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Biogeosciences Discussions

Interactive comment on "Climate-CH_4 feedback from wetlands and its interaction with the climate-CO_2 feedback" *by* B. Ringeval et al.

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Answer to reviewer 2

We thank the referee 2 for his/her comments and provide detailed answers to all his/her comments below (answers are in bold) :

This paper provides estimates of the expected climate and carbon feedbacks on wetland methane emissions under one future emission scenario. The paper topic and format are suitable for Biogeosciences. The theoretical assumptions, methods, and results are well documented. The paper makes an important contribution to the study of climate, carbon, and methane cycle feedbacks. I support publication of the paper after minor corrections. Specific comments:

It would be useful to include a list in the main paper or Appendix of the different symbols used in the paper. Right now the description of each symbol is embedded in long paragraphs, which makes them difficult to reference as the reader progresses through the paper (and I needed to refer to the symbol definitions often!!).

We agree with the reviewer and to make the manuscript clearer we now list the different sensitivity terms on the enclosed summary diagram (added in the Appendix).

Page 3209, line 9: Suggest adding Montzka et al., Science, 2011 as a reference to support the statement that OH concentrations have little interannual variability.

We have added this reference in the revised version of the manuscript.

Line 17, Page 3213 Line 14, Page 3214: This paragraph is difficult to understand without the added information in Appendix C. Thus, I suggest including Appendix C in the main paper or shortening the discussion on wetland extent in the main paper and moving most of the explanation to Appendix C.

We have now moved a part of this paragraph into Appendix C.

Page 3214, Lines 8-10: "The way in which we compute anomalies (absolute or relative) has no influence on the role played by wetland extent in the following (Sect. 3.2.3)". Why? Please elaborate. Is it because the authors normalize the interannual variability in the saturated fraction of ORCHIDEE-WET to Prigent, 2007?

The interannual variability in the saturated fraction of ORCHIDEE-WET is not normalized to Prigent et al. one. Only the mean climatology (average of 1993-2000) of the modelled wetland extent is normalized to the same climatology from Prigent's data. This is now clarified in the text. We also slightly modified the text to better explain that the way to compute anomalies has no influence on the role played by wetland extent in the following.

Also, Sect. 3.2.3 is not in the paper.

Sorry, we intended the reference to read section 3.1.3.

Page 3216, line 12: 251 Tg/yr for global wetland emissions is very high as the authors indicate, much higher than inverse estimates or recent LPJ model estimates from Spahni, Biogeosciences, 2011. The authors provide a good comparison with Bousquet 2006, but can the authors please speculate further on what mechanisms in ORCHIDEE-WET itself might lead to high wetland emissions? Does ORCHIDEE have high biomass estimates? Or larger estimated wetland extent?

Both i) the spatial extrapolation of parameter relative to CH4 flux densities optimized on 3 sites and ii) the mismatch between mean real annual wetland extent and mean annual Prigent et al. data (climatology of Prigent et al. and ORCHIDEE wetland extent are the same: see above) can lead to high simulated wetland emissions. We have now added a discussion on the global emissions estimate in the revised manuscript.

Page 3217, line 25: unclear what is mean by "an anomaly method"

We have changed the sentence to: "The model is forced by climate fields taken from a transient simulation of the IPSL-CM4 climate model with prescribed GHG-forcing for historical and future (SRES A2) scenarios. These climate fields were bias corrected by removing the difference between the climate model climatology (over the period 1961-1990) and the observed climatology from CRU."

Page 3218, lines 20-23: "Regardless, using CH4 flux densities and wetland area from two different simulations to compute wetland CH4 emissions does not allow [for] the possibility of removing the indirect effects of wetland extent [variability] on CH4 fluxes". Why? Which are the two different simulations? What are the indirect effects?

Change in simulated wetland extent leads to change in the computed soil water content (through change in modelled runoff) that could have an effect on the ORCHIDEE modelled carbon cycle. We named such effect as "indirect". We

have not performed specific simulations to isolate such indirect effect. We added explanations to be clearer.

Page 3227, line 12: the C-CO2 fertilization interaction on wetland CH4 emissions are a large percentage of the total change in atmospheric CH4 concentrations. The same $\beta_{C->M}$ is used for all calculations. Is it possible to say something about the uncertainty on $\beta_{C->M}$?

Uncertainty about the value of CO2 fertilizing effect on CH4 emissions is given in section 3.2 ("Literature based estimates of $\beta_{C->M}$") when range of values found on different sites is discussed. The uncertainty about $\beta_{C->M}$ is also largely discussed into the final discussion and in particular, at page 3232 from lines 7 to line 27.
The $\beta_{C->M}$ value used for calculations in the manuscript comes from ORCHIDEE simulations. This value can be considered as a highest boundary limit. Lowest boundary limit could be a value of $\beta_{C->M}$ close to 0 (e.g. as observed in site manipulation of Panacotto et al., 2010). Thus, the value of $\triangle$CH4 given on the Figure 4 when the fertilization interaction is not accounted for could be used as a lowest boundary relative to the uncertainty of the CO2 effect on $\triangle$CH4.

Page 3227, line 19: It would help the reader to get a feel for the numbers if the authors provided the SRES-A2 concentration changes for comparison with the ORCHIDEE-WET values in the text.

$\triangle$CH4 ($\triangle$CO2) given by SRES-A2 is equal to 2925.4 ppbv (549.8 ppm). We have now included these numbers in the revised version.
We modified the following sentence:
"As a first check on our framework, we compared the uncoupled estimates of $\triangle$CH4unc and $\triangle$CO2unc to the values given by the SRES-A2 scenario (2925.4 ppbv and 549.8 ppm respectively), where none of the CH4 feedbacks presented here were accounted for. We find a CH4 concentration increase by 2100 of 3030

ppb and a CO2 concentration change of 496 ppm, not far from the SRES-A2 concentration changes (IPCC, 2001)." and added this one "We note that the CO2 concentrations given in IPCC (2001) already accounts for a C-CO2 feedback."

Page 3228, first paragraph: missing grey and green lines, maybe the authors' mean red and blue?

Correct. We changed the reference to red and blue lines.

Page 3229, line 23-24: Which values of γ_M are the authors comparing? In Table 2, for Q10=5.5 and dynamic wetland extent, one γ_M is more negative (-4.85) and one is less negative (-1.51) than the γ_M estimates for Q10=3 and dynamic wetland extent (-1.83 and -3.27). Please clarify the values used for comparison in the text.

We have clarified this text.
We used here the value of γ_M found when Tmean is considered as a constant. The fact that a higher Q10 can lead to a more negative value when Tmean is variable could be considered as a strange result at first appearance. An indirect effect of a Q10 increase is a little change in the preindustrial latitudinal distribution of wetland emissions. When Tmean is considered as a variable, changing the Q10 has a very little impact on the methanogenesis rate which cannot counterbalance the effect of change in latitudinal emissions. Thus, the γ_M is more negative (-4.85) in this case.
More information with equations is given in Supplement to help to a better understanding.

Page 3230, line 23: "In particular, in some regions, NPP decreases under future climate change..." Would this lead to an overestimation or underestimation of the expected change in atmospheric methane concentrations?

Simulating a more realistic wetland NPP (i.e. without accounting for the variability in the soil water content on the productivity) could lead to decrease the

change in the methanogenesis substrate simulated in the current manuscript version and thus could lead to a more positive γ_M (i.e. a more positive feedback).

Discussion: The authors give a very detailed list of several possible uncertainties which might affect the results. It would be useful if the authors could provide a quick overview of which 2-3 of their list of uncertainties likely lead to the largest error (or create the largest uncertainty) in the results.

We have now included a quick overview of the highest uncertainties into the conclusion and the abstract: namely, the sensitivity of methanogenesis substrate to warming, the CO2 fertilization effect on the wetland CH4 emissions and the evolution of wetland area.

Page 3233, line 10 and line 13, increase in available substrate is mentioned twice as a reason for increasing CH4 emissions.

Indeed, but the first is relative to the substrate sensitivity to climate and the second one to the substrate sensitivity to atmospheric CO2. Thus we decided to maintain the both.

Conclusions: By what percentage do methane emissions increase when both C-CO2 and C-CH4 feedbacks are included vs. when only C-CH4 feedbacks are included? Numbers are given (-0.016 to 0.024 for ex.), but it would be useful to have the information somewhere as a percentage to highlight the importance of the interaction between the carbon and methane cycles.

We prefer to keep the numbers in term of gain (unitless). Values of change in CH4 atmospheric concentration (expressed in ppb) between the no feedback case and the all feedback case are also given (line 1 at page 3234). We do not think adding changes, expressed as percentage, would help the reader. We think also that giving value for intermediate case (as suggested by the reviewer) is not a

simple take away message for a reader who reads only the conclusion.

Technical comments:

All the following technical comments have been addressed in the revised version of the manuscript.

Page 3211: define variable Fcf
Page 3213: extra '(' on line 6; change 'and well as' to 'as well as' on line 6; 'also make use of' on line 7 is written twice
Page 3217, line 4: should read "concentration scenarios"
Page 3217, line 11: delete "the" before combining
Page 3218, line 2: add s after "concentration"
Page 3226, line 19: extra Page 3236, Appendix C: missing several "the" in text.
Page 3236, Line 7: include Figure number
Table 3: define F
The text in Figures 3 and 4 is too small.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/8/C1620/2011/bgd-8-C1620-2011-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 8, 3203, 2011.

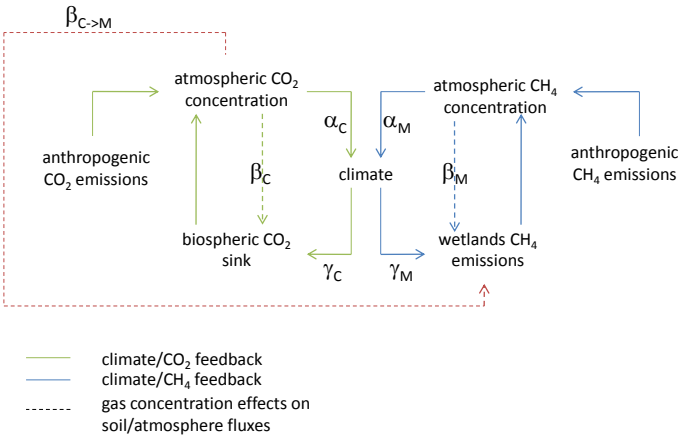

Fig. 1. new Figure of the Appendix where the different sensitivity terms are listed