Atmos. Chem. Phys. Discuss., 13, C2358–C2361, 2013
www.atmos-chem-phys-discuss.net/13/C2358/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stable atmospheric methane in the 2000s: key-role of emissions from natural wetlands" *by* I. Pison et al.

Anonymous Referee #2

Received and published: 15 May 2013

This manuscript presents an evaluation of the methane trends between 1990 and 2009 with a focus on the years 2000-2006. The study presents a comparison between surface fluxes from inversions of surface data and from a process-orientated model for wetlands and they observe inconsistencies between both which they argue are due to problems in the inversion. Explaining the recent methane trend and the role of wetlands is certainly a key area of research of interest to the readers of ACP.

However, the presented study has a few issues. It is well known that the flux calculations from process-oriented models and from inversion of surface data have many issues and without addressing these issues in much more detail such a study is of questionable value. Indeed at least the problems of surface fluxes from inversion for the Tropics are mentioned at the end of the manuscript. Especially, for the S-America

region which is the main focus of the presented study, the used surface network seems to provide little/no information so that a comparison of fluxes for these regions is not very useful. My suggestion is that the manuscript needs to provide much more information on the quality and performance of the process-oriented model and the inversions otherwise I do not think that such comparisons are useful.

-The recent study of Melton et al., 2013 showed that there are very large uncertainties in CH4 emission rate of process-oriented model for wetland regions even when accounting for uncertainties in wetland areas. Please provide a critical assessment of the uncertainties of the ORCHIDEE calculation. Even better would be to use an ensemble of process-oriented model or provide some compelling arguments why ORCHIDEE should give a useful representation of wetland emission.

-Surface fluxes from inversions of atmospheric data will depend critically on the assumed a priori fluxes, transport model errors and representation errors and it is important to discuss these error components. Maybe most important is the influence of prior assumptions and you need to give some information on the impact of the prior on the results as well as of potential correlations between regions and source types.

-The manuscript presents two inversion methods, but the setup of both inversions is so fundamentally different (different surface data, different priori assumptions, different transport models, different correlations) that such a comparison is not very meaningful if not done in more detail. I also do not understand why the variational scheme does not estimate wetland emissions so that it could be used throughout the whole manuscript.

Minor: I would suggest capitalizing 'El Nino' and 'La Nina' throughout the whole manuscript

p.2 l53: (e.g. (Ringeval et al., 2010) and -> (e.g. (Ringeval et al., 2010)) and

p.4: Provide a discussion how well ORCHIDEE performs by validating/comparing it datasets or models (e.g. Melton et al., Biogeosciences, 2013)

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p4. L.108: water detection Papa et al. (2010); Prigent et al. (2012). -> water detection (Papa et al. (2010); Prigent et al. (2012)).

p. 4 l.113: fromWalter et al. (2001a);Ringeval et al. (2010) -> fromWalter et al. (2001a) and Ringeval et al. (2010)

p.5 l.131 Note that ORCHIDEE emissions are not used as prior emissions in INVANA nor INVVAR. -> INVANA and INVVAR are not yet introduced so that this needs some additional explanation.

p. 5 l.145: Please specify which types for sources/sinks are estimated.

p. 5 l.146: Discuss the spatial distribution of the 68 surface sites.

p.5 l.152: Elaborate on the a priori assumption for wetland emission. What is the assumed spatial and temporal distribution of emissions. How does this compare to the Orchidee emissions? Do you assume a spatial distribution within each of the 10 regions?

p. 6 l.161: Why do you only estimate net fluxes with INVAR? It should be possible to also estimate different types similar to INVANA (see eg. Bergamaschi et al., 2009).

p.6, l . 162: Why is a different surface dataset used for INVVAR and INVANA. This will complicate the intercompraisons.

p. 6, l. 173: How do a priori emissions, especially for wetland compare to those used for INVANAN and to ORCHIDEE. What is the assumed spatio-temporal distribution of wetland emissions.

p. 6, l.182: why is the reference period 1993-2007 and not 1990-2009?

p.6, l. 182: why is 1993-2007 written in bold?

p.7, l. 194 The total net emissions are . . . Is this now referring to the mean of the 1990-2009 time period? It might be better to summarize the values given in this paragraph

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in a table.

p.7, l. 204 are in good agreement, in the phasing through time. . . -> This is not true for the 2003-2005 time period

p.7 I would suggest to include the priori fluxes used for INVVAR and INVANA in figure 1. I would expect that this will be interesting to see how much variability has been built into the priori fluxes already.

p. 7, l. 211, .. which indicates that wetlands explain about 90% of the variability of total methane emissions.-> which indicates that wetlands explain about 90% of the variability of total methane emissions inferred with INVANA.

p.9, l.278 The comparison between ORCHIDEE and INVANAN in figure 3 top panel looks pretty poor for the whole time period and the 2000-2006 does not appear very different in this figure except that the duration of the minimum in INVANA (and maximum in Orchidee) is longer

p.9 l. 288 The time series between ORCHIDEE-all and ORCHIDEE-sat with the run with prescribed wetland areas from Pringent show little similarities so that it is difficult to have confidence that the somewhat better agreement for 2000-2006 has real significance.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 9017, 2013.