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Interactive Comment

Interactive comment on "Calibrated sky imager for aerosol optical properties determination" *by* A. Cazorla et al.

Anonymous Referee #2

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Title: Calibrated sky imager for aerosol optical properties determination Author(s): A. Cazorla, J. E. Shields, M. E. Karr, A. Burden, F. J. Olmo, and L. Alados-Arboledas

The main goal of this work is to evaluate the AOD and Angstrom coefficients using a calibrated sky imager (WSI). The authors argue that this technique can supplement the existing aerosol database (mainly the photometric network like AERONET). This is an actual topic because it is necessary to decrease the uncertainties associated to the role played by the particle matter suspended in the atmosphere on the energy balance in the Earth-Atmosphere system, both in a global and regional scale.

It is an appropriated topic to ACP where the authors present promising results in the





sense that a sky imager can estimate the AOD and the algorithm could be applied in the field. All sections are well organized and the data have high quality and are enough to support the interpretations and conclusions.

I think that the paper is acceptable with corrections, as I comment below.

1. Why is necessary to use a methodology based in neural network-based models?. Have the author tested the possibility to use a standard (physical model) based, for example, in the inversion of the radiative transfer equation (only to compute AOD) for each pixel of the image?. The WSI instrument, like authors describe (p. 19995, I1-5) can be considered a scanning radiometer. In the analysis of the Angstrom exponent the authors check the advantage of using a neural network-based models versus the standard model used in AERONET. Similar information regarding to AOD can be useful to evaluate the necessity to use this strategy.

2. One of the motivations of this work is that this methodology can supplement the existing aerosol database, mainly resolving the temporal resolution problem associated to AERONET (p. 19991-19992). In the point 3.3.2: Input selection algorithm and 3.3.3: Results, the authors explain that only one scattering angle is selected by the greedy algorithm to estimate the AOD (this scattering angle depends on the channel). I think that these connected points must be clarified, both the temporal problem associated to AERONET and the improvements obtained with the proposed methodology (employing only one scattering angle). I think that would be very interesting to give an estimation of the number of additional AOD values evaluated with the new methodology, at least during the temporal period used in the work. Have the authors performed any test using a more complete set of scattering angles?

3. One of the bases of this work is the information displayed in figure 4, then to estimate the sensitivity of the radiance, it could be clearer to represent a ratio of the radiance levels regarding a radiance levels compute for an arbitrary alfa and beta value. At the light of that information and taking into account the scattering angle selected by the

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greedy algorithm, do the authors consider that is good the sensitivity showed by the radiance with the alfa values?.

4. In the introduction the semi-direct effect of aerosols must be comment, along with the direct and indirect effects.

5. In the point 2.3: The Whole Sky Imager, a more complete description of the calibration procedure could be given, both regarding to the geometrical characterization of each pixel and the radiance levels. How much is the error associated to the radiance levels evaluated by the WSI?.

6. In point 3, p. 19995, I. 22, can the authors give an quantitative estimation of the shadow system?.

7. The Point 3.3.3: Results should appears in a new point or change the title of point3: Methodology and results.

8. In the conclusions, p. 20005, I. 1-2, the authors comment that it is not necessary a new training and validation processes for WSI installed in different locations. This affirmation must be explained more in depth.

9. In figure 1, the latitude and longitude values must appear for clarity.

10. Perhaps the figure 5 could be removed because this information is given in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19989, 2008.

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