Atmospheric Measurement Techniques Discussions

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

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We would like to thank Referee #1 for all his constructive suggestions and comments. They have been quite useful to improve the paper.

We include as additional information the .pdf file of the final discussion paper with all the corrections highlighted in yellow.

General Comments:

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 impor-

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tant information, as even sun photometers have a limit on measurement related to the product of AOD * airmass, and this limit will be signiïňAcantly 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.

>The only technical difference between the usual Cimel CE-318 and the Lunar CE-318U affects to the four quadrant detector, due to the lower incoming energy from the moon. This means that the silicon detector is the same. In Table 1 we presented a triplet stability checking as a measurement of the temporal stability of the three nocturnal measurements. Following Holben et al. (1998), we can define the precision of the instrument as its ability to accurately reproduce results from multiple measurements under constant conditions using standardized techniques. Holben et al. (1998) proposed, among others, the triplet variability of the DN's taken from Mauna Loa Observatory to evaluate the instrument precision. They also found that the instrument's precision estimated using this technique was in agreement with other analysis, assuming no cloud contamination, stable aerosol concentration, and stable irradiance regime. They found an average variability in triplet lower than 0.4% from 1020 nm to 440 nm channels. These values are similar to those we have presented in Table 1 for daytime and near full moon measurements, being slightly higher for 87% illumination conditions. Since the instrument's precision is a magnitude that accounted for the electronic and other instrumental errors, we consider that this information is more relevant to evaluate the instrument performance than the signal/noise ratio (SNR). However, we have provided the SNR for the new lunar prototype in the paper.

>Regarding the limit related to AOD*air-mass, we have included more information in Table 4 to show that AOD quasi simultaneous differences between lunar and AERONET are not appreciably affected by the moon's illumination. Doing this, from nocturnal AOD comparison on 9-10 August (mean illumination of 84.7%) and 11-12

October (near full moon, 99.8%) we can assume that τ_a accuracy is not affected by a change of about 15% in moon's illumination.

Speciïņ̃Ac Comments:

Question#1: p. 5529, line 24: Please give a reference for the Stellar photometers here, such as Herber et al., 2002.

>Done. Herber et al., 2002 has been included.

Question#2: 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 sufiņ̃Acient signal/noise with this photometer then you should state that.

>The main objective of the CE-318U prototype is to provide high-quality nocturnal AOD and Precipitable water vapor (PWV) measurements as well. To do this, it incorporates two filter channels within the water vapor absorption band with significantly different transmission responses. We have proved that these two filters provide significantly different (PWV) information. Originally this study was performed to be included in the present paper, but due to its final extension we decided to write a second paper concerning PWV which will be soon submitted to AMTD.

>Regarding 380nm and 340 nm channels, they don't provide sufficient signal/noise .

Question#3: 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.

>Done.

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Question#4: P. 5533, line 18: Please state whether the 'maximum minus minimum' is of AOD or raw signal here.

>Done. It is referred to raw signal and it has been included in the text.

Question#5: 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).

>In case of lunar observations, the obvious consequence of the lower magnitude of moon irradiance in 1020 nm, 500 nm and 440 nm is an increase in triplets variation in these channels. In fact, variability in 440 nm channel is also 4 times larger in 87% moon illumination conditions than during daytime and 2 times larger near full moon. However we think that higher values at nighttime could point to problems in the temperature-dependence correction (Holben et al., 1998), which may strongly affect to nocturnal data by means of propagation errors. However, further studies must be developed with the lunar prototype during daytime to assess these differences.

Question#6: 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.

>In page 5533, line 7, we presented the nocturnal measurements performed at "high gain" configuration. However, in order to clarify this point, we have written "maximum gain" instead of "high gain". So, user gains equal to 7 have been used.

>Regarding SNR, it has been explained in the reply to general comments.

Question#7: P. 5534, lines 12-13: Do you mean the sphere accuracy is 5%? A precision of 5% is not very good.

>This precision is not good indeed. There are some references in the literature concerning this issue. Walker et al. (1991) investigated the uncertainty of sphere-source

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radiance measurements, and set an expected uncertainty $\leq$ 5%. This value was lately assumed by many authors (Holben et al., 1998; Eck et al., 2008; Guirado et al., 2012).

>However, the uncertainty associated to this type of calibration techniques is difficult to ascertain since it is a combination of the instrument precision, errors in the calibration procedure, the stability of the sphere's radiance emission, etc. It is also critical the alignment between the sphere and the detector during the sphere's calibration. Thus, the calibration coefficient accuracy can be easily worse than 5%. It is worth mentioning that the sphere calibration transference of the NASA reference sphere is accomplished three times a year by comparison with the NASA travel Master Cimel sun photometer, so we introduce more uncertainties. This sphere calibration followed by AERONET must be improved in the future.

Question#8: 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?).

>In the first stage of our investigations we tried to sign an official agreement between USGS and AEMET, what is required to have access to ROLO modeled data. However and due to bureaucratic issues we couldn't have this agreement signed in reasonable time period for this work. So, Dr. Stone (ROLO-USGS) kindly offered to help us to reproduce the ROLO model using our own computation resources. Thus, we selected a high precision ephemeris calculator adjusted to the JPL DE-404 ephemeris results because it was the most feasible option from a computational point of view. Due to the ephemeris precision, differences of 0.5", 0.33" and 0.36 km are expected in moon's longitude, latitude and distance, respectively. ROLO uses the JPL DE-421. We have not the exact value for DE-404 and DE-421 difference, but both of them belong to the same JPL series DE-4XX, including nutation and libration terms.

>Finally, we want to state that expected 1% ROLO accuracy might also be reduced due to the lack of spectral information. Only spectral irradiance at 32 wavelengths can be

retrieved using the model, obtained from the convolution of the ROLO certain spectral responses. This new comment will be included in the text.

Question#9: 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 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.

>Eck et al. (1999) suggested that the calibration uncertainty in τ_a is better than 0.005 if it is performed in high mountain sites like MLO. Meanwhile they admitted later that the total uncertainty on this term can be between 0.010 and 0.021 for field instruments and between 0.002 and 0.009 for reference instruments. Additionally we have included in the paper PFR-Cimel mean bias differences obtained at Izaña for 2011 by Dr. Wehrli as a personal communication. PFR-Cimel AERONET mean bias differences show values between -0.002 and -0.003 during 2011.

>There a number of possible reasons explain the higher PFR-Cimel AERONET differences we reported in the paper:

1. We have compared in the paper 1 hour moonset vs 1h sunrise and 1h sunset vs 1h moonrise values, and no instantaneous data as it was done in Davos.

2. AERONET data included in this work corresponds to level 1.5, since data from version 2.0 for the dates used in this paper is not yet available.

All this information has been included in the text.

Question#10: P. 5536, lines 20: Please give a reference for the FLEXTRA model.

>We have included the following references:

Stohl, A., G. Wotawa, P. Seibert, and H. Kromp-Kolb (1995): Interpolation errors in wind fields as a function of spatial and temporal resolution and their impact on different types of kinematic trajectories. J. Appl. Meteor. 34, 2149-2165.

Stohl, A., and P. Seibert (1998): Accuracy of trajectories as determined from the conservation of meteorological tracers. Q. J. Roy. Met. Soc. 124, 1465-1484.

Question#11: P. 5537, lines 9-10: The airmass calculations made by AERONET were signiïňÂcantly 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

>We agree with the referee's comment. In fact, in this paper we have used the updated equations corresponding to AERONET version 2, but we had wrongly referenced them in the paper.

Question#12: P. 5537, lines 19: Can you be more speciïňÂc about what "instrument features" you are referring to in this sentence which affect the constant k in Equation 4?

>This sentence has been rewritten in order to be more specific. Particularly, the calibration constants κ_j's depend on the instrument solid angle as well as on the calibration coefficients C_j usually obtained from the integrated sphere.

Question#13: 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 efïňÂciency, therefore I suggest that you remove this sentence.

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>We agree. The sentence has been rewritten.

Question#14: 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 ïňÂne mode dominated size distributions resulted in negative values of delta(alpha) both from AOD data and Mie calculations. Also, bimodal size distributions with signiïňÂcant 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.

>We don't agree Referee #1. Kaufman et al. 1993 states literally (page 2681, first paragraph) "Negatives values (of delta(alpha)) indicate the dominance of a single small particle mode, ..., while positive values indicate the effect of two separate particles modes".

>Eck et al. (1999) presented only two desert dust cases, one from the Gobi desert and the second one from measurements in the Persian Gulf (mixed with anthropogenic pollution), but not for Northern Africa. Meanwhile, Basart et al. (2009) provided a complete aerosol characterization using 39 AERONET stations, 19 of them located on Sahelian and North-Western Africa sites (including Izaña and Santa Cruz stations). So, we think Basart et al. (2009) provide a more solid reference values of spectral differences in alpha for our case analysis. Anyway, Eck et al. (1999) and Basart et al. (2009) have been included.

Question#15: P. 5542, lines 5-8: This method of transferring reference sunphotometer calibrations to ïňÂeld 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

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the Holben et al., 1998 reference here.

>We agree the Referee #1. This calibration procedure was previously briefly described by Holben et al. (1998). However, we propose to keep Toledano et al. (2011) reference, as in this work a more detailed description of this procedure is presented.

Question#16: 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 speciïňĄc 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?

>In the case of 1020 nm, problems are related to temperature correction. However, we do not consider that discrepancies in 1640 nm channel are related to general problems of longer wavelengths. Since this type of discrepancies only occurred in this night, we consider more plausible the existence of uncertainties in astronomical parameters during this night. This comment has been included in the text.

>We consider that the observed "slight" jump in AOD on Oct 12 at midday might be consequence of local atmospheric processes, as a sudden change in local wind direction. Anyway, it supposes a change in AOD of only 0.009 at 440 nm, which is the upper limit of total uncertainty for reference instruments, according to Eck et al. (1999).

Question#17: 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 get the most robust comparisons. You did not say how you interpolated the CE data to the PFR wavelengths.

>We used PFR data as an additional reference to validate CE-318U results. It gives us the possibility to complete the validation procedure using high precision data from an independent reference. AERONET data has been obtained from the AERONET master instrument #244. This instrument operates at Izaňa on a permanent basis. On the contrary the Photons and RIMA (AERONET) masters, are replaced every 3 months.

>We have included a correction in the PFR/Cimel AOD comparison to account for the difference wavelength between 412.1/440 nm channels. We have used the Angstrom equation to derive AOD at 440 nm from AOD measured with the PFR at 412.1 nm. For 500 and 870 nm channels no correction has been applied, since the PRF/Cimel difference in wavelength is lower than the FWHM of these filters, and no significant differences were obtained after the correction.

Question#18: 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.

>Done. See the answer to question#9, related to page 5535, lines 20-25.

Question#19: P. 5547, line 1-5: I suggest using 25% instead of 0.25 to be clearer. Also, the sphere calibration uncertainty is 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.

>Yes, we have used 25% instead 0.25. We mentioned in page 5550, first paragraph, that propagation errors on Method#3 can come not only from the 5% sphere uncertainty, but also from I_0 and omega uncertainties. The high differences found between

omega derived from Li et al. (2008) and the manufacturer's value show important uncertainties involved in the omega determination method.

Question#20: 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.

>We agree. When we said "mean daytime alpha values" they are referred to 1 hour prior to sunset and 1 hour after sunrise. Thus, "mean daytime" will be replaced by "mean daytime alpha sunset and sunrise values". All the alpha values were extracted from the Izaña Master Cimel. This information has been added in the text in page 5535, line 20.

Question#21: P. 5549, lines 5-7: This is an incomplete discussion, as positive values occur for mixed ïňА̨ne/coarse mode AOD cases only when AOD is low and ïňА̨ne mode radius is small (O'Neill et al., 2001; Eck et al., 1999).

>We have rephrased as follows: "For October event, higher $\delta\alpha$ are retrieved, between 0.3 and 0.4. The low τ_a during this period (around 0.07 at 440 nm) and the positive values of $\delta\alpha$ indicate the existence of a bimodal-size distribution (O'neill et al., 2001; and Eck et al., 1999). These $\delta\alpha$ values usually are observed when accumulation and coarse mode aerosols appear well-mixed (Basart et al, 2009), while strong positive values indicate the dominance of fine fraction aerosols (Eck et al. 1999)."

Question#22: 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!

>Done.

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Question#23: Table 5: 421.1 nm (for PFR) should be 412.1 nm.

>Done.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/5/C2438/2012/amtd-5-C2438-2012-
supplement.pdf

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 5527, 2012.