Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-433-SC1, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "The instrument constant of sky radiometers (POM-02), Part II: Solid view angle" *by* Akihiro Uchiyama et al.

B. Forgan

bwforgan@bigpond.com

Received and published: 19 January 2018

The article may prove to be very useful once revised as it contains many positive elements. At present the majority of methods and results are more summaries of summaries and approximations of approximations without references to the considerable work behind them.

For example, the primary argument that the f() function extends out to 2.5 degrees is based on a single paragraph outlining a summary of measurements of a an imaging sensor and its shading with no indication of how and what are the uncertainties of the imaging sensor data. 'The' f() described in the manuscript is one of many f(), and in this case the use of a finite sized object of $\sim$0.5 deg in diameter (the Sun); and neglects the

likelihood of a unfocussed image as suggested by fig 1. Also missing are the algorithm explanation of (a) correction for airmass, (b) the circular approximation (which could be simply matrix addition), and (c) a very light description of the new interpolation; none have references to referred articles. Similarly, terms are introduced (e.g. Fo) without explanation - usually after they have been used in equations. The Table 2(a) summary of the MLO data is interesting though there is no indication of the number of scans collected for each wavelength in the period Oct-Nov 2015. Nor why the (unbiased?) estimate of the standard deviations is lowest for the 'SkyRad' Case 1 compared to the others in some cases by a factor of 2 with no explanation for the increase for the wavelengths < 500 nm; other data from other workers suggests similar std dev across most wavelengths for low AOD (or AOT) locations. Therefore while the bias may (and one repeats may) have improved the estimate of the 'true' SVA the uncertainty of the mean increased.

Given the incorrect use of the term 'aerosol optical thickness' to represent aerosol optical depth in historical papers it would be useful if the authors could define their use of the term (e.g. is it AOT = AOD * M) in the paper particularly near line 288.

The positives of the article are many including the issues with SkyPak smoothing and extrapolation in Fig 4. Though the argument as to why the SkyPak minima is always around 10^-4 (rather than just an algorithmic flaw in SkyPak) after scattering angle 1.4 is missing or why the divergence from the SkyPak f() occurs at about 0.8 deg when the extrapolation of the 'improved' method is implied to start at 1.4?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-433, 2018.