

Interactive comment on “Terrestrial methane emissions from Last Glacial Maximum to preindustrial” by Thomas Kleinen et al.

Anonymous Referee #2

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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 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
C1

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 inter-polar 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?

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.

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

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.

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