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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Statistical evaluation of aerosol retrievals from AERONET using in-situ aircraft measurements" *by* A. R. Esteve et al.

Anonymous Referee #2

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Review of Statistical evaluation of aerosol retrievals from AERONET using in-situ aircraft measurements by A. R. Esteve, J. A. Ogren, P. J. Sheridan, E. Andrews, B. N. Holben, and M. P. Utrillas

The paper presents comparisons of AERONET sun photometer measurements of AOD and secondary products with observations from regular airborne in situ measurements of atmospheric aerosols. The study is of interest for the scientific community since regular airborne particle measurements are scarce and comparable studies are usually restricted to relatively short intensive measurement campaigns. The paper is good to read and easy to follow. The argumentation is detailed, convincing, and addresses the weaknesses of the underlying data set in a proper way. However, I think that important reasons for the identified difference between AERONET and AAO are not

fully addressed. Furthermore, after reading the paper I felt like a real conclusion is missing or at least not explicitly indicated. Since the authors already treat their findings critically and try their best to come up with explanations, I suggest publishing the paper in ACP after the following comments are addressed properly.

Major comments

Most of the comparisons deal with AOD from AERONET and in situ measurements. Therefore and for the reasons mentioned later, I think that the title of the paper is not accurate and even misleading. To meet the title the paper would have to include comparisons of particle size distributions and refractive indices which are unfortunately missing. I therefore suggest renaming the paper to something like "Closing the gap: sources of differences in aerosol optical thickness derived from sun photometer measurements and in situ aerosol profiling."

I missed an introduction to the measurements used in this study. The authors jump right into comparing the two data sets. For the reader it would be good to get an impression about the aerosol situation at Bondsville in terms of histograms of AERONET measurements and mean aerosol profiles from research flights within the time period under investigation (Maybe between Sections 2 and 3). Also, it would be good to know if there is any seasonal variation of the optical properties or of the number of encountered aerosol layers.

The comparison approach should be described in more detail. I could only find that in situ measurements are compared to AERONET measurements no later than 2 h after the measurement flight. What was used from AERONET? Was it a single measurement during the flight? Or averaged data until 2 h after landing (but starting when)? This is not clarified in Section 2.2. Some more information is given in Section 4.7. But this is also not clear (and certainly too late in the text.) Maybe it is possible to visualize the comparison approach in a sketch?

The authors address a lot of possible error sources but they seem to forget the biggest

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source of uncertainty (which could be covered by giving a more general introduction to the measurement at Bondsville): Do they encounter aerosol conditions under which column-integrated findings can be compared with height-resolved measurements (besides temporal homogeneity)? I would suggest that this is only the case if one encounters a single well-mixed aerosol layer. How many aerosol layers were usually observed? What is the influence of long-range transport? Based on these considerations, can you find sub-sets for which better agreement is achieved? Even if all layers are covers by the in situ measurements, it could be the case that aerosol properties are very different within these layers and the average from the in situ profiles is not identical to the AERONET-derived mean of the entire aerosol column. Is there a systematic error by comparing column-integrated values to a mean over individual values for different height layers? The authors should make better use of the height-resolved measurements to address these issues. Please comment on these sources of uncertainty.

Point 3 of the Discussion (aerosol layers below, between or above the fixed flight levels) should be addresses entirely. I don't really understand why the important point of investigating the presence of aerosol layers above the flight level (or rather the use of CALIPSO measurements for such a task) is considered to be "beyond the scope of this study". Such layers would originate from distant sources and should be visible over a rather wide area. Thus, why not taking a look at the top of the first (highest) aerosol layer identified in the CALIPSO level 2 aerosol layer product for measurements in an area of 100 km (or more) around Bondsville? Using the CALIPSO subset tool I found 199 overpasses between 20 June 2006 and 31 October 2008 for 100 km distance from Bondsville. This could be used to estimate the influence of aerosol layers above 5 km height which I would assume to be non-negligible. Such an investigation could be presented without getting too much into details and would cover an important but yet omitted point of the study.

If the authors find that the effect of humidity is dominating (minor effect of elevated

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aerosol layers and no systematic error in the comparison approach), it could be interesting to determine the needed growth factor for which agreement between AOD from AERONET and AAO could be achieved and weather such values are realistic.

The discussion in the conclusion should be expanded to cover the general comparability of column-integrated findings with means of height-resolved measurements. I would also like to read a more significant conclusion or at least some kind of ranking of the influences that lead to the differences between AERONET and AAO.

Some of the figures (e.g., Figs. 3 and 9) could be omitted when their content can clearly be described in the text.

Specific comments

Symbols for scattering and absorption coefficients are introduced in Sections 1 and 2.1 (with varying indices). AOD is also introduced several times. Please only introduce abbreviations if you are going to use them later in the text.

Page 29006, line 5: I suggest adding when between term and using to avoid confusion about the price of aircraft measurements.

Page 29006, line 13: You are not evaluating AERONET AOD! I think everybody agrees that you can find no better standard instrument for the measurement of AOD than an AERONET sun photometer. The inversion products on the other hand depend on the accuracy of the AERONET aerosol model and can always use independent evaluation. You should emphasize that the main objective of this paper is to investigate the sources of the observed disagreement and to assess their influence. Please rephrase.

Page 29007, line 20: I think (D50) should be moved behind 50% for better understanding.

Page 29016, line 18: What is meant by “dry”?

Page 29018, line 24: This exercise would be easier to understand when you clearly

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state/repeat that size distribution measurements were not performed in the aircraft.

Page 29022, line 1: This section 4.5 should be shortened. I don't think there is need for Fig. 19.

I suggest combining Figures 1 and 2 to one figure that gives the correlation of the optical data. Figures 4 and 5 could be combined in the same way.

Please remove Figure 3. This one data point can also be discussed without the picture.

Figure 5: Is there some information in the larger variation of values obtained from the AAO measurements compared to AERONET?

Figure 9: Since no huge difference is found, I don't see a need to show this figure.

Figure 19 shows the same as Figure 1 and is therefore not needed.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29003, 2011.