

Interactive comment on “Carbonaceous aerosol AAE inferred from in-situ aerosol measurements at the Gosan ABC super site, and the implications for brown carbon aerosol” by C. E. Chung et al.

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We (paper authors) thank this reviewer for many constructive suggestions. Below, we explain how we will address the concerns raised by the reviewer.

Structural Concerns 1. In my opinion the Authors do not provide adequate context for their work, particularly in the abstract and introduction sections. For example – they seek to calculate the AAE and MAC, but do not explain why these properties may matter. Further, their most significant result (i.e. OC absorption is 45% of total Carbon absorption) needs to be better related to applied climate science – how does this compare to what present studies and models are suggesting or assuming, for

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instance. How would this number impact our current understanding of aerosol forcing? These questions need to be answered.

==> We acknowledge insufficient explanations in the abstract and introduction sections, and will re-write these sections. That OA absorption is 45% of total carbon aerosol absorption is only one of the important findings in our study, and we consider our OA AAE estimate the most significant finding. We failed to properly emphasize the significance of our findings in the manuscript. Even for our finding of OA explaining 45% of the absorption, we did not explain the implications at all. We will revise the manuscript in these aspects. By the way, we distinguish OC from OA (organic aerosol) in our paper (since BC contains OC). We use OC to refer to organic material; organic material is in OA and BC, and the OC in BC is referred to as BC-intrinsic OC. We will make this point even more clear in the revised manuscript.

2. The manuscript is very poorly organized. For example, Figure 9 and Table 3 are referred to out of order on Page 4510. Similarly, Section 4, which describes errors and uncertainties in the measurement, should precede Section 3, which describes analysis of the measurements. Sections 5 and 6 could be possibly combined.

==> We will re-organize the manuscript.

Major Scientific Concerns 1. Absorption Angstrom Exponent (Section 2.2). The authors determine the AAE by regressing a single curve onto their 7-band absorption measurement. While this satisfies the classical formulation, recently it has been shown [see for example, Flowers et al. 2010 (ACP) and Moosmuller et al. 2011 (ACP)] that the AAE itself is wavelength dependent. Further, the nonlinear features of the AAE, i.e. the deviation from the standard value of “1.0” are more pronounced for OC like aerosols at short wavelengths. By regressing a single curve onto their measurements, it is my opinion that the authors are emphasizing the long-wavelength (i.e. flatter) part of the curve, which may explain their very low value of OC AAE.

==> We provided OA MAC at each of the 7 wavelengths (Table 1). Since OA AAE is

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computed from OA MAC, the readers of our paper can compute OA over fewer wavelengths of their choice. Nevertheless, our estimates of BC (and OA) MAC and AAE depend on the data filtering criteria, and the 520 nm absorption coefficient criterion was derived from CA AAE. We calculated CA AAE using all of the 7 wavelengths. We will conduct the sensitivity of CA AAE to the wavelength choice.

2. Filtering out dust-influenced events (Section 3). The authors eliminate measurements that could potentially contain a large dust mass (and correspondingly absorption) by tracking the PM10/PM2.5 ratio, working on the premise that dust is typically found in the coarse mode. While I find this reasoning to be sound, it is problematic that the authors do not justify their threshold value of 1.6. What is this based upon? As best as I can see, only Figure 4 actually illustrates measured values of this ratio, presented as monthly values. Based on this figure alone, only measurements for November and June are valid –therefore this figure is clearly inadequate, and worse, misleading. The authors need to provide a concise visualization of the entire data set (perhaps as a probability distribution function), and justify why 1.6 was chosen as a cutoff.

==> 1.6 was based on Fig. 5, and a compromise between weakening dust effect and surviving data volume. We will better explain this. Also, we will conduct the sensitivity of the results to the cut-off value.

3. Somewhat minor – as written, Equation 2 is incorrect as the intrinsic OC is being subtracted from both the BC and OC.

==> Originally, we typed the equation as follows: $Abs(\lambda) = MAC_BC(\lambda) \times (EC + BC - \text{intrinsic OC}) + MAC_OA(\lambda) \times (OC - BC - \text{intrinsic OC})$ (2). However, when we submitted the manuscript to ACPD, the ACPD system did not distinguish between – (minus) and -, and made a mess, and we did not correct this during the initial submission. Sorry for our oversight! We'll correct the equation and the new equation will read as follows: $Abs(\lambda) = MAC_BC(\lambda) \times (EC + BOC) + MAC_OA(\lambda) \times (OC - BOC)$ (2), where BOC refers to BC-intrinsic OC.

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4. Assumption that OC constitutes 20 % of the BC mass – again, this assumption needs to be justified in some fashion. The authors find that this does not impact their OA MAC (understandably, since OC concentrations are typically very high in Asian outflow), but it does significantly change their estimate for the BC MAC – this in turn impacts the authors core conclusion that OC absorption is roughly 45% of total carbonaceous absorption. At the very least, the authors need to provide a range of uncertainty here.

==> We assumed that BC-intrinsic OC (which is not the same as OC in OA) carbon mass would account for 20% of the EC mass in a sensitivity test. BC consists of EC and OC (though EC dominates). We thought we made this point clear but apparently it was not clear enough; there were also typos. We will make this point very clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4507, 2012.

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