

Interactive comment on “Aerosol Optical Depth measurements at 340 nm with a Brewer spectrophotometer and comparison with Cimel observations at Uccle, Belgium” by V. De Bock et al.

Anonymous Referee #3

Received and published: 18 August 2010

General comment:

This paper concerns a technique enabling retrieval of the Aerosol Optical Depth (AOD) at 340 nm from Brewer spectrophotometer measurements obtained on cloudless conditions. The measurements used are conducted in Uccle, Belgium. The purpose of this work is to propose a methodology to determine an AOD that can be directly compared to Cimel sunphotometer's AOD at 340 nm, allowing validation of Brewer's AOD. Then time variations of the derived Brewer's AOD are studied over the period September

2006-December 2009.

While the methodology proposed is valuable for validation objective, and the derived set of AOD interesting, it should be noticed that the retrieved Brewer's AOD are not strictly at 340 nm since they result from convolution with the Cimel filter. Thus, the authors should explain why they chose to carry out the temporal study with these data rather than with "true" 340nm-AOD data. It seems to me that there is a default in the present study since AERONET level 1.5 data are used instead of level 2.0. The authors state that the method is new, maybe it is new for their Brewer measurements, but similar technique has been used by other teams for other instruments for Cimel comparison purposes. In addition they state that the difference with their previous work comes from "the use of sun scans instead of direct sun measurements", this statement should be clarified because I believe that the scans are still coming from Brewer direct sun (DS) measurements (at several wavelengths), thus the difference comes only from the wavelength number and their location.

Throughout the text the authors should replace the word "intensity" by "irradiance".

Specific comments:

- Title: specify "Cimel sunphotometer" since Cimel company develops several types of instruments.

- Abstract:

Line 4: replace "Cimel" by "Cimel sunphotometer".

Line 4: add just after the parenthesis "performed in Uccle, Belgium".

Line 5: clarify if the scans are DS or not.

Lines 9: replace "new method" by "improved method".

Line 12: replace "very good linear agreement" by "very good agreement".

AMTD

3, C1179–C1185, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Line 13: replace “intercept” by “intercept of the regression line”.

Line 16: replace “studies” by “studies at other sites”.

- Introduction:

p. 2745, lines 1-8: The semi-direct effect is forgotten, it should be mentioned.

p. 2745, lines 3-4: Cite the references either in chronological or alphabetic order.

This comment is valid throughout the text.

p. 2747, line 2: Say what Hatzianastassiou et al. have observed.

p. 2747, line 3: As told in the general comment the method is not completely new. A similar technique has been used in other works (i.e. Brogniez et al., ACP, 2008).

p. 2747, lines 4-5: replace “direct sun ozone measurements from the Brewer instrument” by “direct sun measurements from the Brewer instrument dedicated to ozone retrieval”.

p. 2747, line 5: “sun scan measurements”, clarify if the scans are DS or not.

- Instruments and location

p. 2748, line 11: I suggest to write “For comparison with Cimel AOD products, the obtained spectral data. . .”

p. 2748, line 13: “. . . allow retrieving the AOD at 340 nm”, the AOD is not exactly at 340 nm since there is a convolution with the Cimel filter. Give some precision.

- Method

p. 2749, line 5: replace “direct sun measurements” by “direct sun measurements at five specific wavelengths, 320.1 nm being the largest”.

p. 2749, line 6: see previous comment on DS sun scans. Give the wavelength step. How long takes a scan ?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



p. 2749, line 7: specify the FWHM of the Cimel filter.

p. 2749, lines 8-9: I wonder if the authors are not making confusion between the need of performing Brewer measurements at 340 nm to avoid extrapolation of the Cimel AOD at 340 nm towards the Brewer wavelength (as they made in their previous work at 320 nm), and the need of convoluting the measurements with the Cimel filter to get an AOD similar to the Cimel product (as they didn't make in their previous work since their was a single wavelength 320 nm). Please clarify the sentence.

p. 2749, lines 17-19: explain what is a "relative optical airmass" (for μ and m) and justify the values 22 and 5 km.

p. 2749, line 22: the authors must justify why they take a constant value for the station pressure P (1000hPa).

p. 2749, line 22: delta is not the "aerosol scattering optical thickness" but the "aerosol extinction optical thickness".

p. 2749, lines 24-25: replace "intensity of the direct beam" by "the direct beam".

p. 2750, line 1: replace "scattering by aerosol" by "extinction by aerosol".

p. 2750, lines 3-5: this sentence is very confusing, I don't understand how the dependence (of what? of S ?) on the "effective ozone T " can be eliminated since in Eq 1 there is only the air temperature T . Please clarify.

p. 2750, line 6: replace "using ozone profiles" by "using ozone and temperature profiles".

p. 2750, Eq 2-5 and 8, and line 19 (AOD value $-A$): there is an inconsistency in the signs.

p. 2751, line 7: replace "range of zenith angles" by "range of solar zenith angles"

p. 2751, line 11: see previous comments on DS sun scans.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p. 2751, lines 21-22: it is stated that the intensities are obtained from the sun scans that are convoluted with the Cimel filter, in these conditions the previous equations that are spectral should be adapted. Indeed, the ozone absorption coefficient should also be convoluted, as well as the Rayleigh contribution. Therefore, sections 3.1 and 3.2 must be reconsidered and rewritten completely.

p. 2751, lines 25-26: explain why the total ozone is not taken from the DS measurements and which constant AOD value is taken for the modeling.

p. 2752, line 6: detail what “best results” means.

p. 2752, lines 16-17: “the selected cloudless days are used to determine THE calibration coefficient”. It does not appear clearly to me if a calibration coefficient is determined for each cloudless day and then the mean of these coefficients is used to calculate all the AOD (it will be stated later). Please clarify here.

p. 2752, lines 16 and 18: since the selected days are said “cloudless” (line 16), there should be no “influence of clouds” (line 18). I suggest writing in line 18: “To avoid the influence of clouds that might remain”.

p. 2753, line 14: the authors use AERONET level 1.5 data, could they explain why they do not use level 2.0 data that are better clarified from clouds. I suggest that this version be used.

p. 2753, line 17: which Angstrom exponent is used ? (Precise also that it is an AERONET product).

- Results and discussion

p. 2753, line 22: explain why the last date is May 2009 instead of December 2009.

p. 2753, lines 24-25: here, I understand that a mean calibration factor is computed, it should be stated before).

p. 2754, line 4: “The remaining 274 ...”: clarify the fact that there remain 274 cases

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

(sorry, I don't see).

p. 2754, lines 24-28: give explanations of the better agreement with the proposed method.

p. 2755, line 2: now the last date is December 2009. Why is it no more May?

p. 2755, line 8: replace “individual values” by “individual values in the whole archive”

p. 2757, line 8: “during winter”, November is not in winter.

p. 2757, line 10: replace “for the summer and autumn months” by “for the late spring - summer and early autumn months”

p. 2757, lines 22-23: replace “mixing height” by “mixing layer height”.

p. 2758, lines 3-16: this section is confusing. It should be reorganized to explain the t test before giving the results. Few sentence need also to be rewritten (for ex line 6: it seems that it is the t value that is not statistically significant!). What does “at 150 degrees of freedom” means? State what \bar{x} and \bar{y} represent. Define μ , σ . “5.1% of all the values. . .” is it not too few? By the way I wonder if such description is useful and should not be removed.

- Summary and conclusions

In this section the authors must account for some remarks made previously.

p. 2759, line 20: replace “AOD measurements may exist that are perturbed by clouds” by “AOD measurements perturbed by clouds may exist”.

p. 2767, Table 2: Lille is located in France

p. 2768, Table 3: precise “340 nm-AOD” and the covered period in the legend.

In several figures (on axes or in legend) replace the word “intensity” by “irradiance”.

p. 2769, Fig 1: could you explain the lack of values, or null values, in the red curve?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



p. 2771, Fig 3: specify AOD in the captions of both axes. Replace “The red curve represents all the data” by “The red curve represents the regression line of all the data”, and “The blue curve shows the data without. . .” by “The blue curve shows the regression line of the data without. . .”.

p. 2773, Fig 5: specify AOD in the captions of both axes and in the legend.

p. 2776, Fig 8: “This explains the low SD”, I am not convinced by this explanation, I wonder if 3 values very close could not give such a low SD. Clarify.

p. 2778, Fig 10: add “layer” in “mixing height” in y-axis and legend.

Technical corrections:

- Typo error p. 2749, line 17: “wavelength”

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 2743, 2010.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)