**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.

Anonymous Referee #2

Received and published: 26 November 2009

Review of "Midlatitude stratosphere -troposphere exchange..." by El Amraoui et al.

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.

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.

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".

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

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

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.

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5) p20686, l14: "strong positive deviation". "strong" is a very subjective word and also seems unmerited here. Objectively, you could say that the elveation of ozone amounts is about the same size as the standard deviation.

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.

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.

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.

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?

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

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

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

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_oˆ2 _ sigma_bˆ2). Also, to say these curves are "nearly gaussian" is completely unsupported. To prove this, fit a gaussian to the departures

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and show it on figure 4.

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?

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.

16) p20692, l18: Why does varying background error have no effect? I can only assume it is because observation error is very much larger in magnitude. Please explain in the text.

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

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

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

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.

21) p20695, l27: "This can be explained by..." it's a fact that O3 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.

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

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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.

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

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

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

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.

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

4) p20680, l24: "low RH" + and + " high O3, PV ..."

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.

6) p20681, l21 remove "has"

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

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

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20677, 2009.

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