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

Thomas Kleinen et al.

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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.

The authors present a comprehensive suite of ESM simulations including methane emissions. They focus in 5 ka intervals from the LGM to the present day. They show that emissions are reduced by around 50% at the LGM relative to the PI, meaning that no atmospheric lifetime change is required to explain the observed low concentration at the LGM, consistent with 3 recent studies of the atmospheric chemistry of the LGM. This is the first study to reconcile the emissions required by the observed CH4 drop without requiring to a large change in

lifetime. Hence, these results will be important for understanding CH4 during ice-ages. I recommend minor revisions as explained below.

Main comments

To me, the main results of the paper require more explanation. If the model is able to correctly resolve a 50% emissions reduction at the LGM relative to the PI, there may be different reasons for this, e.g.: (i) the simulated LGM climate is less favourable for emissions than in older studies; (ii) the CH4 model is more sensitive to LGM conditions than in older studies, either through a different tuning, or because of increased model realism/complexity. (iii) the coupling of the methane model to the ESM increases the sensitivity to LGM conditions. In my opinion these options need to be clearly evaluated, otherwise we only know that it is possible to get this 50% reduction but not why or how.

Yes, we agree that this is the major shortcoming of our manuscript. While we will not be able to explain why older studies did not get similar results – we do not have access to their models after all – we should be able to shed some more light on the reasons why our model does what it does. This will require further model experiments however, in order to isolate the different factors. At the time we originally wrote the manuscript, we did not have a working offline configuration of the land surface model available, so we couldn't perform the necessary sensitivity experiments. In the mean time, we have been able to solve the issues with the offline model, allowing us to run the necessary sensitivity experiments.

Moreover, these results don't show increasing southern hemisphere emissions over the past 6ka that would explain the observed recovery of atmospheric CH4, thus meaning that a human influence is not required. It would be good to understand how and why your results don't replicate this result from Singarayer et al., 2011. You discuss this a bit in lines 397-402, but I believe this could benefit from more detail, without needing further simulations.

We will clarify this in the revised version.

Technical comments Line 1: Sorry to be picky, but past tense would make more sense: "Underwent", "nearly doubled" etc.

Thanks for being picky – we will address this.

Line 50: "However, Levine et al. (2011) found very small changes in CH4 lifetime between LGM and PI" You could mention here that this has also been found by Murray et al 2014, and Hopcroft et al 2017, using different atmosphere-chemistry models. It would also be worth pointing out here why the lifetime didn't change. i.e. because of balance of competing influences from changes in reactive compounds like isoprene and reduced reaction rates due to lower temperatures, and lower generation of OH from lower water vapour levels.

Thanks, we will include that in the revision.

Line 54: Here you can set up the main finding of you paper, by pointing out here that none of these earlier studies have managed to get the right change in emissions and lifetime together.

Yes, we will do so – see also reply to reviewer #2.

Line 177: insured -> ensured

Line 313: delete 'in'

Line 397: This is really interesting, but why/how does your model manage this and previous studies did not? I realise this is complex, but are there any obvious difference in your simulated LGM state? Is it drier in critical areas? Or is it due to the added complexity of your wetland module?

As detailed above, we will perform some additional offline sensitivity experiments to address this. My assumption is that it's a combination of the relatively large change in soil carbon stocks and the temperature sensitivity of methane production that we

included, which is relatively new and likely wasn't included in at least some of the previous modeling studies.

Line 399: This is an interesting finding. What is the explanation though? Do you see the same reduction in precipitation and hence wetland flux in Southern hemisphere as found by Singarayer et al 2011?

We will analyse this in more detail and discuss it in the revised paper.

Line 433: I think I understand the meaning of this sentence: "In total the wetland emissions account for 93−96% of the net CH4 flux, and all other methane sources are of minor importance." , but perhaps you could reword it.

Thank you for pointing this out, we will clarify it.

Figures: It is slightly surprising to see Termite CH4 emissions in Europe.

We agree. However it is what we get when we implement the termite model from the Kirschke et al. / Saunois et al. reviews. We may add a short discussion of this to the section.

References L. Murray, L.J. Mickley, J.O. Kaplan, E.D. Sofen, M. Pfeiffer and B. Alexan- der (2014). Factors controlling variability in the oxidative capacity of the troposphere since the Last Glacial Maximum, Atmos. Chem. Phys. 14, 3589-3622.

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