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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Intercomparison of aerosol optical depth from Brewer ozone spectrophotometers and CIMEL sunphotometers measurements" *by* A. Cheymol et al.

Anonymous Referee #2

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This manuscript of Cheymol et al. describes a validation study of AOD from Brewer instruments, compared against CIMEL measurements. Aerosol optical properties at UV are less well understood, if compared to visible wavelengths. On the other hand, there is a wide network of Brewer instruments with possibilities that have not been yet fully explored. Therefore, this manuscript has importance in its attempt to assess the quality of these AOD data. The paper is written in a very compact manner and there are many occasions where reader is likely interested in further details or analysis. These will be discussed below. I think the manuscript can be published in ACP, if these comments are thoroughly considered.

General comments

From Brewer measurements 320nm is always taken, while from CIMEL it is either AOD at 340nm or at 440nm. The difference by the wavelengths is not discussed very much in detail, although it is speculated here and there as a candidate for the differences seen in the comparisons. For instance, in Figure2 it may seem that the wavelength difference of 320nm and 340nm should not be mentioned at all. However, the Angstrom exponent (AE) is available from AERONET, therefore it would be of interest how the comparison was changed, if CIMEL data were converted from 340nm to 320nm. By typical AE, one would assume AOD at 320nm to be 5-10% higher if compared to 340nm. This type of analysis is easily done. There does not have to be necessarily figures of this type of comparisons, however, they would likely bring additional information about the comparisons that could be mentioned. Particularly those comparisons of 320nm against 440nm are difficult, without even rough analysis whether the order of magnitude in difference could be explained by the wavelength difference.

It is mentioned that the measurements with a maximum time difference of three minutes are included. It would be of interest to know about the performance with the daily mean values, in addition to these instantaneous ones. The effect by occasional problems with the cloud screening (likely included now particularly in the figure 6a), should be then reduced, and one could likely make better conclusions considering systematic differences.

As mentioned above, instead of simple statements on the differences in the wavelength, this could have been explored in more detail. Also, those statements were not always clear, for instance in section 12003, lines 18-22, and particularly the meaning of the sentence starting in line 21 was not clear. Could you please clarify.

Figure 7 is a problematic one, due to suspiciously large values in Brewer data. If the assumption is that these are caused by a "technical problem", then why to include them at all? These data points make it very difficult to draw any conclusions, for instance

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about the influence by the locations, as is attempted now.

Specific comments

In the section 12002, line 16, "This bias of ...". The meaning of this sentence is not very clear. Could you please clarify.

It seems that the numbering of the figures around 6a and onwards is not consistent with the text in the manuscript, where also Figure 9 is mentioned. Please correct these.

In the section 12005, line 8, "In 2006, the CIMEL measures ..". If one looks at the figure 6b, it seems that there are no measurements in 2006, but last ones are in the end of 2005. Is this correct?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11997, 2008.