Replies to Referee 1

The article titled "Satellite-based modeling of wetland methane emissions on a global scale (SatWetCH4 1.0)" presents a pioneering approach to understanding and quantifying methane emissions from wetlands across the globe. The authors have developed an innovative model that leverages satellite-based data to simulate methane emissions with a high degree of spatial and temporal resolution. This study is particularly significant given the substantial contribution of wetlands to global methane emissions and the critical role of methane as a greenhouse gas.
The manuscript is well-structured, with a clear abstract that succinctly summarizes the study's objectives, methods, and findings. Overall, this study represents a significant advancement in the field of wetland methane emissions modeling. It is my pleasure to recommend this paper for publication in GMD journal.

Thank you for your positive feedback on the manuscript. We are honored to read that you find our approach innovative and our model important for understanding and quantifying global wetland methane emissions.

My comments here are relatively minor, but I hope may be useful for the authors to consider.

We greatly appreciate your time and effort in reviewing our work. Thank you for your careful reading of the paper and for raising interesting methodological considerations. Hereafter are point-by-point responses to your comments. We hope that we addressed all of your questions and suggestions.

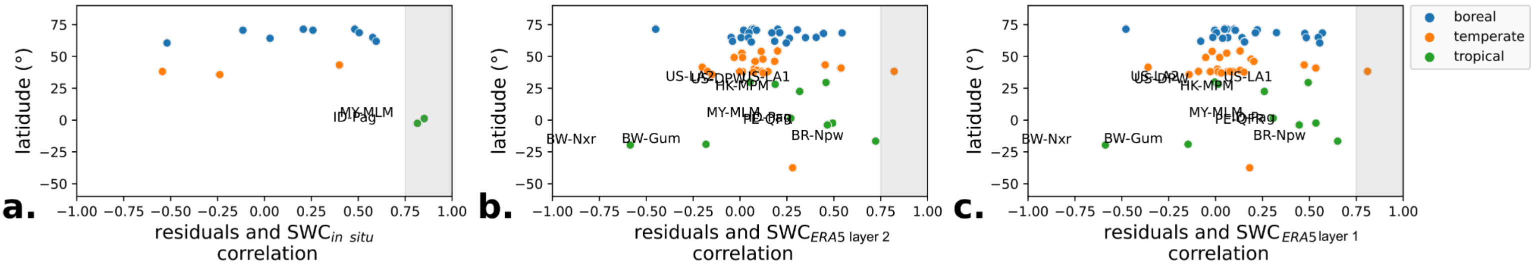
The line numbers given hereafter refer to the tracking-changes PDF manuscript. Changes in the manuscript are given here in violet.

1. L165-L171: Is the reason for using ERA5's lay2 soil moisture because of the high correlation coefficient between the site data and lay2 soil moisture? In this article (DOI: 10.1126/sciadv.aba2724), the soil moisture used is from 0-289cm depth, and I haven't seen how the interannual variation of soil moisture at other layers correlates with the site observations. As far as I know, the interannual variation of soil moisture in lay1 and lay2 is similar, and lay1 is closer to the surface soil moisture. Why not use the data from lay1?

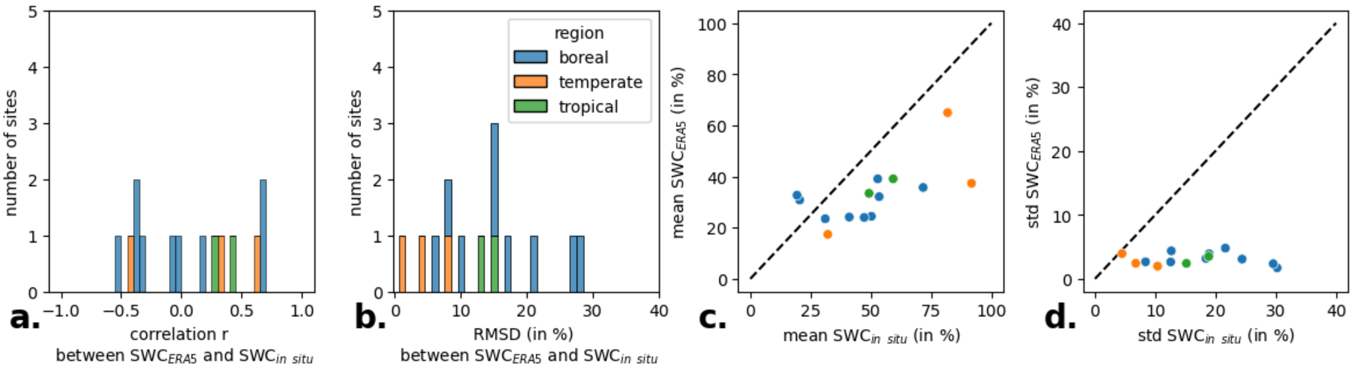
The soil ERA-5 reanalysis data contains 4 layers : layer 1: 0 - 7 cm, Layer 2: 7 - 28cm, Layer 3: 28 - 100cm, Layer 4: 100 - 289cm. We used in this paper the ERA 5 data for two variables (soil temperature-described lines 166 - 172, and the soil moisture - SWC). We used SWC for tests only (Sect 4, line 390-400 of the preprint). Since we are unsure if your comment is on soil temperature or soil moisture, we address both hereafter.

In the model, we use ERA-5 soil temperature. In the literature, methanogenesis is expected to be the highest in the upper saturated layers (Bridgham 2013, Nzotungicimpaye 2021), with maybe highest methanogenesis rates in the ~30 upper cm (Cadillo-Quiroz 2006, Valentine 1994, Priemé 1993). We chose *layer 2* (7 - 28cm) for ERA 5 product to be more representative for the layer where methanogenesis is expected to occur, as the top ERA5 layer is only 7 cm deep. We checked ERA 5 soil temperature, and indeed the temperature of layer 1 and layer 2 were highly correlated ($R^2>0.99$). Layer 2 was also slightly better correlated with site measurements than layer 1 (certainly partly due to probes depth). The difference between layer 1 and layer 2 is very small, and no changes in model output were finally observed using soil temperature layer 1 instead of layer 2.

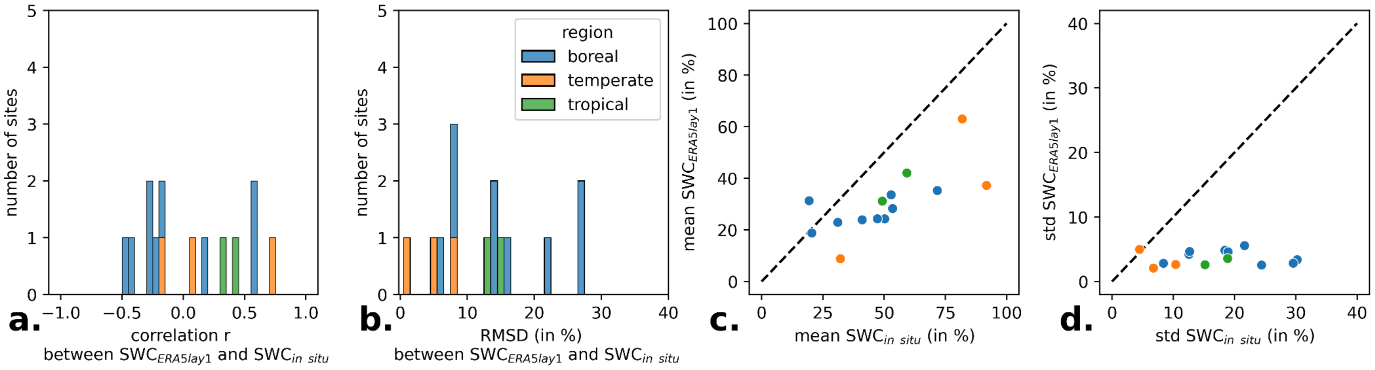
We later discussed the influence of Soil Water Content (SWC) using the data from ERA 5 layer 2 to be consistent with the temperature. Following your suggestion, we checked the differences between SWC layer 1 and SWC layer 2. We repeated the numerical experiments with SWC layer 1 and the conclusions remain the same as with layer 2 (see manuscript L388-395 and Supplementary L19-23). We join here the corresponding Figures. In Fig. 10 we see that the time correlation of residuals with SWC from ERA 5 is below 0.75 for almost all sites, both with layer 1 and layer 2. In adapted Fig.S2 we can see that the SWC layer 2 doesn't reproduce well local wetland SWC measured in situ, especially temporal variations.



Article Fig.10 adapted : residuals correlation with a. SWC in situ, b. SWC lay2, and c.SWC lay 1



Supplementary Fig.S2 with SWC layer 2 (original preprint supplementary Figure)



Supplementary Fig.S2 adapted with SWC layer 1

2. L206-L210: Are you suggesting that the green single-peaked line (Albuhaisi et al., (2023)) is more in line with physical laws but the formula is more complex and prone to non-convergence, which is why such a complex approach was not adopted? So, your point is that the methods of Albuhaisi et al., (2023) should be more reasonable, rather than the others?

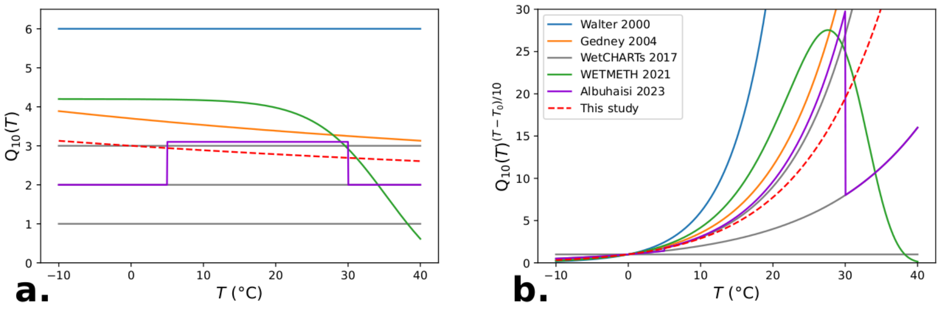


Figure 2. a. Comparison of $Q_{10}(T)$ formulation with Walter and Heimann (2000), Gedney (2004), WetCHARTs Bloom et al. (2017), WETMETH Nzotungicimpaye et al. (2021), and Albuhaisi et al. (2023). **b.** Effect of the different $Q_{10}(T)$ formulations when incorporated in the temperature dependency function.

We believe that you are asking whether the WETMETH approach (the green line) (Nzotungicimpaye et al. (2021)) should be more reasonable. In fact, Nzotungicimpaye et al. (2021) proposed a formulation based on Dunfield et al. (1993); Metje and Frenzel (2007); and Schipper et al. (2014). This formulation enables a temperature optimal range for methanogenesis around 25°C-30°C and include a Q10 formulation as:

$$Q_{10}(T) = 1.7 + 2.5\, tanh(0.1(T_{ref} - T)) \quad \text{with} \quad T_{ref} = 308.15\,°K$$

In their equation, the Q10(T) formulation is set using common Q10 literature values order of magnitude, however this formulation was not fitted with in situ fluxes. During our model development, we derived a Q10(T) equation to have a similar shape using a formulation depending on temperature, having an optimal 25°C-30°C temperature range for methanogenesis, and for which we could fit a $Q_{10\,0}$ parameter like in Gedney formulation. However, when optimizing the parameters with in-situ fluxes, this version of the model did not converge as well as with the simple formulation and, as you mentioned, made the equation more complex. The modeled methane fluxes (local and global) were similar using a simpler formulation. For these reasons, we preferred keeping the simple Gedney version in the model.

Albuhaisi et al., (2023) formulation, on the contrary, has less physical meaning than Nzotungicimpaye et al. (2021). Their formulation produces some abrupt changes, especially an abrupt drop of methanogenesis rates at T = 30°C (Fig. 2.b) which makes it non-relevant for global studies as some tropical wetlands can experience temperatures around 30°C.

The text was modified (lines 210-218 of manuscript with tracking changes) to explain this more clearly, also with respect to comment 3.

3. Fig 2: By the way, could you explain why there are three lines in WetCHARTs, and what do they represent, respectively? What is the significance of such classification? Your results seem to be mainly close to the one with Q10=3.

In WetCHARTs, Bloom et al. used different datasets and parameter values, including 3 different Q10 values (1, 2, and 3). The WetCHARTs ensemble was created by using 3 Q10 values × 9 heterotrophic respiration estimations × 4 wetland extent maps × 3 scaling factors = 324 estimates. This was done to assess the sensitivity of these constraints to the role of carbon, water, and temperature variability in the global spatial and temporal variability of wetland CH4 emissions. We have modified the text of section 3.1 to explicitly explain the different Q10 values shown in Figure 2. The Q10 in WetCHARTs is of a different nature, as the WetCHARTs equation uses heterotrophic respiration, which already contains a Q10. The Q10 from WetCHARTs was then removed from the comparison, as described in the discussion with the Referee #3 (comment 2.).

4. L221: I understand that when you were assessing with the site data, you set $f_w$to 1, and later when generating the product, you utilized WAD2W and TOPMODEL? Or after considering the two different wetland areas, why didn't you compare with this quantity? Was it because you considered the spatial resolution of the gridded data to be too coarse?

For site calibration, we expect the flux tower footprint to be completely - or mostly - covered by wetlands. Therefore, we fit the equation with fw = 1 as you mentioned. Eddy covariance tower footprints are typically between 100 m^2 and 10,000 m^2 (BALDOCCHI et al., 2001; KUMAR et al., 2017).
We considered this approximation to be more accurate than using the WAD2M and TOPMODEL fractions at local sites, as WAD2M and TOPMODEL resolution are quite coarse (0.25°x0.25° pixels ~ 600 km^2 ~ 600 000 000 m^2) and represent the wetland fraction over larger areas than the wetland sites.

5. In Figure 3c, the simulation appears to be quite similar to the observations in the low-value area, but there is a significant difference in the high-value area. Could you briefly explain the reason for this? Is it related to the wetland area f_w being set to 1?

In this graph, SatWetCH4 with the same optimized parameters is applied to all sites. We think the main reason for having the highest differences at the highest flux values is that we did a regression to optimize the fluxes. When minimizing the mean error, a regression tends to flatten the extreme values in estimates. This leads to overestimating some sites (mainly those with lower FCH4 fluxes) and underestimating others (mainly the highest fluxes). Salmon et al. 2022 (https://doi.org/10.5194/gmd-15-2813-2022) have shown these trends in their figure 7, where the multisite optimization is similar to the optimization done here. Moreover, the sites with high mean flux values (mainly tropical sites) are little represented in the flux tower distribution ; and thus have smaller weight in the cost function.

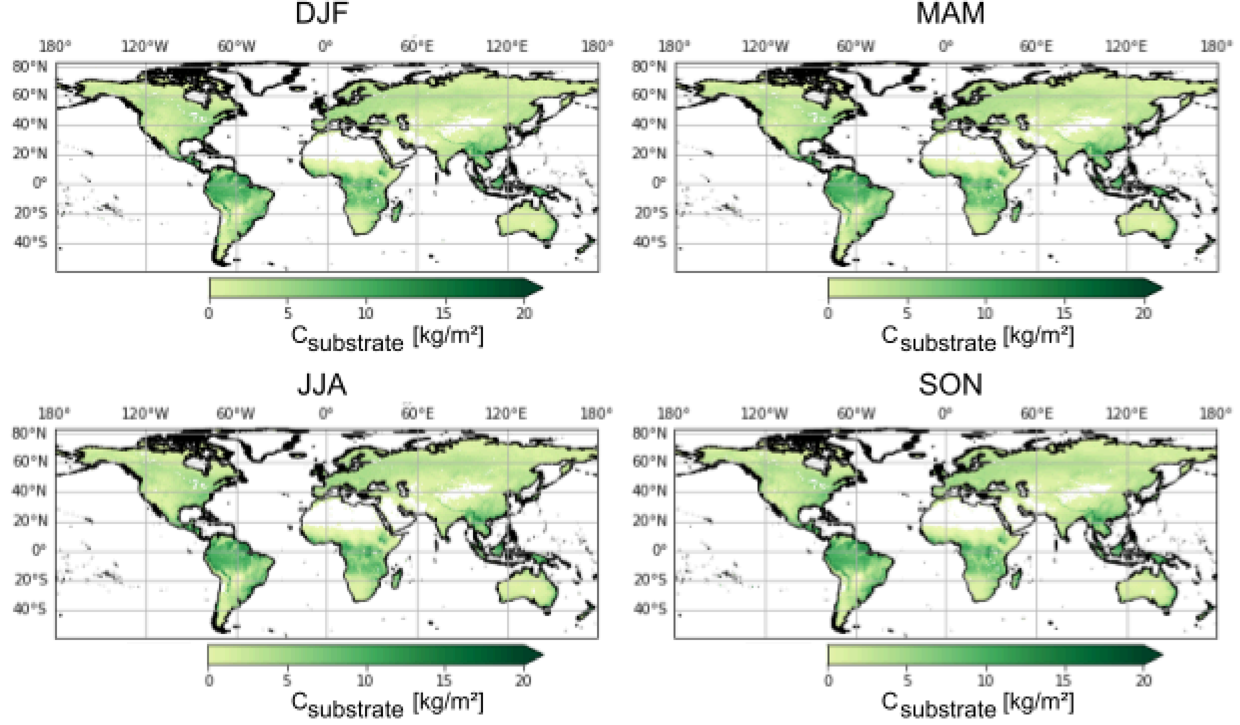
For some sites with high fluxes (tropical and temperate sites), errors in the temporal variations could be partly explained by the lack of a variable for local water, such as soil water content or water table depth (figure 10.a and 10.b). Such variables, if we were able to take it into account, would introduce a different spatial pattern between sites, and hopefully increase the model accuracy in areas with the largest errors.

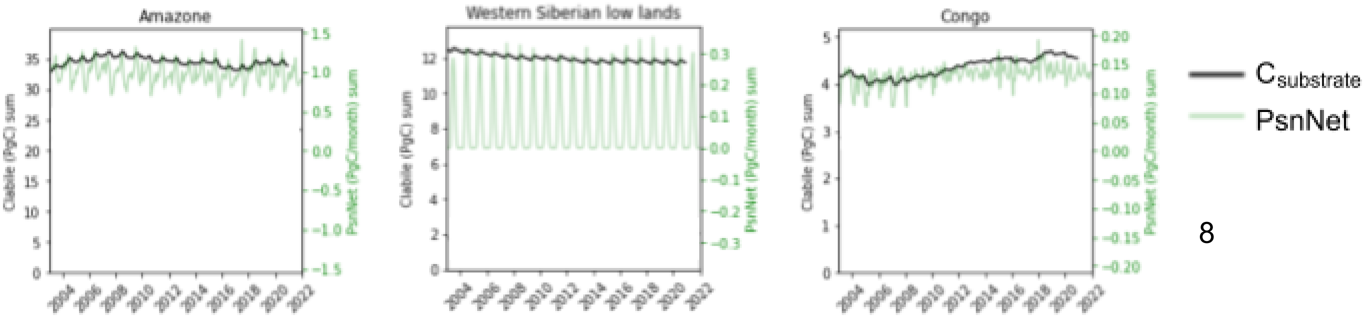
6. L227-L230: From a statistical relationship perspective, it is true that the correlation coefficient between the tropical region and the site data is low. Could you briefly explain the reason? Since your model uses MODIS observations similar to NPP, there should theoretically be more data in the tropical regions. Is it because the emissions are high in the tropics and the mechanisms of methane production activities are not well understood?

The authors believe that indeed, the mechanisms regulating methane production in the tropics is not well represented in this simple model. The model variability is driven only through the wetland extent dynamics and the temperature. However, temperature is not the main driver for the dynamic of wetland emission in the Tropics.

We discuss in Section 4 the role of a local water parameter. We noticed that the model residuals (i.e., errors defined by observation-prediction) of tropical sites and some temperate sites are correlated with a local water parameter. Thus the water table depth or the soil water content could partly explain the temporal variations missed by the model. SWC and WTD are known to be drivers of the seasonal variations over the places where temperatures have small seasonal variations, such as the Tropics. We added a sentence in section 3.2 referring to section 4 : “*Furthermore, the mechanisms driving the temporal variations in tropical methane flux are certainly poorly represented in the model, as discussed in Sect.4.*”

MODIS NPP observations are used to assess substrate availability. The temporal variations of $C_{substrate}$ are rather small (see Figures below). The simulated fluxes reflect on the (spatially heterogeneous) substrate available, weighted (in time and space) by other variables such as the temperature.





7. In Figure 4, should the time period range of the annual average be indicated in the figure title? Is it 2003-2020? In line 249, it is mentioned that WAD2M is from 2003 to 2020, is TOPMODEL climatological? Please clarify.

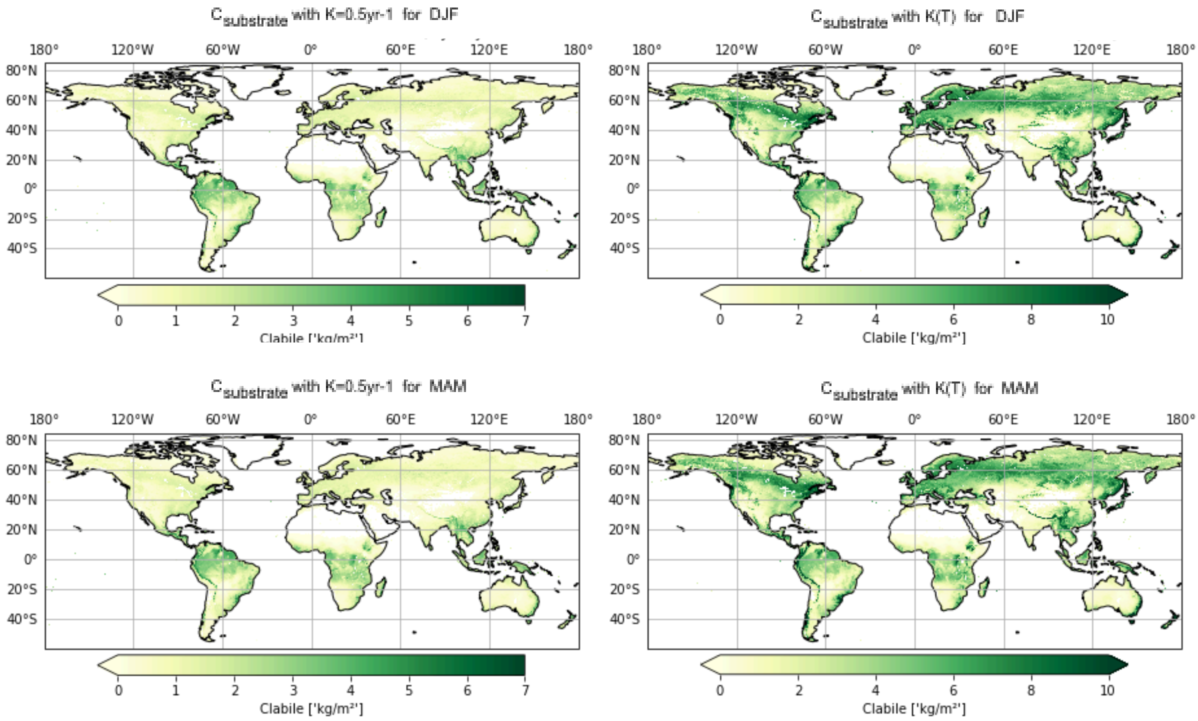
Thank you for your comment. WAD2M is available for 2000-2020 and TOPMODEL 1980-2020, both with monthly resolution. Here we have indeed made a 2003-2020 mean to be comparable with the model run period 2003-2020 (as the time period is limited by PsnNet MODIS data). We have added this detailed information about the considered period in the caption of Figure 4.

8. Line 261: The absolute magnitude of the $C_{substrate}$value is small, and the text provides the corresponding explanation. My question is, after normalization, compared to the other two products (SoilGrids and HWSD), the high-value areas of $C_{substrate}$ in the region north of 60°N and more northerly, could you briefly explain this reason?

These high amount of carbon are due to the turnover rate K(T) value in Eq. 2 that depends on temperature:

$$K\,(T) \;=\; K^{ref}\,Q_{10K}^{(T-TKref)/10}$$

Some easily available carbon is created in summer through NPP in these regions north of 60°N, and not degraded by heterotrophic respiration during winter due to lower turnover rates K(T) because of the low boreal temperatures.
We had run some tests with a fixed turnover rate (K = 0.5 yr^{-1}); i.e, without taking into account the temperature dependency for the heterotrophic respiration. This results in lower carbon amounts (<2kg/m²) in the northern regions, as depicted in the following Figure (left column).
Figure 5 of our prepint manuscript shows that the $C_{substrate}$ spatial distribution is smoother than in SoilGrids and HWSD which are representing total Soil Organic Carbon (SOC). Indeed, SOC and $C_{substrate}$ proxy do not represent the same variable : SOC is mainly contained in peatlands and other organic rich soils, which is not the case for our $C_{substrate}$ proxy that represent easily available organic carbon and is then .



9. L293-294: The overall smaller values of $C_{substrate}$ are due to the lower values obtained from MODIS. Do you think these comparatively smaller values are reasonable? Please explain.

As we fit the methane emissions using a scaling parameter, simulation outputs are fitted so that the equation - that comprises $C_{\mathrm{substrate}}$ variable - fits the fluxes. Then, the order of magnitude of $C_{\mathrm{substrate}}$ doesn't alter the fluxes order of magnitude as the scaling factor k is adapted.

L293-294 are referring to the fact that $C_{\mathrm{substrate}}$ is small (<0.5kgC/m²) over subequatorial Africa due to small values of the MODIS PsnNet input (Fig.5). This leads to small fluxes (<0.05gCH4/m2/month) in SatWetCH4 with both WAD2M and TOPMODEL (Fig.6.a and b.). We discuss this in L310- 318 of the revised version:

"In subequatorial Africa, emissions are highly uncertain from one model to another. The different GMB model outputs show a wide range of emissions (Supplementary Fig.S3). Four of the LSMs have low emissions (<0.1 gCH4/m2 /month), while the other nine have moderate to high emissions (0.1 to 0.5 gCH4/m2 /month). Like the first group of LSMs, the WetCHARTs ensemble mean and the SatWetCH4 model predict almost negligible emissions (<0.05 gCH4/m2 /month), while the LSM ensemble mean estimates emissions around 0.1 gCH4/m2 /month. The number of measurements available to evaluate the simulations is limited (difficult to access areas, no flux towers, no in situ flux or concentration measurements). A hypothetical underestimation of substrate availability $C_{\mathrm{substrate}}$ *in this region could be attributed to cloud cover limiting visible and near-IR observations. Indeed, since the PsnNet parameter of the MODIS parameter is low in this zone, the* $C_{\mathrm{substrate}}$ *dataset estimates a very low available substrate."*

The authors think that the SatWetCH4 emissions could indeed be underestimated over this region due to MODIS data which constrains $C_{\mathrm{substrate}}$ low values. The problem could also arise from an overestimation by other models due to uncertain water detection there in WAD2M. But the lack of flux data over this area makes it hard to know which models are right.

10. L356-358: Why did you previously mention that the global distribution is related to the distribution of wetland areas, yet the wetland distributions in the Southern Hemisphere are opposite for the two, yet the methane emission fluxes from wetlands are similar? In Equation 1, temperature and wetland area are directly proportional to methane emission fluxes; why is temperature considered the dominant parameter in the Southern Hemisphere?

Indeed, simulated CH4 fluxes spatial distribution depends on the wetland distribution, the substrate pattern, and the temperature. Each of these variables have different contributions in terms of spatial pattern and temporal variation.

As the temperature dependency is exponential, the temperature is expected to dominate the seasonal cycle in boreal, and temperate regions (both northern and southern temperate regions) where the temperature variations are important. As a result, the shape of methane emissions seasonality observed in the temperate Southern Hemisphere is driven by the temperature seasonality (high in Nov-March during southern summer).