

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-229

Anonymous Referee #1

Referee comment on "Multi-site evaluation of modelled methane emissions over northern wetlands by the JULES land surface model coupled with the HIMMELI peatland methane emission model" by Yao Gao et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2022-229-RC1>, 2023

Dear Authors,

This study simulated CH₄ emissions from six northern wetlands based on a state-of-the-art land surface model (JULES) coupled with a CH₄ module (HIMMELI). The manuscript is generally well written, and I was able to follow the main conclusion. Authors evaluated model performance in terms of CH₄ emissions with several model settings, and found that neither settings could not "reasonably" represent the observed CH₄ emissions. I felt that the analysis still has room for improvement, and thus recommend a resubmission after a major revision.

Major limitation of this study is that the model lacked ability to reproduce observed CH₄ dynamics. It is not possible to say that a model that can correctly represent an observation is the correct model, but at the very least, a model that cannot reproduce the observation to a reasonable degree is not likely to be the correct model. I recommend that the model be modified and parameterized so that the model can, at least to a reasonable degree, reproduce the observations. Susiluoto et al. (2018) provided a sophisticated method for parameterizing the HIMMELI module; such exercise could improve the representation of the model in each site.

The manuscript showed the model performance in terms of CH₄ emissions, soil temperature, WTD, LAI, and soil carbon. The advantage of using eddy covariance data for model validation is that the various factors, including CO₂ and water vapor exchanges, involved in CH₄ dynamics can be validated simultaneously. Did the model have the ability to reproduce CO₂ fluxes (GPP, ecosystem respiration, NEP), evapotranspiration, and net radiation in each site? Describing their performance is necessary to interpret the model performance for CH₄ dynamics.

This study seems to assume that there was a general parameter set for simulating CH₄ dynamics across the wetlands. Since the type and origin of the wetlands were diverse (Table 1), the parameter set could be site specific. I understand that such site specific treatments are difficult on a global scale, but this is a site-specific study; so, careful consideration of site specific parameterization would be required. Furthermore, please show the list of important parameters for representing CH₄ dynamics in the manuscript.

The descriptions regarding accuracy/precision are not quantitative and thus vague. Please provide quantitative descriptions when describing accuracy/precision (mean bias, RMSE, R²). Please show R² and p-value when describing the presence or absence of correlation.

Specific comments

Line 200: For Taylor diagram of soil temperature, how the authors standardized RMSE and SD (i.e., using mean temperature in Kelvin or Celsius)? Furthermore, if the authors use Taylor diagrams for evaluating precision, be consistent using Taylor diagrams for WTD, and CH₄ fluxes.

References

Susiluoto et al. (2018) *Geosci. Model Dev.*, 11, 1199-1228.