Responses to referees' comments (ACPD-15-108-2015)

We thank anonymous referees for their thoughtful and constructive comments and helpful suggestions. We also thank the editor for his careful handling of this manuscript.

We have fully considered the referees' comments in the revision and improved the manuscript accordingly. The referees' questions are italicized and are immediately followed by our answers.

Anonymous Referee #1

This paper endeavors to characterize the impacts of atmospheric transport errors on CO2 surface fluxes inferred from GOSAT data using the GEOS-Chem transport model. The work does this by propagating a CO2 'adjustment' imposed in the Arctic tropopause region through the GOSAT inversion and examining the change in inferred surface fluxes relative to a baseline inversion. The magnitude and vertical location of the adjustment is set by comparing GEOS-Chem simulation with HIPPO measured CO2 and O3 in the high latitude UT/LS, where sizable profile discrepancies are noted, presumably as a result of excess model mixing. To isolate the transport error impact on CO2, the model O3 field is constrained by assimilation of stratospheric O3 observations, and the resulting CO2/O3 tracer correlations are used to infer a pan-Arctic CO2 error relative to the HIPPO correlation. A second sensitivity run adds a partially compensating adjustment to CO2 in the tropical/subtropical NH upper troposphere, again in the direction of observed HIPPO discrepancies. The results are consistent with what might be expected: if you put a sink in the high latitude UT, then the inferred surface sink is diminished relative to the baseline inversion and the inferred tropical source increases. Adding the second tropical source adjustment brings the inversion back closer to the baseline but perturbs the seasonality somewhat. The bottom line of the paper is that mixing-transport errors in the UT/LS matter for surface flux inference, and that these errors result either from erroneous large-scale dynamical balance or numerical errors.

The paper is interesting. It addresses an important topic with a novel approach and the authors have done a lot of work for it. The difficulty with the paper is that the approach doesn't really test the sensitivity to the problem in question. The basic question is how much do errors in transport affect inferred flux distributions, in particular well known errors in strat-trop exchange in models driven by assimilated winds. Further, do they affect inversions based on column CO2 data differently than those using surface data? Sticking a CO2 sink in the Arctic UT/LS is not really testing the sensitivity to transport error. A few points deserve consideration:

A: We thank Anonymous Referee #1 for the positive and constructive comments, which we used to improve our manuscript. More detailed description of the changes we made per comment is given below.

The purpose of the adjustment is to estimate what the inversion would do if the model did not have excess mixing. Since there is no net source/sink in the UT/LS, the simulation including the offsetting tropical source is the more representative surrogate distribution for a model with *better mixing characteristics. It should be featured. A better test would be a flux-balanced adjustment (as the text recommends on P. 10830). This is model land: do it.*

A: This is a good point. Clearly there is no net source/sink in the UTLS, however, as we stressed in the manuscript, we do not want to impose an arbitrary source or sink just to get global balance since that would not be informative. The Arctic adjustment is data driven, reflecting an observed discrepancy in the model relative to HIPPO. If we were to arbitrarily add a compensating adjustment to the UTLS, say in the southern hemisphere, the model would be balanced globally, but biased in the southern hemisphere, which would have implications for the estimated fluxes. Instead, we have used the estimated bias relative to the HIPPO data to guide us in our specification of the source – it is important that the specification of the sink and source be data driven and that they not exacerbate the bias in the model. Previously, we had specified a source of 0.2 ppm, which we have now increased to 0.25 ppm. This modest increase across the low-latitudes improved the mass balance without significantly biasing the model. Also, previously we estimated the Arctic CO_2 sink using the CO_2 mixing ratio adjustments off-line. This gave an inaccurate estimate of the sink. We now calculate CO_2 mass adjustment on-line, based on the a posteriori CO_2 fields. The estimated Arctic sink is now 0.60 Pg C for (March – August) and the new estimated low-latitude source is 0.55 Pg C. So the source and sink are now much more closely in balance. The global imbalance of 0.05 Pg C is sufficiently small that it does not impact the main results of the analysis.

In revising the analysis, we have also increased the number of iterations in the assimilation. This produce only modest changes estimated fluxes, but we have updated Figs. 9 and 10 and the relevant numbers in the text. These minor updates do not change the main findings of the analysis.

The magnitude of the Arctic CO2 adjustment is not small (p. 10828, line 13). 0.13 PgC/mon would be 1.56 PgC/y, which is more than half of the global residual land sink or greater than the US fossil fuel emissions for 2010. It is not surprising that the perturbation shows up in the CO2 column a long way from its home after a few months, particularly in a model with excessive isentropic transport. Discussion and figures in Section 3.2 are only loosely related to main point of paper.

A: The adjustment is large. However, we would not expect it to be constant over the whole year. If the discrepancy is indeed due to mixing, then we would expect it to be large when the vertical gradient is large. This means that by August, when the summertime drawdown reverses the vertical gradient in the troposphere, we would expect a much smaller bias in the lower stratosphere. Because of the influence of the summertime drawdown, we intentionally ran the simulation only from March to August. Similar biases have been shown by Song et al. (ACPD, doi:10.5194/acpd-15-6745-2015, 2015) in their comparison of their model with HIPPO data and they found similarly large differences in March 2010 (during HIPPO-3), but much smaller differences in November 2009 (during HIPPO-2).

It is not surprising that the discrepancy is transported out of the region and can impact the flux inversion. However, the general belief in the community has been that discrepancies in the lower stratosphere would not be much of an issue for XCO_2 inversions because of the use of the vertically integrated CO_2 abundances. This was mentioned on lines 3-5 of page 10817 of the original manuscript. We then pointed out that Lauvaux and Davis (2014) suggested otherwise. Our results provide additional evidence that vertical transport errors can be an issue for XCO_2 inversions. We felt that Section 3.2 was necessary to illustrate this point; because of the influence of transport these biases could be an issue. Indeed, it was suggested by Song et al. (2015) that because there are few GOSAT XCO_2 observation at the high latitudes, these Arctic biases will not be an issue for the XCO_2 inversions. They stated "The satellite observations of the total column such as GOSAT are also reduced considerably in high latitudes in cold season (Yoshida et al., 2013). Thus this lower stratosphere bias is not likely to deteriorate the transport model performance in the inverse modeling applications." Section 3.2 shows that transport of this stratospheric bias can indeed impact the model performance in regions where there are XCO_2 observations.

Ideally, one would run the same inversion with different transport fields that vary in some known fashion with quantifiable errors. This has proven difficult over the (TransCom) years and that is why this paper retains interest. The one clean test that can be made, and which will answer one of the possible root causes of transport error, is to run the transport at higher spatial resolution. Likely both numerical and dynamical errors contribute. Previous studies have shown that UT/LS tracer gradients can be improved significantly by going to finer resolution than 4 x 5 (Strahan and Polansky, 2006; Considine et al., 2008). Do the transport at higher resolution, answer the question (hopefully), and drop the speculation from the discussion.

A: We thank the reviewer for the suggestion. The GEOS-Chem model can currently be run globally in two horizontal resolutions 4° x 5° and 2° x 2.5°. We have run GEOS-Chem using same initial conditions at both resolutions to determine whether "UTLS tracer gradients can be improved significantly by going to finer resolution than 4° x 5°". We found that going to the higher resolution enhances the vertical gradient in CO_2 at high latitudes. We have added Section 3.4, in which we present the results from this analysis.

One aspect of the analysis where the paper really misses an opportunity is relating the flux sensitivity differences to the baseline posterior error estimates, which are not given at all. A key question is whether the error covariances are adequately scaled to include transport uncertainty in the posterior flux uncertainties. This aspect should be worked into the paper. Similarly, some indication should be given as to how the prior uncertainty estimates (P. 10822-10823) influence the posterior fluxes. It may turn out that the UT/LS flux adjustment does not change the posterior fluxes beyond their error bars, in which case, the basic conclusions would have to be revised, but we might feel more confident of our flux calculations and their uncertainties.

A: In Deng et al. (2014) we described how we scale the covariance to ensure that the a posteriori reduced $\chi^2 = 1$ constraint is satisfied. Also, as shown in Figure 7 of Deng et al., we obtained a good Gaussian distribution for the differences between the a posteriori CO_2 and the observations.

Since we are using exactly the same configuration as Deng et al. (2014), we did not repeat the details of the inversion approach in this manuscript. Characterizing model transport errors is challenging, but we believe that we have adequately scaled the errors.

The magnitude of the fluxes relative to the uncertainties varies from region to region. For Temperate North America, for example, with the Arctic sink the changes are larger than the flux uncertainties for March through June. With the combined source and sink, the Temperate North American flux changes are larger than the uncertainties only in June and about 95% of the magnitude of the uncertainty in May. For Northern Africa, the largest absolute difference shown in Figure 10 is for July with the tropical source, and that difference exceeds the uncertainty. The comparison with the uncertainty is actually not the key issue here. The fact is that the discrepancies represent significant spatially dependent biases, which have implications for the latitudinal distribution of the estimated sources and sinks. In the standard inversion we estimated a global sink of -6.65 Pg C and with the Arctic sink and tropical source we obtained a global sink of -6.64 Pg C. However, the northern land sink (for March – August) was 0.98 Pg C weaker with the Arctic and tropical adjustment than in the standard inversion. That is a large difference. Because of the high latitude bias we estimated a stronger extratropical drawdown during the growing season. Previously, we had mentioned this change in the northern land sink, but did not quantify it. We now highlight this large regional change in the flux estimates.

Finally, P. 10819, line 7-8 promises a discussion of the implications of this work, but the Conclusions section mostly just reiterates what has been done and said above. There is very little here, or in the abstract, to say what the implications are for source/sink inference with GEOS-Chem and GOSAT beyond some speculation about the root causes of the transport discrepancies. Addressing the comments above should give the paper more impact.

A: As a result of our comparison of the different model resolutions, the conclusions are less speculative. We thank the reviewer for encouraging us to do the run with a different resolution. Our results also have implications for the differences obtained between inversions using surface flask data and XCO_2 retrievals in terms of the northern vs tropical land sources and sinks. We have added a brief discussion of this in the conclusions.

Minor Recommendations:
P. 10815, line 5: sub 'whose representation in models is' for 'which are'.

A: Corrected according to your suggestion.

P. 10815, line 12: 'use' for 'used'.

A: Corrected.

P. 10815, line 13, 15: Reword 'correction'. This exercise establishes an error magnitude and location, but it's not really a correction. Maybe 'adjustment' or 'error'.

A: Yes, you are right. We use 'adjustment' to replace 'correction'.

P. 10817: need references for Lauvaux and Davis, and Parazoo et al.

A: These references were added.

P. 10819: it would probably be worth upgrading to a more recent version of the ACOS GOSAT product that includes glint and high gain data. Flux sensitivity to UT/LS transport may well depend on data coverage.

A: We used only high gain data in this study to ensure consistency of our analysis with that of Deng et al. (2014). And also, as mentioned in the manuscript, the biases were not well characterized in the glint data in ACOS b2.10, which is the version of the data used by Deng et al. In our new inversions (not presented in this manuscript) we are using version b3.4 and b3.5 data and we are incorporating glint data in our analyses.

P. 10825, line 27: 'altitudes' should be 'latitudes'?

A: Thanks. It should be latitudes. We have changed it.

P. 10828, line 23 ff: The paper 'would expect a negligible change in the flux estimates: : :in the SH.' This may or may not be, as a 0.2 ppm perturbation might have a significant impact on flux in the region of small variability dominated by relatively small ocean fluxes. The point is that transport errors may impact distant fluxes especially as the run progress beyond a few months. Revise, delete, or run it out for year or so.

A: The reviewer is correct. We have removed the statement.

P. 10831, line 1-4: This reasoning does not make sense to me. Seems like balancing the high latitude sink is the least arbitrary way to test the impact of transport mixing error.

A: As described above, now we use near balancing adjustments for the Arctic and tropical atmospheric CO_2.

P. 10833, line 24 ff: Numerical scheme and resolution are separable issues in model formulation but here they are intermingled. Clarify discussion and its point.

A: In general they are separable. However, some schemes are more diffusive than others and, therefore, model resolution becomes an issue. Indeed, the Prather et al. (2008) study that we cited examined the impact of model resolution on two difference CTMs using different numerical schemes. As we noted in the manuscript they found that doubling the resolution (from 4x5 to 2.2x5) improved the simulation in both models, but the errors were smaller with the less diffusive Prather Second-Order Moments scheme. In the revised manuscript we doubled the model resolution (to 2x2.5) and found that it does improve the vertical gradient in high latitudes.

P. 10834, line 21 ff: There is a fairly rich literature on the subject including O3 and other tracers including CO2 from the ER-2 and balloons in the UT/LS that could be explored to address some of these questions (before calling for more measurements).

Recommended References: Evaluation of near-tropopause ozone distributions in the

Global Modeling Initiative combined stratosphere/troposphere model with ozonesonde data, Considine, DB; Logan, JA; Olsen, MA, ATMOSPHERIC CHEMISTRY AND PHYSICS, Volume: 8, Issue: 9 Pages: 2365-2385, 2008.

Meteorological implementation issues in chemistry and transport models, Strahan, S. E.; Polansky, B. C., ATMOSPHERIC CHEMISTRY AND PHYSICS, Volume: 6 Pages: 2895-2910, 2006.

A: We have added text explaining that the issue for STE has been well-studied using data from the ER-2 and balloons. However, this does not negate the need for more observations to better evaluate the model. The ad hoc assumptions that we made here were due to the limited spatio-temporal observational coverage of profile data that extend from the troposphere to the middle stratosphere.

Anonymous Referee #2
Received and published: 29 May 2015
This study investigates the importance of atmospheric transport uncertainties in stratosphere – troposphere exchange for the estimation of surface fluxes of CO2 using satellite data. This is a very interesting topic and also timely, because of some other studies arriving at conclusions about regional carbon fluxes from the use of GOSAT that are heavily debated. Here a mechanism is proposed that has the potential to resolve part of this intriguing puzzle. The shift in inversion-estimated emissions with latitude seems a logical consequence of the upper air CO2 fluxes that are introduced. It is important to know, however, how justified these corrections really are, whether they make the model more realistic in the end, or whether the impact on the inversion results really represents that of the underlying transport model problem. My main requirement, before this study can be promoted to the next stage of ACP, is to demonstrate more clearly that this is indeed the case as will be explained in more detail below.

A: Thanks for your positive remarks to our effort in using ACOS GOSAT XCO_2 data and other observations to *investigate the importance of atmospheric transport uncertainties in stratosphere – troposphere exchange*. We really appreciate your constructive suggestions. The comments are a great help to improve the manuscript. Below you can find our detailed responses to the comments.

GENERAL COMMENTS
Further motivation and clarification is needed of the different time windows that are used. At the start of the method section it is mentioned that GOSAT data are used spanning July 2009 to December 2010, but surface fluxes are only optimized for the period March-August 2010. For the regression to HIPPO, March-April 2010 was used (the campaign is from March 24th to April 26th), The Osiris O3 simulation was from 20 March to 2 April, whereas the ACE-FTS validation was from 20 March to 3 April. As I understand it, the period of the HIPPO campaign is used to determine the CO2/O3 correlation, which is translated into a CO2 correction using the OSIRIS

optimized model. However, this correction is then assumed to apply to the whole period from March to August. No information is given on whether or not this is justified. Moreover, the correction is quantified for the month of March, although the modeled O3 has only been optimized for the period 20 March to 2 April. It might imply that O3 was off in the first part of the month and that therefore the CO2 concentrations were off as well. Or has the CO2 correction that has been derived for the period 20 March – 2 April been assumed to be constant for the whole period? In that case, it is not a surprise that the derived flux corrections are roughly the same for every month, but that doesn't imply that a constant correction is a valid assumption to make in the first place. In the revised version of the manuscript these issues should be explained much clearer than is the case right now.

A: We used a 2-week assimilation window because the 4D-Var assimilation adjusts the initial O_3 conditions to optimize the model trajectory over the assimilation window. If the window is long compared to the lifetime of ozone, the assimilation system is unable to use the information from observations toward the end of the window to adjust the initial conditions, since that information is chemically destroyed. In the high-latitude UTLS, a longer assimilation window would not be a problem since the O_3 lifetime is long in that region. However, in the tropical and subtropical upper troposphere, the O_3 lifetime is about 3 weeks. Using a longer assimilation window would be undesirable. On the other hand, if the window is too short, there is less data available to adjust the state. Since the Arctic HIPPO measurements were made on March 26th and 27th, we chose the window of March 20 to April 2 so that the timing of the HIPPO data would fall at the midpoint of the assimilation window. We have added some text to explain this.

We looked at the modeled CO_2/O_3 correlations across the Arctic throughout the month of March and they were fairly consistent. This is not surprising since we would not expect the large-scale transport to differ significantly on the timescale of a few weeks. Consequently, we felt justified in applying the adjustment from late March throughout the month. Use of the adjustment from March to August is not physically justified. As we explained in our response to Reviewer 1, in the absence of data to quantify the evolution of the bias from March to August, the simplest assumption was to assume it was constant and assess its impact. Furthermore, as we noted in the response to Reviewer 1, we might expect a bias until about mid-summer, when the drawdown in CO_2 significantly changes the vertical gradient in CO_2 in the troposphere. We have added text to better explain our motivation behind the approach.

The purpose of the ACE-FTS validation is not quite clear. First I thought that it covered a different part of the atmosphere (since it measures down to the mid troposphere), but the comparison is limited to pressures up to 200 hPa. Judging figure 4, this is probably just up to the altitude of maximum correction by OSIRIS. It raises the question in which pressure range OSIRIS data were assimilated (which I didn't find back), and if it extends to pressures above 200 hPa then why the comparison to ACE-FTS is limited to pressures up to 200 hPa. Presently the ACE-FTS validation seems just like a validation of OSIRIS (potentially gap filled using the model), rather than a validation of the O3 assimilation. I wonder actually why after optimization the general shape of the mismatch (under/overestimated O3 at higher/lower pressures) remains. The text mentions that the optimization significantly improves the agreement with ACEFTS.

Looking at figure 5, I wonder how significant this improvement really is, and why substantial differences remain.

A: We assimilated the OSIRIS data at all levels for which there were data. Both ACE-FTS and OSIRIS provide limb measurements, which do not extend deep into the troposphere. There are very few data in the upper troposphere at these latitudes. Furthermore, the precision in the data are lower at higher pressures so the impact of the OSIRIS data in the assimilation will be smaller at higher pressures. As a result, we would expect a more modest correction to the modeled O_3 bias at higher pressures. That is why there is still a large residual bias at the lower altitudes.

Besides the O3 validation, I would have expected a CO2 validation against HIPPO after the regression correction is applied. Figure 3 only shows how the correlation between CO2 and O3 improves after assimilating OSIRIS data, but not how well the applied CO2 correction actually works. This could easily have been included in my opinion. In addition, I think further support is needed for the assumption that this correction is not just valid for part of the HIPPO 3 campaign, but also at other times of year. If the time window of the inversion had been shifted to also cover HIPPO2, then this would have offered a truly independent validation substantially strengthening the case for the method that is used. I also find the validation of CO2 and O3 too much concentrated to higher latitudes, whereas a correction has also been applied to the tropics. Both latitudes are important since the question we are dealing with is about the partitioning of CO2 fluxes between the tropics and the extra-tropics. I see no reason, given the data that are available, not to extend the validation to the tropics.

A: We used the HIPPO CO_2/O_3 relationship in the Arctic to adjust the model, so the modeled CO_2 matches the mean HIIPPO CO_2 in the Arctic. Consequently, we did not show an evaluation of the adjusted CO_2 relative to HIPPO. We did find that the a posteriori CO_2 distribution, with the Arctic adjustment, was slightly in better agreement with data; the mean difference between the model and the flask data was 0.25 ppm smaller with the adjustment and the minimum value of the cost function was also lower with the imposed adjustment. However, because we do not have data to characterize the bias over the seasonal cycle, we are not claiming that the adjustment will give the best fit to independent data. We are only trying to show that the bias, although mainly in the UTLS, can impact the flux estimates, so we did not include the comparison with the surface data in the manuscript.

In my opinion, one issue has been overlooked which could have implications for the size of the surface flux corrections that have been derived. This is that the CO2 correction fixes its concentration in the UTLS, but not the underlying transport problem. It means that the model is still mixing too fast, which the flux correction will need to keep up with. The larger this flux, the larger the compensating inversion-derived correction in the surface flux (because of the large-scale mass balance constraints imposed by the measurements). It means that the surface flux corrections should be an upper limit of transport model induced error at best. This should be made much clearer in the discussion and conclusions section.

A: The reviewer is correct that we are not fixing the underlying problem. It is possible that the flux adjustments obtained here are an upper limit, but in the absence of more data to better characterize the spatio-temporal evolution of the bias we are reluctant to make such a strong statement.

SPECIFIC COMMENTS

Abstract, line 21: From the results I understand that the tropical correction comes on top of the correction in northern latitudes. This sentence suggests that in this case only an emission correction was applied to the tropics. Else I wonder why the correction is larger than that at higher latitudes. If it were the same then it would correspond to a pure mixing problem (from tropics to high latitudes). Which would make sense because I suppose tracer transport in the model is (at least close to) mass conserving.

A: In this revision, we used a near balancing Arctic and tropical atmospheric CO_2 adjustments, a sink of -0.60 Pg C/6-month imposed for Arctic UTLS, and a source of 0.55 Pg C/6-month (an equivalent of 0.25 ppm) imposed for tropical atmosphere. (see details in response to Referee #1)

Page 10822, line 23: Does BEPS simulate terrestrial ecosystem exchange outside the boreal zone? If not, then what was assumed for the a priori ecosystem exchange at mid latitudes and in the tropics?

A: BEPS simulated global terrestrial ecosystem exchange in hourly time step (e.g. Chen et al., 2012).

Chen, J. M., Mo, G., Pisek, J., Liu, J., Deng, F., Ishizawa, M., and Chan, D.: Effects of foliage clumping on the estimation of global terrestrial gross primary productivity, Global Biogeochemical Cycles, 26, 10.1029/2010gb003996, 2012.

Page 10823, line 1: Are the a priori uncertainties of GPP and TER assumed to be independent? How about the time correlation of the uncertainty in the 3 hourly fluxes.

A: We assumed the uncertainties of GPP and TER being independent, and we also did not consider the time correlation of the uncertainties in the 3 hourly fluxes.

We are aware that the covariance and correlation exist and could be very strong. However, our aims of this study are not to separate GPP and TER, and to optimize 3-hourly fluxes. As we described in our previous paper (Deng et al., 2014), we synthesized all state vectors to obtain optimized monthly natural fluxes (excluding fossil fuel emissions). The 3-hourly fluxes are used to better the fit of observations within a monthly time step, in order to help improve monthly natural flux estimates.

Figure 1: From this figure it is actually not so clear how important the differences between HIPPO and GEOS-Chem really are. It depends on how the blue dots are distributed within their range at a certain latitude. Around the equator it is impossible to see if there are blue dots behind the red band. The fact that the data show a larger range may be because the model doesn't resolve small-scale variability. Are the differences significant after averaging to a resolution that the model can be expected to resolve?

A: This should be Figure 2. We have more than 15000 HIPPO observations and the same number of model data plotted in this figure. Our main aim here is to show the trend, and large discrepancies for the northern high latitudes. As we described in our text, the mean difference between the model and the observations at low latitudes is about 0.2 ppm, and this can hardly be visualized in this figure. Because we are using the same inversion configuration as Deng et al. (2014), our standard inversion results are the same as RUN_C of Deng et al., and Figure 10 and Table 4 of Deng et al. give more information on the evaluation of the model with HIPPO data.

Page 10826, line 16: does assimilation increase the bias between 65 and 75 degree north?

A: No. It is already shown in Deng et al. (2014) that the assimilation decreases the bias between 65°N and 75°N for HIPPO data observed lower than 5km. Our calculation also affirmed that assimilation also decreases the bias (from 2.25 to 1.22ppm) in this latitudinal range for HIPPO data observed from 5 km to 14.5 km.

Page 10826, line 27: This sentence confuses me. I suppose what is meant is that the model shows similar CO2/O3 correlations at places where HIPPO data are available and elsewhere in the Arctic. Please rephrase.

A: Yes, it means that the modeled CO_2/O_3 correlation is the same across the Arctic in the model. We have the same correlation at locations where HIPPO data are available and at locations where there are no HIPPO data. This is not surprising given the small areal extent of the Arctic and the rapid horizontal transport in the UTLS. We have modified this to read "Examination of CO_2/O_3 correlations in the model at the locations of the HIPPO data and elsewhere across the modeled Arctic produced negligible differences in the correlations."

Page 10828, line 1: "This is not a concern since : : :" I don't quite understand this sentence. To me it seems that the CO2 source is meant to correct a mixing problem, i.e. a process that conserves mass. The dispersion of the signal to other regions might therefore worsen the agreement with HIPPO there, and influence the fit to GOSAT data and thereby the inversion-derived fluxes. We can exclude the possibility that GEOS CHEM is missing a 0.13 Pg/month sink of CO2 in the UTLS. Since the dispersion into the tropical belt is seasonal it might well influence the seasonality of tropical fluxes (as is found for Tropical Asia)

A: This was referring to the passive tracer experiment in which there were no sinks acting on the imposed source. As we mentioned this was just to show the potential impact of a UTLS adjustment on the atmospheric CO_2 burden. We have removed the sentence since it was confusing.

Page 10828, line 13: Other then suggested here, the UTLS sink of 0.13 Pg/month is not small. It is of the same order of magnitude as the regional changes in surface fluxes that result from it.

A: The reviewer is correct. We have changed the wording here.

TECHNICAL CORRECTIONS
page 10825, line 27: "latitudes" i.o. "altitudes".

A: Change to ‘latitude’

page 10827, line 25: I guess “source” should be “sink” here.

A: No, for the passive tracer experiment it is a source. In the inversion we changed it to a sink, reflecting the adjustment in atmospheric CO_2 suggested by the HIPPO data. We have rewritten this description so that it is clearer.

page 10828, line 13: I guess “source” should be “sink” here.

A: No, for the passive tracer experiment it is a source.