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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Light absorption property and potential source of particulate brown carbon in the Pearl River Delta region of China" *by* Zhujie Li et al.

Anonymous Referee #1

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The manuscript by Li et al. investigated the characteristics of brown carbon (BrC) at a suburban location in the Pearl River Delta region, with focuses on the BrC contribution to total aerosol absorption, BrC sources and the radiative effects of BrC. A suite of state-of-the-art instruments were used for the field observation, which was conducted during the winter of 2014-2015. Given that BrC is increasingly included in climate models, the topic of this manuscript falls well within the scope of Atmospheric Chemistry and Physics. The results, if interpreted and presented properly, can be a valuable addition to the literature. However, I cannot support its publication in its current form. My major comments are given below.

1. In the present study, light absorption by black carbon (BC) and BrC was separated based on the absorption Ångström exponent (AAE). This approach has long been used and there have been many studies of this kind for a variety of locations. A potential contribution that can distinguish the present study from previous ones is the uncertainty analysis (i.e., Section 2.2.3). However, substantial concerns are raised regarding this point. (1) As shown in Equation 12, number size distribution of the BC cores was estimated based on the particle size distribution derived from the SMPS and the volume fraction of BC. The underlying assumption is that all the observed particles were BC-containing, which is obviously wrong. (2) As shown in Equation 13, the volume fraction of BC is determined based on the "volume" of BC (V1) and the "volume" of whole particles (V2). V1 is calculated as the ratio of BC mass to the material density of BC, indicating V1 is related to the mass-equivalent size. However, V2 is based on the mobility size. I think it is not proper to calculate the BC volume fraction simply as the ratio of V1 to V2. (3) Please clarify how the coating thickness is parameterized for the Mie simulations.

2. There are too many grammatical errors and scientific content errors in the manuscript. Only some of them are listed below. (1) Abbreviations should be defined when they are used for the first time (e.g., BC in Line 48, SSA in Line 88, MAAP in Line 251). (2) Line 48-50. Please note that globally, open burning is the largest source of BC. (3) Line 66. BC absorption is wavelength-dependent. As mentioned by the authors themselves, the AAE is about 1.0 for BC. (4) Line 105. It should be "seven-wavelength". (5) Please use either "Aethalometer" or "aethalometer". (6) Line 116. Please check the presentation of the longitude and latitude. (7) Line 163-164. Please note that laser transmittance is monitored throughout thermal-optical analysis, rather than only for the OC stage. (8) Line 296. What does "range substantially" mean? I guess the authors may want to say "vary substantially". (9) Line 381. I can understand that for the chemical components measured in the present study, there was no better tracer than K+ for biomass burning. However, limitations of using K+ should be discussed (e.g., Aerosol and Air Quality Research, 18: 2447–2459, 2018). Maybe it is

better to correct the observed K+ for sea salt and crustal materials and then use the correct K+ for discussions on biomass burning. (10) Line 416-417. Please explain the absence of K+ peak during the lunch and dinner time. (11) Line 432-439. Although NOx and NH3 can be found in biomass burning emissions, this does not necessarily mean that nitrate and ammonium can be used as tracers for biomass burning, i.e., the correlation between BrC and nitrate or ammonium cannot demonstrate biomass burning as an important source of BrC. Anyway, the whole manuscript must be polished and refined before further consideration for publication.

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