest that a major part of the CH₄ effluxes was released through plants by passive diffusion via aerenchyma that discriminates against heavier ¹³C isotope. The delta-¹³C of CH₄ emission was remarkably depleted relative to porewater CH₄ (by 6 ‰ relative to CH₄ at 30 cm, by 16 ‰ relative to CH₄ at 5 cm), and lighter than anywhere in the peat profile. Besides the plant transport that preferably removes ¹²C-CH₄ from the peat, the enrichment of CH₄ in the rhizosphere can also be affected by oxidation, or the dominance of acetoclastic methanogenesis; the effect of these processes on stable isotopic composition of CH₄ cannot be separated with full confidence. However, the positive correlation between CH₄ emission and LAI and negative correlation between delta¹³-C of CH₄ emitted and LAI together suggest that the depleted CH₄ emission was caused mainly by the plant transport.

P. 13932, l. 20: Please state here that it was “negatively correlated” with the vascular plant cover.

Author response: Revised as suggested.

P. 13933, l. 8: This statement is too imprecise: The “soils in the northern circumpolar permafrost region” (Tarnocai et al. 2009, GBC) are not equal to the areas of “arctic tundra” (your previous sentence). Furthermore, the estimates of, e.g., Tarnocai et al. (2009) or Hugelius et al. (2014, Biogeosciences) do not refer to “soil carbon” but to “soil organic carbon”. There is also a lot of inorganic carbon in soils.

Author response: Revised as suggested.

P. 13933, l. 22. According to the IPCC (2014), the GWP of methane (without inclusion of climate–carbon feedbacks) is 28 (not 25 anymore).

Author response: The correct value suggested by the reviewer is now included in the

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manuscript.

P. 13933, I. 23: Remove “non-frozen”. There are no permanently frozen wetlands.

Author response: Revised as suggested.

P. 13934, I. 5: Too vague: How high this resolution should be?

Author response: The GHG measurements across ecosystems in general have been made using chambers. Therefore, we are making the case for continuous direct measurements as measured by the EC technique. We have now revised this part to reflect our view more precisely.

P. 13934, I. 9: What do you mean precisely with “ensemble average”? “Ensemble average” of what exactly? E.g., fluxes from equally sized areas of different land cover types within the ecosystem under study? I see the EC flux more like an estimate for a weighted mean of fluxes from different land cover types within the EC footprint (which changes over time), weighted by the area of the land cover types and the footprint probability density function.

Author response: Revised the text to be more precise.

P. 13934, I. 26: Why is the importance of peatlands growing?

Author response: In the past, most global CH₄ budgets have been estimated assuming the arctic grids as representing a uniform mineral land form. Recent studies have highlighted the importance of better, finer land cover classification showing the importance of wetlands/peatlands as major players in the regional c balance.

Author response: As the reviewer has not suggested any action, we have not made any changes to the text.

P. 13934, I. 27-28: Too general: Permafrost temperatures: Average over all permafrost regions? Some or all permafrost areas?

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Author response: Revised as suggested.

P. 13935, l. 5-7: The sentence is quite vague: How can a study like the present one improve these projections? By model validation and better calibration? By including additional biogeochemical processes? Do you think that the model of Anisimov has specific deficiencies?

Author response: We do not think that the sentence is vague at all. Most points which the reviewer has asked are already in the present text. Therefore, we have not made any changes to the text.

P. 13935, l. 12: I suggest “from pedon to landscape” or similar. “Processes” is not comparable to “landscape”. Some processes can act, e.g., on the pedon scale, others act on the landscape scale.

Author response: Revised as suggested.

P. 13936, l. 6-11: Please indicate the distance of Vorkuta and Salekhard from the investigation site in addition to the coordinates here.

Author response: The Salekhard precipitation data has now been replaced by precipitation data from Vorkuta station that is located closer to the site.

P. 13939, l. 21: Did you correct somehow for the CH₄ content in the ambient air that you used as headspace air? Or did you use synthetic air without CH₄.

Author response: Synthetic CH₄ free air was used.

P. 13941, l. 12: I suggest using the unit “g CH₄ m⁻²” for consistency with the units used later.

Author response: Revised as suggested.

P. 13942, l. 2: This is wrong usage of permafrost terminology: The active layer is the layer of ground that is subject to annual thawing and freezing in areas underlain by

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permafrost. It is not the active layer depth does not equal the thaw depth at a specific time during the thaw season. Also, it is not the permafrost which you encounter at the surface before the thaw season starts (Permafrost is ground that remains at or below 0 C for at least two consecutive years). The top soil is affected only by seasonal (winter) frost.

Author response: Revised as suggested.

P. 13942, l. 10-11: What do the “+/-“ signs indicate? Spatial variability between parallels or uncertainty estimates of modelling over the year?

Author response: The error bars indicate the spatial variability between replicate measurements.

P. 13942, l. 20: Temperature of what? Air or soil (at which depth?)

Author response: Revised as suggested.

P. 13943, l. 13-14: Sentence is difficult to understand: How can a high delta 13C value decrease the average delta 13C value?

Author response: We have now modified the text.

P. 13943, l. 17: For clarity better “the porewater CH₄ at 5 cm and 30 cm depth”

Author response: Revised as suggested.

P. 13944, l. 16-20: But it may be appropriate to also mention that your CH₄ fluxes were very similar to the ones measured during June to mid-September in the Lena River Delta by Wille et al. (2008, GCB; 1.87 g m²) and Sachs et al. (2008 JGR; 1.93 g m²). Also, the measurements of Van der Molen et al. (2007) appear very similar to your

Author response: We have now added these comparisons and references in the manuscript.

Technical comments

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P. 13932, l. 12: Hyphenate: “CH₄-emitting”, also ensure consistent use of “CH₄” or “methane”. E.g., on same page, l. 28, you write “methane emitting” (should also be hyphenated: “methane-emitting”).

Corrected as suggested.

P. 13932, l. 20: comma before “and” (new independent clause).

This is not valid any more as the text has been modified.

P. 13932, l. 21: “The mean:” instead of “A mean”

Corrected as suggested.

P. 13932, l. 24, P. 13933, l. 1: Space between “7” and “_C” This is not valid any more as the text has been modified.

P. 13933, l. 7: No comma before “because “(dependent clause)

This is not valid any more as the text has been modified.

P. 13935, l. 1: “composition “of what?”

The text is now made clear.

P. 13935, l. 10: “:environment, which is vulnerable”

The text is now modified.

P. 13935, l. 13: I suggest hyphenating: “EC- and chamber-based”

Modified as suggested.

P. 13936, l. 24: “waterlogged”

Modified as suggested.

P. 13936, l. 26: Insert “the” before “dominant”.

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Modified as suggested.

P. 13938, l. 9: “headspace”

Modified as suggested.

P. 13939, l. 25: better “ambient air samples”

Modified as suggested.

P. 13940, l. 12: Add hyphen: “landscape-scale CH₄ fluxes”, and throughout the manuscript hyphenate two or more words when they come before a noun they modify and act as a single idea (compound adjective), e.g., plot-scale measurements (e.g. p. 13941, l. 17). But: we compare the plot scale with the landscape scale (no compound adjectives).

Modified as suggested.

P. 13940, l. 18. Better “for CH₄ concentration measurements”

Modified as suggested.

P. 13941, l. 17-18: Awkward sentence structure, please revise.

Modified as suggested.

P. 13942, l. 1: Insert comma before “and”

This is not valid any more as the text has been modified.

P. 13943, l. 17: Place comma before “and”

This is not valid any more as the text has been modified.

P. 13945, l. 4: hyphenate “area-integrated”

Modified as suggested.

P. 13945, l. 18: plural “willow stands”

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Modified as suggested.

P. 13946, l. 1: “I suggest “decreasing” instead of “depleting” in this context.

This is not valid any more as the text has been modified.

P. 13947, l. 16: “low-lying”

Modified as suggested.

P. 13948, l. 4: I suggest adding “the” before “CH₄ exchange”

The indefinite article ‘a’ fits better here. So we have now modified this line accordingly.

P. 13948, l. 16: “drawdown” and “expected to be”

Modified as suggested.

P. 13948, l. 23: “the” before “last glaciation”

Modified as suggested.

P. 13950, l. 2: hyphenate “data-based”

Modified as suggested.

P. 13963: Figure caption: “from June until early October”

Modified as suggested.

P. 13965: Figure caption: Hyphenate “warming-induced”

This is not valid any more as we have now decided to remove this figure from the manuscript.

Interactive comment on Biogeosciences Discuss., 12, 13931, 2015.

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