Atmos. Chem. Phys. Discuss., 14, C892–C895, 2014
www.atmos-chem-phys-discuss.net/14/C892/2014/

Atmospheric Chemistry and Physics Discussions

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 #1

Received and published: 29 March 2014

General Comments

This is a very interesting paper about the evolution of atmospheric methane from the LGM to the PD, and I enjoyed reading it and thinking about the conclusions. The authors should carefully proof-read the paper since there are some confusing sentences and errors. Also, please make sure to use consistent units throughout the paper. For example Tg/yr everywhere and not Tg in some places.

It is interesting that the simulations presented in this paper did such a good job matching the atmosphere and ice core records. On the other hand, this good agreement

relies on the fact that OH didn't change much over the simulated period. After reading this paper, I'm unconvinced how well we know the long-term variability of OH. The Montzka et al study only applies to a short period compared to the period considered by this study, and even small differences can add up to a lot of methane over time. Perhaps it would be useful to look at OH uncertainty in more detail. I agree that sources are likely to have been more important than the sink in driving the millennial growth of CH4, but I think it would help to explore in more detail how robust this conclusion given uncertainty in the OH simulations.

I don't think the latitudinal distribution of wetlands emission presented for the PD in the paper is in agreement with studies that show most emissions occurring in the tropics and high latitudes, with less in the Northern Mid-latitudes. It would be helpful to see how zonally averaged emissions compare with other studies. It would also be nice to have a few details about the climate simulated for the LGM - where was there more/less precip? What were the tropical temperatures compared to current day.

Abstract

L5-7 - "each of these periods" refers to LGM-PI and PI-Modern? Below it seems like there is a 3rd period, but this sentence only has two periods mentioned. Also, what period does the PD cover?

p3195, L6 - GWP's are usually expressed in mass units. Also, a time-horizon is usually specified. The authors should check the latest estimate.

P3195, L24-25 - Unfortunately the methyl chloroform obs only provide a relatively brief window (late 1990's to mid 2000's) during which we can say OH was stable. Although this is a useful piece of information, it is probably not enough to justify constant OH over long periods.

P3198, L1 - which one is the original publication? CARAIB or Kaplan?

P3198, L2-7 - How was this optimization performed? Was it an inversion using atmo-

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spheric data? More details on this would be helpful. What transport model and winds were used and what was assumed for the rest of the methane budget?

P3199, L1-4 - the loss is due to uptake by microbes in oxic soils, not just dry deposition.

P3199, L20 - Should you say “The last 50 yrs” ?

P3200, L11-15 - What is the rationale for scaling the burning emissions? Why not one of the other sources? Is there isotope data that constrain this choice?

P3203, L12 - The wording in this sentence needs fixing - I don’t understand the part about the 26% increment.

P3204, L10-12 - It does not seem reasonable that PD simulation has all wetland emissions occurring in the tropics and Northern mid-lats. What about emissions north of 60N? Atmospheric inversions estimate that there should be about 10-20 Tg coming from High Northern Lats. Also, Tropical emissions are the highest ($\sim$80-90 Tg). And it also doesn’t seem reasonable to not have any emissions from the S. Temperate zones, while the largest share of emissions come from the N. Temperate. I think it would be useful for the authors to show the latitudinal distribution of wetland emissions for the PD and compare to other published estimates.

P3205, L8-10 - Is the fact the inter-hemispheric gradient looks like PD observed due to the tuning of the biomass burning emissions?

P3205, L25-P3206, L5 - Figure 7 shows a very odd seasonal cycle at high latitudes. The data should not reach a minimum in the seasonal cycle in October. I suspect the problem is that the plots are not labelled correctly.

P3208, L3-5 - It’s incorrect to say that the Sunda shelf regions yields a specific concentration. Even near strong sources, the concentration is still a combination of sources, loss and transport.

P3208, L13-14 - So the resulting comparisons with the ice cores imply that the LGM

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wetland emissions are higher than for the PI? PI: 99-88 Tg/yr vs LGM: 96-120 Tg/yr? Also, I don't understand the last sentence of this paragraph about the entire uncertainty being due to wetland emissions.

P3208, L 21-22 - There is a contribution to the growth in atmospheric CH4 coming from wetland increases between the PI and PD though. I'm not sure I would say the growth is easily explained.

P3209, L16-18- Given the uncertainties, I'm not sure I see how this study supports OH during the PI being similar to PD. Maybe more explanation would help me to see why trends in OH can be ruled out. The Montzka results suggest variability of OH is about 2%, but this is still a lot of CH4 (for the current budget). So results should be pretty sensitive even to small OH differences.

P3210, L10 - I think you mean LGM to PI because the PI to PD is dominated by anthropogenic emissions, right?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3193, 2014.