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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ionic and carbonaceous compositions of PM_{10}, $PM_{2.5}$ and $PM_{1.0}$ at Gosan ABC superstation and their ratios as source signature" *by* S. Lim et al.

Anonymous Referee #1

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General comments: This manuscript presents 1-year ionic and carbonaceous aerosol measurements for coarse, fine and ultrafine particles in Gosan ABC superstation in South Korea. The details of the carbon fractions measured with the TOR method, and its association with different sources were discussed. The results are very interesting and worthy being published. My first concern is the lack of the interpretation of EC2+3 concentrations (soot in this manuscript) in these coarse, fine and ultrafine particles. It seems that soot concentrations in ultrafine particles are higher than those in fine and coarse particles. There should be an explanation. In fact, the higher concentrations of soot in ultrafine particles themselves are also interesting since soot is generally found in the sub-micron mode. I think that after re-reviewing previous discussions about the

thermal method for OC/EC quantification, as well as the deep analysis with the artifacts that influence OC/EC and char/soot separation, there should be a reasonable explanation on this point. My another concern is associated with the understanding about OP. The statement that "OP is light-absorbing organic carbon or a strong candidate for brown carbon" in this manuscript seems incorrect.

Specific comments:

Page 20522, Lines 15-18. "Our measurements of EC confirmed the definition of EC1 as char-EC emitted from smoldering combustion and EC2+3 as soot-EC generated from higher-temperature combustion such as motor vehicle exhaust and coal combustion." I know that this is from Han et al.' study. I think that here the references should be cited.

Page 20524, Line 19, "Therefore, BC is considered as light-absorbing EC and is generally lower in concentration than EC." I cannot agree with this point. BC and EC are just two terms that should be used to define the same material in the atmosphere. The difference used in the literature comes just from the methods that used for the quantification of this kind of material.

Page 20526, Line 20, "most of these studies .....were conducted during a specific season, such as spring (Lee et al., 2007; Shen et al.,2007), spring and early summer (Aggarwal and Kawamura, 2009), or fall and winter (Cao et al., 2005)." This seems not the case. Lots of studies in northeast Asia have been done not just during a specific season but from whole year or several year observations.

Page 20529, Line 18, "A considerable fraction of mass other than water-soluble ions, OC, and EC was possibly due to trace metals and silica, which were not measured." I would like to see a reference here.

Page 20529, Line 25. "The difference in OC/EC ratios is likely due to relatively higher EC concentration than OC in this study area." It seems that this sentence is meaningless. Compared with other studies, the EC concentration in this area is not very

high.

Page 20532, Line 19. “Charred EC” should be “char EC”.

Page 20534, Line 13, “It is likely due to the formation of secondary organic aerosols from biomass burning.” However, EC is not influenced by the formation of SOA. In addition, where is the biomass burning? Local or regional?

Page 20534, Lines 21-24, I wonder if there should be an introduction of the Absorption Cross Section Calibration Option. Or there should be a discussion about the Absorption Cross Section Calibration Option.

Page 20535, Lines 10-11. I would like to suggest references here.

In Sec. 4. (such as Page 20537, Line 13), Han et al. (2009, Atmospheric Environment) have also studied the PM2.5 char and soot concentrations in winter and summer from 14 cities in China. I would like to suggest a comparison of char and soot concentrations here. This will give a general picture of the difference in char and soot concentrations between China and South Korea.

Also in Sec. 4. From Table 2, it seems that soot concentration in PM1.0 (0.46 in average) is a little higher than that in PM2.5 (0.40) and PM10 (0.30). I think that there should be an explanation for this. In my view, I think that this is associated with the fact that amounts of ions and metal oxides exist in coarse particles, which would influence the carbon and soot energy stability in thermal oxidation. Please refer to Novakov and Corrigan, 1995, Mikrochim Acta (association with the metal oxides), and Han et al., 2009, Chemosphere (association with ions, perhaps especially with the nitrate in this study).

Page 20541, the second paragraph. I cannot understand “OP ..... is likely a light-absorbing aerosol”, as well as other relevant sentences. I think that OP comes from the pyrolysis of organics in carbon analysis process, and thus itself is not a kind of aerosol. For example, water soluble organics and volatile organics can result in more OP in

carbon analysis process. However, these materials are not “light-absorbing organic carbon or a strong candidate for brown carbon”. I understand that these materials can come from biomass burning and thus may be associated with HULIS production. “The enhanced absorption of shortwave radiation” may be linked to the HULIS in samples, but not with the OP. Thus, I think that the statement “OP is light-absorbing organic carbon or a strong candidate for brown carbon” is wrong. Please check the manuscript and find the corresponding part associated with the description of OP, and to make sure that all these parts are correct.

When discussing about the seasonal variations of chemical components, I would like to know based on what the seasons are separated. Also, the introduction of local human activities (with or without) would be useful.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20521, 2011.