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Atmospheric Measurement Techniques Discussions

Interactive comment on "Quality assessment of solar UV irradiance measured with array spectroradiometers" *by* L. Egli et al.

Anonymous Referee #2

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In this paper, an extensive evaluation of a several spectroradiometers has been carried out. Intercomparisons of this type are very useful in describing the relative and overall performance of such instruments if data are handled in a 'blind' fashion and over a wide variety of atmospheric conditions. Using the data taken at the intercomparison, it is clear that the several instrument performed adequately with respect to the reference. Nevertheless, poor stray light rejection, combined with poor sensitivity and potential variable offsets make the accurate assessment of the UV data of many of the instruments very difficult.

With the increasing drive for measurement devices capable of rapid time responses in atmospheric chemistry, the use of spectroradiometers like the single monochrmomater instruments described here is likely to become much more widespread. The

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time resolution that these instruments offer is not usually found in scanning double monochromator instrument. Thus several of the instruments describe here could be an obvious choice for the measurement of actinic flux and/or UV measurement in the field. However, in order for the data to be useful and accurate with respect to measurement of flux and UV, the data presented in this paper supports other studies that show a full and extensive error correction regime must be employed to overcome the shortcomings of such instruments.

I was a little puzzled by the authors desire to report the agreement between the instruments as plots of the total UV index, rather than as actinic flux action spectra. Although the goal of the paper is to ascertain the performance of the instruments with respect to UV index, the overall UV index between instruments can agree but this can mask fundamental differences in the way instruments characterize the spectra. Two instruments can seem to agree over the total UV index but one my overestimate another by 50% in one region of the actinic flux spectra and underestimate by 50% in the other. The authors should explain this potential drawback in more detail.

Having said that, the paper describes several important findings about the nature of these spetoradiometers and describes how to employ necessary instrument corrections. Therefore, it is worth of publication in AMTD with a necessary corrections and clarifications.

Specific comment that should be addressed.

Page 13612, Line 10: “The knowledge of the entire UV spectrum from 290 to 400nm is essential to weight the measured spectrum with different action spectra” Knowledge of the actinic flux spectra gives info on atmospheric photolysis frequencies or J values. These are vital for quantifying atmospheric photochemistry and the atmospheric radical budget. Reference to this application should be included in the text.

Page 13612, Line 12: “solar UV radiation at the earth surface for human health protection, accurate instruments for measuring global solar spectral UV irradiance are

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required." This is also needed to test radiative transfer models and the authors should acknowledge such.

Page 13612, Line 15: "measuring the solar irradiance spectrum sequentially, which requires several minutes of scanning time" This statement is not true of all instruments as several authors have described instruments with scan times <1 minute over the 280-420nm range.

Page 13613: Line 6: "caused by the sharp cut off of the solar UV spectrum as a consequence of ozone absorption". The word "stratospheric" should appear here to clarify the sentence.

Page 13614: Line 9: "The double monochromator setup ensures stray light reduction to cover 6 orders of magnitude of irradiance levels, while the photomultiplier is able to detect irradiance levels as low as 10-6." Was this measured experimentally? If so, results of these experiments this should be included or referenced.

Page 13614: Line 15-19. A more detailed description of the topography of the field site would help understand the legitimacy of the data, especially at large solar zenith angles. Did the site offer a full 2pi field of view? Were the instruments mounted in an East-West plane of orientation to allow maximum capture of the solar diurnal cycle?

Page 13621, Line 11: "No stray light correction was applied to EKB" As the authors themselves indicate, together with a wealth of literature data describing ASTM type instruments, stray light (SL) evaluation and correction is critical in assessing the accuracy of any spectroradiometers. If this instrument was not corrected for SL then it seems obvious that this instrument will not achieve the accuracy of measurement sought in this intercomparison. Aside from the desire to further the large body of evidence as to the need to correct for SL, I do not see much point in considering the data produced by this instrument as part of the intercomparison.

Page 13621, Line 14: The authors mention the blind nature of the data acquisition.

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This is a crucial feature of any true intercomparison exercise and this point should be made earlier, preferably in the introduction and/or abstract.

Page 13623, Line 14: “overcast or cloudy skies are less critical than for a spectral analysis” I would disagree with this statement as the effect of clouds and aerosol’s in their ability scatter different wavelength’s by different amounts can reveal important bias owing to wavelength dependent effects that may not well characterized by some instruments.

Page 13626, Line 7: “This instrument shows an average ratio close to 1, indicating a good absolute calibration.” I would be careful with this statement as, indeed, one would hope that this sentence to be true but in fact, good agreement, does not necessarily mean good calibration in all cases.

Page 13626, Line 23: “Surprisingly the ratio decreases between 310 and 320nm with a rapid increase below 310 nm” I would not say that these data were surprising, I would say the data were originally quiet poor in terms of agreement, but got closer, (probably due to SL counts) so, not really better, just closer due to this artifact.

Page 13629, Line 23: “single spectra deviate more than 5% from the mean value at large SZA (Fig. 5).” A clarification to the end of the sentence “where SL is more of a factor” should be added.

Page 13630, Line 15: “This indicates that the overestimation in the UV-B part compensates the under estimation of the UV-A part resulting in a reasonable agreement for UV index” This is the crux of the problem with using total UV and not actinic flux spectra in such intercomparisons as described in my overall comments above.

Page 1360, Line 19, “However, a large deviation from the mean value (Fig. 5) may be attributed to the calibration which cannot account for the difference in atmospheric conditions”. A light source based calibration round robin would at a later date be useful in similar exercise in the future.

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Page 1360, Line 29: “Therefore, we may conclude that a stray light reduction using filters improves the measurements of ASRM, at least, for the determination of the UV index” Not really remarkable, just shows excellent SL rejection, a crucial factor that this paper and other authors have suggested (e.g. Edwards et al. 2003) must be very well quantified.

Page 1361, Line 19: “Therefore it may be necessary to add another layer of post processing where the cut-off wavelength in the UV-B range” This is the key take-home-message here. This should probably be done as a routine for all single mono instruments and the authors should make this clear in the discussion.

Page 1362, Line 14. Comma missing after “Therefore”

References A large intercomparison of several spectroradiometers was done in the late 1990s and is described in Calvert et al. 2003. Many of the conclusions of that study mirror those if this paper and hence, these previous findings should be extensively referenced.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 13609, 2015.