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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Annual variation and global distribution of strato-mesospheric carbon monoxide measured by ground-based Fourier Transform Infrared spectrometry" *by* V. Velazco et al.

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Interactive comment on " Annual variation and global distribution of strato-mesospheric carbon monoxide measured by ground-based Fourier transform infrared spectrometry " by V. Velazco et al.,

Reply to Comments from J. Walker, from the MIPAS group at AOPP, Oxford University.

We would like to thank the reviewers for their constructive comments and suggestions.

General Comment: It would be informative to mention the spectral range of the microwindows used. It would be good to give a little information about the spectrometers

rather than just the model names just so that people who normally work with satellite instruments, for example, can have a better idea about the instrumentation. We think you need to mention something about the nature of the retrieval method used, rather than naming the retrieval algorithms, which is somewhat obscure.

Response: We have rewritten section 2.2 in the revised version of the manuscript and addressed these comments by giving more detailed information on the instruments, micro-windows, and the retrieval method (optimal estimation method).

General Comment: Also, we wondered what the reasons were for having used different retrieval algorithms for different stations, even in cases where the method - instrument, microwindows, HITRAN - was the same. Since we are not told anything about the retrieval methods, we had no idea how this might have affected your comparisons.

Response: We have also provided a discussion on the retrieval algorithms and cited results of a publication (Hase et al., 2004), reporting the comparisons of SFIT2 and PROFFIT

General Comment: A bit more detail on the nature of the integration of the VMR profiles to yield partial column number densities would be useful. What do your VMR profiles look like? We thought that there should be more mention of how good you think your measurements are. Including the averaging kernels seems like a good idea but since we haven't been told anything about the retrieval method, for example the strength of the a priori constraint, we cannot be sure how to interpret these kernels. How good is a maximum of around 0.7 on the high altitude partial column? Also, it would be really useful to have an idea of the errors on your CO measurements.

Response: We have reconstructed Figure 1 and added the averaging kernels for Poker Flat superimposed with the averaging kernels from Ny Alesund. Typical examples the a-proiri VMR profiles are also shown. A paragraph discussing the erros has also been added.

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General Comment: Recent high altitude measurements of CO from space exist from instruments such as MLS and MIPAS as well as ACE-FTS and Odin. It would be interesting to compare your results to such satellite measurements if you had time.

Response: This is a very good idea and we plan to do this in the future.

General Comment: Despite SLIMCAT being a well established chemical transport model in the stratosphere, it would be good to include a quick summary about how it has been validated and the level of agreement found. We wondered whether converting all the thermospheric CO2 into CO is appropriate. Perhaps this might lead to too much CO descending in the polar vortex in the model.

Response: We think that this is the case and the reason for some of the discrepancies between the model and the FTIR measurements especially for Arrival Heights. In the revised version of the paper, we have added correlation plots (now Figure 4.). The effect of too much CO descending in the model can be seen during late fall to early winter. Nevertheless, it is worth noting that despite these very simple assumptions with a 2-D model, the comparisons are quite good.

General Comment: Also, if the complete conversion of CO2 into CO is a valid approximation, wouldn't there be a noticable depletion of CO2 in the air in the polar vortex. We thought that this step needed further justification.

Response: One would probably expect to measure a depletion of CO2 in the polar vortex in reality, if one measures thermospheric air, even if the conversion of CO2 into CO was not complete. However, this is not the case for the model, as CO2 chemistry is not explicitly included in the model's chemistry scheme (because it is fairly stable throughout the lower and middle atmosphere).

General Comment: Although we thought that the explanation of the smoothed model lines was OK, we wondered what exactly the unsmoothed model lines represented. Are they a single level output, for example, or some kind of average output?

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Response: The unsmoothed model lines (green curves) are lines that join single points for each day. The points represent a model output for each day for the partial columns of CO from 18 km to the top of the atmosphere in the model.

General Comment: We wondered whether you might try to remove the tropospheric CO contribution from your strato-mesospheric column or explain in more detail the effect you think it has. We think that perhaps it isn't enough to point to the presence of variability from biomass burning in the tropospheric column and the apparent absence of the signal in the stratomeospheric column as an indication that the contribution of the tropospheric column is unimportant.

Response: The absence of the effect of tropospheric variability on the strato-mesospheric columns is a confirmation of what the averaging kernels indicate (on Ďnormal" conditions), that we can separate both partial columns. This is indeed a good question because as we have mentioned, the averaging kernels are not perfect, i.e. the kernel for the strato-mesospheric column is not a step function with zero elements below 18km. In an extreme situation where the CO partial columns in troposphere would be so much enhanced or so much depleted (say by more than plus/minus 50% of the real value) and provided that the a-priori is also off by so much from the real values, then the effect of the columns below 18 km to the columns above 18 km would be noticeable. However, the combination of the two scenarios is unlikely.

General Comment: We still can't really be sure about your estimate of the relative contribution from the tropospheric column derived from the model since, as you say, SLIMCAT is not considered reliable in the troposphere.

Response: This is really a good point. Not all of the tropospheric sources of CO are accounted for in the model. In response to this, we have calculated the smoothed column from the model (for one year) with the following scenarios: 1. With a tropospheric CO amount ($<$ 10 km) doubled. 2. With a tropospheric CO amount ($<$ 10 km) decreased by half. These tests yielded changes of only -2% to +3% on the columns above 18 km

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(much less during winter-spring). These changes on the strato-mesospheric columns are quite small despite the extreme conditions set in the troposphere. This means that on realistic conditions, the troposphere probably has very little or no influence on the columns above 18 km.

General Comment: Also, it is usually the case that subsidence is stronger in the southern polar vortex. Could it be that the apparent stronger subsidence at some of the arctic stations is an effect of the position of the stations and the shape of the vortex? Satellite observations could be useful here.

Response: Yes, this is quite a complicated topic here. We think that the position in terms of latitude should be comparable (78°S and 79°N). We have revised this part of the discussion. "The average curves also indicate that, the partial column amounts above 18 km in spring in the Arctic (79°N) are generally slightly higher than in the Antarctic (78°S). Although the data points for the spring months are quite scarce, we speculate for now that this could be due to the subsidence being stronger above Ny Alesund around spring." Just a quick information, our colleagues running the Poker Flat station are doing a study involving satellite data, data on potential vorticity and FTIR.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 7119, 2006.

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