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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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This paper presents recent advances in moon-photometry to characterize columnar aerosol properties at nighttime, which is a topic of ongoing interest. The paper shows the potential of modifying commercial sun-photometry and to make moon-photometry measurements. 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. Many issues regarding the success of this instrumentation are not addressed in this version of the paper. Particularly, the key question is if the uncertainties in ROLO model are low enough to allow characterization of columnar aerosol properties.

Moreover, uncertainties associated with the low signal-to-noise must be still studied.

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. 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. 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 scientific community in this topic.

As referee 1 said, the manuscript is sensitive to many major comments. But I would like to add a few more to make the paper stronger:

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 fits quite well with 1% residuals as reported by Kieffer and Stone (2005). However, to my knowledge these fits 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? 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?

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 air-mass 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. 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.

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. 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 filters.

Minor Comments:

Generally, it is difficult to read the paper. For example, I would suggest giving first 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? If so, both sections should be include after section 6.3 including also the study of the Angström parameters.

Pag. 5540, ln 1-7: The statements related to the spectral difference of Angström are not correct. Bassart et al., (2009) obtained from pure dust measurements negative values of $\delta\alpha$ 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).

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.

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

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.

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.

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.

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

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 moon-photometer?

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 $\pm$0.1-0.2 and it is true only for the sun-photometry version of this instrument. 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?). Moreover, you make a very good description of the importance of nighttime columnar aerosol measurements that should be better included in the introduction section.

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