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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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GENERAL COMMENTS

This paper presents an original methodology for estimating Aerosol Optical Depths (AOD) from sky images taken by a Whole Sky Imager (WSI). Since the method may be applied to the three wavelengths used by the WSI, an estimation of the Ängström exponent alfa (related with the wavelength dependency of AOD) can also be estimated. The estimation of AOD is an interesting topic by itself, since aerosols play an important role in climatic and meteorological processes. The method presented by the authors is based on the use of a neural network approach.

The paper is in general well written and correctly structured. The title reflects the content of the paper; the abstract is correct, but it could be slightly improved (see

my specific comments below). Observational data come from a single site, but the database seems long enough to allow for correct and general conclusions.

I do have two general criticisms for this paper, however. The first one regards the justification of the work. While the description of aerosols in the atmosphere is an important subject, I do not understand the advantages of using a WSI for this description. First, there are no so many WSI deployed around the World. Second, as far as I know, the WSI is a quite expensive instrument that is built only on demand. Third, the instrument that the authors use as a reference (CIMEL) is broadly used worldwide, provides additional information (ozone and water column) besides the aerosol data, and has become a standard for aerosol characterization. The authors point out several issues related with CIMEL measurements (temporal resolution, unobscured sun required,...) but it is not clear if this issues are overcome by the WSI aerosol-derived data.

My second appreciation refers to the methodology, and more specifically, to the use of a neural network approach to derive AOD. Here, I would like to comment several aspects. First, it is not clear if this methodology is better than other possible alternatives. In particular, since only two input values (SZA and a single radiance) are used to estimate AOD, I am not convinced of the need of such a complex method (neural networks), instead of a simpler and physically clearer method such as a look-up-table. Second, additional effort should be put in writing Section 3 (Methodology) and in particular to Section 3.2. Since the main interest of this paper is the new methodology, this is particularly important. I would say that this section should be readable by any reader interested in aerosol characterization despite of his/her knowledge about neural networks (see some specific comments below). Third, the authors indicate in their conclusions that the methodology could be applied to different WSI without new training of the neural network. More discussion should be held on this very important point. Have the authors performed any test in this sense? Could the methodology be applied to other non-calibrated sky cameras, such as the TSI, WSC, or others?

SPECIFIC COMMENTS

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Abstract

A reference to the site(s) where the method is applied and validated should be added.

Introduction

First paragraph: the sentence "changing how clouds reflect and absorb the Earth's energy budget" is not very clear, since the energy budget cannot be reflected and absorbed, can it? In any case, aerosols also affect the longwave (infrared) radiation fluxes, and this should be mentioned here.

Last paragraph and several other places in the manuscript. The coefficients alpha and beta of the Ängström formula are cited as "coefficients", "exponents", "parameters". Are all these terms correct?

Section 2.3

p. 19994, l. 17, The sentence "Open hole is used for...conditions" is not clear. In addition, I suggest writing this sentence before the sentence that regards the wavelengths used.

p. 19995, l. 5-6, Some details should be given regarding the radiance and geometric calibration of the WSI. Or, this sentence could be rewritten and attached to the previous paragraph.

Section 3. Methodology

Fourth paragraph, Why there are only 1047 images available for training and validation? What is the limitation: the available CIMEL data, the number of cloudless cases? Or, did the authors set a restriction on SZA or time in the day?

Section 3.2

As I have already noted, this section should be improved to make it clearer to non-experts. For example, the "topology" of RBF is explained twice (first and third para-

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graphs in page 19997) but this is not useful to make RBF more understandable. Or, the first sentence in l. 25 is not quite clear either.

Section 3.3

The first paragraph (p. 19998, l. 22-27) could be better placed in Section 3.1. The second paragraph (p. 19999, l. 1-2) is unnecessary, since this has already been said in Section 3.1.

Sections 3.3.2 and 3.3.3

It is a little bit surprising that after explaining the greedy algorithm as an iterative method for selecting the inputs for the neural network, its result turns to be the selection of only one radiance as a single input. In addition, most of the text in the first paragraphs of section 3.3.3 (p. 20000) is well summarized in Table 1, so the text could be easily shortened.

Section 3.4.

The second and the third paragraphs could be combined in a single paragraph, by setting sequentially the presentation of Fig. 8a and 8c and the corresponding comments.

Section 4. Conclusions

Most of this section is in fact a summary of the results shown in previous sections. I would suggest shortening or removing some paragraphs (first, third) and put more emphasis on the ability of this method to substitute or complement the CIMEL estimations of AOD. As I have already commented in my general comments, some additional discussion on the generalization of the method would be also appreciated.

TECHNICAL CORRECTIONS

Abstract

l. 9, units (nm) are missing after "440-870"

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l. 15, "...in 50 per cent of cases"

Introduction

p. 19991, l. 26, It is not clear what Cazorla et al. (2008) reference are the authors citing here. Note that there are two Cazorla et al. (2008) references in the Reference list, one cited as (2008) and the other as (2008b).

l. 28-29. The Atmospheric Physics Group is cited before is explained (in Section 2.1).

Section 2.1

p. 19992, l. 17, The affiliation of the Atmospheric Optics Group has already been given in the Introduction.

Section 2.3

p. 19995, l. 3, It is unnecessary to repeat the "185 000 points".

Section 3.3.2

The first paragraph is again a repetition of something that is already known.

Section 3.3.3

p. 20002, l. 11-12. The sentence about Fig. 7 is unnecessary.

References

I am not sure if the IPCC 2007 report is correctly referenced.

Figures

Figure 2 is probably unnecessary, since the authors give many references regarding the WSI instrument, where other pictures can be found.

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