

Reply to comments from Anonymous Referee #1 on “Methane variations on orbital timescales: a transient modeling experiment” by T. Y. M. Konijnendijk et al.

We thank both reviewers for their extensive and constructive comments. The comments of Referee #1 are addressed here, with our replies printed in bold.

The main issues pointed out by the reviewers involve the low resolution of the climate model and the crude parameterizations for methane production and release. We address these issues in our answer, but also note here that many improvements will have to be left for future research. This is the first attempt to make a simulation of CH₄ over such long time scales. We demonstrate that doing so is an achievable goal: the reconstructed emissions show that our modeling approach is capable of reproducing the ice-core methane record adequately.

We are aware, however, of the uncertainties regarding our assumptions and results. In the new version of the article, the abstract and our conclusions have therefore been reformulated with appropriate reserve, and the discussions section has been extended.

General comments:

The paper presents results from a modeling experiment looking at global wetland 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. 3) 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.

Below we address the impact of our neglect of changes in sea level and the effects of orography, as well as the estimation of methane lifetimes. Note that, in going back 650,000 yrs, we focus on large-scale features, which is to a certain extent easier to model than the details of shorter timescale developments. We carefully went through the paper in order to improve and clarify the presentation.

Specific comments:

P48.

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.

Flux density, defined as the amount of CH₄ release per area unit per time unit, follows from changing methane production due to changes in wetland extent, temperature and vegetation cover as explained in section 2.2. We analyze changes in flux density due to these different factors in section 3.2

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

Indeed we mean release to the atmosphere. This was adapted

P49.

L7: Put an approximate date beside Early Holocene

Done (11 000 years BP)

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

We have changed 'components' into 'oscillations' in the sentence before to clarify this.

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

Done

L14: Explain target curve.

We have adapted this part of the text as follows:

'Ruddiman and Raymo (2003) devised an age model for the Vostok ice core by tuning changes in CH₄ to be synchronous with low latitude mid-July insolation. This insolation curve, which is dominated by precession, effectively lags 21 June 65°N insolation by ~1.6 Kyr'.

L25: Specify that this is for the modern period.

Done

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

Turnaround time is the term used for the time needed to complete a task, like computing one time step in the model. The word may seem odd to people unfamiliar with modeling. However, we prefer to keep this formulation because it describes the situation the most accurately and compactly (for instance: computation time depends on the speed of your computer as well).

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.

We have adapted this text to make clear that the given resolution is both the atmospheric and land surface resolution.

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

Because the inverse ice-sheet model only computes ice-sheet volumes, we used the (interpolated) areas of the NH ice-sheets from ICE-5G set on the spatial grid of CLIMBER-2. These ice sheet areas were extrapolated back to 650 kyr BP using the ice sheet volumes as computed by the 3-D ice sheet model. In this way, the total area as well as the area for each ice sheet separately includes the same temporal behavior and the same (orbital) periods as the volumes reconstructed by the 3-D ice sheet model. From the known areas and volumes the height for every grid box can now be determined. This is now explained in the manuscript. Furthermore, now we also refer to Weber and Tuenter (2011) in which more detailed information can be found.

Note that we also adapted this section following referee#2

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 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.

We agree with the referee that the effects of sea level lowering must certainly have had consequences for wetland distribution and, therefore, CH₄ production. We did perform additional model runs where we identified areas with large continental flats, and simulated the CH₄ production from wetlands in these areas at times of low sea level. The effect was a shift towards more tropical CH₄ production (in agreement with, for example, Kaplan et al., 2006) and a 7% higher overall production during the LGM than in the ‘normal’ run.

The setup of this ‘sea level effect’ run was speculative, however, due to the low resolution of CLIMBER-2. Simulating continental flats falling dry meant adding percentages of land area to some grid cells. The effect (7% difference in LGM production) was found to be quite limited, in agreement with results found in the GCM-based study by Weber et al., 2010.

Because the focus of this paper is on analyzing the behavior of different factors (climate, vegetation) and regions (NH extratropics, tropics, Indian/Asian monsoon area) over time, we decided that the benefits of including this effect did not weigh up against the extra uncertainties.

We have added an adapted version of this answer to section 2.1 of the MS.

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

Yes, we mean forced. We adapted this text.

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 behavior of the monsoon and its effects on wetland emissions.

This is a valid question that addresses the performance of the CLIMBER-2 model. The model has been extensively validated and documented in the literature. We give a number of key references in our paper. The application of the model to simulate climate variations over orbital timescales for the late Quaternary, using orbital forcing as well as time-varying ice-sheets and GHG concentrations is quite novel. A few papers have

discussed its performance on this point, showing that the simulated and reconstructed Indian monsoon agree quite well (Ziegler et al., 2010) while the same holds for tropical temperatures (Groot et al., 2011). Obviously, such a model-data comparison should be extended, but this is outside the scope of the present paper.

L24: Again, what is the spatial scale?

This text was adapted (spatial scale is sub continental).

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

The very high resolution wetland location algorithm (5' x 5' grid) used by Kaplan (2002) requires a digital elevation model to determine areas with low enough relief to sustain wetlands. The orography in CLIMBER-2 is obviously crude in the sense that 'relief' consists of sharp edged plateaus that mainly serve to influence atmospheric processes such as wind patterns.

For the purpose of simulating wetlands we chose to disregard slope as a factor: within the large grid cells of CLIMBER-2 (10° x 51°) the area represented by one cell is in reality not uniform. The chance of wetlands occurring somewhere within that grid cell's area is more determined by climatic factors than local slope. In addition, we do not know past orography (it can change locally due to the weight of the NH ice sheets). For this reason, we assume that slope can be neglected, but are aware that this is an assumption that we cannot test. It will no doubt influence the precise location of wetlands, but this does not seem too important on the large spatial scales that we are considering here.

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.

Considering the large scale of grid cells, we certainly miss all these wetlands by using this scheme. We briefly mention this in the revised MS. We have adapted the section on model setup to specify what the soil moisture variable (driver for changes in wetland area) is based on.

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?

It is quite true that 5% of maximum saturation seems hardly adequate to be described as 'wetlands'. However, one has to consider the size of grid cells here. Soil moisture is computed over 10° x 51° grid cells (the resolution of the atmosphere as well as land surface model). If such a large non-uniform area is above the threshold, smaller pieces of it will be wetlands. We have modified the text of the MS here to make this more apparent.

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.

We agree that the wetland scheme as used should not result in unrealistically high estimates of wetland cover. As it is, the highest peak of wetland cover is 20% of a grid cell land area. This occurs in Central Asia during spring snow melt.

We have performed checks on the geographical distribution of wetlands to confirm that known wetland complexes such as the Hudson Bay lowlands and the present day Beringia area, as well as the obvious tropical belt and monsoon areas, are represented. Likewise, e.g. the Tibetan Plateau does not sustain wetlands in the model. We present a summary on these results in the form of ‘boreal’ and ‘tropical’ wetland extent in section 2.3, because this allows for quick comparison with observational studies, such as Prigent et al. (2007) and Lehner and Doll (2004).

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

We have added a reference to section 2.3 here, where these values are compared to literature values for present day from, a.o. Prigent et al. (2007).

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

We have added a reference here to the explanation that is given later in the section.

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

CLIMBER-2 has 2 soil layers, this is the soil surface.

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 one sentence on the chosen Q10 is required.

The Q_{10} value determines to some extent the distribution of methane emissions over the boreal/tropical areas, so it is different from tuning the model via k, which determines the global amount of emissions. But we agree with the referee that this discussion can be reduced and this is done in the revised text.

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?

The study that we mention in the text (Rice et al., 2010) involves the efficiency of methane release via root systems. Our study does not distinguish between produced methane and methane actually released to the atmosphere (because our model does not resolve rooting depth): they are implicitly assumed to be identical.

The study of Rice et al. (2010) encouraged us to allocate a different weighting to the two vegetation variables of CLIMBER-2, but this does not produce significantly different results from a run with vegetation in a 50/50 weighting. We realize the representation of vegetation in methanogenesis in this study is very basic, but consider this the best available estimate.

Following the comments from both reviewers we have adapted the text in the new version so that the significance of the reference to Rice et al that we give becomes clearer, as well as the model's relative insensitivity to the choice.

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

This is now defined at several places

L8: global annual mean what? Areal extent?

This was adapted in the text: global annual mean wetland area

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 than partitions the boreal vs. tropical at this level of emissions and tuning.

We reworded the sentence to stress that it is a tuned value.

L16: This statement would be easier to judge if I knew what where 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.

With apologies for the oversight of the definitions issue addressed earlier. We chose the regions as in earlier studies.

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

We have added the following explanation to the article:

‘The decrease is especially strong in the boreal areas, which are active in the NH summer. The tropical zone is active year-round and is relatively unaffected or even positively affected considering wetland area during glacials.’

P56.

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

Yes, the factor analysis is done over those fractions of grid cells that can support wetlands at all times, so excluding those fractions of cells that are some times covered with ice. Therefore, the temperature factor is a pure climatic factor which we separate from the geographic control of ice cover (as is also done in Weber et al., 2010).

L21: average temperature of soil or air?

Soil temperature. This was adapted in the text

P57:

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

Done

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

Atmospheric lifetime of methane was estimated as follows: first the size of the atmospheric methane reservoir was estimated using the atmospheric concentrations measured in the EDC ice core record. Using a conversion factor (2.78 Tg/ppbv; Denman et al., 2007) the methane reservoir during e.g. the PIH (676 ppbv) amounts to ~1880 Tg of methane. Because our 100 yr time steps are much greater than the atmospheric lifetime of methane, we assume that the methane reservoir is (roughly) in steady state, meaning input=output. That means we can calculate the atmospheric lifetime of methane:

lifetime = reservoir/(modeled input+additional sources)

The size of the additional sources are derived from literature. For the PIH these sources amount to 70 Tg/yr. That means a lifetime of $\sim 1880 \text{ Tg} / (150+70)_{\text{Tg/yr}} = 8.6 \text{ yrs}$

The purpose of this calculation was to perform a check on whether reasonable lifetimes were required to convert our results of wetland methane emissions into atmospheric concentrations. If our modeled methane production would require extreme lifetime changes (e.g. ranging from 1 to 100) in order to match the EDC atmospheric concentrations, this would signal a flaw in our modeling. However, this is not the case as diagnosed lifetimes vary over a very reasonable range.

There was no influence of the calculated lifetimes on our model results or analyses: all other results such as from the factor analysis are based on the unaltered modeled methane production. The calculation of lifetime has now been extended in the article text, and its purpose is more clearly presented.

P58 L19: Why were the lags the longest for the boreal regions? Is this simply a reflection of your prescribed ice extent?

Certainly the ice extent we prescribed has the most pronounced impact on the boreal zone, as we have added to the new manuscript.

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?

We are not entirely sure what the referee means by our model 'lacking freezing soils'. The soil temperature can certainly drop below 0°C, and the amount of energy needed for this is parameterized.

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.

This section was completely reformulated.

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

Done

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.

Done

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).

Our wetland finding algorithm does have a reality check built in, as explained in sections 2.2 and 2.3

In general, the statement that changes in wetland extent contribute little to changes in emissions between glacials and interglacials is supported by other studies using GCMs, which have a much higher resolution, less crude parameterizations of methane production and release, and that do take the effect of wetland expansion onto exposed continental shelves into account. Weber et al. (2010) figure 2 shows that there is little difference in wetland cover between LGM and PIH in the output of 8 GCMs in a study that includes the effects of sea level. We have elaborated upon this in the discussion section.

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!

The first figure features large boreal wetland expanses and resulting emissions because it is a snapshot of northern hemispheric summer during the PIH. In the caption this was mentioned as ‘the month of maximum wetland extent’. We have now specified this as June.

Figure 2 seems to show suspiciously high temperatures during winter. This is because it only displays the average temperature of the cells that emit methane (the mean emission temperature) within that 55-75°N belt. Most likely during December the number of cells emitting methane is very limited and concentrated around 55°N. This explains why in April the average temperature drops when more and more high latitude grid cells are incorporated in the average as they start emitting methane. The figure caption mistakenly read ‘soil temperature’ for 55-75°N, which was revised. The definition of ‘emission temperature’ was also added to the figure caption.

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.

No it is not, see revised text. We presented this high latitude belt because it best illustrates the behavior that we find in our simulation: the peaks in wetland area are not simultaneous with peaks in temperature, making boreal wetlands less efficient as a CH₄ source than tropical wetlands.

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.

We adapted the caption as well as information in the figure to clarify that it involves the weighted vegetation factor.

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

The emission temperature is now defined in the article text as the soil temperature at the soil surface

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

The TRENCH output is not subject to high frequency variations because the model forcings only change on longer time scales. There was no need for low-pass filtering.

Fig 7: This figure appears misleading. Why do the colored 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.

The figure is presented as such because it most accurately represents the world as simulated by CLIMBER-2. e.g. Southern Africa comprises 60% of a grid cell, Northern Africa comprises 100%. This fractionation is used for computational means (such as heat capacity, evaporation etc). We plotted the basic contours of the continents to accommodate readers, but the model itself does not distinguish where in each grid cell the ocean is and where the land is. The colored areas only include grid cells (51° wide in longitude!) that contain a land fraction. The authors feel that this is the most honest representation of the simulated globe.

Indicate a few latitudes on the figure.

Done

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.

We have carefully read the text and rewritten the sentences to be more specific.

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.

Done.

L21-24: These first two sentences are very awkward, please reword.

Done. The sentences are now:

'Methane (CH₄) is a forcing factor for climate as a greenhouse gas (Wang et al. 1996). Vice versa, CH₄ concentrations are also subjected to climate changes, which cause changes in the natural CH₄ production rate'

P50. L7: period after glacial termination

Done

L9: Need a 'The' or 'Some' before 'questions'. Also this whole sentence is awkward. Reword.

Done. The sentence is now:

'The questions that we want to address are 1) whether the oscillations in CH₄ observed in ice core data originate from the tropics, the boreal zone and/or specifically from the Indian/Asian (I/A) monsoon area; 2) which climatic parameters play an important role in each of these regions; 3) how we can interpret the lags that have been found in the measured data with respect to the orbital forcing.'

L18: Use 'Section 3' **Done**

L20: Put the (I/A) definition on line 11. **Done**

P51. L8-9:

Awkward sentence, reword.

We clarified this sentence. It is now:

'It parameterizes atmospheric transports caused by synoptic-scale variations (i.e weather) in a sophisticated manner (Petoukhov et al., 2000).'

L26: 'falling dry'- strange wording. Replace with 'The increased capacity of newly-exposed continental shelves for wetlands...' or something similar.

Done

P52. L10: change relevant to necessary

Done

P55. L6: specify that each of the numbers (5.9×10^6 and 2.1×10^6) are for which months/time of year.

Prigent et al. (2007) present this value as the maximum value within a year, averaged for 1993–2000. They do not specify during which month this maximum occurs.

P56. L21: change 'over' to 'of'

Done

P60. L11: Unclear: 'For both temperature and vegetation lags tend ...', lags in what?

This was adapted in the text to:

'Compared to insolation, the lags of both temperature and vegetation effects tend to be longer in the boreal zone than in the tropics (Table 1)'

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