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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Positive sampling artifact of carbonaceous aerosols and its influence on the thermal-optical split of OC/EC" *by* Y. Cheng et al.

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This paper looks interesting, but I have a couple of corrections and comments regarding the results in this manuscript and interpretation of results from my papers.

Page 13743, bottom: Subramanian et al. (2006) did not suggest criteria to choose the peak inert mode temperature - that likely was Conny et al. (2003). Rather, we suggest that biases are likely present in all temperature protocols! Subramanian et al. demonstrate that the peak inert mode temperature affects the OC/EC split, particularly that too high of a peak inert mode temperature underestimates EC due to premature EC evolution (also shown by Chow et al. 2001) and that too low a peak inert-mode temperature (550 C as in IMPROVE) would overestimate EC due to non-light-absorbing, less-volatile OC (attributed in the current manuscript to Schauer et al. 2003).

Page 13746, Lines 10-17: Non-light-absorbing carbon will not affect the reflectance signal. The increase in the reflectance signal while the transmittance signal remains flat indicates that the transmittance signal is saturated, while light-absorbing carbon is evolving from the top of the filter (perhaps EC and some charred OC), and light-absorbing carbon within the filter (perhaps charred OC) remains behind; in other words, premature evolution of EC.

Page 13750, lines 16-19: Subramanian et al. (2004) showed that for a sampling volume of ∼24 m3, the Q-QBQ OC was effectively the same as the particulate OC determined by the denuder system. Thus, the authors' interpretation on lines 18-19 that this finding of Subramanian et al. (2004) indicates that "QBQ OC underestimated the positive artifict even for a sampling volume of 24m3" is incorrect. The authors' finding of a significant intercept (i.e. QBQ underestimating the positive artifact) is more similar to the 4-6 hour samples in Subramanian et al., for sampling volumes of 4-6 m3 (not surprising, given the low sampling volumes in the present study). The Cabada et al. (2004) reference cited in Table 2 is for 4-6 h samples (the intensive sampling period of the Pittsburgh Air QUality Study). The authors are mixing up statements and results from Subramanian et al. (2004) that refer to 24-h samples and 4-6h samples, for example the statement on Page 13751, lines 9-12. Please be more careful/specify the data subset.

Page 13751: In section 3.2.2, the authors should look at the QBT-OC results from Subramanian et al. (2004). It is likely that the QBT OC is exposed to a higher concentration of semivolatile OC due to volatilization from the particulate OC on the upstream Teflon filter. The difference in "positive artifact" between the bare quartz (undenuded) OC and the QBT OC is likely due to this difference in gas-phase SVOC concentrations to which each filter is exposed (altering the equilibrium concentration of adsorbed gas-phase SVOC on each filter), rather than the adsorptive capacity of the quartz filter, with or without particles. I believe Turpin et al. (Atm. Env. 1994) did a calculation to suggest that the quartz filter surface area is significantly higher than the surface area provided

by the deposited particles (or this could be McDow and Huntzicker, Atm. Env. 1990).

For the regression analysis on Page 13752, I would caution the authors that assuming HeOx carbon as "PC+EC only" works only if there is no non-light-absorbing OC in the HeOx phase, which is possible with the IMPROVE protocols, particularly if there is a strong influence of wood smoke or other heavy organic matter.

I would also like to see some statistical significance tests on the difference between kPC_undenuded and kPC_denuded.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13739, 2009.