

Interactive comment on “Model estimates of climate controls on pan-Arctic wetland methane emissions” by X. Chen et al.

X. Chen et al.

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"In their paper Chen et al. describe the sensitivity of their wetland and methane model to historical and future climate change. Based on observations of wetland maps and methane fluxes they find a model derived increase of methane emissions of 20% over the second half of the 20th century period. While this result itself is not ground breaking, and most probably model dependent, they achieve to incorporate dynamic wetland area changes into their methane estimates, which is rarely done in methane process modelling studies. Hereby also lies the shortage of the study. Methane fluxes are analysed in great detail, but the simulated wetland tile fractions, e.g. changes of wetland area in the future, are not presented. I thus encourage the authors to cover this aspect and suggest a publication of the paper after other minor revisions. The paper is very

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well organised and written concisely."

We thank the reviewer for the careful reading of the manuscript, as well as his/her insights into the analysis. However, it is important to point out that our model does not include dynamic wetland area. Rather, in our model the wetland tile fraction is fixed, and wetlands are peatlands, which has a fixed area. What does change is the seasonal area of inundation those fractions of our grid cells that are prescribed to be peatland. This is a subtle but important difference. For this reason, it is incorrect for reviewer to characterize this as "dynamic wetland area". But if the reviewer is referring to inundated area by "wetland area", then we do have these dynamics in the VIC model. Regarding the fact that our results are model-dependent, reviewer #1 also had this concern; we have added an analysis of parameter uncertainty to address this point (see response to reviewer #1). Our responses to other specific comments are included below.

General:

"I really like the analysis on methane fluxes and spatial changes for one future projection. As mentioned above I would like to see which part of the methane changes is related to changes in wetland area. Are they spatially or temporally correlated? If precipitation is the dominant factor for methane emissions in the future, instead of temperature, would this mean wetlands are more susceptible to dryness (on-off state of emissions) and thus larger interannual variability in methane emitting areas and emissions? In addition to mean changes, a paragraph about interannual variability of methane changes would be worthwhile."

We assume by "wetland area" the reviewer is referring to "inundated area" (see above). We have added the analysis of inundation area over the historical period: 1) We added a map (figure 13) showing the changes in inundation area and water table level from each wetland grid cell between the periods 1960-2006 and 2081-2100; 2) We included a paragraph in the manuscript that talks about this change, as well as its possible relations to changes in the methane emitted from the wetland. We also calculated a

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spatial correlation coefficient between changes in inundation fractions, and changes in methane emitted, to check whether they are spatially correlated. The discussion of interannual variability in methane emissions is handled with the comments from reviewer #1.

Specific:

p. 5942, l. 10: add CH₄ emissions in "... sensitivities of CH₄ emissions to air temperature, ..."

The sentence is now rewritten as "We characterized historical sensitivities of annual CH₄ emissions to annual average air temperature, precipitation, incident long- and short-wave radiation, and atmospheric [CO₂] as a function of average summer air temperature and precipitation".

p. 5945, l. 13: typo: Seneviratne

Corrected.

p. 5948, l. 12: What type of plants are simulated by VIC? Is plant productivity dependent on the wetland water table? Are there plant stresses incorporated?

VIC simulates all the MODIS land cover types. When it comes to plants, it does not simulate each type of plant, but only several categories (such as broadleaf evergreen forest, broadleaf deciduous forest, needle leaf evergreen forest, needle leaf deciduous forest, shrubland, grassland, and peatland). Yes, the net primary productivities of these plant categories depend on the wetland water table depth, with an inhibition effect when the water table is above the soil surface (Bohn et al., 2013). We have inserted a statement to this effect into section 2.2.

p. 5949, l. 5: Soil carbon pools normally have turnover times of several centuries. 50 years spinup thus seems to be relatively short. How are they brought into equilibrium? What's the impact on methane emissions?

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This is a good point, about which we were not sufficiently clear. Model spin-up consisted of two stages: (1) initialization of carbon pool storages, and (2) 50-year spinup to stabilize moisture and carbon pools. We initialized soil carbon pools via an iterative procedure in which we identified the initial storage that would result in zero net change in carbon storage over the period 1948-1957. Then, to account for the pools' not yet having reached equilibrium with recent Holocene climate, we rescaled all three pool storages by the ratio of observed to simulated total carbon storage across West Siberia, using observations from Sheng et al. (2004). Then we ran the model for 50 years ($5 \times$ the decade 1948-1957) to stabilize its moisture and carbon storages. Starting from the model state at the end of this 50-year spin-up, we then performed simulations for 1948-2006. We have added a statement to this effect in Section 2.2.

p. 5949, l. 14: typo : '... expressed as a function ...'

The sentence is now corrected: "Air temperature and longwave radiation were considered together, since downward longwave radiation can be expressed as a function of near-surface air temperature (e.g., Brutsaert, 1975)".

p. 5949, l. 18: typo: '... each year's ratio ...'

This is now corrected as "For annual total precipitation and annual average shortwave radiation, we linearly regressed the annual values, computed each year's ratio of detrended to original annual values, and multiplied all original daily values by that ratio for each day within the year".

p. 5951, l. 10: typo: '... dominant emission controls'

This is now corrected as "Identifying the dominant emission controls".

p. 5952, l. 20: Please also show a map of modelled changes in lake-wetland fractions over the historical period.

We have now included a map showing the difference of inundation fractions between historical (1960-2006 average) and future (2081-2100) periods (figure 13). We realize

that this is not exactly what the reviewer asked for, but we believe that showing the difference between historical and future fits better into the structure of the paper, since we show the differences in correlations between these same two periods in Figure 12, which allows for comparison between areas that got wetter or drier and areas that changed from T- to P- limitation or vice versa.

p. 5953, l. 13: If emissions are strongest in the forest belt: are there forested wetlands present in the model? Or is this a combination of two variables, i.e. forest derived NPP and wetland fraction, that happen to maximize?

The boreal forest belt does contain some forested peatlands, as well as nonforested peatlands and peatlands that contain sparse tree cover. But this is not what we meant. By this statement, we simply mean that peatlands in the boreal forest belt have the highest emissions rates in our model. Our model does not explicitly simulate forested peatlands; peatlands are represented as a combination of shrubby, herbaceous, and moss cover. However, we prescribed LAI with the MODIS MCD15A2 product, and peatlands in the boreal forest belt do have higher LAI values than elsewhere (Fig. 2); thus those peatlands did receive higher labile C inputs than elsewhere. We have added statements to sections 2.2 and 3.1 to clarify these points.

p. 5954, l. 1: This "north"- "south" difference is it because of wetland vegetation type, i.e. sedges versus sphagnum moss, or peat types, i.e. bogs versus fens? What does it mean biogeographically. Please introduce these terms and give a bit more detail.

We apologize, “wetland type” was a poor choice of words here. “Bioclimatic zone” is more appropriate. The north-south difference is simply a result of the calibration in Bohn et al. (2013) achieving a better fit to the observations when a different set of model parameters was applied in the northern half of the West Siberian Lowland domain, following the approximate boundary between the southern/middle taiga and northern taiga/tundra climate zones. To transfer these parameters to the rest of the high latitudes, we chose an LAI threshold to approximate the boundary between these

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two portions of the boreal forest belt. We have inserted a statement clarifying this point in section 2.2 (where this is first explained), replaced “wetland type” with “bioclimatic zone” on page 5954, and also have added text referring the reader to section 2.2 on page 5954.

p. 5954, l. 25: typo: Table 4, 4th column

This is now corrected as “Emissions from the control runs are shown in Fig. 5b–f. Defining the net impact of a driver as the difference between the historical trend in CH₄ emissions and the trend of the corresponding control run (Fig. 5a and Table 4, 4th column), . . .”.

p. 5963, l. 10: Please cite Stocker et al., 2013 that find a constant feedback climate CH₄ factor, albeit an increase in arctic methane emissions in the future. They use CMIP5 simulations paired with a dynamic vegetation model, wetland plant functional types and related methane emissions.

We have modified this sentence: “. . . (the wetland–climate–CH₄ feedback as discussed by Ringeval et al., 2011, Koven et al., 2011, and Stocker et al., 2013)”. Also the reference list is updated to include this paper.

Figure 10: Years on time axis are not nicely spaced.

fixed.

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