

Interactive comment on “

Methane variations on orbital timescales: a transient modeling experiment” by T. Y. M. Konijnendijk et al.

Anonymous Referee #1

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Referee report on:

Methane variations on orbital timescales: a transient modeling experiment

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General comments:

The paper presents results from a modelling experiment looking at global wetland

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methane emissions over the last 650 000 years. This paper is one of the first attempts to look at wetland methane emissions, generally considered the largest single natural source of methane to the atmosphere, over such a long time period. The results of the model experiments are used to interpret the ice core methane concentration record from Antarctica with particular emphasis on the lag between atmospheric methane concentration and orbital forcing.

In general, I support the eventual publication of this work, however I do have several significant questions and concerns that need to be addressed before this work is published.

One of the most major concerns I have for this work is based upon two assumptions that I question the validity of: 1) the assumption that the impact of changes in sea level are not significant, and 2) the assumption that orography does not play a role at the present spatial resolution. Several assertions based on the model results rest upon these two assumptions being reasonable. I do not believe there is enough evidence presented to support the assumptions. I also question the treatment of model results for comparison with ice core measurements and then estimation of atmospheric lifetime.

I think that this paper is an interesting attempt at a difficult problem. Simulating wetlands properly over much shorter timescales remains a formidable problem, going back 650,000 yrs is thus even more difficult. I think that many of the assumptions made to simplify the calculations are necessary but should be looked at more critically by the authors. Most importantly more comments highlighting the shortcomings of the approach need to be made in the text. I can not comment on the spectral analyses section as I do not have experience with that technique.

Overall the level of writing in the paper is only adequate. There are several areas of the text that are vague or confusing. These should be fixed up prior to publication as well.

Specific comments:

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L26: What about flux density? I did not see very much mention of changes in flux density as opposed to simply changes in wetland areal extent.

L26: 'influencing CH₄ production and release' - I assume you mean release to atmosphere? Please be more specific.

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L7: Put an approximate date beside Early Holocene

L11: 'These oscillations...'- Be specific. Oscillations of what?

L13: 'with no additional lag'- Be specific, lag between what and what?

L14: Explain target curve.

L25: Specify that this is for the modern period.

P51. L6: Do you mean fast computation time? I don't think turnaround time is the correct term for what you are talking about.

L15: More importantly, what is the land surface resolution? Considering this a paper on wetlands this is a very important piece of information and is lacking totally. Readers should not have to chase this down in other CLIMBER-2 publications.

L23: Does this mean that you scaled the ice volumes to ICE-5G then just moved that through time?

L25: Considering sea-level changes to be unimportant is a large assumption. I question that it is a valid assumption, so I would like to see some rudimentary calculations to demonstrate that this assumption is valid. During the LGM, sea level was depressed by 120 – 135 m below present day sea level [Clark and Mix, 2002; Jansen et al., 2007] with the water displaced onto the continents as massive Northern Hemisphere ice sheets. On the now exposed continental shelves, Kaplan et al. 2006 has simulated, using the

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BIOME4-TG global vegetation model driven by climate model output, large wetland expanses during the LGM in Beringia. However, even if these areas did contain expansive wetland complexes, it is unlikely they would have been strongly CH₄ producing given the very low productivity of large boreal wetland expanses at more moderate latitudes, such as the present-day Hudson Bay lowlands [Roulet, 1994; Worthy et al., 2000]. So for the boreal region, I can see that ignoring the continental shelves could be valid. My main concern is that for the tropics, the situation is different and these exposed areas can not be ignored. For eg., looking again at Kaplan et al., (2006), they found very large and highly-productive wetland complexes on the exposed Sunda and Gulf of Carpentaria shelves. A simple test could be to quantify the increase in land area at the LGM compared to the present modern configuration. Then, using the wetland flux density for the neighbouring cells, determine an estimate of how much increase methane emission can come from these exposed continental shelves. If these rudimentary calculations show that ignoring sea-level changes significantly affects the main results then the conclusions should be rewritten to reflect the restrictions placed by this assumption both in the conclusions and in the abstract.

P52. L6: 'the simulated climate is only determined'- do you mean forced?

L18-19: Has there been any attempt to compare the model simulated monsoon strength to paleo-reconstructions like cave speleotherms? If not, how can you show that the model can produce reasonable monsoon strength over the timescales in this study? I realize that it is mentioned that the model monsoon strength at mid-Holocene is in line with other GCMs, but how does it evolve through time and in comparison to other records. I think this is important since you make a lot of conclusions based upon the behaviour of the monsoon and its effects on wetland emissions.

L24: Again, what is the spatial scale?

P53: What is the orography dataset used? The Kaplan 2002 scheme requires some knowledge of the slope of the gridcell, how was this determined?

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L3: What wetlands are missed by the Kaplan (2002) scheme? Deltas, groundwater-fed etc. you should note what wetlands you are missing by using this scheme.

L7: 5% of maximum saturation seems incredibly low. Is this because the authors use the entire gridcell soil saturation level? Does the model have soil levels (again this is important to a wetland paper so the reader should not have to hunt this down through other CLIMBER2 papers)? Which soil depth are you taking the saturation level?

L14: The assumption that orography does not play a role seems weak. To assume that there is always land that can form wetlands also then does not put a limit on the amount of land that can form a wetland. This does not seem correct, as there will be slope fractions that are simply not able to support wetlands, i.e. the water just runs off too fast for anaerobic conditions to develop. Did the authors perform any checks to ensure this was a valid assumption? For eg. did they take the PIH simulation and look at the fraction of land that is wetlands and compare that to some sort of dataset such as Prigent et al. 2007 (which understandably would need to be aggregated up to their grid cell resolution). If the authors are simulating more wetland areal extent than is possible, they should restrict the maximal extent.

Again, you need to give your land model resolution.

L16-18: What values in literature? When? Modern estimates, PIH?

L26: What is a vegetation factor? For strange terminology, don't make the reader hunt it down in a VECODE publication.

L27: Again. What soil depth? How many soil layers are there, 1?

P54. L1-12: This is an overly in-depth discussion of Q10. I don't see the purpose of this, since the model has such a simple soil model and huge spatial resolution. The Q10 chosen is really pretty unimportant given that you can just tune TRENCH emissions (via k) anyway. This whole discussion can be shortened significantly as it gives the appearance of a more mechanistic model which I don't feel is justified. Only

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one sentence on the chosen Q10 is required.

L18-20: I don't agree with your assumptions about trees vs. grasses. Does the land model resolve rooting depth? If not, how is the depth of tree vs. grass roots important for the wetland emissions? Can you provide some references that say that trees provide better organic material for methanogenic substrate than grasses?

L23: What different weighting did you test?

P55. What are the latitudinal limits of the 'boreal' and 'tropics' regions?

L8: global annual mean what? areal extent?

L12: Yes, obviously it sums to 151 Tg as you tuned it to be that value. You should reword it so that is obvious that it is a tuned value. The important part of the sentence is how your model then partitions the boreal vs. tropical at this level of emissions and tuning.

L16: This statement would be easier to judge if I knew what were the limits of boreal and tropical in this paper...Make sure the other papers cited are all using the same latitudinal definitions of these regions.

L23: Why is the decrease in wetland areal extent so much more pronounced in the summer than the winter?

P56.

L10: Can you quantify the difference between the temperature-effect and the direct loss of wetland area by ice cover?

L21: average temperature of soil or air?

P57:

L6: It is perhaps safer to say 'As wetlands are assumed to be the largest natural source'

L14: I don't understand how you came up with the atmospheric lifetime calculations?

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Did you use your wetland emissions, estimated other sources and then looked at atmospheric concentrations? You will need to expand this to make how you calculated lifetime much more explicit. As it is now I can not judge how you came at your numbers quoted.

As far as I can tell, you assume a constant atmospheric lifetime to compare the model CH₄ output to the ice core record, and then assume that it can change to get estimates of atmospheric lifetime changes. These conflict with each other and nullify any conclusions drawn with either assumption. How is this treatment justified?

L19: Why were the lags the longest for the boreal regions? Is this simply a reflection of your prescribed ice extent?

P59. L15: I don't believe that your model can really comment with much certainty on temperatures influence on the boreal wetlands due your model lacking freezing soils. I would argue that you are just seeing the effects of your ice sheets as they are prescribed. Can you demonstrate that this is not case?

P60.

L21: The 8.2 yrs lifetime is rather obvious as the model was essentially tuned to this value. The way it is presently presented does not make that obvious.

L22: The Fischer (not Fisher) et al. 2008 paper only goes back to the LGM. You should make that more clear.

P61. L2: reword: monsoon precip and global emissions 'appear' to co-vary, or: in our simulations appear to covary. The language used at present is far too certain considering the limitations of the simulations.

L3: I don't believe this assertion considering that you did not account for wetland expansion onto exposed continental shelves, and your wetland finding scheme appears to not have a reality check built-in (maximal amount of land that a wetland can realistically inhabit).

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P68. Fig 1 and Fig 2: I would argue that your very large 'boreal' wetland emissions are due to your soil lacking the ability to freeze. Can you contradict that suggestion?

An average soil temperature in January in the band 55-75 degrees N of ca. 5 degrees C is unrealistic. The soils only cool close to 0 (2 deg C) in April!

Fig 2: So is the boreal definition 55-75 deg N? If not, why did you present only that most northern band? Also by choosing 55, you miss most of the Hudson Bay Lowlands.

Fig 3 caption: Can you put some explanation in the text what a vegetation factor is? Or do you mean fraction? I am confused as I have seen vegetation factor several places but here I think fraction would be correct here.

Fig 4 caption: Temperature is for the soil? What depth?

Fig 5: Is the TRENCH output also low-pass filtered?

Fig 7: This figure appears misleading. Why do the coloured areas include ocean pixels? You do not change the ocean level during the simulations and only simulate wetlands. This figure needs to be changed to be more in-line with the actual areas simulated. Indicate a few latitudes on the figure.

Typographical:

P48. L14-15: Be more specific. This is a new paragraph so you must be more specific about what this paragraph will be talking about. It should be something like: 'Simulated variations in methane emissions' etc. This problem comes up repeatedly in the MS. L15: Be specific. Say: 'The simulated lags between X and Y...'. Using 'with respect to orbital forcing' is only specifying one part of the comparison. L21-24: These first two sentences are very awkward, please reword. P50. L7: period after glacial termination L9: Need a 'The' or 'Some' before 'questions'. Also this whole sentence is awkward. Reword. L18: Use 'Section 3' L20: Put the (I/A) definition on line 11. P51. L8-9: Awkward sentence, reword. L26: 'falling dry'- strange wording. Replace with 'The increased capacity of newly-exposed continental shelves for wetlands...' or something

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similar. P52. L10: change relevant to necessary P55. L6: specify that each of the numbers (5.9×10^6 and 2.1×10^6) are for which months/time of year. P56. L21: change 'over' to 'of' P60. L11: Unclear: 'For both temperature and vegetation lags tend ...', lags in what?

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