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Atmospheric Chemistry and Physics Discussions

Interactive comment on "MIPAS measurements of upper tropospheric C_2H_6 and O_3 during the Southern hemispheric biomass burning season in 2003" *by* T. von Clarmann et al.

T. von Clarmann et al.

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The authors would like to thank Dr Simon Chabrillat for his helpful comments. In this reply, the original comments are inserted in *italics*.
Since all issues of the general comment are specified in the specific comments, we provide a point-by-point reply only to the specific comments.

The abstract is too short. It should mention the large uncertainty on the absolute values for the ethane retrieval, the use of CFC-11 to filter out lower stratospheric masses, and the discussion on the possible causes for different chemical compositions in the biomass burning plumes originating from tropical America and Africa.

The requested information will be included in the abstract.

Section 2.1. The observations are averaged in latitude-longitude bins in order to increase their significance. The statistical significance of the averaged dataset should be outlined. This could be easily achieved with the addition of a simple map figure, showing the number of (ethane) retrievals averaged in each latitude-longitude bin.

The significance of the mean values of latitude-longitude bins does not only depend on the number of single measurements but also on the variability within the averaged sample. Thus, instead presenting the number of retrievals, we will present maps of the standard errors of the mean values. We feel that this is both more appropriate and more reader-friendly.

Section 2.2. There is no attempt to compare the MIPAS observations of upper tropospheric ozone with other observations. This should be corrected. The most useful kind of data, for this purpose, could be the maps of tropospheric column ozone (TCO) obtained by the Tropospheric Ozone Residual technique (Fishman et al, ACP, p. 1453, 2003). There seems to be a data gap between the TOMS/SBUV products (until 2001), GOME-based products (until June 2003, www.temis.nl) and the OMI/MLS products (since September 2004). If this is confirmed, it should be mentioned in the text because it increases the value of the ozone retrieval. For October and November 2004, the TCO obtained from OMI/MLS (fig.5a in Ziemke et al, JGR, 2006) shows a clear plume extending from tropical America, which contradicts the findings presented here for year 2003. This could be mentioned in the discussion.

A comparison to the Fishman at al data will be included. We will include a statement that no co-incident tropospheric O_3 data from other satellite instruments have been found. The different findings of Ziemke et al. will be mentioned in the discussion. By the way: Gloudemans et al, GRL, 2006 report on large interannual variability in

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emissions between 2003 and 2004. While this refers to CO, this still might also explain differences in O_3. This reference will also included.

Sections 2.3 and 3.4. It makes lots of sense to use MIPAS observations of a tracer to discriminate between "tropospheric" and "stratospheric" bins at 275 hPa, because they have the same horizontal resolution and similar vertical resolution. But it is explained that the CFC-11 filter has a suboptimal behaviour (p.12076, l.12). Among all species, water vapor has the largest vertical gradient at the tropopause. Hence I wonder if the MIPAS H_2O retrievals would not be more efficient for stratospheric filtering than CFC-11 retrievals (even if they are less precise).

We have decided not to use tropospheric water vapour as a filter for stratospheric signal. This is for the following reasons:

(a) The worse precision of H_2O in the upper troposphere deteriorates the significance of this filter.

(b) Contrary to CFC-11, tropospheric H2O is extremely variable, leading to a strongly variable (since state-dependent) altitude resolution of H_2O profiles. This leads to a poorly characterized filter.

(c) The vertical gradient of CFC-11 is largest above the tropopause. The vertical gradient of H_2O below. Thus CFC-11 is the better choice.

Section 3.1 and figure 9. This figure is very difficult to read because the trajectory symbols completely cover some longitude-latitude bins. I propose to zoom on the latitude band of interest (15 N-45 S, as in Edwards et al, 2006) and stretch the map vertically

Agreed, will be done.

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The stratospheric filtering should be already applied on this figure, shading in black the bins where ozone is above 70 ppbv and CFC-11 is below 245 pptv (or water vapor below some threshold). As in the right panel of figure 11, this would remove the spurious black bins over the Pacific and east of Southern Argentina.

We are reluctant to apply the filter already here for two reasons:

(a) When Figure 9 is referenced in the text, the CFC-11 filter has not yet been introduced, and the reader cannot understand the figure (which is not quite easy to understand already in its original version).

(b) The filter is extremely sensitive to the CFC-threshold chosen, and the impression the figure leaves would largely depend on the threshold actually chosen.

Thus we prefer to plot the unfiltered data here and to discuss the potential stratospheric contamination of the signal in the text.

Finally, an attempt should be made to plot only the trajectory symbols which have reached the pressure layer of the observations (center 275 hPa, depth corresponding to the vertical resolution of the instrument). While this could remove the overall shape of the plumes, it would put the focus on the observed part of the plumes. The correspondance between trajectory symbols and grey/black bins would become much more relevant.

As stated in the line 4 of the caption of this figure, only those points of the trajectories are shown which fall between 7 and 11 km, i.e. the requested altitude filtering had already been applied to the plot in the original version of the paper. So we understand that with respect to this there is no action left to do.

Section 3.3. The plume originating from equatorial America is assumed to be due entirely to tropical rainforest fires, which is an oversimplification. Some tropical American

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fires also originate from savanna (it is possible to identify them using geographical information and TRMM observations, but this is probably out of scope for the paper). Still this fact should be mentioned, as it removes some weight from the hypothesis "smouldering in America versus flaming in Africa" to explain the difference between the two plumes.

Agreed. A more careful wording will be chosen with respect to this throughout the paper. By the way, we meanwhile have changed the latitude threshold between "Equatorial America" (red symbols) and "Southernmost America" (light blue symbols) from 20 to 15 degrees South. The new threshold is better consistent with the borderline between rainforest and savanna-type vegetation. As a consequence, the difference in the chemical composition of the plumes becomes more pronounced. Since now trajectories of different injection altitudes are considered, the plume threshold is different (216 rather than 180 pptv). This also leads to the removal of some previously unexplained points in Figure 9.

Section 3.3 and figure 11. From figure 9, it is clear that both the tropical American plume and the African plume extend in the Indian Ocean, with many red and blue trajectories touching the same latitude-longitude bins. Hence I propose a clearer writing for the ozone morphology within the plume: the best argument for a different ozone abundance in the two plumes is the absence of elevated ozone above the tropical Atlantic, which is covered only by the equatorial American plume. The mixing of the two plumes must also be taken into account for the interpretation of figure 11: how were the two plumes separated into the different symbols of that figure? If it simply shows the composition of the bins themselves, some red and blue symbols should be totally superimposed, which never appears to be the case. Some clarification is necessary here.

Some description of the mixing of the plumes will be added to the "Trajectory calculations" as well as "Ozone morphology ..." sections. Clarification on the choice

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of symbols in ambiguous cases will be added: In these cases the relevant bin was assigned to the source region whose trajectories predominated in terms of number of trajectories touching the bin times the residence time of the air parcel within the bin.

Section 3.5, first paragraph (transport issues). It must be noted that MOZART simulations at 700 hPa during September 2003 show the African plume extending westward and equatorward (fig. 3b in Edwards et al, 2006). The "blue" trajectories (fig. 9) show a quite different plume. I suppose that this is not due to the different height (700 hPa 2900 m above S. Africa) since it is reported that the trajectories are not sensitive to injection height between 2500 and 4500 m. So the different outcome of these two simulations could be due to different views on transport (Eulerian at 700 hPa versus Lagrangian in 3D) or to the different dates (1.5 month difference). In any case the MOZART simulations put more weight on the dynamically-based (Andreae et al., 2001) and now chemically-based (p.12078, l.4) suggestions that "the African savanna plume resides longer in the lower troposphere than its Amazonian counterpart".

We think that the differences between the September 2003 windfields presented by Edwards et al. and our trajectories are not surprising. While we use injection altitudes between 2500 and 4500 m, our Fig. 9 includes, as discussed a few lines above, only those fragments of our 3D trajectories which fall into the altitude range between 7 and 11 km. In other words: Those of our trajectories which stay below 7 km are not shown, because these do not help to explain the MIPAS measurements. Thus, if Fig. 9 is compared to 2D windfields, it should be compared to windfields at about 275 hPa, not 700 hPa. We think that this and the different dates of the datasets do not allow any meaningful comparison of our Fig 9 and Edwards' et al figure 2. Certainly, the 750 hPa mean windfields presented by Edwards et al. and related discussion in their Section 3.1 supports the hypothesis of long residence times of polluted air in the lower troposphere. However, if the lower tropospheric transport schemes discussed by Edwards et al. explained also the MIPAS measurements,

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we should expect African trajectories to cross the 7 km altitude level somewhere west of Africa. Since this is not the case, we are reluctant to use the windfields and MOZART calculations shown in the Edwards' et al paper directly to interpret the MIPAS measurements. Instead, we will include a statement that Edwards et al. report on long lower tropospheric residence times particularly of African pyro-polluted air.

Conclusion of this review: (the authors do not need to modify their own conclusions unless they wish to). The African plume seemed to have a different morphology 6 weeks earlier (see previous comment) ...

We do not quite agree: Our C_2H_6 distribution shown in our Fig. 3 is very similar in shape and extension to the MOPITT CO at 250 hPa as presented by Edwards et al.. We attribute the differences to the comparison of 700 hPa data to 250 hPa data.

...and the ozone plume extending from America was much more intense during the biomass burning season of 2004 than presented here for 2003 (fig.5a in Ziemke et al, JGR, 2006). Hence I think that the actual background meteorology plays an major role. Some interesting light could be shed on this issue by a comparison of Southern Hemisphere biomass burning in 2003 and 2004. This comparison could combine MIPAS observations, meteorological analyses and an evaluation of the importance of savanna versus tropical rainforest fires in Southern America.

These issues will be discussed at more depth.

Technical corrections:

p.12076, line 17. "After application of the filters the ratios are...". It would be more precise to write "...the averaged ratios are..."

p.12076, line 25. "which attributes to convection" should be "which contributes to con-

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vection".

These will be corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12067, 2007.