Atmos. Meas. Tech. Discuss., 5, C3282–C3289, 2012
www.atmos-meas-tech-discuss.net/5/C3282/2012/

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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Received and published: 17 December 2012

We would like to thank Referee #2 for his constructive and useful suggestions. Many of them have been incorporated in the final version of the manuscript. Please, find below our answers to his general and specific comments.

General Comments:

The paper shows the new technology and the different possibilities to calibrate the Moon measurements. The presentation of the background and technical modification is excellent and very good understandable (Section 1-3, 5). The presentation of the Calibration procedure and Discussion should be improved; sometime a clear line is

missing due to a mixture of calibration procedure and data interpretation (Section 4 and 6). The paper is well written and the original manuscript is now improved within the actual review phase. However, I am wondering that few comments from Reviewer 2 (D. Perez Ramirez) were not better answers/explained, especially, question 11, 13, 14.

> Comments relative to sections 4 and 6 will be answered in specific comments. In relation to the explanation to Mr. Pérez's comments:

1) Question #11 was no clearly formulated. So we asked for a clarification to Mr. Pérez which was not answered.

2) We consider that question #13 was clearly answered since we stated that atmospheric scintillation caused by atmospheric turbulence can be certainly removed by means of the triplets stability checking.

3) Regarding the signal to noise ratio of the instrument, this information was included after referee #1 comments.

4) Finally, question #14 is a personal feeling of Mr. Pérez. We replied that certainly 6.1.1 and 6.1.2 were two independent case studies but we considered the organization of this section in three separate subsections clear enough, since we want to show the results from three different calibration methods.

Specific Comments:

1) The detailed discussion on the limitation of Moon measurements is still poor. It is well-known, that during the half Moon phase the measured Moon light is only 10% in compare the full Moon phase and I am doubtful that you can get proper results in case of Moon phase less than 50%. In all figures it would be night to give the information on the present Moon phase. It would be great if in the summary exist also an recommendation in which case we can use the Moon technology. The advantage and disadvantage could be summarized here too.

> It is well known that Moon's illumination undergoes a drastic decrease from full to phases less than 50%. Anyway, in section 1 we have included the disadvantages of lunar photometry related to this problem, as well as the need to have high sensitivity instruments to capture the low incoming energy from the Moon. At this moment, the instrument is able to perform lunar measurements for fraction of illumination (FI) $\geq$ 50%. The instrument CE-318U presented in this study is just a prototype, and the results that we have shown correspond to preliminary measurement. We are working to improve the performance of the CE-318U instrument in order to extend the measurements to lower FI cases. We appreciate the Referee's comment about FI of each AOD episode, and thus this information has been included in section 6.1.

> Specifically, in section 1, it has been included the following: "Another problem presented in lunar photometry is the reduced observational frequency compared to sunphotometry. In case of having a sufficiently precise instrument capable of measuring under moon fraction of illumination $\geq$40% conditions, we could obtain a nocturnal observational percentage $\approx$ 60% per month. However, and due to the phase lag in lunar and solar cycles, only a fraction of each night will be useful in practice for moon measurements, except for full moon events, when lunar observations can cover the entire night. "

2) One of the objectives of the paper is the presentation of new calibration procedure for Moon measurements and the author present three different methods, but the advantage and disadvantage of the different measurements is shown only by few episodes – is the accuracy of the derived AOD. I am missing information on the quality of the calibration procedure and the final demonstration of it, similar to Berkoff et al paper, see Figure 5 (Langley analysis of the data from Figure 4, with linear regression fits – solid lines - to independently determine optical depths).

> As we mentioned in the previous question, the results we report in the present paper correspond to the preliminary measurements performed with a prototype started in July 2011 and which doesn't work yet in operational mode. Of course, further analysis

can be accomplished with the operational/commercial instrument and its performance will be tested in many other conditions and intercomparison experiences. The linear regression fit of the Langley plot developed on 9 February has been included as suggested by Referee#2.

3) Some of the information under 2 (Site information) are not really relevant for the actual discussion and this part could be shorter. The authors mentioned in 3.4 (Ancillary information for data validation) to use FLEXTRA, but later on it is not really use.

> In section 2 we simply describe the site of measurements. We consider relevant to introduce the main features of this Observatory because its location in altitude, above the temperature inversion layer, is crucial for the CE-318U absolute calibration. We have also considered remarkable the fact that the observatory runs a WMO GAW programme, and it is an absolute calibration site of AERONET and RIMA networks, because it means that independent data used in the instrument validation meet the highest quality assurance international requirements.

> Concerning the FLEXTRA information, it was included to illustrate the Saharan origin of the air dust laden masses (high AOD conditions) satisfying Mr. Pérez's question#15. This tool was briefly described in just 5 lines (section 3.4). Moreover, many readers might wonder about the source of the dust intrusion and confirm, with independent sources, that high AOD events corresponded certainly to Sahara/Sahel air mass intrusions.

3.bis) The number of tables should be reduced and combined. It is for the reader not completely clear, which additional information occurred by separation of the long list of tables. The main focus should be here to present the results from calibration Method 1 and Moon system CE-1.

> This comment has been answered more specifically in the following questions.

4) Section 6.1 (Method#1). In case of the focus of the paper lie in the calibration proce-

dure, then you should start the discussion in section 6.1.2 with the discussion on low and stable aerosol burden, which are occurred in February 2012. The quality of the Langley procedure is strongly depended on the quality of the atmospheric conditions. The measurements in August (Section 6.1.1) are can demonstrate here only the variation during day and night and the conclusion should be, that this time period is not really useful for calibration. I think this are the concern from D. Perez Ramirez.

> The instrument calibration by means of Method #1 was performed using data from 8-9 February and then this calibration was applied to other time periods, like August or October test cases, in order to check the stability of this calibration (see section 5.1). That means that Figures 1 and 2, and the corresponding tables, are useful not only to see the smooth variation during day and night but to show the stability and validity of the calibration over time, and under quite different atmospheric conditions with very low and very high values of AOD.

5) Section 6.2 (Method#2): You have written: "comparison for the day after the calibration (10 February) shows a good concordance between the values obtained from the master and the secondary instrument, with averaged differences up to 0.002." – I cannot agree it, this is only true for few wavelengths, see my comments point 10. Please specify it better in the text.

> Answered in question #10.

6) Section 6.3 (Method#3): It seems that Method#3 is less accurate in compare to Mehtod#1 due to uncertainly by the determination of solid angle . I cannot read, what do you proposed to increase the accuracy of this parameter. You mentioned only that the quality of Method#3 will improved. Please specify it in more detail.

> We had included in section 7 (summary and conclusions) the following sentence: "Our study highlights the importance of accounting for a high-performance integrating sphere and an accurate determination of Ω to assure a good calibration following Method#3".

7) Section 6.4 (Ångström's exponent) could be removed completely. The topic of the paper is to present a new method of nocturnal aerosol measurements with a new lunar photometer. The derivation of Ångström parameter and their interpretation is a separate story and more attractive when you discuss separately the differences between aerosol burden during day and night. The aim of the day measurements here is only to show the coincidence between day and night measurements, based on the selected calibration procedure.

> We don't agree with this Referee#2 comment. As he says, the topic of the paper is to present a new method of nocturnal aerosol measurements with a new lunar photometer, and to demonstrate its good performance we show results of both AOD and Ångström's exponent. We consider that the latter introduces relevant information for the aerosol characterization during day and night. Since α is affected by the instrument calibration, the temporal evolution (diurnal) of this atmospheric parameter is useful to detect calibration errors on nocturnal data. Thus, the combined information AOD-α as well as its temporal evolution provide consistency to this comparative study.

8) Table 5 could be removed. Here is the only important information, that CIMEL and PFR during night measurements are comparable, that can be mentioned in the text. The comparability between Sun and Moon measurements is shown in different. Table 6 is also not really substantial due to doubling information with Table 7, where the absolute difference between two different Moon photometers is clearly present. I don't see a real reason to present here the results in more detail of the second Moon photometer, called CE-2. I think it is enough to present with table 7 the comparability of both systems during night measurements and concentrate of the output of CE-1.

> We don't agree with Referee #2. We consider the information of Table 5 is relevant and is not redundant with information contained in other tables. In Table 4 an AOD comparison between daytime AERONET and CE-1 is shown. Although other studies in the literature have concluded that PFR and Cimel-AERONET measurements are comparable, here we are comparing measurements from a new Cimel prototype.

Furthermore, the nocturnal AOD validation performed using PFR data constitutes an opportunity to validate CE-318U data against an independent reference instrument, showing that AOD differences found between CE-318U and PFR are of the same order as those found between Cimel-AERONET and PFR in quasi-simultaneous measurements. Concerning Table 6, we agree with the Referee#2 and this Table has been removed.

9) Table 9 – 10 and Table 11 and 12 could be combined in each one table. I don't see a reason, why you have it separated. But you have to consider giving here detailed information on the differences between day and night and on the quality of the calibration. The number of tables is here not really attractive. Sometime it is better do present the difference between different methods by plots.

> Done.

10) Table 7 and Figure 4 give the same information, whereby the Table 7 is much better, whereby it seems for channel 440 nm, 550 nm and 1020 nm the absolute differences are at least partly bigger. Based on Figure 4, I had concluded, that not for all wavelengths exist an excellent coincidence.

> We agree to remove Table 7. Notice that maximum AOD discrepancies ≈ 0.003 were found between both prototypes for 440, 500 and 1020 nm spectral bands (shown in Figure 4).

11) Are Figure 1,2,3 based on the calibration values derived from Method#1 ? To see the still existing differences (uncertainty in determination of calibration values in Method#3) it would be nice to have a similar plot like Figure 4 to show the coincidence or discrepancy in the derived AOD. Exist here a wavelength dependence or is the coincidence similar for each wavelengths.

> Yes, Figures 1, 2 and 3 are referred to calibration Method #1 (performed on 9 February moonset). Notice that Method#1 and Method #3 comparison is presented in Table

9, and it is certainly a wavelength dependence (maximum differences in 1640 nm and 500 nm channels, and minimum at 1020 nm and 675 nm channels). We therefore consider that this information is clearly presented in Table 9, and the inclusion of a new graphic would be redundant.

12) Section 7 (Summary and Conclusion): I mentioned under point 1, that for the reader finally some statements on constrains, advantage and disadvantage of the Moon measurements would be helpful. It is true, that your recommendation is finally to use Method#3 instead of using Method#1, after improvement of the solid angle.

> Answered in question #1.

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