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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.

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**Question#1:** It shows the efforts to advance in moon photometry and make the paper good for publication. However, the novelty of this paper is not clear regarding the previous work of Berkoff et al., (2012) when this instrument and methodology was introduced.

>We don't really understand this statement. First of all, we note that Dr. Berkoff is coauthor of this paper. As we mentioned, in the paper of Berkoff et al. (2011) they tackled the calibration problems in lunar photometry using an unmodified Cimel photometer, the ROLO model and an integrating sphere laboratory procedure. In this



paper we presented three different calibration methodologies that can be used in lunar photometry: 1) Lunar Langley Method, 2) Calibration transference from a master and 3) integrating sphere. Methodologies 2) and 3) have been widely used in sunphotometry, and we present them as a recent recent application to lunar photometry. However the Lunar Langley Method presented here, which is a a modification of the usual Langley-plot method to calibrate sunphotometers, and as far as we know, is completely new. Furthermore, we demonstrate the inherent calibration problems involved in the calibration-sphere procedure in case of lunar photometry, and results have been obtained with a Lunar photometer specially designed for moon observations.

**Question#2:** Many issues regarding the success of this instrumentation are not addressed in this version of the paper.

>We don't understand this general negative comment.

**Question#3:** Particularly, the key question is if the uncertainties in ROLO model are low enough to allow characterization of columnar aerosol properties.

>See Berkoff et al. (2011) and reply to Question#8 to Referee #1.

**Question#4:** Moreover, uncertainties associated with the low signal-to-noise must be still studied.

>See reply to General Comment of Referee #1.

**Question#5:** It is very concern the systematic lack of references in this paper. I guess that the authors are not familiar with the previous work done in this topic. In the introduction section, where the author should include why their study is important, they do not reference any work regarding to the use of AERONET to evaluate satellite products. Moreover, the importance of nighttime measurements of columnar aerosol is not well addressed.

>We have introduced extensively from page 5528 to 5531 the importance of monitoring continuously the total column aerosol information, the problems at nighttime or the different techniques capable to detect aerosols at night (i. e., lidar, stellar photometry or common-aperture photometers). We don't consider relevant for our purposes to include any reference to the use of AERONET data for satellite products evaluation. Why? In the conclusions section (page 5551) we present the validation of satellite information as a straightforward application of lunar-photometry.

**Question#6:** But the more discorageous is the lack of references regarding columnar aerosol properties at nighttime. Previous works in moon photometry has been done (e.g. Esposito et al., 1998, Journal of Aerosol Science; Herber et al., 2002, Journal of Geophysical Research) and the authors should clarify the development they propose. Indeed they extend what Berkoff et al., (2012) did regarding this topic.

>According to Referee #1's question#1 we have include a reference to the important study using stellar photometers developed by Herber et al. (2002).

**Question#7:** Moreover, the authors claim that the star-photometry is not an appropriate technique. This is not true at all. In fact many instruments are being deployed worldwide and some research papers were done (e.g. Leiterer et al., 1995 (Contributions to Atmospheric Physics), Ansmann et al., 1992 (Journal of Geophysical Research), Perez-Ramirez et al., 2008 (Atmospheric Environment), Baibakov et al., 2009 (AIP Conference Proceedings)). Actually, Herber et al., 2002 presented a large database of star photometry measurements at an Artic place, and Perez-Ramirez et al., 2012 (Atmospheric Chemistry and Physics) presented four year of day-to-night columnar aerosol measurements. These studies should be mentioned to show the efforts of the scientiïňĄc community in this topic.

>We invite Mr. Perez to read again page 5529 (lines 24- 27) we literally say: "Stellar photometers are proven to be more effective in determining $\tau_a$ at nighttime than lunar



photometers, but due to the low-income energy from the stars, the complexity of the large-aperture instrumentation limits their use and their implementation in standardized regional or global networks.".

>As far as we know there is not a standardized stellar instrument, with common quality assurance and data evaluation procedures, normal requirements of an instrument network. On the contrary Cimel photometer is the AERONET instrument. So, we hope you will understand that a Cimel lunar photometer, quite similar to a Cimel sunphotometer, might be easily integrated in an operational network as AERONET.

**Question#8:** As I commented above, the main point for the success of the moon photometer model proposed in this work faces with the utility of the ROLO model. As the authors said, this model is based on ground-based observations over multiple years of the moon. The ROLO model ïňĄts quite well with 1% residuals as reported by Kieffer and Stone (2005). However, to my knowledge these ïňĄts made use of an atmospheric model for gases absorption and aerosol extinction. ROLO was born to support calibrations of space-based sensors, and thus a sensitivity study of the uncertainties for all moon phases should be done. Typical errors on columnar aerosol optical depths are below 0.02. Can the authors provide an error analyses and give the uncertainties associated with this new moon-photometer design?

>We don't understand this question about ROLO. ROLO provides the exo-atmospheric irradiance, so we don't understand why is necessary to include aerosol/gases extinction in this model. Furthermore, it has been extensively validated by USGS and similar uncertainties were retrieved for all moon phases.

**Question#9:** Moreover, the ability of ROLO for moon photometers should be checked, either by Langley technique at high mountain site or by comparing with correlative star-photometry measurements. Can the authors provide any comparison?

>ROLO exo-atmospheric irradiance is a necessary parameter to calibrate a lunar pho-

tometer. This parameter is changing continuously during a whole night, from the moonrise to the moonset. If we don't use ROLO, the only way to calibrate is performing a Lunar Langley each night, which furthermore is not valid for the whole night since moon's illumination changes during the night (see Herber et al., 2002). However, when using ROLO, it is possible to obtain a good $I_0$ from the moonrise/set Langley and applying this value to the entire night. So, it has no sense to compare with moon Langley.

**Question#10:** Concerning the calibration methods, many questions come up: For Method 1, the authors do not say anything about the relative air-mass interval used. AERONET limits the relative air-mass interval between 2 and 7. As the author claim, the moon photometer present larger noise and this could greatly affect the measurements at larger air-masses. For star-photometry, Perez-Ramirez et al., 2011 (Journal of Aerosol Science) used the Astronomical Langley method that allows calibrations for a shorter airmass interval. Moreover, the authors should clarify how the measurements of this new moon-photometer design are affected by atmospheric turbulence. Regarding Astronomical Langley calibrations, Perez-Ramirez et al., 2011 shows that atmospheric turbulence effects are minimize. Thus, I encourage the authors to study this method as well. Furthermore, the Lunar-Langley and the Astronomical Langley should be studied on different nights and the agreement in the calibration constants between different nights should be presented. Calibration constants must be the same every night, and thus will allow the study of different uncertainties regarding to their moon-photometer.

>In page 5539, line 6 we clearly stated that air masses between 2 and 5 were used in the calibration using the Lunar Langley Method. On the other hand we inform to Mr. Perez that the Astronomical Langley method was implemented as the first stage of our work, and no satisfactory results were obtained. So this method was discarded.

**Question#11:** On the other, Method 2 the error induced is twice sensitivity of the



instrument. This should be also noted and taken into account in the discussions of the calibration procedures.

>We do not understand why Mr. Perez states that in Method #2 the error is twice the instrument's sensitivity. In Table 7 we can see $\tau_a$ differences below 0.003. We do not understand why Mr. Perez talks about sensitivity. Does he refer to instrument's precision?

**Question#12:** The work of Smirnov et al., (2000) (Remote Sensing of Environment) uses the triplet of measurement to detect cloud-affected data. The large values of the triplets obtained in the present work make investigate alternative methods for cloud screening. In this sense, for star photometry Perez-Ramirez et al., (2012) (Atmospheric Measurement Techniques) developed an alternative method based on moving averages. The authors should mention these two methods. Moreover, if possible they should study the applicability of every method or proposed an alternative one.

>In this work the triplet's variability presented in Table 1 was used to check the instrument stability, following Holben et al. (1998). We consider the method proposed by Smirnov et al. (2000) should be a suitable technique to screen clouds in lunar photometry, although further investigations must be developed. For the purposes of our work, automatic cloud screening was not necessary since very accurate cloud monitoring was made with observers (by the instrument) and with ancillary data from lidar. Of course, automatic cloud screening for moon observations using modified Smirnov's algorithm will be considered in the future.

**Question#13:** They also should give a description of the data analysis procedure. Particularly, due to the low signal-to-noise they should study what is the effect of the atmospheric turbulence on the different ïňĄlters.

>We don't understand this question. Does Mr Perez mean the scintillation caused by atmospheric turbulence? If so, this "noise" and others are jointly analyzed and

assessed using triplets' variability.

**Question#14:** Generally, it is difïňĄcult to read the paper. For example, I would suggest giving ïňĄrst the results of the inter-comparisons among the different calibration techniques and later the day-to-night evolutions. Personally, I feel lost with sections 6.1.2 and 6.2.2. Are they independent study cases?

We don't agree with Mr. Perez. The results section is organized as follows:

6.1: Results using Lunar Langley Method with two independent case studies (6.1.1 and 6.1.2).
6.2: Results using Method #2.
6.3: Results using Method #3. In this section we included an inter-comparison between Methods #1 and #3 in order to show the important differences found.

We consider appropriate and clear the section's organization.

We don't understand why Mr. Perez talks about section 6.2.2, because it doesn't exist.

**Question#15:** Pag. 5540, ln 1-7: The statements related to the spectral difference of Angstrom are not correct. Bassart et al., (2009) obtained from pure dust measurements negative values of but it does not imply that all negative values corresponds to dust particles (see initial work of Gobbi et al., 2007 at Atmospheric Chemistry and Physics).

$>$Well, in our case there is no doubt at all we have mineral dust in the corresponding episodes. A part of the Flextra trajectories (plus ECMWF, and Hysplit trajectories), Lunar photometer measurements and MPL lidar, we have a Global Atmospheric Watch (GAW) program of in-situ aerosols, which includes the determination of optical properties and chemical composition of aerosols. This ancillary data (not shown in the paper for the sake of brevity) allows us to confirm that observed aerosol is Saharan desert



dust when we classify as such.

**Question#16:** Pag 5537: Should the Earth-Sun distance must be include in the Beer-Bouger-Lambert law (equation 2) for moon-photometry? For sun-photometry this changeable distance is taken into account.

$>$In lunar photometry this factor is considered in the ROLO model. In this case, the moon phase, the Earth-Moon distance, the Sun-Moon distance and many other astronomic parameters are taken into account in the $I_0$ modeled (please, see section 3.3 and section 4.1, where we stated "$V_0$ represents the extraterrestrial voltage, which includes all temporal variations (lunar phase as well as Earth-Moon and Moon-Sun distances)".

**Question#17:** Pag. 5537, ln 9-10: "For the air-mass and the spectral optical depth calculation we have followed the speciïňĄcations given by Holben et al. (1998)". This must be clariïňĄed. What do you refer spectral optical depth? Is it about gases absorption? Molecules scattering?

$>$We refer to AOD calculated at different wavelengths.

**Question#18:** Pag. 5538, ln 13-20 and Pag 5530. ln 1-6: The authors give a description of the calibration methodology. This is later included in section 5.1. Please avoid repetitions and clarify.

$>$We don't understand what Mr. Perez means. In page 5530 (lines 1 to 6) there is not any description about the calibration methodology. It is the introduction section.

**Question#19:** Pag. 5541, ln 10-13: "In this paper we present the calibration strategy for the lunar CE-318U instrument, which can be approached by three different methods, depending on available calibration facilities." This statement must be included in the introduction section where the objectives of the study must be clear.

>In the introduction (see page 5531, line 18) we stated: "Three methods have been used to assess the instrument calibration: the Lunar-Langley Method, the calibration transference from a master and the calibration using an integrating sphere". So, we consider again that this information is included and the objectives are perfectly clear.

**Question#20:** Figure 4: It is expected that shorter wavelengths have larger aerosol optical depths. However, in the figure it is not always observed. How do you explain this? The data plotted present low aerosol optical depths. Can be this associated with the uncertainties in aerosol optical depth retrievals? I insist again on the needed of making uncertainties analysis.

>We have detected an error in the legend of the graphic. Yellow values correspond to 440 nm and blue ones to 550 nm. This mistake has been corrected.

**Question#21:** Pag. 5548: Plotting the day-to-night evolution of the Angstrom parameter should improve the paper a lot. The table with mean values does not show the potential of the moon-photometry.

>These results have been presented in a table. In Tables 11 and 12 we did not present mean values, but the corresponding averages during moonrise, moonset, sunrise and sunset, in order to perform a comparison between daytime and nighttime data. Since we can't compare simultaneous AOD data we have considered this is the most suitable and clear way to show a comparison between day and night data.

**Question#22:** Figures 1 and 2 shows correlative moon-photometer and range corrected signals gtom MPLNET network. Although these lidar systems need extinction-to-backscatter ratio, Why not comparing extinction at Izana obtained by lidar to those obtained by the moonphotometer?

>This is a very controversial matter because, as you probably know, it is not possible,



strictly speaking, to determine the extinction using monochromatic lidar. This problem is out of the scope of this work and it could be a challenge for the lidar community

**Question#23:** I also recommend re-writing the conclusions section after making all the changes because there statements that are not true at all. For example, you claim that instrument precision is 0.1-0.2 and it is true only for the sun-photometry version of this instrument.

>We suppose that Mr. Perez is referring to 0.01-0.02. We have stated that the instrument precision is within this limit as a conclusion of the present work. See results from Table 4 in those cases of stable $\tau_a$ conditions.

**Question#24:** Another example that is not clear is when you state that efforts should be made to transfer sun-Langley calibration to moon observations (if the gains of instrument are changed, what you mean?).

It is clear from this statement that we are referring to a future work. It consists, basically in using a unique instrument (Cimel photometer) to perform both daytime and nighttime observations with automatic lunar/sun mode shift. Therefore it would be desirable to calibrate the future instrument using sun langleys . Of course, daytime and nocturnal measurements must be taken with different gains. This instrument will represent a huge advantage over those instruments designed to measure only at night.

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