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Atmospheric Measurement Techniques
Discussions

Interactive comment on "A new method for the absolute radiance calibration for UV/vis measurements of scattered sun light" *by* T. Wagner et al.

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Reply to reviewer #2

First of all we want to thank the reviewer for the positive assessment of our study and for many valuable comments and suggestions. We followed most of them, as decsribed in detail below. Before we respond in detail to the reviewer's comments we briefly summarise the most important changes of the manuscript compared to the original version:

A) We carried out additional sensitivity studies for strongly absorbing aerosols (single

scattering albedo of 0.8). These results are added to figures 7 and 8 (old figures 5 and 6). We also carried out additional sensitivity studies for individual changes of temperature or pressure. The results are not shown in the paper (but in our detailed reply below), because the effects are very small. We added this information to the text.

B) As suggested by the other reviewer, we shifted the section on the influence of ozone on our calibration results from the appendix to main text. We also added a discussion on the importance of future studies on the effect of ozone, especially for the UV-B region, at the end of that new section.

C) We corrected two mistakes in the original version: 1) In Fig. 5 (old Fig. 3) and Fig. A3 (old Fig. A4) the simulation results were calculated incorrectly: instead of multiplying the output of the radiative transfer model (the normalised radiance) with the solar irradiance spectrum, the RTM output was divided by the solar irradiance spectrum. Because the values of the solar irradiance spectrum (in units of W/m^2/nm, see Fig. 2) are close to unity, this mistake was not immediately obvious). Fortunately, this mistake did not affect any of the results of our study.

2) The figure caption in Fig. 8 (old Fig. 6) was wrong: The original text: 'the scaling factors are divided by the scaling factors for the standard scenario' was corrected to: 'the scaling factors for the standard scenario are divided by the scaling factors for the different scenarios'

D) A new figure showing the spectral resolution of our instrument (Fig. A1) was added (to the appendix).

General Comments

Is the method applicable to solving the vexing question of irradiance calibrations too? Langley methods to calibrate direct beam irradiance are not generally applicable to global irradiances because of differences in the entrance optic configuration between the two modes of measurement.

Author reply: We think that our method is probably not suited for the calibration of irradiance measrements, because for such measurements, the effect of aerosols will be very similar for different SZA (the solar irradiation is attenuated by the total AOD (multiplied by the AMF) according to the Beer-Lambert law. Thus the (relative) SZA dependence will be largely independent from the aerosol properties (as long as they are constant with time). We added the following text at the end of the introduction: 'Here it should be noted that our method can not be applied to irradiance measurements, because for such measurements the relative dependence of the observed irradiance on SZA hardly changes with the aerosol load and properties (except for very high aerosol loads).'

This is an interesting and potentially very useful paper that makes ingenious use of what may be a fortuitous sza-dependence in zenith radiance signals, to calibrate radiances by means of the corresponding variations calculated with a RT model. The authors show that the calibration is largely independent of the aerosol parameters they tested (though further testing is recommended). The method seems to be most accurate in the UV-A region, with accuracies decreasing at longer visible wavelengths due to effects of aerosols, and decreasing at shorter UV-B wavelengths due to uncertainties in ozone absorptions. In addition to its demonstrated applicability with MAX-DOAS systems, the method may also be applicable to calibrating measurements of global diffuse irradiance (and hence global total irradiance). Also, in addition to providing an independent radiance calibration, the method can also be applied to estimate aerosol parameters, such as aerosol optical depth, and single scattering albedo. The latter is a particularly important parameter controlling irradiances in the UV-B region, where some anthropogenic organic aerosols absorb strongly. It would therefore be worth further exploring the extent to which the method can be used to infer aerosol properties in the UV-B region. The paper is well-written and clear. I recommend acceptance for publication in AMT subject to the minor changes suggested, and I look forward to open discussion around the points noted above.

Author reply: Many thanks for this positive assessment. Concerning the recommendation to further explore the accuracy of the method in the UV-B spectral range, we fully agree, and we added this suggestion to the conclusions. There we added: 'The accuracy of our method in the UV-B spectral range should be further explored in future studies based on measurements under constant ozone layer thickness during the period of the measurements.'

We also added the following text in the (new) section 2.2.1: 'From the measurements used in our study (which represent a rather extreme situation with a relative change of the ozone VCD of about 5% within 3 hours) we can not further explore the potential and accuracy of our calibration method in the UV-B spectral range in a meaningful way. Nevertheless, we expect that in the UV-B spectral range similar accuracies as for the larger wavelengths could in principle be expected, because usually the thickness of the ozone layer is well known from independent observation. Satellite observations have an accuracy of about 1 to 2 % (e.g. Loyola et al., 2011). Here it should also be noted that in principle the ozone VCD can also be derived from the DOAS measurement itself, but usually this is no standard retrieval product. Future studies based on measurements under constant ozone columns should explore the accuracy of our method in the UV-B spectral range.'

In this study, however it is not possible to further address this question, because of the strong spatio-temporal variation of the ozone VCD during our measurements.

Minor Points

Line 74. . . .and other instruments? Not confined to just MAX-DOAS?

Author reply: Many thanks for this hint! We changed the text to: 'Estimation of UV doses at the surface: From absolutely calibrated MAX-DOAS measurements (or other measurements covering multiple viewing directions) the UV fluxes at the surface can be estimated.'

Line 77. UV-B (UV-A is largely unaffected by ozone).

Author reply: Many thanks for this hint! We changed the text to: 'Here it should, however, be noted that a constant and known O3 column during the measurements is a prerequisite for an accurate radiometric calibration in the UV-B spectral range (315–280 nm).'

Line 81. In this application it could be argued that irradiance (not radiance) is the "essential" quantity.

Author reply: We agree and changed the text to: 'Estimation of the energy yield of photovoltaic cells. Here measurements of the angular distribution of the sky radiance are important for photovoltaic cells, which are not directly opposed to the direct sun light. But also for photovoltaic cells opposed to the direct sun light, the yield resulting from the diffuse radiation can be significant (especially for high aerosol loads).'

Line 94. Should this be "zenith sky irradiance"? Is there any dependence on the field of view?

Author reply: Many thanks for this hint! We replaced 'sky radiance' with 'zenith sky radiance' We also added the following information to the text: 'Here it is interesting to note that typical (MAX-) DOAS instruments have rather small fields of view (usually about 1°), but also measurements with larger fields of view could be in principle be used as long as the correct field of view is considered in the radiative transfer simulations.'

Line 168. Seems odd that the validation paper precedes the publication by 6 years.

Author reply: The reason for this apparent discrepancy is that the later study (Chance and Kurucz, 2010) scaled their spectrum to the spectrum described in Thuillier et al. (2004).

Line 176 (and elsewhere). Should this word be "convolved"?

Author reply: Many thanks for this hint. 'convoluted' was replaced by 'convolved' in the

whole text.

Line 189. How does it vary with wavelength. Linearly, or otherwise? A plot might help.

Author reply: We added a new figure (Fig. A1) to the appendix.

Line 209. For some aerosol conditions, the single scattering albedo might be very different from your assumed value of 0.9. I think some sort of sensitivity analysis is needed to investigate possible effects of changes in the assumed value (e.g., in the range 0.8to 0.99).

Author reply: We added results for a scenario with a single scattering albedo of 0.8. Figures 7 and 8 (old figures 5 and 6) are updated accordingly. Interstingly, for this scenario the deviation of the derivd scaling factors from those of the standard scenario were the largest of all scenarios (except for the neglect of polarisation). Such large error would occur if scenarios with weak aerosol absorption would be applied to measurements with strongly absorbing aerosols. Fortunately, for such cases, also the derived AOD strongly deviates from the true AOD. Thus a wrong value of the assumed single scattering albedo could be easily identified.

We updated the text describing the effect of the single scattering albedo in section 3: 'For increasing aerosol absorption decreasing scaling factors are found, but for moderately absorbing aerosols (single scattering albedo ^{3}0.9) the differences are small (< 5 %). However, for a strongly absorbing aerosol (single scattering albedo of 0.8) the derived scaling factors are about 5% to 10% smaller compared to the standard scenario. Thus if during the measurements such strongly absorbing aerosols are present the derived scaling factors would be by 5 – 10% too small if less absorbing aerosols were assumed in the radiative transfer simulations. Fortunately, such cases could be easily identified by the large deviation of the simultaneously determined AOD from the AOD derived from sun photometers. Also the RMS is slightly larger.

Line 239. Add "in this wavelength range".

Author reply: Added

Line 270. To test this more fully, Temperature differences and pressure differences should be perturbed separately.

Author reply: We followed this suggestion and performed additional simulations for scenarios with either temperature or pressure changed. It turned out that the deviations compared to the results for the standard scenario are typically a little bit larger than for the combined modifications of temperature and pressure (see attached figure). This indicates that the effect of both changes partly cancel each other. Nevertheless, the changes of the resulting scaling factors are still small (<2%) and below or in the same range as other uncertainties. We added the following information to section 3: ‘Here it is interesting to note that the effect of a combined change of temperature and pressure is slightly smaller than the effects of individual changes of pressure and temperature (not shown). Nevertheless, they are still small compared to other uncertainties.’

Line 312. Should this be “smallest”, rather than “largest”? (see Fig 6).

Author reply: ‘largest’ is correct. To make this statement more clear, we added the boundaries of the largest SZA range (36° - 90°) to the text.

Line 359. Same Earth-Sun separation? Also, would you expect close agreement in radiances, unless aerosol extinctions at both sites were similar?

Author reply: Many thanks for this hint! We added the following information to the text: ‘Note that the sun-earth distance during the measurements in Hannover was almost the same compared to our measurements (0.3% difference). Also the AOD and the ozone column were similar: the AOD at 550 nm derived from MODIS TERRA is about 0.15 (obtained via the NASA Giovanni website: http://giovanni.sci.gsfc.nasa.gov/giovanni/). The ozone column density derived from OMI was about 330 DU (obtained from the TEMIS website: http://temis.nl/index.php).’

Line 415. It would be useful, if possible, to quantify the additional uncertainty (as a

function of wavelength) that would result from reasonable specified changes in ozone or aerosol properties. Perhaps the sensitivity to ozone can be derived from Fig A3?

Author reply: The effects of changing aerosols properties can be directly derived from the differences of the scaling factors obtained for the different (aerosol) scenarios shown in Fig. 8 (old Fig. 6). So in our opinion, these tests already explored the sensitivity on the aerosol properties in detail (and found that it is rather small). Note that we investigated three more scenarios for singly scattering albedo of 0.80 and separate changes of the temperature and pressure profiles.

Concerning the suggestion to investigate the sensitivity for ozone absorption, we agree that this is an important issue, especially for the UV-B spectral range. As stated in our reply to the general comments above, we recommend that this question should be addressed in future studies (using measurements with constant ozone VCD).

Line 579. I would suggest adding a vertical dashed line at SZA=79 too, and to label the key SZA values in the upper x-axis.

Author reply: Many thanks – we followed this suggestion and updated Fig. 1.

Line 629. Shows that the sky radiance at 435 nm is about half that at 335 nm, despite the lower ET spectrum. You could make the point that this demonstrates the importance of increased scattering at shorter wavelengths.

Author reply: Many thanks for this hint! In particular it helped us to identify an error of the original plots: instead of multiplying the output of the radiative transfer model (normalised radiance) with the solar irradiance spectrum, the RTM output was divided by the solar irradiance spectrum. Because the values of the solar irradiance spectrum are around unity (in units of W/m^2/nm, see Fig. 2), this mistake was not immediately obvious, but we now corrected Figures 3 and A4. Fortunately, this mistake only occurred in the preparation of the figures. All other results were not affected.

Line 856. Please clarify if this is “ratio”, or a “difference”. Also, a smaller range for the y-axis (e.g., 3% rather than 30%) would be appropriate in the lowest panel. Or could use a log y-axis in all cases?

Author reply: We changed the figure caption to: 'Left: Simulated radiances for the different O3 profiles shown in Fig. A1. The profiles differ in shape and O3 VCD. Right: Relative differences of the radiances compared to the standard profile (345 DU) (the radiance differences are divided by the radiances at SZA = 70°).'

We think that the use of the same y-scale in all sub plots allows for the direct comparison between the different results. Thus we decided to keep the y-scale unchanged.

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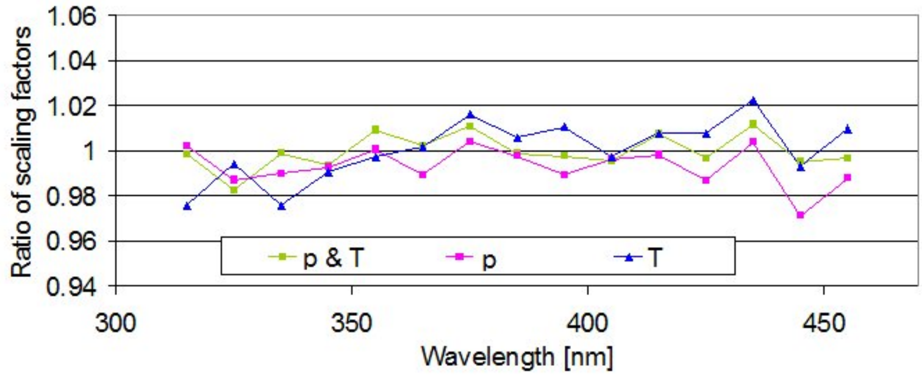

Fig. 1. Ratio of the scaling factors for the standard scenario divided by those for the modified scenarios. (p & T: temperature and pressure profiles are both changed; p: only pressure profile changed; T: onl