

## Authors' point by point responses to reviewers' comments (indicated in blue below)

### Reviewer 1:

The paper aims to contribute on knowledge of CH<sub>4</sub> emissions in a mosaic of vegetation forming subarctic Russian tundra. Flux estimates by chamber and EC are compared regionally and temporally. Isotopic signatures are used to characterize the relative differences of vascular transport in different vegetation types. QuickBird high-resolution land cover classifications are employed in order to resolve the distribution of vegetation types and the landscape methane emissions, assuming similar characteristics of CH<sub>4</sub> emissions in similar vegetation. Furthermore, a scenario analysis is attempted as part of the Discussion. What if climatic warming, thawing of the permafrost, would affect the relative abundance of wet versus dry habitats? HIRHAM-4 RCM climate output is used to predict a scenario of landscape CH<sub>4</sub> release at the end of 21st century when a 10% increase in coverage of wet habitats may have occurred.

The field work is well done, the setup earlier published along with CO<sub>2</sub> and N<sub>2</sub>O results. Results of CH<sub>4</sub> are enough for the present paper especially when the 13-C isotope ratios are measured. Comparisons between EC tower and chamber-derived flux estimated have been published earlier, but given that such data is sparse in the vast European Russian tundra, the different views to the data are welcome.

Author response: We thank the reviewer for an accurate synthesis of our work and for the kind words on the overall quality of our paper.

Methodology involved in the scenario analysis is not fully described, and leaves the reader a bit confused on how the climatic data is conveyed to the CH<sub>4</sub> flux model (Equation 1).

Author response: Based on this and other reviewer feedback, we have reconsidered the presentation of the scenarios for future CH<sub>4</sub> emissions, and decided to exclude the projections based on temperature response of CH<sub>4</sub> 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 CH<sub>4</sub> of the study site is underway, and will be presented in a later publication.

The authors do not provide sensitivity analyses to support the temporal and regional extrapolations. The nonlinear regression applied has temperature and water table level in its exponential terms. After playing with the model with a range of temperatures and water table levels, it was clear that the model is highly sensitive to temperatures approaching and exceeding 10 degrees Celsius. I recommend that the authors add a statement how much the CH<sub>4</sub> prediction they give is impacted by the sensitivity of the model.

Author response: The point is well taken. Please refer to our response above.

### Minor comments:

Page 13936/lines 17-: Plants are referred to by their genus only. The authors should consider if more accurate taxonomy or adding a table with dominant species composition in each vegetation type would be beneficial also in this paper.

Author response: Dominant plant species of the high CH<sub>4</sub> emitting sites (willow stands and fens) have now been added in the paragraph on site description.

13937/21: As far as the sedges are concerned "plant roots and rhizomes"

Author response: The page and line numbers indicated by the reviewer do not correspond to the issue raised. Therefore, we are not sure how we should respond to it.

13942/8-11 and Fig. 7: The annual CH<sub>4</sub> emissions from the different vegetation types (willow habitats show highest emissions) are slightly controversial compared to what is said in 13947/19 (“fen sites are strongest emitters”). Please clarify.

Author response: As willow stands are dominant on fen like wet environments at the site, willow habitats are also grouped under fens. This point is now made clear in the manuscript.

13944/6: Reference to Table 2 should be to Table 3?

Author response: Thanks. This is now corrected as suggested.

Reviewer 2:

This manuscript presents valuable data on CH<sub>4</sub> fluxes from the understudied permafrost region of NE Europe. CH<sub>4</sub> 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 CH<sub>4</sub> which allows new insights in the processes that are involved in the CH<sub>4</sub> 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 permafrost degradation demonstrates the potential effects of such climate-induced landscape changes on CH<sub>4</sub> fluxes. However, the projection of much higher CH<sub>4</sub> 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 CH<sub>4</sub> emissions, and decided to exclude the projections based on temperature response of CH<sub>4</sub> 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 CH<sub>4</sub> 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 <sup>13</sup>C isotope, which is heavier than <sup>12</sup>C; however, it cannot discriminate against a high (better than “heavy”) delta-<sup>13</sup>C value, which is the result of the discrimination.

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

P.13932, l. 18: I think that your statements about the reasons for the light emitted CH<sub>4</sub> 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 CH<sub>4</sub> is light because it is transported from deeper peat layers. Here in the abstract, you argue that the light emitted CH<sub>4</sub> 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 CH<sub>4</sub>’, ‘3.3 Isotopic signature of C-CH<sub>4</sub> in emission and porewater’, and ‘4 Discussion’. Seasonal variability in stable carbon isotopes of CH<sub>4</sub> 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 CH<sub>4</sub> in deeper peat layers? This is often the case since there you find CH<sub>4</sub> that is not influenced by the <sup>13</sup>C discrimination by CH<sub>4</sub> oxidation like in the upper peat layers (or maybe also a higher contribution of hydrogenotrophic methanogenesis to CH<sub>4</sub> production as opposed to acetoclastic methanogenesis), but did you sample also the deeper peat layers at your site?

Author response: Lighter CH<sub>4</sub> 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 CH<sub>4</sub> sampled occasionally (please refer to the new Figure 7).

To the second argument (abstract): Do you have discrimination by diffusional transport in mind? Diffusion across the rhizodermis? In the aerenchyma? Does CH<sub>4</sub> also get lighter by CH<sub>4</sub> oxidation along this plant-mediated transport?

Author response: Our data suggest that a major part of the CH<sub>4</sub> effluxes was released through plants by passive diffusion via aerenchyma that discriminates against heavier <sup>13</sup>C isotope. The delta-<sup>13</sup>C of CH<sub>4</sub> emission was remarkably depleted relative to porewater CH<sub>4</sub> (by 6 ‰ relative to CH<sub>4</sub> at 30 cm, by 16 ‰ relative to CH<sub>4</sub> at 5 cm), and lighter than anywhere in the peat profile. Besides the plant transport that preferably removes <sup>12</sup>C-CH<sub>4</sub> from the peat, the enrichment of CH<sub>4</sub> 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 CH<sub>4</sub> cannot be separated with full confidence. However, the positive correlation between CH<sub>4</sub> emission and LAI and negative correlation between delta<sup>13</sup>-C of CH<sub>4</sub> emitted and LAI together suggest that the depleted CH<sub>4</sub> 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 manuscript.

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

Author response: Revised as suggested.

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

Author response: In the past, most global CH<sub>4</sub> 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, l. 27-28: Too general: Permafrost temperatures: Average over all permafrost regions? Some or all permafrost areas?

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 CH<sub>4</sub> content in the ambient air that you used as headspace air? Or did you use synthetic air without CH<sub>4</sub>.

Author response: Synthetic CH<sub>4</sub> free air was used.

P. 13941, l. 12: I suggest using the unit “g CH<sub>4</sub> m<sup>-2</sup>” 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 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 CH<sub>4</sub> 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 CH<sub>4</sub> 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 m<sup>2</sup>) and Sachs et al. (2008 JGR; 1.93 g m<sup>2</sup>). 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

P. 13932, l. 12: Hyphenate: “CH<sub>4</sub>-emitting”, also ensure consistent use of “CH<sub>4</sub>” 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”.

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 CH<sub>4</sub> 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 CH<sub>4</sub> 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”

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.