

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-220

Anonymous Referee #1

Referee comment on "Methane emissions from Arctic landscapes during 2000–2015: an analysis with land and lake biogeochemistry models" by Xiangyu Liu and Qianlai Zhuang, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-220-RC1>, 2022

General comments:

This manuscript has done parallel modeling work using two models to estimate the methane emission from wetlands and lakes in the Arctic region. Two latest dynamic inundation data and lake distribution data were used to drive the model. The separation of the lake/wetland area in the dataset reduced the main source of uncertainty in previous bottom-up simulations for lake/wetland methane emissions called "double counting", which is caused by massing up the lake/wetland distributions.

The objectives of this study are meaningful, trying to reduce the uncertainty of bottom-up methane estimation. I find the story inside complete and exciting. Moreover, the detailed analysis of different drivers of methane emission may contribute to the community of methane-related biogeochemistry modeling. The only part missing or not clear from my perspective uncertainty of the model simulation. Thus, I suggest a publication after minor revisions. Below are some of my comments that may help elucidate the strength and limitations of the proposed work.

Specific comments:

Line 16-17: What are the uncertainties?

Line 20-22: As previously mentioned the emissions are significantly affected by humidity and vapor pressure.

Line 27: IPCC 2014 is not the latest one. IPCC6 is published one year ago. What is the newest IPCC6 report number?

Line 29-35: I feel people will get confused about these introductions about wetland emissions. It would be more clear if put them together instead of two paragraphs.

Line 40-42: People may expect a little bit more mechanisms of lake emission. Like why it will be high in spring?

Line 59-61: 1) As you mentioned the resolution will be the key issue for the double counting issue. Then what is the resolution for the data you used or what model resolution should be highlighted here. 2) Or if resolution is not the matter. You may need want to mention why involving those two datasets or models can avoid double counting.

Line 60: "unpublished data". Is the data going to be published somewhere? How people are going to access them

Line 100-105: What are the input of vegetation types, soil types coming from?

Line 118-120: This part needs to be highlighted and extended for details.

Line 135: Here should be "wind speed at 10 m"

Line 137-138: Are they the same as what are inputted in ALBM?

Line 154: Should be "(using TEM-MDM)"

Line 158-159: Have you mentioned what data you used for spin-up?

Line 167: Under what scenarios? and we have the latest IPCC6 report, have you checked whether the number is changed?

Line 174-176: Why do you suddenly mention the isotope here? I don't think you have used any of the isotope-related analyses.

Line 182-183: What is the uncertainty of your simulation in each year? Some analyses like considering the uncertainty of input data may be good to be included.

Line 184: How are those uncertainties coming from?

Figure 1 and table 1: Fig1a is duplicated with table 1. Also, table 1 is more suitable for a time-series plot

Line 209-211: I don't understand here.

Figure 2: Please label the simulation type also in the figure instead of only the caption.

Table 2: why do some of the results have no superscript? Also, these superscripts should be explained in a footnote instead of the table title.

Table 3: why 0 values here?

Figure 4: So can I understand this way that the high correlation of radiation is caused by the heating effect of radiation and the high sensitivity of temperature in your model?

Line 264: I didn't see how your simulation results fit in the previous ranges. Could you summarize the results and also your results for comparison?

Line 275: This part is interesting and should be talked about more about why you can attribute this.

Line 288-290: This is a very interesting conclusion since we always know that there are huge differences between bottom-up modeling and top-down modeling results of CH₄ emission. I may expect to see emphasizing more explicitly how the uncertainty is reduced based on your study and what ranges you suggested.

Line 296-298: Are they possibly due to your study area being mostly located in boreal so the seasonal cycle of all inputs and methane emissions are strong? Then they should be easily correlated. Their being highly correlated may not be simply explained as they are significant to wetland emission, since another possibility is that they can be confounders, correlated with each other (e.g, SR, and temperature). Causal relations may be considered here