Reply to comments from Anonymous Referee #2 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 #2 are addressed below, with our replies printed in bold.

The authors assess emission changes in atmospheric CH4 on orbital and precessional time scales. They use a simple climate model with a very coarse box resolution. This is the first known assessment of model estimates over the last 650’000 years that can broadly reproduce CH4 emission changes in agreement with ice core reconstructions. It is a first simulation that tests simple concepts proposed so far only in paleo data studies. Its simple parameterization on one hand offers long integrations, but on the other hand also bears the risk of wrong attribution of causes that lead to CH4 emission variations. Thus for the reader the last section and conclusions seem to be highly speculative.
We have adapted the text of the abstract, last section and conclusions to better express the tentative nature of the results. The present results on the role of temperature and vegetation should be considered as a hypothesis, which is a valid alternative to the purely data-based monsoon hypothesis. Our hypothesis is based on basic and transparent modeling assumptions. Obviously, it should be tested in the future by different and more advanced models

There is little said about the uncertainty in the modeling approach and how robust the given parametrisations are. I therefore strongly suggest that for a publication the analysis includes estimates of these uncertainties and sensitivities to different factors, rather than an interpretation of individual factors to the total CH4 emissions.
While we agree that there are uncertainties in the results – understandable, given the limits of the climate model – we would digress too much if we were to stipulate them at every point. The approach taken in this study is quite novel, and could be improved upon in future research. The performance of the climate model has been found to be adequate in earlier studies, certainly compared to more comprehensive GCM-type models and, as Referee #2 already remarks, the emission results agree with the measurements from ice cores. The methane emission model is basic but it is in fact fairly standard (Gedney et al). The vegetation model coupled to Climber is also basic and we acknowledge its limitations in our conclusions on the relative role of vegetation.

Before submitting this article, many variations on our parameterizations (including but not limited to the used soil moisture threshold, weighting of F_{trees} and F_{grass}, Q_{10} value and other tuning factors) were tested. The values presented are the result of this extensive investigation, which has made it clear that either the model is not significantly sensitive to variation within a reasonable range or the value used is the best available. Results have been compared against other modeling and observational studies for the pre-industrial and modern situation.

I would envisage a publication if also the following comments are addressed.

Specific comments:
p. 50, line 6: typo, missing point "termination. Here"
Done.

p. 51, line 15: are mean annual surface temperature and precipitation resolved on the same grid resolution?

Yes, we have adapted this text to clarify that

how about land carbon fluxes?
They are not incorporated in the model

p. 51, line 24: Did you use the EDC3 timescale also for the reconstructed ice sheet? Or how was the ice sheet extent matched to the Dome C ice core data? Please clarify as this is important for the analysis of leads/lags of CH_4 to climate or ice sheets.
The reconstructed ice sheet is based on the LR04 time scale (Lisiecki and Raymo 2005), which is independently tuned to orbital forcing. We have rephrased this section to make this clearer.

p. 51, line 27: Well in general I would disagree, but for the coarse resolution of the model this might be indeed of a minor correction as other uncertainties are much larger.
p. 52, line 4: add reference for the edc3 timescale: Parrenin et al., 2007
Done

p. 53, line 7: 5% of maximum saturation is very low for the support of CH_4 emissions. Normally CH_4 gets immediately oxidized in high oxic soils. If you would assume that only a fraction of your grid cell supports wetlands, you might argue for a more reasonable soil moisture threshold.
It is quite true that 5% of maximum saturation seems hardly adequate to be described as 'wetlands'. However, we point here to the low resolution of CLIMBER-2. The area represented by one grid cell is in reality not uniform, and therefore the chance of wetlands occurring somewhere within that grid cell's area increases. We have modified the text here to better express our reasons for the way we chose each value.

p. 53, line 26: How is V defined?
The definition of V is explained in a later sub-section. We added 'defined below' to make that clear.

p. 54, line 19: By which comparison do you assume this? Is there a study showing that tree litter has a larger impact on substrate availability for methane production than grass litter per m2?
The study that we mention in the text (Rice et al., 2010) does not have to do with substrate availability but it does involve the efficiency of methane release via root systems. Our study does not distinguish between produced methane and methane actually released to the atmosphere. They are implicitly assumed to be identical.
The study of Rice et al (2010) incurred us to allocate a different weighting to the two vegetation variables of CLIMBER-2, but (as is mentioned) this does not produce significantly different results from a run with vegetation in a 50/50 weighting.
Following your comments we have adapted the text in the new version.

p. 54, line 22: Does V also consider vegetation productivity? One could think of identical vegetation cover but different productivity of an order of magnitude that would certainly affect methane emissions.
Vegetation factor V does not take vegetation productivity into account. It is not available from the CLIMBER-2 output. Our vegetation factor only includes the dynamical vegetation response (in coverage) to climate perturbations for two vegetation types (trees and grasses). We acknowledge that improvements on the dynamical vegetation response to climate would be one of ways forward in follow-up studies

p. 54, line 24: does that include soil uptake of atmospheric CH_4?

No, this is purely the estimate of the wetland source by Houweling et al (2000) and Chen and Prinn (2006). The total sink of atmospheric CH_4 (including soil uptake) is assumed to be in equilibrium with the sources on our 100 yr averaged output.

p. 55, line 28: "decrease" involves a time dependence which suggests that LGM followed PIH, please reorder it chronologically.
Rephrased.

p. 57, line 17: correct to "Fischer et al. 2008"
Done

p. 58, line 1: is the model able to simulate a shift in the ITCZ? And how is it defined at the given grid box resolution?
The ITCZ (defined as the latitude of highest vertical air velocity) shows a seasonal cycle in the climate model of 10 degrees.

p. 58, line 21ff: This section is rather speculative as to my opinion it streches the limit of the interpretation of the model results considerably. I might believe that at the given coarse resolution global CH4 emissions respond reasonably to orbital and precessional forcing. But I wouldn't trust emission estimates for individual grid cells or regions at the presented level, especially since regional estimates are not quantified or constrained well enough with the current approach. On top of that a factorial analysis for regional estimates is even more uncertain given that parametrisations are rather crude.
We understand this comment. However, we are not looking at individual grid cells, but at large-scale areas such as the boreal region (gridcells north of 30N) and tropics (area between 30N and 30S). The exception is the Indian/Asian monsoon group (as the smallest agglomeration of grid cells), which consists of three cells only. These cells display very characteristic behavior and are therefore studied separately. In addition, methane variations are often ascribed to monsoons in the literature.

The monsoon is a phenomenon that is in fact explicitly captured within the resolution of CLIMBER-2 (Montoya et al., 2005). A model-data comparison study for the Indian monsoon (Ziegler et al., 2010) shows reasonable agreement between simulated and reconstructed timeseries and their spectra.

p. 59, line 6: typo "PIE" should be "PIH"
Thank you for pointing this out. It is corrected in our revised version.

p. 60, line 24-27: Again I think the parametrisations not necessarily describe the real governing processes to make a robust statement about attribution and contribution to CH4 emissions.
The statements of this section were adapted to better express the tentative nature of this study.

p. 61, line 3: It is not astonishing that the 10_ latitudinal resolved grid boxes might average out large changes in soil moisture which greatly reduces variability in wetland extent and non linear amplification of CH4 fluxes, see e.g. Ringeval et al., 2010.
The study of Ringeval et al (2010) covers a time span of about a decade and looks at seasonal and interannual differences. In our model we certainly find seasonal differences (e.g. figure 2). The simulated influence of wetlands on methane production is averaged over 100 yrs. Consequently, the interannual variability is neglected and we are left with Milankovich-scale forcings on wetlands. According to our model results, the effects of these forcings on global wetland formation are minor in comparison to other effects on methane emissions.

This conclusion is supported by results of GCMs, which have a much higher resolution and less crude parameterizations of methane production and release. 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.

p. 61, line 21: In Figure 5 your emissions show a decreasing trend over the last 5000 years whereas measured concentrations are increasing. A recent model study using a gcm and much finer spatial resolution by Singerayer et al. 2011 shows that this trend might be explained by a shift in precipitation. I do not want to devaluate your model approach, but on the other hand one must be very careful with the attribution of causes from simple and coarse resolution models. Can you explain this disagreement?

Our model results show no such increase in methane production from natural wetlands. In fact, while Singarayer et al. attribute their results to subtle differences in precipitation between NH and SH, they do not give a detailed explanation of the cause for these differences between the Holocene (with an increasing methane concentration) and the previous interglacial (with no increase of methane concentration). They present a very interesting result, but it is based on just one simulation. As pointed out in de accompanying News&Views comment, such a subtle effect should be tested in other models before it is taken as a solid truth (Wolff, 2011).

p. 64, line 24: check Loulergue et al., 2008 it contains a wrong author list.
Done. We corrected that.

References:
Parrenin, F. et al. The EDC3 chronology for the EPICA Dome C ice core. Clim. Past 3, 485–497 (2007).

Ringeval, B., de Noblet-Ducoudre, N., Ciais, P., Bousquet, P., Prigent, C., Papa, F., and Rossow, W. B.: An attempt to quantify the impact of changes in wetland extent on methane emissions on the seasonal and interannual time scales, Global Biogeochem. Cy., 24, GB2003, doi:10.1029/2008GB003354, 2010.

Singarayer, J.S., Valdes, P.J., Friedlingstein, P., Nelson, S., Beerling D.J., Late Holocene methane rise caused by orbitally controlled increase in tropical sources, Nature 470, pp 82-85, 2011

Wolff, E.W., Global change: Methane and monsoons, Nature 470, pp. 49-50, doi:10.1038/470049a, 2011

Montoya, M., A. Griesel, A. Levermann, J. Mignot, M. Hofmann, A. Ganopolski, S. Rahmstof, The earth system model of intermediate complexity CLIMBER-3α. Part I: description and performance for present-day conditions, Clim. Dyn. 25, 2, pp. 237-263, 10.1007/s00382-005-0044-1, 2005

Ziegler, M., Lourens, L.J., Tuenter, E., Hilgen, F., Reichart, G.-J., and Weber, S.L., Precession phasing offset between Indian summer monsoon and Arabian Sea productivity linked to changes in Atlantic overturning circulation Paleoceanography, 25, PA3213, doi:10.1029/2009PA001884, 2010b