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Biogeosciences Discussions

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

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Interactive Discussion

Discussion Paper

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 non-growing season. Yan et al, (2008) find tropical rainforest soils to be net sinks of CH_4 (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.

- 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.

- 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?

- 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 '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.

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.
- 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).

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Interactive Comment

- Page 230, line 12, what is the justification for the range 20% to 25%?
- Page 230, line 13, insert comma after classification
- Page 232, line 18, what is the justification for choosing the numbers 2.4% and 4.15%?
- Page 233, line 3-5, which version of EDGAR is used? Is the Olivier,1999 reference the most appropriate here?
- 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?
- 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?
- Page 234, line 11, what is the justification for choosing 0.52%.
- Page 234, line 26, ‘with’, not whith
- Page 236, line 6. How are the oceanic and geological emissions distributed?
- Page 241, line 11. A reference should be given for the statement that fluxes should peak in Sept/Oct.
- Page 241, line 19. ‘Nicely’ is subjective. A more specific comparison should be given.
- Page 242, line 14. The result is not ’confirmed’, it is ’supported’.
- Page 244, final paragraph. It should be clarified whether the error bounds in this section are uncertainties or variability.
- Page 245, line 13. See second minor comment.

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- Page 245, line 23. Contributed similarly, rather than equally?
- Page 248, line 20. Was the OH field constant, or seasonally-varying and inter-annually repeating?
- Page 251, line 16. Errors were ASSUMED not to be correlated?
- Page 251, line 28, and page 252, line 6. Are these errors not inconsistent (2% and 1.5%)?

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.

Interactive comment on Biogeosciences Discuss., 8, 221, 2011.