Interactive comment on “Constraining global methane emissions and uptake by ecosystems” by R. Spahni et al.

Anonymous Referee #1
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This paper outlines the development of a process-based methane emissions dataset. Output from the LPJ terrestrial ecosystem model are used, along with some empirical parameterizations, to derive methane emissions from Boreal peatlands, tropical wetlands and mineral soils, as well the soil sink. The paper presents two emissions scenarios (denoted S1 and S2), each with differing values of several model parameters. The derived emissions fields are used as prior emissions fields in two inversions. Finally, another version of the model is used to to examine recent inter-annual emissions variability. There is a pressing need for process-based, time-varying methane emissions estimates to explain some of the recent trends in atmospheric methane levels. This paper goes some way to meeting this need, and aims to develop a comprehensive emissions dataset. The comparison of ‘bottom-up’ and ‘top-down’ estimation strategies, as presented, is an excellent approach to this type of work. However, I have some concerns about the methodology that need to be addressed if the paper is to be published in Biogeosciences:

• One of the major results of the paper is the prediction of global emission rates from mineral soils. The authors propose that these are a significant contributor to the global methane budget, with emissions of 50-90Tg/yr. This finding is based on the extrapolation of results from three field studies (Sanhueza and Donoso, 2006; Xu et al., 2003 and Yan et al., 2008). However, it is not clear to me that such a large source can be justified in these studies. Xu et al., (2003) focus on the influence of soil conditions in the non-growing season on emissions from rice-growing regions during the inundated season. However, they do not appear to offer observations of emissions from partially-inundated soils during the nongrowing season. Yan et al, (2008) find tropical rainforest soils to be net sinks of CH4 (as have many previous studies). Since this is such a key component of this paper, I suggest that the authors strengthen the argument for the inclusion of such a large source in their estimates.
The argument for a global significant wet soil source was indeed not well presented in the manuscript. We relied our assumption on more than 'just' three field studies, but didn't show it explicitly. We therefore have added a table in the appendix of the revised manuscript. It includes the full list of field studies that we think are suitable to support our hypothesis of a globally significant wet mineral soil source. These studies report seasonal or annual CH4 emissions from non-saturated soils across various natural ecosystems. It illustrates that wet mineral soils might be globally relevant for many different soil-vegetation systems.

The above mentioned table also lists daily fluxes calculated from LPJ model results for annual mean and the month with maximum emissions. Model fluxes were given for the grid cell at the measurement site or the grid cell average for the representing region. Overall LPJ fluxes are in the same order of magnitude as the reported measurements. This is now detailed in the Appendix A of the revised manuscript.

• Most of the parameterizations in the manuscript are not fully justified or referenced. For example, on page 234 line 11, it is stated that the carbon conversion ratio should be 0.52% for mineral soils, however, it is not stated how this figure was arrived at. Other specific instances are given in the minor comments below.
Basically the carbon conversion ratio for wet mineral soils and for inundated wetlands are global tuning parameters. They must satisfy two criteria:

1) ***The global budget:*** *For wet mineral soils the global CH4 emissions have to fit within the global budget. Here we initially used the budget of Bousquet et al. 2006. Based on simulated emissions from inundated wetlands, rice paddies, peatlands and soil uptake, one can derive the magnitude of methane emissions of global wet mineral soils. The carbon conversion ratio is then tuned to match this global source.*

2) ***Regional fluxes:*** *This parameter must be chosen additionally in order to let LPJ fluxes match the range of methane fluxes per m2 as given in sources in the new table of the revised manuscript.*

For inundated wetlands the carbon conversion ratio is within the range of values reported in Table 1 of Christensen et al., 1996. Citation added to the text.

These clarification has been referenced in section 3.2, 3.3 and 4.1 and explained in Appendix A.

• Justification should be given for using a different version of the LPJ model for the investigation of inter-annual variability. Why were the models S1 or S2 not used?
There are several reasons for using an updated and calibrated version of the model:

1) *As analysed in section 4.2 and described in section 4.3 our inversion suggested different variations in the seasonal cycle of peatland emissions. We reassessed our LPJ ebullition parametrisation in peatlands that lead to a change compared to SC1 and SC2.*
2) *An important outcome of our study is that simulated peatland emissions are too large in SC1 and SC2 compared to inversions and the interhemispheric gradient of atmospheric CH4.*
3) *For the interannual variability the CRUNCEP input data was used to extend our analysis period from 1990-2006 to 1990-2008.*
4) *We used the result of the TM5-4Dvar inversions to calibrate global CH4 emissions of the individual source and sink categories. This was done as a linear pattern scaling with the factors given in section 4.3.*

All the above reasons led to very little change in total CH4 interannual variability. Attached to this reply are two figures showing the revised LPJ simulations (peatlands and global) and their impact on interannual variations (Fig. R1 and Fig. R2 below). The most important impact on the interannual variability of simulated CH4 emissions using LPJ are thus not the flux rates, but the variability in methane source area. This is shown in Figure 8a. These reasons are included in section 4.3 in the revised manuscript text.

• Two versions of the methane emissions model were compared against one another and against the inverse model results. Four parameters were simultaneously changed in the two different models. However, little justification is given for why only these two sets of parameter values were investigated. Furthermore, by changing all four parameters together, is it not possible that emissions signals due to a change in one parameter are being masked by changes caused by another? Could a more suitable combination of parameters exist outside of the two options presented? If the purpose of the inter-comparison exercise is to improve the parameterization, then surely a more suitable experiment would involve changing parameters independently? The authors should explore an experimental design that allows for this, or explain why an experiment involving only two parameterizations is justified.
The reviewer is right: multiple possibilities for parameters exist that would fit the results of the inversion. While a more comprehensive parameter test is relatively easy to do with LPJ emissions (which needs only a few global scaling parameters), it takes considerably more computational time and effort to run an atmospheric inversion to test an emission set for consistency with atmospheric chemistry, transport and observations. Ultimatively, such experiments fell outside of the scope of this project.

Moreover, our results also show considerably strong changes in the wetland-methane emission scenario i.e. larger CH4 source from wet mineral soils than from inundated wetlands, can be consistent with inversion results. This means that a wide range of possible parameter combinations could be reconcilable with the concentration observations.
Therefore, in our study we have chosen to vary the parameters that are most uncertain in the current LPJ methane emission modelling (many of the other model parameters are better constrained physically or biogeochemically). A major goal of this study is to demonstrate that multiple compositions of different wet ecosystem sources are possible in terms of the methane concentration inversions. Section 4.1 was changed and Appendix A was added to clarify this point.

• The 'bottom-up' model is compared to two inversions using atmospheric measurements. In section 4.2, the re-distribution of the methane budget between various source types is discussed. However, no mention is made of the ability of the observations to resolve different sources. For example, are the inversions really able to resolve a redistribution of 4% from fossil sources to ruminants and waste (page 239, line 19)? The covariances in the solution should be discussed, if possible, and uncertainties should be placed on these numbers as a minimum.

As stated in the response to Reviewer 2, prior and posterior errors were given in Table 3. It can be seen that the relative changes per category are substantial (+40% in domestic ruminants and -23% in oil/gas), and quite a bit bigger than the initial assumed uncertainties. Thus, while we can't directly address statistical significance, the per-category change suggested by the observations is quite strong. Uncertainties are not mentioned at the corresponding position in the text.

1 Minor comments

• Page 224, line 5 and 6. According to the latest IPCC assessment, CH4 radiative forcing is around 0.48W/m2, compared to 1.66W/m2 for CO2, so CH4 has approx. 30% of the contribution of CO2.
Text changed accordingly.

• Page 225, line 18 and 19. It is unclear what the authors mean by this sentence. Are the authors referring to the methane-OH feedback? Reference could be made in this section to the work of Prinn et al., (2005), and Montzka et al., (2011).
Yes, we refer to the methane-OH feedback. Sentence clarified and references added.

• Page 230, line 12, what is the justification for the range 20% to 25%?
Justification results from flux tuning to observational sites as done in Wania et al., 2009. Citation and clarification was added in text.

• Page 230, line 13, insert comma after classification
Thanks.

• Page 232, line 18, what is the justification for choosing the numbers 2.4% and 4.15%?
Justification is given by the global budget and regional fluxes as given in a Table A.1 in the revised manuscript (see answer to the main point above).

• Page 233, line 3-5, which version of EDGAR is used? Is the Olivier,1999 reference the most appropriate here?
We used EDGAR version 3. New EDGAR reference added to text.

• Page 233, line 3-5, how do we know that the 'missing' emissions are not due to an inaccuracy in EDGAR, rather than LPJ/CRU?
The disagreement is not because of emissions, but because of the location of rice paddies relative to precipitation. Even more, the rice map used in LPJ (Leff et al., 2004) and the location of EDGAR emissions agree very well. The Leff et al., 2004 map is based on the observed physical presence of rice paddies in the Indo-Gangetic plain, thus very reliable. The problem arises definitely that there is not much precipitation in the plain itself. This is needed for vegetation growth in LPJ, as LPJ has no river routing scheme between grid cells. In reality the Himalayan foothills provide the water to the river Ganges and to the Plain and thus the rice paddies. Added clarification in the text.

• Page 233, line 7-12, what is the sensitivity of the derived emissions to the cutoff at 45 degrees North? Why was 45 degrees chosen?
The reason for the 45degN cut-off line is that LPJ-WHyMe was specifically developed for methane emissions from peatlands in cold areas (either high latitude or high altitude), of which the majority is found north of 45degN. Since LPJ-WHyMe has not been tested yet for wetlands other than this kind of peatland, we chose to use the 45degN cut-off as a boundary between simulating CH4 emissions within LPJ-WHyMe for northern peatlands and using a more generic correlation approach described in Section 3.2 in our manuscript for the rest of the inundated areas. Clarification added to the text.

• Page 234, line 11, what is the justification for choosing 0.52%.
Justification are the global budget and regional fluxes as given in a new table in the revised manuscript (see answer to the main point above).

• Page 234, line 26, 'with', not whith
Thanks.

• Page 236, line 6. How are the oceanic and geological emissions distributed?
Geological emissions are assumed to be entirely oceanic, with a uniform flux in coastline gridcells. We used the same ocean/geological emission distribution as in Bergamaschi et al, 2007. This information has been added to the text.

• Page 241, line 11. A reference should be given for the statement that fluxes should peak in Sept/Oct.
Actually, it should NOT peak in Sept./Oct., as this is late in the growing season. What we mean in this section: emissions through ebullition should peak together with diffusion and plant mediated emissions in summer. Added "wrongly" in the text for clarification.

• Page 241, line 19. 'Nicely' is subjective. A more specific comparison should be given.
Formulation changed to 'now have an improved time correlation'.

• Page 242, line 14. The result is not 'confirmed', it is 'supported'.
Text changed.

• Page 244, final paragraph. It should be clarified whether the error bounds in this section are uncertainties or variability.
Clarified in the text as 'variability'.

• Page 245, line 13. See second minor comment.
What we mean is that increasing pollution could have increased OH to the point that just as much CH4 was lost as was emitted. See also answer to second minor comment.

• Page 245, line 23. Contributed similarly, rather than equally?
Changed in text.

• Page 248, line 20. Was the OH field constant, or seasonally-varying and interannually repeating?
In the TM5-4DVar setup, the OH field is prescribed and constant. In the LMDz setup, the OH is interactively changing and modelled in the interactive chemistry module.

• Page 251, line 16. Errors were ASSUMED not to be correlated?
Yes, if you prefer. This assumption is not a strong one with surface stations, but should be questioned for satellite data for example. Text changed.

• Page 251, line 28, and page 252, line 6. Are these errors not inconsistent (2% and 1.5%)?
The two models don't have the same resolution, e.g. in LMDz-SACS, 2% is for the mean in a grid cell. This "instrumental error" is quite small compared to the transport error (10%). So it's not a big issue if in the TM5-4DVar inversions did not use exactly the same value.

2 References

Montzka, S. a, M. Krol, E. Dlugokencky, B. Hall, P. Jockel, and J. Lelieveld (2011), Small Interannual Variability of Global Atmospheric Hydroxyl, Science, 331(6013), 67-69, doi:10.1126/science.1197640. [online] Available from: http://www.sciencemag.org/cgi/doi/10.1126/science.1197640

Prinn, R. G. et al. (2005), Evidence for variability of atmospheric hydroxyl radicals over the past quarter century, Geophysical Research Letters, 32, L07809, doi:10.1029/2004GL022228.

Christensen et al. 1996, Methane flux from northern wetlands and tundra - An ecosystem source modelling approach, Tellus 48B, 652-661, Table 1.

Bergamaschi, P., Frankenberg, C., Meirink, J. F., Krol, M., Dentener, F., Wagner, T., Platt, U., Kaplan, J. O., Koerner, S., Heimann, M., Dlugokencky, E. J., and Goede, A.: Satellite chartography of atmospheric methane from SCIAMACHYon board ENVISAT: 2. Evaluation based on inverse model simulations, J. Geophys. Res., 112, D02304, doi:10.1029/2006JD007268, 2007.

3 Figures

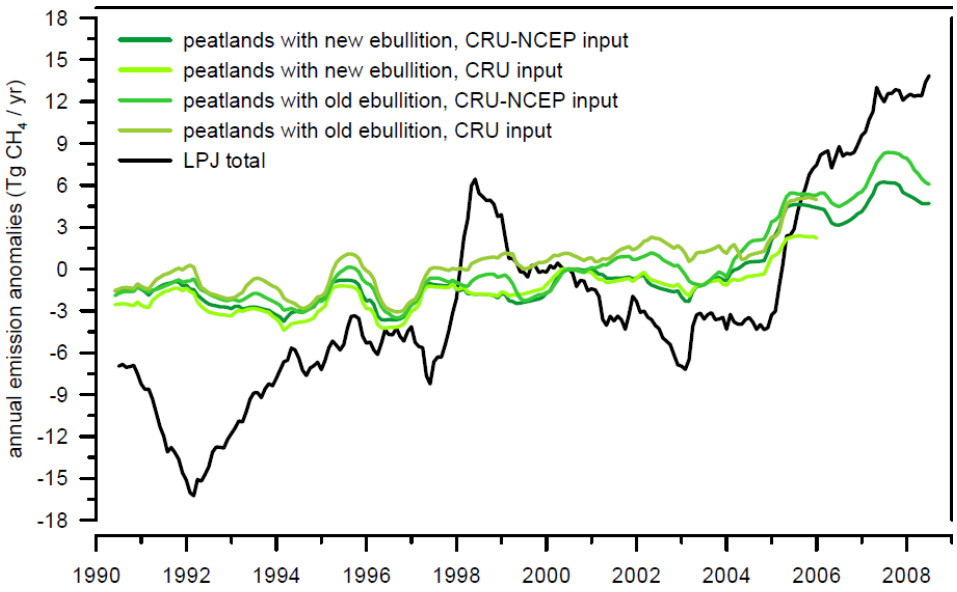

Fig. R1 Influence of new ebullition parametrisation and new input data (CRUNCEP instead of CRU) on interannual variability of CH4 emissions from northern peatlands (green curves) compared to interannual variability of net emissions from wet ecosystems as simulated with LPJ (black).

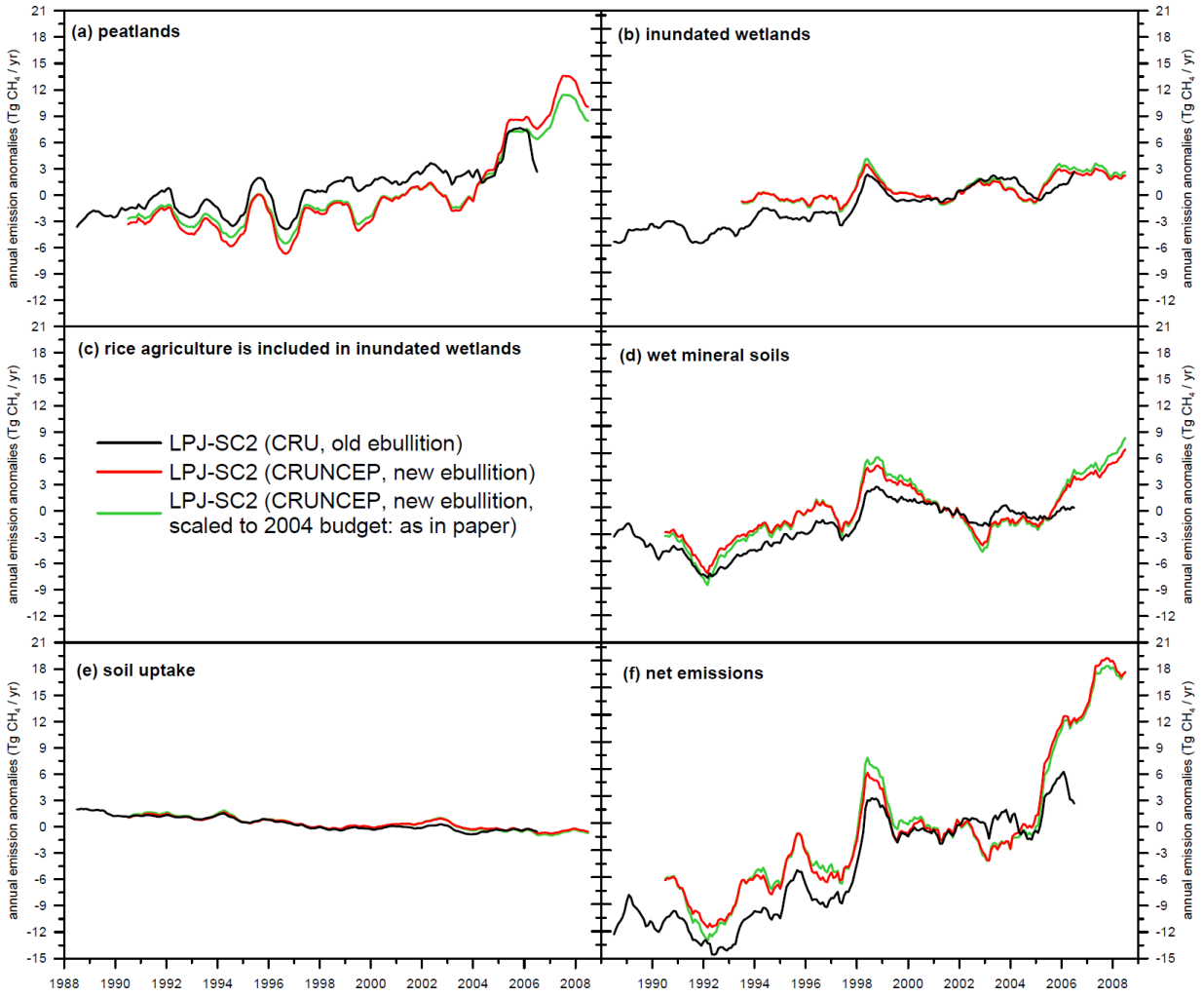

Fig. R2 Influence of new ebullition parametrisation, new input data (CRUNCEP instead of CRU) and global linear scaling to budget in 2004 on interannual variability of CH4 emissions from northern peatlands (a), inundated wetlands (b), wet mineral soils (d), soil uptake (e) and net emissions (f). Note that differences are small compared to variations with variable inundation area as shown in Fig. 8(a).