Biogeosciences

Discussions

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Interactive comment on "Year-round simulated methane emissions from a permafrost ecosystem in Northeast Siberia" *by* Karel Castro-Morales et al.

Anonymous Referee #1

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General comments

The manuscript by Castro-Morales et al. reports simulated methane emissions for a permafrost region in Siberia using an updated version of the JSBACH-methane model. The revised model specifically accounts for (a) diffusion through snow and (b) varying fractions of wetland extent in model grid-cells (and an improved plant-mediated transport scheme). Castro-Morales et al. compare the modelling results to ground-based observations from an eddy covariance tower and from chamber measurements. The manuscript aims to improve current methane models for the permafrost region. Improved model performance for these regions is particularly important as methane

emissions are expected to become more important for the global methane budget in a warming climate. Thus, the manuscript could represent an important contribution to improved modelling of high-latitude methane emissions. The authors present a detailed analysis of the model results, but their results remain often of qualitative nature. The manuscript is mostly well written and discusses in detail many aspects modelling performance. However, a more focused presentation of key results and conclusions could make this manuscript more accessible to the reader. The authors did a thorough job to present and discuss improvements and shortcomings in the performance of the revised methane model. The topic of the manuscript is within the scope of the journal and could be considered for publication. In my opinion, the manuscript would substantially improve if the following issues would be addressed.

Specific comments

In my opinion, the comparison between observed and simulated methane emissions would however benefit from using an upscaling approach to avoid issues arising from the mismatch of scales. This was done for the chamber measurements, but it remains unclear how representative the flux tower footprint is of the entire grid cell. Comparing flux measurements from a single location to the entire grid cell is only meaningful if the grid cell is characterized by spatially homogeneous methane emissions. This is only rarely the case for such high-latitude landscapes (e.g., Sachs et al., 2010; Parmentier et al., 2011; Helbig et al., 2017). The authors should also address how representative the location of tower and chamber flux measurements is of the entire grid-cell. The authors estimate the fraction of inundated land for the grid-cell and demonstrate how this fraction is an important predictor for methane emissions. The same should apply for flux tower measurements where the fraction of wetlands is tightly coupled to the magnitude of methane emissions (see for example Helbig et al., 2017). How would the wetland fraction at the grid cell-scale compare to the same fraction at a smaller scale at the study sites?

The authors report "comparable" (line 30) methane emissions when comparing model

and measurements. The analysis could be much stronger if the authors give a quantitative measure for the performance (e.g, Root Mean Square Error or any other suitable metric).

The authors state that the aim of the work is to "improve our understanding". However, in my opinion, the manuscript mainly focuses on improvements in methane modelling and an evaluation of the performance of a revised methane model. The authors may consider reframing their research objectives and focus results and discussion on the specific research questions.

Large areas in northern Siberia are covered by polygonal tundra. The distinct microtopography of these landscapes has important implications for surface hydrology and thus also surface inundation (see Cresto-Aleina et al., 2013; Helbig et al., 2013; Liljedahl et al., 2016). I was wondering if such polygonal tundra covers a significant proportion of the study area? And if yes, what would be the consequences of distinct microtopography on the performance of the TOPMODEL and on the simulated methane emissions. Using a mean water table for methane modelling in such heterogeneous landscapes can lead to significant underestimation of methane emissions (Cresto-Aleina et al., 2016).

With the TOPMODEL approach, the authors can distinguish between inundated and non-inundated land. However, many peatlands are characterized by a water table just below the peat surface and are thus not inundated. Nevertheless, they can emit large amounts of methane, which would be neglected in the current modeling approach. At the same time, lakes (i.e., inundated land) may be characterized by lower methane emissions than these peatlands due to a lack of fresh organic carbon input. What are the implications of this for the modeling performance? The authors may consider discussing this shortcoming.

In the current manuscript, the authors "decreased or increased [the parameters] by a fixed value" (line 343). Could the authors use a Monte-Carlo approach instead to as-

sess the parameter sensitivity? The authors mention "reported values in the literature". Could they specifically discuss/show the observational constraints on the individual parameters?

Line 406-408: Why do the authors only show one adjacent cell? What is the justification to compare a neighboring grid cell to the ground-based observations? To demonstrate the spatial heterogeneity the authors could consider using more than just two grid cells.

In line 464-465, the authors mention the "parameter adjustment", but do not elaborate how exactly the parameter for the TOPMODEL was adjusted. Did the authors use an objective (cost) function to optimize this parameter?

The authors demonstrate in their sensitivity analysis that the threshold TOPMODEL parameter and "allocation-of-decomposition-to-CH4" are the most important parameters determining the magnitude of simulated methane emissions. In my opinion, the authors should strengthen these results throughout the manuscript. It appears as if their results indicate that methane emissions mainly depend on methane production dynamics (i.e., fCH4anox) and on inundation as "on-off" switch of methane emissions. Transport pathways and methane oxidation appear to be less important (merely changing the timing of emissions). Are these modelling results supported by observations in the field? The authors may consider discussing this in more detail.

Line 61-62: Perhaps the authors could mention another important permafrost thaw effect on methane emissions here: increasing surface wetness due to surface subsidence of ice-rich soils (see for example Christensen et al., 2004; Johnston et al., 2014, Helbig et al., 2017).

Line 94-100: Wintertime methane emissions have also been reported by Helbig et al. (2017) for a boreal peat landscape in northwestern Canada, where they found winter emissions to contribute about 25% to the annual budget.

Line 121: Could the authors discuss here the most important "shortcomings in the

parameterization" of the state-of-the-art methane models?

Line 133: Perhaps the work by Cresto-Aleina et al. (2013, 2016) on microtopography effects on surface water and methane emission dynamics could be mentioned here too.

Line 500-501: Only mineral soils are considered for the methane modelling? How common are organic soil in the study area? I would assume that at least top-soils in the floodplain would be organic-rich. How would "considering" organic soils change the results?

Line 577-579: The authors may consider supporting this statement with information on the exact magnitude of interannual variability.

Line 589-592: What is the uncertainty in the eddy covariance flux measurements? Could the authors quantify uncertainties due to random errors, gap-filling, u*-threshold, and footprint heterogeneity? An uncertainty quantification of eddy covariance fluxes would further strengthen the model-observation comparison.

Line 691-711: I am not sure how this section contributes to the research questions of this manuscript? Perhaps the authors could mention differences in environmental characteristics of grid-cell A and B briefly in the manuscript and move figure 9 to the supplementary material?

Line 808-810: The impact of cooler early summer temperatures on soil warming and methane emissions has been demonstrated recently using multi-year methane observations in a boreal peat landscape (see Helbig et al., in press). The authors may consider discussing their modelling results in relation to these observations.

Line 847-851: The authors may consider starting the discussion mentioning the parameters that actually made a difference and not with the parameters that did not change the results. It should be highlighted what process/parameter matters in the model.

Line 991-992: Few studies have shown that non-inundated upland areas may take

up methane (e.g., Flessa et al., 2008). As far as I understand, such uptake is not considered in the current work. How could uptake in the drier areas of the model domain change simulation results? There are large areas in the model domain that appear to be characterized by upland landscapes and thus potential methane uptake (see Fig. 1).

Line 1134-1141: The authors may consider not to introduce a new concept (e.g., anaerobic microsites) at the very end of the conclusions. I would recommend to only refer here to what has been shown in the manuscript so far.

Line 1252-1255: What would happen if the model would run with the old order of processes? Shouldn't this be part of the uncertainty analysis?

Fig. 1: Why did the authors use such a large study area, if ground-based observations were only available for a very small fraction of the model domain? How can the model performance be evaluated for the other non-floodplain grid cells that appear to be characterized by different landscape characteristics?

Fig. 6: Why do the authors compare the mean grid-cell soil temperature profile to measured wet and dry soil temperature profiles? Physical soil properties differ drastically between wet and dry soils and consequently strongly determine soil temperature dynamics (see end of discussion). Wouldn't it be therefore necessary to at least model soil temperature dynamics of the inundated and non-inundated land surface separately?

Fig. 7: Methane emissions increase considerably in the model at sub-zero soil temperatures. In contrast, measured methane emissions appear to be quite insensitive to soil temperature below $0°C$. The authors mention this mismatch in lines 655-659. Perhaps the authors can discuss this mismatch between temperature-emission responses in more detail. How is it possible that such cold simulated soil temperatures result in emissions of > 30 mg CH4 m-2 day-1?

Fig. 8: Here, an uncertainty estimate for the measured cumulative methane emissions would help interpreting the comparison between simulated and measured fluxes.

Fig. 11: I am not sure how this figure contributes to the research questions. The seasonality of different methane emission pathways is already shown in Fig. 10. How does a representation of the spatial distribution of the methane emissions add to the manuscript?

Technical comments

Line 149: Remove "done".

Line 150: Remove "are".

Line 196: Please define what "hospitable and inhospitable" land means in this context.

Line 534: What do the authors mean with "visually"? They state in the previous sentence that differences are not statistically significant.

Fig. 3: Please clarify what the inset figures show.

Reference:

Christensen TR, Johansson T, Åkerman HJ et al. (2004) Thawing sub-arctic permafrost: Effects on vegetation and methane emissions. Geophysical Research Letters, 31, L04501.

Cresto-Aleina F, Brovkin V, Muster S, Boike J, Kutzbach L, Sachs T, Zuyev S (2013) A stochastic model for the polygonal tundra based on Poisson – Voronoi diagrams. Earth System Dynamics, 4, 187–198.

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Flessa H, Rodionov A, Guggenberger G et al. (2008) Landscape controls of CH4 fluxes

in a catchment of the forest tundra ecotone in northern Siberia. Global Change Biology, 14, 2040–2056.

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Helbig M, Chasmer L, Kljun N, Quinton WL, Treat CC, Sonnentag O (2017) The positive net radiative greenhouse gas forcing of increasing methane emissions from a thawing boreal forest-wetland landscape. Global Change Biology, 23, 2413–2427.

Helbig M, Quinton WL, Sonnentag O (in press) Warmer spring conditions increase annual methane emissions from a boreal peat landscape with sporadic permafrost. Environmental Research Letters. doi: 10.1088/1748-9326/aa8c85.

Johnston CE, Ewing S a, Harden JW et al. (2014) Effect of permafrost thaw on CO2 and CH4 exchange in a western Alaska peatland chronosequence. Environmental Research Letters, 9, 85004.

Liljedahl AK, Boike J, Daanen RP et al. (2016) Pan-Arctic ice-wedge degradation in warming permafrost and influence on tundra hydrology. Nature Geoscience, 9, 312–318.

Parmentier FJW, van Huissteden J, van der Molen MK, Schaepman-Strub G, Karsanaev SA, Maximov TC, Dolman AJ (2011) Spatial and temporal dynamics in eddy covariance observations of methane fluxes at a tundra site in northeastern Siberia. Journal of Geophysical Research, 116, G03016.

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