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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biomass burning aerosol properties over the Northern Great Plains during the 2012 warm season" *by* T. Logan et al.

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In Logan et al.'s "Biomass Burning Aerosol Properties over the Northern Great Plains during the 2012 Warm Season", the authors examine differences in aerosol optical and physical properties retrieved AERONET for six biomass burning plumes. Specifically, the authors use volume size distributions as well as spectral AOD, AAOD, and a calculated "co-albedo" parameter to distinguish between the six case study days. Differences in the spectral shapes of the AOD and AAOD were primarily influenced by differences in particle size distributions. On the other hand, variations in the spectral dependence of the co-albedo was attributed to differences in the carbonaceous aerosol concentration and type (i.e., strongly versus weakly absorbing). Finally, a re-

lationship between plume transport time and variability in the spectral dependence of the three optical parameters was sought. Though the manuscript presents an interesting approach to examining AERONET data, it has some significant shortcomings that need to be addressed prior to publication. In general, both the methods and results and discussion seem underdeveloped and lack both detailed explanations and robust comparison to previously literature. For instance, there is no direct mention of how the six case study days examined were chosen. It is suggested in paragraph 2 of section 3.1 (starting on page 32275, line 6) that HYSPLIT was used after the case study days were chosen to identify biomass burning source region. However, presumably HYSPLIT was actually used, along with AOD440, to identify case study days. A more detailed description of the process involved in case selection needs to be included in the methods.

We agree with your comment. First we identify each case based on AOD440 > 0.4 to ensure the best data quality since we are using the absorptive parameters of AAOD and 1-SSA which represent small fractions of aerosol scattering/extinction. We also choose these specific cases as these were the only occurrences of moderate to strong biomass smoke intrusions from wildfires to the south and west of the Grand Forks AERONET site. We also include the entire time series of AOD440 during the warm season to show other minor intrusions but could not report on those cases due to possible poor data quality (AOD440 « 0.4).

Additionally, reasons for calculating the co-albedo as the ratio of AAOD to AOD are unclear. Why not just use (1-SSA) for the co-albedo (should be the same value since AAOD=(1-SSA)*AOD)? Even if the calculation and usefulness of AAOD/AOD is explained elsewhere (e.g., Logan et al., 2013), the reason for its use should still be mentioned here.

We use the AAOD calculation to be consistent with the methods used in the Logan et al. 2013 study. We have calculated the co-albedo using 1-SSA and AAOD/AOD and found that both methods yield values are not exactly the same but very close.

It is also not clear which values for AAOD, AOD, and size distributions are used in the analysis. Are daily mean values or one "representative" hourly measurement for each day plotted in Figure 4 and discussed in Section 3.2? This should be explained in both the discussion of Figure 4 as well as in the Figure 4 caption. Additionally, assuming there is more than one AERONET measurement for each day, the variability in each parameter for each day should be noted both in the text and by the addition of error bars to Figure 4.

These are daily mean values that represent at least one almucantar measurement. The AERONET is not operational during cloudy periods and may shut down due to other circumstances. Error bars are not provided in Figure 4 because we use normalized spectral dependences for AOD and AAOD. We can provide error bars for the single scattering co-albedo.

The results seem a bit over interpreted at times, primarily due to the absence of any statistical analysis. For instance, while there appears to be a relationship between size distributions and the Angstrom exponent for some of the case study days, all values fall within a fairly narrow range (1.54-1.98) and indicate the presence of smaller particles. It is possible that despite apparent differences, these values are not statistically different therefore eliminating the need to explain trends. Similarly, the absorption Angstrom exponent (AAE) values fall around 1 for all cases (though Case VI is a bit lower) suggesting BC is the dominant absorber. As with the Angstrom exponent, it is possible these values are statistically similar and therefore differences between case study days are simply due to random noise and not differences in carbonaceous aerosol as suggested by the authors. If there is more than one measurement per case study day, statistical analysis should be performed to assess the actual differences in daily mean Angstrom exponents, AAOD, and co-albedos. Further, it would be helpful if values reported by the same AERONET station for non-biomass burning days were reported and included in the statistical analysis to highlight the claim that these biomass burning cases consist of aerosols with unique properties.

We completely agree with your comments. We will add additional radiative parameters such as radiative forcing (delta F) and radiative forcing efficiency (delta Feff) (provided by AERONET) to show how each case is unique and the varying carbonaceous content in the smoke aerosols is related to a "warming" or "cooling" within the aerosol layer. For example, Case III (Case V) showed the highest (lowest) absorption (Fig. 4) and therefore a stronger warming (cooling) effect which we attribute to the relative contributions of strongly absorbing BC (in Case III) and weakly absorbing OC (in Case V). We will also provide information on non-biomass burning days as per your suggestion to serve as a baseline to compare to the biomass burning smoke intrusion days.

It is incorrect to attribute all absorption at 440 nm to OC as BC is noted to absorb strongly at all wavelengths. Thus, changes in the co-albedo at 440 nm may not solely reflect changes in the OC as is suggested on page 32277, lines 19-22. Distinguishing between strongly and weakly absorbing OC should therefore be avoided without supporting chemical measurements. Further, based on the AAE values reported in Table 1 (nearly all 1) as well as the positively-sloped spectral co-albedos plotted in Figure 4d, OC appears to have a minimal influence on absorption for any of the plumes. It is possible that cases study days with more "spectrally-flat" co-albedos (e.g., II and V) have a larger contribution from OC to total absorption however this is merely speculative without chemical measurements. The authors note that interpreting OC and BC contributions to absorption from AERONET data is difficult (lines 17-22, page 32276), yet there is considerable effort made to explain trends in spectral optical properties using changes to OC and/or BC. The authors need to be more cautious about making such conclusions, especially since it is not known whether or not the differences in co-albedo and AAE values are statistically significant.

We agree with your comment and chemical analysis is indeed essential. However, there are few (if any) in situ measurements of smoke chemistry along the transport path. Though this study cannot provide exact BC/OC measurements, we can at the very least comment on the absorptive properties of the smoke and relate them to rela-

tive BC/OC influences. We also note that our conclusions do seem to support the other studies we list in Line 26-27 pp 32276-32277. Though we should not ignore the possible significance of this study in the face of “statistical significance”, we will carefully word our discussion to reflect issues due to lack of observations.

Finally, a more complete comparison of the results presented here to those previously published for biomass burning aerosol needs to be included to validate the claims made by the paper. Specifically, the Angstrom exponents, AAODs, and co-albedo should be compared to those values measured previously for biomass burning aerosols, especially those for which the combustion conditions are known (see publications on the FLAME 1, 2, and 3 experiments). Comparisons to previous literature should include both daily mean as well as max and min hourly values.

We agree with your comment and will include more comparisons with previous biomass burning studies such as the ones you mentioned.

A few more editorial comments: -The first paragraph in Section 1 (pg.32271, line 1-12) seems out of place. The introduction should instead start with paragraph 2 (currently starts pg 32271, line 3) and the first paragraph should be moved down, either placed above or merged with the paragraph starting on line 18 (pg 32272).

We agree and will make the necessary changes.

-Omit “solar” on line 3, pg 32272.

Completed.

-Omit “of the retrievals” on line 7, pg 32274.

Completed.

-“Will use” should be “used” (pg 32274, line 14).

Completed.

-Include co-albedo and transport time values in Table 1.

Completed.

-Figure 3 needs a more descriptive caption (i.e., what are the data shown? Model results? Concentrations?).

Completed.

-All figures need larger axes labels and titles

Completed.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 32269, 2013.