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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.

T. Logan et al.

timothy.logan@my.und.edu

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In this manuscript "Biomass Burning Aerosol Properties over the Northern Great Plains during the 2012 Warm Season", Logan et al. selected six days of high AOD and plotted the corresponding AERONET measurements. The authors state themselves in the beginning of 3.2.1 that "First we note that the exact contributions of the black and organic carbon constituents to the spectral dependences of AAOD and tau_abs are difficult, if not impossible to determine solely from AERONET ..." They later repeat this same statement two more times. One can easily agree with them, but wonders then why all this speculative discussion in that section? I was not able to see what would be original in this study so that it should be published. I think it is therefore fair to ask that

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the authors would kindly explain and answer that same question: what is original here and what new can we learn from this study?

We agree with your comment that it was not entirely correct to infer the exact contributions of BC and OC based on spectral dependences but other published studies were able to at least make a first order approximation of BC/OC contributions, many of which we referenced in this study. In addition, our published study "Logan et al. 2013" extensively discussed the merits of using spectral woabs dependences to discern between weakly and strongly absorbing carbonaceous compounds. We will revise the manuscript to focus on comparing/contrasting the varying absorptive nature of the smoke particles for the six cases. We also point out that this is a brief quantitative study on the biomass smoke plumes over a specific site since such incidences are not only rarely observed, but also rarely investigated due to lack of ground-based observation stations in this area. The Grand Forks AERONET facility is the only such facility with hundreds of kilometers between the wildfire source regions in the intermountain west and sink regions in the Midwestern United States and Great Lakes.

I would like to mention below few points that I see as main weaknesses in the current study. Total of six cases is a very limited set to draw conclusions. Moreover, the selection is not very convincing, it seems rather that there were six cases that reached Level 2 (reached AOD at 440nm higher than 0.4) in the inversion product (for SSA and AAOD) and those were then simply selected.

We agree that six cases are limited but as mentioned in our introduction, the warm season of 2012 was an anomalous year for wildfires in North America. Though satellites can track the movement and optical properties of the events, there are few ground based measurement platforms in the path of the smoke. Previous studies using AERONET data stress that the retrievals have large uncertainties when the AOD is less than 0.4 and we were careful to only investigate the occurrences where AOD was greater than 0.4 but did include a time series of all AOD retrievals throughout the warm season to show the instances of wildfire smoke plumes over the Grand Forks

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AERONET site. Again, there are few AERONET stations along the path of the smoke, let alone, ground-based stations of any kind.

Many of these days are based on single measurement only and they are interpreted, or speculated rather, beyond what one could really conclude from those measurements alone (as the authors stress themselves several times). For instance, it seems that there are only single one AAOD and SSA spectral measurement for the following cases: I, II, VI. And the maximum case is 3 for IV, rest of the cases have 2 measurements. These very small measurement numbers should be stressed and included in the Table 1.

We agree with your comment that the data may have been over-interpreted and we will tone down our conclusions by limiting our analysis to discussing weakly vs. strongly absorbing aerosols and include more radiative parameters such as the radiative forcing and radiative forcing efficiencies provided by AERONET. These parameters can give insight as to the possible warming or cooling effect the smoke particles exhibit over the northern Great Plains region.

It seems that the data analysis has not been done very carefully. For instance, the authors state (in the block 32276, line 13) that it is surprising that the case V had higher alpha than case II. I argue that there is nothing surprising if you had considered the different temporal sampling of direct sun and sky measurements. For example, in case V there are two size distribution measurements, at 21:32 and 22:10 UTC, while direct sun and therefore Angstrom Exponent (AE) measurements from 17:30 to 22:30, which is decreasing in time. If you considered these data from the same time, there would have been nothing surprising.

We agree with your comment and will remove this statement.

The current discussion, unfortunately, seems rather vague, to give just couple of examples. Block 32278, line 6, it is not at all clear how in your case (1-wo) shows more of the internal composition than the AAOD parameter. We agree with your comment

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and will remove this statement. About the case VI you say that there is strongly absorbing BC and OC. For this statement, as I said above, there is a single one spectral SSA measurement. If you looked at spectral imaginary spectra in addition, you would have seen a strong decrease with decreasing wavelength. So there is no justification to discuss OC presence. If the imaginary spectra were additionally included, it would have been already strengthen the discussion and conclusions.

We agree with your comment. The spectral dependence of the imaginary part is flat for all cases except for Case VI which was thought to have a large BC contribution. At any rate, we still believe that it is possible to discern between BC and OC using this method but further validation by in situ chemical measurements is needed.

What are in the Figure 2? It seems NAAPS data, but there is no single word about that kind of analysis.

We apologize for the omission here. This is a NAAPS product.

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