

Interactive comment on
**“Stratosphere-troposphere ozone exchange from
high resolution MLS ozone analyses” by J. Barré
et al.**

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General comments

Comment 1:

I am not convinced that the low-resolution simulations (LR) need to be discussed throughout the manuscript. In fact, the basic message becomes clear if only the high resolution (HR) simulation is discussed. The message is: There is a significant improvement of UTLS ozone fields if MLS ozone analyses are used, compared to free

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MOCAGE runs. The reader might even be distracted from this main message if he/she always has to read about LR/HR differences - in particular because these differences are not very surprising!

In this paper, we want to compare the performance of the model at two different resolutions (2° and 0.2°). To explain the performance at the higher resolution, we think it is important to refer to the performance at the lower resolution. Our low resolution set up is quite typical for chemical data assimilation papers currently in the literature (except for those addressing surface air quality). We take onboard the referee comment but still think that it is important both resolutions are included in the discussion for the reasons mentioned immediately above.

Comment 2:

Furthermore, the manuscript could be more clearly structured. At present the sections are chosen according to the type of analysis, and within these sections the cases I and II are discussed. Personally, I would strongly prefer a structure according to the cases, hence avoiding the need to switch always between the two cases. Of course this only reflects a personal preference! However, there are other structural deficits which need to be addressed. For instance, section 4.5 starts again with methodology, but this should already be done so in section 2. Or, in section 4 discussions and interpretations at several places "intervene" between results from the study, e.g. the second paragraph on page 33437. And as a final example: section 4 is entitled "Results" and presents the two case studies. But then follows section 5, which still presents results! In general, a clear splitting between results and a discussion would be much clearer.

We agree with this suggestion from the referee. As a result, we have completely re-structured section 4, which now follows the suggested outline. The revised structure of the paper is now:

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1. Introduction
2. Methodology
 - 2.1. CTM model
 - 2.2. Data assimilation system
 - 2.3. Aura/MLS ozone observations
 - 2.4. Ozone flux diagnostic
3. MLS assimilation
4. Meteorological situation and ozone analysis fields
 - 4.1. Filamentary processes
 - 4.2. Case 1: stratospheric intrusion on 23 June 2009
 - 4.2.1. Meteorological situation
 - 4.2.2. Comparisons between the low resolution and the high resolution runs
 - 4.2.3. Ozone analysis fields
 - 4.2.4. Comparisons with independent data sets
 - 4.3. Case 2 : stratospheric intrusion on 17 July 2009
 - 4.3.1. Meteorological situation
 - 4.3.2. Comparisons between the low resolution and the high resolution runs
 - 4.3.3. Ozone analysis fields
 - 4.3.4. Comparisons with independent data sets
 - 4.4. Impact on the troposphere
5. Discussion
 - 5.1. Stratosphere-troposphere ozone fluxes
 - 5.2. Backward trajectories: impact of high resolution
 - 5.3. Ozone forecasts at high resolution: impact of MLS ozone analyses
6. Conclusion

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Comment 3:

At two parts, the troposphere comes into play. This is a slight lack of focus because it deviates from the main focus (UTLS), and I could not perfectly follow the argument. For instance, in section 4.4.3 the enhanced positive tropospheric bias of the MLS analysis compared to the free run is discussed. It is stated that the enhanced ozone bias in the troposphere is found because too much stratospheric ozone is advected through the tropopause and that this originates from too smooth ozone gradients. In short, some clarification is needed: I could not follow the argument.

We believe that, in order to describe in a comprehensive way the phenomena occurring in the case under consideration, we need to refer to both the UTLS and the troposphere. We clarified the text in order to address the referee's second point (regarding the advection of stratospheric ozone into the troposphere; see section 3.5 in the revised paper).

Comment 4:

The troposphere is also the topic of section 5.1. Here, backward trajectories are calculated. However, the motivation for these calculations remain somewhat unclear, at least to me. So, the whole section 5.1. starts with some technical details, but no motivating background is given. This should clearly be added. Finally note that the final sentence of section 5 is "studying the tropospheric ozone budget is out of scope of this paper". This is fair enough! However, then I wonder whether the short contributions dealing with the troposphere should be included at all.

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We now provide the motivation for section 5.1, which is now section 4.2 entitled "backward trajectories: impact of high resolution".

Comment 5:

Some relevant literature is missing: For instance, the Wei method and its limitations was critically discussed in: - Wirth, V. and Egger, J. (1999), Diagnosing extratropical synoptic-scale stratosphere-troposphere exchange: A case study. Quarterly Journal of the Royal Meteorological Society, 125: 635–655. doi: 10.1002/qj.49712555413 I think that the authors must include a paragraph where they justify the calculations based on the Wei method, simply because there are more robust methods for STE calculations. Note that the Wei formula gets into trouble if there are step gradients or where there are multiple tropopauses, i.e. exactly in regions which are of particular interest for STE.

We have added the reference suggested by the referee. We now justify why we use the Wei method (see section 2.4 in the revised paper); this is indeed a useful addition to the manuscript.

Finally note that the manuscript needs some improvement with respect to language. Myself not having English as first language, I would encourage the authors to have proof-read by a native English speaker. Possibly, several unclear points can be removed by a streamlined language!

The text has been proof-read by a native English speaker.

Specific comments

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1) In the text and the figure captions reference to the different panels is made by means of "right and side", "left hand side",... Readability would be increased if panels are referred to by different labels: Fig.1a, Fig.1b.

Fixed wherever possible.

2) P33421, L4: What is a significantly high value of PV? Without context, one cannot speak of "significant" PV values!

"significantly" has been removed from the sentence.

3) P33421, L19: Which "methods of calculation" are meant?

Details have been added in the text.

4) At several places it is stated that the MLS sounder is able to detect stratospheric profiles between 215 and 0.46 hPa. I wonder a little how the MLS is able to improve the ozone at the UTLS because 215 hPa is already quite high and situated mainly in the stratosphere. But maybe my wondering comes from not knowing too much about satellites?!

Please refer to the reply to comment 2 from referee 1.

5) P33423,L26-P33424,L2: Some remarks are made about further possibilities of MOCAGE. But as far as I can see, these possibilities are not relevant for the present study. So they should be omitted.

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We improved the sentence and these possibilities are discussed in section 4.2: "Backward trajectories: impact of high resolution".

6) P33426,L8-10: Difficult to understand during first reading: please reformulate.

Fixed.

7) Fig.1 "Zonals means for ..." The figure caption should give all needed pieces of information. Please clarify: Zonals means of what?

Fixed.

8) P33427, L17: streamers -> potential vorticity (PV) streamers. In addition to the referenced literature, more recent, relevant studies could be cited. For instance, the following two which compile a climatology of PV streamers and quantify the link to STE:

- Sprenger, Michael, Heini Wernli, Michel Bourqui, 2007: Stratosphere– Troposphere Exchange and Its Relation to Potential Vorticity Streamers and Cutoffs near the Extratropical Tropopause. J. Atmos. Sci., 64, 1587–1602.doi: <http://dx.doi.org/10.1175/JAS3911.1>

- Wernli, Heini, Michael Sprenger, 2007: Identification and ERA-15 Climatology of Potential Vorticity Streamers and Cutoffs near the Extratropical Tropopause. J. Atmos. Sci., 64, 1569–1586. doi: <http://dx.doi.org/10.1175/JAS3912.1>

References added in the text.

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Note also that the formulation is not "optimal". PV streamers are not generated by the Rossby wave breaking (RWB) at mid latitudes. They are more a manifestation of RWB, i.e. RWB in the PV perspective.

The statement has been reformulated.

Furthermore, the formulation "Streamers are considered as an irreversible isentropic process" is not correct. Streamers are not a process! Please reformulate.

The statement has been reformulated. All references to processes concerning the streamers have been replaced by 'event'.

9) P33428,L7-9. Please add a reference for this statement regarding deep convection.

The sentence has been removed.

10) Fig.3: The figure caption is incomplete. At what levels are the PV maps shown?

Fixed.

300 hPa? The description of the white-dashed line is rather "bulky". Please consider reformulating into "The white dashed line shows the position of the vertical cross section".

The white dashed line gives the relative position between the left and right panels.

Finally, I wonder why the horizontal winds are shown in panel b). They are not discussed in the text and I am not convinced that these panels are needed.

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The winds are shown to provide insight on the meteorological situation during the events in order to identify the type of streamer studied.

10) Section 4.2.2.: This case is described as a type II streamer according to Thorncroft et al (1993). I think that this is not completely clear. The PV streamer is still rolling-up in the same direction as the one in case I. Or am I miss-reading the figure?

The situation shown for this case is quite early in the time-period of the event studied. The orientation of the streamer is NE-SW (see figures 3, 5 in the discussion paper and figures 3, 4 in the revised paper). We looked at the last three days before this meteorological situation and the situation was first cyclonic and then changed into an anticyclonic circulation. This is why we label this streamer as type I following the definition from Thorncroft et al (1993): "The type I streamer starts with a cyclonic type of behaviour and changes to a predominantly anticyclonic type, with a transition period when both cyclonic and anticyclonic behaviour are conspicuous.". The text is now clarified in section 3.3.1.

11) P33430, L6-7: "White solid lines identify the 2 PVU iso-line, which is often used to define the dynamical tropopause". This information should come much earlier. Note that the 2-PVU isoline is already used in Fig.2.

Fixed.

12) Fig.7: The figure labels are far too small. Furthermore, The position starts at 0 and ends at 1900, but only a part of this whole flight is really needed for the discussion. In short: Only the observed time period relevant for the study should be shown! I would also prefer if ozone concentrations and flight altitude are not shown in the same panel. This would reduce the already too large number of lines in the figure.

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The figure labels are now bigger. It is important to show the tropospheric and the stratospheric part of the flight and not just the region of interest. The tropospheric flight measurements show the tropospheric bias discussed in section 4.4. We choose to show the flight altitude on the same figure to reduce the total number of figures, which is already large. If the editor wants us not to show the altitude on the same figure, we will do so.

13) P33435, L18-23: the position of the box should be shown.

We opted for not showing the boxes in order to reduce the total number of figures. If the editor thinks that a new figure showing the boxes should be added, we will do so.

14) Fig.11: Possibly I missed it. But where are the error bars around each STE flux determined? According to formula (1) on P33435 the uncertainty must come from the individual terms: [O3], ... But it is not clear how this is done?!

Please see our reply to comment 3 from referee 3.

15) Section 4.5.2 starts with a repetition of what was already said in the previous section, two paragraphs before! I think the whole text needs a careful streamlining in this respect, i.e. in avoiding unnecessary repetitions...

Fixed.

16) P33438, L11-12: "The global domain is used to constrain the regional domain". What is meant by "constraining"?

This text has been clarified.

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