Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-148-RC2, 2016

Atmospheric Measurement Techniques Discussions

Interactive comment on "Absolute calibration of the colour index and O_4 absorption derived from Multi-AXis (MAX-) DOAS measurements and their application to a standardised cloud classification algorithm" *by* Thomas Wagner et al.

Anonymous Referee #2

Received and published: 13 July 2016

The paper gives two main results: 1) new calibration methods for absolute calibration of colour index (CI) and O4 absorption and 2) location-independent threshold values for an earlier developed cloud-classification scheme. The calibration methods developed in this paper together with the new threshold values and the adapted colour index are an important step towards a uniform cloud classification scheme for DOAS measurements of scattered sunlight, as the authors state correctly. The paper is well-written and the method is well-demonstrated. Therefore, I recommend to have this paper accepted for publication in AMT, after considering the minor comments below.

General comments:

- The authors make extensive use of radiative transfer calculations. The method or model used for that should be described.

- Section 4 is difficult to read, since it assumes the reader to be very familiar with the original cloud-classification scheme, and in addition is a mixture of actual new methods, comparison to the old scheme, discussion, and recommendations for future research. I recommend to restructure and shorten this section considerably, and add a separate section "discussion". It should be made clearer what is new, and what contributes to a more uniformily applicable classification, and what not. Also a table summarising all differences between the old and the new classification scheme, would be illuminating. In addition, I recommend a figure similar to Fig. 14 from Wagner et al 2014, where the affected steps are marked.

Specific comments:

- The introduction contains some text that does not belong there. The reader expects only the context, history, and objectives here:

a) The text between page 2, line 29 and page 3, line 5 contains important references to previous methods or suggestions for absolute calibration, but are mixed with statements on what is or is not being done in this study. I recommend to remove these statements, and rephrase so that it is a discussion on previously published methods for the absolute calibration of the important parameters.

b) The text between page 3 lines 6 and 14 provides information on important modifications to the cloud-classification scheme. I recommend to make a separate section for this.

c) Page 3, lines 15 and 16 belongs in a conclusion, or can be rephrased as an objective of this study.

- Page 4, line 7 refers to the "original CI". Some explanation should be added here,

either the reference to the Wagner et al, 2014, paper or the old wavelengths used.

- Page 4, line 28-29, "Instead, the low CI values are probably caused by 3D effects of broken clouds": Please explain what is meant by 3D-effects and how would they lead to lower CI values.

- Page 4, line 29-31, "However, even the lowest measured CI are close to the simulated minimum": this is not completely true, there is one clear outlier. Please also quantify the percentage of measurements with CI lower than the simulated minimum.

- Page 5, line 5, "Their maxima directly represent the ... proportionality constant": This is not necessarily true for locations where there are mostly clear skies. It might be necessary to remove clear-sky data from this analysis.

- Section 3: This section determines the O4 AMF for one single FRS. How would this method change if different reference spectra are used throughout the data set? Also only one VCD is used for the Cabauw data set, and it is unclear if this is also the case for the Wuxi data set (a complete year). What is the expected error of using one single VCD?

- Page 7, line 6-7 "A very similar value for AMF_{FRS} (1.75) was also found by Wagner et al. (2014).": Please explain what is different between the methods used in Wagner et al 2014 and the method used in this study.

- Page 7, line 11-12: "... under similar conditions." Please describe what makes them similar.

- Page 8, line 5-6: "simultaeous AERONET measurements": please quantify, or include in figure caption.

- In Figure 10, there is a distinct different behaviour between the black line and the other lines for small solar zenith angles. Is there just a mistake in the legend? Otherwise this needs more explanation.

- There is no need for an appendix, the table and Figures in the appendix are important enough to be included in the main paper.

- Section 4.2: A polynomial for TSI thresholds for zenith view is provided in Tables 1 and A1, although in the text it is explained that it can not be universally used. Therefore, I recommend to leave it out of the Tables. The authors should give a clear discussion on how this influences their objective to come to a uniform cloud-classification scheme. An alternative is to leave out Section 4.2 entirely. In this respect also the statement in the conclusion (page 13, lines 18-20) needs some refinement.

- Section 4.3, including 4.3.1: I can not see what is new here. The threshold value for spread in CI is the same as in Wagner et al (2014). This section further only contains discussion and recommendation for future research, and is too long. See my general comment on Section 4 above.

- Section 4.1.-4.3, Figure 10-13: The comparison between the old and new algorithm is done on fraction of measurements with a certain classification. However, if the old and the new algorithm result in the same fraction, that does not necessarily mean that the classification for individual measurements is the same. What is interesting is whether individual measurements get the same classification. Please quantify and discuss how many classifications were different between the two algorithms, and in what way.

- Section 5.2: I recommend to change the title, since this is a discussion on the effect of surface albedo, which can be high also at moderate or low latitudes. Please adapt the text in this section accordingly.

- Section 5.2: From Figs 6 and 17 it is clear that the SZA for which the spread in the O4 AMF is almost independent of AOD is different for different surface albedo.It would be interesting to see a figure of this "optimal" SZA as a function of surface albedo. As already suggested by the authors, it seems straightforward to make the method applicable for regions with high surface albedo as well. The only difference is in the selection of data points from which the O4 AMF for the FRS is determined.

For low surface albedo the SZAs between 30 and 50 degree are used, for high surface albedo the SZAs between, e.g., 70 and 90 degree, following from the above-mentioned dependency. For each location the data can be filtered for periods with similar surface albedo (snow or no snow).

Tecnical comments:

page 3, line 7: change "were" to "are"

page 3, line 10: change "420" to "440"

page 5, line 8: change "cloud sky" to "suspected broken cloud" (or clarify)

page 6, line19: change "at he" to "at the"

page 6, line 27: change "applied to the remove" to "applied to remove"

page 6, line 27: remove " from the O4 AMF"

page 7, line 5: change "poor statistics" to "low number of observations"

page 8, line 4: change "CI for AOD" to "CI for AOD=0.75 and 0.85 (red lines)"

Table 1: The coefficients in the table should have less significant digits, and should be written in scientific notation. In addition the normalisation of S should be given in the caption (1 degree?). I recommend to normalise S with 90 degrees, so that all coefficients have similar orders of magnitude, and to have a maximum of 5 or 6 significant digits. The first polynomial would read, e.g., $y = 8.666s^5 - 16.364s^4 + 6.604s^3 + 0.464s^2 + 0.180s + 0.540$, with s between 0 and 1.

page 8 line 30: different spelling of "Angstrom"

page 9 line 10: change "versions of or algorithm" to "versions of the algorithm"

page 11 line 19: "compared for and elevation angle" to "compared for an elevation angle"

page 10, line 23: threshold of old algorithm is 0.74 (table 2 in Wagner et al, 2014) instead of 0.8

Figure 1: make the distinction between aerosols and clouds visible in the legend, e.g., AOD=0.2, and COD=2.

Figure 1, caption: change "Heyey" to "Henyey", and "Asymmetry" to "asymmetry".

Figures 1, 2, and A2 caption: put a symbol for radiance (I) in the formula for CI (e.g. I_{320nm}/I_{340nm} instead of 320nm / 340nm)

Figure 6, caption, line 5: change "to th eleft" to "to the left". Quantify which aerosol layer height and surface albedo are used for the simulations.

Figure 8, caption: change "represents" to "represent", and "Cabuw" to "Cabauw".

Figure 9, caption, line 5: change "The the simulations" to "The simulations". Explain that the red line represents the threshold value distinguishing clear-sky from cloudy.

Figure 14, caption: specify wavelength of radiance

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