Authors' reply to comment of Anonymous Referee #1

We thank the referee for the constructive review of our paper.

Before we address the individual referee comments, we give a brief overview of the most important changes compared to the original version.

1) MLER UVAI

We only recently found out (see comments of referee 2) that the so-called MLER cloud description, described in Ahmad et al. (2004), was used to improve NASA's UVAI algorithm of TOMS in the update to version 7 (besides many other changes). Unfortunately, this information is not available from existing algorithm description documents and caused us to believe that the TOMS UVAI were still based on the LER assumption (like the operational UVAI from GOME, SCIAMACHY, and GOME-2). The application of the MLER concept for the calculation of UVAI constitutes an important improvement, especially for measurements in the presence of clouds: cloud effects on the UVAI are largely reduced. In the revised manuscript we include MLER UVAI and discuss the differences to the LER UVAI. One interesting finding is that although the cloud effects are reduced using the MLER concept, they are still not sufficiently corrected: in the presence of clouds (and absence of aerosols), and MLER UVAI can still substantially deviate from zero. Also, like for LER UVAI, the MLER UVAI determined in the presence of clouds cannot be interpreted in a quantitative way, because they are not representative for the respective UVAI without the cloud influence.

2) Cloud correction for UVAI

As suggested by one of the reviewers, we investigated whether the effects of clouds and aerosols on the UVAI were additive. This was a very valuable suggestion, because it turned out that this is generally not the case! This finding had two important consequences:

a) cloudUVAI cannot be used to correct the cloud effect for satellite measurements that are also affected by aerosols. But it can still be used to test whether a measured UVAI in the presence of a cloud is caused by the cloud alone, or if there may be an additional effect from aerosols.

b) we developed a scheme for the quantitative correction of the measured UVAI in the presence of clouds, taking into account the simultaneously measured cloud properties (mainly the effective cloud fraction). Although this correction scheme is not perfect, we were able to derive more realistic UVAI for measurements with small effective cloud fraction than without that correction.

3) Simulations with a Mie model

In addition to Lambertian and Henyey-Greenstein (HG) cloud models, we also simulated cloud effects based on more realistic Mie models. While in general the results are similar to the results of the HG model, in specific cases substantial differences between Mie and HG cloud models occur caused by the different phase functions. These effects are systematically investigated and described in the revised version.

General comments

The authors simulated the effect of clouds on the UV Aerosol index using MERIS and SCIAMACHY data. The method could be interesting for the study of effect of clouds on aerosols. The authors mainly discussed the negative part of the UV aerosol index, which is not so popular for readers. It might be good to emphasis the differences between the UVAI used here and the positive part used to detect absorbing aerosols. As I understand the authors try to use UVAI quantitatively, it would be nice to see some examples, especially the negative UVAI.

Author reply:

In the revised version of the manuscript we give similar emphasis to both the negative and positive part of the UVAI. Both parts are affected by all three cloud effects mentioned in the paper. However, the magnitude of the cloud effect is systematically different. In the revised version we added new figures (Figs. 11 and 12) which describe in detail the cloud effects on the UVAI depending on effective cloud fraction and (relative) cloud height.

Although the aim of the current paper is the investigation of the effects of clouds on UVAI, we also added a new section on the correction of measured UVAI based on simultaneously retrieved effective cloud fraction. It should however be noted that this simple correction scheme has its limitations and should be rather seen as a starting point for more sophisticated future algorithms.

We have re-written the abstract and introduction to make the focus of the paper more clear.

Specific comments

Page 24137, line 20 'If aerosol radiative effects are to be quantified on a global scale, both ...' What aerosol radiatve effects do you mean here? How to calculate aerosol radiative effects using aerosol index? Do you mean the effect of aerosols on the radiative fluxes, the radiative forcing of aerosols?

Author reply:

We meant the radiative forcing of aerosols, the quantification of which requires information on aerosol absorption, layer height, as well as optical thickness. This section was re-written and the sentence was removed, but aerosol radiative effects are still mentioned elsewhere in the text.

Page 24138, line 1 "The quantitative interpretation of UVAI is not straightforward ..." Why do you want to use UVAI quantitatively? SSA can be derived from smoke particles, is it possible to derive SSA for the scattering aerosols from UVAI?

Author reply:

There are several reasons to use aerosol information from UVAI quantitatively, most notably: (1) UVAI are determined in the presence of clouds; (2) UVAI give information on aerosol absorption without *a priori* input; and (3) there is a long time series of UVAI in existence, starting with the TOMS instruments. Reason (1) was mentioned elsewhere in the text, but reasons (2) and (3) are now added to the introduction section, which reads: "Although difficult to use in a quantitative manner, UVAI are very suitable to indicate the presence of aerosols and their absorption properties without *a priori* input, such as assumptions on size distribution or particle shape. The long time series of UVAI available, starting with the TOMS instruments in the late 1970s up to the currently active sensors SCIAMACHY, OMI and GOME-2 provides us with a unique and interesting dataset."

It is not easy to derive SSA for scattering aerosols due to the relative insensitivity of UVAI to scattering aerosols (compared to absorbing aerosols). The following statement was added in the revised manuscript:

"In principle, the retrieval of AOT and SSA of scattering aerosols from UVAI is less complicated because UVAI is nearly independent of the altitude of a scattering aerosol layer (Penning de Vries et al., 2009). This advantage is partially negated by the reduced sensitivity of UVAI to scattering aerosols compared to absorbing aerosols."

Page 24138, line 14 "Much less known is the fact (2)..." Is it (2) or (3)? What is the effect if the clouds and aerosols are mixed? Could you explain more about (2)?

Author reply:

There was an error in the previous manuscript, and (2) should read (3). This section was thoroughly revised, as were other parts in the manuscript referring to cloud and aerosol effects on UVAI, which are now explained in more detail.

Page 24138, line 24 - 25. What UVAI do you get after the correction, subtracting the cloudUVAI from UVAI? Is the corrected UVAI equal to the clear UVAI?

Author reply:

This was an extremely useful comment (see also comments of referee 2) that led us to completely rethink our cloud-correction concept. On inspection, our assumption that cloud and aerosol effects on UVAI could be summed was found to be incorrect. As a consequence, we re-wrote large parts of the manuscript and added Section 5, which is dedicated to a new cloud-correction procedure.

Page 24139, line 2-3 "information on cloud heterogeneity" It is not clear to me. Do you mean the 3D structure of the clouds?

Author reply:

No, we mean the 2D structure: the distribution of the cloud within the plane of the satellite pixel. In the process of re-writing, the referred sentence has disappeared.

Page 24139, line 13-15 The UVAI sensitive to aerosols is because of less Rayleigh scatter event? Why not to explain aerosol and Rayleigh scattering have different wavelength dependence and the scattering phase matrix of aerosol particles and air molecules are different? If the aerosols is close to the surface it will not shield the *underlying atmosphere.*

Author reply:
The phenomenological explanation of UVAI was improved and now reads:
"Because aerosol optical properties (scattering phase function, spectral dependences of optical thickness and absorption) differ from those of molecules, the presence of an aerosol layer causes a change in contrast between a wavelength λ and a reference wavelength λ_0 in the UV range."

Page 24140, line 26-27 'SCIAMACHY was developed to . . .. by use of . . . (DOAS)? Could you reformat this sentence? Although many DOAS algorithms are used in SCIAMACHY product, SCIAMACHY is more than a DOAS instrument.

Author reply:
The referee is right; the sentence was rephrased. It now reads:
"SCIAMACHY is mainly used for the measurement of the gaseous composition of the Earth's atmosphere (Bovensmann et al., 1999; Wagner et al., 2008) by use of Differential Optical Absorption Spectroscopy (DOAS, (Platt and Stutz, 2008))."

Page 24141, line 4 'The feature that make SCIAMACHY unique is the limb-nadir matching . . .'. Could you reformat this sentence? Normally people understand that SCIAMACHY has limb and nadir observation mode. If you say 'limn-nadir matching' it feels like more emphasized on the matching but not limb or nadir measurements themselves.

Author reply:
This sentence was also rephrased:
"The feature that makes SCIAMACHY unique are the alternating measurements in the limb and nadir modes, which allow collocated measurements of the total column of a trace gas (in nadir geometry) and its stratospheric profile (in limb geometry)."

Page 24141, line 25 'visible wavelength range'? Do you mean UV wavelength range or visible? Why do you use H-G phase function? How difference is the H-G phase function compare to phase function for ice clouds particles? How can you know the clouds are water, ice or mixed phase? Change 'Henyey-Greenstein parameterization' to 'Henyey-Greenstein scattering phase function parameterization', also for the texts later in the paper.

Author reply:
The UV-visible wavelength range is meant; it is corrected in the manuscript. The Henyey-Greenstein phase functions were initially used for their simplicity. In the revised manuscript, simulations using phase functions from Mie calculations were included and the results compared to those obtained using Henyey-Greenstein phase functions. For nadir geometry and small solar zenith angles, the differences were found to be minor, but for selected viewing geometries differences on the order of 0.5 UVAI unit or larger were found. These results are presented and discussed in the revised manuscript.

Page 24142, line 8-12? Do you need to mention cloud droplet size?

Author reply:
The droplet size is now mentioned.

Page 24142, line 24-25 What do you mean 'homogeneous and heterogeneous clouds'? From your example it seems that the cloud algorithm cannot distinguish cloud fraction and cloud albedo.

Author reply:
The referee is right in saying that the HICRU cloud algorithm cannot distinguish cloud fraction and cloud albedo. We have rephrased the sentence, which now reads:
"A major shortcoming of CF_{eff} is that it cannot discriminate between optically thin and optically thick clouds: for example, a CF_{eff} of 0.5 can be due to clouds with a cloud albedo of 0.8 (COT = 50) covering 50% of the pixel, by clouds with a cloud albedo of 0.4 (COT = 10) covering the complete pixel, or anything in between."

Page 24143, line 4-5? '. . .determined by the sub-pixel structure..? What is the subpixel structure of the cloud? Do you use the sub-pixel structure in your correction for UVAI?

Author reply:
We mean the distribution of the cloud within a satellite pixel. In Sect. 4.1 we use cloud properties from MERIS to determine the sub-SCIAMACHY-pixel structure of clouds and improve the agreement between measured UVAI and modelled cloudUVAI. The sentence was changed to:
"The dependence of cloudUVAI (UVAI resulting from an unpolluted cloud) on CF_{eff} is determined by COT as well as CF_{geom}."

Page 24143, line 13 '. . .UVAI does not depend strongly on cloud altitude' Could you give some numbers here? How strong is the UVAI dependence on cloud altitude? What is the surface albedo used in your calculations in Fig. 1?

Author reply:
The dependence is only very small: less than 0.1 UVAI units. This is now mentioned in the paper. The surface albedo is 0.05, which is now mentioned in Sect. 2.3 and in the figure caption.

Page 24143-24144 Could you refer to 'Fig. 1(a)' instead of 'left panel in Fig. 1'? Please change it for all the figures in the page. Fig. 1b Why do you use effective cloud fraction for 'thick clouds' instead of 'thin clouds'? Are they the same values? Is the UVAI actually cloudUVAI? Could you specify in the caption of the figures what results instead of 'Results from. . .'? Please clarify UVAI and cloudUVAI in the texts.

Author reply:

The different panels in Figs. 1-4 are now labelled A and B and the figure captions were changed to be more informative.
The effective cloud fraction is equal to the (geometrical) cloud fraction for thick clouds, but for consistency the axes of Figs. 1-3 are all labelled CF_{eff}. This is now explained more explicitly in the manuscript.
The referee was right in criticizing the inconsistent use of UVAI and cloudUVAI. This was improved throughout the manuscript, and explained in more detail in the introduction.

In Fig. 1 The thin and thick cloud type are not important for CFeff larger than 0.7 which means in reality the thin or thick cloud type assumption is important. How many percentage of the clouds with CFeff larger than 0.7?

Author reply:
The frequency distribution of CF_{eff} depends on region and time of year, but it can be estimated for the studied region in the Pacific Ocean by looking at the size of the data points in Fig. 8. (see also the average cloud fraction in the figure below). A large fraction of pixels has $CF_{eff} < 0.7$, therefore, as the referee observes, the choice of thin or thick cloud assumption is important.

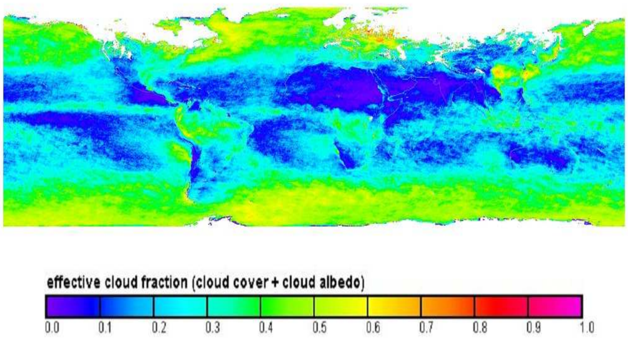

Figure 1. Average HICRU effective cloud fraction for winter (From the PhD thesis of M. Grzegorski, University of Heidelberg, 2009)

Page 24144, line 8 Change 'Figure 2' to 'Fig. 2' Can you add the lines in Fig. 1(a) to Fig. 2 to make the comparison more clear?

Author reply:
Both figures were changed in the revised manuscript, but we decided not to add the lines from Fig. 1 to Fig. 2 because we felt that the figure would become too crowded.

Page 24145, line 11 – 19 In your simulation you use positive and negative viewing angle. Is it better to explain the east-west bias as relative azimuth dependence? Have you looked the UVAI dependence on the scattering angle?

‘The scatter on the curve in Fig. 3b is caused by . . .’. Why are there small errors in the calculated cloud reflectances? How accurate is SCIATRAN for these calculations? The scatter in Fig 3b might indicate that the simulations are not stable. Why does it not happen in Fig. 3a? Have you used enough ’streams’ or ’quadrature angles’ in the simulations.

Author reply:
In our simulation we use positive viewing angles with relative azimuth angles of 0° and 180°, as is now explained in the text. For simplicity, in Fig. 4 viewing geometries with a relative azimuth angle of 0° are given positive viewing angles, for relative azimuth angles of 180° the viewing angles were multiplied by -1. The East-West bias is caused by the dependence of cloudUVAI on relative azimuth angle, or rather, on the scattering angle. This is now formulated as follows:
“The largest effects are seen for the positive viewing angles (RAZI = 0°), nominally on the eastern part of SCIAMACHY’s swath, and imply an intrinsic East-West bias of cloudUVAI for cases where SZA is larger than about 20°.”
The referee is thanked for the observation that not enough streams might have been used for the SCIATRAN cloud simulations, as this was indeed found to be the case. The calculations were repeated with higher precision, as shown in Fig. 4.

Fig. 3 caption, please move ‘(a)’ in front of ‘Viewing geometry is nadir’, because viewing angles are changing in Fig. 3b. The symbol in Fig. 3b should be ’triangle’ according to the caption, but ’circle’ is used.

Author reply:
This has been done.

Page 24145, line 24 ‘The LUT of cloudUVAI contain . . .’, please also add relative azimuth angle. Do you have surface albedo in the LUT? How about O3 absorption?

Author reply:
The relative azimuth angle was added in the sentence. Surface albedo was 0.05 for all calculations, as mentioned in Sect. 2.3. Variation in ozone absorption was not taken into account; an ozone profile for mid-latitude summer was assumed. This is now also mentioned in Sect. 2.3.

Page 24146, line 3 ‘. . .compared to UVAI measured by SCIAMACHY’. UVAI cannot be measured. Please reformat this sentence.

Author reply:
The sentence was changed.

Page 24147, line 6 Change ‘unclouded pixel’ to cloud-free pixel

Author reply:
The sentence was changed.

Page 24147, line 9 Can you give the UVAI values?

Author reply:
The effect of non-absorbing aerosols with AOT = 0.1 is less than 0.2. This is now given in the text.

Page 24148, line 1-4 Have you tried other cloud models or RT models in the simulations? What are the possible calibration problems? Can you do some simulation including the calibration errors? It could be interesting to see the effects on cloudUVAI due to different calibration errors.

Author reply:
We have now added results based on a Mie cloud model, but this could not explain the discrepancy between measured UVAI and modelled cloudUVAI, as discussed in some detail in Sect. 6. Comparison with results from simulations using the McArtim Monte Carlo model or the DAK model (in both cases with Henyey-Greenstein clouds) showed excellent agreement with the results obtained using SCIATRAN.
It is known that SCIAMACHY still suffers from calibration problems (see references in the text), which are worsened by the degradation of the optical channels. Also, the polarisation correction is imperfect, partly due to the fact that the polarisation measurement devices do not work properly. As it is unclear what the exact effects on SCIAMACHY reflectances are (if we would know, we could in principle correct for them), it is unfortunately not possible to assess the errors in UVAI caused by these calibration inaccuracies.

Page 24149, line 15 How do you explain the small variability in the simulated UVAI? What parameters could impact the simulated UVAI values?

Author reply:
The variability in simulated cloudUVAI comes from the dependence of cloudUVAI on viewing geometry, as is demonstrated in Fig. 4. Small variations of CF_{eff} within a CF_{eff} bin can also lead to differences in modelled cloudUVAI.

Page 24150, line 13 Fig. 8-9, the labels for the figures are wrong. Change Fig. 8 A2-F2, A3-F3 to A1-F1, A2-F2. Also change the labels in Fig. 9

Author reply:
The figures were changed and it was made sure that the labels are correct.

Page 24152, line 25 '. . . the clouds cause significant UVAI'. What is significant UVAI?

Author reply:
Significant UVAI here means: on the order of 1 UVAI unit. This is now said explicitly in the text.

Page 24153, line 3-6 Does the aerosol and clouds information have the same resolution? It seems that your aerosols information has higher resolution than clouds.

Author reply:
Aside from the high-resolution MERIS data used for the case studies in Sect. 4.1., the cloud data have the same spatial resolution as the UVAI.

Page 24153, line 10-12 Why the thick cloud assumption is better than the thin cloud assumption?

Author reply:
The agreement between the cloudUVAI determined using the thick cloud assumption and the measured UVAI appears to be better than for the thin cloud assumption (at least for $CF_{eff} > 0.5$). This section was thoroughly revised and this finding is now explained better: "Comparison of panels A-C in Fig. 10 suggests that for $CF_{eff} < 0.5$ the assumption of "thin" clouds (optically thin clouds covering an entire satellite pixel) is more appropriate, whereas for larger CF_{eff} "thick" clouds (optically thick clouds with variable geometrical cloud fraction) occur more frequently."

Page 24152, line 7 How can you know the phase function for a specific cloud?
Fig. 5. caption change (G, F) to (G) (F). change '(for the thin clouds)' to '(for thick clouds)'

Author reply:
Unfortunately, we do not know the phase function of (the scatterers of) a specific cloud. For this reason, we approximate the phase function with an assumed, effective Mie or Henyey-Greenstein phase function.
The figure captions were changed in the revised manuscript.