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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 #3

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Review of the manuscript Stratosphere-troposphere ozone exchange from high resolution MLS ozone analyses by J. Barré et al.

The manuscript presents a study on Stratosphere-Troposphere Exchange (STE) using the CTM MOCAGE at two horizontal resolutions driven by ARPEGE winds, with and without MLS ozone data assimilation. It suggests that ozone peaks in tropopause folds are better represented in the high resolution case with the assimilation of ozone satellite data, when compared to in-situ ozone measurements. It then calculates the ozone flux across the tropopause using the Wei 1987 method. In my opinion, the manuscript has several major issues that need to be addressed before it can be considered for

publication.

Major comments:

1. Vertical resolution in the CTM: It says on p. 33423 l.9 that the vertical resolution in the UTLS is about 800m. Since it does not say that this resolution varies together with the horizontal resolution, I suppose that it is the same for both horizontal resolutions (2 and 0.2 degrees, respectively). The high horizontal resolution roughly corresponds to a 20km grid cell in the horizontal. In this case, the ratio horizontal/vertical is around 20 for the grid while it is around 100 in the flow. Horizontal motions are over-resolved with respect to vertical motions. In a semi-Lagrangian advection scheme, this means that the advection will be overly diffusive in the vertical as compared to the horizontal. As a result, I am not convinced that the high resolution mode can accurately resolve the small scale filaments (and ozone STE), since these are usually developing along isentropes that are not following the model levels (especially around high PV anomalies). Why use such a configuration in the CTM?

2. The vertical resolution in MLS is 3km in the UTLS, and therefore assimilation increments are likely to be blind to the detailed vertical structure in the ozone around the tropopause which the CTM produces. Since the CTM has a low bias in ozone in the lowermost stratosphere (see the peaks between 200 and 100hPa in Figs. 8, 10, 13), it is most probable that these increments will erroneously add ozone below the tropopause as well, which will increase the background ozone in the free troposphere. I think this is what Fig. 1 suggests: With zero ozone increments below 215hPa (since no data is assimilated below this level), the analysis still sees a significant increase in the free tropospheric ozone concentration. At least part of it is due to artificial injection of ozone from data assimilation. Figs. 8, 10, 13 all show that this free tropospheric ozone is higher in the analyses than in the forecasts or in observations. Furthermore, this erroneous injection of ozone in the troposphere by way of assimilation increments is disconnected (and can not be estimated) from the ozone STE flux estimated using the Wei formula.

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3. The Wei (1987) formula has been shown to lead to a problem of cancellation between the terms in equation (1), with the result being a small residual compared to individual terms. Since the estimates of individual terms are subject to errors (derivatives taken on finite spatio-temporal resolution data), the relative error on the residual is likely very large (e.g. Wirth and Egger, 1999). This is especially of concern with the low vertical and temporal resolutions used here. It would be good to show maps of the ozone STE fluxes to show how noisy your estimates are. In Fig. 11, error bars are provided but there is no explanation in the manuscript about how they are calculated. A stratospheric tracer would give more reliable estimates than the Wei method.

4. With these comments in mind, I conclude that the assimilation only helps to correct the low bias in the lowermost stratosphere (see the peaks between 200 and 100hPa in Figs. 8, 10, 13), but does not help with any laminae structures found in the profiles (would be better seen using mixing ratios in Figs. 8, 10 and 13), and produces a concerning positive bias in the free troposphere. It does not help either with ozone STE fluxes.

Minor comments:

p. 33421, l.5: How can an intrusion bring high PV in the troposphere if you use PV as the tropopause definition?

l. 11: PV is not quasi-conserved in the UTLS region.

p.33424, l.11-15: There is an error with the 1 June 2009 date.

l.21-22: Needs to be clarified.

p.33426, l.9-10: Needs to be clarified.

l.15-19: Needs to be clarified.

l. 20: Not only advection, but also includes artificial injection of ozone due to data assimilation (see comment 2 above)

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p.33427, l.4: dynamical tropopause, not tropopause height estimation

l.6: it is simplistic to say that STE is an irreversible isentropic process

l.25: What is middle-scale?

Section 4.1: Does not belong to Results.

p.33428 l.3: What is a PV height anomaly?

l.17: What do you mean with 'high PV'?

l.18: Clarify 'anticyclonic curve'

l.23-25: Circular argument: In fact, a positive PV anomaly is associated with a cyclonic circulation

p.33429, l.9: What do you mean with 'south tip'?

Section 4.2.3 is trivial – could be removed.

p.33430, l.12-14: Strange statement! I would expect the free forecast to have a better alignment between ozone and PV. How do you quantify this alignment? Why is that a representing an added value to the ozone distribution?

l.20-23: Trivial.

p.33431, l.28: This conclusion is incomplete, a mention on the increased ozone bias in the free troposphere is needed.

p.33433, l.21-22: I disagree with this statement. The horizontal resolution is not limiting here, but the vertical resolution is, and so is the vertical resolution of MLS observations.

p.33434, l.2: It is suggested that advection is responsible for the ozone bias in the troposphere, but the assimilation artificially injects ozone as well!

l.11: The artificial injection of ozone by data assimilation is not included in the ozone flux estimates.

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Interactive Comment

Section 5: Needs rewriting. I have not understood what is done here.

Conclusions section: Needs to be corrected according to the comments above.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 33419, 2011.

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