Clim. Past Discuss., 7, C62–C65, 2011
www.clim-past-discuss.net/7/C62/2011/

Climate of the Past Discussions

Interactive comment on "

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

Anonymous Referee #2

Received and published: 4 March 2011

General comments:

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 parametrisation 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 higly speculative. There is little said about the uncertainty in the modelling approach and how robust the given parametrisations are. I therefore strongly suggest that for a publication the analysis includes estimates of these uncertainties and sensitivies to different factors, rather than an interpretation of individual factors to the total CH4 emissions. I would envisage a publication if also the following comments are adressed.

Specific comments:

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

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

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 CH4 to climate or ice sheets.

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

p. 53, line 7: 5% of maximum saturation is very low for the support of CH4 emissions. Normally CH4 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.

p. 53, line 26: How is V defined?

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?

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.

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

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

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

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?

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 CH_4 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.

p. 59, line 6: typo "PIE" should be "PIH"

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 CH_4 emissions.

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 CH_4 fluxes, see e.g. Ringeval et al., 2010.

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 spacial 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?

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

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.

Singerayer et al., Late Holocene methane rise caused by orbitally controlled increase in tropical sources, Nature, 470, 82–85, doi:10.1038/nature09739, 2011

Interactive comment on Clim. Past Discuss., 7, 47, 2011.