

Interactive comment on “A multi-scale comparison of modeled and observed seasonal methane cycles in northern wetlands” by Xiyan Xu et al.

Anonymous Referee #1

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The paper is devoted to calibration and validation of the CLM4Me model, that is the methane module embedded in the Community Land Model, version 4.5. It presents an important step towards further model development, i.e. the identification of the major drawbacks of the model performance in the area of northern wetlands. The methane model output is compared to different sources of data, covering spatial scales from particular sites (towers, chambers), through regional (WRF-based footprint analysis) to global (inverse modeling estimates). The parameter, characterizing the aerenchyma area, was tuned to get better agreement with empirical data on surface CH_4 emissions. Two methods of inundation parameterization were applied, and compared in the model output. The special focus is made on the Alaskan methane emissions, however a number of chamber and eddy covariance measurements from Swedish and Finnish sites are involved in the model validation as well. One of the main results

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of the study is that CLM4Me significantly underestimates wintertime CH_4 fluxes calling for deeper understanding of snow-period methane release to the atmosphere from terrestrial ecosystems.

I don't have principal concerns on the results of this study. There are some suggestions, however, which I hope could improve the paper:

- The title of the paper presumes a wider scope that has been actually taken place in the study: "cycles" mean much more than "emissions". I suggest to change the title as: "A multi - scale comparison of modeled and observed seasonal methane emissions in northern wetlands"
- The structure of the paper could be bettered. For instance, in the model description section 2.1.1 some of the model results are discussed. I recommend to move the latter to the appropriate sections.
- I could not understand why aerenchyma-related parameter S was the only one that was calibrated, whereas there are lot of others in any methane model. Moreover, I didn't see any significant impact of changing S on the zonally-averaged methane emission annual cycle in the northern latitudes, depicted at Fig.1, whereas such an impact had been anticipated as one of the main points of the paper.

I have a number of more specific remarks, that are given as **sticky notes** in the manuscript pdf.

I propose to accept the paper for publication after corresponding revision.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2016-167/bg-2016-167-RC1-supplement.pdf>

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