RC2

In “Ensemble estimates of global wetland methane emissions over 2000-2020,” Zhang and co-authors simulate global methane wetland emissions using 16 process-based wetland models with varying levels of complexity that are participating in the Global Carbon Project. Authors simulate wetland methane emissions for 2000-2020 and simulate the decadal changes in emissions and their large-scale drivers. The modeling ensemble shows an increase in 2010-2020 vs 2000-2010 emissions, and that temperature is the primary driver followed by precipitation and atmospheric CO2 concentration. Authors show that these changes and the drivers are generally support by inversions and observational evidence.

Overall, I think this is a well written study and useful study. In my opinion, it should be accepted after addressing a few comments and questions.

Response: Thank you for your constructive feedback. We appreciate your acknowledgment of the importance of our findings.

Overall comments

The multiple linear regression lacks detail in how the predictors were selected, so it is unclear how robust those conclusions are. Authors choose global mean temperature, global total precipitation, and mean atmospheric CO2 concentration as the predictors, and then state that modeled eCH4 was “primarily associated” with those variables (line 340), but were those the only variables tested? In that case, did the exercise reveal anything new? Authors say that “other confounding drivers might influence eCH4 as well, such as solar radiation, wind speed, and nitrogen deposition” (line 325), but don’t explore these as predictors. Could authors provide more justification for their choice of the three main predictors? Did authors test model performance after leaving any of these predictors out, or adding any of the additional predictors they mentioned?

Response: Thank you for your insightful feedback. We appreciate your questions regarding the selection of predictors in our multiple regression analysis. The choice of global mean temperature, total precipitation, and atmospheric CO_2 concentration as predictors was based on findings from previous studies (Piao et al., 2013; Zhao et al., 2016), which identified these variables as dominant drivers of carbon fluxes in process-based models. We acknowledge that other meteorological factors, such as solar radiation, wind speed, and nitrogen deposition, may also influence eCH4. However, it is important to note that only a few models currently implement wind speed as inputs and the nitrogen cycle, making it practical to focus on the three primary factors. While these additional factors were not explicitly included as predictors, their effects are implicitly captured in the regression coefficients. To address this issue, we state: *"Changes in other meteorological forcings may also influence the estimation of eCH$_4$. These confounding drivers, such as solar radiation and wind speed, although they are considered to have minor impacts on the variations of eCH$_4$, were implicitly accounted for in the regression coefficients."*

How do the ensemble modeling results for the 2020 surge compare with other studies that used satellite data to interpret the surge? Authors mention Peng et al. 2022. In addition, Feng et al. 2023 (https://doi.org/10.5194/acp-23-4863-2023) and Qu et al 2022 (https://doi.org/10.1088/1748-9326/ac8754) attribute the surge to emissions, largely from wetland and water sources in Africa. A note on how your results agree or not would be useful given the attention in this manuscript and in the literature on the 2020 surge.

Response: Thank you for bringing up this important point. We have added statements in the manuscript to discuss the consistencies and discrepancies between our findings and those from the studies mentioned by the reviewer.

Our model ensemble suggests that a large portion of the methane increase in 2020 originates from the tropics, which aligns with the conclusions of Peng et al. (2022), Feng et al. (2023), and Qu et al. (2022). However, our results do not indicate the same magnitude of increase as reported by Peng et al. (2022). This difference may be partly attributed to uncertainty in the climate forcing inputs used in the model simulations—specifically precipitation data—where Peng et al. (2022) utilized three sets of reanalysis data, whereas our study use CRU and GSWP3-W5E5.

Additionally, there are discrepancies in the increase in wetland CH4 emissions between our study and those of Feng et al. (2023) and Qu et al. (2022). While our study suggest that Africa in 2020 has various change -0.2[-1.5-0.7] Tg CH4 yr-1. These discrepancies are primarily due to differences in methodology. Feng et al. (2023) and Qu et al. (2022) used atmospheric inversion with GOSAT satellite measurements as constraints. The GOSAT data over the tropics is limited by availability and is influenced by factors such as aerosols and clouds, which affect the accuracy of XCH4 estimates based on XCO2 measurements. In contrast, our process-based models cannot produce such high increase. This is an area that requires further investigation.

The strengthened discussion about 2020 surge is as follow: "The models consistently show that 2020 is the strongest positive anomaly year during 2000-2020, with a net increase of 2 [-2, 7] Tg CH_4 yr^{-1} (mean [min, max]) in 2020 compared to 2019. This positive anomaly in 2020 (Table 1) is broadly consistent with a recent study (Peng et al., 2022) that reported 6±2.3 Tg CH_4 yr^{-1} based on simulations of two bottom-up models with different climate datasets. The discrepancy in estimated magnitude between the Peng et al. (2022) and our results are partly due to the parameterizations of CH_4 module that causes lower annual magnitude in this study (~ 162±23 Tg CH_4 yr^{-1} in 2020) compared to the Peng et al. (2022) study (177±31 Tg CH_4 yr^{-1} in 2020). Additionally, the precipitation inputs in the climate forcing used in this study show a lower positive anomaly (~ of 20 mm yr^{-1} in CRU over global wetland) in precipitation in 2020 compared to the reanalysis-based estimates (~ 40-117 mm yr^{-1} over global wetland used in the study by Peng et al., (2022), which leads to lower estimates of wetland area and consequently lower emissions in this study. Moreover, our model ensemble does not indicate a strong increase (-0.2[-1.5-0.7] Tg CH_4 yr^{-1}) in eCH_4 in Africa in 2020. This contrasts with recent atmospheric inversions (Feng et al., 2023; Qu et al., 2023), which suggest a large increase of 11-17 Tg CH_4 yr^{-1} above 2019 levels in African CH_4 emissions for 2020. The estimated increase from these inversions is equivalent to 55%-85% of total wetland CH4 emissions in Africa during 2010-2019 in our study (Figure 2). These discrepancies highlight the need for further studies to investigate the differences between these two approaches, including uncertainty in climate inputs in process-based bottom-up models and partitioning difference sources in atmospheric inversions.".

Minor comments

Line 52-53: This seems like a strong statement. I think this has been addressed, for example in inversions and in the authors' previous works, though perhaps not in the way it is addressed here. Consider being more specific.

Response: We have revised the sentence to *"However, despite reports of rising emission trends, a comprehensive evaluation and attribution of recent changes remains limited."*.

Line 55-56, "with an average decadal increase…": this sentence is a little unclear.

Response: We have revised the sentence to *"Our results estimated global average wetland* CH_4 *emissions at 158±24 (mean ± 1σ) Tg* CH_4 yr^{-1} *over a total annual average wetland area of 8.0±2.0* Mkm^2 *for the period 2010-2020, with an average increase of 6-7 Tg* CH_4 yr^{-1} *in 2010-2019 compared to the average for 2000-2009."*

Line 97-98: Y Zhang et al. 2021 (https://doi.org/10.5194/acp-21-3643-2021), using GOSAT, is a useful comparison here.

Response: Thanks for providing the reference. We have cited the Zhang et al., 2021 in the text.

Line 136-137 “different prescribed parameters”: Does this mean that each model has a different set of parameters and inputs, or that a different set of parameter values is given to each model? The current statement is vague.

Response: We have modified the statement to *“The prognostic wetland areal dynamics were independently determined by each model’s hydrological modules, which use water table depth or soil moisture, combined with sub-grid topographic conditions to determine saturated areas within a land surface grid-cell (Zhang et al., 2016; Xi et al., 2021).”.*

Line 144: Authors mention high correlations for the temperate region and high latitudes, but what about the tropics with the most emissions? Ensemble mean agreement with GIEMS2 in that region seems important, but it is not discussed and it is hard to tell the performance of the tropics from Figure S1.

Response: We have modified the statement to clarify as follow: *“The modeled temporal variations in wetland areas show high correlations with satellite-based products for temperate regions and high latitudes (Fig. S1), except in the tropics. The limited agreement in the tropics may be due to the influence of aerosols and clouds on satellite-based measurements, as well as the process-based model's performance limitations in representing wetland areas.”.*

Line 205 and apparent Q10: Could authors comment on the choice ambient vs soil temperature here? Given the hysteresis effect, and evidence that methane emissions follow soil temperature rather than air temperature, soil seems the more logical choice, but I may be misunderstanding.

Response: Thank you for your insightful comment. All the models used in this study do indeed calculate soil temperature as part of their internal processes. However, for the purpose of unifying the analysis across models, we opted to use air temperature for a consistent comparison across models that may handle soil temperature dynamics differently.

Line 226, “Suggesting enhanced wetland-CH4 sensitivity under climate change”: To me, authors haven’t demonstrated that the larger IAV in the second decade considered is evidence of larger sensitivity under climate change. The statement may be true, but I don’t think authors have demonstrated it, so I suggest adjusting the statement or providing more evidence.

Response: We have revised the statement to *“The model ensemble demonstrates a consistent increase in interannual variability (IAV) in ΔeCH_4 from 3.6±1.6 Tg CH_4 yr^{-1} during 2000-2009 to 4.7±1.5 Tg CH_4 yr^{-1} during 2010-2020, suggesting a potential increase in eCH_4 variability under climate change.”*

Figure 2: Could authors add identifying markers for the regions in panels c,d to the maps?

Response: Thank you for the suggestion. We attempted to add markers to the maps a and b. However, after testing various options, we found that the markers cluttered the visual presentation and detracted from the clarity of the maps. To maintain readability, we decided not to include them. Instead, we have enhanced the borders between different regions to improve visualization.

Line 317-318, “with a range of -0.4 and 9.0 Tg…”: Is this the distribution of coefficients among all the wetland models?

Response: Yes this is among all the wetland models. We have modified the sentence to clarify: *“The regression coefficients for γ is 4.6 Tg CH_4 yr^{-1} ℃$^{-1}$, with a range of -0.4 and 9.0 Tg CH_4 yr^{-1} ℃$^{-1}$ between the 10th and 90th percentiles among all models.”*.

Figure 3: It’s unclear what the Gaussian density distribution curves represent, could more description be added to the caption? In panel b, the dashed lines are too faint to distinguish.

Response: The curves represent the probability distributions of the fitted parameters, derived from values of the individual models. The Gaussian distributions are fitted to show the range and central tendency of these sensitivity coefficients across the models. We have revised the figure caption for clarification: *"The curves represent the probability distributions of the sensitivity coefficients across the models, assuming a Gaussian distribution."* We’ve modified the dashed lines to be thinner for better virtualization.

Line 411-413, “Furthermore…eCh4”: The meaning of this sentence is unclear.

Response: We have revised this sentence to clarify as follow: *“Furthermore, the modeled ensembles of prognostic wetland extents offer a complementary approach to satellite-based estimates (Prigent et al., 2020; Zhang, et al., 2021) and their impact on the spatial distribution of global eCH_4.”.*

Lines 418-421: The MLR analysis seems to show a lower relative importance of the CO2 fertilization effect. Could authors reconcile the MLR analysis with the factorial analysis on this point?

Response**:** The MLR analysis of the CO_2 fertilization effect is consistent with the values calculated from the factorial analysis. The mean sensitivity coefficient β is 0.18 Tg CH_4 yr^{-1} ppm^{-1}, which corresponds to an approximate 2.3% increase relative to the annual total of 158 Tg yr^{-1} under a 20 ppm increase in atmospheric CO_2 concentration. As suggested by the reviewers, we have revised the statement as follows: "The mean sensitivity coefficient β and the results from the factorial experiment suggest a net increase in eCH_4 of 0.1%-2.3% relative to the annual total under an average ~20 ppm increase in atmospheric CO_2 concentration."

References:

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