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Atmospheric Chemistry and Physics Discussions

ACPD

15, C3684–C3708, 2015

Interactive Comment

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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Final response to the comments from Referee 2

The full text has also been provided as a PDF file in the supplementary material.

Questions/comments from the Referee, answers to the comments and changes to the manuscript are presented according with the following notation:

Q) Questions, general, and technical comments A) Answers to the comments C) Changes to the manuscript

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Interactive Discussion

Discussion Paper

Q) 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 helped improving the analyses and discussions of our results. We hope that our answers to his comments demonstrate that we have strengthened these analyses and discussions.

Q) It is unclear whether this is due to an inherent limitation of global inversion frameworks, due to artefacts 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 new analyses in the revised manuscript help characterizing 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 is now better discussed in relation to the General/Technical Comments of the reviewer.

Q) 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.

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A) Still, our attempt at analyzing results from global inversions at high resolution over Amazonia in such detail, 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.

Q) 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 have included a discussion on the uncertainties in Sect. 2.1 (see response to General Comment Q.2) and on the significance of our results in Sect. 3.1 (response to General Comment Q.1.3) and Sect. 3.2 (response to General Comment Q.3) that follows the answers given below to the comments of the reviewer on those specific topics.

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General Comments:

Q.1.1) 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 is clarified in the manuscript (see also our answer to General Comment Q.2 from Referee #2). Of note is also that, in general, such Monte Carlo experiments are conducted for a typical year only, due to their huge computational 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 had to be conducted for at least 4 different years and for the two MACCv10.1 and INVSAm configurations. Actu-

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ally, since this study focuses on mean seasonal cycles and inter-annual variations, the Monte Carlo computations would have had to 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.

Q.1.2) 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 response to General Comment Q.3 of Referee #2, and figures 7 and 9 in the revised manuscript) 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 high provided 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 is now discussed in a new section in the revised manuscript.

Q.1.3) At a minimum, do the simulated observations from INVSAm capture the assimilated observations within 95% of their confidence intervals?

A) Table A1 (provided as supplementary material) compares the standard deviations

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of the prior and posterior misfits between the simulations and the observation, and the ∼95% confidence interval (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 much larger than our observation errors at ABP, MAX, and GUY which makes 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 much smaller than the prior misfits at the coastal sites ABP and MAX, 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 within this interval at SAN, while it was not the case for MACCv10.1. This and the high increments (in terms of relative difference to the prior fluxes) 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 discuss this in Sect. 3.1 of the revised manuscript.

C) In the revised manuscript, Sect. 3.1, we include the following discussion:

“The significance of the reduction of the misfits between the mole fractions observed and simulated from the inversion is seen from the comparison between the standard deviations of these misfits and the estimate of the standard deviation of the observation errors (i.e. of the transport model errors) for hourly values in the configuration of the R matrix (Table A1, in supplementary material).According to this comparison, the prior misfits are much larger than the observation errors at ABP, MAX, and GUY, but are

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slightly smaller than these at SAN. Misfits between MACCv10.1 and the observations are similar to the prior misfits at SAN and GUY and are much smaller than the prior misfits (and smaller than the 95% confidence interval of the observations) at the coastal ABP and MAX sites. Misfits are further decreased when assimilating the data from the South American sites: they are about the standard deviation of the observation errors at all sites but GUY (where they are twice as large)."

Q.1.4) 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 (see our answer to General Comment Q.1.1 from Referee #2). Furthermore, as detailed in the answer to the reviewer's General Comment Q.1.2, such theoretical numbers are not critical for judging the performance of the system. Even though we prefer not to launch such computations of the theoretical uncertainties, we discuss better this topic in the revised manuscript, based on our answers to the reviewer.

Q.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 uncertainty and uncertainty reduction 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 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 for this 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, which 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 analysis of model data comparisons using mainly atmospheric data from the mid latitudes in the Northern Hemisphere (due to the limited coverage of other areas). Specific sources of transport modelling 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 is often the case in global inversion systems, have been evaluated at very large spatial scales, which are the primary target 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 modelling. The analysis and discussion of our results with real data suggested little confidence on these statistics for Amazonia. This leads us to think that the theoretical computations

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of the uncertainty reduction would not bring more insights about the reliability of the increments from MACCv10.1 and INVSAm. We now discuss this topic in the revised manuscript.

C) In the revised manuscript, Sect. 2.1, we comment:

"There is a moderate confidence in the adequacy of these error statistics assigned in the global inversion system for the specific TSA area studied here, both because B was designed mostly with statistics gathered in the Northern Hemisphere, and because R may not well account for the uncertainty in the atmospheric convection model, while this could be high in Amazonia (Parazoo et al., 2008). We also investigate here variations of the fluxes within TSA at spatial scales that are not much larger than the e-folding correlation length in B, and these variations in the inversion results may be affected by our simple hypothesis of isotropic correlations in the prior uncertainty. This lack of confidence in the input error statistics weakens our confidence in the posterior error statistics that can be derived based on the inversion system, even though they may be realistic at zonal scale for the Tropics (Chevallier and O'Dell, 2013). In this context, and given the relatively high computational burden of the posterior uncertainty computations for grid-point inversion systems (using Monte Carlo approaches with ensembles of inversions, Chevallier et al., 2007), we do not derive these posterior uncertainties for our domain and its sub-domains."

Q.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

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insights on the performance of the inversion system.

A) The answer to General Comment Q.2 from the reviewer gives more details about the lack of confidence in B over Amazonia. However, regarding the dipole, it seems to be mainly driven by a large-scale behaviour of the inversion connected to the atmospheric transport rather than by the B matrix, as demonstrated by the increments to the ocean fluxes. We comment this in the revised manuscript. Our original discussion on the dipole could have been misleading regarding the role of B in the dipole and has been reformulated in the new section 3.2. Previous Fig. 8 has been updated (new Fig. 6 in the revised manuscript) and now depicts corrections for both the ocean and land fluxes (with different colour 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 manuscript now explains that the increments from both 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. This is commented in the new Sect. 3.2.

C) A new section, “3.2 Characterization of the monthly to annual mean inversion increments to the prior fluxes” has been included in the manuscript. In this section we state:

“Figure 6 depicts the increments from both inversions, showing large patterns which are nearly zonal (or along the prevailing winds) and which overlap continuously over land and ocean. Since there is no correlation between the uncertainty in ocean and

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land fluxes in the B matrix, and given the typical length scale of the correlations in this matrix, this can be directly connected to the signature of atmospheric transport. The contiguous zonal patterns have alternate negative and positive flux increments. There is thus an opposition between corrections in the North and in the South of the TSA region. These corrections are rather negative in the North and positive in the South (positive in the North and negative in the South) during the austral summer (winter). As these corrections are stronger during the austral winter, it results in positive (negative) corrections in the North (South) at the annual scale. Such dipoles are a typical behaviour of inverse modelling systems in data-poor regions (Peylin et al., 2002). However, changes in the amplitude and latitudinal position of this zonal dipole appear to be the main impact from the assimilation of data in the TSA region. This dipole structure may thus yield sensible corrections to the NEE in the TSA area. The dipole has a high amplitude for MACCv10.1, and even higher for INVSAm. The increments from INVSAm to the annual fluxes often exceed 150% of the prior estimate in terms of absolute values. The highest increments are obtained during austral winter and when the SAN data are available (during the period 2002-2005, see Fig. S1), which is in line with the fact that this site is located more inland than the others. Such high control of the data in the TSA region (even when checking the SAN and MAX, or the MAX, ABP and GUY datasets only) over the zonal patterns of flux corrections also highlights the very large-extent impact of these data, and of the data in the southern hemisphere in general, despite the relatively small spatial correlation length scales in the B matrix, and the limited area in which the station footprints are very high. The inversion also generates patterns of corrections of smaller spatial scale close to the measurement sites in the TSA region when these sites are used by the inversion. This raises hope that the NEE over the whole TSA region is strongly constrained by the observations, but can also raise questions regarding the spatial variations of the corrections applied by the inversion to the NEE within the TSA region, at least when considering areas at more than 500 km from the measurement sites. However, various pieces of evidence (Fig. 5 and 6, the analysis of the decrease in misfits to the observations from the in-

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version in section 3.1, and the previous analysis of the high increments to the monthly mean and annual mean NEE over the entire TSA region) indicate that the corrections from the inversion are significant."

Q.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 statement was a bit confusing and has been reformulated. 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 Referee #1, in figure S1, 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 Q.5 of Referee #2). 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. In the revised manuscript we discuss this topic based on the answers to the Referee #1 and to the General Comment Q.5.

C) Lines 11-20, p1934, of the original manuscript have been rewritten. The original statement above has been reformulated as follows:

"The limited overlap among the TSA observations is a critical issue since measure-

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ments are often only available at a single site at once, and consequently, temporary model errors at this site can get far more weight in the inversion than if it had been balanced by information from other sites."

Q.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. This is now clarified in the revised manuscript. Furthermore, figure A.6 (showing the inversion increments in 2003) 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 (cf. new Fig. 6) this leads to a clear negative anomaly in zone 2 for INVSAm. The discussion on the dipole (cf. answer to General Comment Q.3) 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 measurement sites and by their difference to SAN data. This corresponds to the amplification and displacement of the zonal dipole discussed in answer to the General Comment Q.3

and which we also observe in 2003 as indicated by Fig. A.6. The anomaly in 2003 for INVSAm can thus be considered as an artefact from the limited data availability in South America. This is discussed in the revised manuscript. The comparison to J2011 is delicate since J2011 exhibits too little interannual variability for region TSA and bears substantial uncertainties (see answer to the Technical Comment Q.T13).

C) In Sect. 3.3.2 of the revised manuscript we state:

"Of note is that even if increments on the NEE annual budget of a given year from an inversion are weak, the changes in the corresponding annual anomaly from the inversion can be high because the inversion modifies the 2002-2010 average against which the anomaly is computed."

C) Also in Sect. 3.3.2, we state:

"The example of the divergences of the results between MACCv10.1 and INVSAm in 2003 in Zone 2 illustrates, again, some weak ability to precisely constrain the fluxes in such a small area, which is quite distant from the measurement sites in TSA. Indeed, the analysis of the maps of increments from MACCv10.1 and INVSAm, for the annual mean NEE in 2003 (not shown), demonstrates that the assimilation of data at SAN during this year shifts the northern border of the pattern of negative corrections in MACCv10.1 from North of Zone 2 to the south of Zone 2.Since, on average, over 2002-2010, both inversions apply positive increments in this Zone (see Fig.6) this leads to a clear negative annual anomaly in Zone 2 and for the year 2003 for INVSAm."

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Specific/Technical Comments:

Q.T1) 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 modelling reports, and compared their top-down estimates to these bottom-up estimates.

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C) The text has been reformulated as follows:

“The estimates of net ecosystem exchange (NEE) optimized by the inversion were compared to an independent estimate of NEE upscaled from eddy-covariance flux measurements in Amazonia. They were also qualitatively evaluated against reports on the seasonal and interannual variations of the land sink in South America from the scientific literature.”

Q.T2) 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 now specify the analysis period in the abstract.

Q.T3) Page 1918, Line 4: Change from “...is the topic of active research” to “...a topic of active research”.

A) We have incorporated the suggested change.

Q.T4) Page 1919, Line 16: There is an extra ‘)’ after the word emissions. Delete.

A) We have corrected the error.

Q.T5) Page 1921, Line 13-14: It is unclear what the authors mean by –“...the reliability of these modelled fluxes should be analyzed”.

A) The text has been reformulated based on our answers to the reviewer and the referred sentence has been suppressed.

Q.T6) Page 1921, Line 22: Replace the word ‘were’ with ‘where’.

A) The correction has been made. In addition, as suggested by Referee 1, the paragraph in lines 15-23, p1921 in the original manuscript, regarding the description of the product J2011, has been moved from this section to a new section: “Sect. 2.3 Analysis of an alternative estimate of the NEE for the evaluation of the inversions.”

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Q.T7) Page 1922, Line 9: Replace the word ‘henceforward’ with ‘hereafter’

A) The word has been replaced.

Q.T8) Page 1926, Line 17: Do the authors mean “spatial and temporal variability”, or only “temporal variability”? Kindly clarify.

A) This phrase has suppressed since the text in lines 9-24 in the original manuscript has been removed. We concluded that this part of the text was not clear for the reader and we decided to convey the message through the discussion in sections 3.1 and 3.2

Q.T9) 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) The phrase has been suppressed. See previous Technical Comment (Q.T8).

Q.T10) 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) We have systematically changed the references in the figures and in the text as suggested.

Q.T11) 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”.

C) The sentence has been reformulated as follows:

“The results, however, do not provide any further information than Fig. 7c,d and are not shown.”

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Q.T12) 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.

C) In the revised manuscript, we have added:

“This is an expected result due to insufficient data in the southern part of the TSA to constrain fluxes in that region.”

Q.T13) 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) Based on the comparison of the gross primary productivity (GPP) simulated by 10 process-based models and the GPP estimated by Jung et al. (2011), Piao et al. (2013), Glob. Chang. Biol., doi:10.1111/gcb.12187, comment on the likely underestimation of the interannual variability of GPP by Jung et al. (2011): Jung et al. (2011) use spatial gradients among the available flux towers to train their algorithm. The derived relationships are then extrapolated to temporal gradients. However, this supposes that spatial and temporal response of GPP to climate is the same, which might not be the case.

C) In the revised manuscript we comment:

“However, the product of J2011 must be used cautiously, especially when evaluating IAV of NEE. J2011 relied on a limited number of EC stations across the Amazon basin, with short time series, to estimate MTE based on spatial gradients among the sites, and then extrapolated to temporal gradients. This is valid assuming that spatial and temporal NEE patterns have the same sensitivity to climate, which may be incorrect (Piao et al., 2013).”

Q.T14) 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 gen-

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eral. We have updated Fig. 3, which now depicts a climatology of wind fields from NCEP/NCAR reanalysis (1981-2010), 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. Our selection of figures aimed at illustrating this pattern. This is now better commented in the revised manuscript.

C) Lines 22-25, p1925 of the original manuscript have been rewritten as follows:

“Prevailing winds in the lower troposphere across TSA convey air masses entering from the Atlantic Ocean near the Equator across the continent and back into the southern Atlantic Ocean generally south of 20° S. There are no critical seasonal variations of the mean winds in the area so that this typical behaviour applies throughout the year. The climatology of wind fields from NCEP/NCAR Reanalysis (over the period 1981-2010) for February, July and annual mean, shown in Fig. 3, illustrates this circulation pattern.”

Q.T15) 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 fields of optimized CO_2 mole fractions. The results are now more consistent with the expected results from the assimilation of data at ABP. The figure below replaces Fig. 4 panel a. The corresponding statistics of the misfits between measurements and simulated mole fractions have also be updated in Fig. 5.

A) Figure 4c (below) has been updated, since it erroneously included observations and simulated mole fractions outside the assimilation time window (12:00-15:00 LT). The corresponding Fig. 5 has also been updated (see previous Technical Comment).

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Q.T17) 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) See our answer to the General Comment Q.3 from the reviewer. Figure 8 in the original manuscript corresponds to Fig. 6 in the revised manuscript.

Q.T18) 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) We have set a new scale for the y-axis: -0.3 to 0.25.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C3684/2015/acpd-15-C3684-2015-supplement.pdf

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	Standard deviation of the misfits			
Station	Prior	INSAm	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	4.6	4.0	4.6	9.6
GUY	4.0	3.5	4.1	3.3

Comparison of standard deviation of the misfits between the mole fractions observed and simulated from the inversions prior and the estimate of the standard deviation of the observation errors (i.e. of the transport model errors) for hourly values in the configuration of the **R** matrix.

Fig. 1. Table A1

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Spatial distribution of 2002–2010 mean flux corrections at the transport model resolution ($3.75° \times 2.50°$) to ORCHIDEE from (left) INVSAm and(right) MACCv10.1 over the study region: mean for **(a,d)** February, **(b,e)** July, and **(c,f)** mean over the full period 2002–2010. Flux increments over land and ocean are represented with two distinct colour scales and units: green–yellow for land, in $gC\,m^{-2}\,hr^{-1}$; blue–red for ocean, in $mgC\,m^{-2}\,hr^{-1}$. Filled circles indicate locations of sites with continuous measurements; open circles indicate locations of sites with discrete air sampling.

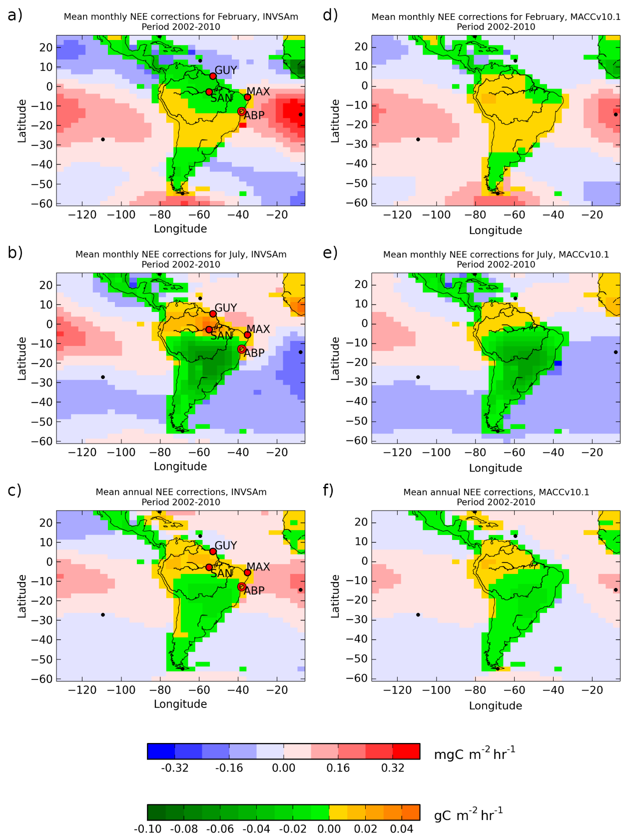

Fig. 2. New Fig. 6 in the revised manuscript

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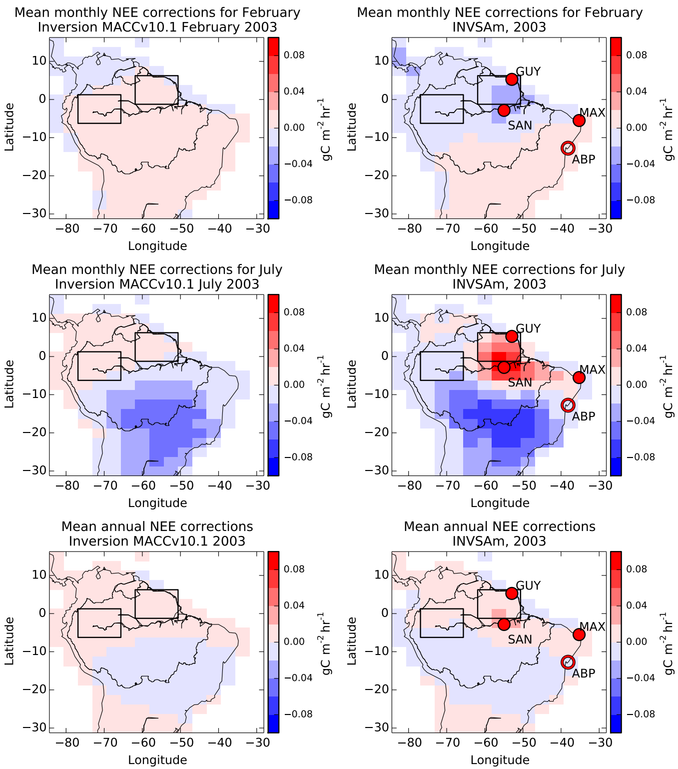

Spatial distribution of mean flux corrections in 2003 at the transport model resolution (3.75° × 2.50°) to ORCHIDEE from (left column) MACCv10.1 and (right column) INVSAm over the study region. Mean for February (top), July (middle), and mean over the whole year (bottom). Filled circles indicate locations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

Fig. 3. Fig. A.6

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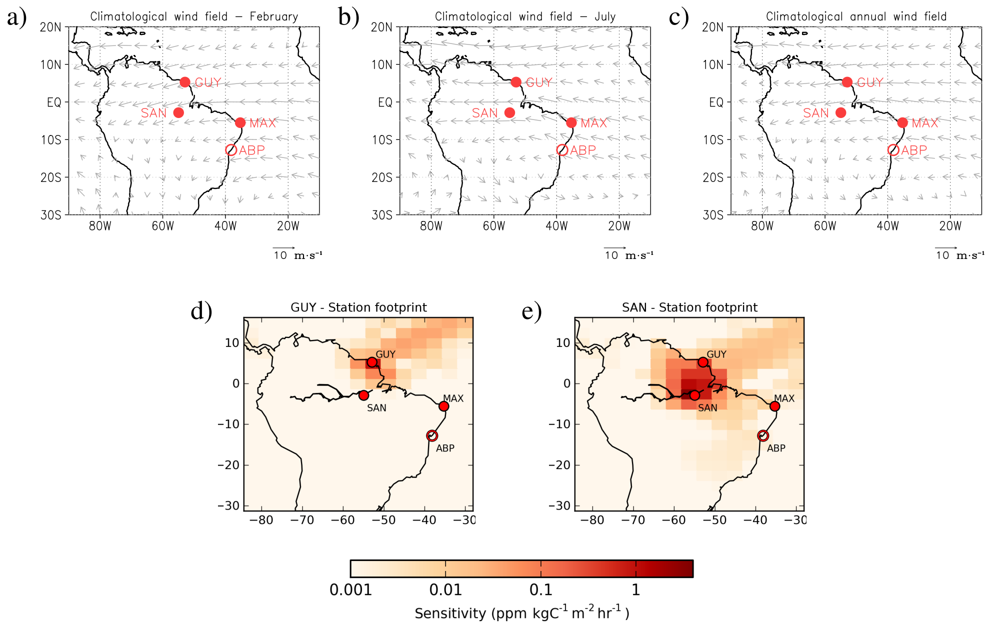

Top: Climatological wind speed and direction for (a) February, (b) July, and (c) annual mean for the period 1981–2010 (from NCEP/NCAR Reanalysis), averaged between the surface and 600 hPa. Bottom: Sensitivity of surface atmospheric CO_2 mole fractions measured on 20 February 2009 at 10:00 UTC, at Guyaflux (d) and Santarém (e), to a constant increment of surface fluxes during the two days prior to the measurement. Sensitivity values are expressed in log-scale. Circles indicate location of surface stations in South America. Open circles: sites with discrete air samplings. Filled circles: measurements taken with continuous analyzers.

Fig. 4. New Fig. 3 in the revised manuscript

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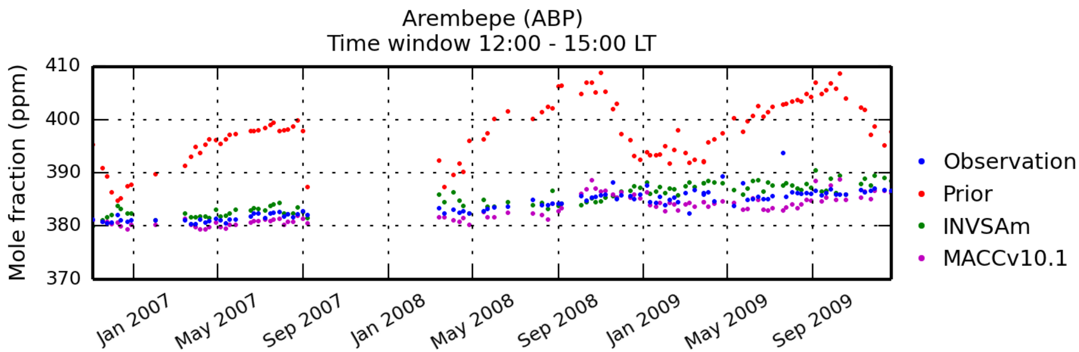
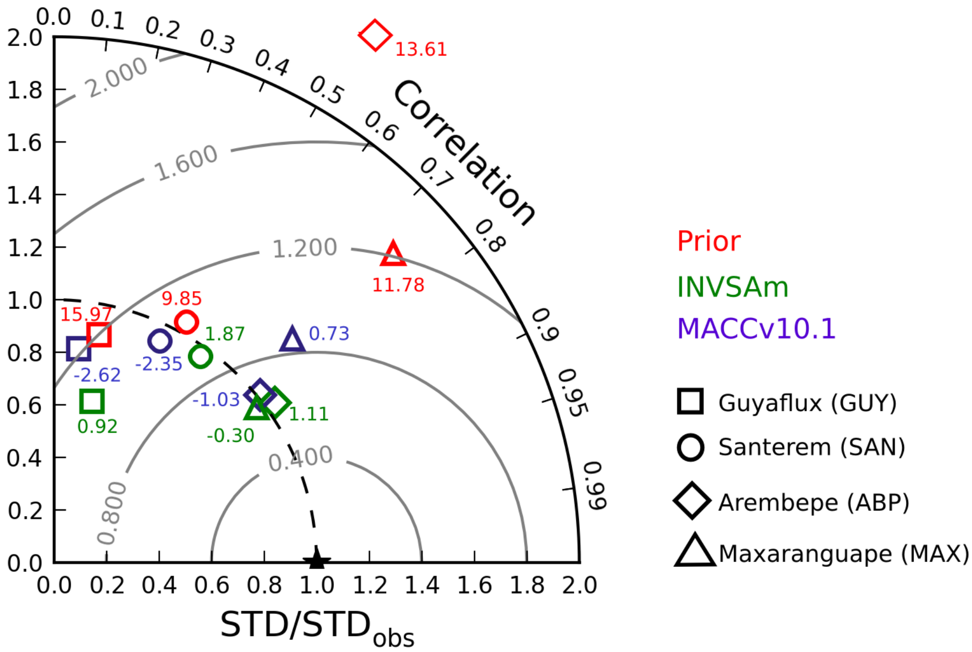


Fig. 5. New Fig. 4a in the revised manuscript



New Fig. 5. Taylor diagram of the statistics of misfits between observations and simulated CO_2 mole fractions between 12:00 and 15:00 LT at Guyaflux (square), Santarm (circle), Arembepe (diamond) and Maxaranguape (triangle), when wind speed $> 2\,\mathrm{m\,s^{-1}}$, using prior fluxes (red), INVSAm (green) and MACCv10.1 (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).

Fig. 6. New Fig. 5 in the revised manuscript

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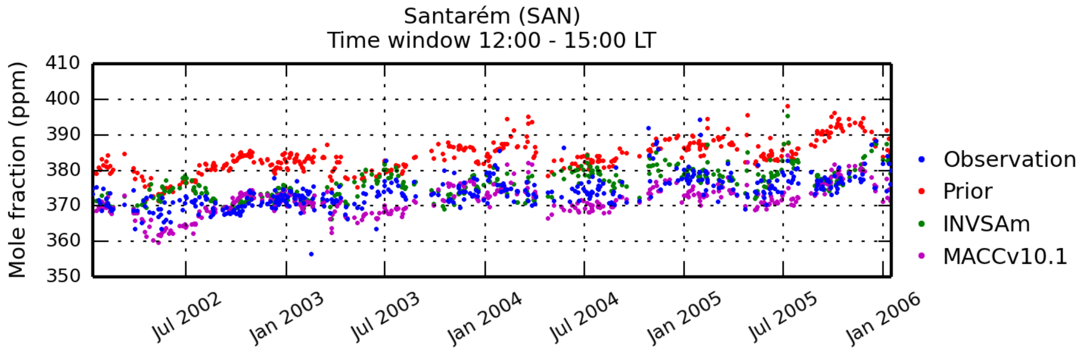

Fig. 7. New Fig. 4c in the revised manuscript

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