

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-55

Anonymous Referee #2

Referee comment on "The importance of spatial resolution in the modelling of methane emissions from natural wetlands" by Yousef Albuhaishi et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-55-RC2>, 2022

This manuscript investigated the importance of spatial resolution in quantifying CH₄ fluxes from natural wetland. My main issue with this manuscript is that the authors made **a lot of** simplifications (chose certain values/threshold, and 'randomly' took different datasets, for instance using reanalysis soil temperature data and then modelled soil moisture at 5 cm) without sound reasons. By taking a lot of unrealistic values, the authors want to demonstrate the impacts of resolution on the modelled total methane emissions. There are so many steps/simplifications which could lead to different results: such as data resampling approach (how did the authors get different resolutions of wetland maps?), soil moisture and soil temperature dataset used (why only top soil layer, and why took two very different datasets?) and also why use the same dataset to calibrate model parameter and evaluate the results.

The authors should really work through their approaches, and thoroughly investigate how different steps/assumption might influence their final results. It is really difficult to understand the simplification/approach and the article is lacking in depth and impartiality. Thus, I would not recommend it for publication in its current form.

Detailed comments:

L42-43: Another important process which could contribute to the differences between top-down and bottom-up methods are the details in CH₄ transport pathways.

L128, the unit for the KCH₄ is not correct. The authors should define the meaning of K values, especially if using T₀ equals to 273.15.

L134, Is it true to assume all upland soils take up methane?

L157-158, what do you mean by "network density"? Then in the earlier part of the sentence, it says the reason to choose this study area is due to data availability at a few sites, but then it turns out only 2 sites were monitored. It reads contradictory.

L176: why here comes with scenario 5 now? I did not follow here.

L176-182, I am not sure I follow the authors' argument to make this type of assumption. 1, why do you need to stick to the highest density values from a dataset that is limited by the peat fractions? 2. It is fine to just assume zero emissions from upland soil, but on L180-192, why this assumption is linked to different soil carbon contents?

L192-193, not sure why you only need to use the top 5 cm soil layer, and which approach did you use it for resampling? It is not clear for me why you introduce another hydrological model for getting soil moisture? Have you evaluated the modelled soil moisture? Why not use the soil moisture data from ERA5?

L 208-210: not clear how the authors obtain the K_CH4 value? What values did you get for KCH4?

Figure 3, where are the color legend?

Figure 4, why are there emissions for Sn2 if you only have upland?

Figure 6, not clear for me how you can use the same observation data to calibrate K_CH4 and then evaluate the calculated model, please clarify this.