Response to referee 3

Interactive comment on “Carbon monoxide climatology derived from the trajectory mapping of global MOZAIC-IAGOS data” by M. Osman et al.

Anonymous Referee #3

Received and published: 10 December 2015

General comments

The authors present a new climatology based on the unique dataset of CO profile observations from MOZAIC and IAGOS measured since 2001. This data is an invaluable complement to the existing global observations from surface stations and satellite retrievals, particularly for the free troposphere and UT/LS regions. An attempt is made to widen the unevenly distributed observations by calculating forward and backward trajectories. As can be seen from the comparison with MOPITT retrievals this works out quite well in most parts of the Northern Hemisphere and the Tropics, while the lack of flight data in the Southern Hemisphere hampers the climatology completeness and probably also its correctness there. The paper structure is sound and the data and methods used for this study are relevant and of importance for global chemistry and air quality monitoring. I’d like to see the manuscript published, if the following basic items will be addressed:

We thank the referee for his thoughtful remarks.

- The influence of the available number of MOZAIC/IAGOS observations on your climatology should be examined in more detail. Particularly for the southern edges of the MOZAIC flight corridors the agreement of forward and backward trajectories and the comparisons to MOPITT seem to be weak as also pointed out in my specific comments.

Can you give an error estimate for each grid box of the climatology simply based on the number of trajectories calculated or give at least a (map) overview of the number of trajectories used in each grid box?

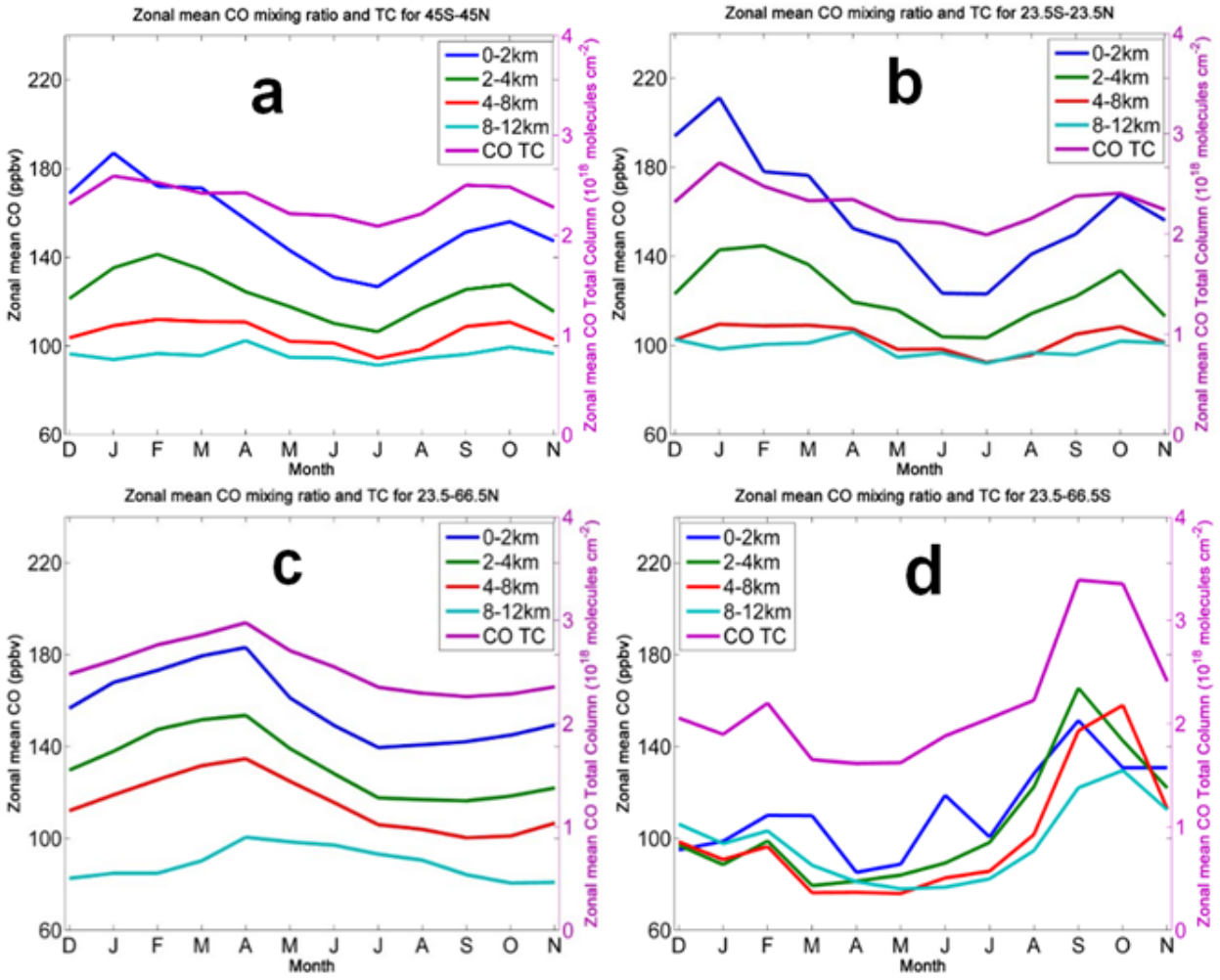
Following the reviewer's suggestion, sample plots of number of samples per grid cell and the standard error of the mean associated with the trajectory-mapping have been added. These are available for each month/year/decade and level in the climatology. We have added text discussing these results as well. We thank the reviewer for drawing our attention to this oversight.

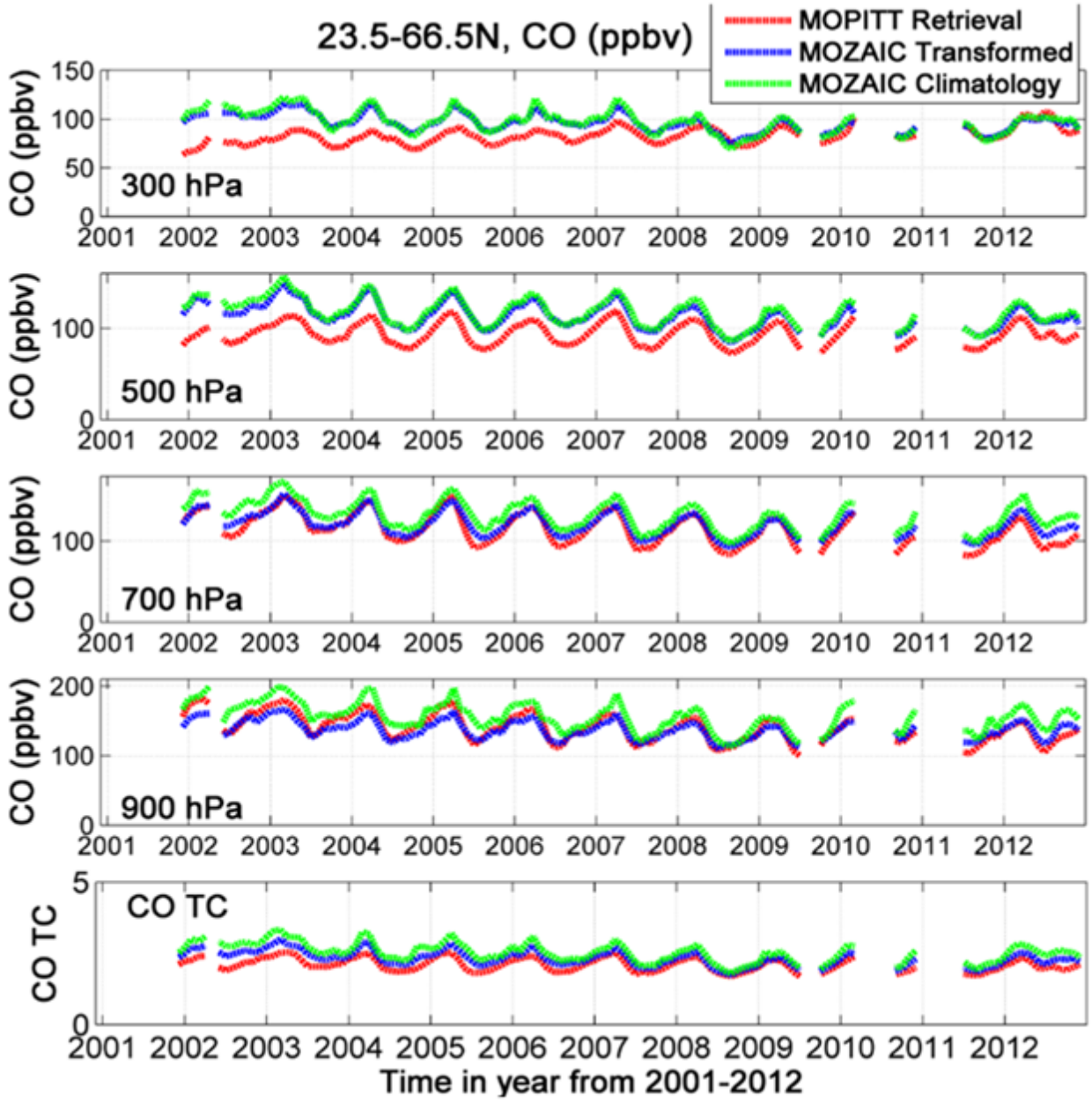
- In several cases it would be more comprehensive to compare zonal averages instead of maps. Moreover the chosen months and years in the Figures often look arbitrary.

We changed Figs. 10 and 11 (now Figs. 12 & 13) to seasonal CO column mean, but have kept Fig. 7 (now Fig. 9) as it is, noting in the text that other months show similar distributions.

Please stick to seasonal climatologies or to a few same examples wherever possible. Total column comparisons to MOPITT could be added to Figures 13 and 17.

Done.





- The manuscript is too long which can be seen most prominently in the number of Figures. I propose to shorten section 6.1 (including Figures S4-S6) significantly as well as the discussion of Figures 19-21, skipping Figures 19-20. Figures 2 and 9 can be minimized and Figures 4 and 5 can be combined into one Figure.

In order to reduce the size of the paper, section 6.2 has been removed from the manuscript. As suggested by the referee, several figures were reduced or removed.

Specific comments

p.29875, l.10-14: You could give upper limits of observed values for these polluted regions. Values can be higher than 1000 ppb (see e.g. Wang et al., 2004)

Added. p.29875, l.10-14: We found the seasonal mean maximum of 1516 ppbv during Fall 2001-2012 in the same region.

p.29877, l.3-5: The sentence only repeats your statement on p.29876, l. 25-28. Please combine.

Done.

p.29877, l.5-7: Please give some short information (or reference) here on how the source distribution of CO can be derived from the O3-CO correlations.

This section has been removed from the paper to reduce the size of the manuscript.

p.29878, l.13: Please insert here that you are using only profile data from takeoff and landing.

OK.

p.29879, l.6-8: Are there any references available for these MOPITT products? What is the version of the total column product?

Deeter et al. (2014) is a reference for MOPITT V6L3.

p.29879, l.19-29: To which versions of the MOPITT retrievals do these bias estimates refer to? What is the expected or reported difference of version 6 retrievals to those products?

As they refer to versions of MOPITT other than V6, we have removed from them from the manuscript as suggested by Referee.#1.

p.29880, l.3: In the abstract you limit the maximum altitude to 14 km. Please clarify.

The maximum altitude of the aircraft at cruise height is about 12 km and the number of advected air parcels reaching altitudes above 14 km is very small. Consequently, we report our data for a maximum altitude of 14 km. In fact, our upper limit is 20 km, but only a few parcels are seen above 14 km.

p.29880, l.2-6: Did you use the exact horizontal position of the airplane during ascent/descent to start the trajectories or did you always start from the airport geographical location? The aircraft position can deviate considerably from the airport position, particularly for higher flight altitudes.

We used the exact location of the aircraft to start the trajectories. Text revised to note this.

p.29882, l.2: Please give the exact path to the CO data. On the given ftp server, several directories would fit.

ftp://es-ee.tor.ec.gc.ca/pub/ftpdt/MOZAIC_output_CO/

p.29883, l.25-26: I don't understand why a horizontal mapping from 1x1 to 5x5 needs a linearity in log pressure and CO mixing ratio. Please expand or omit. Figure 2 could be shortened to one example.

This was an error. We have corrected the text.

p.29884, l.24 – p.29885, l.3: From Figures 3 and S1 I can see quite large differences for many occasions, e.g. regions in Northern Asia or the tropical Atlantic ocean, where differences are higher than 50%. The quality of Figure 3 needs to be improved.

As suggested by the referee, Figure 3 (now Figure 4) has been replotted and the quality improved.

p.29885, l.13: Why did you choose the month of May?

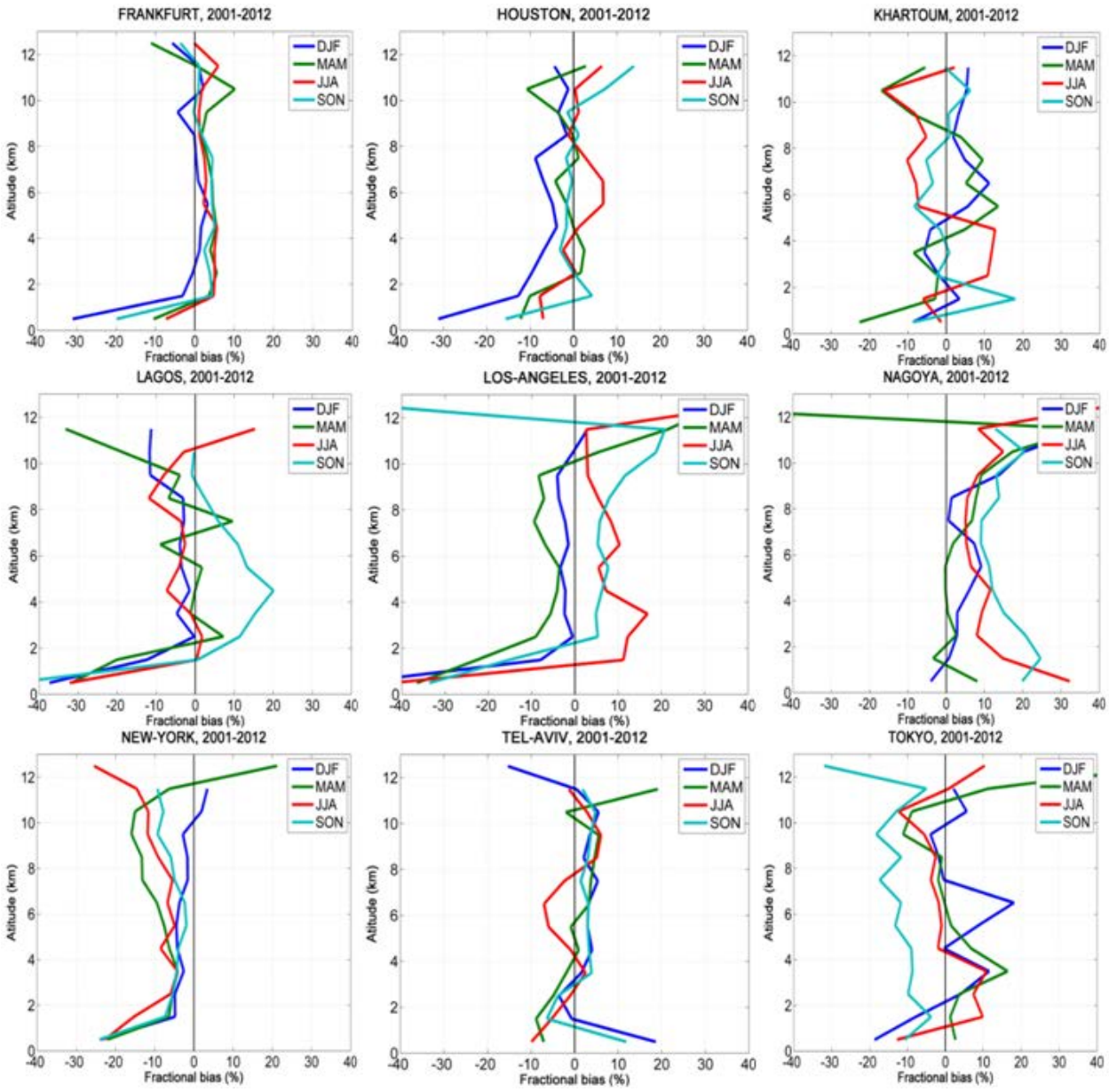
The bias calculations have been done for all months. It does not seem to show seasonal dependence and this can be seen in Fig. 5 (now Fig. 6). As a result, we chose May as an example to show the bias between the trajectory-mapped and the in situ measurement.

p.29885, l.14 – p.29886, l.11 and Figures 4-5: You should consider to combine Figures 4 and 5 to one Figure with less examples. Please give total numbers of profiles which contribute to the mean MOZAIC-IAGOS profiles from the airports shown in the Figures. Airports shown here can be marked additionally in Figure 1. I doubt that all selected airports represent basically different conditions (e.g. Cairo / Tel Aviv or Atlanta / Houston).

Following the reviewer's suggestion, we reduced the number of figures in Fig. 5 (now Fig. 6) to 9 (from 12) by removing those airports which do not represent different atmospheric conditions. We have removed Atlanta, Cairo, and New Delhi.

Airport	latitude	Longitude	Number of profiles
Frankfurt	50.04	8.51	12324
Houston	29.97	-95.35	130
Khartoum	15.59	32.55	118
Lagos	6.57	3.31	265
Los Angeles	33.94	-118.41	280
Nagoya	34.88	136.8	439
New York	40.63	-73.78	978
Tel Aviv	32.02	34.88	341
Tokyo	35.77	140.37	929

Selected airports used in the validations.



p.29886, l.18-20: You should emphasize that the MOZAIC data for the Southern Hemisphere is almost entirely related to flight tracks over the land surfaces. The higher SON values could be mostly related to biomass burning events and the values over the southern oceans may be much lower.

Done.

p.29887, l.12: Which product? V6L3?

That is correct. MOPITT V6L3

p.29888, l.25-27: Correlations in Figure 7 are only shown for January, land and ocean are not distinguished. Even then you report a correlation coefficient of 0.68 for 300 hPa. Thus I can't believe that correlation coefficients of 0.7 are always reached.

Figures 9 and 10 (now Figures 11 & 12) suggest that the mapping does a pretty good job over the oceans, too, as CO is advected long distances in four days (which is also short compared to its lifetime).

p.29888, l.28: Do you mean for height levels above 700 hPa or for levels with pressures above 700 hPa?

We mean (geometric) altitudes above the 700 hPa pressure level. Text revised.

p.29889, l.9-14: What exactly is compared in Figure 8? Is it monthly means of in-situ profiles which afterwards have been transformed using the averaging kernels against monthly means of MOPITT retrievals or was the transformation being done with the single profiles?

The former, monthly means of in-situ profiles have been transformed using the MOPITT averaging kernels and plotted against monthly means of MOPITT retrievals.

p.29889, l.3-19: What are the conclusions you draw from this paragraph?

We have added: |"This implies that the differences at 500 and 300 hPa are not a result of the trajectory mapping."

p.29889, l.24-28: I cannot see the dispersion towards the Arabian peninsula from Figure 9.

Indeed, the transport toward South America is much clearer. We have revised the text.

p.29889, l.28-29: Which region exactly?

In southern Africa. Text corrected.

p. 29890, l.1-14: There is not much new insight for the CO comparison from this paragraph. Also your comments on the MOPITT retrieval refer to an older version. You can skip this.

Agreed.

p.29890, l.17-20: I don't agree with this statement. From Figure S2, there are many regions where MOPITT is lower than MOZAIC. Showing zonal averages instead of maps could help here to quantify.

The statement is quoting the results of averaging the maps shown.

p.29891. Eq. 5: How did you derive this equation? Is there any reference?

It is described in Deeter (2002).

Deeter, M. N. (2002), Calculation and Application of MOPITT (Measurements of Pollution in the Troposphere) Averaging Kernels. Available at http://www.acom.ucar.edu/mopitt/data/avg_krnls_app.pdf. Citation added.

p.29891, l.13-23: Skip the section "The interface . . . column retrievals." or explain why this information is necessary here.

Removed lines 13-23.

p.29892, l.26- p.29893, l.2: Your findings are hard to see from Figure 10. It would be more elucidating to see a climatology of zonal mean values for both data products.

We've replaced this figure with one of seasonal means.

p.29893, l.20-22: Figure 12 shows monthly patterns. Seasonal maps eventually would be sufficient.

We would like to keep it.

p.29893, l.20- p. 29894, l.19: Section 5.1. needs to be reworked. NH concentrations are not higher than SH concentrations for all months, NH biomass burning is negligible in winter. Moreover it is not explained why you find so much CO over the southern oceans, far away from the sources. How strong is your observational basis (even together with trajectory modeling) for these regions? Over Australia and South America there are very few measurements.

We have rewritten this section to be clearer. There are limited MOZAIC-IAGOS data in the SH. The CO over the southern oceans comes from relatively long trajectories as the observations (airports) are far away, and so the associated uncertainties are larger. However, CO has a long lifetime, so it is not unreasonable to find a lot of CO over the southern oceans, far away from the sources. We have compared with global maps of MOPITT CO data, averaged over the same period, and we do not see larger differences in those maps between land and sea. We do see a bias between our results an MOPITT (our values are larger. One puzzling difference is that MOPITT does not show the high values we see between Australia and the southern tip of Africa; MOPITT values are elevated there but not as much as ours. A possible explanation, which we have not investigated, is that MOZAIC-IAGOS data are not evenly distributed in time; if the aircraft sampled some large biomass-burning events these could bias the averages.

p.29894, l.22-24: You can skip either Figure 13a or 13b, basically it shows the same information. Alternatively you could show only one Figure with a comparison to total column CO from MOPITT, but for different latitude bands.

Following the reviewer's suggestion, we have added total CO column for different latitude bands to Fig. 13 (now Fig. 15).

p.28896, l.15-21: As can be seen from the previous figures, large regions of the extratropical SH are not covered by the MOZAIC/IAGOS climatology. It is thus misleading to interpret a zonal average profile from this part of the world. I cannot trust the conclusions you draw from this section.

That is true. It is now noted in the manuscript that the trajectory-mapped CO in the SH extratropics is mainly representative of the tropics, unlike in the NH extratropics.

p.29897, l.5-8: It would be intriguing to add a total column comparison to this interesting Figure 17.

Done.

p.39898, l.1 – p.29899, l.2: Given the length of the manuscript and the limited information content of the section, I'd propose to skip the section completely or at least to shorten it significantly.

We have shortened this somewhat, and we have removed section 6.2 entirely, to reduce the size of the manuscript.

p.29900, l.8-9: Maximum monthly mean values of about 80 ppbv for ozone at 4.5 km seem to be quite high. Is this supported by the literature?

Yes, such values are common at northern mid-latitudes. For example, Liu et al. (2013) have shown monthly mean CO values of about 80 pbbv at 4.5 km. This section has been removed, however.

p.29900, l.12 – p.29901, l.24: This section can be condensed considerably. Your main conclusions can be drawn from Figure 21, without showing Figure 19 and 20.

Agreed. We have removed p.29900, l.12 – p.29901, l.24 since section 6.2 has been removed entirely.

p.29901, l.26: Insert: "as well as interannual variability".

Done.

p.29901, l.29 – p.29902, l.5: Skip the sentence referring to Figure 21. The rest of this paragraph belongs to the conclusions section.

Done. we thank the referee for thoughtful comments.

p.29902, l.17-20: I have problems to agree with this conclusion about the SH maximum. I don't think your results are based on a solid fundament of observations for this region.

Text has been added to reflect the fact that our results in the SH are derived from limited data in the region.

Figure 1: Airports from which profiles are shown later on could be marked here.

O.K.

Figure 2: Show only one example (max. two).

Done.

Figure 4: Combine with Figure 5.

We have reduced Fig. 4 (now Fig 5).

Figure 7: Blue line is missing. The last sentence of the Figure caption can be skipped.

Removed "the blue line is the line of best fit," from the Fig. 7 (now Fig. 9) captions.

Figure 8: mention Frankfurt in the Figure caption.

Done.

Figure 9: e)-f) can be skipped.

These show comparisons in the mid-troposphere.

Figure 11: Why you do not show here a climatology for e. JAN/JUL? Blue line is not explained.

Now the monthly plot is replaced by a seasonal climatology. The blue line is the line of best fit, which is forced to pass through (0,0).

Figure 13: The last sentence of the Figure caption can be skipped.

OK.

Figure 14: How well are the data on the southern edge constrained. Can you compare to MOPITT?

In the SH MOZAIC-IAGOS data are limited. As a result a clear CO trend in the region cannot be observed, unlike the NH. However, the trajectory-mapped CO climatology reflects the major features of the region seen in MOPITT maps, for example, in Fig. 9 (now Fig. 11). However, we cannot compare with higher SH latitudes since there are no available MOZAIC-IAGOS data in that region.

Figures S4-S6 can be skipped when rewriting section 6.1.

Figures S4 & S5 have been removed from the manuscript, but we have kept Fig. S6 (now Fig. S4).

Technical corrections

p.29875, l.2: Give a reference for IPCC AR5.

IPCC, 2013 (IPCC AR5): Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 1535 pp.

p.29875, l.14: The reference Zbinden et al., 2013 is missing in the reference section.

Done.

Zbinden, R. M., V. Thouret, P. Ricaud, F. Carminati1, J.-P. Cammas, P. Nédélec, Climatology of pure tropospheric profiles and column contents of ozone and carbon monoxide using MOZAIC in the mid-northern latitudes (24◦ N to 50◦ N) from 1994 to 2009, doi:10.5194/acp-13-12363-2013, 2013.

p.29875, l.29: NOy is not explained.

NO_y refers to the sum of NO_x plus all compounds that are the products of atmospheric oxidation of NO_x. Text added.

oxidation products p.29879, l.16: The reference Worden et al., 2010 is missing in the reference section.

Done.

Worden, H. M., Deeter, M. N., Edwards, D. P., Gille, J. C., Drummond, J. R., and Ned´ elec, P. P.: Observations ´of near-surface carbon monoxide from space using MOPITT multispectral retrievals, J. Geophys. Res., 115, D18314, doi:10.1029/2010JD014242, 2010.

p.29879, l.16-17: The reference Deeter et al., 2012 is missing in the reference section.

Done.

Worden, H. M., M. N. Deeter, D. P. Edwards, J. C. Gille, J. R. Drummond, and P. P. Nedelec, Observations of near-surface carbon monoxide from space using MOPITT multispectral retrievals, J. Geophys. Res., 115,doi:10.1029/2010JD014242, 2012.

p.29879, l.19: “Lou et al., 2007” should be “Luo et al, 2007”.

Done. Luo et al, 2007

p.29884, l.14: skip “with”.

Done.

p.29884, l.13-14: skip “Eq.” (two times).

Done.

p.29886, l.24: Replace “ a increasing” by “an increasing”.

Done.

p.29889, l.5: The reference Deeter et al., 2010 is missing in the reference section.

Done.

Deeter, M. N., Edwards, D. P., Gille, J. C., Emmons, L. K., Francis, G., Ho, S.-P., Mao, D., Masters, D., Worden, H., Drummond, J. R., and Novelli, P.: The MOPITT Version 4 CO Product: Algorithm Enhancements, Validation, and Long-Term Stability, J. Geophys. Res., 115, D07306, doi:10.1029/2009JD013005, 2010.

p. 29891, l.6: Replace “total column vectors” by “total column operator”.

Done.

p. 29892, l.8: Replace “times” by “time intervals”.

Done.

p. 29892, l.13: Insert “is” in between “which” and “not”.

Done.

p. 29892, l.21: Replace “African” by “Africa”.

Done.

p. 29893, l.20: Skip “As an example,”.

Done.

p. 29899, l.6: Please update this reference in the reference section.

Manuscript is still under preparation and modified to Osman et al., 2016

p. 29899, l.20: Replace “correlation” by “positive correlation”.

Done.

p.29901, l.6: Replace “Bowman” by “Bowman et al.”.

Done.

p.29902, l.25-26: Skip parantheses.

Done.

Figure 2: The quality of the Figure needs to be improved.

Done.

Figure 3: The quality of the Figure needs to be improved.

Done.

Figure 12: The quality of the Figure needs to be improved.

Done.

Figure 14: Add “km” in the Figure caption.

Done.

Figure 15: The quality of the Figure needs to be improved.

Done.

Figure 18: Replace “winter” by “summer” in the Figure caption.

Done.

We thank the referee for his/her careful review and very helpful comments.