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Interactive comment on "Calibration of an all-sky camera for obtaining sky radiance at three wavelengths" *by* R. Román et al.

Anonymous Referee #3

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The paper "Calibration of an all-sky camera for obtaining sky radiance at three wavelengths" presented by R. Roman et al. General comments This work explains the methodology followed to calibrate an all sky-camera. This is based on modeled radiances by Libradtran-UVSPEC model package. To run this model standard profiles and inputs indicated by Anton et al (2011b) and the aerosol properties provided by a sun-photometer CE-318 following the AERONET processing algorithms are used. Hence, in this way the camera provides sky radiances to be used to study different atmospheric components, as it is said by the authors. To test the resulted calibrated radiances a set of almucantar and principal plane measurements were taken from Cimel-AERONET and the resulted differences were lower than 15% in general with the exception of those regions where the radiances increase very quickly, i.e. for zero zenith angles. Also, previous to the calibration of the camera system, Cimel almucantar and PP data

were compared with Libradtran model obtaining similar results to the above comparison. According to the opinion of the authors, both results demonstrate the reliability of Libradtran model to be used for this task. Therefore, the points of discussion are: a) Is the method proposed correct? b) Are the errors associated to the calibrated radiances acceptable for a reliable application?

Most of the paper is presenting the application of the methodology for calibration of the all sky camera and in general is well described and documented, well-structured which makes the ideas easily understandable for the reader. However, in the results section there is plenty of platitudes instead of demonstrations of a useful application, which would properly show that the level of accuracy of the calibrated radiances gives a reasonable error for retrieving the atmospheric properties. The calibrated radiances suffer a high uncertainty which limits its application.

The conclusions of this work are far from being realistic. The authors indicate that the mean differences between modeled and measured sky radiance (by AERONET sun-photometer CE-318) are lower than 15% to state, afterwards, that the UVSPEC model can be used to estimate cloudless sky radiances if some inputs are taken into account (derived from AERONET procedure using the data of the same sun-photometer). In my opinion, an error of 15% in radiance accuracy is quite high, especially as the authors suggest as a future line of application to use this calibration strategy to retrieve the aerosol properties. Besides this, the short spectral range (the three wavelengths) of the camera is not suitable for an adequate retrieval of aerosol properties (size distribution, SSA, etc.. mentioned by the authors) because there is not enough sensitivity according to Mie theory to retrieve information of the whole range of particle sizes of atmospheric columnar aerosols. However, other properties as AOD, or the cloud aerosol optical depth seem to be reliable, but this is not shown in the paper.

Obviously, taking into account the optical and geometrical characteristics of the camera system, perhaps a more reliable method with a lower calibration error than the one obtained by the authors is not possible. These errors are intrinsic to the method or to the optical system itself. In this case the system will be limited in to the applications that support this level of errors. As mentioned, it is not adequate for aerosol properties retrieval. For example, an error of radiances about 5% has been shown in the AERONET inversion algorithm as a reasonable limit to have sound associated errors in the retrieval aerosol parameters.

The end of the work envisages that the authors do not have a clear idea on how to use, or the utility of these calibrated radiance data of doubtful quality. However, previous papers published by the authors showed their experience about this issue. On the other hand, the paper of Olmo et al., 2008 (Applied Optics) appears similar to the one described here but using other methodology for calibration. Why have the authors not explained in more details the reasons of this new proposed method in relation to the one used previously. What are the drawbacks and advantages of this new proposed method?

Coming back to the two open questions, in my opinion using a radiative transfer model, as a unique tool for calibrating an optical system, as a radiometer, a spectroradiometer or an all sky-imager, as that proposed in this paper, is a very risky alternative which depends on the goodness of the input parameters and the confidence of the model used and hence not at all recommended (although the optical system will be used for atmospheric or environmental measurements).

The option suggested by reviewer 1, using the radiance from almucantar and PP measurements (obtained by Cimel-AERONET) to do a direct calibration does not seem easy to implement as every single pixel of the all-sky camera needs its own calibration (all the directions in the sky). The method will require measurements along a complete day with perfect or near perfect atmospheric conditions, making this a hard task with high associated errors. Using this method and taking into account the broadband filters of the camera and the variability of the spectrum in this covered region it is reasonable to think that the calibration matrix would show a dependence on the SZA. The non observed dependence may be that the 5 months (or days selected in this period) are

not sufficient to show this effect. This point needs to be analysed more exhaustively, as extending to whole year and selecting a day or two for every month of the year.

The most reliable alternative is (as it has already been done) to calibrate with a sphere in the laboratory under controlled conditions. However, according to the referenced papers which follow this method, the comparison with other reliable radiance measurements or modelled data yield the same differences. Thus, these errors seem to be intrinsic to the system and also we come back to the open question: what is the level of uncertainty allowed in the system for a given application? The authors consider that the differences lower than 15% between LibRadtran and Cimel-AERONET radiance data (or between all sky camera and experimental Cimel calibrated radiances) showed the goodness and confidence of the model and also the method.

In my opinion the methodology applied and the obtained results are correct but the high associated errors are inherent to the method because of the characteristic of this instrument.

In addition, the comparison between radiance involving azimuth and zenith angles (for almucantar and principal plane respectively) would be much better described in terms of the scattering angle. The use of the scattering angle is much more suitable than the use of the azimuth angle for the comparison between radiances in almucantar (it would be recommended to change zenith angle with scattering angle in the study of the principal plane, even though zenith angle and scattering angle are the same in that geometry).

There is also some relevant vagueness. 1. The introduction contains a lot of unnecessary sentences which are not directly related to the subject of the paper (and hence the related references). Certainly the solar radiation is the responsible of sky radiance but most of the references on solar radiation characteristics are superfluous. The whole introduction must be shortened.

2. The paragraph describing the inversion strategy in the introduction is mostly wrong

(page 1875, lines 15-25). For example, the sentence "different authors (e.g. Nakajima et al. 1996; Dubovik and King, 2000) proposed an inversion algorithm for the retrieval of aerosol optical properties using sky radiance measurements in the almucantar" is false. Both algorithms do not depend on the geometry of the radiance measurements, even though they use almucantar measurements in those papers. This issue becomes worse in the following sentence "Olmo et al. (2008b) developed an inversion method to obtain the columnar aerosol size distribution, the single scattering albedo (SSA), the phase function (PF) and the asymmetry parameter from sky radiance measurement in the principal plane" while in fact Olmo et al. (2008b) uses SKYRAD.pack (Nakajima's code) to derive aerosol properties from principal plane measurements. Although the paper presents also some novelties as the treatment of non-spherical particles, it can not be considered as a new inversion method.

3. In the section 3.2, the authors propose a comparison of 50 almucantar cloudless measurements with modeled radiances in order to study the reliability of the UVSPEC. This comparison shows errors higher than 20% (for 677nm) for short azimuth angles. These high differences are justified in the text by the presence of clouds in the measurements, which contradicts the conditions settled in the beginning of the section. On the other hand, in the last paragraph of the section the authors conclude that the sky radiance estimated by the UVSPEC model, used afterwards as reference for the calibration of the all-sky camera, is in agreement with experimental measurements. This affirmation is too generous since as it can be directly observed looking at the figure 3, ARE is only lower than 8% for 441nm channel between 10 and 50° and for 677nm for angles larger than 60°.

Other specific comments - As the section 2.3 describes the input values to model the radiance the name "Data" is quite ambiguous and should be replaced. - Page 1874 in the abstract, line 13, "The comparison between the output signal.........," I think that use the word "comparison" in this sentence is not adequate, rephrase this sentence - A few spelling errors have been found along the text, although they are not so much

relevant. -Page 1882, line 6. To use implemented in this sentence is wrong. Rephrase this sentence.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1873, 2012.