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Atmospheric Measurement Techniques Discussions

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.

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We would like to thank the reviewer for his/her useful comments.

ANSWER TO GENERAL COMMENTS:

1)Sun scans were performed with the Brewer and convoluted afterwards with the Cimel sunphotometer band pass filter, to simulate the Cimel observations and thus to make sure that the Brewer measurements at '340 nm' are completely comparable with the

Cimel sunphotometer measurements at '340nm'. In this way we compare physically exactly the same quantity.

2)For the revised manuscript, we used AERONET level 2.0 data instead of level 1.5 data, as was the case in the first manuscript.

3)We state that this method is new, since it is new to use Brewer sun scan measurements for the retrieval of AOD. Up to now, standard direct sun measurements, which are performed at 5 specific wavelengths, of the Brewer were used. The novelty of the method described in the article, is the use of sun scans between 335 and 345nm. We want to emphasize that sun scan measurements are NOT the same as direct sun measurements! The former are performed on direct sun light, but the measurement method of sun scans is completely different from direct sun measurements, where 5 slits are opened successively to measure at 5 specific wavelengths. In the case of sun scans, only one slit is used and the scan is performed at wavelengths from 335 until 345nm, with steps of 0.5nm.

ANSWER TO SPECIFIC COMMENTS:

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

This is done.

- Abstract:

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

This is done.

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

This is done.

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

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The scans are made on direct sun light, but there is an essential difference between standard DS measurements for ozone at 5 specific wavelengths and sun scans between 335 and 345 nm, convoluted to 340nm. The difference between DS measurements and sun scans is discussed in the paper (in section 2).

Lines 9: replace “new method” by “improved method”.

We decided to replace “new method” by “adapted method”.

Line 12: replace “very good linear agreement” by ” very good agreement”.

This is done.

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

This is done.

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

This is done.

- Introduction:

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

An extra sentence, explaining the semi-direct effect of aerosols, will be added to the revised manuscript: “Aerosols also influence the Earth’s radiation budget in a direct, semi-direct and indirect manner. The semi-direct effect describes the warming of the boundary layer, through the absorption of radiation by aerosols, which can lead to evaporation of clouds. This will allow more solar radiation to reach the surface (Ramanathan et al. 2001, Cazorla et al. 2009).

p. 2745, lines 3-4: Cite the references either in chronological or alphabetic order. This comment is valid throughout the text.

This was taken into account.

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

Added to the revised manuscript: “Hatzianastassiou et al. (2009) studied the spatial distribution of AOD over the Mediterranean basin and found significant geographical variation of AOD within the study area (e.g. large AOD values over North Africa and smaller values in relatively remote oceanic areas such as Crete island).”

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).

The method is new in the fact that it uses sun scan measurements instead of direct sun ozone measurements of the Brewer! See answer on general comments.

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”.

This is done.

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

See previous response.

- Instruments and location

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

This is done.

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.

To make the retrieved Brewer AODs comparable to the Cimel AOD values, we performed scans with the Brewer instrument, to simulate the Cimel observations. (See section 2).

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- Method

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

This is done.

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

(See also previous response.)

For the sun scan measurements, the measuring step is 0.5nm and one scan has a total duration of 21 seconds. This information will be added to section 2 (p2747-2748) in a revised manuscript.

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

The FWHM of the Cimel filter is 4.756nm. Added on page 2748.

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.

We will add the following sentence on page 2749, line 10: after ’since it no longer necessary... to the Brewer wavelength: “Moreover, due to the convolution with the Cimel band pass filter, we compare physically exactly the same quantity.”

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

A relative optical air mass is the path length relative to that at the zenith at sea level.

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The mean height where the maximum of ozone in the atmosphere resides is 22km and 5km is used as the standard height for Rayleigh scattering in the Brewer operating system.

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

In the standard Brewer operating procedure, the mean station pressure is used. For Uccle, this is 1000hPa at sea level.

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

This is changed in the revised manuscript.

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

This is done.

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

This is done.

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.

We want to eliminate the dependence of the AOD retrieval on the effective ozone temperature. In Eq. (1), the term $\alpha(\lambda,T)$ is the ozone absorption coefficient in which T is the temperature of the entire ozone profile. Not all ozone is located at the surface and we have to take this into account by calculating the effective ozone temperature using the ozone soundings.

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

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This is done.

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

The sign is changed.

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

This is done.

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

See previous response to this comment.

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.

For the revised manuscript, we changed the calculation of the Rayleigh contribution so that this term is also convoluted with the Cimel filter. When comparing the new AOD to the old ones (calculated without the convoluted Rayleigh term), we noticed very small and negligible changes in the AOD. The maximum difference in the AOD was 0.00023. Since the contribution of the ozone absorption term to the computation of the AOD is very small compared to the contribution of the Rayleigh term, we conclude that the convolution of the ozone absorption coefficient would cause even smaller changes in the AOD and decided not to convolute this term. In addition, for the computation of the ozone absorption term, we use the Bass and Paur ozone absorption coefficients, which we only have for wavelengths from 245 to 340nm. For the convolution, we would need them until 345nm. To summarize, the AOD that are analyzed in the new manuscript are calculated using convoluted Rayleigh contribution, but without a convoluted ozone absorption coefficient.

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The following part is added to section 3.1 to explain this to the readers: “Since the sun scans are convoluted with the Cimel band pass filter, we should also convolute the Rayleigh scattering and the ozone absorption coefficients. The convolution was done for the Rayleigh scattering term and this caused only a small change in the retrieved AOD values with a maximum difference of 0.00023 with respect to AOD values calculated without a convoluted Rayleigh term. Since the contribution of the ozone absorption term to the computation of the AOD values is very low, compared to the Rayleigh contribution, we did not convolute the ozone absorption coefficient. This would lead to negligible changes in the AOD.”

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.

Since the absolute value of the curve of the expected intensities doesn’t play a role, we decided to use climatological values for the total ozone. We used a constant AOD of 0.7777. This is a standard value for polluted air. (Added on p 2751)

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

Several threshold values were tested and the resulting cloudless days were compared with the observed and thus real cloudless days. In the case of a 20% threshold value, no days were selected as cloudless when in reality they were cloudy. When we used a higher threshold value(e.g. 35%), the algorithm selected days as cloudless when in fact they were not cloudless at all. With a lower threshold, the selection was too strict and some days that ought to be selected as cloudless (based on the observations) were not selected. We will add on page 2752, line 6: “In this context, a day is considered cloudless if the maximum deviation of the individual ratios (of a day) from the mean ratio is smaller than 20% (different threshold values were tested, but the 20% value generated the best results, meaning that the selected cloudless days were in agreement with the observed cloudless days).”

p. 2752, lines 16-17: “the selected cloudless days are used to determine THE cali-

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bration 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.

Changes made in the revised manuscript: (p2752, line 16) "After applying these criteria, the calibration coefficients are calculated for each selected calibration quality clear day. From this calibration coefficient, the mean value is calculated which will be used as mean calibration coefficient of the instrument. With this mean calibration coefficient, the AOD can now be calculated for each individual observation. "

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".

This is done.

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.

For the revised manuscript, we made the comparison with AERONET level 2.0 data. We want to mention however, that they are only available until May 2009.

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

Following a comment made by referee 2, the Cimel AODs at 320nm are now calculated using a second order fit of ln(AOD) to ln(λ) (using the AERONET data from 500, 440, 380 and 340 nm). The previous sentence with the Angstrom exponent is thus removed.

- Results and discussion

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

This was done since level 1.5 AERONET data after May 2009 were not available at the

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time the comparison between brewer and Cimel was done. Since recalculations had to be done to account for the different referee comments, the period of the calculated AODs is extended in the revised manuscript to Sep 2006 until Sep 2010. For the comparison with the Cimel 2.0 values, data after May 2009 are not yet available through the AERONET website.

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

See previous answer.

p. 2754, line 4: "The remaining 274 . . .": clarify the fact that there remain 274 cases (sorry, I don't see).

From the 2951 individual Brewer AODS, only 251 values had a quasi-simultaneous Cimel measurement. These are the remaining cases. (The numbers changed since the period of analysis is changed in the revised manuscript.) New sentence in the manuscript to clarify this: (p2754, line 3) " Only quasi-simultaneous measurements from Brewer and Cimel (level 2.0 data from AERONET) were used for comparison. From the 2951 individual Brewer AODs, only 251 had a quasi-simultaneous Cimel measurement."

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

It is to be expected that the AOD values at 340 nm, calculated from the sun scan measurements are better than the AOD at 320nm from the DS measurements. At 320nm, even the smallest error in the ozone values will cause an error in the AOD, whereas at 340nm, the effect of ozone is negligible. The results of the comparisons of AOD at respectively 320 and 340 nm with the Cimel AOD show that the ones at 340nm agree better with the Cimel values.

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

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In the new manuscript, the period of analysis is extended to September 2010.

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

This is done.

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

New sentence: “It can be seen that during late autumn, winter and early spring (November - April) the dry AODs are clearly higher than the wet values.”

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

This is done.

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

This is done

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 xi and yi represent. Define mu, sigma. “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.

For the sake of completeness, we decided not to remove this section. We will however reorganize it and add the requested information, based on your comments above, so that it becomes clearer to the readers.

- Summary and conclusions

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

This section was changed so that all the previous remarks were taken into account.

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

This is done.

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

This is corrected.

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”.

This is done.

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

The null values are an artifact of the plotting. The ratios that appear to be zero in this figure, are not zero in reality. The ratios are just really small compared to the other values, which is the result of the influence of clouds for those points. Clouds cause the observed irradiances to be very low in comparison to the expected irradiance. We will explain this in the caption of the figure: “Ratio of the observed and expected irradiance for a cloudless (05/08/2007; in blue) and for a cloudy (20/07/2008; in red) day at Uccle. The points that appear as null values are points for which the ratio is very small. This can be explained by the influence of clouds, which causes the observed irradiance to be very low.”

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. . .”.

This is done.

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

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This is done.

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.

We will change this as follows: “The mean monthly value is based on only 3 individual AOD values (which were accidentally very close). This explains the low standard deviation for this month.”

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

This is done.

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

This is corrected.

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