Atmos. Meas. Tech. Discuss., 5, C1525–C1528, 2012
www.atmos-meas-tech-discuss.net/5/C1525/2012/

Atmospheric Measurement Techniques Discussions

Interactive comment on "SCIAMACHY WFM-DOAS XCO_2: reduction of scattering related errors" *by* J. Heymann et al.

Anonymous Referee #1

Received and published: 9 July 2012

To my knowledge, the paper under review is the fourth paper in a series of papers which compare the SCIAMACHY WFM-DOAS CO2 product v2.x to TCCON and/or CarbonTracker. The other papers are Schneising et al., 2011, Schneising et al., 2012, Heymann et al., 2012. These studies are referenced appropriately by the manuscript. The present manuscript shows that the reanalysis of two postprocessing filters and the addition of a cirrus filter improve the WFM-DOAS CO2 product. This is new insight into the dataset but – in the view of collecting scientific mass - one might wonder if a previous publication such as Heymann et al., 2012, was the more appropriate place to cover it.

I have some methodological concerns which have to be addressed before publication

in AMT can be considered.

Comments: Increasing importance is indicated by '+, ++, +++'.

Section 4.1.1 (p.4292):

++ Why do you use such a wide spectral range for cloud detection (1.395-1.410 micron, while 1.390-1.410 micron in the figures)? There should be wavelength ranges in this band that are entirely opaque for essentially all occurring ambient H2O abundances. This would greatly simplify the empirical definition of a reference intensity.

+ "If the deviation of the measured intensity [. . .] to a reference intensity [..] is larger than a factor of 2". I find the wording confusing. The threshold is put at 3-times the reference intensity (figure 3), right? Consider to rephrase.

+++ Did the simulations resulting in Figure 3 and Table 1 use Mie scattering properties or cirrus scattering properties? This is important, since previous publications [eg. Schneising et al., 2011] found that cirrus scattering was the most important error contaminating the WFM-DOAS dataset. If cirrus scattering is not considered by the simulations, this has to be reevaluated.

++ Table 1 covers too few scenarios too draw conclusions such as discussed in the paragraph beginning with p.4293, l.21. Please extend the parameter range.

Section 4.1.2 (p.4294):

+++ "The reference O2 column is determined from the US standard atmosphere." (p.4294, l.10) Does this imply that meteorology ie. high- and low-pressure systems are not considered to calculated the O2 column for each individual sounding? One might wonder if deviations between reference O2 and retrieved O2 at least partially originate from wrong reference O2 due to this crude assumption.

+++ Equation (2) aims at correcting errors in XCO2 due to aerosol- and cloud-related errors in retrieved O2. The main reason to retrieve O2 simultaneously with CO2 is to

correct for aerosol- and cloud-related errors in XCO2 by ratioing (p.4290,l.21). Either equation (2) is actually undoing the O2 lightpath correction or the O2 lightpath proxy does not work and one could have calculated XCO2 with reference O2 in the first place (if it was meteorologically correct, see previous comment). Please seriously examine the approach (or the wording if I misunderstood things).

Section 4.2 (p.4296)

++ 8 locations are selected to estimate a monthly regional-scale scatter and a single measurement precision. How are these stations selected? Are they representative of geophysical variability or are they potentially challenging regions for the algorithm?

Section 7 (p.4300):

+++ Figure 7 shows exemplary seasonal mean WFM-DOAS and CarbonTracker XCO2. I suggest showing differences between WFM-DOAS and CarbonTracker which would save half of the panels and give more relevant insight. Further, I suggest showing additional exemplary maps for fall and winter. Why are the datasets smoothed by a 2D-Hann window (figure caption)? Seasonal averaging on 0.5deg x 0.5deg should be sufficient to remove statistical error components. Additional smoothing operations are to justify in the manuscript.

Technical comments:

+ Abstract: The abstract is too long. Its first part reads like an introduction.

+ p.4288, l.1: Avoid extensive self-referencing.

+ p.4288, l.17: Important references missing eg. Oshchepkov et al., Connor et al., Yoshida et al.,

+ p.4290, l.6 and elsewhere: integration time -> exposure time

+ section 3: The algorithm description lacks a short paragraph on how aerosols are treated.

+ Table 2: Merge with table 3.

+ Figures: Most figure legends lack units.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 4285, 2012.