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Atmospheric Chemistry and Physics Discussions

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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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 MOPITT 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 cannot 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).

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

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.

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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.

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 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.).

4. This paper offers no validation other than a comparison with MOPITT, which, given the use of MOPITT for the normalization of SCIAMACHY, cannot 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 [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

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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.

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 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.

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 correction thus is required or not.

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.

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

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further relates to bullets 1 and 9 and the question what the effect of the a–posteriori normalization is.

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 cannot be determined if the cloud correction indeed does improve matters in case of all aerosols. It just may not be the case.

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.

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.

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.

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,

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in particular in the warm seasons). This is not mentioned in the paper, but it is an important result.

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.

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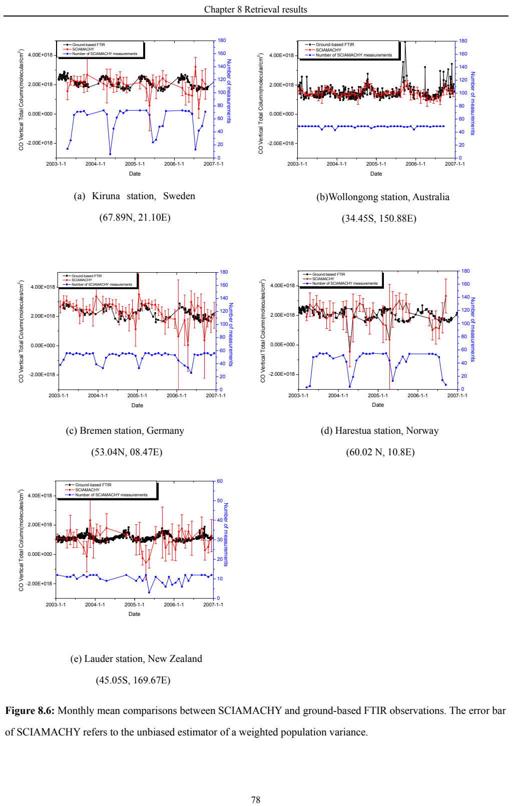

Fig. 1. Page 78 PhD thesis Liu

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