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

Anonymous Referee #2
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General Comments

Overall I like this paper because there are some interesting results presented in it. It is not very well written though, and it could use some revision to make it clear, concise and less confusing. I like the fact that inversions were used to improve a bottom-up model. However, the disagreement between the inversions, and lack of justification for believing the inversions was a disappointing. The authors give the impression that the inversions are the same thing as the observations, and this is most certainly not true.
This is certainly not intended. One can say that the observations are a not strong enough constraint for the inversion to distinguish between all of the sources, because they have a significant spatial overlap. This statement was added to section ections 4.2:

“Although, the overall reduction in uncertainty is considerable (66%), the atmospheric inversions can only give us estimates of how the LPJ fluxes should be corrected. The observations alone are not a strong enough constraint to distinguish between all sources, because they have significant spatial overlap.”

In particular, I think the issue of whether the seasonal cycle of TM54dvar or LMDZ-SACS is closer to the truth should be examined - they can’t both be right!
This is impossible to do, since the two seasonal patterns produced by the two types of inversions are by definition both reconcilable with the observations. Really, the difference between them gives us a good idea of how uncertain the fluxes estimated by inversion are. Thus one of our results was that the seasonality of wetland methane emissions estimated from the observations is sensitive the transport model used in the inversions. Text added in section 4.2.

Also, it would be nice to run the improved LPJ as a prior for one or both inversions. Another problem in my view, is that the TM52Dvar inversion stays really pretty close to the priors. It seems the only information coming out of it at the scales discussed in the paper is that it likes a more distributed source like ruminants rather than point sources like oil/gas.
This result is discussed in detail below, but it is not the only result of the KNMI inversion. For the purposes of this paper, the two main results of the KNMI inversion are (1) that northern peatland emissions seem to be weaker than assumed a priori, and (2) that available observations cannot distinguish clearly between wet mineral soils and wetlands.

Because of the issues with the inversion, I was less interested in the trend calculations in the last sections of the paper. I was wondering whether this section could be a separate paper eventually. Also, no discussion was given of why the Bousquet inversion was also brought into this study.
We admit, one can get the impression that the LPJ results of the interannual variability depend on the global scaling of the inversions. But in fact this is not the case (see figures in reply to referee #1). The scaling does only apply to global totals of the individual LPJ source and sink categories, not the temporal, nor the spatial patterns. The interannual variabiliy is thus mainly an outcome of the LPJ dynamical global vegetation model reacting on interannual climate variability. Text was clarified in section 4.3.

The authors advance the idea that mineral soils are very large component of natural methane emissions, and this seems like a relatively new idea. I'm looking forward to reading the next version of this paper.

Specific Comments

P225, L9 - there's a new paper by Montzka that seems to be more specific about what the interannual variability of OH is. The variability is about a few %, but this is still on the order of observed interannual variability as reported by Dlugokencky.
Reference added.

P225, L14 - But increases in anthropogenic sources should be fairly steady over the past decade, while the increase appears to have abruptly started around 2006.
We agree, sentence was changed: "The renewed increase seems to reflect growing anthropogenic emissions of CH4, while the temporary balance was not."

P225, L16-19 - The discussion of the chemistry changes here is rather vague. It would be nice to have a sentence more describing what pollution at sub and tropical latitudes is limiting CH4 growth, and how increases in CH4 emissions may have caused this.
We changed text to:
"The rapid rise in air pollution at (sub)tropical latitudes may have enhanced tropospheric OH to the extent that it could be increasingly limiting the growth of CH4 concentration, which may itself originate from increased anthropogenic CH4 emission."

P226 - Just to be clear - is the Wania et al part of LPJ just doing the peatland emissions? Or is it also being used for inundated wetlands too? I thought it was just peatlands....
Yes - in LPJ-WHyMe the mechanistic modelling of CH4 emissions including production, oxidation and transport is ONLY applied to peatlands. But at the same time LPJ-WHyMe also produces all necessary output for the parametrisation of emissions from wet mineral soils and inundated wetlands. It is well described in the follow-up section 2.1.

P232 - A general question: do this framework need to know the depth of standing water? How is this treated? If the water is too deep, CH4 won't make it to the atmosphere.
Peatland methane emissions are modified by the actual depth of standing water, inundated wetland emissions are not. Inundated wetlands are assumed to have equal depth of standing water globally for the period of inundation. However, the loss of methane during the transport is implicitly considered by choosing the global tuning parameter $r_{C[CH_4]/C[CO_2]}$*. This is described on P232, L17.*

P234, L22 - Both of these scenarios suggest that a significant portion of what we think of as wetland emissions is coming from "wet mineral soils". This strikes me as a considerable revision of our thinking on this. I think the authors might want therefore to give a little more detail on the field studies. Is it just the Yan paper? I admit that I haven't read it and don't want to hold up the review any longer (but I will have a look at it!).
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 revised manuscript and also attached a more comprehensive table to the reply to referee #1. 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 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. Text was added in section 3.3 to clarify this point.

As I understand it, if there is a layer of dry soil over the wet layer, not much CH4 will come out. Does the model take this into account?
Actually, this is the standard case for wet mineral soils that have a low soil moisture content. That's why emission rates are so small. We also assume that after a rain event or a flooding event, soil moisture levels are increased, although not saturated, and lead to less oxidation and more emission for a short period of time. The tuning factor $r_{C[CH_4]/C[CO_2]}$ for wet mineral soils is assumed to reflect this additional oxidation compared to $r_{C[CH_4]/C[CO_2]}$ for inundated wetlands, thus it is about an order of magnitude smaller (see fluxes from literature in the new table in the manuscript).

P235 - I think it's really nice to have uptake and emission in soils coming from the same model.
Thanks.

P236, L21 - I'm confused by the statement that the global tuning parameter is applied to each ecosystem type, I though they were applied to each category of wetland.
Each category of wetland is considered to be a type of a "wet" ecosystem. Added "wet" in the text.

P237, L1 - remove "on" before "the latitudinal...".
Done.

P237, L3 - I don't understand what is meant by satisfying the "regional magnitude of flux rates".
Meant are actually "regional averages of local methane flux rates" based on estimates from field studies. Text was clarified.

P237, L13 - replace "for the" with "in".
Done.

P237, L14-15 - what is meant by "temporal pattern"? The seasonal cycle?
Yes, changed accordingly.

P237, L20 - How does one get the range 5-15gCH4/mˆ2*month from 60 GgCH4 grid/cell/month? Is this because the grid cell areas change with latitude? I general, the units discussed should be in units that people are familiar with and can compare easily with field studies. gCH4/mˆ2/month is ok, but mgCH4/mˆ2/day is better. The authors should get rid of all gridbox-based units. This goes for Figs 1,2,and 6 as well. Otherwise it's very difficult to compare results.
This is indeed not simple and can't be calculated directly without more information. The reasons are: First, we combine four different emission fluxes (peatlands, inundated wetlands, rice paddies, wet mineral soils) and a sink flux (mineral soils) in one grid cell. Second, each category has a different fractional area (not shown in paper) in this grid cell and thus a different weight. Third, temporal averages may be different for the different units. And finally, yes the total grid cells area change with latitude.
While people who work in the field are more familiar with units per square meter, the total integrated emissions for a larger region is more important from an atmospheric point of view. Therefore, we added a figure for the individual categories with the units in g CH4 per m^2 and year (Figure A1 in revised manuscript). We strictly handle units in figure and text in emissions per year if it is an annual average or emissions per month if its a monthly average for a specific month, since we do not calculate daily averages in the model. However, we put LPJ emissions in mg CH4 m² and day in Table A.1 in the revised manuscript to be comparable with field studies. We further added the resolution of the grid to figure captions. Text dealing with rates have been adapted and explanations are adde in

the Appendix A.

P237, L20 - I’m not sure whether the 5-15g CH4/mˆ2/month applied to tropical or boreal latitudes, but both of these numbers seem much higher than estimates I’ve seen (e.g. 40 mgCH4/mˆ2/day).
The numbers given in our manuscript can't be directly compared to annual emissions from other studies because we use the units of CH4/month for one specific month. You can't multiply the monthly values by 12 to get an annual value as they are not averages over the entire year. If you compare emission rates in t Table A.1 in the revised manuscript you can find seasonal/annual averages for individual sites compared to model means. Rates and description were moved to the Appendix A.

P237, L21-22 - a comma is needed after "regions", but not after "areas".
Thanks.

P237, L22-23 - where and why is the model emitting more consistently throughout the year? This must be at low latitudes, right?
Yes, text clarified.

P237, L27 - 30S - 0 (to be consistent with 0-30N).
Done.

P238, L 15 - What is meant by emission areas varying seasonally but not interannually? That wetland areas don’t vary from year to year? What about the wet mineral soils? Are they being driven by met. data?
Statement clarified in text: only peatlands and inundated wetlands have no interannual varying emission areas. Wet mineral soils and soil uptake areas do vary from year-to-year depending on met. data.

P238, L18 - Replace "to" with "from" and please run a spell checker over this sentence!
Done.

P239, L17 - At least some mention is needed here of whether or not these small differences are significant! It is interesting that the inversion wants to move emissions from point sources (like oil/gas) to spread out sources (like ruminants), but without mention of uncertainties, one doesn’t know whether this is interesting or significant. I appreciate that it’s difficult to estimate uncertainty with the 4dVar assimilation, but this doesn’t mean the issue can be totally ignored!
Please note here that uncertainties were computed in the inversion, with both prior and posterior errors given in Table 3. Comparing the changes in, for example, domestic ruminants and oil/gas to their prior uncertainties (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 added at the corresponding position in the text.

It can indeed be argued that the inversion favors a more spread-out source. This is not necessarily an unphysical result; it shows that, given the modeled transport of the TM5 model,, the observed methane concentration is difficult to explain by "point sources". But more importantly, the transfer of emissions from oil/gas to domestic ruminants seems to be largely due to the difference in where these sources are on the planet: oil/gas emissions are heavily located in the Northern Hemisphere (e.g. Russia / Siberia), while domestic ruminant emissions are strong in South America. Focusing Thus the shift from oil/gas to ruminants largely reflects the increase in tropical / southern hemisphere emissions required by the inversion. This finding has been added in section 4.2.

P239, L20 - This sentence is very vague. Are peatland emissions in both really halved? From the fig, it

seems this is not the case, though it is reduced.
Not both, but just for SC1. Sentence was clarified.

P239, L24-25 - This is an interesting statement and deserves more discussion. I would have expected this result from inversions using only surface obs, but why don't the satellite observations allow these sources to be distinguished? Is it because the wet soils are co-located with the inundated wetlands? Or is the satellite data resolution not good enough? Or is the satellite data weighted less than surface obs. in the inversion?
Indeed, wet soils and permanent wetlands are difficult to distinguish in the satellite inversion because their spatial overlap is strong (this can be seen in Fig. 1). Though the satellite observations have high spatial resolution in measured concentrations, we must remember that this does not necessarily translate to the same resolution in emissions inferred by inversion. The reason is that the modeled transport connects emissions and concentrations. In other words, after atmospheric transport and mixing have taken place, these two emission scenarios yield roughly similar concentration distributions.

P240, L14-16 - The previous page mentioned a 6% increase in global total (shown also in Figure 5), so the 40% increase in ruminants over the Western Hemisphere must be compensated by decreases elsewhere. Is this the case - is the decrease over one particular region? Or is it spread out?
It can be seen from Table 3 and Figure 6(f) that the net change in CH4 emissions is relatively spread out over various regions, but with strong reductions/increases in particular regions.

Figure 5 - Now that I'm looking at Figure 5, I'm struck by how little things change from the priors. With the exception of a trade-off between oil/gas and ruminants. The inversion doesn't seem to want to move away from SC1 priors or SC2 priors. This could indicate that the data are not a strong enough constraint. One way to address this would be to try extremely different scenarios and figure out how much a change in a prior it would require to get a response.
The apparent shift of emissions from oil/gas to domestic ruminants is indeed a dominant feature of this inversion, yet not the focus of this paper. Though the other sources don't change as much, there are two important points to Fig. 5: (1) that SC1 and SC2 are equally justifiable in the face of observations, and (2) that emissions from Northern hemisphere peatlands are consistently reduced to about 6% of the total budget. Changes made to section 4.2.

The reduction in peatland emissions is partly due to including wet soils as a source to the boreal region. Previous estimates of peatland methane emissions e.g. Chen and Prinn (2006) allowed for more peatland emissions (33+/-18 TgCH4 yr-1) because peatlands were the only natural northern source, whereas in our case peatlands have to share the 'allowance' with wet soils.

In order to investigate the strong change in domestic ruminant emissions, we have performed a sensitivity test where this category is not allowed to vary. The result (not shown in this paper) is that optimized regional emissions end up at about the same level, with biomass burning and wetland / wet soil emissions compensating for the emissions normally attributed to domestic ruminants. However, we believe this result to be outside the scope of this paper since it deals with anthropogenic emissions - rather, we will focus on points (1) and (2) above.

P240, L19 - The constraint is imposed by the observations, not TM5-4dvar (which itself is constrained by obs).
That's correct, text changed.

P241, L3,4 - I don't understand the part about how total fluxes in LMDZ-SACS are constrained over 8-day periods. What is apparent from the Fig 7 is that the LMDZ fluxes are very close to the LPJ prior a lot of the time, though there is a correction towards earlier emissions. I think the differences in seasonality are very interesting, and it would be good to explore this further. What do the error bars on

the inversion look like? My intuition would tell me that the largest emissions would occur towards the end of the growing season, after things have heated up all summer, and in this regard the LPJ model looks most reasonable.
It means the inversion gives one correction coefficient for each eight-day period. The less-than-one-week variations are then the same than in the prior (but not in intensity). The errors for the 2004 inversion using LMDz-SACS are reduced globally from 11 to 9 Tg/yr and also regionally, e.g. from 2.8 to 1.7 Tg/yr in boreal Eurasia. The error bar for anomalies very much depends on how it is defined in the first place. We think that uncertainties involved are substantial if the seasonal timing from two inversion results are so different. Since the scope of this paper lies not in a detailed understanding of each inversion, we do not investigate this further.
However, the timing of the late methane peak is dominated by the ebullition emissions in LPJ. The revised parametrisation now coincides with emissions through diffusion and plant mediated transport, which we find more reasonable.

Also, if you integrate over the whole year, do you get the same totals?
No. Total peatland emissions after inversions using TM5-4Dvar are reduced as stated before in section 4.2. In addition total peatland emissions are reduced after implementing the new ebullition (see next comment).

P241, L8-23 - I really like the fact that inversions are being used to revise the prior model. I think I would caution the authors against referring to the inversions as the "observed concentrations" though. September/October sounds too late for peak emissions, so it may be that revising the ebullition was necessary. But the TM4-4dvar still looks significantly earlier. Also, the resulting flux is not 10% lower than the original LPJ model - peak values are about 1/2. Why? What is the yearly integrated total emissions for the revised LPJ model?
*Changed "observed concentrations" with "the emissions inferred from observations by inversion". The flux is indeed **not** 10% lower. The carbon conversion rate was reduced **from** 20% (SC2) **to** 10% and total emissions were reduced from 38.6 (Table 2) to 25.6 Tg/yr in 2004. The sentence was clarified accordingly.*

P241, L19-22 - After pointing out that the two inversions having different maximum emissions timing, it's not correct to say the new model agrees with both inversions!
Yes, we should say that the revised LPJ agrees better with the KNMI inversion result. Text changed.

P241, L19-22 - why wasn't the new LPJ calculation used as priors for the inversions? It would be interesting to see how things change.
Of course one could redo more iterations between LPJ and the atmospheric inversion system and analyse further changes. We decided to stop the iteration after revising the original biogeochemical model, because atmospheric inversions are computationally expensive and we do not expect that another iteration would offer much information beyond what has been found here. We would expect that the inversion would keep the peatland emissions close to the prior and again put most of its energy into changing shifting anthropogenic towards domestic ruminants..

P241-242 - Why was the TM5 inversion chosen for the scaling? What criteria were used to find that it is a better inversion?
The TM5 inversion result was used for scaling the NH peatland emissions simply because the LMDz inversion does not differentiate between emission types within the inversion. Only the timing of LMDz optimised peatland emissions could be extracted from peatland grid cells that had no other source type attributed. This is described in the Appendix A1.3.

P242, L16 - I think "interannual variability" should be "anomaly" here.
Text changed.

P242, L20 - Instead of "climate change", "climate variability" should be used.
Text changed.

P242, L23-26 - this is quite an interesting result in my opinion.
Thanks.
P244, L21 - Should "soil source" be "natural source"?
No, since natural sources would include e.g. geologic & oceanic emissions or emissions by termite, which is not the case.

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