

Interactive comment on “Application of

SCIAMACHY and MOPITT CO total column measurements to evaluate model results over biomass burning regions and Eastern China” by C. Liu et al.

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First of all we want to thank Jos de Laat very much for his constructive comments! They helped us to improve our manuscripts and to add further information. Our detailed answers to the comments are found below.

Although an interesting paper, there are quite a number of questions and issues as outlined below. The most important one is the normalization procedure using MO-
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PITT data that is being used. Throughout the paper it is insufficiently evaluated and quantified what the effect of this normalization is. As the authors correctly note, the normalized SCIAMACHY data can not be considered as independent data. However, despite this important concern and drawback, little to no effort is put in evaluating and quantifying the impact of this normalization on the eventual data. The hope is that the normalization will not affect spatio-temporal patterns too much, but there is no way of determining from what is presented in the paper whether or not this is the case. Without such an evaluation, it is impossible to determine whether or not we are looking at basically SCIAMACHY, a combination of SCIAMACHY and MOPITT or in the worst case basically only MOPITT. Furthermore, as outlined in the specific comments, there are several indications suggesting that the IMAP CO is positively biased, possibly or even likely related to the normalization (bullets 3, 4 and 5.4).

Author comment: We agree that more information on the effects of the normalisation should be added to the paper. The following new figures have been added:

New Fig. 3: Spatial patterns: The figure shows global mean CO VCDs for four years (2003-2006) for different stages of the normalisation and cloud correction: a) without normalisation and cloud correction, b) with normalisation but without cloud correction, and c) with normalisation and cloud correction. While the absolute values of the CO VCDs change after each processing step, the spatial patterns remain almost unchanged. This indicates that the spatial patterns of the SCIAMACHY CO VCD are independent from the normalisation with MOPITT data (and also from the cloud correction).

New Fig. 7: Validation and temporal variation: This figure shows validation results using ground based FTIR stations at different latitudes. Again results for different stages of the normalisation and cloud correction are shown: a) without normalisation and cloud correction, b) with normalisation but without cloud correction, and c) with normalisation and cloud correction. The comparison results indicate that: a) similar seasonal patterns are already found in the uncorrected data for most stations b) in general the

agreement of the seasonal patterns improves after the normalisation with MOPITT data (and also after the cloud correction). c) also the absolute values show better agreement after the normalisation with MOPITT data (and also after the cloud correction).

New Fig. A2 in the appendix: Temporal variation for selected areas: Like in new Fig. 7 the seasonal variation for some of the areas is shown, which were also used for the comparison with MOPITT and model results. In contrast to the results in new Fig. 7, here SCIA data are averaged over larger areas. Thus, the scatter of the data is smaller and the seasonal patterns are more obvious. The comparison is again shown for different stages of the normalisation and cloud correction: a) without normalisation and cloud correction, b) with normalisation but without cloud correction, and c) with normalisation and cloud correction. It is seen that the seasonal patterns are already obvious in the uncorrected data. The new figures indicate that the relative spatial and temporal patterns are of the new SCIAMACHY data set are independent from the MOPITT normalisation.

Detailed comments

1. Figure 1 indicates that the IMAP SCIAMACHY CO total columns after the regular retrieval are still significantly too small compared to MOPITT and thus reality, since a recent validation study MOPITT V4 CO shows no biases [Emmons et al., 2010; ACP]. This begs the question to what extent the a-posteriori normalization with MOPITT is justified when so much of the signal is determined by this normalization. If, as is suggested, the normalization only affects background conditions and thus acts as a sort of offset-correction, then spatial patterns and temporal variations in the SCIAMACHY data should compare well with other observations (see also bullet 9). Otherwise, there is a serious problem with the interpretation of this dataset. Unfortunately no attempts are present to study the contribution of the normalization procedure. Given the potential importance of the normalization procedure this should be analyzed and presented in more detail. Such an analysis should include the spatiotemporal variations of the normalization, i.e. the differences before and after normalization spatially (averaged

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over all years) and temporally (seasonal cycles for certain areas like in Fig 1).

Author comment: We agree with Jos de Laat that additional information should be provided. This was done in the revised version of the manuscript, see also our reply to the first comment (above). We added the Emmons et al. (2009) reference to our paper, and also the Deeter et al. (2010) paper, see next comment).

2. Note that for validation of MOPITT V4 data one should refer to Emmons et al. [2010; ACP]. Also note that MOPITT V4 does not suffer from the biases detected in MOPITT V3 which are discussed in section 3, page 1279 (line 20-28). Since there does not appear to be a bias in MOPITT the MOPITT V3 biases should not be referenced to. Finally, there still is a MOPITT bias over dry desert areas [de Laat et al., 2010; JGR], but those areas are not covered by the validation data in Emmons et al. [2010; ACP]. This will be discussed under the next bullet.

Author comment: We added the Emmons et al. (2009) reference from the list below. It presents validation of MOPITT version 3 and a short outlook on reduced biases in the (at that time) upcoming version 4. Since we found no Emmons et al. 2010 ACP paper about validation of the MOPITT version 4, we searched for additional validation papers. A detailed description of the MOPITT version 4 algorithm and validation is given in Deeter et al., 2010: Deeter, M. N., D. P. Edwards, J. C. Gille, L. K. Emmons, G. Francis, S.-P. Ho, D. Mao, D. Masters, H. Worden, James R. Drummond, Paul C. Novelli The MOPITT version 4 CO product: Algorithm enhancements, validation, and long-term stability, *J. Geophys. Res.*, 115, D07306, doi:10.1029/2009JD013005, 2010. We added this reference and information on the validation results of version 4 to the revised version.

3. The normalized SCIAMACHY columns are compared with MOPITT in Fig. 1 for an area over the Sahara. Interestingly, in de Laat et al. [2010; JGR] it is shown that MOPITT appears to suffer from a bias over dry desert regions (~ 0.3 - 0.5 10¹⁸ molecules/cm²), something which has also been detected in IASI data and has been

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attributed to uncertainties in InfraRed emissivity of dry sandy surfaces by the same IASI team [George et al., 2009; ACP]. If CO columns over the Sahara for SCIAMACHY and MOPITT are then this suggest that due to the normalization the SCIAMACHY columns actually overestimate CO and that the normalization thus biases the SCIAMACHY measurements. Note that this “desert” bias is still present in MOPITT V4, see also the comparisons in this paper with model data (Figs. 11b, 11d and 11f.).

Author comment: This is an interesting point and we added this information to the revised version. However, after having a detailed look at the selected regions, it turned out that the region studied by de Laat et al. (2010) is located south of the area selected in our study (north of 20°N). Investigating global maps of MOPITT CO VCDs (version 4) it turns out that the ocean land-bias is stronger in the south, especially during months of biomass burning. Also the land-ocean bias seems to be smaller in version 4 compared to version 3. Thus it seems that the agreement between MOPITT and our SCIAMACHY VCD over the Sahara is not necessarily in contradiction with the findings in de Laat et al. (2010). It should also be noted that good agreement between MOPITT and SCIAMACHY over the Sahara was also found in Buchwitz et al. (2007). We added this information and also the George et al. (2009) reference to the revised version.

4. This paper offers no validation other than a comparison with MOPITT, which, given the use of MOPITT for the normalization of SCIAMACHY, can not be considered as an independent inter-instrument evaluation. However, the Liu [2010] PhD-thesis does present a limited validation with five FTIR stations [Liu et al., 2010; Figure 8.6, page 78; see attachment]. The results show that there is a good agreement for Wollongong for both absolute values and seasonality. For Kiruna, Bremen, Harestua (all European and close in vicinity) and for Lauder, the average appears to be OK but the observed seasonality is not seen in the SCIAMACHY observations. This is highly relevant information which unfortunately is not present in the paper. Given these validation results, an obvious question then becomes what this means. In a recently published detailed validation study of the IMLM SCIAMACHY CO total columns with FTIR measurements

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[de Laat et al., 2010; AMT] a very good correlation was found for example for Kiruna, although that validation also used SCIAMACHY measurements over low-altitude ocean clouds [see Gloudemans et al. 2009 for a description of the method]. The not so good comparison of the IMAP data with FTIR may be related to the use of SCIAMACHY land measurements only, but may also reflect other retrieval errors. Note that in de Laat et al. [2010; AMT] for IMLM a difference (bias) was found with the Wollongong FTIR station. This was attributed to localized biomass burning in the Wollongong region and local geography, which is a well established phenomenon, thus suggesting that the good correspondence between IMAP and FTIR for Wollongong may be spurious and potentially the result of the normalization, which appears to introduce a positive bias in CO (see bullet 1). Given the number of corrections that must be applied and the unknown impact of the normalization the authors should put in much more effort in the validation and evaluation of their data. Because of the current status of other SCIAMACHY products and recent studies that have been published (as well recent publications for MOPITT, IASI and AIRS CO), assessment of the data should move well beyond qualitative analyses and “visual” identification of similarity in spatio-temporal patterns.

Author comment: We agree that validation results should be added to the paper (see also our reply to the first comment (above). In addition to the validation exercises shown in Liu (2010) (PhD thesis), further ground based stations were considered (new Fig. 7). The validation results show that the agreement improves after applying the different correction steps (normalisation and cloud correction). Also the seasonality is in general well reproduced by our SCIAMACHY CO product. This information was added to section 4 and to the abstract and conclusions.

5. The authors introduce a cloud correction scheme for land observations. In itself this is an interesting and probably valuable procedure, but there are a number of drawbacks the way it is performed in this paper which also in general should be considered.

5.1. It is not convincingly show that the FRESCO cloud top pressures can be used here. FRESCO uses the O2A band around 760 nm whereas the SCIAMACHY CO

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measurements are made around 2350 nm. Gloudemans et al. [2009] show that for low and optically thick clouds FRESCO cloud tops compare well with cloud tops estimated from IMLM CH₄ measurements around 2350 nm. However, and unfortunately, Gloudemans et al. [2009] did not discuss the same comparison for high altitude clouds. This – unpublished – data suggests that there are significant differences in FRESCO and CH₄ cloud top heights for clouds with altitudes of 400 hPa or higher. As such, use of the FRESCO cloud tops for the cloud correction may thus introduce errors in the cloud correction over land for high altitude clouds. It should be shown more convincingly that those errors do not make matters worse.

Author comment: It is true that cloud properties are different in the spectral range, in which FRESCO is analysed and in the spectral range used for the CO retrieval. In particular, the cloud top albedo decreases towards longer wavelengths. We accounted for this dependence by using a cloud top albedo of 40% instead of 80% in the red spectral range, see e.g. Nakajima and King, 1990: 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. We can not comment in detail on the deviations of the CH₄ cloud top heights and FRESCO cloud top heights for high altitude clouds. However, we have no reason to assume that FRESCO cloud top heights have major problems, as it was validated by independent data sets. In any case, since the bulk of the atmospheric CO column is usually located close to the surface, the exact cloud top height for high altitude clouds has only a very small influence on the cloud correction. From the validation of our SCIAMACHY CO VCDs (see new Fig. 7) we conclude that in most cases the cloud correction improves the agreement with the independent data.

5.2. The fact that there are differences for the corrected and uncorrected data itself does not justify the use of a cloud correction (see Fig. 5). It is important to show that there exist biases or errors that can be attributed to cloud contamination and that are reduced after the correction. Otherwise, it is unclear if results improve and whether a

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correction thus is required or not.

Author comment: In our quick response to the comments of Jos de Laat (Fig. 4) (http://editor.copernicus.org/index.php/acpd-11-C610-2011.pdf?_mdl=msover_md&_jrl=10&_lcm=oc108lcm109w&_acm=get_comm_file&_ms=95) We have shown that there exists a systematic shielding effect of clouds on the SCIAMACHY observations of the atmospheric CO absorption, even if only small cloud fractions are considered. The strength of this shielding effect depends on the amount of pollution. Both findings indicate that systematic errors occur, if no cloud correction is performed. We added this information to the text (section 4). We also added the figure to the appendix (new Fig. A1). Also the validation results (new Fig. 7) show that in most cases the cloud correction improves the agreement with the ground based observations.

5.3. The cloud correction is based on surface reflectances from MODIS. However, only annual means surface reflectances are used (page 1280, line 16-18). It is well known that the surface reflectances around 2350 nm are strongly dependent on vegetation and that there are large areas where the surface reflectance exhibits a (strong) seasonal cycle. Not using seasonally varying surface reflectances thus potentially introduces another bias. Better would be to use seasonally varying surface reflectances rather than annual means, or otherwise to estimate how large this bias can become.

Author comment: This is a very good point! We investigated the effect of the seasonal variation of the surface albedo on the cloud correction. Although there is a systematic seasonality of the surface albedo, it turned out that the amplitude is typically only a few percent. The resulting errors of the retrieved CO VCD are below 1% in almost all cases. Even for the strongest amplitude of the surface albedo of about 20%, the resulting deviation of the CO VCD is still <5%. We added this information to the revised version (section 4).

5.4. For Fig. 7 it is suggested that some enhancements in CO over China are probably

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related to the effect of clouds, also because published results from other algorithms do not show a similar enhancement. However, the difference plot of SCIAMACHY CO columns due to the cloud correction (Fig 5.) does not show large changes in this particular area. Hence, it appears unlikely that the systematically higher values could be attributed to the cloud correction. Furthermore, the comparison of SCIAMACHY and MOPITT in Buchwitz et al. [2007; ACP] shows a rather good agreement for the same region. In addition, validation of IMLM CO [de Laat et al., 2010; AMT] with FTIR measurements over Japan – strongly affected by outflow of China - also shows a good agreement and not significant differences. Yet in this paper the comparison for this region with MOPITT (Fig. 8) as well as with models (e.g. EMAC-H; Fig. 10) shows that SCIAMACHY is higher than MOPITT, which, given the SCIAMACHY results from other groups, suggests that SCIAMACHY is positively biased. Note that the fact that for different regions biases differ as well (see comparison with MOPITT and models in Fig. 10) could be related to the latitudinal dependence of the normalization. This further relates to bullets 1 and 9 and the question what the effect of the a-posteriori normalization is.

Author comment: In our opinion the effect of the cloud correction in Fig. 5 (Fig. 6 in the revised version) shows a rather strong effect (about 40%), which indicates that differences between our data and the results of other studies might be caused by cloud effects. However, since this is no proof that the differences are indeed caused by the cloud effect, we removed this statement.

6. It is noted that the cloud correction should to a first order lead to a correction of aerosol effects, although only in case of scattering aerosols. Unfortunately it is not verified that this indeed does improve the data product. Furthermore, it is well known that in particular over Southern and Eastern Asia there are a lot of absorbing aerosols, in which case the cloud correction will not help. Absorbing aerosols also are present over tropical biomass burning areas. Hence, without some sort of evaluation of the cloud correction in relation to aerosol type it can not be determined if the cloud correction

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indeed does improve matters in case of all aerosols. It just may not be the case.

Author comment: We agree that in the presence of absorbing aerosols, the cloud correction might not be able to correct for the effects of aerosols. Thus we added the following statement to the revised version: 'It is, however, important to note here that in the presence of absorbing aerosols, the situation is more complex (see Leitão et al., 2010), and in extreme cases, no correction of the aerosol effects will be accomplished. However, if the absorbing properties of the aerosols are similar in the spectral range of the CO absorption and the spectral range where the cloud properties are determined, the aerosol effects are at least a partly corrected.' We also added the Leitão et al. (2010) reference.

7. Fig. 9 shows a comparison between MOPITT and normalized SCIAMACHY measurements. The SCIAMACHY instrument-noise errors are not considered here yet they would help identifying whether differences are statistically significant [see how that works in de Laat et al., 2010; JGR]. It is important to see if any of these differences are statistically significant.

Author comment: This is a good point! Indeed over almost all regions the deviations are strictly not significant according to the SCIAMACHY noise errors. Nevertheless, since across large areas the deviations are consistent (e.g. over the USA in Jul-Sept), we conclude that these maps still provide valuable information on systematic differences between MOPITT and SCIAMACHY. Again, it should be noted that no exact agreement between both sensors should be expected, because different a-priori assumptions are made. We added this information to the figure caption of Fig. 9 (Fig. 11 in the revised version) and to the text.

8. As with Fig 9., in the comparison of SCIAMACHY with the models significance levels are missing in Fig 11a, 11c and 11e. Again, it is high valuable to know where differences between SCIAMACHY and models are significant given the large noise errors of SCIAMACHY observations.

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Author comment: In principle we agree to take into account the uncertainties of the SCIAMACHY CO VCDs. However, it seems that the noise error is probably not an appropriate measure here. By inspecting Fig. 11 (Fig. 13 in the new version), it becomes obvious that the spatial patterns of the SCIAMACHY-model differences are not dominated by noise. Instead, consistent and rather smooth patterns are found. It seems that the SCIAMACHY noise error overestimates the true random error (see also Tangborn et al., 2009). This can also be concluded from the comparison of the seasonal variations (Fig. 10 or Fig. 12 in the revised version), where the variation between successive months is smooth and much smaller than the SCIAMACHY noise errors. We added the following information at the end of section 2.3: 'From the spectral retrieval, also the uncertainty of the derived CO VCD is determined. Typically, for individual measurements, these errors are dominated by the limited signal to noise ratio (see e.g. Gloudemans et al., 2006). Here it is interesting to note that especially from the comparison with model results it can be concluded that the true random error is probably substantially smaller than the noise error derived from the spectral retrieval.'

9. Figs 10. As mentioned earlier, it is crucial to know what the pre-normalized SCIAMACHY data looks like in comparison to the model (and indirectly to MOPITT). If the difference is basically an offset, then it is clear that the seasonal cycle is present in the SCIAMACHY observations. However, if not, then the seasonal cycle stems from the normalization and thus from MOPITT, which begs the question what the added value of SCIAMACHY then is. This also applies for Figs A1 and A2.

Author comment: We added a new figure (Fig. A2) which clearly indicates that the seasonal cycle is also present in the uncorrected SCIAMACHY CO VCDs.

10. Figs 11b, 11d and 11f all show that the MOPITT "desert" bias is still present in V4 (see the clear enhanced differences over for example the Sahara or Australia, in particular in the warm seasons). This is not mentioned in the paper, but it is an important result.

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Author comment: We added the following information to the text (at the end of section 7.2): 'It might be interesting to note that over desert regions, MOPITT CO VCDs are often higher than the model results indicating a possible 'desert bias' as discussed in de Laat et al. (2010) and George et al. (2009).'

11. Figs 11b, 11d and 11f also show large differences between land and ocean during cold seasons. This is clearly an effect of the reduced sensitivity to the lower troposphere of the IR MOPITT measurements. Hence, care should be taken with using those observations. In previous studies in general the practice was to exclude measurements where the a-priori contribution was more than 50%. This effect should be discussed and preferably those measurements should be avoided in case of total column measurements. Or, alternatively, it should be quantified. Given the use of MOZART model results as a-priori for MOPITT there is a real danger that differences with MOPITT are interpreted as real differences where they actually reflect differences with the MOZART model.

Author comment: We disagree that the differences between MOPITT and model results are caused by varying sensitivity as long as the sensitivity is correctly described by the averaging kernels. Since we apply MOPITT averaging kernels, the differences must be related to other reasons like e.g. errors of the model simulations or satellite measurements.

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