

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2021-280

Anonymous Referee #1

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Referee comment on "Assessing methane emissions for northern peatlands in ORCHIDEE-PEAT revision 7020" by Elodie Salmon et al., Geosci. Model Dev. Discuss.,  
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This paper presents a land surface model with an explicit representation of northern peatlands, ORCHIDEE-PCH4. The model simulations are compared to data from 14 wetlands. This paper focuses on methane emissions and refers to previously published work for peat carbon accumulation and carbon balance. The authors use the root mean square difference between simulated and observed methane emissions to optimize 7 model parameters. They first perform the optimization separately for each of the 14 sites, then perform a multi-site optimization.

### General comments:

The paper is well written. The introduction, the model description and the site description are very clear. The optimization method is very hard to understand (although I am not a specialist). The results are sometimes hard to follow with very small figures. The discussion and conclusion are clear.

My main comment is related to the representation of some processes in the model. From what is shown by the authors it seems like for most sites, methane emissions are pretty much independent of the water table depth. It is particularly obvious for the sites with multiple years of data. Fr-Lag for instance has a simulated high water table the first summer followed by two summers and autumns with very low water tables. Methane emissions are low the first summer and increase the two following ones (contrary to the observed fluxes). This behavior can also be seen at DE-Sfn, Fi-Lom, Pl-Kpt, and to a lesser extent at DE-Hmm and Dk-Nuf. This contradicts most of the existing literature on observations showing a strong correlation between water table depth and methane emissions (the higher the water table, the higher the emissions). In terms of processes, for a same site a higher water table is related to a higher soil moisture content and a lower oxygen concentration over the whole peat column, which favors methanogens (and also limits methanotrophs). It is particularly important to correctly represent the link between peat water content and methane emissions for a model designed to be used in

climate change studies. I would request the authors to at least clearly discuss this issue in the paper and modify their conclusions accordingly.

The second comment is related to the Dk-Nuf site. I happen to be familiar with this dataset and I noticed some imprecisions in the text (see specific comments), so I would ask each co-author responsible for a site to carefully proofread the manuscript.

### **Specific comments:**

L 243: Dk-Nuf : The methane emissions are measured by automatic chambers on this site. There is a flux tower but it only measured CO<sub>2</sub> fluxes (besides turbulent energy and radiative fluxes). Also, the water table depth is not available at this site.

Table 1: measurements for Dk-Nuf don't cover 2006-2009 but 2008 – 2014 (actually, the dataset extends to 2019)

L 266 : I don't understand the 0.5 degree grid cell. What is actually run at this resolution? The authors say at line 270 that they impose site level meteorological forcings. They also seem to indicate that the spin-up to reach close to observed peat carbon content and depth was done by using the site specific meteorological data. So is it the texture that is at 0.5 degree or the hydrology? If it is the hydrology to calculate the peatland fraction then what is used to force this calculation? A gridded meteorological forcing or the site specific one ? If a gridded meteorological forcing was used then it should be mentioned. This is a bit confusing.

Table 2: I am surprised by the value of observed carbon stock at DK-NuF. The only study known to me (Morel et al, Earth Syst. Sci. Data, 2020) gives 36.3 kg/m<sup>2</sup>. This is much lower than the 54.6 given in the Table.

Section 2.3: this is very hard to follow. I am absolutely not a specialist but I wondered if the whole time series of observation was used, and at what time step? (hourly, daily, monthly, yearly?) and why.

L326-327: this is very strange. If  $z_{root}$  is increased to 0.75m then, if I am not mistaken,  $f_{root}=0$  (I am assuming  $z_{soil}=0.75m$  since this is the peat depth), so that  $f_{pmt}=0$ . If  $z_{soil}$  is not equal to 0.75m, increasing  $z_{root}$  decreases  $f_{pmt}$  (in absolute value). Is this wanted?

Similarly, why increase the rate of methanotrophy to get higher methane net emissions?

Another question: what did the authors do with missing data for methane emissions? There are very few winter measurements of methane emissions at Dk-Nuf. Did the authors gap-fill the data and then optimized their parameters on this? It should be clearly stated.

L327: to be coherent with the rest I believe Table 4 should have values in parenthesis for qMG at PL-Wet site (4 is outside the range given in table 3)

Figure 2-5 a): why not give the observed water table depths when available

L 459: how does permafrost explain a deeper simulated water table position ? explain.

L 562: I couldn't agree more with the authors comment on the need for more data on vascular plants in peatland

L 590-591: I am not sure the authors really showed that these 2 sites were limited in methane substrate. It is likely the case in the model, but is it the case in reality ? Because it seems that this model result might be related to the partitioning between active, slow and passive C pools.

L 727: I couldn't agree more with this last sentence.

### **Technical comments:**

L103: Qiu et al, missing year

L 285 : there is something strange with the end of the sentence "... and driven"

L316 : reached instead of reach

L318 : that emits instead of that emitting

L357 : "discharged" is not the right verb here

L365 : "significantly lower" : that is quite an understatement

L 385 : "simulated diffusion of atmospheric methane" instead of "diffusion of simulated atmospheric methane"

Figure 2, 3, 4 and 5: b and c : the units of the flux should be mg/m<sup>2</sup>/d on the Y axis

e : on the Y axis on the right

L 482: Table 6 instead of 5