

Interactive comment on “Technical Note: Comparison of methane ebullition modelling approaches used in terrestrial wetland models” by Olli Peltola et al.

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Comments

This paper does address a subject that is relevant for BG and provides a good overview of existing methane ebullition models. The explanation of the models is clear and background information from the literature is provided. The aim of the paper is straightforward and the study intends to test ebullition models using observed data. Furthermore, the models are used to produce outputs that cannot be directly verified using observations. This is fine, and is one of the major points of modelling systems. What is unclear, is how the performance of the models are gauged against the observed data.

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The main topic of this study is methane emissions via ebullition and the authors do not clearly describe how ebullition events are measured using Eddy Covariance (EC) data. In the end, the authors compare model output from all methane transport mechanisms combined (ebullition, plant mediated, and diffusion) against the EC data. Unfortunately, this comparison does not allow the authors to derive any definitive conclusions about modelling methane ebullition.

The paper can also benefit from more explanation on the collection and processing of the field data. On the modelling side, the authors should provide information on calibration of the models and provide reasons if calibration was not performed and how this could affect model results. A brief model sensitivity analysis is presented, and it would be interesting to expand this section with a more in depth, systematic sensitivity analysis that includes a figure. The majority of the paper is well written, but some sentences need restructuring (see comments below). To improve clarity the manuscript should be read by a native English speaker.

Technical corrections

Pg 1 Line 25: Ebullition is not important in all wetlands. perhaps change sentence by adding, "in some wetlands"

Pg 1 Line 28: Ebullition is not only sporadic in space, but also in time and you should provide background evidence for this (see introduction of Ramirez et al. [2017]).

Pg 1 Line 36: Total volume of what?

Pg 2 Line 8: Mention that both increasing and decreasing atmospheric pressure trigger ebullition and provide references.

Pg2 Line 19: Avoid starting sentences with a number, instead spell out the number. This sentence also needs to be restructured.

Pg2 Line 20: Replace questioned with questionable?

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Pg 2 Line 28: Replace inflict with produce.

Pg3 Line 1: Is this upward transport, and/or also lateral transport? Be specific.

Pg3: Does the model have a spatial resolution? How many layers exist in the peat column and how thick are the layers? Introducing this information early on helps the reader visualise how the model operates.

Pg4 Line 8: Can you explain better what the is meant by the lowest air layer. Here you mention the model time step, can you provide the actual time step early in the model introduction (e.g. hrs or days).

Pg 4 Line 20: Again, the lowest air layer is vague, perhaps define it earlier. Replace ascend with ascent.

Pg 6 Line 9: You introduce a layer thickness 0.2 m, is this the case for all three models (see comments above regarding spatial resolution).

Pg7 Line 1: replace gapfilled with gap-filled

Pg 7 Line 4: Can you provide evidence in the literature that ebullition events from peat can be measured using EC.

Pg 7 Line 10: Can you provide further explanation as to how ebullition was detected using the EC data. Include details about the post-processing of the data.

Pg 7 Line 33: Avoid starting sentences with a number, instead spell out the number. Apply to other instances within the manuscript.

Figure 4: Dashed line is not clearly visible.

Pg 8 Line 36: Change “got stuck” and “alive” with formal words.

Pg 8 Line 38 and 39: “Inflicted” is not the correct word choice.

Figure 5a: It is difficult to distinguish between the four datasets plotted. Consider re-structuring this figure with a separate panel for each model output, with the observed

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data superimposed. Additional figures you may consider are histograms and scatter-plots between observed and modelled emissions (with the later the differences are clearly noticeable).

Pg 9 Line 4: Does figure 5a compare modelled and observed CH₄ emissions considering all three transport mechanisms (diffusion, plant mediated, and ebullition)? If this is the case, it is quite confusing because the manuscript up to now was focusing on ebullition, and I was expecting a comparison between observed and modelled ebullition emissions. Please provide further explanation.

Pg 9 Line 10 and 11: This variability is difficult to see in Figure 5a, see comment above.

Pg 9 Line 14: Can you provide more explanation how the R² were derived for each model.

Pg 9 Line 22: Replace the phrase “complicates the picture” with formal wording and explanation.

Pg 9 Section 3.4: This a model sensitivity section and should be presented before the results section.

Pg 10 Line 11-13: How do the models support the rupturing of confining layers if peat micro structure (e.g. woody layers) is not entirely represented within the models? Can you provide explanation for this conclusion.

Pg 11 Line 5: Sentence needs restructuring.

Pg 11 Line 23: You mention calibration of methane models performed in other studies, can you explain why you chose not to perform a calibration prior to model testing and how this affects your results?

Pg 11 Discussion section: Assuming that you cannot definitively identify ebullition events in the EC data, it is possible that the EC data contains few ebullition events. Could this further explain the mismatch between observed and modelled CH₄ emis-

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sions? Please address this possibility in the discussion section.

References

Ramirez, J., A. Baird, and T. Coulthard (2017), The effect of sampling effort on estimates of methane ebullition from peat, *Water Resour. Res.*

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