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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Midlatitude stratosphere – troposphere exchange as diagnosed by MLS O_3 and MOPITT CO assimilated fields" *by* L. El Amraoui et al.

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First, we really want to thank both reviewers for the excellent report they have provided. Their remarks and comments have been of great importance for us to improve the quality of the paper, and also to give us more insight for further work in particular regarding the characterization of the mixing layer during a STE event.

This paper makes novel use of chemical data assimilation to investigate a stratosphere-troposphere exchange (STE) event. It would be suitable for publication if the following

points are dealt with.

Main points:
1) This paper is a combination of quantitative parts (the validation of analyses against MOZAIC and independent total column ozone and CO) and highly qualitative parts (the assessment of quality of the assimilated ozone and CO fields in the STE event). These need to be clearly distinguished and the results of the qualitative assessment should not form a major part of the abstract or conclusion because such claims are very hard to substantiate. Further, the assessment should be made more quantitative, particularly by (1) applying the MOZAIC, OMI and AIRS validations to the model fields as well as the analsyes. (2) Comparing the analysed and modelled vertical profiles of ozone and CO to the Lerwick sounding, on a graph.

⇒ **Validation results have been reported in the abstract and the conclusion.**
⇒ **The model result has been added for comparison with the assimilation product including the corresponding statistics with regard to MOZAIC, OMI and AIRS observations (see responses to referee 1).**

2) Is there a possible chemical origin for the differences in quality of the ozone and CO analyses? In the case study, CO appears to behave mostly like a tracer, following the PV distribution. If the chemical lifetime of ozone is quite short compared to that of CO in the upper troposphere of MOCAGE, the excessively low ozone values could come simply from an inappropriate chemistry. What are the chemical lifetimes here? Ideally, assimilation runs without chemistry would throw more light on this.

⇒ **The photochemical lifetimes of ozone and CO are relatively long in the UTLS (typically longer than a month for ozone ; several months for CO: see e.g., Shepherd 2007, Duncan et al., 2007). With lifetimes of this order, the model**

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chemistry will have little impact on the O_3 concentration in the UTLS, especially if the ozone is assimilated. Even if the chemistry is not adequately resolved in MOCAGE, ozone has a lifetime long enough (much more than the size of the assimilation time window) to be driven to a realistic state with the help of data assimilation. The validation of this product in the UTLS region by comparison with MOZAIC data is the proof of that.
I can also cite another example in this same context. El Amraoui et al., (2008) have shown through the assimilation of ozone data from Odin/SMR that although the model has a poorly resolved heterogeneous chemistry within the polar vortex where the ozone destruction rates are much more important, data assimilation was able to calibrate the model to a more realistic state in the lower stratosphere. Finally, it should be noted that the comparison of two assimilation runs: one with and one without chemistry will give about the same results because the weight of observations in determining the assimilation state, is much larger than the weight of the model.

Minor points:

1) Abstract - this is a little too long and detailed. It should be shortened. Also, try not to make claims which are not objectively substantiated, e.g. "horizontal distribution of CO is consistent with meteorological analyses" (see point X later). I would prefer to see a statement along the lines of "The CO analyses appear more promising than the ozone analyses in terms of their ability to capture the STE event".

⇒ **The abstract has been shortened and rephrased according to the referee.**

2) p20683, l16: What is the source of the a-priori?

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⇒ **We added a reference (Deeter et al., 2003) concerning the MOPITT CO retrieval algorithm. The reader can referred to this citation in order to have all details about the retrieval of MOPITT data including the origin of the a priori profile.**
More precisely, it is indicated in this reference that : MOPITT employs a fixed global CO a priori for all CO retrievals. This a priori profile is generated from a master set of 525 in-situ profiles measured from aircraft during eight atmospheric chemistry field campaigns and at two fixed sites and Cape Grim, Australia. Typically, these in situ profiles extend from the surface to approximately 400 mbar (the aircraft maximum flight altitude). At higher levels, in situ data were extended vertically with monthly climatology values from the chemical transport model "MOZART".

3) p20683, l19: Is the DFS information used? If not, don't mention it here

⇒ **The sentence is now removed.**

4) Section 2.3: contains much irrelevant (even confusing) detail. Please remove it unless there is a clear link to the subject matter of the current paper. Examples: (a) don't mention of ECMWF fields for use as dynamical forcing since only ARPEGE is used in this study; (b) don't mention of "more realistic advection terms over Africa" unless there is some implication for the present study that you can justify in the text; (c) don't mention the study on transport processes in the tropics unless its conclusions have some bearing on the current work.

⇒ **We removed all irrelevant details and we simplified that part of text according to the suggestion of the referee.**

5) p20686, l14: "strong positive deviation". "strong" is a very subjective word

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and also seems unmerited here. Objectively, you could say that the elveation of ozone amounts is about the same size as the standard deviation.

⇒ **The sentence is now rephrased following the suggestion of the referee.**

6) p20686, l15: "peaking at 250hPa". There is no peak here that I can see. The ozone profile is dominated by the rapid increase in the vertical.

⇒ **The sentence is now rephrased following the suggestion of the referee.**

7) p20686, l15/17: Elevated ozone levels are seen down to "300hPa" low relative humidity values are seen down to "250hPa". In fact, the strat/trop boundary in these terms seems to be in a very similar place in both profiles, at about 270 or 280 hPa. Please be consistent.

⇒ **We are now more consistent, 250 hPa is replaced by 280 hPa.**

8) p20686, l23: "a break in the .. lapse rate at .. 315K isentropic level". Isentropes are not marked on the figure, so please refer to the vertical scale used there, i.e. pressure, not potential temperature.

⇒ **The sentence is rephrased: It refers to the vertical altitude (6km) which corresponds to the isentropic level of about 315 K.**

9) p20687, l12-13: "The PV contours are almost cut off from the stratospheric continuum". There is no obvious "stratospheric continuum" on the plot. What is meant by this?

⇒ **This sentence is now removed.**

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10) p20687, l19: "The tropopause" please define it here in the text (=1.5PVU), not just on the figure.

⇒ **Fixed.**

11) p20689, l8-15: Please make it more clear that the ozone and CO assimilation experiments were entirely separate.

⇒ **Fixed.**

12) p20689, l22: Please give the formula for the chi square test

⇒ **the formula for the chi square test is now in the text (see section 4).**

13) p20690, l10, Fig.4: It is erroneous to plot all the OMF/OMA values on the same histogram. They come from very different height levels (215 to 10hPa), with orders of magnitude difference in the ozone amount and observation error. This figure would only make sense if the departures were normalised by the expected FG departure standard deviation, i.e. sqrt(sigma_o^2 _ sigma_b^2). Also, to say these curves are "nearly gaussian" is completely unsupported. To prove this, fit a gaussian to the departures and show it on figure4.

⇒ **This figure has been completely changed. The OMF distribution has been normalized with respect to the corresponding observation errors. The corresponding histogram is fitted by a Gaussian function. The good agreement between OMF histogram and the corresponding Gaussian fit function supports the assumption that observations and forecasts have Gaussian errors.**
Besides, the mean of OMF/OMA distributions have been plotted as a function of

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the vertical pressure in order to evaluate the improvement of the assimilation process in the different observation levels. The corresponding STD.DEV of the OMF/OMA mean is also plotted as a function of the pressure level. Figure 4b,4c clearly demonstrate the improvement of the model after the assimilation process, since the mean of OMA is closer to zero than that of OMF at all pressure levels with a corresponding STD.DEV which is also small than that of OMF.

14) p20691, l15: Please explain what this time-series represents. How many aircraft have been averaged in a day? What geographical region does it relate to? Why does the ozone field vary in time - what does this represent in geophysical terms? Or is it just a funcion of where the aircraft happen to be?

⇒ **The comparison between MOZAIC and O_3/CO assimilated fields in figure5 and figure9 is done in terms of times series. Collocated MOZAIC observations and assimilated field are averaged over a constant time bin (e.g. one minute) whatever the number of aircraft and position of the MOZAIC measurements. Each average is then plotted as a function of the day time. Assimilated fields are plotted with respect to their STD.DEV. This has been added in the paper. However, the number of aircraft as well as vertical profiles varies from one day to another, It is therefore difficult to report this information over one month of comparison.**

15) p20691, l20: What are the bias, rms, and correlation for the model runs against MOZAIC? This would give a more quantitative basis for any claim that the analyses are better than the model.

⇒ **Fixed.**

16) p20692, l18: Why does varying background error have no effect? I can only

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assume it is because observation error is very much larger in magnitude. Please explain in the text.

⇒ **That is true; this is indicated in the text, section 4.2: "Thus, the low value of chi2 is most likely due to the overestimation of the error covariance matrices of MOPITT observations."**

17) p20692, l26, Fig. 7: As point 13.

⇒ **Fixed: Same as for point 13.**

18) p20693, l15: the "error bars" here are not error bars at all, but the 1 standard deviation range of the field.

⇒ **Fixed.**

19) p20694, l14, Fig. 10: Please say in the text which date and averaging period have been used for the comparison with AIRS.

⇒ **AIRS observations used in this figure (now Figure 11) correspond to the average of 15 and 16 August 2007 data binned in 2°x2° boxes. We clarified this in the manuscript and in the figure caption.**

20) p20695, l15-16: "there is no evidence of an ozone maximum in the 300-200hPa layer" Why should we expect to see a maximum? We're looking for a "positive ozone anomaly". It would really help to show the Lerwick ozonesonde profile and the analyses on a figure.

⇒ **Fixed: see the added figure (Figure 7).**

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21) p20695, l27: "This can be explained by..." it's a fact that O_3 is not available below 215hPa but it is pure speculation that it "does not propagate below 200-300hPa". As mentioned in main point 2, one can also speculate a chemical explanation here.

⇒ **We added a new figure (Figure 7) showing the three vertical profiles (ozonesonde, model and O_3 assimilation. This new figure clearly supports the improvement of the model via the assimilation, especially between 300 and 150 hPa. Below 300hPa, the improvement of the O_3 assimilation is limited. The assimilated O_3 is not capable to detect the tropopause folding at 400 hPa.**

22) p20697, l15: "a relative minimum of CO... over western Spain to Brittany". There is a much larger, more obvious low CO feature over eastern spain and the Mediterranean that does not agree at all with the PV fields. Please discuss this in the text. It would suggest that there are remaining uncertainties in the CO analyses.

⇒ **A minimum of CO is not necessarily of stratospheric origin, and therefore does not necessarily correspond to a maximum of PV.**
The distribution of MOPITT CO data for 15 August 2007 (not shown), shows that there are no data over Spain and over the Mediterranean during this day. However, over Spain, the model also shows a minimum, this may therefore be the cause of the minimum reproduced in the assimilated product over this area. Over the Mediterranean, the model shows no minimum in this region. Moreover, in the O_3 distribution (now Figure 13) there is no O_3 maximum occurring in this region. So the CO minimum which appears in the assimilated field over the Mediterranean could be due to a phenomenon of horizontal transport from another air mass. We think that determining the origin of this minimum is beyond the scope of this paper. Moreover, the a posteriori self-diagnostics as well as the comparison with other independent data (mainly with MOZAIC in the

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UTLS) have shown that the assimilation product is good.

23) p20697, l26: " assimilation ... is a very efficient tool.." - this statement appears to contradict the conclusions of the paper, and would best be removed.

⇒ **Fixed.**

Other points (style, grammar, technical, etc.)

1) p20679, l9-10: Remove "The" and "across the tropopause" to improve this sentence.

⇒ **Fixed.**

2) p20679, l26: "In the STT events..." The reader has just been told where TST events occur (the extra-tropical tropopause) and is now expecting to be told where STT events occur. The text does not say, but should do.
⇒ **The sentence is now rephrased and contains more details about STT events.**

3) p20680, l17: "theses" -> "these"

⇒ **Fixed.**

4) p20680, l24: "low RH" + and + " high O_3, PV ..."

⇒ **Fixed.**

5) p20681, l9-10 "...high vertical gradients, which is a well-known limitation... of CTMs." High vertical gradients are not the limtiation. Presumably the limitaiton is their ability to represent them.

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⇒ **Fixed : the sentence is now rephrased.**

6) p20681, l21 remove "has"

⇒ **Fixed.**

7) p20688, l7 "place" -> level?

⇒ **Fixed.**

8) p20691, l18 "distribution" -> amount?

⇒ **we replaced distribution by concentration.**

9) Figs. 2, 11, 13 - the thick black line is invisible against the blue background. A more visible colour should be used.

⇒ **The thick black line in these figures was replaced by a white thick line.**

10) Figs. 5, 8 - there are numerous periods of missing data which have been crossed by straight lines. This makes the plot misleading. No line should be shown in periods of missing data.

⇒ **The figure have been updated following the recommendation of the referee.**

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References:

- Duncan, B. N., Strahan, S. E., Yoshida, Y., Steenrod, S. D., and Livesey, N.: Model study of the cross tropopause transport of biomass burning pollution, Atmos. Chem. Phys., 7, 3713–3736, 2007.
- El Amraoui, L., Peuch, V.-H., Ricaud, P., Massart, S., Semane, N., Teyssèdre, H., Cariolle, D., and Karcher, F.: Ozone loss in the 2002/03 Arctic vortex deduced from the Assimilation of Odin/SMR O_3 and N2O measurements: N_2O as a dynamical tracer, Q. J. Roy. Meteor. Soc., 134, 217–228, 2008.
- Shepherd, T. G.: Transport in the Middle Atmosphere, Journal of the Meteorological Society of Japan, 85B, 165–191, 2007.

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