

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

Anonymous Referee #2

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This manuscript presents an application of the cloud slicing technique to SCIAMACHY CO retrievals. This method generates valuable vertical information on the distribution of CO which is then used to test calculations of two transport models. The manuscript is overall well presented and it should be of large interest to the readers of ACP and I recommend publication of the manuscript after addressing my comments below.

This manuscript deals with a new method for the use of SCIAMACHY CO columns and I believe that it is necessary to provide some information on the quality, uncertainties and characterization of the inferred CO sub-columns. As briefly mentioned in the manuscript, the method involves multiple uncertain steps (e.g. photon path in clouds,

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effect of non-cloudy fraction, assignment of cloud heights, effects of interfering gases) and it appears necessary to attempt some level of quantification of the expected uncertainties. Furthermore, as for any remote sensing dataset, it is difficult to judge the value and quality of the datasets without any validation (although this might turn out to be very difficult).

As a consequence, I believe that several conclusions drawn on the performance of models are somewhat pre-mature. You would first need to establish the quality and uncertainty of the satellite dataset before you can argue with confidence that observed model-measurement differences are the result of model shortcoming.

Minor comments:

p. 11661 IR -> infrared (IR)

p. 11662 Our retrieval of the total atmospheric CO vertical 5 column density (VCD) and its validation is described in detail in Liu et al. (2011). -> It would be beneficial for the reader when the manuscript would include a brief summary of the clear-sky CO retrieval and its validation

p. 11663 Therefore, the photons that 20 the satellite detects are either scattered by the cloud or reflected at the Earth's surface

->Or scattered by aerosols

p. 11663: However, different from the study of Liu et al. (2011), here we use only observations for (partly) cloudy conditions (effective cloud fractions > 10%) -> The CO columns as described in Liu et al., 2011 are corrected for effects of clouds. For the cloud slicing method, I assume that you need to turn off such a correction. Is this what you do?

p. 11663 . . .the signal from the clouded part usually still dominates the measured spectra, which thus mainly contains information from the atmospheric above the cloud. -> I am not convinced that this is necessarily true for a cloud fraction of 10% only. If we as-

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sume a cloud albedo of 1 (probably much too high for 2.3 micron) and a surface albedo of 5% then the weight of the clouded part is $0.1 \times 1 = 0.1$ compared to $0.05 \times 0.9 = 0.045$. So the non-clouded part can easily contribute 50% to the total radiance.

p. 11664 In contrast to the systematic dependence of the CO PVCD on cloud height, the CO PVCDs are almost independent of the selected effective cloud fraction threshold (see Fig. 2). -> There is a somewhat larger difference between 20 and 40 N for high clouds between 10%CF and 40%CF. Could this be caused by ice clouds that have a relatively low cloud albedo (due to strong ice absorption at 2.3 micron) so that a 10%CF criteria is too low?

p. 11667: In other words: The retrieved (too low) cloud top height fits well to the retrieved (too high) CO PVCD. -> Do you have some indication that the magnitude of both effects is similar otherwise there will be no significant compensation.

p. 11667 In this section, we compare CO profiles from SCIAMACHY observations with the results of two atmospheric models. -> Do you also consider averaging kernels in this comparison?

p. 11670 . . . were also reported in other studies (e.g. Gloudemans et al., 2009, De Laat et al., 2010) -> this is only including SCIAMACHY and MOPITT. What about TES or IASI? -> How do these models compare to aircraft profiles ?

p. 11672: In general, very good agreement between SCIAMACHY observations and model results is found (except for the systematic underestimation of the measurements by the models discussed above), with some distinct differences discussed below. -> I do not believe that you can argue that there is a very good agreement. As you rightly point out in brackets, the values are very different. So do you mean that there is a very good agreement in profile shapes or in the latitudinal and longitudinal distributions??

p. 11673: These spatial patterns are not found in the model data, which might be related to the vertical distribution of biomass burning emissions in the model, partly

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related to mixing processes between the boundary layer and the free troposphere

-> The heat generated by fires will cause pyroconvection which is not well captured by models while most models simply assume that biomass burning emissions are injected only in the boundary layer. Which schemes are adopted by the models here?

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