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

Anonymous Referee #1

Received and published: 3 February 2014

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

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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.

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.

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.

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.

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

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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.

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

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