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Atmospheric Measurement Techniques
Discussions

Interactive comment on "The instrument constant of sky radiometer (POM-02), Part I: Calibration constant" *by* Akihiro Uchiyama et al.

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The manuscript reports some very interesting data and application of various methodologies mainly from the the AERONET/SKYNET calibration framework for two instruments but the arguments for some of the results and conclusions are not convincing or not well explained. As in the part II paper there are a significant amount of assumptions and previous results without references that need to be listed. An expanded and re-written manuscript could fix these issues would be most welcome to all interested in atmosphere-based spectral Sun radiometer calibration. A brief summary of the major issues is below. Any manuscript for a global audience needs to conform to some international standards of nomenclature. Unfortunately, the authors use 'accuracy' as a quantitative property for the majority of the paper when 'accuracy' is a qualitative term

(i.e. good, bad, excellent); only in the last parts of the paper is the term 'uncertainty' used but without any explanation of what coverage factor or degrees of freedom. Similarly, the paper quite clearly calls aerosol optical depth (AOD) 'aerosol optical thickness'(AOT) when AOT = m*AOD, and it is only through equation (1) at line 327 that what the authors mean by 'optical thickness' becomes clear. The paper quite clearly demonstrates the 'calibration constant' of the POM-02 is not a constant for the majority of channels (if any) [and likely true for a number of spectral radiometers!]. Instead it may have been useful to define it as the 'coefficient used on a day that represents the signal at the top of the atmosphere at 1'AU and at a representative temperature of X degC'. So why persist in using the term 'constant'? This could have been a key conclusion of the paper rather than implied or assumed. What is a 'normal Langley method'? There are so many variations of 'the Langley method' in the literature that they could be listed on several pages. No reference was provided for the specific method used, and what was more confusing was the application of at least 4 variants of the 'normal' method resulting in Table 1 - and no reference on how the gaseous applications or temperature correction were done, or the reason for the very high standard deviations in an unknown set of MLO calibrations when gaseous and temperature corrections were applied. The non-description of the applied methodologies and the non-explanation of the variances is an example where some references or further detail is required. The temperature coefficients and their application to the raw signals is a key piece of information for other users. However, the section is another example where minimal methodology is presented. There was no experimental setup provided only that it 'was used to measure the temperature dependence of the pyranometer' or the likely uncertainty of the process and the choice of a representative temperature for each sensor. As written, it could almost be assumed that a single value was applied per 'Langley' period rather than individual measurements, and one would have to guess on the representative temperature. It was also disappointing not to see a comparison of the derived coefficients to the sensor manufacturers' specification sheets. The description of the temperature environment in the POM-02 is a very, very useful - though one could argue that use of the term 'temperature control' was not appropriate. In section 4 (line 180+) the results of the 'normal Langley method' are described in terms of 'errors' but there was no reference only a mean (weighted by an unknown weight or unweighted) hence use of the term 'error' is inappropriate for an unknown parameter of a probability distribution. But an examination of the table suggests these just the (unbiased) standard deviations and therefore only contribute to a single component of the total uncertainty of the 'normal Langley method'. As indicated previously, no indication is given for the increase in this uncertainty component when the sensor signals are corrected (in a manner unknown) when compared to no temperature correction. The lines 232 to 244 describe the likely variation in the 'calibration constant' obtained at MLO over a period of years for the reference POM-02, and summarized on Figure 5 which has a log scale likely because of the range in the Vo values. If the variation is important, then the results should have been scaled to say the 2010 calibration. It would then also be a better lead into the discussion of the interpolation method (and associated uncertainty) that could be required to ensure a required uncertainty (i.e. 2% for high AOD environments for an unknown air mass range - see the WMO (2005) for the working POM-02. The discussion on the reasons for the seasonal variation of the ILM was not convincing, and the lack of opportunity to perform of verification by using calibrating the working instrument with the reference instrument when the seasonal peaks and troughs of the ILM occur was disappointing. Given that the selection of true or apparent solar zenith angle, the airmass type, the rate of change of airmass, and the airmass range used are known to have a seasonal impact on derived 'calibration constants' derived from almost any Langley method variant it was disappointing they could not be examined even for the 500 nm channel of the Tsukuba POM-02. The authors applied a variant if the general method for the calibration of near-infrared channels. It is a pity that it wasn't applied to all wavelengths either using the reference POM-02 AOD or the most stable channel of the Tsukuba POM-02, and hence also test the small variations in wavelength over time. The comparison of the general and ratio (i.e Dobson spectrophotometer) method results to the 'AERONET/SKYNET' method-

ologies that have not changed since the inception of AERONET and largely based on the hand-held sunphotometer comparison procedures developed at NOAA by Ed Flowers in the 1960s would have been very interesting given the breadth of the excellent JMA data set.

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