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Atmospheric Chemistry and Physics Discussions

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. We will address the concerns raised by this reviewer as follows.

There is currently an increasing evidence of "non-BC" absorption (brown carbon) and this study brings an interesting addition. However, the approach of this study should be described in more detail (with likely additional and supporting figures, as explained below) and the overall discussion should be strengthened. Moreover, altogether this manuscript reads quite difficult - many parts are superficially covered, including vague statements. I give some examples of these below, but not trying to give an exhaustive

list. Significant revision is required, before this manuscript can be published.

==> After reading these comments and re-examining the manuscript, we identified insufficient or unclear explanations in many places. We will re-write the abstract and introduction section, and strengthen the other sections.

The main results (essentially in the Figure 9) are interesting, however the reader should be given a better idea how well the OC BC separation actually works and how robust this approach is. I think it would be important to show also the actual fit to the remaining data (after filtering criteria) with statistics about the goodness-of-fit. Equally important would be to discuss/estimate how robust this estimate is, for instance, by adding fits by using only the first five Aethalometer wavelengths or last five Aethalometer wavelengths. I think it is important to give a reader some idea about these aspects now when you are assuming a wavelength-independent AAE over such a large wavelength range. With the current description, the reader might have some doubts about how clearly OC and BC were separated, when OA MAC in Tables 1 and 2 gets exactly the same values in that sensitivity study. Therefore, I think it is necessary that you show both how good and robust your fit was.

==> This is an extremely valuable point. We agree that we did not conduct sufficient sensitivity tests to understand the robustness of the estimates. We will conduct all the sensitivity tests suggested by the reviewer, and will also explore the sensitivity of the results to the dust effect cut-off value.

Discussion in 4510, line 21 serves as an example of vague statements. It is not easy to see what the authors really want to say, however, it is easy to see that apparently they read neither of those references carefully. So please remove or somehow clarify to give more exact statement. There are tens of more papers, and also more recent ones than those two you referenced, which state that it is common to assume AAE_1 when BC is the dominant absorbing aerosol. It is a common assumption, because currently there is no strong evidence to assume otherwise. Still, in many of the papers

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the influence of coating to possibly decrease AAE below 1 is mentioned (e.g. in Arola et al.). In neither of those references, you chose to site, the assumption of AAE for BC did not really influence the analysis, so it is unfortunate that your statement gives that impression to the reader. Khatri et al., for instance, analyzed 0.9 < AAE < 1.1 separately, without really differentiating that data relative to 1. Also, Arola et al. did not assume explicitly anything at all about AAE in their retrieval. Although, again your statement easily gives that erroneous impression to the reader. They had to assume spectral imaginary index of BC, for which they assumed a wavelength-independent case. If you consider that this particularly was the "strange" thing, then please change your statement from superficial and vague to useful by giving references of strong and clear evidence of wavelength dependent BC imaginary index that would have been clearly a better choice in that study.

==> We will change the references and adjust the text.

I think you should discuss more your uncertainty of 5% for TOT method? It is the separation between OC and BC that is most crucial in your analysis, more than the uncertainty of total carbon, and in the literature more significant uncertainties are discussed. There are many more references, but for instance in Reisinger et al. "Intercomparison of Measurement Techniques for Black or Elemental Carbon Under Urban Background Conditions in Wintertime: Influence of Biomass Combustion", they wrote "Usually, thermal methods differ little in total carbon (TC) concentrations. The major problem is the OC (organic carbon)/EC split. EC (measured with thermal methods) or BC (measured with optical methods) can differ in intercomparisons by factors of three or four depending on aerosol characteristics." I think your measurement uncertainty deserves to be discussed more, also against the results from intercomparison campaigns of OC BC measurements techniques.

==> The accuracy of this instrument should be $\pm$ 5%, which is given as a criteria for calibration using the known standard. Therefore, the uncertainty is greater than this for ambient measurements, particularly for a long period of time (e.g., Polidori et al.,

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2006). We will strengthen the paper in this regard.

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