We thank reviewer #2 for their helpful critical comments and suggestions. Below, we respond in detail to their comments and suggestions (our responses are in read)

Meng et al. present a sensitivity analysis of a methane emission model in the Community Land Model (CLM4) of the Community Earth System Model. This study very nicely highlights the difficulties of simulating methane emissions from wetlands on a global scale. The authors find very large global emissions of 256 Tg CH4 /yr, whereof the contribution from northern peatlands and wetlands (12 Tg CH4 /yr) are substantially lower than previous estimates. Beside the standard parametrisations of heterotrophic soil respiration and net primary production, CH4 emissions are also parametrised as a function of soil pH and redox potential. Simulated emission are compared to site data with acceptable agreement.

1) Unfortunately, beside the site data there are no real constraints for the model results on a regional or global scale. This makes it impossible to understand why the presented global emissions show these considerable differences to previous estimates. 2) Another shortfall, is the lack of a discussion of the models hydrology that plays a major role in the CH4 emission parametrisation. The sensitvity study shows that model parameters affecting the production and plant mediated transport are most important. 3) Thus the vegetation representation in the CLM4 and the deduced heterotrophic respiration and the net primary production are essential, and deserve a more thorough analysis in a revision of the manuscript.

Response: 1) There is certainly additional work to be done in comparing the model to atmospheric measurements. In this first paper we emphasized site level comparisons, Most global model studies are not evaluated at the site level. As we show in the paper, evaluating at the site level gives a much different result than evaluating on the grid cell level. However, we did extend our sensitivity results globally in section 4.9 (see Figure 13) and discuss the global budget in section 5. In the new version we present a more thorough discussion of why our results differ from previous results and possible model errors. However, while our high latitude emissions are low compared to previous estimates, there is considerable evidence that many previous results may be somewhat biased high. In fact, the recent inverse study of Spahni et al. (2011) found the posteriori emissions in the northern peatlands (north of 45^o) decreased by approximately 25% from their apriori values (from 38.6 to 28.2 Tg CH_4 y^{-1}). Another inverse study of Kim et al. (2011) estimated a methane emission of 3.0 Tg CH_4 y^{-1} from West Siberian wetlands which is much lower than the GISS inventory estimation (6.3 Tg CH_4 y^{-1}). Methane emissions from Canadian wetlands are estimated to be 3.5 Tg CH_4 y-1(Bachand et al.,1996). Canada and West Siberia have approximately 40% and 30% of the northern wetlands(Rydin and Jeglum,2006). So 70% of the northern wetlands account for ~7 Tg CH4 y-1. Assuming that other northern wetlands have similar methane emissions as Canadian and West Siberian wetlands would add another 3 Tg CH4 y-1. Walter et al. (2006) estimated an emission of 3.8 Tg CH4 y-1 from melt lakes in Siberia that might also appear as inundated land. Therefore, total methane emission from northern wetlands is likely to be about ~14 Tg CH4 y-1 which is close to our estimate.

2) We have modified our text in abstract and on page 16 and clearly stated that we did not use CLM4 hydrology simulated inundated fraction and water table positions. We forced the model with satellite inundated fraction.

3) As discussed in more detail on Page 6 our goal here is to include methane emissions into an Earth System Model, as a first step to being able to understand the impact of climate change on methane. Unfortunately, imperfections in the base climate model impact the methane emissions. However, as has been true with the inclusion of many additional processes into Earth System Models, it is worthwhile to include the additional processes before we have perfected the base model (as no model is ever perfect). We provide a more extended discussion in the revised manuscript of what the likely impacts are. Vegetation representation, heterotrophic respiration and net primary production have been studied in previous work including Bonan et al. (2011) and Thornton and Zimmermann (2007). We address the impact of these biases in the revised version of paper (see especially discussion in revised section 5.1), but it is outside the scope of the work to correct them.

—- General —-

It is very crucial that a global CH4 emission model is somehow validated against a global observational data set. An evaluation at site level is certainly helpful for the processes, but can not replace a global comparison. Global sources of 256 Tg CH4 /yr, already including the terrestrial soil sink of ~30 Tg CH4 /yr, are difficult to reconcile with anthropogenic CH4 emissions, the atmospheric CH4 sink and the atmospheric CH4 burdon.

Response: We include a more nuanced discussion of the magnitude of the sources in the newest version of the paper (section 5.1). The AR4 report suggests an uncertainty in the global source strength of +/- 15% or 87 Tg/year. The AR4 also suggests estimates of anthropogenic sources range between 264 and 428 Tg/year. Thus while 256 Tg CH4 /yr is on the high end of published estimates it is within the uncertainty of the budget. We note that the inverse study of Mikaloff Fletcher et al., 2004 suggest a wetland source of 231 Tg/year, close to our central estimate. In that study the net balance of sources and sinks of methane were well within the range of the error in the global budget. In addition it is important to remark that there is considerable uncertainty in our derived estimate of 256 Tg CH4 /yr as discussed in the paper.

A global comparison has been done partially for the initial model version by Riley et al. 2011. What are the big differences in spatial emission distributions compared to Riley et al. 2011? From Fig. 15 it is obvious that norhern high latitude emissions are much larger in Riley et al. 2011 as proposed to the 12 Tg CH4 /yr in this study. I assume that in both studies the vegetation and carbon fluxes are simulated by the same land model and that the inundation fraction is identical. So why is there this big difference? It obviously can not be the underestimated vegetation productivity as claimed in section 5.1.

Response: The differences between Riley et al. (2011) and our study are: 1) Riley et al. (2011) used a Q10 of 2 for their base simulation while we used a Q10 of 3; 2) The mean NPP-weighted inundated area in Riley et al. (2011) is approximately 80% higher than the satellite inundated area used in this study in Northern High latitudes; 3) Riley et al. (2011) excluded the several features used in this study including pH, pE, and faere. Our global sensitivity analysis indicates that using a Q10 of 2 will increase methane flux from northern latitudes by 80% (increases from 12 to 22 Tg CH_4 y^{-1}) and decrease methane flux from tropics by 22% for the year 1993. We repeated their simulations using similar inundated area, a Q10 of 2, and exclusion of pH, pE, and faere and estimated the same methane fluxes from northern latitudes. We added this discussion to the text on page 31.

In the model description it is mentioned that a fraction of the grid cell is non-inundated and emissions are reduced compared to the inundated fraction, and depend on the water table depth. How big is this difference per unit area? How much do CH4 emissions from non-inundated areas contribute to the annual total? Could this explain the large emissions in the tropics despite a reduced inundation area?

Response: Methane is mainly emitted to the atmosphere from the inundated fraction. There is generally a methane sink in the non-inundated fraction because water table depths are generally below 0.6 m. The average methane emissions from non-inundated fraction in 1993-2004 are -1.3 Tg / year. Therefore, non-inundated fraction does not significantly contribute to the annual total. Our estimation of global methane budget (256 Tg CH4/yr) is net methane flux that includes soil sink. We add a comment on this point in Section 5.1

It is further mentioned that you do not simulate wetland pfts, but that e.g. gas transport through grass arenchyma, which is determined by pft type, is most important for methane emissions and oxidation. One could thus argue that pft dependent parameters in the CH4 emission parametrisation differ for inundated and non-inundated wetlands and for northern peatlands. For which region are the impacts of parameter uncertainty, as given in Tab. 5, largest?

Response: In fact, we did not specifically investigate which region is mostly impacted by the parameter uncertainty. However, we suspect the parameter uncertainty will mostly likely impact tropical region mainly because tropical region emits the most methane into the atmosphere in our model. We did sensitivity analysis of soil pH, redox potential, and other features on regional methane budget and all of them largely impacted tropics as shown in Figure 13.

—- Specific —-

p. 6102, l. 20: How is the water table level calculated in the non-inundated fraction of the grid cell? What is the total soil layer depth and the soil layer resolution?

Response: We assumed that water table level (z) in the non-inundated fraction is strongly correlated with inundated fraction over each grid cell. Therefore, we calculated z as:

$$z = -1/(C_s*f)*\log(f_{inundate}/f_{max})$$

Where $f_{inundate}$ is the satellite inundated fraction, f_{max} is the maximum inundated fraction, C_s and f are two spatial variables used in Niu et al. (2005). Niu et al. (2005) used this equation to derive inundated fraction in each grid cell from CLM estimated water table depth.

We have added this information on page 16 and Appendix D.

There are totally 15 layers in the Community Land Model (CLM4) with a total depth of ~35 m. Only the total 10 layers are used for hydrology calculations. The soil layer depths for the top 10 layers are approximately 3.4 m. The depth of soil layer i, zi is calculated as

$$zi = f_s \{exp\ [0.5(i-0.5)]-1\}$$

where $f_s = 0.015$ is a scaling factor. The thickness of each layer Δzi is

$$\Delta zi = 0.5(z1+z2) \quad i = 1$$

$$\Delta zi = 0.5(zi+1 - zi-1) \quad i = 2,3, \ldots N-1$$

$$\Delta zi = zN - zN-1 \quad i = N$$

$$N = 15$$

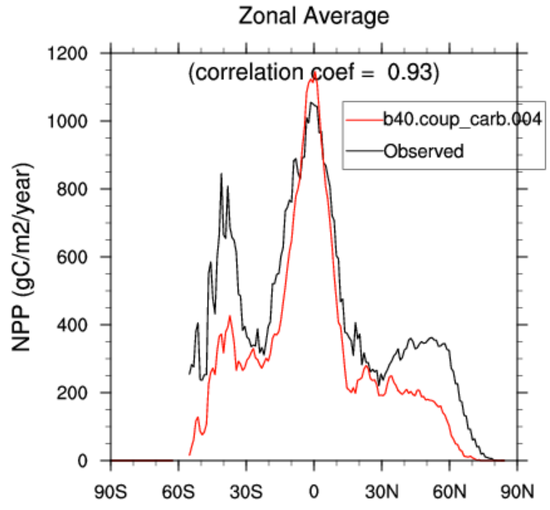
We have added this information in Appendix E.

p. 6104, l. 15: What is the reason for weighting the inundation fraction with NPP? Since you do not simulate a wetland pft, I assume NPP is not affected by inundation. Why would years with high NPP from a non-wetland pft lead to an increased mean inundation fraction? Is the weighting calculated annually or monthly?

Response: We weighted the inundated fraction with NPP because we wanted to take into account the seasonal variation in inundation and growing season, which could be correlated or anticorrelated: we really want the mean inundation area during the growing season. We have added text in the revised manuscript to clearly state the reason we used weighted NPP for the continuously inundated land on Page 12.

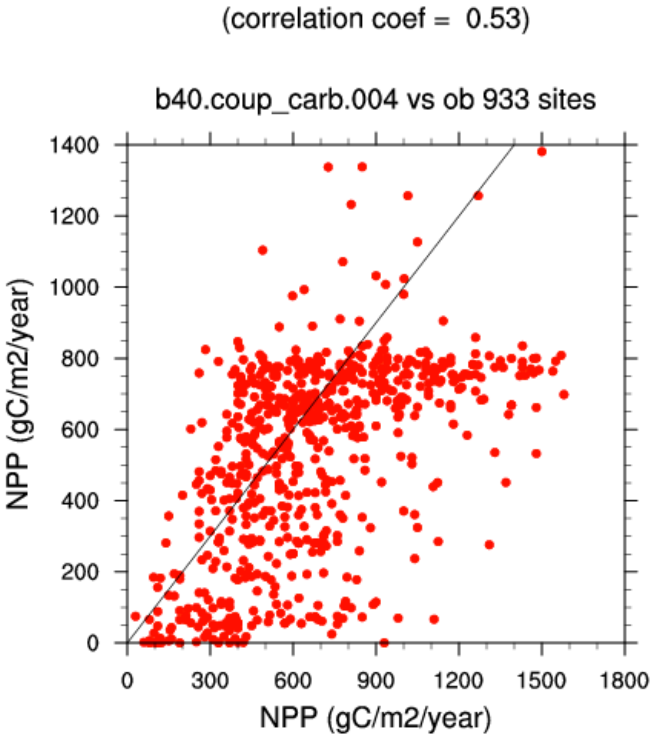
p. 6105, l. 8: How big are the errors of simulated NPP compared to MODIS NPP at the global scale? Is the NPP comparison at the sites (Fig. 6) representative for the globel errors?

Response: According to CLAMP project for Biogeochemical model evaluation, CLM4CN simulated NPP is quite low compared to MODIS NPP in high latitudes (45N-90N) (see the figure below).



(The figure is obtained from

http://www.cgd.ucar.edu/tss/clm/diagnostics/clm4cn/b40.coup_carb.004/npp/zonal_model_vs_ob.png)
Model estimated NPP is approximately 43% lower than MODIS NPP in 30N-60N and around 40S based on the above figure.
Based on Figure 6, the correlation of model NPP vs. MODIS NPP is 0.69 which is close to the correlation between observation and model simulations at 933 sites (see the following figure obtained from http://www.cgd.ucar.edu/tss/clm/diagnostics/clm4cn/b40.coup_carb.004/npp/scatter_model_vs_ob_933.png.



p. 6114, l. 8: Do you mean "seasonal mean and maximum fluxes" in units of "CH4 emissions per day"? It looks like to me that in Fig. 11a there is not a daily mean and maximum flux for each day.
Response: They are annual mean and maximum daily fluxes in Fig. 11a. We have modified the caption and added a sentence to make it clear in figure 11 caption.

p. 6117, l. 5: Are there measurements for aerenchyma properties in tropical grass? Wania et al. 2010 used peatland specific parameters. A better parametrisation of this obviously important parameter could help to narrow down the uncertainty.
Response: To the best of our knowledge, we are not aware of any measurements specifically related to tropical grass. I agree that developing a regionally variable parameterization would help narrow down the uncertainty. Unfortunately, some of the information is not readily available.

p. 6118, l. 7: The fact that CLMNC overestimates NPP in the tropics could be one main

reason for the high tropical production. What is the quantitative effect on CH4 emissions originating from this bias? l. 16: Did Riley et al. 2011 get the same production in northern high latitudes (see general point above)?
Response: Riley et al. (2011) and this study used the NPP predicted from the same CLM4CN model. We did not have the same methane production because this study applied pH, pE, and faere to control methane production while Riley et al. (2011) did not use them in methane production. There are several differences between Riley et al. (2011) and this study : 1) Riley et al. (2011) used a Q10 of 2 for their base simulation while we used a Q10 of 3; 2) The mean NPP-weighted inundated area in Riley et al. (2011) is approximately 80% higher than the satellite inundated area used in this study; 3) Riley et al. (2011) excluded the several features used in this study including pH, pE, and faere. We add a note that the model overpredicts NPP in the tropics, which could be responsible for an overprediction of methane from the tropics as well.

l. 26: Some numbers got mixed up, compare text and table 6.
Response: We removed the total budget from Chen and Prinn (2006) because we only compared our high latitude methane emissions with theirs. In fact, their global total budget in Table 6 is right, but we just did not put their emissions for tropical and midlatitude regions. Thanks for catching this mistake.

Figs. 1, 2, 14A: lines and dots leave the plotting range.
Response: Fixed, thanks!

Fig. 15: There have been great efforts in constraining global wetland emissions over the last 10 years through global satellite concentration data, biogeochemical modelling and atmospheric inversions. So, it is kind of unfair to compare emissions to results from the 80s and 90s that did not have the information at hand. Maybe you could add newer biogeochemical studies, e.g. Ringeval et al., 2010, Spahni et al., 2011, that have been evaluated themselves using atmospheric transport and chemistry models.
Response: Thank you for your suggestions. We have included them in our revised manuscript. Please note that Ringeval et al. (2010) only used inundated fraction for methane production and only optimized several variables at three sites (each from one climatic zone) and applied these values to the climatic zone to estimate regional methane fluxes. Ringeval et al. (2010) also did not consider redox potential and soil pH impacts on methane production. Exclusion of these features will definitely overestimate their regional fluxes. In addition, their model validation at the only tropical site (Panama) was very poor. So we cannot comment on the total tropical fluxes in their study.
We have also modified our text on Page 28-29 to reflect this comparison.

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