Comments on ACP manuscript on 'Modeled and Measured Effects of clouds…' by de Vries and Wagner.

The Henvey and Greenstain approximation is not an adequate model to represent scattering properties of clouds. The authors are aware of the inadequacy of the H-G phase function. In several parts of the manuscript the authors blame this deficiency as the possible reason why the obtained results are not as expected. Yet, in the manuscript they offer no justification for its use. The analysis presented here would more valuable if realistic representation of cloud microphysics was used for the radiative transfer analysis.

The authors use the term 'CloudUVAI' to refer to the residue associated with the presence of clouds when the LER is used to explain the satellite-observed spectral spectral dependence of UV reflectance. CloudUVAI is a misnomer. Clouds are not aerosols. What the authors refer to as CloudUVAI is simply a residual quantity used as a measure of the success of the LER approximation to predict the observed spectral dependence using a simple approximation of the surface-atmosphere column[Torres et al, 1998; Herman et al., 1997]. The residue is zero when the LER adequately explain the observation. The term UVAI should only be used when the source of non-zero residues is the presence of aerosols. In all other instances the term 'residue' should be used.

The theoretical definition of the AI in equations 2a and 2b ignores the fact that there is a level of noise associated with the measurements. In practice the rigid cutoff at zero does not apply. In fact most of the residue values the authors try to explain as physically meaningful fall in the range of the noise. The noise level depends on the presence of other geophysical non-aerosol-related sources of error as well as sensor calibration. Known calibrations issues of the SCIAMACHY sensor contribute to a larger noise level. The authors should clearly establish what the expected AI uncertainty associated with the sensor is. Based on calibration problems discussed elsewhere the quoted 0.2-0.3 range is not realistic.

When discussing the results in figure 1 (H-G phase function) to those of figure 2 (LER approximation) which shows the performance of the two approaches in predicting the observed spectral dependence, the authors conclude that the H-G parameterization is better. I do not understand the basis of their conclusion. The comparison of simulated residue shows that the LER approximation explains better the spectral dependence of the 'observations' (in this case the synthetic data). The LER yields lower residues and shows less angular dependence than the H-G. It also converges quickly to zero with cloud fraction. The H-G based calculations, on the other hand, show larger dependence to viewing geometry as well and dependence on the assumed asymmetry parameter. Thus, in the context of the residue concept, the LER is a better model than the H-G parameterization. Fig 1b also shows significant effect of g on thin clouds.

The authors should refer to the work of Ahmad et al [2004, JGR] who evaluated the LER approximation. Ahmad et al [2004] used accurate Me calculations to compare the performance of the more rigorous model representation of cloud scattering effects in explaining the 340/380 ratio a parameter directly related to the residual quantity. They showed (by comparing to actual measurements) that the modified LER approximation (MLER, an approximation that represents the reflectance of clouds as the combination of two LER surfaces of low and high reflectivity), explained the TOMS observations under a variety of conditions. The more rigorous Mie-theory based approach was not better than the LER method in explaining the observations.

Other comments:

In their literature review of AI applications the authors missed the most comprehensive use of near-UV observations for global aerosol characterization in terms of AOD and SSA derived based on the AI's information content [Torres et al, 2002, 2005, 2007].