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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 #2

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Summary

In this work the authors derive a new wetland and wetland CH4 emission scheme. Using climate simulations for the present day (PD), pre-industrial (PI) and last glacial maximum (LGM) they then use this to explore how well changes in atmospheric CH4 can be explained by changes in natural and anthropogenic sources depending on the time period.

In general this paper is within the scope of the journal, includes to some extent novel methods and shows new and interesting results. However, in several places, the

manuscript is lacking in terms methodological detail, and explanations of how various results have been achieved are not clear. Occasionally conclusions made do not follow very well from the results presented. The authors need to rewrite several parts of the manuscript (see below) in order to properly illustrate their methods and to place their work in context of previous findings. Overall I recommend major revisions are required.

General comments

I have 5 major concerns that should be addressed:

1. Given the fundamental influence of the wetland area on many of the results in this paper, the lack of detail concerning the simulation of wetland area is a major weakness. For the CARAIB simulations there are many unanswered questions: What is CARAIB forced with? What resolution was it run? Which soil property data is used? Does the CARAIB model include some parameterisation of organic (peat) soils? How many soil moisture stores are there? Is topography in the hydrology module accounted for in CARAIB? How long was the spinup for bringing the soil carbon into equilibrium? Was the vegetation potential natural vegetation in each time period, and is this as simulated by CARAIB or do you used observed for the modern? What distribution is used for the LGM? Did you use different atmospheric CO2 values for the 3 time periods to force the CARAIB model?

2. You use lower resolution topography data to calculate wetland area for LGM (30minute) compared to the PD and PI (10min). What is the dataset that you used for the LGM and PI/PD? How was it constructed for the LGM? Are you convinced that using different resolutions here will not impact the results for wetland area? I ask this because you used the same slope condition (2 degrees) at two different resolutions. Might this increase the wetland area at the LGM? Ideally you would re-run the pre-industrial wetland calculation at the same resolution as the LGM run to make the comparison cleaner. Is this a possibility?

3. There is no detailed description of how figure 1 was produced. Were these using different climates? What magnitude of forcings have been implemented? Are the changes imposed realistic? How were they estimated? A table showing by how much these forcings were changed and quantifying the separate and net affects on OH is needed here.

4. Throughout the abstract and conclusions you refer to the difference in wetland emissions between the present day (PD) and LGM, however the number that is reported in previous work, is the difference between the pre-industrial (PI) and LGM. Your PI-LGM change LGM/PI = 62% whereas the number you quote is the LGM/PD =55%. Your simulated PI/LGM value is actually not that different from much previous work. For example though Valdes et al 2005 simulated 73%, and Kaplan et al 2002 found roughly 100%, more recent work by Weber et al calculated a range of 58 to 64%. I think you need to rewrite various parts of the manuscript focussing on the PI/LGM change not the PD/LGM change and to more clearly discuss your results in light of previous work.

5. You use source estimates for pre-industrial anthropogenic methane sources based on Ruddiman et al 2001, and your non-wetland sources are reduced by 33% between the PI and LGM. Thus your non-wetland sources make a reasonable contribution to your simulated LGM/PI concentration reduction. The non-wetland sources are reduced by 22Tg/yr, about half as much as the wetland reduction of 43Tg/yr. You don't highlight this anywhere. Also recent work by Singarayer et al 2011, using a similar modelling framework to yours, argues against the early anthropogenic CH4 source postulated by Ruddiman 2001, 2003. Some discussion of this would be beneficial.

Specific comments

Page 3197, line 14: You use different model parameter value for normalized soil water content North and South of 30N. One could argue that this would need to change for the different climates. Have you thought about this at all? How important was this division at 30N for the results for the PD?

Page 3197, line 22: In the CH4 model what soil temperature depth do you use? Where are these soil temperatures from?

Page 3199, line 12-15: Are surface geopotential and snow depth prescribed? What is the global temperature change between your 3 different climate states?

Page 3199: line 23-25: Here do you mean that the 3D atmospheric CH4 field is initialised from CMIP5 MIROC-ESM simulation? If so why this model, and can you state this explicitly in the text?

Page 3203, line 1: Please include references for these % changes.

Page 3203, line 10: Please remove this 'personal communication', since T. Laepple is a co-author!

Page 3206, line 6: Remove 'in' after 'Further'

Page 3206, line 26 and Page 3204, line 20: You refer to the control of soil temperatures on CH4 emissions, but do not discuss soil carbon changes. What role does soil carbon have here?

Page 3207, line 21-23: I disagree, the non-wetland PI source is 81Tg/yr!

Page 3208: line 8: Can you update the inter-polar gradient values you quote using the newer values from Baumgartner et al 2012? The Dallenbach et al 2000 data has been shown by Baumgartner et al., to overestimate the inter-polar gradient for some time periods.

Page 3209: line 20: Please clarify what these numbers are here otherwise the reader might think they are model results rather than model-inferred values.

Page 3210: lines 15-19: As mentioned above, rewrite this paragraph based on the LGM to PI change (not LGM to PD) as was done in the 3 studies you are discussing here (Kaplan et al, Valdes et al & Weber et al)

Page 3210: Line 3-5: If you discuss the inter-polar gradient here, you should also include the values derived from ice-core from Baumgartner et al 2012.

Page 3211, line 3: I do not follow why the results of Levine et al 2011 are in contrast to your assumptions? You have assumed that OH does not change, as was found in the model of Levine et al.

Page 3211, line 2: Can you describe in more detail exactly how the chemistry scheme is coupled to the climate and to what extent the chemistry is interactive or not?

Tables: Table 1: I don't really see the justification for increasing termite CH4 emissions at the LGM? Can you discuss why you choose to do this?

I think you need extra tables summarising your results. Could you include the globally (or Greenland/Antarctica) averaged CH4 mixing ratio in the different time periods?

Figures: Figure 1: As I mentioned earlier, a proper explanation of the methods used to produce the numbers plotted in this figure is required.

Figure 9: I can't see any areas where wetlands fall outside of the modern coastline i.e due to the $\sim$120m drop in sea-level. Is this correct? Could you change the coastline in this plot to the LGM coastline?

I also would encourage you to include a figure which reproduces one of the analyses included in the WETCHIMP project paper. For example, could you make an equivalent of figure 6 from Melton et al 2013 using your present day simulation? This would allow your work to be more easily compared with existing models.

References:

M. Baumgartner et al (2012), High-resolution interpolar difference of atmospheric methane around the Last Glacial Maximum, Biogeosciences, 9, 3961-3977, doi: :10.5194/bg-9-3961-2012.

J.R. Melton et al (2013), Present state of global wetland extent and wetland methane

modelling: conclusions from a model inter-comparison project (WETCHIMP), Biogeosciences, 10, 753-788, doi: 10.5195/bg-10-753-2013.

J.S. Singarayer et al (2011), Late Holocene methane rise caused by orbitally controlled increase in tropical sources, Nature, 470, 82-86.

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