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Interactive comment on "Large-scale validation of SCIAMACHY reflectance in the ultraviolet" *by* G. van Soest et al.

Anonymous Referee #1

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General Comments:

The paper is scientifically sound and describes an interesting new method for reflectance validation which has been applied to a larger set of SCIAMACHY data. Some of the results of previous studies which were based on only a few data (like the 10 to 20% offset in the reflectances) could be confirmed, and there are also new findings presented, e.g. the possibility to identify errors in the data processing.

The paper fits well within the scope of ACP and may be published after some corrections described in more detail below.

Especially, it is suggested that the authors add some more information about the sen-

sitivity of the method to the various input parameters of the radiative transfer model (RTM). Although this is partly addressed in Tilstra et al. (2004), there is no estimation given on the errors resulting from e.g. remaining clouds or aerosols in the ground pixels. An end-to-end error estimation for the reflectances (or for the computed differences) would help to judge upon the quality of the results of the study.

Specific Comments:

1. p. 1775, equation 1:
   Please define μ_0.

2. p. 1776, equation 2:
   Please define R_{sim}.

3. p. 1776, 2nd last paragraph:

   (a) The wavelength spacing of the RTM grid is the same (1 nm) as the spectral resolution (after convolution), which means that the spectra which are ratioed are effectively largely undersampled (sampling 1). This may cause problems if the wavelength calibration of the SCIAMACHY spectra is not the same as for the RTM data.

   (b) Is it adequate to run the RTM with a 1 nm wavelength grid using convoluted ozone cross sections, instead of performing the RTM calculations at higher spectral resolution (with corresponding ozone cross sections) and convoluting the resulting reflectance spectra? What is the error resulting from that?

   (c) Please specify which type of synthetic slit function has been used (boxchar, Gaussian, ...).

4. p. 1777, 3rd paragraph:

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(a) Please clarify: Does “only pixels with a cloud fraction less than 5% are included” mean that if one pixel has a cloud fraction larger than 5%

i. the whole substate is excluded, or,

ii. only this pixel is excluded from the average over the substate?

(b) Even a small amount of clouds (5%) may have a significant impact on the absolute reflectance. Please given an error estimate for the effect of clouds.

5. p. 1778, line 14:
Please justify the assumption that no aerosol is present and give an error estimate for this assumption.

6. p. 1779, line 21:
How can a difference in d_{R} of 0.01 between East and West pixels be identified if the bin size of the histograms is 0.02? Moreover, it is not clear if the method is sensitive enough to identify such small differences at all. Tilstra et al. (2004) give some values for the sensitivity to surface albedo, ozone column and profile which are in the order of some percent (depending on wavelength) for the calculated reflectance, and the error due to remnant clouds and aerosols is not even included. On the other hand, the error is probably reduced by binning/averaging. Please clarify.

7. p. 1780, lines 7 and 8:
The sentence “One reason why the distribution appears wider in Fig. 2 is the normalisation of the colour scale to a smaller number of substates.” is unclear. How can a different normalisation have an influence on the standard deviation?

8. p. 1780, lines 10 an 11:
The sentence “A residual effect may persist in the comparison between observed and simulated reflectance, leading to a positive bias (1% in Fig. 2) and smaller accuracy for East substates.” is also unclear. Where is the bias coming from?

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Interactive Comment

9. p. 1781, last paragraph:
   Have data in the SAA been masked out for the results presented in section 3.1? If not, the influence on the statistical results (at least for lower wavelengths) should be estimated.

10. p. 1782, line 15:
    Why is a different filter for ground albedo inhomogeneity used and what is the difference?

11. p. 1782, lines 19 to 21:
    No real conclusion can be drawn from the comparison between 2003 and 2004 data because it can not been distinguished between a seasonal/annual effect and an effect due to the different ozone profiles. A direct comparison between results using SCIAMACHY limb ozone profiles and results using the TOMS climatology (for the same data set) would help here.

Technical Corrections:

Only one technical correction:

p. 1784, line 19: Remove quotes around “lower”.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1771, 2005.