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Interactive Comment

Interactive comment on "Analysis of the global atmospheric methane budget using ECHAM-MOZ simulations for present-day, pre-industrial time and the Last Glacial Maximum" *by* A. Basu et al.

Anonymous Referee #3

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Review of: "Analysis of the global atmospheric methane budget using ECHAM-MOZ simulations for present-day, pre-industrial time and the Last Glacial Maximum"

This paper introduces a simple parameterization of methane emissions that runs in conjunction with a high-resolution map of slope surfaces and hydrological parameters from the CARAIB vegetation model. Methane emissions were calculated for the Last Glacial Maximum (LGM), Preindustrial Period (PI) and Present Day (PD) within the ECHAM-MOZ model. The loss of methane is set through a specified OH sink with other methane sources specified (ocean, biomass burning, termites).

This was an interesting paper and seems to represent an advance in our understanding of the long-term evolution of wetland methane emissions.

However, I have some real concerns about the validity of the methane model used in this study. In particular it seems that that authors changed biomass burning emissions with latitude by large amounts so as to improve agreement of the methane emissions parameterization with measurements. Offhand, this seems difficult to justify. A reasonable agreement with present day methane distributions is necessary to justify using the methane emission model during PI and LGM simulations.

In addition, the latitudinal boundary assumed in the methane parameterization between boreal emissions and tropical wetland emissions may very well have changed during the LGM. This is not accounted for in the parameterization nor discussed in the paper. The impact of this is not clear.

1) Wetland methane emissions are based on the empirical formula of Gedney et al. (2004). While this formulation might be appropriate for the 0th order calculation done here, there have been numerous rather sophisticated wetland methane models developed since 2004. These are not mentioned in the paper. Some background on these models would be helpful, and in particular a discussion on the simplifying assumptions used in the Gedney et al formulation; in particular, what processes does the parameter KCH4 encompass, and what evidence (or justification) is there for setting this parameter constant over wide latitude bands?

More detail on the methodology for fitting this parameter to the data would be appropriate. It is fitted to present-day methane concentrations, but the methodology for this is not mentioned.

Setting this parameter to one value poleward and equatorward of 40 degrees is a bit strange. Very little justification is given here. Were two parameters optimized when fitting to measured methane concentrations (the value of KCH4 poleward and equatorward of 40)? Moreover, if the argument is that the parameter for boreal wetlands should be different from more tropical wetlands then certainly the border between these regions changes with climate. How would changing the latitude for this change-over impact the calculation of preindustrial methane flux? Some discussion is needed here.

2) The mean methane lifetime should be given in each of the simulations. Other details of the model simulations also seem to be missing. I may have missed it, but for how long was the methane simulated for each of the periods? Are the simulations run to steady-state? In the present and preindustrial periods what were the years of the simulation? As the present day methane budget is constantly changing, what years do the present day emissions represent (Table 1)? For what year is the present day biomass emission estimate valid?

3) It would be interesting to know if the methane loss by dry-deposition changes appreciably in the LGM simulation.

4) Scaling biomass emissions to improve the fit of methane (especially by large scaling factors) seems rather questionable (p 3200, l 12-14). In fact with the GFED fire emissions derived in part from satellite data and other measurements the biomass burning emissions are likely known better than wetland methane emissions. It is my guess that this scaling covers up for errors in the methane emission parameterization. The authors absolutely need to justify this assumption for the paper to be valid. How does the spatial distribution of the scaled biomass burning emissions compare with other estimates (e.g., from GFED)?

5) The authors used historical biomass burning emissions from Valdes et al. (2005). Since the time of the Valdes paper there have been a number of historical estimates of biomass burning (e.g., the work of Marlon et al.) The authors need to reconcile their emission estimates with those from these other estimates for the PI period.

6) Page 3202, line 18. The Montzka et al (2011) paper supports rather small interannual variations in OH. To my knowledge it does not comment on preindustrial OH, when atmospheric chemical composition was rather different than present-day (p 3202, line

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17).

7) LGM OH. The text (lines 16-24) makes it rather confusing about what the authors actually assumed for LGM OH. After presenting sensitivity studies the authors assume a 26% increase in LGM OH (line 16). It is somewhat of a mystery where this number comes from. The authors need to justify. Then in the last sentence they assume no change in OH between PI and LGM (line 24). Please clarify. A table to the simulations and sensitivities might be appropriate (see comment 13).

8) Line 11-13, page 3204. The PD emission estimates and meridional distribution need to be compared here again with other published estimates. How do they fit in with the uncertainties derived in Prather et al. (2012)?

9) Figures 4 and 5 could be easily combined and might be instructive to do so (albeit possibly using different scales for tropical and extratropical wetlands).

10) An additional figure of the wetland emissions during the three periods would be instructive. In addition, during the present day period, it would allow a comparison to other derived wetland emission estimates.

11) The authors statement: “there is no evidence suggesting any significant changes in natural wetlands” (page, 3206, line 25-26). Surely there have been landuse changes since the preindustrial that may altered wetland area. While deriving these changes may be difficult, this is surely a source of uncertainty.

12) In the discussion section (or somewhere) it should be mentioned that this parameterization of wetland emissions would seem to be on the low side in terms of the emission sensitivity to wetland area (the present day wetland emissions are on the low side while the wetland area is on the high side).

13) A table summarizing results would be nice. It might include: Measured methane (with error bars), wetland emissions, simulated methane, and OH sensitivity experiments.

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