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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Some considerations about Ångström exponent distributions" *by* F. Wagner and A. M. Silva

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The reviewer expressed doubts on the applicability of this simulation study to real experimental situations, especially about the validity of some hypothesis and the conclusion is that we need to apply the theoretical results measurements of several AERONET stations.

Reply:

* Our simulation study is a theoretical study, only. Therefore we didn't intend to apply the results to experimental data. We think a validation of the results of our simulation study is a different work and should not be included in the current study. Therefore we only pointed out in our manuscript under which circumstances the results could potentially become important (very low AOD, relatively high calibration error). Besides this, it

will be very difficult to demonstrate the effect of different relative errors on an Angstrom exponent distribution created of many measurements of a large database of AOD. The reason is that the effect can be best shown in case of the (exclusively) presence of a constant aerosol population at a certain station. In that case, the Angstrom exponent (AE) would be fixed, too. But in reality the Angstrom exponent, even for the same aerosol type, varies with changes in the atmospheric relative humidity due to particle growth. Additional larger changes of the AE at a certain station occur when the aerosol type changes. This does not mean that the effect of different relative errors on the AE distribution is absent; it rather means that it is difficult to demonstrate the effect for several stations. Therefore we will not try to extend our simulation with a study about the application to experimental data.

* To do a very clear simulation study we had to restrict ourselfs to a small number of assumptions which we chose carefully in a way that these assumptions are connected with the "real world". Our study is based on 3 assumptions:

(a) AOD's are lognormal distributed. As already written in our manuscript several authors reported such a distrubition. It might not be true for all places all over the world, but at least for some places, which justifies this assumption.

(b) The error of the calibration constant which translates then in an error of the AOD is independent for each wavelength. This independency of the errors is what we believe common knowledge and it is based on that fact that usually the calibration constants are determined via the Langley-plot method which is based on the concept of monochromatic light and hence on the independency of each wavelength.

(c) The errors of the AOD's follow a normal distribution. This might be the most questionable assumption in our simulation study but there a strong arguments to accept a normal distribution. Here we would like to point out that as far as we know no one ever determined the real error distribution for AOD's. We've already tried to tackle this topic in our manuscript, but obviously it was not clear enough. Therefore we would like to clarify the problem of the errors and to justify the use of a normal error distribution for the case of large aot-databases which is the fundamental assumption of our work:

1) It is well known that the major source of uncertainty in sunphotometry is due to the error of the calibration constant for each wavelengths.

2) This calibration error propagates to the error in aot and the latter one is proportional to 1/m (m...airmass) under the assumption that the true value of the calibration constant does not change. This means also that the error works as a bias. The corresponding aot is either systematically too high or too low. But the assumption of an unchanged calibration constant does not hold over longer time scales. It might be true for short time scales, e.g. a single day.

3) It is also well known that the calibration constant changes with time. Therefore sun photometers should be re-calibrated regularly. As a compromise between effort and benefit a calibration once a year is recommended (see e.g. VDI) and well-maintained networks (e.g. AERONET) try to follow this recommendation.

4) With increasing number of calibrations it becomes more and more probably that the calibration constants are sometimes too high and sometimes too low with respect to the "true" value. This is intrinsically included in large database which is a necessary condition for the application of our simulation study.

5) The value of a calibration constant is usually linear interpolated between 2 subsequent calibrations. Under the assumption that the calibration constant changes smoothly in time this procedure should reduce the actual error of the calibration constant for times when no real calibration is available. Furthermore in all cases where the differences between the used calibration constant and "true" value of the calibration constant changes sign for 2 subsequent calibrations the interpolation procedure guarantees that there will be a measurement interval with errors close to zero. Hence the occurrence of small errors has higher probability than the occurrence of large errors. Furthermore, as already written in our manuscript, Campanelli (2007) showed

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that the calibration constant varies over short time scales and compared with the value of the calibration constant due to the interpolation procedure the error changes sign over short time scales.

6) The comments 1-5 support an error distribution function for aot which contains positive and negative values and with a higher frequency of occurrence of small errors than of large errors. This general error distribution is further modified by various but small factors: - the solar constant varies not only over the well-known 11 year cycle but also over small time scales. See e.g. Wehrli et al. - errors caused by the instrument (e.g. instrumental noise, temperature variations, pointing error etc.) - Rayleigh correction depending on the presence of high and low pressure systems (see comment by Wehrli) which becomes increasingly important with decreasing aot.

7) Therefore we think that any error distribution which is more or less symmetric and has a maximum around an error value of zero may be used for the simulations. We chose a normal distribution which has an additionally important feature: it can explain why some Angstrom exponent histograms as reported in the literature can be approximated by a normal distribution.

We think the comments above answer all questions/comments 1-5 by reviewer # 1.

Further responses to other points highlighted by this reviewer:

Question 6: why the second wavelength was not chosen randomly in the simulation study?

Reply: The wavelengths to retrieve the AOD and further AE are determined by the hardware (interference filter) of a sunphotometer. The study is restricted to the calculation of the AE based on 2 wavelengths. From the combination of both arguments it follows that there is no room for a random selection of the second wavelength. But one must have in mind that a different second wavelengths (different from the example wavelength of our manuscript) will only alter the magnitude of the results. Qualitatively

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the results will be the same, e.g. about the direction of the shift of the maximum of the AE distribution.

Minor point 1: AOD and AOT are mixed

Reply: We will follow the recommendation by Wehrli (comment to this manuscript) and use always AOD in a revised version of our manuscript.

Minor point 2 about numbering all equations:

Reply: we will number all equations.

Minor point 3 about strange text at the end of paragraph 4.1:

Reply: The two phrases do not belong to the manuscript. We don't know how they entered into the manuscript and how they survived the technical control. We would like to apologize.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12781, 2007.