

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.

B. Funke et al.

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We thank Reviewer 2 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.

Overall, I consider the paper well written and especially the many challenges addressed with the new retrieval algorithm deserve publication. The broad scope of the paper (covering algorithm development, some case studies with focus on stratospheric and mesospheric air masses and a case study on the troposphere and biomass burning) makes it sometimes hard to read as a mixed audience (and reviewers) is not neces-

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sarily very familiar with all topics. In particular the case studies are somewhat too descriptive and personally I think that the authors try to put too many findings into one paper while the paper would have benefited from a stronger (and more quantitative) focus on less case studies. However, this might be more of a personal taste and is not a reason not to publish the paper which I recommend after the following specific comments are addressed.

Reply: The principal goal of our paper is to describe a new MIPAS data set and how it can be scientifically exploited. With the inclusion of several case studies covering the atmosphere from the troposphere to the mesosphere, we aim at demonstrating the potential of MIPAS CO data to contribute to a variety of different scientific topics. The analysis of our MIPAS CO data by means of case studies shall give a "taste" of what can be done with the data and is not thought to substitute dedicated future studies. Nevertheless, in order to provide a more detailed and quantitative analysis we will extend the scientific discussion significantly in the revised version, including also a large number of new references and two additional figures.

Specific comments

Introduction, line 8 tropospheric lifetime of 1-2 months: The mean trop. Lifetime is about 2 months and can, depending on season and latitude be far longer.

Reply: This will be changed to "tropospheric lifetimes of about 2 months".

Page 20609, line 16 SCIAMACHY/ENVISAT: Please add at least one citation from other SCIAMACHY groups (Buchwitz et al and Gloudemans et al)

Reply: These additional citations will be added.

Page 20612, line 7 Are spectral shift and instrumental line shape correction performed for each scan separately or fixed for longer time periods? What impact would it have to change this?

Reply: While spectral shift (as function of wavenumber) is fitted individually for each

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scan (von Clarmann, 2003b), a fixed instrumental line shape (ILS) is used. The ILS function was determined during the inflight calibration and its stability over time has been verified over a long time period (Frank Hase, pers. communication). This will be clarified in the revised version.

Page 20612, line 26 How is gamma being determined (arbitrarily, using the Lcurve, etc?). How does the choice of gamma impact the retrieval? Does the side constraint consist of the derivative of VMR or log(VMR) and what impact would it have?

Reply: The gamma profile has been adjusted by means of a serie of test retrievals optimizing the trade-off between precision and vertical resolution for a large variety of possible geophysical conditions. This will be explicitly stated in the revised manuscript. The first-order Tikhonov operator used as side constraint acts in the retrieval parameter space $\log(\text{vmr})$ by smoothing the differences between $\log(\text{vmr}_{\text{retrieved}})$ and $\log(\text{vmr}_{\text{a priori}})$. The principal difference between a smoothing constraint acting on $\log(\text{vmr})$ compared to that acting on vmr is that the first smoothes the ratio profile $(\text{vmr}_{\text{retrieved}} / \text{vmr}_{\text{a priori}})$, while the latter smoothes the difference profile $(\text{vmr}_{\text{retrieved}} - \text{vmr}_{\text{a priori}})$. Hence, the $\log(\text{vmr})$ constraint introduces a weaker dependence on the magnitude of the vertical variability of the a priori profile. This is favorable for the retrieval of CO which exhibits a large natural variability (over several orders of magnitude).

Page 20613, line 2 Why is a height-independent radiance offset needed (fixed offset, relative offset, origin of the offset)?

Reply: A wavenumber- and height-independent offset is retrieved for each micro-window simultaneously with the CO abundances in order to compensate for offset calibration errors. This will be explicitly stated in the revised manuscript.

Page 20614, lines 10+ The authors mention that the differences between ACE and MIPAS are largest in the unusual strong CO-downward events. Why is this the case? Is it caused by the different measurement principles or is any of the instruments having

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problems in this situation and how does it affect the interpretation? Please elaborate.

Reply: The pronounced spatial variability close to the vortex boundary introduced significant differences in the CO distributions at MIPAS and ACE locations, even within the coincidence criterion of 18 h in time and 800 km in space. This will be explicitly stated in the revised manuscript.

Page 20618, lines 11+ Why don't you restrict the retrieval to weaker lines, avoiding the problems of nonlinear absorption along the LOS? Would the retrieval noise be too high then?

Reply: These problems arise at polar winter conditions with very high CO abundances. However, the stronger lines are needed for the retrieval under "low CO" conditions (i.e. not polar winter) in order to reduce the noise error. Therefore, a trade-off between weak and strong lines has been included in our selection of micro-windows, which is kept for any conditions in order to avoid artificial biases when comparing different regions or periods.

Page 20620, lines 4+ Are fit residuals stochastic or partially systematic? A plot of a fit would be nice (perhaps also illustrating the impact of the algorithm improvements on fit quality (residuals, etc))

Reply: Fit residuals reflect the measurement noise except for tangent heights below 40 km where systematic deviations due to uncertainties in the O₃ non-LTE modeling are encountered. These deviations are in the order of the instrumental noise (2-3 nW/cm² sr cm⁻¹). Fit residuals are presented in Figure 1 of Funke et al. (2007) to which we will refer in the revised manuscript.

Ch. 4, Seasonal variations. Can't a seasonal change in CO lifetime cause some of the seasonality?

Reply: Yes, it does. As discussed in Section 4, seasonal changes of CO in the tropical and polar summer stratosphere are closely linked to changes of methane abundances.

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This indicates that under sunlit conditions, the chemical lifetime of stratospheric CO is less than typical vertical or meridional transport times.

Page 20622, lines 10++ Where is CH4 data taken from and why does it have a seasonality (the authors mention it shortly in the conclusion but it belongs in the main text)

Reply: CH4 abundances were retrieved simultaneously with N2O from MIPAS spectra around 8 μm with an accuracy of 10-20% (Glatthor et al., 2005). The CH4 decrease discussed in this paragraph is produced by photochemical losses involving photolysis and reactions with OH and O(1D) during polar summer. This will be stated in main text of the revised manuscript.

Page 20622, lines 15+ How is the descent rate being calculated and what is the estimated accuracy of it? Is MIPAS equally sensitive to all heights or could differences in averaging kernels at different height levels impact the computation of descent rates (if, for instance, CO is being transported to a height level with higher sensitivity or narrower AK). What are typical expected descent rates in literature and how does the 1200m/day fit in (ie how extraordinary is it?)

Reply: Descent rates have been calculated from the vertical shift (subsidence) of zonal mean CO profiles corresponding to consecutive days when observations are available (i.e. 1200 m/day has been derived from the shift between 8 and 16 January 2004). This will be explicitly stated in the revised manuscript.

The accuracy of these estimates at a given altitude depends, on one hand, on the total error of the average being precision/ \sqrt{n} + systematic error. For the estimated descent rate at 60 km corresponding to mid January 2004 the number n of included measurements is around 200, precision is about 1 ppmv, and the systematic error is 0.2 ppmv at 60-90N which results in a total error of 0.21 ppmv. On the other hand, the accuracy of the estimated descent rates depends on the vertical gradient (around 0.45 ppmv/km at 60 km). The observed vertical shift between 8 and 16 January is

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then 9.5±0.8 km which gives a descent rate of 1200±100 m/day. This error margin will be included in the manuscript. Due to the finite vertical resolution, retrieved CO profiles are smoother than in reality. However, since the averaging kernels do not change significantly within one week, the smoothing is the same for both reference profiles and, hence, their difference (and thus the vertical shift) is not affected, assuming homogeneous descent (both in the vertical and in time).

Typical observed descent rates are in the order of 100 - 600 m/day (i.e., Nassar et al., 2005; Hauchecorne, 2007). In this sense, our observations hint at extra-ordinary descent. Such high descent rates 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_x (Randall et al., 2005; Rinsland et al., 2005; Hauchecorne et al., 2007; Funke et al., 2007b). A detailed discussion of descent rates derived from MIPAS CO observation in the context of other studies will be included in the revised manuscript.

Ch. 6 Upper tropospheric CO Please also refer to Velazo et al (GRL, 2005, D16807) and Gloudemans et al (GRL, 2006, D16807) both showing similar findings caused by BMB in South America in the Oct. season. Interestingly, MOPITT doesn't see an enhancement at the 250 hPa layer in the time periods under investigation and Velazo et al had similar discrepancies and argued that the height resolution of MOPITT is too coarse making MIPAS one of the few instruments that can actually measure the upper trop.

Reply: References to Velazco et al. (doi:10.1029/2004JD005351) and Gloudemans et al. (doi:10.1029/2006GL026804) will be added.

Page 20630, lines 10++ The highest CO total column abundances from NADIR sounders (SCIAMACHY, MOPITT) are mostly seen over industrial China and less over Bangladesh.

Reply: Our trajectory analysis is not sensitive to the source strength of different in-

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jection areas. Hence, we can only identify potential source regions (i.e., China and Bangladesh), but not their relative contributions.

Page 20631, line 7 over the Tibet...

Reply: This will be changed to "over Tibet".

Conclusions The conclusions actually present partially new discussions which are missing in the main text. Please ensure that all discussions and citations are discussed earlier and are only summarised in the conclusions!

Reply: We will move all new discussions and citations presented in the Conclusions to Section 4.

Figure 3 How is the distance-weighted averaging being performed (weighting function, width!)?

Reply: Contributions of individual measurements within 10 deg latitude and 20 deg longitude around each grid point of the maps were weighted by the inverse of their distance to each grid point. This is already mentioned in the figure caption.

Figure 7 and CO-CH4 correlation in general What is the lifetime of CH4 at this level and how does it correspond to the 0.05 scaling factor and why is there no time lag between CH4 and CO abundances? (eg at a lifetime of 8 years, 2% of methane is oxidised within 2 month. In the plot, CO always seems to be almost exactly 5% of the methane VMR, can this be explained by a simple model with given CH4 and CO lifetime?)

Reply: The chemical lifetime T of CO (CH₄) at 45 km in the tropics is around 5 days (3 months) (Brasseur Salomon, 1986). In a simple photochemical model (assuming steady state and negligible dynamical losses) CO densities can be calculated as $[CO] = T(CO)/T(CH_4) \cdot [CH_4]$, which would lead to $[CO] = 0.06 \cdot [CH_4]$. Adding a dynamical lifetime by meridional redistribution of about 20 days, the calculated CO amount would further reduce slightly to approximately 5% of the CH₄ amount, which corresponds exactly to our results. Further, given the chemical lifetime of CO of around 5 days, a

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time lag between CH₄ and CO in the order of a few days would be expected. Taking into account the sampling rate of the measurements and the rather weak modulation of CH₄ abundances, such a time lag can be hardly resolved from the presented temporal evolution.

Figure 8 Continental outline are hardly visible (please change like in Fig 12) Weighting method?

Reply: Figures 8 and 9 will be changed accordingly. All measurements within 10 deg latitude / 20 deg longitude around each grid point are weighted by the PV-difference of the measurement's location and the actual grid point. This is already stated in the figure caption.

Figure 12 In the individual measurements, there seems to be quite high scatter, is this in line with the estimated single retrieval precision and what is the estimated error in the mean maps?

Reply: Due to the pronounced spatial and temporal variability of upper tropospheric CO, the standard deviation of CO observations within a given lat/lon box taken during a 3-day-period is expected to be considerably higher than the precision of single measurements. The estimated total error of the smoothed CO abundances (i.e., the distance-weighted averages over a 3-day-period) in the maps shown in Figures 12 and 14 increases from 7% at 10 km to 24% at 20 km. These error estimates which will be included in the revised manuscript have been calculated as $\sqrt{\text{average precision}^2 + \text{sys. error}^2}$. Here, average precision is calculated as $\frac{\text{single meas. precision}}{\sqrt{\sum_i \text{weight}_i}}$ where the sum runs over all contributing measurements i .

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

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