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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Response of methane emissions from wetlands to the Last Glacial Maximum and an idealized Dansgaard-Oeschger climate event: insights from two models of different complexity" *by* B. Ringeval et al.

Anonymous Referee #2

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Manuscript: Response of methane emissions from wetlands to the Last Glacial Maximum and an idealized Dansgaard-Oeschger climate event: insights from two models of different complexity

Authors: B. Ringeval et al.

Dear Editor,

General comments:

This paper uses two wetland CH_4models of differing complexity to look at wetland methane emissions during the LGM and an idealized Dansgaard-Oeschger (DO) event. The differences in model response are investigated in terms of the model parameterizations and comparisons are made between the model results and previous bottom-up and top-down estimates for the LGM and some DO events. Much of the focus of the paper is on the influence of model parameterizations between SDGVM and ORCHIDEE-WET.

I have some concerns about this manuscript that I would like to see addressed.

First, I have some difficulty in understanding the approach given to some variables in Equation 2. In equation 2, a T_{ref} value is derived for the ORCHIDEE model run (V_0). The value is defined for that model version as the mean surface temperature computed by ORCHIDEE-WET when forced with the 1960-1991 CRU climate. In other model versions (V_1,V_2, and opt), T_{ref} is set to 30 degrees C everywhere. If this T_{ref} is intended to represent some sort of local adaptation of the Q_{10} formulation for local conditions and climate, does this make sense to parameterize it for modern CRU climate for a study where all runs are performed for paleoclimate conditions? To my mind, the value of T_{ref} should be different during the LGM (assuming it represents an adaption for conditions) than modern, thus parameterizing it for modern conditions does not make sense. Perhaps the authors just need note that the approach is intended for modern conditions and just applied as-is to the paleo?

Second, the same equation has a parameter, α_0, which represents both the fraction of labile C pool that can be used as a methogenesis substrate and a tuning parameter for T_{ref} that is optimized against 3 field sites. For each model version, α_0 is retuned alongside each change in the other parameters. I find this to confuse the influence of changes in the other parameters (which are the only ones that are discussed). For example, changing between model version ORCHIDEE-WET V_1 and V_2, the important changes for the sensitivity test is to set the soil water to the maximum, to remove any water stress on the vegetation growing in wetlands. The purpose of this test is

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to understand if a model shortcoming (the lack of PFTs that grow only in wetlands, thereby only being influenced by wetland hydrology – not a mix of upland and wetland) has a strong influence on the model result. However, the values of α_0 change from 5.5, 8.5, 20.1 (V_1) to 6.9, 5.4, 24.5 (V_2) for the different latitudinal bands; making it difficult to understand what is the primary influence – the lack of water stress on plants or the changes in α_0?

Third, the analysis of idealized DO event presents some problems. The authors use a two-box model to determine the relative interpolar CH_4concentration difference (termed rIPD). While I understand that the rIPD approach was taken from Baumgartner et al. (2012), I do not feel it is a realistic approach. Splitting the atmosphere into two seperate boxes at the equator ignores the most basic atmospheric circulation patterns (a three-box model would at least allow a rough approximation of Hadley cells). The authors do alude to this by demonstrating how sensitive the rIPD value is to assumptions about the limits of the two-boxes (see p 3117 line 24). Given the uncertainties with this calculation, I would prefer to see it left out.

Fourth, the grid cells of the models are assumedly the same as the FAMOUS climate data (5 degrees x 7.5 degrees). At such a coarse resolution, I have some concern about how realistic the treatment is for the land exposed on the continental shelves due to low sea level. The approach of taking the same topography of the nearest land cell for newly exposed land would be possibly okay if the cells were small, but at this large resolution, I wonder if these (possibly) flat continental shelf regions are given far rougher terrain than in reality, thus biasing the model results low at the LGM due to less area suitable for wetlands. I would like to see some demonstration that the very large grid cells don't create artifacts resulting in lower than reasonable area suitable for wetlands.

The manuscript also requires a thorough proof-reading as there are numerous typographical and grammatical errors.

Overall assessment:

This paper is well within the scope of Climate of the Past. I think readers will find it of interest. If my concerns can be addressed, I believe the paper is publishable, albeit with revisions. Given my concerns stated above, I obviously have some reservations about this paper, but those aside, I do think it is an interesting paper and well worth seeing through to publication.

Detailed review:

p. 3097 line 3-6: Processed-based wetland CH_4models have been around a fair while. While it is subjective, over a decade does not seem 'recent' (e.g. Walter et al. (2001)). However I do agree that a reasonable approximation of LGM climate is only very recent (if indeed we are there yet).

p.3097 l 14: Reading Hopcroft et al. (2011), I don't get a sense of how well the LGM climate of FAMOUS has been checked against terrestrial temperature proxies. A recent example for checking FAMOUS against would be Bartlein et al. (2010) although this also suffers from lack of proxies in the tropics where perhaps they are needed most for wetland studies. Most of the discussion relates either the Atlantic meridional overturning circultation (AMOC) or global mean temperature. AMOC is essentially irrelevant to wetland simulations, its influence on the climate is important but we are given no information about the spatial distribution of the climate change. The global change of 4.1 degees C from preindustrial gives an idea. Plotting FAMOUS AMOC and Greenland temperature in Fig A1 do not give relevant information about the conditions the wetland regions are experiencing. While I do understand that the actual forcing data is a secondary concern in this paper, the main being the differences between the wetland models, I think it is relevant to demonstrate how the climate was spatially and

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how it differs from present. Fig A1 should be replaced with a figure showing a map of anomalies from present day for temp and precip. Fig A3 is somewhat useful for the climate changes, but more is needed.

p. 3101 l 10: How was -3 cm chosen?

l 15: Does the model include canopy drip?

p. 3103 l 15: 2m air or soil temperature?

l16: Why above 5 degree C? Is this the monthly, annual, mean, max value? Please specify.

p 3105: How is the soil texture of the continental shelves handled? I repeat my concern as stated in the general comments that this approach of using the neighbouring cell is not valid for such large grid cells (assuming the soil texture information was treated the same as topography).

p. 3106 l 21: So how does the bathymetric information differ? I would like to see a discussion of how this would influence the results as I am not convinced the present approach is appropriate.

p. 3107 line 12: I do not understand why α_0 would require a new optimization for each change in T_{ref} or Q_{10}. If it is indeed required (please add in some further justification), why is it then required between V_1 and V_2? The values of T_{ref} or Q_{10} remain the same between these model versions. As I note in General Comments, this just confuses the impact of the main change in V_2. On page 3108 l. 7, apparently this was not a re-optimizaton but a correction. What is the difference and why is it needed?

p. 3108 l. 10-12: I would like to see a plot showing how the influence of constant soil field capacity conditions is more through effect on substrate than methanogensis or transport. This is interesting in its own right.

p. 3108 l 20: I wouldn't say an 'over-estimation', these values are very poorly constrained and thus very much open to debate. Look at how much trouble we have with modern estimates (Denman et al. , 2007; Melton et al. , 2012).

p. 3109 l 9: I think this section deserves more comment on the influence of using just one modelled climate (with no information given on how well it compares to proxies).

p. 3110 second para: How much land gets added at such large grid cell sizes? Do you allow for fractions of grid cells to be added due to newly exposed land at LGM? I would suggest that the discussion of how much exposed continental shelves contribute to wetland emissions should be tempered by how realistic the treatment was of the exposed shelves within the model.

p. 3111 line 19: No discussion is made of the change to vegetation in SDGVM, which were dynamic. Could this have much influence on the results compared to the prescribed PFT distributions of ORCHIDEE-WET. Please discuss this.

p. 3112 l 11-15: Confusing, please reword.

p. 3113 l 18-26: Confusing, please reword.

p. 3114 l 16: Please do not use this form '(respectively ...)'. It is consistently confusing, unnecessary, and much simpler to just write the sentences like: 'the sourthern tropical band is characterized by an increase in CH_4emissions, while the northern band sees a decrease'.

p. 3116 l 25: The match at the top end of the range from Weber et al. (2010) is pretty unsurprising as ORCHIDEE-WET was optimized to be within the range.

p. 3117: My objection to the two box approach is in the general comments. I think this part of the discussion is too uncertain to add to the paper and could be removed.

p. 3118 l 22: I think the authors place a very large emphasis on the Q_{10} formulation. Much of the discussion focuses on this one parameter. I think that it is likely worthwhile to discuss other parts of the model parameterization rather than putting such weight

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on this one parameter.

p.3119 l5-10: How did the influence of CO_2versus soil freezing get quantified? Are those results presented?

p. 3119 l 24-25: But inundation datasets have problems of their own, e.g. discussion in Melton et al. (2012).

p. 3120 l 14: It is possible that the value of 15 Tg yr^{-1} is reasonable. For the Younger Dryas termination (if that can be assumed similar to a generic DO event), Melton et al. (2012b) estimated that tropical wetlands would increase a maximum of 14 Tg for a global increase of 64 Tg yr^{-1}, with a very minor increase in boreal wetlands. My point being that the contribution of wetlands to DO events is still pretty open for debate.

–Did SDGVM use it's N-cycle components? If so, how would that influence the results compared to ORCHIDEE-WET (which I believe has no N-cycle)?

p. 3122 l. 13: Yes, I agree that flood plain processes are needed. There is a recent paper that models groundwater contribution to wetlands in the Amazon that would be worth refering to here (Miguez-Macho and Fan , 2012).

Table 2: Confusing caption, please reword

Fig 1: The multiple WTD arrows for SDGVM make it look like there are multiple WTPs, not a variable one, perhaps revise to make it less confusing.

Fig 2: What is mean that each PI ORCHIDEE-WET lat distribution has been normalized to match SDGVM PI global emissions? This point might have been lost in the text but I think it is interesting that changes from ORCHIDEE V_0 to V_2 are of opposite direction for the boreal region than the tropics. Has this been noted and discussed in the text?

Fig 3: Please add plots of the climate changes between LGM and PI so there is something to reference against.

Fig 8: What does 'SDGVM -non divinding by f_wtp' mean? Please move the legend out of the top left box and into the open space bottom right. Please darken the yellow to make it easier to read.

Please give a thorough check of the MS for typos, grammatical errors, and general flow. I do not list them but they are extensive.

References cited:

Bartlein, P. J., Harrison, S. P., Brewer, S., Connor, S., Davis, B. a. S., Gajewski, K., Guiot, J., et al. (2010). Pollen-based continental climate reconstructions at 6 and 21 ka: a global synthesis. Climate Dynamics, 37(3-4), 775–802. doi:10.1007/s00382-010-0904-1 http://www.ncdc.noaa.gov/paleo/pubs/bartlein2010/bartlein2010.html

Baumgartner, M., Schilt, A., Eicher, O., Schmitt, J., Schwander, J., Spahni, R., Fischer, H., and Stocker, T. F.: High-resolution interpolar difference of atmospheric methane around the Last Glacial Maximum, Biogeosciences Discuss., 9, 5471-5508, doi:10.5194/bgd-9-5471-2012, 2012.

Denman, K. L., Brasseur, G. P., Chidthaisong, A., Ciais, P., Cox, P. M., Dickinson, R. E., Hauglustaine, D. A., et al. (2007). Couplings between changes in the climate system and biogeochemistry. In S. Solomon, D. Qin, M. R. Manning, Z. Chen, M. Marquis, K. B. Averyt, M. Tignor, et al. (Eds.), Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change (pp. 1–90). Cambridge UK and New York, NY, USA: Cambridge University Press.

Hopcroft, P. O., Valdes, P. J., & Beerling, D. J. (2011). Simulating idealized Dansgaard-Oeschger events and their potential impacts on the global methane cycle. Quaternary Science Reviews, 1–11. doi:10.1016/j.quascirev.2011.08.012

Melton, J. R., Schaefer, H., & Whiticar, M. J. (2012). Enrichment in 13C of atmospheric CH4 during the Younger Dryas termination. Climate of the Past, 8(4), 1177–1197. doi:10.5194/cp-8-1177-2012

Melton, J. R., Wania, R., Hodson, E. L., Poulter, B., Ringeval, B., Spahni, R., Bohn, T., et al. (2012). Present state of global wetland extent and wetland methane modelling: conclusions from a model intercomparison project (WETCHIMP). Biogeosciences Discussions (Vol. 9,

pp. 11577–11654). doi:10.5194/bgd-9-11577-2012
Miguez-Macho, G., & Fan, Y. (2012). The role of groundwater in the Amazon water cycle: 1. Influence on seasonal streamflow, flooding and wetlands. Journal of Geophysical Research, 117(D15), 1–30. doi:10.1029/2012JD017539
Walter, B. P., Heimann, M., & Matthews, E. (2001). Modeling modern methane emissions from natural wetlands. 1: Model description and results. Journal of Geophysical Research, 106(D24), 34189–34206.
Weber, S. L., Drury, A. J., Toonen, W. H. J., & van Weele, M. (2010). Wetland methane emissions during the Last Glacial Maximum estimated from PMIP2 simulations: Climate, vegetation, and geographic controls. Journal of Geophysical Research, 115(D6), 1–13. doi:10.1029/2009JD012110