

***Interactive comment on* “Different characteristics of char and soot in the atmosphere and their ratio as an indicator for source identification in Xi’an, China” by Y. M. Han et al.**

Anonymous Referee #2

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The primary result of this study shows that comparing char-EC to soot-EC is a better indicator of EC combustion sources than the often used OC/EC ratios. The authors successfully explain the serious inadequacy in using OC/EC as a marker for combustion source and provide a strong case for the use of char-EC/soot-EC ratios instead. The primary weakness in the method is the complete reliance on the TOR method to separate the EC into char and soot components. In this method, EC is separated into 3 categories based entirely on the refractive properties of the sample (i.e. the temperature at which the EC is evolved). This assumes that the refractive properties of EC don't change after initial formation, which is a faulty assumption. The presence of salts and other catalysts in the sample can drastically alter the temperature at which opti-

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cally active EC evolves (Novakov and Corrigan, 1995). Treating the samples with water and acetone removed different species from the samples and changed the temperature at which soot evolves by over 100°C in some cases. There is a strong potential for soot EC in the presence of catalysts to be mistaken for char-EC using the TOR method alone. While I agree that the authors present strong evidence for their conclusions, relying solely on the TOR may lead to errors and this should be discussed. In the conclusions the authors also mention that char-EC absorption has a much stronger spectral dependence than soot-EC. Combining this observation with the TOR method may provide more robust results.

Other minor issues: Page 13273 “EC is generally subdivided into char and soot.” EC is certainly divided that way in this paper, but char is not generally referred to as EC, but rather “brown carbon” or a component of light absorbing carbon (LAC).

Page 13276, line 17 “Comparing EC concentrations in Xi’an. . . are well correlated with EC in different Chinese cities.” This sentence is awkwardly written and I am unclear as to what is meant by “rank highly”.

Page 13277, line 3, “. . . which confirms the very small difference in soot-EC concentrations at large.” I’m not sure what the authors mean by this, but I think what they are trying to say is that atmospheric soot-EC concentrations do not vary much across China. Either way, it should be clarified.

Line 24 -26, Why does neglecting wet scavenging imply that soot may have a longer lifetime? This doesn’t really make sense, as wet scavenging is such a strong factor in soot lifetime that it should not be neglected. I agree with the first reviewer that there seems to be a discrepancy here. Early in section 4.4, the authors state that soot is very susceptible to wet removal, however at the end of section 4.4, make the statement that char is larger and therefore more easily removed by wet deposition and therefore soot remains longer in the atmosphere. This really needs to be clarified as the different removal rates of char and EC strongly affect the usefulness of using their ratios as a

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source identifier.

Page 13278, line 8, "...and soot from gas-particle re-condensation." I think this should just be condensation. Page 13281, line 12 "... neglected in summer." Remove "in summer" from that sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13271, 2009.

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