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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "CO profiles from SCIAMACHY observations using cloud slicing and comparison with model simulations" *by* C. Liu et al.

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First of all, we want to thank Joanna Joiner for her very valuable comments!

We agree with the comments to a large extent, but - as we have tried to explain in the manuscript - we believe that the raised issues do not strongly affect the conclusions, which are based on relative, rather than absolute, CO profiles. Nevertheless, we agree that some aspects of our procedure could be substantially improved following some of the suggestions of Joanna Joiner (especially with respect to points 1 and 2). We will now reply to each comment in detail:

To point 1

We agree that for the selected threshold for the effective cloud fraction of 10%, especially over land surfaces, a substantial fraction of the observed signal can originate from the cloud-free part of the satellite ground pixel. From the good agreement of the CO cloud slicing profiles for different thresholds (10%, 20% and 40%), however, we conclude that the dominant part of the signal indeed comes from the cloudy part of the pixels. And it should be noted here that this is a general finding, which does not only hold for the example shown in Fig. 2 (note that the differences between the shown cross sections are rather small - certainly much smaller than the differences between the measurements and model results). Nevertheless, we agree with Joanna Joiner that it would be interesting to make better use of the available cloud information. As suggested, we will apply the concept of cloud radiance fractions in the revised version of our manuscript: we will calculate the relative fractions of the received signal from the clear and clouded part of each SCIAMACHY ground pixel. We will sample the model results according to these contributions and re-calculate the seasonal averages. We will compare the model values sampled in this way to the SCIAMACHY CO profiles. In addition, we will provide cloud radiance fractions for the selected threshold values of effective cloud fractions for a set of representative surface types.

To point 2

It is true that cloud properties are different in the spectral range of the CO analysis compared to the spectral range used for the FRESCO cloud algorithm. Of course, it is impossible to accurately determine cloud properties in the CO analysis spectral range from the FRESCO results for individual observations, because the retrieved cloud properties from FRESCO can not simply be extrapolated to the spectral range of the CO analysis. However, we think that it is a valuable approach to estimate the overall effect by assuming a reduced cloud top albedo compared to the spectral range of the FRESCO cloud algorithm. We think that it is a reasonable assumption to use a reduced cloud top albedo of about 40% (see e.g. Nakajima and King, 1990) compared to a cloud top albedo of about 80% used for the FRESCO cloud algorithm. Because of the lower cloud albedo (and depending on surface albedo) the contribution from the clouded part of a satellite ground pixel is generally systematically smaller compared to the spectral range used for the FRESCO cloud algorithm. However, this effect is partly compensated by the fact that also the surface albedo over most land surfaces is smaller in the spectral range of the CO analysis compared to the spectral range used for the FRESCO cloud algorithm. In particular, over regions with dense vegetation, e.g. over the investigated biomass burning regions, the surface albedo is typically smaller than in the spectral range used for the FRESCO cloud algorithm. Thus we conclude that – in spite of the reduced cloud top albedo – a dominant fraction of the received signal still originates from the clouded part of the satellite ground pixel. Nevertheless, as stated in our reply to point 1, in the revised version of our manuscript we will calculate the relative contributions from the clear and clouded part of each SCIAMACHY CO observation based on cloud radiance fractions. According to these contributions we will sample the model results and then calculate the seasonal averages. Concerning the interpretation of the effective cloud top pressure: Of course we are aware (as discussed in section 2.1 of the manuscript) of the fact that the FRESCO cloud top heights do not represent the geometrical cloud top heights. Fortunately, the deviation to the geometric cloud top height cancels partly, because the CO measurements are affected in a similar way (see section 2.1).

To point 3. We agree that it would be good to have a comprehensive validation of the SCIAMACHY CO profiles. However, since the retrieved CO profiles do not constitute 'real' atmospheric profiles, but complex composites of CO measurements made under different meteorological conditions, we have some doubts that MOZAIC data are really appropriate for such a comparison. Note that MOZAIC also samples the globe quite inhomogeneously: particularly biomass burning regions are not very well covered. In addition, the spatial resolution of MOZAIC does not match the SCIAMACHY resolution. Instead we think that comparison with model results is a good choice, because: a) both models are well validated b) they can be sampled in exactly the same way as

SCIAMACHY c) they consistently cover the whole globe

In general, for most regions of the world the derived CO profiles from measurements and model simulations show very good agreement (despite an offset of the total CO VCDs, which is also found in other studies involving total CO columns). Thus we conclude that the derived CO profiles using the cloud slicing technique indeed constitute quite realistic profiles, and we expect that the application of the concept of cloud radiance fractions (see above) will constitute only small changes.

Again, we want to emphasize that the main focus of our study is on the comparison of relative patterns from models and cloud slicing, which reveals, aside from the overall good agreement, significant differences in particular over biomass burning regions..

To point 4

In principle, the measurements and model results could be compared either as partial CO VCDs or as average mixing ratios. Both procedures are almost equivalent in mathematical terms. However, we decided to use CO PVCDs for the comparison because of several reasons (see also page 11665 of the manuscript):

a) The PVCDs, derived from the measured column densities averaged over distinct cloud height bins, are closer to the original satellite measurements. In addition, they still provide information on the total column (though the agreement of total VCDs is affected by a bias), which would be lost if concentrations would be calculated. Note that for a comparison of concentrations, generally better agreement between model and observation is found, as the additive bias is lost in the derivative of PVCDs. b) In cases of higher CO PVCDs at higher altitudes (e.g. due to convective lifting or long-range transport), negative mixing ratios are derived. In instantaneous profiles, negative mixing ratios are not realistic, and thus we wanted to avoid them (this is more of an 'aesthetic' consideration). We appreciate the comment regarding the low gradient in CO PVCDs for the two lowest levels over China during summer. This probably results from comparing measurements made under different meteorological conditions. We will mention this finding in the revised version of our manuscript.

Reference: Nakajima, T. and King, M. D.: Determination of the Optical Thickness and Effective Particle Radius of Clouds from Reflected Solar-Radiation Measurements 1. Theory, J. Atmos. Sci., 47, 1878–1893, 1990.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11659, 2013.