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

G. van Soest et al.

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We thank referee #1 for her/his careful reading of our paper, and for helpful comments and suggestions. We will use those to improve the manuscript further. In this document we respond to the remarks of the referee.

General comments:

Besides a generally positive judgment, the referee suggests to include more information of the sensitivity of our method to input parameters of the RTM. We agree with the referee that this information is currently lacking from the paper. However, a thorough

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discussion of this subject is a study in itself, and the inclusion of it would make the manuscript twice as long and would distract from the point we wish to make here: the features that can be revealed by studying a large set of reflectance data.

A paper describing this sensitivity study, building on our previous work (Tilstra et al., 2004), was accepted for publication in JGR very recently. We will add a reference to this work (Tilstra et al., 2005). We will also include a paragraph in sect. 2.2 of the manuscript stating the main conclusions of that paper: An end-to-end error estimate is about ± 3% over most of the UV, rising up to ± 8% around 305 nm where the sensitivity to ozone is largest. Our treatment of clouds, or lack of it, adds an offset of up to about 2% for the longer wavelengths. The sensitivity to ozone is the most important source of error by far for $\lambda < 300$ nm; surface and lower atmosphere parameters have no effect at all in this regime. For $\lambda > 330$ nm, the sensitivities to surface and lower atmosphere parameters such as clouds and aerosol are the main determinants of error. In the intermediate range $300\,\mathrm{nm} < \lambda < 330\,\mathrm{nm}$, sensitivities to all input parameters contribute to the simulation error.

In addition, the referee suggested inclusion of an error estimate at specific points in the analysis, justifying certain assumptions we make about cloud filtering, the neglect of aerosol, etc. We have calculated these errors and will discuss them at appropriate locations in the paper. See our response to specifics below.

Specific comments

1. + 2. We have included definitions of μ_0 and R_{sim}.

3a. The referee raises concerns about the undersampling of the ratio d_{R}, the quantity that is central in our analysis. After submission of the paper to ACPD we became aware that there might indeed be a problem. We performed a study on a limited number of spectra, studying the effects of undersampling. The result is that the additional r.m.s. error incurred in the simulation was $< 1\%$, but that in certain places, a remarkable odd-even stagger appeared, most likely as a result of discrepancies in the wavelength calibrations of observations and simulations. We also observed this stagger in our data (cf. Fig. 2, primarily between 310 and 340 nm). The errors due to undersampling appeared to have a very limited impact on our conclusions, which do not concern fine spectral scales. Hence we decided not to redo our entire analysis, but rather chose to confess to our ignorance and include the above discussion in the paper. A paragraph outlining the effects of undersampling on our data, along with a warning for future users, will be added to section 3.1.

3b. If inelastic scattering and small-scale structure in the scattering cross sections are negligible, light of different wavelengths is independent, and the scattering properties of the atmosphere at a given wavelength are representative of a small spectral window around that wavelength. We believe these conditions are met, and therefore do not expect problems as a result of properly resampling the absorption cross sections and performing the RTM calculations on a coarse grid (though we could have been more careful w.r.t. undersampling, see above). We will add a remark to this effect to the manuscript.

3c. The synthetic slit function was Gaussian. This will be mentioned in the manuscript.

4a. + b. We will clarify our cloud filtering mechanism, and include a discussion of the errors caused by treating all pixels with less than 5% effective cloud cover from the cloud algorithm FRESCO as cloud free.

We estimate the error due to ignoring clouds by plotting $\bar{d}_{\mathrm{R}}(380)$ (the average d_{R} be-

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tween 370 and 390 nm) as a function of cloud fraction as given by FRESCO. In this wavelength range, the upper atmosphere is optically thin, so the error resulting from clouds or surface parameters should show up most clearly here. We find that d_{R} increases linearly by about 0.05 when the cloud fraction is increased from 0 to 0.05. (The scatter is of a similar magnitude.) In addition, we made a histogram of the cloud fraction. The average cloud fraction was 0.020, close to the maximum of the distribution. This means that, on average, we underestimate the reflectance by 2% compared to the observations. This error contributes to the scatter in the “upper” flank of the histograms.

The effect can be compensated in principle by adjusting the ground albedo, but our study of the albedo sensitivity of the TOA radiance (Tilstra et al., 2005) demonstrates that this is not straightforward, as for most common surface types the sensitivity is wavelength dependent.

5. The effects of neglecting aerosol are very limited. Significant contributions to TOA radiance due to aerosol scattering basically only occur in desert regions. These areas are usually (erroneously) flagged as clouded by the used version of FRESCO and henceforth not included in the analysis. Furthermore, scattering aerosol is detected as cloud by FRESCO. Again based on results from Tilstra et al. (2005), we have estimated the error, and found it to be approx. 2% at most for wavelengths longer than 300 nm.

6. Part of the statistical analysis of the histograms was done by fitting Gaussian distributions to the data. If the scatter around the model curve is small enough, and if the model represents the actual distribution of the data well, it is very well possible to determine the fit parameters with a better resolution than the histogram bin size. The analysis showed that the difference between East and West pixels is indeed significant. We will make our use of statistical fits more explicit in the paper.

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We believe the difference we find between East and West pixels is real, because a significant difference persists in an average over thousands of scenes. We also have an explanation, in terms of sun glint, which will be outlined more clearly in the article (cf. 8).

7. The referee is totally right. This is a very confusing sentence. What we meant to express by it is the following: the spectrum of histograms has been normalized to the absolute maximum count. For the West states, this maximum count turns out to be a bit of an outlier, and hence, in the normalized colour scale, the West distribution *appears* narrower. We economized a bit too much in phrasing this. Since, on second thought, we do not see how the appearance of the width of a distribution in a certain colour scale confers a whole lot of important physics, we will remove the sentence.

8. The next confusing sentence, however, *was* meant to transmit some important information, being the effect of sun glint in East substates. Sun glint causes more pixels to be flagged as clouded (accounting for the smaller number of included substates) and raises the average radiance, which is why, we believe, the d_{R} is 0.01 less negative for East substates. We will explain this more clearly.

9. The reviewer is concerned about the effect of inclusion of data from the SAA in the analysis of Sect. 3.1. These data have indeed not been masked out, but their effect is very limited. About one in every 25 data points is in the SAA, and these would appear in Fig. 2 as a very low density cloud of points for short wavelengths at (large) positive d_{R}. The fits to the histograms are hardly affected by the presence of a few scattered points in the wings. We will explain this in the manuscript.

10. The study of geographical dependencies in 2003 data was performed before we had a good analysis of the error due to surface albedo. Hence we set the albedo

inhomogeneity filter to a conservative value of 10% allowed difference, leading to a high rejection rate. Later we discovered that we could relax this filter to 20% without compromising the analysis, and changed the setting for the subsequent studies. We will detail this in the paper.

11. The difference between 2003 data obtained with limb ozone profiles and 2004 data obtained with climatological profiles can indeed not be strictly attributed to the ozone input data. We had no intention of doing that at this point in the text. However, drifts and seasonal variations in SCIAMACHY reflectance data have been minor until now, whereas the validation study of Brinksma et al. (2004; reference in the paper) has demonstrated a number of shortcomings of the SCIAMACHY limb ozone profiles. Therefore we do believe that the most prominent differences we observe are due to the ozone profile data used for the RTM input. We will add a paragraph to the discussion voicing the above disclaimer.

Reference

Tilstra, L. G., van Soest, G., and Stammes, P.: Method for in-flight satellite calibration in the ultraviolet using radiative transfer calculations, with application to SCIAMACHY, J. Geophys. Res. doi:10.1029/2005JD005853, 2005

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