Biogeosciences Discuss., 8, C403–C408, 2011
www.biogeosciences-discuss.net/8/C403/2011/

Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 1 April 2011

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. 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! 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.

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.

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.

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.

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.

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....

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.

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!). 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?

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

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.

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

P237, L3 - I don't understand what is meant by satisfying the "regional magnitude of flux rates".

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

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

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.

P237, L20 - I'm not sure whether the 5-15 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).

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

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

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

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?

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

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!

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.

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?

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?

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.

P240, L19 - The constraint is imposed by the observations, not TM5-4dvar (which itself is constrained by obs).

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. Also, if you integrate over the whole year, do you get the same totals?

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?

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!

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.

P241-242 - Why was the TM5 inversion chosen for the scaling? What criteria were used to find that it is a better inversion?

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

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

P242, L23-26 - this is quite an interesting result in my opinion.

P244, L21 - Should "soil source" be "natural source"?

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