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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spectral absorption of biomass burning aerosol determined from retrieved single scattering albedo during ARCTAS" *by* C. A. Corr et al.

Anonymous Referee #3

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Review of Corr et al., (2012), Spectral absorption of biomass burning aerosol from retrieved single scattering albedo during ARCTAS.

The paper is basically a good paper and should be published. It calculates the spectral absorption coefficient and single scattering albedo for two different biomass burning plumes sampled during the 2008 ARCTAS field program. The first, an aged biomass burning plume from Siberia and the second, a fresh plume in the boreal forest of Canada.

The authors used the actinic flux measurements made aboard the DC-8 and a radiative transfer model that the first author has used before in a 2009 paper. Use of the

actinic flux measurements is somewhat unusual and the authors discuss some of the difficulties with its use. However, the results seem reasonable for the most part and the authors discuss the values that are questionable. They do a good job of relating their results to values published in the literature.

The authors spend a considerable amount of discussion on the effects of organic aerosol absorption and the reason for the difference between retrieved aerosol absorption properties between the first plume and the second plume. The discussion is pretty speculative and I have some comments for the authors' consideration.

My comments/questions are as follows:

1. The decrease in SSA from 500-550 nm in Figs 3 and 4 is hard to understand. The authors discuss the possibility of the effects of a low AOD (for the April 27th case). This is actually a common problem as the AOD falls off considerably with wavelength. Perhaps the authors could come up with an estimate of the error in the SSA due to low optical depths.

2. pg 13978, line 22 - what are "non-aerosol spectral features inherent . . ." ?

3. pg 13979, line 10 – sentence beginning with "However, minimum . . ." needs to be rewritten.

4. Subtracting off of a $1/\lambda$ absorption to estimate the absorption due to OC while not necessarily a bad idea, ignores the fact that most of the particles are made up of BC and OC in some combination. Thus, this is just a technique to estimate the effects of OC. (Also, Arola et al., might not be the best reference – Yang, M., Howell, S. G., Zhuang, J., and Huebert, B. J.: Attribution of aerosol light absorption to black carbon, brown carbon, and dust in China - interpretations of atmospheric measurements during EAST-AIRE, Atmos. Chem. Phys., 9, 2035–2050, doi:10.5194/acp-9-2035-2009, 2009 – might be better)

5. I'm not a chemist but it seems to me that the AAE for organic aerosols is a function

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of the type of burning conditions more than aging. That's not to say that there aren't any effects of aging but in a cold, relatively dry atmosphere in the northern latitudes it seems unlikely. Again the AAE will be sensitive to the low AOD.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 13967, 2012.

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