Atmos. Chem. Phys. Discuss., 7, S4341–S4346, 2007
www.atmos-chem-phys-discuss.net/7/S4341/2007/

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.

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Received and published: 27 August 2007

General comments

The paper presents global distributions of upper tropospheric ethane (C2H6) and ozone on a global scale, derived from MIPAS limb observations during a 25 day period between October 21 and November 14, 2003. Simultaneous retrievals of CFC-11 at the same pressure level (275 hPa) are also presented, in order to discriminate between upper tropospheric and lower stratospheric air. It is reported that the chemical composition of the plumes originating from Tropical America fires is different from the plumes originating from African fires, and the possible causes for this difference are discussed.

C2H6 is a very interesting tracer for biomass burning and delivers important information about the burning processes. As stated in the paper, this is the first global upper tropospheric C2H6 data set which also covers tropical latitude. Upper tropospheric ozone adds a substantial contribution to the study, because it allows a global view on the impact of different emissions from biomass burning in tropical America and Africa.

A quantitative evaluation of the different C2H6/O3 ratio in the two plumes is obscured by three difficulties:
1) The calculation of the plume trajectories is quite difficult, because the injection heights are not known. This fact is well explained, as are the altitudes chosen for this injection height. Appropriate sensitivity tests have been made. Hence this difficulty apprears to have been treated as well as possible.
2) The observations sample some ozone-rich (and ethane-poor!) air masses which belong to the stratosphere. This has been adressed through filtering based on the retrieval of CFC-11, but there is no attempt to compare the filtered dataset of ozone with other measurements of tropospheric ozone.
3) The two plumes mix in the Indian Ocean, a fact which is ignored in the interpretation and deserves some clarification (see Specific comments).

The paper is written very well: it is well structured, the English is good, and precise enough information is provided to understand the retrieval procedures. The amount and origins of uncertainty in the retrievals are well explained. Given the limited vertical range of the dataset, its quantitative interpretation may have been pushed a little too far. The qualitative comparison of the ethane and ozone maps, assisted with the CFC-11 retrieval and trajectory simulations, already allows a fruitful discussion. In any case the originality of the observations make this paper a significant contribution for tropospheric chemistry, and should certainly be published in *Atmospheric Chemistry and Physics* after the specific comments below are adressed.

Specific comments

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- 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.

- 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.

- 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.

- 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 H2O retrievals would not be more efficient for stratospheric filtering than CFC-11 retrievals (even if they are less precise).

- Section 3.1 and figure 9. This figure is very difficult to read because the trajectory

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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. 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 treshold). As in the right panel of figure 11, this would remove the spurious black bins over the Pacific and east of Southern Argentina. 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.

- 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 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.

- 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

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here.

- 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 $\approx$ 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".

- 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) 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.

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 convection”.

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

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Interactive Comment