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Atmospheric Measurement Techniques Discussions

Interactive comment on "A new method for nocturnal aerosol measurements with a lunar photometer prototype" *by* A. Barreto et al.

Anonymous Referee #1

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Review for Atmospheric Measurement Techniques

Title: A new method for nocturnal aerosol measurements with a lunar photometer prototype

Authors: A. Barreto, E. Cuevas, B. Damiri, C. Guirado, T. Berkoff, A. J. Berjon, Y. Hernandez, F. Almansa, and M. Gil

General Comments:

This is an important paper on a new and potentially very useful instrument, a lunar photometer, which may provide significant additional measurement capability over existing commercially available instruments. In general I find the paper to be well written and the main discussion on the calibration methods of the lunar photometer to be detailed and relatively complete. However I find the paper lacks any mention of one of the most important issues in lunar photometry. There is no discussion of the maximum AOD that this photometer can measure as a function of wavelength, optical airmass (path length), and moon phase. This is important information, as even sun photometers have a limit on measurement related to the product of AOD * airmass, and this limit will be significantly lower for moon measurements due to lower signal, especially when the moon is less illuminated and also for shorter wavelengths. It would be useful to include some technical details such as signal/noise ratio values of this instrument for full moon illumination levels and also lesser moon phases. I also provide numerous specific comments below that I suggest the authors should consider in revising this paper.

I recommend that this revised paper be published after significant revisions and suggest that it could make an important contribution to the literature.

Specific Comments:

p. 5529, line 24: Please give a reference for the Stellar photometers here, such as Herber et al., 2002.

P. 5533, line 8: Why use two wavelengths, 938 and 937, that are only 1 nm apart? Only one is required to retrieve columnar water vapor (such as by AERONET), and it would be more useful to include a channel such as 380 nm or the WMO standard 368 nm to characterize AOD spectral variation at shorter wavelengths. If short wavelength measurements do not provide sufficient signal/noise with this photometer then you should state that.

P. 5533, lines 9-10: You should include the reference of Smirnov et al., 2000 at the end of this sentence for triplet based cloud screening.

P. 5533, line 18: Please state whether the 'maximum minus minimum' is of AOD or raw

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signal here.

P. 5533, line 20: Explain why the 1020 nm triplet variation is 3 to 4 times as large at night as compared to day (Table 1).

P. 5533, line 26: This would be a logical place to say something about how much the gain has been increased over the standard sun version of the Cimel, and give the resulting signal/noise ratio analysis.

P. 5534, lines 12-13: Do you mean the sphere accuracy is 5%? A precision of 5% is not very good.

P. 5535, lines 12-14: Please state here how much lower your accuracy is since you used a different ephemeris calculator, and explain why you decided to use a less accurate method (was it computational resource restrictions?).

P. 5535, lines 20-25: It needs to be stated here that the AERONET master Cimel instruments calibrated at stable high mountain observatories (such as Izana and MLO) have accuracy of 0.005 to 0.009 (Eck et al., 1999; the higher errors in the UV) and the PFR similarly calibrated at the same observatories has $\sim$0.005 AOD accuracy (Nyeki et al., 2012; based on 0.5% calibration uncertainty), both for airmass=1. Additionally you should mention that the agreement between collocated AERONET Cimel and PFR measured AOD at Davos has been shown to be excellent, with an average difference of 0.0024 (Nyeki et al., 2012, JGR in press) at 500 nm.

P. 5536, lines 20: Please give a reference for the FLEXTRA model.

P. 5537, lines 9-10: The airmass calculations made by AERONET were significantly updated from the equations used in Holben et al., 1998, in the Version 2 data set, see: http://aeronet.gsfc.nasa.gov/new_web/Documents/version2_table.pdf

P. 5537, lines 19: Can you be more specific about what 'instrument features' you are referring to in this sentence which affect the constant k in Equation 4?

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P. 5539, lines 13-14: This explanation of the Angstrom exponent is not exactly true as the extinction optical depth is used in its calculation, not the scattering optical depth. It is not accurate (and very odd) to say that the Angstrom exponent is a measure of scattering efficiency, therefore I suggest that you remove this sentence.

P. 5540, lines 4-7: Kaufman et al. (1993) do not offer any explanation for spectral differences in alpha, only saying that this occurs due to size distribution departures from the power law. This sentence suggests that you did not really read the Kaufman et al. (1993) paper. Eck et al. (1999) showed that fine mode dominated size distributions resulted in negative values of delta(alpha) both from AOD data and Mie calculations. Also, bimodal size distributions with significant AOD contributions to both modes often show d(alpha) = 0, and additionally Eck et al (1999) showed small negative values for a dust case 10 years before the publication of the Basart et al. (2009) paper.

P. 5542, lines 5-8: This method of transferring reference sunphotometer calibrations to field instruments is an old technique, used for decades and also described in Holben et al., 1998. It is misleading to reference only the Toledano et al. 2011 paper, thereby suggesting that this is a relatively new method. I suggest adding the Holben et al., 1998 reference here.

P. 5545, lines 13-15: "Although the increment in aerosol concentration during nighttime is well captured by CE-1, it can be seen from Fig. 2 a probable calibration problem affecting the 1640 nm and 1020 nm channels between moonrise and moonset." Please be more specific about the calibration problem you mention and why that affects the longest wavelengths most, and also please mark the moonrise and moonset times on the Figures. Also on Figure 2 what causes the daytime jump in AOD (all channels) on Oct 12 at 13 UT?

P. 5545, lines 22-25: Since the PFR has only 3 channels why not use the Photon master Cimel instead? Additionally you should interpolate the CE-1 values to the PFR center wavelength values using the 2nd order polynomial of ln AOD versus ln WL to

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get the most robust comparisons. You did not say how you interpolated the CE data to the PFR wavelengths.

P. 5546, line 1: "...similar to the differences found between simultaneous reference AERONET-PFR measurements, with values up to 0.017." Please note here that Nyeki at al (2012, JGR in press) found better agreement than this between a PFR instrument and an AERONET Cimel measured AOD at Davos.

P. 5547, line 1-5: I suggest using 25% instead of 0.25 to be clearer. Also, the sphere calibration uncertainty is $\sim$5% or less so please put this very large discrepancy (25%) into the context of the sphere radiance uncertainty. There must be larger sources of error than the 5% sphere calibration to result in 25% calibration inconsistencies.

P. 5548, line 9: You say 'mean daytime' values of alpha were used from the AERONET database. I assume you mean averages for the hour closest to sunrise or sunset, as it does not make much sense to compare the mean value for the entire day. Also for the AERONET database, I assume you mean the Izana master, please be clearer about what Cimel you are comparing.

P. 5549, lines 5-7: This is an incomplete discussion, as positive values occur for mixed fine/coarse mode AOD cases only when AOD is low and fine mode radius is small (O'Neill et al., 2001; Eck et al., 1999).

P. 5549, lines 26: You say 'differences below 0.01', however I suggest you give the more precise number here of 0.004 (from Table 7). You should emphasize this excellent result!

Table 5 - 421.1 nm (for PFR) should be 412.1 nm

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