

Interactive comment on “Seasonal differences in the vertical profiles of aerosol optical properties over rural Oklahoma” by E. Andrews et al.

Anonymous Referee #1

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1 General comments

Andrews et al. present in their publication the result of 6 years of vertical profiles measurements of aerosol absorption and scattering recorded over a rural site in Oklahoma. The paper is well written and presents an important dataset needed to improve our understanding of the effects of aerosol particles on climate.

However, I miss real new scientific insights gained by this work, especially with respect to the publication of Andrews et al. from 2004 in JGR, where the results of the same suite of measurements were already presented although for a shorter time period of 2 years. The investigation of the seasonal patterns is the only real new result, but not sufficient for a publication in ACP at this stage. I would like to encourage the authors to

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add further analysis to their work before publishing this paper in ACP (see comments below).

Profiles: How did the mean profiles of RH, T and absolute humidity compare to the optical measurements? This could help in the interpretation of the results with respect to the mean PBL height of the different seasons. It could also help in estimating the humidity effect when later comparing the profiles to AERONET data.

Comparison to AERONET: Why is the Angstrom exponent not discussed here? Please add a discussion on how this parameter compared to the AERONET measurements. At the end of the AERONET/in situ comparison a few arguments are brought up in one sentence to explain the differences encountered. In addition, a thorough discussion is announced in an upcoming paper. This is a comfortable and too easy approach and a discussion of the encountered differences should definitely be part of this paper. It would also be important to give some recommendations on how these measurements could be improved.

Clustering of back trajectories: In addition the optical profiles could be discussed as averages according to the air mass origin. This could give more insights on the sources and effects on the specific profiles.

Cloud scavenging: The hypotheses on cloud scavenging being responsible for the decrease of the single scattering albedo with altitude is an interesting one, but needs more back up arguments. So far, I would explain the difference by the higher uncertainty in winter due to a decrease in signal to noise ratio. In addition, could it be that at higher altitudes the RH inside the nephelometer is lower (due the larger temperature change from outdoors to the warmer cabin). This could also explain the encountered change in the single scattering albedo.

2 Specific comments

Page 11940 Line 12-13: Please remove the reference to CALIPSO and IMPROVE from the abstract since this is not shown and discussed in the paper.

Page 11943, Sect.2.1: How was the (pressure dependent) Rayleigh correction of the nephelometer being established at the different height levels?

Page 11944 Line 26-ongoing: The correction of the dry in situ measurements to ambient conditions is a crucial step and needs more attention. For example: Was the same humidity dependency correction being used for all profiles? If so, what parameters (a and b) from Eq. 3 in Andrews et al., 2004 were used? Or was the change in season (and change in mean aerosol composition / hygroscopicity) considered?

Same line: Please clarify on how the supermicrometer scattering and absorption fractions from the SGP site were used to correct the airborne in situ measurements.

Page 11947 Line 15-18: If the sulfate is clearly changing with season, while the organic mass is fairly constant with time, this would mean that the hygroscopic growth is also changing with season (see comment above). How was this accounted for in the adjustment of the dry in situ measurements?

Page 11948 Line 1-4: I doubt that the constant inverse wavelength correction for the PSAP measurement is the main explanation for the differences in the seasonal pattern. At this relatively high SSA the influence of the absorption is compared to the scattering small. Are aethalometer measurements from the ground SGP site available to investigate the seasonal pattern of the Angstrom exponent of the absorption coefficient?

Page 11950 Line 11: Please remove the reference to CALIPSO and IMPROVE from the conclusions (see comment above).

Page 11950 Conclusions: Besides the seasonal behavior it should also be mentioned that the AERONET AOD and the other parameters were systematically lower than the

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in-situ values due to the reasons mentioned earlier.

Page 11955 Fig.1: Could the difference in the SSA profile (winter/summer) also be explained by the changes in the RH inside the nephelometer cell?

Page 11955 Fig.1: Were the error bars for the fall and spring averages similar to the summer and winter cases? Please include a short sentence on that in the manuscript.

Page 11955 Fig.1: Please add error bars also for the asymmetry parameter for the winter and summer case (panel d).

Page 11955 Fig.1: The x-axis for panel (c) can be optimized e.g. from 0.5 to 1 to improve the visualization.

3 Technical comments

Page 11943, Line 11: Please explain the abbreviation PSAP to the reader.

Page 11942, Line 10: Please explain the abbreviation DOE/ARM.

Page 11943, Line 21: The changing number of profiles is a bit confusing in the paper (e.g. Tab.1 = 448 flights while in the text "458" and "over 450" flight are mentioned as being analyzed). If 458 flights were conducted within March 2000 and July 2005 (as stated in Line 4) this would rather be 1-2 flight per week (458 flights / 280 weeks \approx 1.63) instead of 2-3.

Page 11959 Fig.2: Please replace WIN with DJF to be consistent within the figure.

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