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Comment

Interactive comment on “Carbon monoxide distributions from the upper troposphere to the mesosphere inferred from 4.7 μm non-local thermal equilibrium emissions measured by MIPAS on Envisat” by B. Funke et al.

Anonymous Referee #1

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Review of the paper entitled "Carbon monoxide distributions..." by Funke et al.

The paper entitled "Carbon monoxide distributions..." by Funke et al. presents vertical profiles of carbon monoxide as retrieved from the ENVISAT/MIPAS IR limb-viewing measurements from the upper troposphere to the upper mesosphere. A sophisticated radiative transfer and retrieval methodology is applied to the measured spectra at 4.7 μm that includes: non-local thermodynamic effects, retrieval in $\log(\text{vmr})$, inhomogeneous retrieval and illumination-dependent vibrational population gradient along the line of sight of the instrument. Error characterization is presented depending on the

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latitudinal band and altitude layer considered. Using a complete CO data set recorded from September 2003 to March 2004, several studies are performed: seasonal variations, stratospheric and mesospheric distributions during the NH major warming in December 2003, and UTLS CO distributions in September–October 2003. Rapid descent within the NH polar vortex, mesospheric air reaching the lower stratosphere, impact of biomass burning and anthropogenic pollution up to the tropical UTLS are then analysed and interpreted using various tools from PV maps to back-trajectory calculations.

The paper is well written and the methodology is clearly presented. The scientific outcome from the manuscript is fair. Some Figures would need to be slightly modified in order to be more readable. Further scientific discussions would also help quantifying the processes highlighted by the CO measured fields, including more references (descent rates in the vortex), model outputs (WACCM results), CH₄ retrievals (no information is given), and above all, a clarification of the single vs. average errors and related vertical resolutions. In conclusions, I would recommend the manuscript to be published in the ACP journal, once the main points are treated.

Main points

1. Error characterization

In the section "3.4. Error estimation and retrieval characterization" (P. 20619-20620) together with the Figure 4 and the Table 2, single profile errors (total, random and systematic) are presented together with the averages over different periods of time that include about 3000 profiles corresponding to 3 days of measurements. I would strongly recommend to clearly separate single profile errors (total, random and systematic) to the averages over the 3-day period. This is particularly important since, depending on the analysis, the authors present either averaged profiles (Figs. 10 and 11) or single profiles (other Figures).

If we consider the averages presented in Fig. 4, I would rephrase the two sentences P. 20619 and L. 25 into something like: "precision is less than 40% for altitudes greater

than 40 km and lower than 15 km for any latitudinal band whilst it reaches 40-90% within 15-40 km for any band except in polar summer for which the precision is 30-35%".

At that stage, it is not clear to me why the average relative precision (for instance 80% at 20 km) corresponding to 3000 profiles (Fig. 4) is of the same order of magnitude (~80%) as the single profile total error (Table 2) since the average relative precision should have been reduced by about a factor ($1/\sqrt{3000}$) compared to the single profile error.

Consequently, the vertical resolution presented in Fig. 5 only applies to (3000?) averaged profiles. In theory, averaging kernels for single profiles should be different and the vertical resolution should be altered too. This needs some comments.

2. Methane

At several points in the manuscript (P. 20622, L. 10; and P. 20624), CH₄ fields suddenly appear although no information from this particular molecule is given. This data set (I guess from the MIPAS instrument) needs to be presented with errors, wavelengths and references.

3. Models

The measured correlation CH₄-CO in the middle atmosphere is very convincing. The outputs from the WACCM model are presented in P. 20624, but not shown. Note that there is no QBO in WACCM (see Jin et al., ACPD, 2008). The discussion related to the CO-CH₄ correlation would be better supported if the authors were showing WACCM outputs on Fig. 7.

4. Scientific discussion

a. Seasonal variations of CO. The discussion on the descent rates is not supported by any reference except Manney et al. (2005). It is also surprising to see 3 references appearing in the conclusions but not in the core of the manuscript: Konopka et al.,

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2007; Engel et al., 2006; and Stiller et al., 2008. I would check and discuss whether the value of 1200 m per day is consistent with other works.

b. Stratospheric and Mesospheric CO. Again in this section, the results of MIPAS are not compared to any other works except Manney et al. (2005). Consequently, the discussion is by far too qualitative.

c. UTLS CO. The three surfaces along which CO profiles are presented in Figs. 13 and 15 are: 270, 170 and 100 hPa. This corresponds to an average altitude of 9.5, 12.5 and 15.5 km, respectively, with a vertical resolution of approximately 3.5, 4.5 and 6 km from Fig. 5. Consequently, the information presented along the three surfaces is not independent. Indeed, the CO field at 170 hPa appears to be a linear interpolation of the fields at 270 and 100 hPa. Could the authors please clarify this point and/or comment on that?

P. 20629, L. 15: "Only trajectories originating at altitudes below 3.5 km were considered for further evaluation (see Labonne et al., 2007)". The reference paper does not consider the same period (July and August 2006) and the same areas (South America and Australia). In addition, the referenced study shows that pyro-convection and direct injection to the free troposphere are not frequent. Did the authors make a sensitivity study relative to the minimum height considered, namely below 3.5 km, and how their results are affected by the imposed altitude threshold?

5. Conclusions

P. 20634, L. 1: the estimated total retrieval error for a single limb scan is 20-80% and not 20-40% as it is written. This also needs to be consistent with the abstract (P. 20608, L. 7): 15-40%. In any case, the authors will need to clarify average vs. single profile errors, together with random and systematic errors in the core of the manuscript, in the abstract and in the conclusions.

6. Figures

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Fig. 6 is very difficult to read, and particularly in the range 15–30 km where the authors discuss the seasonal evolution of CO, its interhemispheric difference, and the impact of sources up to the UTLS in the tropics and extra-tropics. Why not presenting the same field either with the same Figure or with another Figure as an absolute or relative anomaly to highlight small-scale structures in the temporal evolution of CO?

Fig. 8 and 9: the information contained in the SH is rather weak, and I would push to only consider the NH side. In addition, I would centre the Figure on the North Pole to highlight the discussion.

Fig. 12: is a very difficult Figure to read. I would first get rid of the MIPAS measurement locations highlighted by coloured dots and (as for Figs. 13 and 15) would start from bottom to top from the lowermost surfaces (270 hPa) to the uppermost surfaces (100 or 50 hPa).

7. Tables

As for Fig. 12, in Table 2, I would start from bottom to top from the lowermost surfaces (10 km) to the uppermost surfaces (70 km). It could be rather interesting to show the content of this Table along with the Figure 4, namely showing vertical profiles, precision, total and relative errors.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20607, 2008.

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