

Interactive comment on “HIMMELI v1.0: Helsinki Model of MEthane buiLd-up and emLsion for peatlands” by Maarit Raivonen et al.

Anonymous Referee #2

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This manuscript presents a sensitivity analysis of a methane module that could be included in peatland models. The authors argue that the novelty of this study is that the model has been developed independent of a full peatland carbon model and can then be tested for sensitivity allowing for dependencies within the methane models itself to be assessed separately from the entire C model. The fact that it is a module without the complete C cycling that feeds input to the methane module, makes it difficult to assess the ability of the model to estimate fluxes as the test that compares it to field-measured fluxes did not optimize the anoxic respiration input and this would actually be generated from the entire peatland C model. Also, sensitivities are difficult to assess this way as important drivers (e.g., temperature driving CH₄ production) are not included as this would happen in the other part of the peatland C model that would drive anoxic respiration rates.

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Aside from testing the sensitivity of a methane model outside of a full C model, the novelty of the model itself is not clear. The way in which methane production, oxidation and transport is considered in the model appears to be largely developed according to methods used in previous models and therefore it is not clear what improvement is expected here. The way in which ebullition is handled, for example, is quite simplistic and not consistent with literature that clearly illustrates trapping of free-phase gas over time as opposed to release as soon as a bubble is formed (e.g., Comas et al., 2014; Ramirez et al., 2015). I think a clear justification of why another peatland CH₄ model is needed must be included to illustrate the utility of this model.

Specific additional comments are given below:

Page 2, Line 5: Maybe the 2nd largest anthropogenic radiative forcing after CO₂? Water vapour causes the greatest radiative forcing in the atmosphere, followed by CO₂ and then CH₄

Page 2, Line 13: Saying no other alternative electron acceptors exist is a bit extreme. Many freshwater wetlands will have cycling of NO₃, Fe, SO₄, etc., in addition to CH₄ production. I suggest rewording this sentence.

Page 4, Line 28: I guess 45-60% is meant (as opposed to ...). This happens throughout the manuscript in my version.

Page 5, Lines 1-3: And also, peat properties and pore sizes are likely to vary within and between peatlands based on composition of the peat (i.e., sedge vs. wood vs. moss) as well as decomposition status.

Page 5, line 7: the effect of tortuosity on the diffusion coefficient indicates that it is not only the porosity that is important, but the interconnectivity and shape of that porosity and probably the pore size distribution

Page 6, Line 7: In reality WT is the not the divide between water-filled and partially water-filled pore space. Above the WT there is always some fully saturated layer as

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the capillary fringe. In practice in the model it doesn't make a difference as the boundary would instead be the capillary fringe, but the way it is written here is technically incorrect.

Page 6, Line 11: When WT is above the surface it can become oxygenated by wind-mixing. Is this considered?

Equation 7: What about inhibition by other electron acceptors? I know you are not following them in the model, but they could be important in some fen systems.

Is CH₄ production from the peat matrix accounted for – anaerobic respiration is driven by rooting depth, but CH₄ could be produced from other substrates.

Equation 11: Is this really realistic? This would allow a bubble to form, but that doesn't mean that ebullition occurs. Also, once bubbles form, they are often trapped and this affects the concentration gradients and also the ebullition fluxes. A very large bubble release is likely to provide such a high concentration when released that even if the WT is below the surface, not all the CH₄ will be oxidized (see page 14, line 15 in the manuscript).

Page 12, Lines 20-25: Was the model parameterized with the data from Siikaneva? If so, how appropriate was the test?

Page 16, lines 26-27: Does this illustrate that evaluating sensitivities in the methane only module, especially when production rates are not appropriately driven by changing conditions, is problematic? Temperature is a very important driving factor for CH₄ production, but it is not included in the way the module is constructed making it very difficult to interpret the actual sensitivities of the model.

Page 17, line 10: In this model, the peat column and layering is not important, but what about if the gas is being trapped prior to ebullition or even once mobilized from one layer and then trapped in another (e.g. Comas et al., 2015). We know this happens in reality, but it is not included in this model. If it was how would the results of the study

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change?

References: Comas X, Slater L, Reeve AS. 2011. Atmospheric pressure drives changes in the vertical distribution of biogenic free-phase gas in a northern peatland. *Journal of Geophysical Research*, 116, G04014, doi: 10.1029/2011JG001701.

Comas X, Kettridge N, Binley A, Slater L, Parsekian A, Baird AJ, Strack M, Waddington JM. 2014. Effect of peat structure on the spatial distribution of biogenic gases within bogs. *Hydrological Processes*, 28, 5483-5494.

Ramirez JA, Baird AJ, Coulthard TJ, Waddington JM. 2015. Ebullition of methane from peatlands: does peat act as a signal shredder? *Geophysical Research Letters*, 42, 3371-3379.

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