

Reply of Comment on gmd-2021-280

Referee comment on "Assessing methane emissions for northern peatlands in ORCHIDEEPEAT revision 7020" by Elodie Salmon et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-280-RC1>, 2021

We thank the reviewer for his/her constructive comments. Our manuscript has been significantly improved through addressing the reviewer's comments.

Below, comments have been numbered and a response is exposed for each of them. Line numbers correspond to the Preprint version. Modifications are highlighted in blue in the present document.

## **Anonymous Referee #2**

**Comment 2.1:** This is my first time reviewing this manuscript. The authors evaluated single-point applications of one land model ORCHIDEE in simulating methane emissions at 14 northern peatland sites. The PEAT model version they employed has been incorporated with several previous methane algorithms.

General comments:

The paper is well written and organized. The topic on peatland methane simulation is also very interesting because it remains a challenge to perform the methane processes well for most land surface models. The ORCHIDEE-PEAT model can be a potential helpful addition for the wetland modeling studies. However, in the current manuscript there are still some problems regarding the methods and results the authors need to clarify.

**Response:** We carefully revised the manuscript by addressing each of the comments and suggestions.

**Comment 2.2:** 1. The study sites are northern peatlands and the authors have mentioned the importance of snow layer on calculating the diffusion of  $\text{CH}_4$  flux in the study, but I never found any data or plots of snow observations/simulations to support their claims, such as L385-386, L403-407, L432-433, L466 in the manuscript.

**Response:** The methane model has been optimized employing observed methane emissions. Our goal has never been to evaluate or optimize every process (methanogenesis, methanotrophy and gas transport) since there is not enough observation data available in the literature to achieve this. Nevertheless, it is of significance to highlight the contribution of each process in order to fully explain simulated results of methane emissions. More specifically these "claims" described results from Fig 2 to 5, line e, that display simulated methane content in the model snow layers. The snow model has been fully evaluated in Guimberteau et al., (2018) which has been reported L118.

**Comment 2.3:** 2. For wetland simulations, the anaerobic environment is crucial for methane production/oxidation. It is mainly controlled by water conditions and microtopography. I found the bad performance of methane emissions always with poor water table simulations in the

study. for example the US-Wpt and RU-Che sites in fig5, Fig2s, L455-466. How did you consider the impacts or limitations from the biases of hydrological simulations?

**Response:** ORCHIDEE is a global scale land surface model (LSM), while it is commonly employed to perform site simulations for calibration purposes or study specific processes, it is mainly dedicated for global scale studies. At global scale, all LSM methane models are optimized against observed methane emissions which implicitly enabled compensation effects between processes which are at the origin of methane emissions. In order to assess model uncertainties that will occur during global scale simulations, we choose to consider simulation conditions as if the model was employed for global scale simulations and to enable compensation effects between processes involved in methane emissions that include biases of the hydrology model. For instance, water table positions at US-Wpt in our simulations are fluctuating between 0 and -40m which is also the case at FI-Lom, DK-Nuf and DE-Sfn. While simulated methane emissions are in good agreement with observations for FI-Lom, DK-Nuf and DE-Sfn, methane emissions are largely underestimating at US-Wpt. Therefore, we have not found any obvious relationship between a lower water table position and an underestimation of methane emissions. In addition, simulated water table positions are prognostic variables estimated from the simulated soil moisture content and the correlation between observed and simulated water table position is provided in supplementary Figure S2. We also demonstrate in supplementary document Figure S3 that at each site maximum methane emissions are correlated with the optimum of both soil temperature and moisture. Finally, we show at US-Los and at DE-Spw, in supplementary document Figure S4 and S5 that above the simulated water table position soil moisture content is higher than 0.8 which is sufficient for methanogenesis to occur. Our results show that conditions or processes promoting the largest methane fluxes such as US-Wpt and RU-Che and the lowest one such as at US-Los and DE-Spw are the least understood. A better understanding of these processes/conditions will serve to better constrain models.

**Comment 2.4:** 3. Some key information on parameter definitions, parameter values and units were missed in the method section. It is a bit hard for readers to understand the methane modeling structure. See specific comments below.

**Response:** In order to improve the method section and ease its reading we have addressed the reviewer's specific comments. Point by point responses are described below (see comments 2.10 to 2.25).

**Comment 2.5:** 4. The authors should include more details on the parametrization method. (1) Please provide the details of how you determined the final accumulation year for ending the simulations at L279. Because there are two factors of soil peat depth and carbon content, which factor is prior? What are the referential resources of soil peat depth and carbon stock?

**Response:** We added some details on how we defined soil carbon profile in the model, L278 [“The peat model \(Qiu et al., 2019\) enables a vertical buildup of peat by simulating a downward movement of C when the discretized organic layers reach a threshold defined from a regression relationship between the carbon fraction and measured bulk density. This scheme in ORCHIDEE-PCH4 serves to constrain the vertical distribution of the soil carbon stock to the observed maximum peat depth. Simulations with ORCHIDEE-PCH4 driven by repeated site-](#)

specific meteorological conditions were performed for various periods of time to reach the observed soil carbon content and maximum peat depth (Table 2). “

We also added references in Table 2 and full references details have been added to the references list at the end of the manuscript:

**Table 2. Simulations conditions and framework to constrain peatlands soil carbon stock. Grey color reports the groups of sites with equivalent levels of methane emissions (Table 1).**

Sites identification	Peat fraction	Vcmax	Carbon accumulation period	Maximum peat depth		Soil carbon stock		References
				Observed	Simulated	Observed	Simulated	
	fraction	$\mu\text{mol m}^{-2} \text{s}^{-1}$	numbers of years	m	m	kg/m <sup>2</sup>	kg/m <sup>2</sup>	
US-Los	0.16	65	214	0.5	0.75	27.5	28.0	<a href="#">Sulman et al., 2009;</a> <a href="#">Chason and Siegel, 1986</a>
DE-Spw	0.14	89	272	1.2	1.5	84.0	84.2	<a href="#">Dettmann et al., 2014</a>
DE-Sfn	0.18	45	4 544	5	5	372.8	372.5	<a href="#">Hommeltenberg et al., 2014</a>
DE-Zrk	0.9	33	10 060	10	7	696.7	696.6	<a href="#">Zak et al., 2008</a>
CA-Wp1	0.16	38	620	2	2	51.0	51.0	<a href="#">Benscoter et al., 2011;</a> <a href="#">Long et al., 2010</a>
US-Bog	0.27	42	4 305	2	3	207.4	207.7	<a href="#">Manies et al., 2017</a>
FR-Lag	0.22	42	937	1.6	2	121.0	121.4	<a href="#">Gogo et al., 2011;</a> <a href="#">Leroy et al., 2019</a>

DE-Hmm	0.9	35	8 963	3	3	265.0	266.4	Vybornova, 2017
FI-Lom	0.27	28	6 396	3	3	200.3	200.5	Lohila et al., 2010
DK-NuF	0.5	31	8 959	0.75	1.5	54.6	54.6	Bradley-Cook and Virginia, 2016
PL-Kpt	0.14	52	3 819	2.5	3	250.0	250.3	Jaszczynski, 2015
PL-Wet	0.11	52	261	0.5	0.75	37.6	37.8	Milecka et al., 2016; Zak et al., 2008
US-Wpt	0.27	80	32	0.3	0.75	5.3	5.4	Chu et al., 2014
RU-Che	0.05	35	2 968	0.56	0.75	45.8	45.8	Dutta et al., 2006

**Comment 2.6:** (2) For you parameter sensitivity analysis at L305-310, which period did you run for the analysis?

**Response:** We are not sure to understand this comment, optimization experiments serve to define “the set of parameters that reduce the gap between observation and simulation data.”(L304). Therefore, for each site we run the simulation over the entire observation period defined in Table 1 for which we have temporal monitoring of methane emissions. Moreover, this is not a sensitivity analysis *per se*, this is more a comparison between two optimization strategies. The first one is based on single sites and we assumed it will perform better for a site but each site will at the end have their own parameter sets. Since ORCHIDEE is a global model, we needed a single set of parameters that we obtained using the multi sites parameterization. Thus, the single site optimization is the best the model can do whereas the multi-site is the best trade off we can find.

**Comment 2.7:** 5. I have also a couple of concerns on the parameter settings of single-site and multi-site simulations. (1) There are several conflict descriptions on parameter ranges between single-site and multi-site simulations the authors should clarify. See specific comments below.

**Response:** We addressed the reviewer's concerns by addressing specific comments below (comments 2.28 to 2.30 and 2.40 and 2.41).

**Comment 2.8:** (2) When doing parameter optimization for the multi-site simulation, why didn't you set all parameter ranges to cover the already obtained single-site optimized value, such as L482, L491-499 qmg, zroot and Tveg. For example, the specific optimized value of qmg at PL-Wet is 4; according to the range of qmg from 9 to 10 in table 6, the final multi-site optimized value will miss its single-site optimized value. This may be one reason why the simulations with multi-site optimized values for PL-Wet and DK-Nuf were worse than their single-site simulations.

**Response:** We performed simulation experiences that the reviewer proposed using a so-called extended range that include all the parameters values obtained after the single site optimization. Results are shown in the supplementary document Table S3 to S5 and Figure S9 and S10. These results are also reported in the manuscript L500-503.

**Comment 2.9:** 6. How did you consider the limitation for peat depth simulations in section 4.2? In table 2 I found there are several sites whose peat depths do not consist of observations, such as DE-Zrk, DK-NuF, which should impact the distribution of soil carbon within 0-0.75m.

**Response:** The peat depth limitation is defined based on the diagrams of maximum methane production depth display in Figure 2. To explain this, we added that sentence L625 "In Figures 2e to 5e, that display the depth of maximum methane production, reveal that the deepest methane production depth is 0.75 meters in all the simulation results."

We also checked the carbon content value integrated up to 1.5m which corresponds to the depth of the model layer below the 0.75 meter layer. Even though the numbers change for some sites, the conclusions drawn are the same.

up to 1.5m	carbon content			
Sites identification	active	slow	passive	total
	kg/m <sup>2</sup>	kg/m <sup>2</sup>	kg/m <sup>2</sup>	kg/m <sup>2</sup>
US-Los	13.94	13.85	0.05	27.84
DE-spw	36.56	47.12	0.20	83.88
DE-Sfn	33.78	127.02	1.29	162.09
DE-Zrk	72.07	127.21	0.75	200.03
CA-Wp1	15.46	35.34	0.22	51.02
US-Bog	16.84	176.32	2.70	195.86

FR-Lag	40.51	80.40	0.45	121.36
DE-Hmm	50.99	177.61	1.59	230.18
FI-Lom	15.24	140.59	3.75	159.57
DK-NuF	4.18	49.20	1.18	54.56
PL-Kpt	15.49	185.09	4.94	205.52
PL-Wet	15.36	22.08	0.11	37.55
US-Wpt	3.94	0.84	0.001	4.78
RU-che	3.51	40.04	2.14	45.69

### Specific comments:

**Comment 2.10:** In equation 1-13 of section 2.1, do variables, such as  $f_{mga}$ ,  $f_{mgs}$ ,  $f_{magp}$ ,....  $f_{mt}$ , also change with layer depth and time? I assume  $z$  is depth and  $t$  is time in the equation 1, despite you not giving their definitions.

**Response :** To ensure that the reader understand that these variable change in depth and time we modify the sentence L150: “where each term **that varies in time (t) and with depth (z)**, expresses methane production [...] “

**Comment 2.11:** Also lacks the related information on parameters. For example, are  $k_{mt}$  of equation 5 at all layers the same value?

**Response :** Yes,  $k_{mt}$  and all the other parameters are the same value for all model layers unless it is mentioned otherwise.

**Comment 2.12:** L164-165 What's the units of  $[C]$ ,  $k_i$ ?

**Response:** The unit for  $[C]$  has been added L164 :“each type of soil carbon pools ( $[C]_i$ ,  $i = a, s, p$ ; **in  $gC\ m^{-3}$  of soil**), active, slow and passive “ and  $k_i$  L176: “where the rate of methanogenesis ( $k_i$  **in  $s^{-1}$** ) “

**Comment 2.13:** L165 For soil, it should be 'soil moisture', not 'soil humidity'.

**Response :** We changed it to soil moisture:“where the rate of methanogenesis ( $k_i$ ) depends on soil temperature and **moisture** “

Terms ‘soil humidity’ were changed to ‘soil moisture’ in all the document, L128, L166, in Table3 (‘Connectivity of soil **moisture**’), L381, L535, L550 and L606.

**Comment 2.14:** L169 Why didn't you mark  $O_2$ ,  $p$  and  $O_2^*$  using brackets as  $[CH_4]$  and  $[C]$ ? For example, is the  $O_2$  here different from  $[O_2]$ soil in Fig1?

**Response :** O<sub>2</sub>, p and O<sub>2</sub>\* are notations employed by Kvonostianov et al. (2008) but the review is right it is more consistent to harmonize the notation within the manuscript.

We modified equation 3 to

$$f_{MG_i} = [C]_i \frac{k_i}{q_{MG}} e^{-[O_2]_p/[O_2]_{anoxia}} f_{clay}$$

And L169 to : “ [O<sub>2</sub>]<sub>p</sub> is the oxygen concentration in the soil per unit porous volume, [O<sub>2</sub>]<sub>anoxia</sub> is the soil oxygen”

**Comment 2.15:** L174 What's the unit of clay content? For these peatland sites, what are the values of clay you used?

**Response :** The clay content is a fraction whose values are defined between 0 and 1. Here it is defined at 0.2. We modified L174 “where clay is the clay **fraction and has a value of 0.2**,”

**Comment 2.16:** L177 Again, what's the difference among [O<sub>2</sub>]<sub>soil</sub>, O<sub>2</sub> and O<sub>2</sub>\*? And their units should be added.

**Response :** To improve the definition of O<sub>2</sub>,p and O<sub>2</sub>\* We modified L169-170 “ [O<sub>2</sub>]<sub>p</sub> is the oxygen concentration in the soil [O<sub>2</sub>]<sub>soil</sub> (**in gO<sub>2</sub> m<sup>-3</sup> of soil**) per unit porous volume (  $\frac{\epsilon_{O_2}}{\pi_{soil}}$  ,  **$\pi_{soil}$  is the soil porosity**), [O<sub>2</sub>]<sub>anoxia</sub> is the soil oxygen concentration at which anoxic conditions are reached and enable methane production. This oxygen concentration threshold is assumed to be 2 g m<sup>-3</sup>(Duval and Goodwin, 2000).”

And [O<sub>2</sub>]<sub>soil</sub> is defined L175-176 : “The amount of methane consumed by methanotrophy is limited by the soil oxygen concentration, [O<sub>2</sub>]<sub>soil</sub>, following a 1:2 CH<sub>4</sub>:O<sub>2</sub> molar ratio”

**Comment 2.17:** L178 What is the exact unit for k<sub>MT</sub>, hour or day?

**Response :** We modify the unit L178 “where k<sub>MT</sub> is the rate of methanotrophy which value range from **0.06 to 5 d<sup>-1</sup>** (Morel et al., 2019) “ to be consistent with k<sub>MT</sub> unit in the tables.

**Comment 2.18:** L184 Are you sure that kebu= '1 h<sup>-1</sup>'? This would mean the constant is unuseful during calculation.

**Response :** Here the kinetic variable is defined for the physico-chemical sense of the equation. However, since no observation value are available, it is not possible to constrain the threshold for ebullition by a kinetic rate therefore in the present study and in Khvorostyanov et al., (2008a, 2008b), the kinetic rate is defined to 1h<sup>-1</sup>.

**Comment 2.19:** L185 What is the unit of P<sub>soil</sub>? Why did you set the 0.75m depth as limitation?

**Response :** The unit of P<sub>soil</sub> is Pascal (Pa) we added to the manuscript L185 “pressure (P<sub>soil</sub> **in Pa**) “

The limitation at 0.75m is defined L191-194 “We estimated that in our model below the layer corresponding to 0.75 m the hydrostatic pressure is always higher than the partial pressure of dissolved gases. Therefore, we considered below 0.75 m that methane ebullition threshold is constant and equal to the value defined at 0.75 m in order to avoid methane accumulation in the deeper layers. “

**Comment 2.20:** L192-193 Again, how did you estimate the layer of 0.75m? Please add more information or results.

**Response :** The full explanation is provided L 190 “It has been suggested that ebullition in soil occurs when the partial pressure of dissolved gases exceeds the hydrostatic pressure (Chanton and Whiting, 1995). We estimated that in our model below the layer corresponding to 0.75 m the hydrostatic pressure is always higher than the partial pressure of dissolved gases. Therefore, we considered below 0.75 m that methane ebullition threshold is constant and equal to the value defined at 0.75 m in order to avoid methane accumulation in the deeper layers. ” This description is in the method section, we think it will be confusing to provide results in that section. For more details readers should refer to the work of Chanton and Whiting, (1995) that is cited.

**Comment 2.21:** L199 What's the unit of tortuosity  $n$ ? Does it have a constant value?

**Response :** L200 have been modified to “the tortuosity  $n$  that depicts the sinuous path of bubbles, is defined to be  $2/3$  (Hillel, 1982). “

**Comment 2.22:** L201-202 Do you mean  $w_{size} = 1\text{cm}$  here?

**Response :** The sentence, L201-202, has been modified to “Khvorostyanov et al., (2008a, 2008b) defined  $w_{size} = 1\text{cm}$  for a carbon rich loess deposit of the Yedoma. “

**Comment 2.23 :** L206-207 It is not clear. Do you mean that the oxidized methane in the root zone accounts for 39-98% of total methane oxidation in soil? Please provide the exact value used in your study.

**Response :** We modified L206 to “The proportion of methane oxidized ( $M_{rox}$ ) in the root zone is emitted as  $\text{CO}_2$  to the atmosphere. Walter and Heimann, (2000) estimated  $M_{rox}$  to range between 39 and 98% of methane located in the root zone. “

We explained in section 2.1, L139-142 that the parameters values are defined in section 2.2. In order to put forward the presentation of the method subsections we moved sentences L139-142 ahead after the section title, 2.Model description, and added a sentence to introduce subsection 2.1 ORCHIDEE-PCH4. This part reads now L111:”A general presentation of ORCHIDEE-PCH4 and of processes considered in the methane model are exposed in section 2.1. Implementation of methane production and oxidation and transport are specified respectively in sections 2.1.1 and 2.1.2 whereas parameters values established for the formatting site simulations conditions before observation periods are given in section 2.2. Then, section 2.3 describes the parameter optimization approaches. “

**Comment 2.24:** L210 Again, what's the value of  $T_{veg}$  used in your study?

**Response :** As explained in response to comment 2.23 above, parameters values are defined in section 2.2. This is specified in line 111-115.

**Comment 2.25:** L215 I didn't find the root biomass in the root distribution function 10, only the root depth. Please clarify this.

**Response :** We defined in a different way the  $f_{root}$  L215 to “This function describes the vertical distribution of roots in the soil in which  $z_{root}$  is the rooting depth and  $z_{soil}$  the soil depth. “



**Comment 2.26:** L219 Is the z in this equation the same as that in other equations? How did you distinguish the depths of soil, water or snow coverage?

**Response :** In the model, soil carbon and the hydrology schemes are vertically discretized into 32 layers corresponding to a depth of 38 meters. Each layer is ascribed a soil depth and a layer thickness. The snow is divided in 3 layers on top of the soil layers. We added L217 “The gas diffusion scheme features the diffusion of CH<sub>4</sub> and O<sub>2</sub> in the 3 top layers of snow when snow cover is formed and in the 32 soil layers that correspond to 38 m depth. This scheme considered (1) the diffusion of oxygen from the top soil to the soil layer, (2) the diffusion of methane produced and remaining in the soil and (3) methane exchange between the soil and the atmosphere at z=0:”

**Comment 2.27:** L224 Please provide the sources for the values.

**Response :** We added the reference L224 (Khvorostyanov et al., 2008a)

Khvorostyanov, D. V., Krinner, G., Ciais, P., Heimann, M. and Zimov, S. A.: Vulnerability of permafrost carbon to global warming. Part I: Model description and role of heat generated by organic matter decomposition, Tellus, Ser. B Chem. Phys. Meteorol., 60 B(2), 250–264, doi:10.1111/j.1600-0889.2007.00333.x, 2008a.

**Comment 2.28:** L286-287 Did you run the carbon accumulation process again for the historical period of each site with these calibrated parameters?

**Response :** We added this sentence to be more accurate, L287 “Then site-specific simulation over the observed period is run again using the optimized parameters.”

**Comment 2.29:** L305-310 For the parameter sensitivity analysis. Which period did you run? Please clarify this.

**Response :** We added this precision to the sentence L305: “Two types of simulations are performed over the site-specific observed period defined in table 1: single site (SS) experiment for which parameters are optimized for each site and a multi-site (MS) experiment that aims at refining one set of parameters considering all sites together.”

**Comment 2.30:** L324-327 Can you explain why the optimized q<sub>mg</sub> at PL-Wet is so low at 4? The value is beyond much from the observed ranges in other studies. Do you have any evidence to support this value? The questions are also about q<sub>mg</sub>, z<sub>root</sub> and T<sub>veg</sub> for DK-Nuf.

**Response :** Since, methane results from the decomposition of soil carbon in order to produce large amount of methane, large amount of soil carbon need to be degraded in the model. Therefore, L321 we started by explaining that “The other three sites for which some of the optimized parameters are out of the initial range, DK-Nuf, PL-Wet and US-Wpt, are among the sites that emit more than 150 mg CH<sub>4</sub> m<sup>-2</sup> d<sup>-1</sup>. The carbon stock at DK-Nuf and PL-Wet are respectively 55 and 38 kg C / m<sup>2</sup> which is lower than at FI-Lom and PL-Kpt that accumulated more than 200 kg C / m<sup>2</sup>.” This suggests that for both PL-Wet and DK-Nuf methanogenesis is limited or near to be limited by the soil carbon content in the model. Then we explain L327 “PL-Wet required also to modify range values of q<sub>MG</sub> to 1.0-11.0 leading to the lowest optimized q<sub>MG</sub> value of 4.0 which significantly reduced the RMSD from 227.4 to 80.5 (Fig. S1 and Table S1).” Indeed, a smaller minimum of the cost function have been found when extending the range

values of the parameters leading to  $q_{MG} = 4.0$ . In the discussion section we point out that L532 : “This ratio [ $q_{MG}$ ] was first established from experimental studies that determine the microbial production ratio  $CO_2$  to  $CH_4$  (Potter et al., 1996; Segers, 1998) for various water table positions. These ratio values were found to be between 0.58 and 10000.” Then we provided model values employed in the literature for  $q_{MG}$  L 534 : “Because of this wide range of values, process - based models employed this  $CO_2$  to  $CH_4$  ratio as an adjustable parameter that is weighted by environmental factors such as soil humidity and temperature. Wania et al., (2009) performed a sensitivity analysis study of the LPJ-WHyMe model using 7 sites in which the multi-site optimization value of the  $CO_2/CH_4$  ratio was defined at 10 while other models as CLM4Me use a value of 5.” Therefore, we disagree with the reviewer, a  $q_{MG}$  value of 4 for PL-Wet or 7 for DK-Nuf is not beyond much from the observed ranges nor from values employed in other models. This value only reflects, as we pointed out L321, explain here above and discuss in section 4.2, the limitation of methanogenesis by soil carbon content in the model.

For DK-Nuf, we added some details in the results description L324 “Three parameter ranges were modified for DK-Nuf, the minimum value of  $q_{MG}$  was lowered to 7.0,  $z_{root}$  maximum is increased to the maximum peat depth at 0.75m [in order to consider plant mediated transport in all the peat layers](#), the maximum value of  $T_{veg}$  was increased to 40.0 and the maximum rate of methanotrophy  $k_{MT}$  was enlarged up to  $8\text{ d}^{-1}$  [to decrease the methane oxidation](#) and to obtain in the simulation methane emissions higher than  $150\text{ mg CH}_4\text{ m}^{-2}\text{ d}^{-1}$ .” Here again, these parameters values result from the minimization of the cost function for a site where methanogenesis is limited in soil carbon content in the model. In the discussion section 4.1 we explain L572 that in the present study “Optimized  $z_{root}$  values at sites ranges between 6 and 68 cm depth with the average depth defined at 26 cm which is also the value obtained using the multi-sites approach. These values are consistent with values utilized by Walter and Heimann (2001) that ranged between 0 and 74 cm.” For  $T_{veg}$ , we explain that it is arbitrary defined L586 “ $T_{veg}$  has been introduced by Walter et al., (1996) to describe the density of plants and their efficiency in methane transport for site estimation. It is an adjustable parameter that was scaled to be between 0 and 15 with lower values for ecosystems dominated by trees and shrubs and the highest values for ecosystems dominated by grasses and sedges.” The range of  $T_{veg}$  has been established by Walter and Heimann (2001) based on the study of 4 peatlands sites and a swamp site which may not be representative of all northern peatland sites.

**Comment 2.31:** L335 Why the range of  $q_{mg}$  here is not 4.0-10.7 as before?

**Response :** We modified L335 to “Across sites,  $q_{MG}$  values extend between [4.0](#) and 10.7, optimized  $k_{MT}$  values vary between 1 and  $5.25\text{ d}^{-1}$ . ”

**Comment 2.32:** L381/L550 'soil humidity' should be "soil moisture"

**Response :** Terms 'soil humidity' were changed to 'soil moisture' in all the document L128, L166, L381, L535, L550 and L606 and Table3.

**Comment 2.33:** L385-386 Please provide the snow cover depth data to support the claim on diffusion transport.

**Response :** These sentences are referring to results in Figure2 line c and d which show line e, the simulated amount of methane fluxes via diffusion, ebullition and plant transport and line d, on the right hand side of the y axis, the simulated amount of methane contained in the simulated

snow layers. We added references to Figure 2 in the sentence L385 “This explains the negative methane flux (Figure 2c) produced in winter by the model via simulated diffusion of atmospheric methane in the snow cover (Figure 2d).”

**Comment 2.34:** L403-407, L432-433, L466 Again, please provide the snow depth evidence.

**Response :** Similarly than for comment 2.33, we added references to figures in sentences L402 “As for US-Los and DE-Spw, at CA-Wp1, during the winter simulations show that in the top soil layers some methane is transferred by diffusion (Figure 3c) to the snow cover (Figure 3d). Then a small part of the simulated methane is temporarily stored in the snow (Figure 3d) and the other part is released to the atmosphere via diffusion (Figure 3c). More simulated snow accumulated at DE-Sfn, DE-Zrk, CA-Wp1 and US-Bog where up to 0.8 - 0.04 gCH<sub>4</sub> / m<sup>2</sup> are temporarily stored in the snow (Figure 3d). At FR-Lag and DE-Hmm, fewer methane, less than 0.005 gCH<sub>4</sub> / m<sup>2</sup>, are contained in the simulated snow cover (Figure 3d).”

L432 “In the winter the methane fluxes are stored in the simulated snow cover at FI-Lom (Figure 4d), therefore the simulated surface fluxes above the snow are driven by diffusion (Figure 4c). However, during summer simulated methane fluxes essentially originate from plant mediated transport. At DK-Nuf, PL-Kpt and PL-Wet, simulation results show that fewer methane, less than 0.4 gCH<sub>4</sub> m<sup>-2</sup> d<sup>-1</sup>, accumulates in the simulated snow during winter (Figure 4d).”

L464 “Though, at RU-Che, simulated methane production rate is higher around 100 mg CH<sub>4</sub> m<sup>-2</sup> d<sup>-1</sup> and occurs at 20 cm depth during summer and few centimeters below the surface during winter when snow covers the surface.”

**Comment 2.35:** L411, Can you explain why there are so large differences in the depth of maximum methane production among sites. The main controlling factors for methanogenesis are temperature and water conditions. Did you check the relationships/dynamics of soil temp along depth?

**Response :** The reviewer is correct, variability of the depth of maximum methane production results from the effect of soil temperature and moisture. This relationship derived directly from the implementation of peat decomposition function described in detail in Qiu et al. (2019). In the supplementary document, Figure S3 displays the temperatures and soil moisture at the depth of maximum methane production

**Comment 2.36:** L440 "not sufficient to cause methane ebullition" How much ch4 concentration is enough for the ebullition under your study conditions? Please clarify this.

**Response :** The ebullition threshold is defined by equation 7 (L186). It varies in space and time and depends on the soil pressure and temperature and on the mixing ratio  $m_{xCH_4}$ . In addition to this threshold, the remaining amount above the methane threshold is affected by a probability function defined in equation 8 that depends on soil moisture and  $w_{size}$ . Therefore, it is difficult to define how much CH<sub>4</sub> concentration will result into a flux by ebullition since it is a complex multivariable problem. However, Figure 4c shows the simulated amount of methane that is computed by the ebullition, the diffusion and the plant transport scheme. There are no methane fluxes by ebullition for FI-Lom, there are only methane fluxes by diffusion (in blue) and plant transport (in green).

We added references to the Figure 4 in the sentence L438 : “In contrast, at FI-Lom simulated soil methane concentrations are near 50 gCH<sub>4</sub> m<sup>-2</sup> during summer whereas the winter

concentrations are near  $80 \text{ gCH}_4 \text{ m}^{-2}$  (Figure 4d) which is not sufficient to cause methane ebullition (Figure 4c). Indeed, the ebullition (equation 7 and 8) results from the balance of soil temperature, pressure and gas content, which explain the large diversity of methane fluxes response by ebullition at each site. ”

**Comment 2.37:** L534 Can you give some citations or explanations for the  $q_{\text{MG}}$  range of 0.58~10000 because in table 3 you state the range is 9-11.

**Response :** We do not understand the reviewer comment 2.37 since the citation for the  $q_{\text{MG}}$  range of 0.58~10000 are provided L528 (Potter et al., 1996; Segers, 1998), the value used by other models are reported L533 and explanation for the  $q_{\text{MG}}$  range in table 3 is provided L537 “Khvorostyanov et al., (2008a) and Morel et al., (2019) used respectively  $q_{\text{MG}}$  value of 9 and 10 to simulate methane emissions from arctic peatlands therefore in the present study at first  $q_{\text{MG}}$  were optimized in the range of 9-11 then this range was enlarged only for sites that underestimate methane emissions. “

**Comment 2.38:** L554 Still lacks snow depth or accumulation evidence.

**Response :** Here we are discussing the model implementation and results to enforce these two aspects we modified sentences L551 to “Thus, the optimization of the oxidation rate results from the balance between model inputs and outputs that are respectively available methane and oxygen substrates and methane fluxes which explain this large variability in oxidation rate. In addition, in our model, snow is considered in the diffusion scheme which is in part controlling diffusivity of oxygen from the atmosphere to the ground in winter (e.g. Figure 2c).”

**Comment 2.39:** L641 What are the full names of 'SS' and 'MS'?

**Response :** To ease the reading of the manuscript we added in the section title L 314 “3.1 Single site optimization (SSO)”, L475: “3.2 Multi-site optimization (MSO)” and L641: “by employing single site (SS) and multi-site (MS) optimized parameters”

**Comment 2.40:** L645-646 I am curious that did the plant transport dominate total  $\text{CH}_4$  flux for the whole year, or it only occurred in the summer?

**Response :** Plant transport process is defined by equation 9 in which “The leaf area index (LAI) influences the methane flux depending on the growing stage of the plants” (L216). This means that methane plant transport occurs during the plant's growth period. We modify the sentence L645: “Plant mediated transport (PMT) were the largest simulated fluxes, during the plant's growth period.”

**Comment 2.41:** Table 3, For  $k_{\text{MT}}$ , it is not consistent with the values at L178; for  $M_{\text{rox}}$ , it is not consistent with the values at L206-207; for  $m_{\text{rxCH}_4}$ , the citation is not the same at L188;

**Response :** We revised the citation of Morel et al. 2018 to Morel et al. 2019 in table 3. The citations of Walter and Heimann, 2000; Riley et al., 2011 have been added for  $m_{\text{rxCH}_4}$ . The range for  $M_{\text{rox}}$  defined by Walter and Heimann, (2000) is based on the study of 4 peatlands sites and a swamp site which may not be representative of all northern peatlands sites therefore we employed a larger range 0-1 rather than 0.39-0.98.

**Comment 2.42:** Table 6, Why is the span of mxrch4 not the same as table 3?

**Response :** We do not understand this comment, the range for mxrCH4 are the same in both table 3 and 6. Table 3 presents the parameters and the default values employed for the simulation set up as explained in section 2.2 and Table 6 presents the multi-site optimization results discussed in section 3.2.

**Comment 2.43:** Fig 5 with Fig2s, The performances of water conditions for US-Wpt and RU-Che look not so good. How do you consider this?

**Response :** This comment is the same as comment 2.3, please see our response to comment 2.3.

#### Technical corrections:

**Comment 2.44:** L149 Full names of fmga, fmgs and fmgp?

**Response :** L150 "methane production (fMG, MG: methanogenesis, [a: active pool](#), [s: slow pool](#), [p: passive pool](#)) "

**Comment 2.45:** L186-187 I found both mxrch4 and mwrch4, which one is correct?

**Response :** L187 "where mxrCH4 is the methane mixing ratio in the bubbles"

**Comment 2.46:** L189 What's the full name of 'RR'?

**Response :** RR is the ideal gas constant and is defined at L189. To avoid any confusion we modified it to its more common name R in equation 7 and L189: "It is converted to gCH4 per unit porous volume by the ideal gas constant (R), "

**Comment 2.47:** L189 "MwCH4 and BCH4 the Bunsen... " should be "MwCH4 and BCH4 are the... "

**Response :** To ease the reading, we modified this sentence L188-189 to "It is converted to gCH4 per unit porous volume by the ideal gas constant (R), MwCH4 and the Bunsen methane solubility coefficients (BCH4). "

**Comment 2.48:** L293 Again, mwrch4 or mxrch4?

**Response :** L293 "transport (mxrCH4, wsize, Tveg, zroot) "

**Comment 2.49:** L685 Citations?

**Response :** L685 "global methane budget for natural wetlands located northern of 30°N of 12-61 Tg CH4 y-1 for bottom-up approach and 31-64 Tg CH4 y-1 for top-down approach ([Saunois et al. 2020](#)) "

**Comment 2.50:**

L185 'depend' to 'depends'

L482, Not correct. 'table 5' must be table 6.

Fig 1, 'Lai' should be 'LAI'

**Response :** The manuscript has been modified respectively at L185, L482 and in Fig 1, following specific review suggestions.

