Atmos. Chem. Phys. Discuss., 6, S3163–S3165, 2006
www.atmos-chem-phys-discuss.net/6/S3163/2006/

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.

J. Walker

walker@atm.ox.ac.uk

Received and published: 18 September 2006

Comments are from the MIPAS group at AOPP, Oxford University.

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.

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.

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.

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.

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. 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.

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

ACPD

6, S3163–S3165, 2006

level output, for example, or some kind of average output?

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 strato-meospheric column as an indication that the contribution of the tropospheric column is unimportant. 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.

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.

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