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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 comment from P. Kiedron:

Authors are commended for doing this computationally extensive task to obtain radiometric calibration. Most likely running Monte Carlo vector RT code is amounts to "tracing" more virtual photons than there were in actual measurement. Below I list several remarks that occurred to me when reading the manuscript.

Author reply: Many thanks for these very helpful comments!

By the way: concerning the numbers of 'virtual' and 'real' photons, we found that the

real measurements record at least one order of magnitude more photons than used in the simulations (the actual ratio depends on wavelength and solar zenith angle).

(1) The normalized radiance in eqs. (1) is atmospheric transmittance of radiance for given SZA, zenith and azimuth (SZ, SA) viewing angles, field of view angle, wavelength and wavelength FWHM.

Author reply: We added this information to the text.

(2) Page 5335: solar irradiance spectrum generated by Bernhard G. Bernhard, C.R. Booth, and J.C. Ehramjian, 'Supplement to "Version 2 data of the National Science Foundation's Ultraviolet Radiation Monitoring Network: South Pole"'SUPPLEMENT TO VERSION 2 NSF UV DATA: SOUTH POLE (Version 2 data of the National Science Foundation's Ultraviolet Radiation Monitoring Network: South Pole," J. Geophys. Res. 109, D21207, doi:10.1029/2004JD004937, 2004.) who used Kitt Peak high resolution spectrum with low frequency envelope from Gueymard's spectrum. I trust this experimental spectrum much more than the synthetic Kurucz's spectrum. The low resolution Gueymard's spectrum is also based on experimental data. Perhaps it should be emphasized that errors in solar spectrum are transmitted directly to the calibration constants of the proposed method.

Author reply: We added the following statement to the text: 'Here it should be noted that several solar irradiance spectra are available and are described in the scientific literature (see e.g. also Bernhard et al., 2004). But it is beyond the scope of this study to comment on the possible advantages or disadvantages of the different solar spectra. It should, however, be noted that the uncertainties of the derived radiance calibration will directly be proportional to the uncertainties of the used solar spectrum.'

(3) Page 5339. I find notation used in the equation (4) somewhat confusing if not misleading. The scaling factors S(AOD, lambda) is not dependent on i, i.e., i=SZA. Furthermore as it is instrument responsivity that converts detector counts to radiance units, it is independent of AOD. Formally, it might be dependent on AOD in the scheme

proposed in this paper in terms as it is merely a result of model errors and erratic model assumptions on the actual AOD and other parameters that parametrize the atmospheric state in the sense of the model. For this reason the actual responsivity (the scaling factor) is not dependent on AOD or any other atmosphere's parameters. The described method can yield many values of S depending on the "scenario". BTW, I would add AOD to the parameters (like SSA, g, phase function, pressure, profiles… and surface albedo) that describe atmosphere or define the "scenario" as a function of SZA. The only difference is that unlike other parametrs, AOD is presumably known from an ancillary measurement.

Author reply: Many thanks for this hint! We agree that the scaling factor does not depend on AOD and we removed this dependency from equation 4. However, we think that in our scheme the AOD is indeed treated in a different way compared to the other aerosol properties, as we determine the minimum of the RMS as a function of AOD for each scenario. Of course this choice is somehow arbitrary (also other dependencies could be exploited), but it is advantageous, because AOD is the most important quantity, which determines the sky radiance in ALL scenarios (including variations of temperature, pressure, surface albedo and Ring effect). Thus we chose the AOD dependence to determine the best scaling factor for each scenario.

Which S should be decided upon in the described scheme? The residuals r(i)=R(AOD,lambda, i)-S*D(lambda, i) should have no discernable trends or systematic excursions from r=0 line as function of i=SZA. Perhaps this should be used as an auxiliary criteria to decide which scenario is "right".

Author reply: This is an interesting suggestion. At the moment, however, our inversion scheme does not allow to use a more complex criterion for the determination of the best scenario. Also, because of the rather small differences between the results for the different scenarios (a few percent), we think it is not necessary to include such an additional criterium. But we mention this possibility now in section 3: 'In principle also other quantities than the RMS could be calculated, e.g. a function decsribing the

systematic dependence of the difference between R and S $\times$ D (see equation 4) as a function of the SZA. But we did not use such an option in this study, because the differences between the derived scaling factors for the different scenarios were found to be rather small (a few percent, see below).'

For the day considered in the paper (24/06/2009) AOD is not constant. One could use notation for scenario: SC={sc(i)}={AOD(i), SSA(i), g(i), phase function(i), pressure(i), profiles(i)… and surface albedo(i)} and then in eqs.(4) one would minimize RMS of residuals r(i)=R(sc(i),lambda, i)-S*D(lambda, i) with respect to S for a given set SC (scenario) of sc(i) and then from within different scenarios SC's pick that scenario for which r(i) demonstrate the smallest trends. However if two scenarios SC1 and SC2 produce the same values of RMS and different values of scaling factors S1 and S2 there is no way to tell which one is correct without experimental data.

Author reply: We do probably do not fully understand, what is suggested here. But we think that the proposed procedure will not add more useful information compared to the approach applied in our paper. Thus we decided not to introduce the suggested formulae in the revised version. Concerning the last point: Of course, from the RMS alone, not always a clear decision on the best suited scenario can be made. In such cases, the differences of the scaling factors for the different scenarios with the same minimum RMS indicate the uncertainty of the method. Fortunately, in most cases, this is no big limitation, because the differences of the derived scaling factors are small (especially if AOD information s available).

The statistics of S among the likely scenarios SC's that produce similar RMS values should define the uncertainty of this calibration method. The statistics could be subdivide in different cases when AOD is known or not, when SSA is known or not... It seems intuitively obvious that optimal day for calibration should be picked up among days that have clear sky and have constant AOD and furthermore have constant Angstrom coefficient. The latter brings you closer (necessary condition) to a case that within a given day scenario's SSA, g, profiles are constant. Meaning, lower number of scenarios.

Author reply: We agree and added the following text to the conclusions: ‘At best, even further aerosol properties like the single scattering albedo or the asymmetry parameter are known. This would allow to reduce the number of scenarios used for the calibration.’

(4) What is polarization sensitivity of the spectroradiometer in this study?

Author reply: We added the following information to section 2.1: ‘The measured light is transferred via a 1.5 m long wound up quartz fibre to a temperature-stabilised miniature spectrometer (Ocean Optics USB2000) and recorded by a one-dimensional CCD detector (Sony ILX511). Because of the transmission through the quartz fibre the polarisation sensitivity of the instrument is negligible.’

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5329, 2015.