**Atmospheric
Chemistry
and Physics
Discussions**

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

Anonymous Referee #2

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Barre et al. present two cases studies of stratosphere-troposghere exchange (STE), with special focus on the impact of high-resolution MLS ozone analyses. There are many interesting aspects in the study; however, there are also several aspects which need to be addressed before the manuscript is suitable for publication in ACP.

MAJOR CONCERNS:

i) 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

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 diffreneces are not very surprising!

ii) 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.

iii) 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 folllow the argument.

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 setence 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.

iv) 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 steep gradients or where there are multiple tropopausees, i.e. exactly in regions which are of particular interest for STE.

Finally note that the manuscript needs some improvement with respect to language. Myself not having English as first langauge, 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!

MINOR COMMENTS:

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 refered to by different labels: Fig.1a, Fig.1b.

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

3) P33421, L19: Which 'mehods of calculation' are meant?

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

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the stratosphere. But maybe my wondering comes from not knowing too much about satellites?!

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

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

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

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 Cut-offs 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

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.

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

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

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

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.

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?

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.

12) Fig.7: The figure lables 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 oserrved 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.

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

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?!

15) Section 4.5.2 starts with a repetitiuon 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...

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

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