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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.

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General comments

Comment 1:

Of primary concern is the "disconnect" between the dynamics/transport and analysis ozone fields and the interpretation in the context of the Wei STE diagnostic. The parenthesized term in Eqn. 1 represents the total mass flux through the tropopause. For a given model resolution, this term does not change in the CTM. I don't believe it should change much for the different resolutions either since the ARPEGE meteorological fields from which this term is ultimately calculated are the same and simply interpolated to the CTM resolution. The transport fields remain the same between the various simulations. The difference in the calculated ozone flux must be solely due to the change in the ozone mixing ratio on the tropopause due to the assimilation.

The diagnostic ozone flux could be affected in three different ways: 1) physical effects due to changes in horizontal resolution; 2) numerical effects due to changes in horizontal resolution; and 3) changes in the ozone field through the assimilation of ozone data. For the 2^o resolution, interpolation effects are certainly important, while in the 0.2^o resolution case, they should be much smaller because the ARPEGE and MOCAGE horizontal and vertical resolutions approximately match. The ARPEGE meteorological analyses used are computed on a stretched T798 spectral grid, providing an horizontal resolution of around 10.5 km over France and of 60 km over the South Pacific. The resolution of ARPEGE meteorological analyses over Europe is thus in the range of the resolution of the MOCAGE high resolution domain (0.2^o, thus 10 to 20 km). Also, at 0.2^o we have better resolved flows in the meteorological analyses than it is possible at 2^o. This is why we support the idea that the flux diagnostic calculation at 0.2^o is more realistic than the one obtained with the 2^o horizontal resolution. In addition, the ozone assimilation process also changes the ozone flux by providing better-resolved information on the ozone field itself. The revised text will clarify this point.

Comment 2:

The authors show that the ozone mixing ratio is too small in the lower stratosphere in the free running model. This must be from either too much chemical loss in the CTM, the net extratropical downward transport is too weak from the driving meteorological fields, or from the boundary conditions at the model top. Given the results in Figure 1, I would hazard a guess that it is due to the downward transport. This suggests that

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the total mass transport across the tropopause is also possibly too weak, and if so, also suggests that the ozone flux estimates are biased low. In the free running experiments, the ozone fields remain "connected" or consistent with the transport fields. The increments added in analysis produce ozone fields that are not truly consistent with the driving fields. I understand that this is the "nature of the beast" when using assimilation in these CTM experiments. However, any increase in ozone flux within the assimilation domain can arise from the analysis increments and not "transport". The authors need to be careful to address these topics in their descriptions and interpretations rather than simply discussing differences in "transport".

In our set-up, and considering the characteristics of the observations assimilated, the ozone fluxes through the dynamical tropopause are not only modified by "instantaneous" assimilation increments in the ozone field brought by "fresh" observations. Another contribution to the modification of the ozone flux is due to the advection of assimilation increments from all the previous assimilation windows, which propagate from the lower stratosphere (250 hPa and above) down to the troposphere. On figure 1 (left), assimilation increments, averaged for the month of July 2009, are shown as a function of altitude: clearly, there are no assimilation increments below 250 hPa. Figures 3, 4, 7 and 8 in the revised paper (3, 4, 5 and 6 in the discussion paper) depict the altitude of the dynamical tropopause (as defined by the 2 PVU isoline, plotted in white) for the two resolutions studied here. At the location of the Stratosphere Troposphere Exchange (STE) filamentary structures, the 2 PVU level is between 200hPa and 400hPa. In places where the dynamical tropopause is relatively high (near 200 hPa), the increase of the ozone amounts could very well be due to the effect of an "instantaneous" assimilation increment. However, where the dynamical tropopause is relatively low (near 400hPa), and where the STEs actually takes place, the flux increase cannot be the direct effect of the data which has just been assimilated. The validation with independent data (see figures 5, 9 in the discussion paper and figure 7, 9 in the revised paper) shows that the STE structures near 300 hPa are improved by *the assimilation procedure. We thus show that the filamentary structures associated with the STE event studied are also improved, and that this is due to the advection of the assimilation increments, initially higher in altitude. We clarify the text in response to the referee comments.*

Comment 3:

I also did not follow the author's reasoning of why the ozone analyses produced a positive bias throughout the troposphere in the vicinity of the filaments. The assimilation adds increments down to 215 hPa and I can see how the gradients can get smoothed when the resolution of the model is insufficient compared to the scale of the features at these levels. Yet, these features will not get "smoothed" from the analysis below the assimilation domain. A better description of the reasoning needs to be presented on this subject.

Although there are no assimilation increments below 215 hPa, the information provided by these increments is transported in the background model field at all subsequent time steps and, in particular, it reaches the troposphere -where it cannot be directly modified by the assimilation of new data. Therefore, there is an impact from assimilation in the ozone field below 215 hPa. We clarify this point in the text.

Specific comments

P33427 L5-7, Not all STE is a result of irreversible isentropic processes. The authors even acknowledge this in the last sentence of section 4.1.

Fixed.

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Figures 3-6 and related text, I assume the horizontal plots are on a theta surface. What surface is this?

The horizontal plots are for a model level situated in the UTLS; MOCAGE uses (sigma, pressure) hybrid vertical coordinates and this level corresponds approximately to 300 hPa over the European domain. This will be added in the caption and in the text.

P33430 L18-19, The filament IS visible in ozone in the LR case. However, it is more clearly defined with sharper gradients and is defined by the 2 PVU contour in the HR case.

The text has been modified following the referee's comment.

P33431 L3-5, How is this conclusion reached? The authors do not give any evidence here for this statement.

This sentence is now removed.

P33438 Section 5.1, This appears contradictory to the statements above where the HR run results in less STE. A comment on this is needed.

A comment at the end of the new section 5.2 has been added.

Technical corrections

P33431 L22, I assume the authors intend "5 E"?

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Fixed

P33437 L9-11, Forgot the "10" in the exponents.

Fixed

P33438, L1, should be "greater" not "increasing" which implies getting larger over time.

Fixed

P33438, L14-16, The English usage should be corrected.

The English usage has been improved.

P33440, L20-21, ":represent [the] strong gradients: "

Fixed

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Interactive Comment