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Comment

## ***Interactive comment on* “Carbon monoxide distributions from the upper troposphere to the mesosphere inferred from 4.7 $\mu\text{m}$ non-local thermal equilibrium emissions measured by MIPAS on Envisat” by B. Funke et al.**

**B. Funke et al.**

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We thank Reviewer 1 for his/her very helpful comments and suggestions. The "Reviewer Comments" are noted first and then we give our "Reply:" to the comment. We are submitting a revised manuscript that includes all the actions noted below.

*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), CH<sub>4</sub> retrievals (no in-*

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formation 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.

Reply: In the revised version, we will extend significantly the scientific discussions included in Sections 4 and 5. We will add a large number of further references and two additional figures. A detailed description of these changes is given below.

*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).

Reply: Errors discussed in Section 3.4 (and corresponding Figures 4+5, Table 2) refer exclusively to single measurement errors. In order to achieve a more representative error analysis, we present averages of profiles and single measurement errors (not the error of the averages!) for typical atmospheric conditions. This will be clarified by stating: "In order to account for the different response of the retrieval of different geophysical conditions, we have performed the error analysis separately for typical atmospheric situations, namely polar winter and summer, mid-latitude, and tropical conditions. Figure 4 shows typical CO vmr profiles (obtained from the averages of the retrieved profiles within the corresponding latitude band) and single measurement precision for these atmospheric situations."

*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*

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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%".

Reply: We will change the text accordingly.

*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.*

Reply: See above. Also, vertical resolution shown in Fig. 5 refers to single measurements, which will be clarified in the figure caption of the revised manuscript.

2. *Methane At several points in the manuscript (P. 20622, L. 10; and P. 20624), CH<sub>4</sub> 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.*

Reply: CH<sub>4</sub> fields discussed in Section 4 are indeed obtained from MIPAS observations. In the revised manuscript we will state that CH<sub>4</sub> abundances were retrieved simultaneously with N<sub>2</sub>O from MIPAS spectra around 8  $\mu$ m with an accuracy of 10-20% (Glatthor et al., 2005).

3. *Models The measured correlation CH<sub>4</sub>-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-CH<sub>4</sub> correlation would be better supported if the authors were showing WACCM outputs on Fig. 7.*

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Reply: We will include an additional Figure, similar to Fig. 7, but showing WACCM results.

#### 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., 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.*

Reply: We will refer to Konopka et al. (2007), Engel et al. (2006), and Stiller et al. (2008) in Section 4. Further, a detailed discussion of descent rates derived from MIPAS CO observations in the context of other studies will be included in the revised manuscript. In particular, descent rates of 1200 m/day have been reported, to our knowledge, only from model studies (Siskind, 2000). However, extraordinary descent in this period has also been detected from satellite observations of NO<sub>x</sub> (Randall et al., 2005; Rinsland et al., 2005; Hauchecorne et al., 2007; Funke et al., 2007b). Our estimated descent rates during February and March 2004 are in good agreement with results from GOMOS NO<sub>2</sub> observations (Hauchecorne et al., 2007). Overall, MIPAS CO observations indicate that polar mesospheric air masses descended about 25 km (i.e., from 50 km to 25 km) from November 2003 until March 2004, resulting in an average polar winter descent rate of 5 km/month, in good agreement with Nassar et al. (2005) who derived values of 3.2 - 5.3 km/month in the upper stratosphere and mesosphere from ACE-FTS observations of H<sub>2</sub>O, CH<sub>4</sub>, and N<sub>2</sub>O for the same winter. Rosenfield et al. (1994) also modeled descent in both hemispheres at a range of altitudes and calculated 27km of descent for air originating at 50km.

*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.*

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Reply: A comparison of our results to other works dealing with stratospheric and mesospheric CO distributions during the December 2003 - January 2004 warming event is difficult due to the lack of other observational data. As mentioned in the paper, MIPAS was the only remote sensing instrument in space at this time which measured vertically resolved profiles of stratospheric and mesospheric CO. However, we compared our CO distributions with the CH<sub>4</sub> fields derived from the same MIPAS observations and found a well defined spatial anti-correlation, as expected. Further, we will discuss in the revised version our analysis of stratospheric and mesospheric descent during this particular warming event in the context of findings related to other events, including additional references to Randall et al. (2006), Siskind et al. (2007), and Manney et al. (2008).

*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?*

Reply: Since vertical resolution is defined as full width at half maximum in our study, a resolution of 6 km around 15 km (4 km around 10 km) is sufficient to ensure substantially independent vertical information given in the retrieved CO distributions at 100, 170, and 270 hPa. We do not agree that the CO field at 170 hPa appears to be a linear interpolation of the fields at 270 and 100 hPa. For instance, the highest vmrs inside the AMA (Fig. 13) are found at this particular pressure level. This excludes that CO abundances, there, are generated by interpolation related to insufficient vertical resolution.

*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?*

Reply: Sensitivity tests have shown that the final trajectory pattern varies only marginally with the injection altitude varying between 2500 and 4500 m.

*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.*

Reply: We will specify the single measurement total retrieval error in the abstract and the conclusions of the revised manuscript to "15-40% for altitudes greater than 40 km and lower than 15 km and 30-90% within 15-40 km".

*6. Figures 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?*

Reply: We will include a new Figure 7 which shows the absolute anomalies of the temporal evolution of tropical 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.*

Reply: We have chosen a cylindrical projection because it allows for a better represen-

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tation of planetary waves, important for the discussion within the dynamical context. Further, during the particular event discussed in the manuscript, CO-rich air masses were transported even to tropical latitudes. This could hardly be visualized by means of an orthographic pole-centered projection. Instead, we will restrict the latitude range of Figures 8 and 9 (new Figures 9 and 10) to 40S-90N.

*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).*

Reply: As in Figure 8 and 9, the colored diamonds represent individual MIPAS observations. We think that it is of interest for the reader to know from which data the smoothed fields used in the further discussion have been constructed. It should be noted that the purpose of such kind of figures is to identify possible differences between individual data points and the background field. Where there is no contrast between symbol and background color, the individual data points are consistent with the underlaid interpolated field, and there's no need to pay attention to the symbols. We will change the order of the panels of Figures 12, 13, and 15 (new Figures 13, 14, and 15) as suggested.

*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.*

Reply: We will change the order of altitudes in Table 2 accordingly. With respect to the inclusion of the content of Table 2 in Figure 4 (or an additional Figure), we think that the information given in Figure 4 already reflects the content of Table 2, given that (i) both Table and Figure shows single measurement errors, (ii) the total error is dominated by the random component (precision) and hence precision and total error are nearly the

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same.

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**ACPD**

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