

Biogeosciences Discuss., referee comment RC2  
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## Comment on bg-2021-81

Anonymous Referee #2

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Referee comment on "Field-scale CH<sub>4</sub> emission at a subarctic mire with heterogeneous permafrost thaw status" by Patryk Łakomiec et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-81-RC2>, 2021

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Łakomiec et al. presented a manuscript discussing CH<sub>4</sub> fluxes from a sub-arctic permafrost mire. Experimental conception, data acquisition, and statistical analysis were conducted following current state-of-research recommendations. The authors obtained a data set of high quality and present a well done analysis with novel insights and aspects.

The manuscript is within the scope of "Biogeosciences" (BG) and improves the scientific knowledge in the context of thawing permafrost CH<sub>4</sub> emissions. It is well written and structured. The manuscript is already of good quality, thus I just add a few remarks to consider below. I recommend the final manuscript for publication in BG after minor corrections.

Generally: in l. 58 you first introduce methane (CH<sub>4</sub>), but later you switch randomly between "CH<sub>4</sub>" and "methane" in the text. To provide consistency, please use always "CH<sub>4</sub>" in the text after first mentioning it in l. 58. Please check the same also for other abbreviations you introduced.

l. 99: This sentence might be difficult to understand. I recommend to divide it into two sentences for each first and second area.

l. 124 and others: there is a space character missing between value and unit. Please write "0 °C" instead of "0°C" and check also the other parts of the manuscript regarding that.

l. 126 and later: in many parts of the manuscript you give both air and peat temperatures with 2 decimal places. Is this really justified, considering the uncertainties of the sensors?

l. 153: The intake tube of the LGR analyzer had a length of almost 30 metres, which is a relatively long tubing. Did you carefully check whether the measured CH<sub>4</sub> signal was dampened due to the flow characteristics of the sampling tube? How does the co-spectra look like? Are there any signs for a dampening effect in the high-frequency range, and if possible, did you apply a suitable correction? Please provide a short statement on that in your manuscript.

l. 160: The LI-7200 is an enclosed path analyzer. Additionally, the official notation of the manufacturer is "LI-COR". Please write it consistent in the manuscript.

In l. 257, 316 you write "global radiation", in l. 465, 467, 565 you name it "shortwave radiation". I recommend to write "shortwave incoming radiation" generally in the entire manuscript.

l. 436: You report an average emission of 24 mg-CH<sub>4</sub> m<sup>-2</sup> d<sup>-1</sup> for the eastern sector in wintertime, which is in accordance with Table 6. However, referring to Fig. 4, wintertime emissions at the eastern sector seem to be substantially lower than 24 mg-CH<sub>4</sub> m<sup>-2</sup> d<sup>-1</sup>. Are the mean values, maybe, in Table 5 and 6 the gap-filled ones? a) If yes, please clarify in the table descriptions and in l. 433, l. 437. b) If yes, why does the gap-filled value seem to be substantially higher than the the non-gap-filled data? c) If no, what is the reason for this discrepancy?

l. 637, "Method...": is there a word missing at the beginning of the sentence?

l.699f: You conclude a "gentle increase" of CH<sub>4</sub> fluxes in spring, and a "more rapid decrease in fall". Figure 4 somewhat differs to that finding: I see no difference in increase / decrease ratio for 2016, while for 2014 and 2015 there seems to be a more rapid increase in spring, followed by a less rapid decrease in fall? Am I wrong?

Fig. 1: change m/s => m s<sup>-1</sup>.

Fig. 2: The water table level (WTL) is given in metres above sea level. For what reason? I guess it could be more intuitive to give relative values referencing to the ground level. In l. 164 you introduced a ground level (a.g.l.) baseline - maybe you could do that also for WTL?

Fig. 3, upper panel: To avoid misunderstandings, I recommend to add the information that the red contour lines correspond to the 10 % to 90 % contributions of the flux.

Fig. 4: Shouldn't you change "temp" to "surface peat temperature" in the x-axis label? Additionally, you never use the term "breakout week" in neither text nor the figure itself. Please clarify the figure and/or figure description.

Table 1: Tables are always harder to understand than figures, especially when comparing different years and footprints. I suggest to replace table 1 by a figure with the DOY (1 - 366) on the x-axis, and the years (2014, 2015, 2016) on the y-axis. You then draw the unfrozen periods into the plot, using the color codes (grey = western, green = eastern) of Fig. 4. This makes it much simpler to compare different years and footprints.

Table 7: I guess, "normalization" means that you used temperature-based normalization approach following Rinne et al. (2018) as stated in l.559? This makes sense, however it remains somewhat unclear until reading l. 559 later. Please clarify in the table description and in l. 450 to avoid misunderstandings.

Table 9: Please write the unit "g-C m<sup>-2</sup> yr<sup>-1</sup>" to be consistent with Table 8 and Table 10.

Table 10: The annual emissions of the type "thawing wet surface" is 11 +/- 2 g-CH<sub>4</sub> m<sup>-2</sup> yr<sup>-1</sup>? Do you mean +/- 2.0? Additionally: why is the cell in first column, fifth row, which refers to 28.3 +/- 1.7 g CH<sub>4</sub> m<sup>-2</sup> yr<sup>-1</sup> empty? It is also "thawed fen", I guess?