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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mass absorption efficiency of elemental carbon and water-soluble organic carbon in Beijing, China" *by* Y. Cheng et al.

Anonymous Referee #1

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This manuscript quantifies the mass absorption efficiency (MAE) of EC in Beijing using a DRI thermal-optical carbon analyzer. It suggests that MAE of EC is much lower in the regions heavily impacted by biomass burning, based on inter-comparison of MAE values across studies and regions. Moreover, absorption spectral of PM2.5 water extracts in Beijing is also presented, and the seasonal variation of MAE of the water-soluble organic carbon (WSOC) is attributed to the difference in the precursors of secondary organic aerosol (SOA). In recent years, increasing attention has been paid to the optical properties of ambient aerosol in China, especially with respect to visibility and regional haze. However, optical measurements are still very lacking, and importantly, optical properties of OC has rarely been investigated in China. Thus, this work is important

and interesting. It is also a valuable contribution to our understanding about brown carbon. It should certainly be published on ACP after a few points are addressed.

Major comments:

Page 24742. The equivalent MAE values quantified by this study should not be compared with the reference value suggested by Bong and Bergstrom (2006), because the measurements methods are different and the "converting factor" is unknown. As suggested by the authors, optical measurement performed by the carbon analyzer is comparable with Aethalometer. Therefore, the authors should only focus on results from carbon analyzer and Aethalometer.

Figure 3 and Figure 4. The data shown in Figure 3 and Figure 4 seems exactly the same as Table 1. I do not think Figure 3 and Figure 4 provide any useful information in addition to Table1. By the way, their captions are very difficult to follow.

Page 24745. The authors discussed too much about biomass burning in Beijing. Though several representative literatures are cited, disappointedly, it only concluded than the contribution of biomass burning to Beijing aerosol is still high uncertain. These discussions should be more brief or totally removed if the authors can not provide solid evidence for the influences of biomass burning on the MAE of WSOC.

Specific comments:

Page 24730, what does Rabs mean?

Page 24731, HULIS is another typical kind of brown carbon. It should also be included. And it is not necessary to introduce tar balls in such a detail.

Page 24733, the discussion about selecting of EC method should be more brief, because it is not the focus of this study.

Page 24746, the conclusion is too long. For example, seasonal variation of BVOCs should be removed.

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Figure 6, uncertainties of regression should be presented.

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Interactive Comment