Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-109-AC2, 2019


Climate of the Past Discussions



# *Interactive comment on* "Terrestrial methane emissions from Last Glacial Maximum to preindustrial" *by* Thomas Kleinen et al.

**Thomas Kleinen et al.**

thomas.kleinen@mpimet.mpg.de

Received and published: 8 November 2019

We very much thank the reviewer for taking the time to review our manuscript. I have included the reviewer's comments in bold font, while our reply is in normal font.

**Manuscript summary: Thomas Kleinen et al. present an analysis of changes in methane fluxes from wetlands, termites and wildfires since the LGM. The analysis is performed using the Max Planck Institute for Meteorology Earth System Model, which explicitly simulates methane emissions (and the soil sink). Time-slice experiments are performed in the model, at 5 kyr intervals beginning at 20 kyr. The model is also run for the present day and compared with best available methane budget assessments. The authors find that wetland methane emissions**



Printer-friendly version

Discussion paper

**dominated the changes in atmospheric methane over this time, and that tropical wetlands were the most important component of this.**

**Overall assessment and major comments: It is difficult for me to assess the technical aspects of the MPI-ESM work, as I do not work with ESMs myself; I hope that another reviewer is able to do this. That said, the provided descriptions suggest a comprehensive and well-grounded approach, and the MPI Meteorology group does very good work in my opinion. The model simulates present-day methane emissions that are reasonable and generally compare well with top-down and bottom-up constraints. The model also produces methane emissions that appear to be mostly consistent with the ice core atmospheric methane record. My main concern with this submission to CP is its relative lack of novelty. I view CP as one of the leading journals publishing on paleoclimate, and as such I think that successful submissions to this journal should add substantially to our understanding of some aspect of paleoclimate. The major finding of the paper (that tropical wetland emissions were the main factor driving the LGM - PI atmospheric methane change) has been argued for many times previously, including by model-based studies. While there have been studies arguing for other factors (e.g., the Kaplan et al 2006 study the authors cited), the leading role of tropical wetlands is the most accepted explanation. I think additional model results are valuable, even if they only reinforce the currently accepted hypothesis, but I'm not sure that CP is the best place – Earth System Science Data may be a better fit for this kind of study.**

**It may be possible that the work described in this manuscript is much more technically advanced than prior efforts. In this case, a publication in CP may be warranted, but the authors should then make a very clear argument for why their model is superior to what has been done before, and is expected to produce the most reliable results. Additional comments: I would recommend the addition of ice core constraints regarding the methane interpolar gradient (e.g.,**

**Baumgartner et al., 2012, Biogeosciences) into the analysis – is the partitioning between tropical and extratropical sources in the model consistent with these constraints?**

We very much thank the reviewer for the overall praise that we read from her or his comments. However we have to disagree in some aspects: Yes, the reviewer is perfectly correct that our finding that tropical wetlands are the dominant source of methane is not novel in itself. However, to our knowledge nobody has been able to show this in Earth System Model results, certainly not in a setup as internally consistent as ours. We are able to show that we get reasonable emissions for the present-day situation, including a latitudinal distribution that is consistent with atmospheric inversions. Most other studies that we are aware of were not able to show this. We are also able to show that our emissions for other time slices are reasonable, in the sense that they are similar enough to ice core reconstructions to fall within a quantified uncertainty range, and we do not require major adjustments of the atmospheric lifetime of methane in order to achieve this. We therefore argue that our results are more technically advanced than previous efforts. We will also add a more thorough analysis on the reasons why we do get these better results than previous studies, as detailed in the reply to referee 1. The reviewer's point about the interpolar gradient, however, we regard as a very good suggestion, we will certainly take it up in the revision.

**Page 7, last paragraph (around line 210). The disagreement between model results and satellite observations for surface inundation is discouraging. I would recommend more discussion regarding how much uncertainty / error this could potentially introduce into the model wetland emissions estimates.**

We will attempt to do so. Unfortunately the current state of remote sensing of inundation under closed canopies, as in tropical rainforests, leaves much to be desired, and major discrepancies exist between different remote sensing products. We acknowledge that we did discuss this well, we will attempt to improve it for the revised version.





Printer-friendly version

Discussion paper

Minor comments: Line 15 – 17. The Oldest Dryas – Bolling was an interval of similarly rapid methane change, I recommend mentioning this

Thank you for reminding us, we will do so.

Paragraph around line 50. I would recommend adding the GESO-Chem LGM and PI results of Murray et al., 2014, ACP into the discussion of methane lifetime.

Yes, thank you for reminding us. Somehow the Murray et al. reference was lost in one of the previous revisions of the manuscript, we will add it again.

---

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-109, 2019.