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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.

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Interactive comment on "On the ability of a global atmospheric inversion to constrain variations of CO2 fluxes over Amazonia" by L. Molina et al. Anonymous Referee #2 Received and published: 24 February 2015 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). A) We thank the reviewer for his acute comments and sensible suggestions, which strongly help improving the analyses and discussions of our results. We hope that our answers to his comments demonstrate that we will strengthen these analyses and discussions. 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. A) It is definitely difficult to distinguish between the limitations that are inherent to the specific global inversion system we use and those that are universal. However, the analyses in the next version of the manuscript should help strengthen the characterization of the limitations that are inherent to the existing in situ ground based network. The lack of information to improve the regional configuration of the inversion parameters such as the prior error covariance matrix and the observation error covariance matrix in Amazonia will also be better discussed in relation to the following comments of the reviewer. 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. A) Still, our attempt at analyzing results from global inversions at a high resolution over Amazonia in such details, and the analyses of the impact of the assimilation of regional measurements that have been barely (never for some of them) used previously, is new. Some conclusions are directly connected to these specific aspects of the study. 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. A) We will include a discussion on the uncertainties that follows the answers given below to the comments of the reviewer on that specific topic. The results and discussions of our manuscript actually highlight our limited confidence in the inversion configuration for the Amazonian area.Such a limited confidenceand the huge computational cost associated to the posterior uncertainty estimates make such an exercise quite vain in the context of this study, as explained below. 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). A) Yes, this is the case and it will be clarified in the manuscript. Of note is also that, in general, such Monte Carlo experiments are conducted for a typical year only, due to their huge cost. However, here, in order to assess the impact of the South American sites which have a weak overlapping in time, such experiments would have to be conducted for at least 4 different years and for the two MACCv10.1 and INVSAm configurations. Actually, since this study focuses on mean seasonal cycles and inter-annual variations, the Monte Carlo computations should be conducted for an even larger number of years. We should also mention that this request for computationally intensive Monte Carlo simulations is the drawback of solving for the fluxes at the weekly and transport grid scale. A coarser resolution inversion system may have provided posterior error estimates much more easily. However, it would have been more difficult to investigate the spatial variability of the fluxes within Amazonia and to avoid aggregation errors (which likely already hamper the results in this study) with such a coarser system. 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. A) The analysis of the increments from INVSAm vs. those from MACCv10.1(see the new figure 8 below and figures 6 and 9) demonstrates that the impact of the South American sites is high (at the transport grid scale, the increments from INVSAM to the annual fluxes generally exceed 150% of the prior estimate in terms of absolute values). Large increments from the inversion indicate that the theoretical uncertainty reduction is highprovided that the error statistics assigned in the inversion system are consistent with the actual errors.In that sense, the impact of the South American sites should be significant. The computation of theoretical uncertainty would not bring much more information about the significance of the impact of the South American stations given the modest confidence that we have in the error statistics for the Amazonian area, as explained in the answer to the second major comment of the reviewer. This will be discussed in the new manuscript.

At a minimum, do the simulated observations from INVSAm capture the assimilated observations within 95% of their confidence intervals? A) Table A.1 below compares the standard deviations of the hourly prior and posterior misfits between the simulations and the observation, and the ∼95% confidence interval (actually two standard deviations) of the configuration of the observation errors (for hourly observations) in the inversion system (following section 2.1). The prior misfits are well larger than our observation errors at ABP, MAX, and GUY which make the prior simulation lie outside the 95% confidence interval of the observation error except at SAN (where prior misfits are still slightly larger than the observation error). Misfits between MACCv10.1 and the observations are similar to the prior misfits at SAN and GUY and well smaller than the prior misfits at the coastal ABP and MAX sites which could be related to a very large scale improvement of the fluxes in the Southern hemisphere.The corrections from MACCv10.1 thus make the posterior simulation fall within the 95% confidence interval of the observation error at all the sites but GUY. When assimilating the data from the South American sites, misfits are decreased compared to both the prior and MACCv10.1 at all sites. The INVSAm posterior simulation still lies in the 95% level interval of the observation error at ABP, MAX, and SAN and nearly reaches the threshold at GUY. It is close to the 68% confidence interval at MAX and in this interval at SAN while it was not the case for MACCv10.1. This and the high increments (interms of relative difference to the prior fluxes, figure A.6) applied to the fluxes in South America both in MACCv10.1 and when adding South American stations lead us to consider that the corrections from the inversion are significant even though we do not have the means for deriving the actual statistical significance. We will discuss this in the new manuscript. Table A.1 Standard deviation of the misfits Model – Observation Station

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Prior INVSAm MACCv10.1 2 * (Standard deviation of the model error) ABP 4.4 1.5 1.6 2.2 MAX 2.1 1.1 1.5 2.0 SAN 4.9 4.5 4.9 9.6 GUY 4.0 3.5 4.1 3.3

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. A) As explained above, deriving theoretical uncertainties for the mean seasonal cycle and the inter-annual anomalies is not affordable in the framework of this study. Furthermore, as detailed in the answer to the reviewer's second comment, such theoretical numbers are not critical for judging the performances of the system.Even though we prefer not launching such computations of the theoretical uncertainties, we intend to discuss this topic better in the manuscript following our answers to the reviewer. 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? A) The reviewer is right about the fact that some lack of confidence in the configuration of the prior and observation error covariance for the limited and specific area on which this study focuses is an important explanation why we think that the computation of theoretical uncertainties would not be useful while highly expensive.A reliable estimate of the posterior uncertainties and uncertainty reductions strongly depends on the reliability of the description of prior and observation errors in the configuration of the inversion system. The statistics of B are based not only on results from Chevallier et al. 2006 but also on that from Chevallier et al. 2012 which made use of available eddy covariance sites in south America (see the figure 1 in Chevallier et al. (2012), GBC, doi:10.1029/2010GB003974). We believe that the use of eddy covariance flux measurements is presently the best way to assess the statistics of the prior uncertainties at the time and space scales for which the B matrices need to be setup. Some computations of the standard deviation of misfits between ORCHIDEE and eddy covariance measurements in South America indicated that the configuration of the standard deviation of the prior uncertainty at the weekly scale was robust forthis continent as well as for others. However, the small number of eddy covariance measurement sites in South America prevented us from deriving spatial correlations specifically for this continent. This explains why we used in South America the scales derived using the global eddy covariance dataset, that is strongly biased by the higher number of sites in the Northern hemisphere. Furthermore, the method used to model the observation error in CH2010 and in our study has been developed and evaluated based on analysisof model data comparisons using mainlyatmospheric data from the mid latitudes in the Northern hemisphere (due to the limited coverage of other areas).Specific sources of transport modeling errors in Amazonia (Parazoo et al., Atmos. Chem. Phys., 8, 7239–7254, 2008), such as the deep convection, may not be well reflected by the computation proposed by CH2010. Finally, the configuration of the prior and observation error covariances in MACCv10.1, as often in global inversion systems, have been evaluated at the very large spaces scales which are the primary targets of such global inversion systems. Focusing on Amazonia, and even on some specific sub-areas of this region questions the reliability of this configuration when analyzing finer scales, and in particular the use of an isotropic and homogeneous correlation modeling. The analysis and discussion of our results with real data suggested little confidence on these statistics for Amazonia.It leads us to think that the theoretical computations of the uncertainty reduction would not bring more insights about the reliability of the increments from MACCv10.1 and INVSAm. We will conduct deeper discussions on this topic in the new manuscript. 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. A) The answer to the second major comment from the reviewer above gives more details about the lack of confidence in B over Amazonia. However, regarding the dipole, it seems to be mainly drivenby a large scale behavior of the inversion connected to the atmospheric transport rather than by the B matrix, as demonstrated by the increments to the ocean fluxes. We will now discuss this in the manuscript as suggested by the reviewer (but in the main text when discussing the dipole rather than in a supplementary material).Our original text on the dipole could have been misleading regarding the role of B in the dipole and the corresponding part will be corrected. Figure 8 will be replaced by the figure below, which depicts corrections for both the ocean and land fluxes (with different color scales and units due to the different order of magnitude between increments over land and ocean) and over an area larger than that shown originally. Based on this figure, the paper will explain that the increments from both the inversions have large patterns which are nearly zonal (or along the prevailing winds) and which overlap continuously the ocean and the land. This continuity, and the fact that in the B matrix there is no correlation between the land and the ocean, demonstrate that the dipole is not mainly driven by the structure of B. Actually, the dipole opposes different zonal bands rather than some ocean areas vs. some land areas. The zonal positions and strength (i.e. the amplitude of the dipole or of the zonal gradient) of these zonal increments are modified by the inclusion in the inversion of the data from the new stations in the Tropical South America region. These effects are more visible when focusing on specific months, while the annual averages smoothens the patterns.

New Fig. 8. Spatial distribution of 2002–2010 mean ïňĆux corrections at the transport model resolution (3.75âŮę × 2.50âŮę) to ORCHIDEE from (left) INVSAm and (right) MACCv10.1 over a larger area encompassing TSA: mean for February, July, and mean over the full period 2002–2010. Flux increments over land and ocean are represented with two distinct color scales and units: greenâĂŤyellow for land, in gC m-2 hr-1; blueâĂŤred for ocean, in mgC m-2 hr-1. Filled circles indicate locations of sites with continuous measurements; and open circles indicate locations of sites with discrete air sampling. 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? A) Our sentence was a bit confusing and will be simplified.Corrections applied in response to a specific event at a given site should not spread in time to such an extent that it would impact the results during years when there is no data available at this site, and we do not think that we should verify it by conducting inversions on 2-3 year periods (however, see the analysis of the results for 4-5 year periods in answer to the reviewer 1, in figure S.1, which helps isolate the impact of the different sites; see also the results for the year 2003 when SAN data only were available in answer to the general comment 5 of the reviewer). Still, these specific corrections would have less weight in the average increments in the area if the data availability was higher. We confusingly made a shortcut between giving more weight to a short term event in the mean corrections and applying mean corrections in answer to such short term events. We will better discuss this topic based on the answers to the reviewer 1 and to the fifth comment of reviewer 2. 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? A) The anomaly for a given year can actually be modified by increments during other years given that the posterior annual anomalies are calculated against the posterior average of the NEE during 2002-2010. Furthermore, figure A.7 (showing the inversion increments in 2003) below demonstrates that while MACCv10.1 applies positive increments in zone 2 in 2003, INVSAm applies negative increments due to the assimilation of SAN data. Since, on average over 2002-2010, both inversions apply positive increments in this zone (see figure 8) this leads to a clear negative anomaly in zone 2 for INVSAm. The discussion on the dipole and on its zonal structure indicates that the footprint of the sites needs to be considered entirely, i.e. that the inversion strongly uses the parts of these footprints where the values of sensitivity are relatively low to apply long range corrections. Corrections in zone 2 in INVSAm could be driven by remote measurements and by their difference to SAN data. This corresponds to the amplification and displacement of the zonal dipole discussed in answer to the major comment 3 from the reviewer and that we also observe in 2003 as indicated by figure A.6. The anomaly in 2003 for INVSAm can thus be considered as an artifact from the limited data availability in South America. This will be discussed in the paper. The comparison to J2011 is delicate since J2011exhibits too little interannual variability for regionTSA, which bears substantial uncertainties (we comment on this again in answer to the technical comment #13, and better clarify it in the manuscript).

Fig. A.5 Spatial distribution of mean flux corrections in 2003 at the transport model resolution (3.75° × 2.50°) to ORCHIDEE from (left column) INVSAm and (right column) MACCv10.1 over the study region. Mean for February (top), July (middle), and mean over the whole year (bottom). Filled circles indicate locations of sites with continuous measurements; and open circles indicate locations of sites with discrete air sampling.

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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. A) We will reformulate the sentence to avoid the plural, and to clarify the fact that the confrontation to the scientific literature on fluxes in Amazonia will be qualitative only. 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. A) We will clarify it in the abstract. 3) Page 1918, Line 4: Change from ". . .is the topic of active research" to ". . .a topic of active research". A) It will be done 4) Page 1919, Line 16: There is an extra ')' after the word emissions. Delete. A) It will be done. 5) Page 1921, Line 13-14: It is unclear what the authors mean by –". . .the reliability of these modeled fluxes should be analyzed". A) We will rephrase the sentence. 6) Page 1921, Line 22: Replace the word 'were' with 'where'. A) It will be done. 7) Page 1922, Line 9: Replace the word 'henceforward' with 'hereafter' A) It will be done. 8) Page 1926, Line 17: Do the authors mean "spatial and temporal variability", or only "temporal variability"? Kindly clarify. A) We meant both spatial variability in one hand, and temporal variability at scales smaller than a year on the other hand. We will rewrite the sentence to clarify it. 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). A) At each site we calculated the quadratic mean of the annual differences in the CO2 budget predicted from the two sets of priors. The "flat prior" does not exhibit interannual variability over the simulation period, but seasonal and spatial variations from the reference prior are preserved when deriving the flat prior. As the reviewer suggests, this derivation of the flat prior will be better explained in a separate paragraph. 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. A) It will be done as suggested. 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". A) We will rephrase the sentence as suggested. 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. A) It will be clarified in the manuscript. 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. A) J2011 comment on the performace of their method to predict interannual variability of NEE. The differences arise from their underestimation of the terrestrial ecosystem respiration (TER), likely due to the lack information on carbon pools and soil conditions, and theappropriate predictors of TER, which according to their results, is strongly related to NEE particularly over the Amazon basin. Also their estimate bares the likely bias of a limited number of FLUXNET towers available over Amazonia (Fig. 2 in Jung et al., 2009, Biogeosciences, 6, 2001-2013. doi:10.5194/bg-6-2001-2009) located in sites with carbon pools not in equilibrium. This will be commented in the text. 14) Figure 3: Is there a specific reason for showing the footprints only for February? Are these footprints typical of the entire year? A) The seasonal changes in the atmospheric circulation in TSA are not critical in general. There are illustrated in figure S.1 (below). The figure depicts a climatologicallyof wind fields from NCEP/NCAR reanalysis (2981âĂŤ2010), averaged between the surface and a level of 600 hPa, in TSA during (a) the austral summer (February), (b) austral winter (July), and (c) annual mean. Across the Amazon Basin, the dominant, or typical, circulation pattern in the lower troposphere is that of winds entering the Atlantic coast in north-eastern Brazil, then continue across the basin, and as they approach the Andes, turn back into the Atlantic Ocean south of 20°S. This will be better commented in the text. We will also include the figure below in the supplementary material. a) b) c) Fig.S.1. Long-term

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mean wind fields (1981âĂŤ2010), averaged between the surface and a level of 600 hPa for (a) February, (b) July, and (c) annual mean. Data obtained from NCEP/NCAR Reanalysis.

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? A) We made a mistake when sampling the posterior concentration field. The results are now more consistent with the expected results from the assimilation of data at ABP. The figure below will replace Fig. 4 panel a. The corresponding statistics of the misfits between measurements and simulated mole fractions will also be updated in Fig. 5 (below).

New Fig. 4 panel a. Comparison of assimilated CO2 observations (blue) and corresponding simulated mole fractions using prior fluxes (red), INVSAm (green) and MACCv10.1 (purple). Measurements were collected at Arembepe. Data shown here correspond to daily average mole fractions between 12:00 and 15:00 local time (LT), when wind speed > 2 m s−1.

New Fig. 5.Taylor diagram of the statistics of misfits between observations and simulated CO2 mole fractions between 12:00 and 15:00 LT at Guyaflux (square), Santarém (circle), Arembepe (diamond) and Maxaranguape (triangle), when wind speed > 2 m s−1 , using prior fluxes (red), INVSAm (green) and CH2010 (purple). Radial distance from the origin: ratio of SD of simulated mole fractions and SD of the observations. Angle measured from the y axis: coefficient of correlation. Numbers next to the symbols: bias (in ppm). Gray circles: SD of the misfits (in ppm).

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. A) We will discuss this in the manuscript.

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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. A) We will replace the previous Figure 8, shown in the answer to General Comment #3. The new Fig. 8includes the results from MACCv.10.1, as well as the increments over the ocean surrounding the TSA region.We will comment on their comparison (see, in particular, the discussions in answer to the first and third general comments of the reviewer) 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. A) It will be done.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C2169/2015/acpd-15-C2169-2015-supplement.pdf

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