

Interactive comment on “On the ability of a global atmospheric inversion to constrain variations of CO₂ fluxes over Amazonia” by L. Molina et al.

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Q) General Comments This paper evaluates CO₂ fluxes over Amazonia that have been calculated using two atmospheric inversions, a control case and one that includes extra atmospheric CO₂ measurements for the Amazonian region. The analysis focuses on the seasonal cycle of fluxes and on interannual variations, particularly years that were notably dry or wet. The overall impression of the results is that the fluxes vary quite substantially across the Tropical South American region and at times it is diffi-

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cult to determine what extra information the Amazonian CO₂ data adds. The authors acknowledge this, noting in their abstract that ‘the results revealed critical limitations that prevent global inversion frameworks from capturing the data-driven seasonal patterns of fluxes across Amazonia’ and recommending in their conclusions that denser observing networks and regional models might be required to overcome the limitations. A) We thank the reviewer for her positive comments and sensible suggestions which make this review very helpful. We will try to take into account all these comments as well as possible as illustrated by our following answers. While I agree that this is a valid conclusion from this study, there are two other suggestions that I would like to make, one which could be incorporated into a revision of this paper, while the other targets future inversion work. Firstly, much of the current analysis looked at, for example, the seasonal cycles averaged across the full time period of the inversion. Given the intermittent nature of the Amazonian atmospheric CO₂ data, I wonder whether analysis targeted at periods when certain sites were active might yield clearer inversion impacts. Some suggestions are given in the specific comments below. A) We agree with this general comment and we will follow the more specific suggestions provided below for including analysis and discussions on this topic in the paper (see our answers to the corresponding comments). Secondly, I think that as an inversion community we need to be smarter about how we include continuous CO₂ measurements into our inversions. Each site has different characteristics and each transport model will represent those sites in different ways. We need to understand what parts of the CO₂ record we can most reliably simulate and consequently include in the inversion. Afternoon measurements (as used here) may be appropriate for continental sites with large diurnal cycles, but I would suggest that coastal sites need a different selection strategy. Likewise the choice of sampling location from a transport model (nearest grid-point or an interpolation between points) might be dependent on the characteristics of the observing location. A) We agree with these recommendations for better fitting the data with the model through site-specific studies. Actually, we feel that they are in line with our preliminary investigations regarding the representation of the diurnal cycle and day-to-

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day variations of CO₂ at the different sites that we have used. We finally based our data selection on rather traditional criteria (i.e., during the afternoon and when the wind speed is above a given threshold), supported by previous studies (e.g., Buttler et al., Tellus (2010), 62B, 550–572; Gatti et al., Tellus (2010), 62B, 581–594), and we finally located the sites in the corresponding (in terms of space coordinates) model grid cells. However, such traditional decisions were based on a site by site investigation of the diurnal cycle and day-to-day time series at each of the nine model grid cells at and around the site geographical locations. More details will be provided on this topic in the manuscript both when describing the strategy for the selection and representation of the observations in the model and in the discussion section. They will follow some of the ideas brought by the reviewer's detailed comments on this topic below (see our answers to the corresponding comments) I recommend that the paper be published with minor revisions to address the technical corrections and to clarify and extend the analysis a little based on the suggestions in the specific comments. A) We hope that our answer to the reviewer's comment demonstrate our intent to apply such revisions in a way that is fully consistent with her suggestions. Specific Comments Q) Sometimes it is not clear which region an analysis has been performed for, with various terms used e.g. 'Tropical South America/TSA', 'whole region' (p 1926, line 15; p 1932, line 17), 'entire study area' (p 1926, line 16). Please ensure that each region is defined. Also in the text the inversion without extra sites is usually referred to as MACCv10.1 while the figures are labelled with CH2010. It would be preferable to use one or the other consistently in text and figures. A) We will systematically clarify the region that is discussed by using a unique term for a given region and by clearly emphasizing the corresponding notations as soon as they will be used. Labelling the figures using CH2010 was a mistake which we will correct. For consistency, the term MACCv10.1 will refer to the results or analysis of the control inversion. The term CH2010 will be used to refer to results or conclusions from Chevallier et al. (2010) only. Q) p 1924, line 5: Were the ocean fluxes not examined or just not presented in this paper? Just as the discussion of Fig 8 mentioned the possibility of dipoles in the flux across the South American re-

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gion, a change in fluxes over the land can end up being compensated in the ocean. Given that some of the extra observing sites are coastal, I would expect that it would be worth at least checking the impact of the inversion on the ocean regions around South America. A) Ocean fluxes were not presented in the manuscript and we agree that we should include some comments and figures regarding the corrections applied to these fluxes, since it brings some interesting insights regarding the general behaviour of the inversion. Figure 8 will be replaced by the following figure below which depicts corrections for both the ocean and land fluxes 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. Therefore, the dipoles oppose 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 region Tropical South America. These effects are more visible when focusing on specific months, while the annual averages smoothens the patterns. In all cases, there is little evidence of direct compensation between land and ocean increments in the area.

New Fig. 8. Spatial distribution of 2002–2010 mean flux corrections at the transport model resolution ($3.75^\circ \times 2.50^\circ$) to ORCHIDEE from (left) INVSAm and (right) MACCv10.1 over the study region: 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: green–yellow for land, in $\text{gC m}^{-2} \text{ hr}^{-1}$; blue–red for ocean, in $\text{mgC m}^{-2} \text{ hr}^{-1}$. Filled circles indicate locations of sites with continuous measurements; and open circles indicate locations of sites with discrete air sampling.

Q) p 1924, line 9: how does this length scale (500km) compare with the distance between the four sites added to the inversion? A) Between SAN and GUY the geographical distance is roughly 1000km, but between the other sites it ranges between

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2000 and 2600 km approximately. Considering the 500km correlation length scale in the B matrix only (i.e. ignoring the effect of atmospheric transport), this could suggest that the area directly constrained by the South American sites is relatively small and that GUY and SAN would be the only couple of sites with overlapping areas of influence. However, the station footprint can be significant over land, as illustrated by Fig. 3. Furthermore, as demonstrated by the figure given above (which will become the new Figure 8), large increments are applied by both inversions, and the South American sites have a large impact on these increments over the entire South America. This is due to the long range extent of the footprints of the South American sites and other sites in the southern hemisphere. The South American sites are actually shown to constrain the large scale balance and between the positive and negative corrections north and south of South America and their spatial extent. We will include comments on this topic in the manuscript. Q) p 1925, first paragraph: it would be good to have some additional information about each site e.g. latitude, longitude, a brief site description e.g. the surrounding vegetation, distance from coast, sampling height. For ABP (line 10), are the weekly measurements selectively sampled under onshore flow, i.e. are they intended to minimise continental signals? How is the transport model sampled to represent these sites e.g. interpolation to the site location, nearest grid-cell? An offshore grid-cell can be more appropriate for a coastal site (e.g. Law et al, Tellus, 62B, 810-820, 2010). A) We will include additional information about each site in the manuscript: geographic location in latitude and longitude, altitude of the station and/or sampling height, conditions of the site (i.e. coastal or inland, dominant vegetation type surrounding the site), and for ABP: strategy for the weekly sampling. As guessed by the reviewer, the weekly measurements at ABP are sampled under onshore flow, and are also collected when wind speed > 2 m/s (PI personal communication). This could support the idea of representing this coastal site using an off-shore model grid cell. However, we checked the wind directions from the ECMWF Interim Reanalysis (which drives the LMDZ transport model) during the time of the day when air samples are available at ABP. The figure A.1 below shows the resulting frequency distribution of the

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ECMWF wind direction (i.e. direction from which the wind blows, in degrees, measured clockwise from the geographical North) at ABP when CO₂ is sampled. The figure confirms that according to ECMWF, ABP is mostly under marine influence, but this is not systematic and the instantaneous wind measurements that have been used to sample onshore flow at ABP may hide the fact that these measurements were done under intermittent wind conditions so that the air masses could still bear the signature of land fluxes (e.g. from the North). In any case, our final selection of the best transport model grid-cells to represent each site was based on an objective analysis of the day-to-day variations of the CO₂ selected during the analysis window (12:00 to 15:00 local time) and when the wind speed is > 2m/s. For a given station, the measured variations were compared to the ones modelled in the grid cell corresponding to this station (in terms of space location), and in the 8 neighbouring cells (which encompassed inland and ocean grid cells when analysing CO₂ at a coastal site). The figure A.2 below shows the resulting time series of observed and modelled CO₂ mole fractions at ABP (the layout of the plot corresponds to the geographic layout of the model grid-cells). For this site there is no critical difference between the mole fractions at the coastal and ocean model grid cells, certainly due to the threshold on the wind speed for the data selection. Based on the statistics of the misfits to the observations, we concluded that the grid-cells corresponding to the actual stations locations were systematically better adapted for the representation of these stations, even in the case of ABP. We should mention that a more flexible method where a given site could be modelled using different grid cells depending on the wind directions could raise better results. The text will describe these preliminary investigations (but the figures A.1 and A.2 will not be provided in the manuscript). A significant discussion on this topic will be raised in section 2.2 and reminded in the conclusion section. It will highlight the fact that, in principle, using an offshore grid cell to represent ABP could have been sensible given the sampling strategy at this site (using the reference mentioned by the reviewer) but that in practice, the misfits are lower at the coastal grid cell.

Fig. A.1 Frequency distribution of ECMWF wind direction at Arembepe, when CO₂

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samples area available in the time window 12:00–15:00 local time.

Fig. A.2 Evaluation of the grid-cell of the transport model that best represents the observations at Arembepe. Observations (blue) are selected within the time window 12:00 to 15:00 LT and have been already filtered for wind speed > 2 m/s. Simulated mole fractions (green) are calculated by transporting the prior surface fluxes described in the model setup (Section 2.1).

Q) p 1925, line 23 and p 1926, line 5: ‘typical’ circulation, ‘typical’ footprints. Is there much of a seasonal shift in circulation? A sentence to comment on this might be helpful. A) The Figure S.1 below depicts a climatology of wind fields from NCEP/NCAR reanalysis (1981–2010), averaged between the surface and a level of 600 hPa, over TSA region during (a) the austral summer (February), (b) austral winter (July), and (c) annual mean. The figure does not show critical seasonal changes in this average atmospheric circulation. The dominant, or typical, mode of horizontal circulation in the lower troposphere across Amazonia throughout the year is characterized by winds entering the Atlantic coast in north-eastern Brazil, through Amazonia and entering back into the Atlantic Ocean south of 20°S. Our selection of figures in Fig. 3 aims at illustrating this pattern. This will be better explained in the text. a) b) c) Fig. S.1. Long-term mean wind fields, 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 (mean 1981–2010).

Q) p 1925, line 25 to p 1926, line 1: Since MAX is a continuous site, are you able to distinguish in the CO₂ observations between periods of onshore vs. offshore flow (e.g. periods of relatively constant ‘background’ CO₂ versus highly variable CO₂ events). If so, what proportion of the data is from onshore? Is your afternoon data selection favouring onshore flow e.g. due to a sea-breeze circulation? It seems plausible to me that your data selection may be removing those observations that are more likely to have been influenced by the land region. A) The time series of hourly CO₂ and wind direction measured at the MAX station at any time or selected in the time window

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12-15 are given figure A.3. The reviewer is right about assuming that we can see a clear signature of onshore and offshore flows. Indeed, there are two periods when larger variations in the CO₂ observations can be identified, associated to wind directions > 150 degrees: between 2004-07-08 and 2004-09-21, and between 2005-03-20 and 2005-05-12. The rest of the time, CO₂ observations are rather stable. The reviewer is also right about assuming that our selection of afternoon data makes us lose onshore signal as demonstrated by the absence of occurrences of wind directions > 150° and of sub-periods of larger variations in CO₂ when selecting 12-15 data. However, assimilating data outside the chosen time window would have been a challenge, given the difficulties of the models to correctly represent the dynamics of the PBL. We agree with the reviewer that this reveals that we could select and represent the data in a more flexible and sensible way than we do here (see the answer to the previous comment on ABP) and this analysis at MAX will be discussed in the manuscript.

Fig. A.3 Time series of (top) wind speed and (bottom) CO₂ measured at station Maxaranguape.

Q) p 1926, line 18-22: ‘root mean square of the annual biases’ It’s not clear to me what exactly has been calculated here- the difference between the CO₂ predicted at a site from the two sets of priors?? ‘given that the mean transport error at the yearly scale should be far smaller according to the hypothesis made when setting-up the inversion system.’ I don’t understand this sentence. A) Actually, the reviewer correctly describes the comparison and assumptions made, and the text needed some clarification for explaining the aim of this comparison, which will be done. At each site we calculate the quadratic mean of the annual mean differences between the CO₂ simulated using the two prior estimates of the fluxes (we do not check the data availability at a given site and just take all simulated afternoon values throughout the 2002-2010 period). These differences revealed that the differences in annual budgets between the two prior estimates of the fluxes should yield strong signals at the annual scale at all sites in South America. Given that the weight of the transport error at the annual scale is, in theory,

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very small (we assume that there is no temporal correlation in the transport), this strong signal should be easy to detect and correct by the inversion system. Therefore we can hope that the inversion system can control the IAV of the fluxes.

Q) p 1927-1928, section 3.1 and figure 4: These figures are quite hard to read as the observations are sometimes obscured but they are probably adequate to illustrate the main points covered by the text. (Figures that showed more detail might lead to more insights into the inversion behaviour?) As noted throughout the section the ABP results do not seem consistent with the other sites. The simplest explanation would be that somehow in the analysis/figure the CH2010 and INVSAm time series have been inadvertently switched. Assuming that this has been checked, it is really difficult to explain how an inversion without ABP (CH2010) can fit the ABP data better than the INVSAm case where ABP is included, especially when there is almost no temporal overlap of other Amazonian sites with ABP, so little possibility that the ABP fit is being compromised by fitting other nearby sites. As plotted it appears that the INVSAm case is weakly retaining the seasonality of the prior at ABP while CH2010 manages to almost completely remove it. Were there any inversion settings different between CH2010 and INVSAm which could explain this? A) Thanks to this comment of the reviewer, we carefully checked the results for ABP. While there was not a switch between the MACCv10.1 and INVSAm timeseries, we had made a mistake when extracting the time series from INVSAm. The new plot and statistics of the posterior model data misfits are much more consistent with what is expected from the assimilation of the ABP data. We will update Fig. 4 panel a, with the figure below and the analysis in the manuscript accordingly. Figure 5 in the manuscript, which summarizes the statistics of the misfits between observations and model simulations, will also be updated accordingly. We apologize for this mistake and thank the reviewer for having helped to detect it.

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 where

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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).

Q) p 1928, line 12: I'm guessing the correlations are relatively low because you are using daily data but are estimating 8 day mean fluxes. It might be worth calculating the correlations on a monthly timescale as it would be interesting to see if they show a clear improvement between CH2010 and INVSAm because of an improved seasonal cycle. A) We have now calculated the correlations for monthly mean data at each station and, indeed, in general correlations to the observations are increased for the prior, MACCv10.1 and INVSAm. The corresponding Taylor diagram is provided below in figure A.4. However, this increase of correlations is not so high, and given the few number of monthly averages at MAX, ABP and GUY, the corresponding scores of correlations at these sites are not really reliable. We will comment these results in the manuscript.

Fig A.4 Taylor diagram of the statistics of misfits between observed and simulated monthly mean CO₂ mole fractions at Guyaflux (square), Santarém (circle), Arembepe (diamond) and Maxaranguape (triangle). Observed monthly means are calculated with observations available between 12:00 and 15:00 LT, and when wind speed > 2 m s⁻¹. Simulated monthly means are calculated from simulated mole fractions between 12:00 and 15:00 LT. 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).

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Q) p 1929, line 25-27: Perhaps it is also worth reinforcing the limited temporal coverage of the observations as another reason why there isn't a large impact on the seasonality. A) We will add such a comment in the text but will make in sort that this does not raise redundancies between the analysis in section 3 and discussions in section 4 by updating the section 4. Q) p 1930, line 3-12 and figure 6b: Am I correct in understanding that for the inversion this is just a regional selection of the data, the inversion itself doesn't do anything differently depending on the pft? If this is right, it might be worth mentioning. In Fig 6b the CH2010 line looks very similar to the CH2010 line in Fig 6a. Is this correct? The other cases all look noticeably different between Fig 6a and Fig 6b. A) The reviewer is correct about the fact that the PFTs are not accounted for in the inversion configuration and that this analysis for TBE forests is, strictly speaking, just a space selection of the data. Still, we can hope that the spatial patterns of the increments from the inversion could be consistent with the spatial patterns potentially induced by the heterogeneity of the vegetation types in the actual world. This will be briefly discussed in the manuscript. There was an error in Fig. 6b concerning the fluxes from MACCv10.1 and we thank the reviewer for pointing out this. This figure will be replaced by the figure below which shows differences for MACC between fig 6a and 6b as for other flux estimates. a) b)

New Fig. 6, panels a,b. Monthly mean NEE integrated over (a) the whole study region and (b) over pixels dominated by TBE forests in ORCHIDEE for 2002–2010. The shaded areas denote dry seasons, defined as months with precipitation < 100 mm, based on monthly totals from TRMM data over 2002–2010. Estimates from prior fluxes (red), INVSAm (green), MACCv10.1 (purple) and J2011 (dashed blue).

Q) p 1930, line 13-26: The flux tower precipitation and NEE plots (Fig 6c-f) are not really described in the text and need to be more strongly linked with the results presented in the rest of Figure 6. A) The flux tower and precipitation data in Fig. 6c-f were not fully exploited in the manuscript, as the reviewer observes. The plots depict the seasonal behaviour of both NEE and precipitation at those sites, and the message we

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meant to convey from these figures, i.e. the spatial variability of the seasonal cycle of NEE across Amazonia, was already well illustrated by the number of studies referred to in the introduction. We will thus move these figures to the supplementary material and describe them in a more concise way in the manuscript. Q) p 1931, line 14-18: The change in seasonality in Zone 1 for the INVSAm case might be even clearer if the seasonal cycle was calculated separately for 2002-2005 (when SAN was active) and 2006-2010. A stronger signal in the earlier period would be good confirmation of the influence of SAN data. A) Following this suggestion, the seasonal cycle of the four NEE estimates is calculated for zone 1 over the two proposed periods: 2002–2005 and 2006–2010 and shown in Figure A.5 below. The dry season extends September–November on both periods. As anticipated, the strongest changes between MACC and INVSAm take place during the first period, when data from SAN are available, which confirms the critical influence of this site in zone 1. However, there are still significant changes between MACC and INVSAm occurring in zone 1 between 2006 and 2010. And, as noticed in answer to the 5th major comment of reviewer 2, corrections in a zone can be driven by remote measurements and by their difference to South American data as revealed by the large scale structure of the increments shown in the new figure 8. So there is no need for having a South American site located in the vicinity of a zone for getting a significant change between MACC and INVSAm in this zone. The text will include a comment based on this analysis.

Fig. A.5 Monthly mean NEE at the Zone 1 predicted by (red) prior fluxes, (dashed blue) J2011, (green) INVSAm and (magenta) MACCv10.1 for two periods: (left) 2002–2005, when SAN is active, and (right) 2006–2010.

Q) p 1932, line 1-13 (Figure 8): This is an interesting figure but how do the INVSAm flux corrections compare to the CH2010 flux corrections? Are the CH2010 ones more uniform across the region? This is another figure where averaging over 2002-2005 and 2006-2010 separately would be interesting to try and maximise the signal from SAN. A) The new figure 8 displays the results for MACC. The average increments over land

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from INVSAm and MACCv10.1 and for the periods 2002-2005 and 2006-2010 are also shown in the figure S.1 below. This helps complementing the discussions given above in answer to the comment on ocean fluxes and on the corrections applied in zone 1 by the reviewer. The assimilation of data in South America generally shifts the zonal dipole in the increment (with a negative / positive gradient from the South to the North in February / July) to the south and amplifies it. Such a behaviour applies to winter and summer, and for the 2002-2005 and 2006-2010 periods. But it is particularly strong in July and for the period 2002-2005, emphasizing the higher weight of data at the most inland site i.e. SAN. This figure S.2 will be inserted as a supplementary material and the discussion on figure 8 will be expanded based on this.

Fig S.2. Spatial distribution of mean flux corrections at the transport model resolution (3.75×2.50) to ORCHIDEE from INVSAm and MACCv10.1 over the study region: mean for (left column) February, (middle column) July, and (left right) mean over: the full period 2002–2010 (rows 1,2), for 2002–2005 (rows 3,4), for 2006–2010 (rows 5,6). Filled circles indicate locations of sites with continuous measurements; and open circles indicate locations of sites with discrete air sampling.

Q) p 1932-1933, section 3.2.2: I assume the FLAT inversion included the 4 Amazonian sites. This should be noted in the text. Fig 10a shows a large difference in flux anomaly between CH2010 and INVSAm for 2008. Any ideas why, since during this year only ABP data is available and it is relatively remote from zone 1? I'm not sure that I am convinced that 'some patterns of the IAV in the NEE from the inversion seem robust and strongly driven by atmospheric measurements' (p1934, line 24) - even for the significant drought/wet years the results seem quite mixed depending on which region is considered and what prior was used. A) We will clarify in the text that FLAT includes the four new surface sites in TSA region. Regarding the interannual anomaly for a specific year such as 2008, they can 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 will be better explained in the manuscript). This explains why

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large differences in such anomalies can occur between MACC and INVSAm even during years there are few data in South America. The general behavior of the inversions illustrated by the new figure 8 and figure S.1 above indicate that zones 1 and 2, such as the area of TBE forest, are often located at the edge of the zonal dipole controlled by the assimilation of data in south America, leading to varying (depending on which data are assimilated) inter-annual anomalies in these zones/areas, as discussed for zone 2 and year 2003 in answer to the fifth comment of reviewer 2. However, results should be more stable when considering the entire TSA region. When referring to the robustness of some IAV patterns (based on the similarity of the results from the different inversions), we referred more specifically to the anomaly observed in 2009 over TSA region, when considering all PFTs (Fig. 9a). This discussion about the fact that the edge of the dipole crosses the TBE forest area and zones 1 and 2 may give further insights on why the consistency between the IAV from the different inversions does not apply when restricting the analysis to these areas / zones and will be conducted in the new manuscript. We will also modify the text to be more cautious when speaking about the 'robustness' of the results.

Technical corrections Q) p 1917, line 13-15: Suggest rewrite start of sentence as 'We focused on the NEE impact of the strong droughts ...' A) We will change the sentence as suggested. Q) p 1919, line 14: 'reversal' instead of 'reversion'? A) We will incorporate this change. Q) p 1919, line 16: delete ')' at end of sentence A) This error will be corrected in the text. Q) p 1920, line 20: 'the inverted pattern...' Do you mean the opposite pattern is seen in S and W Amazonia compared to E Amazonia? I would rewrite this sentence and avoid the word 'inverted' because of the potential confusion with using an inversion method to estimate fluxes. A) We will rephrase the sentence. Q) p 1921, line 5: Figure 1 could be referenced here A) We will insert a reference to Fig. 1 as suggested. Q) p 1921, line 15-22: I would consider moving this description of the J2011 data until later (maybe have a short section 2.3 for 'comparison data') in which case you need to change 'J2011' on p1921, line 27 to 'independent flux estimates'. A) We will modify the text as suggested. Q) p 1921, line 22: replace 'were'

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with 'where' A) We will incorporate this change. Q) p 1925, line 17-21: I think the sampling periods are adequately covered in the figure and it is probably sufficient to reduce these three sentences to 'The longest records were from ABP and SAN.' A) We will rephrase the sentence. Q) p 1926, line 9: Suggest paragraph break before 'To further ...'. Suggest add 'designed to remove interannual variations' following 'flat prior'. A) We will rephrase the sentence. Q) p 1926, line 17: insert 'variability and' between 'spatial' and 'the temporal' A) We will make this correction. Q) p 1928, line 7: 'amplitude of variations', on what time scale? Seasonal? A) We will rephrase the sentence. Q) p 1930, line 26: suggest paragraph break before 'To examine' A) We will incorporate the change suggested. Q) p 1931, line 19: suggest add 'other' before 'sub-regions' A) We will incorporate the change suggested. Q) p 1931, line 21: suggest add 'where the dry season is potentially earlier and more extreme (Fig 6c,f)' after 'Amazonia.' and delete following sentence 'Both ... (2011)'. A) We will modify but keep the two sentences because it was actually the study of Lewis et al. (2011) that motivated us to inspect areas most affected in terms of water deficit during the extreme climatic events of 2005 and 2010, first to look at impacts on the seasonality and then for interannual variations of NEE predicted by the different inversion estimates. Q) p 1931, line 23: suggest delete 'here' and add 'any' between 'provide' and 'further' A) We will incorporate the change suggested. Q) p 1931, line 28: might want to note that the slight modifications to NEE are to be expected since there is not much data in the southern part of the TSA region. A) We will add a sentence to remark the scarcity of data in that region, especially in southern Amazonia, as suggested. Q) p 1933, line 2: suggest adding to the end of the sentence 'opposite to the response for the whole TSA region.' A) We will incorporate the change suggested. Q) Figure 1: It would be helpful to label the red sites, perhaps with their initial letter. A) We will update the figure. Q) Figure 2: The vertical line between 2008 and 2009 appears to be missing A) We will correct this mistake. Q) Figure 3 caption: perhaps give local time as well as UT for the sensitivity plots A) We will incorporate the change suggested. Q) Figure 6 caption: The caption doesn't actually say that it is a NEE anomaly that is shown. A) We will

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clarify this in the caption of the figure. Q) Figure 7 caption: Replace 'Dominating PFTs' with 'Dominant PFT' A) We will incorporate the change suggested.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C2153/2015/acpd-15-C2153-2015-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 1915, 2015.

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