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Atmospheric Chemistry and Physics Discussions

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.

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The determination of char EC/soot EC in PM2.5 aerosols is an approach which may yield useful supplementary information to global and regional BC/EC studies. The basic idea of the authors is to try to lend physical meaning to data derived indirectly from a widely used (but somehow arbitrary) standard analytical protocol. The authors call the two subsets 'char EC' and 'soot EC' as if they were existing physical entities: this might be so but definitely not proven in the manuscript nor in previous papers.

The char EC signal (I would call it 'signal' to indicate that it is not more than that until its physical existence is explicitly proven), depends strongly on degree of sample pyrolysis (charring) and the validity of the assumptions behind its optical corrections (which

are inherently imperfect). It is well-known that a part of the water-soluble organic compounds in aerosols are very much prone to charring which cannot be compensated by the optical correction method: therefore, for example, water extract of urban or marine aerosols, as well as pure starch or cellulose do give an EC signal in the thermal-optical method (e.g. Yu et al., 2002; Schauer et al., 2003). Since biomass burning and coal combustion do give off a lot of large molecular weight soluble compounds, which may char and yield false char EC signal (artifact), one may question whether such 'char particles' do exist in the PM2.5 fraction, at least in such a large proportion compared to soot? In coal combustion aerosols it may also be possible that abundant cations such as sodium or potassium may cause premature evolution of soot carbon and again yield a false char EC (and soot EC) reading in the TOR method. Part of these uncertainties could have been greatly reduced by the authors if they have performed water extractions on another part of the filters (thus removing WSOC and cations) prior to the TOR analysis to prove that their 'primary' char EC/soot EC signals are indeed independent from such artifacts. Without that, I am not convinced that char EC particles do exist at such large concentrations in biomass smoke and coal burning impacted aerosols. Another possibility would have been to conduct a parallel electron microscopic study to prove that such particles are indeed so numerous in the PM2.5 size range. The compounds the authors call 'char EC' most likely belong to 'brown carbon' or 'humic-like substances', and are unlikely to bias optically based BC measurements in large cities. Part of them may be determined as 'EC' even in TOR measurements, as it was shown in many previous studies. An alternative option for the authors is to refrain from implying that char EC and soot EC signals correspond to existing physical entities, and restrict their discussion as such. In this case it is straightforward to state that char EC to soot EC signal is 1-45 for biomass smoke, whatever the reasons are for that. Overall, the manuscript present a large body of useful data but with a very one-sided focus. Its statements are not well founded and could well be misleading.

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