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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On the ability of a global atmospheric inversion to constrain variations of CO_2 fluxes over Amazonia" *by* L. Molina et al.

Anonymous Referee #2

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This study attempts to examine the seasonal and interannual variations of NEE over Amazonia via a top-down approach. Using the MACC project as a baseline, the study added four more surface stations to the observational network and compared the resultant flux estimates. The authors also compared their estimates to those obtained from a bottom-up study in order to isolate the value of: (a) global inversions to constrain fluxes over Amazonia, and (b) additional information from the four surface sites that were not used in the MACC project. Results are disappointing, however, in the sense that these four surface sites added modest positive information, and in certain instances seemingly degraded the quality of the flux estimates (see General Comment #5). It is unclear whether this is due to an inherent limitation of global inversion frameworks, due to artifacts with the specific inversion framework used in this study,

or combination of both. Neither the methodological framework nor the overall conclusions (i.e., challenge associated with teasing out subtle regional signals from a global coarse-resolution inversion) are new. While the paper may be acceptable for publication in ACP (as part of the special issue), I would strongly recommend that the authors incorporate a discussion on the uncertainties associated with their flux estimates (see #1 below). This would make the study, and the overall findings, more robust and valuable to the community.

General Comments:

1) My biggest disappointment is that no attempt has been made to provide posterior uncertainty estimates, which makes the study incomplete. The authors sidestep the calculation of uncertainties due to the computational expense (Page 1922, Lines 25-27); presumably because for the variational approach a Monte-Carlo algorithm has to be implemented (e.g., Chevallier et al. [2007], JGR-A, doi:10.1029/2006JD007375). But any attempt to reconcile the top-down and bottom-up estimates cannot be assessed when we do not know whether the differences between the two sets of estimates are significant or not. At a minimum, do the simulated observations from INVSAm capture the assimilated observations within 95% of their confidence intervals? Error bounds will also allow better judging the performance in Figures 6 and 9. Hence, I would strongly encourage the authors to reconsider their decision to skip the calculation of these posterior uncertainties.

2) The lack of discussion on uncertainties is also related to choices that have been made about the prior covariance. Why did the authors persist with using correlations in B that are based on data from towers in the Northern Hemisphere? Are there alternatives to the Chevallier et al. [2006] approach that the authors could have used to determine a more suitable B for the study region? Even though this study solves for global fluxes, the use of correlations that are appropriate for the Amazon basin seems necessary. Can the authors comment on their choice?

3) How likely is it that the dipole issue (Figure 8, also Page 1932, Lines 5-12) is related to the spatial correlations that have been pre-specified in B? In fact in Lines 10-12, the authors seem to question their own choice of B. In order to completely investigate this dipole issue, the authors may need to look at the ocean fluxes. As the focus of this study is on the land component, I agree with the decision of the authors to skip any discussion on the ocean fluxes (Page 1924, Line 4). But in light of the dipole issue as well as the negative results, it may be worthwhile to add as supplementary material a discussion on the ocean fluxes; for example, even a spatially-aggregated evaluation with respect to the MACCv10.1 (or CH2010) product may provide some insights on the performance of the inversion system.

4) Page 1934, Lines 18-20: The authors state – "…the inversion system may have applied corrections in response to events registered by only a single station at a time". I am not sure what the authors mean here. Do the authors imply that even though observations from a particular site were available for a few years, it negatively impacted the analyses over other time periods? Based on my understanding, in the variational system the analysis window spanned the full period from 2002-2010. If so, did the authors consider breaking up the analysis window into smaller time-chunks, for example, 2 or 3 year periods with overlapping 2-3 months in between?

5) Figure 10, Panel b: For 2003, the annual NEE anomalies in Zone 2 are extremely counter-intuitive. What causes the difference in sign of the anomalies, i.e., negative anomalies from INVSAm but positive anomalies from MACCv10.1 (or CH2010)? If we use the J2011 as a baseline (ignoring the magnitude and only looking at the sign of the NEE anomaly), then the INVSAm anomaly is likely inaccurate. For Zone 2, a plausible cause of the difference between INVSAm and MACCv10.1 is due to the assimilation of data from the SAN site. But again based on the limited footprint information (Figure 3), the observations at SAN may not be sensitive to Zone 2 fluxes. Hence if there are no useful information in the SAN observations to constrain Zone 2, shouldn't the INVSAm fluxes and thereby the anomalies be of similar sign and magnitude to the MACCv10.1

and/or close to the prior flux estimates?

Specific/Technical Comments:

1) Page 1917, Lines 9-13: Consider rephrasing this sentence. The only comparison presented in this paper is to Jung et al. [2011]; but this statement gives the impression that the authors have looked at a suite of bottom-up modeling reports, and compared their top-down estimates to these bottom-up estimates.

2) Abstract: The authors should mention at the outset the time period/duration over which fluxes are being estimated, i.e., 2002-2010. The reader does not get this information till the end of the Introduction.

3) Page 1918, Line 4: Change from "…is the topic of active research" to "…a topic of active research".

4) Page 1919, Line 16: There is an extra ')' after the word emissions. Delete.

5) Page 1921, Line 13-14: It is unclear what the authors mean by –"…the reliability of these modeled fluxes should be analyzed".

6) Page 1921, Line 22: Replace the word 'were' with 'where'.

7) Page 1922, Line 9: Replace the word 'henceforward' with 'hereafter'

8) Page 1926, Line 17: Do the authors mean "spatial and temporal variability", or only "temporal variability"? Kindly clarify.

9) Page 1926, Line 18: It is unclear what the authors mean by "root mean square of the annual biases". How is this quantity calculated? In fact the entire discussion about the "flat prior" or the poor man's prior is difficult to follow. The authors may want to revise this piece, and make it a separate paragraph (for e.g., paragraph break at Line 9).

10) Section 3: Throughout the text the authors mention MACCv10.1 but in the figures, the results are presented as CH2010. This is highly confusing. It is better to stick with

MACCv10.1 in both the text and the figures, and use CH2010 to specifically refer to a conclusion/finding from that study.

11) Page 1931, Lines 23-24: Consider rephrasing part of this sentence as – "…not shown here since these did not provide further information than presented in Figures 6g, 6h".

12) Page 1931, Lines 27-28: It should be clarified here that this is an expected outcome, given that there are no observations to constrain the fluxes in this region.

13) Page 1933, Lines 11-12: It is not clear why there is a difference in magnitude between the NEE anomaly estimates from this study, and those from J2011. The authors need to comment on this discrepancy.

14) Figure 3: Is there a specific reason for showing the footprints only for February? Are these footprints typical of the entire year?

15) Figure 4, Panel a: In 2009, the simulated mole fractions from MACCv10.1 (or CH2010) seem to fit the observations better than INVSAm. This is also true for early-2007 period. Differences are as large as 10-15 ppm. Can the authors comment on the reason(s) for the poor performance of INVSAm?

16) Figure 4, Panel c: Again over periods in 2002-2003, the INVSAm estimates are closer to the prior (and farther from the observations) than MACCv10.1 (or CH2010). It is very discouraging that using the observations from the site degrades the result. The authors need to discuss/clarify this in the text.

17) Figure 8: Have the authors looked at the corresponding figures from MACCv10.1 (or CH2010)? If so, it would be worthwhile to add a second column to this figure showing those results.

18) Figure 9, panel b: Change the scale on the y-axis (for e.g., -0.15 to 0.15). Currently this figure cannot be evaluated.

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