Atmos. Chem. Phys. Discuss., 9, C3930–C3933, 2009
www.atmos-chem-phys-discuss.net/9/C3930/2009/

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.

Anonymous Referee #2

Received and published: 17 August 2009

The paper generally addresses two important aspects of OCEC analysis problems: the "Artifact" difficulties; and the question of Attenuation Coefficients of the various EC's. Overall, it is a very well designed experiment with sufficient sampling variations to be able to draw the conclusions necessary. However, some aspects need to be addressed within each of the two questions.

A. Comments on Positive and Negative Artifact evaluation

This seems to be a very thoroughly designed section of the

experiment. However:

1. The authors give several citations of earlier methods development on page 13743, but fail to cite what is the actual original development of this method by Huntzicker, et. al. Are the authors completely unaware of this, or is there a reason?

2.On page 13746, the authors state that for the TOT method, "...the re-partitioning of PC and EC is based on the attenuation coefficient..." This is not true. The split between PC and EC by the Thermal/Optical method never has depended on knowledge of attenuation coefficients; only on the original optical signal and when the signal returned to that point during oxidation, whether or not the optical signal was Reflection or Transmission.

3.On page 13746, the authors comment on the fact that the Reflectance signal increases during OC-4, yet the Transmission signal remains constant. They attribute this to "non-light aDsorbing carbon" (which should accurately be "non-light aBsorbing..."). Are the authors implying that this is due to scattering of light? If so, why doesn't the Transmission signal also respond?

4.On page 13750, last paragraph, should read "...DQ-OC(no breakthrough..."

5.On page 13751, first paragraph under 3.2.2 should read

"...The amount of positive..."

B. Comments on Attenuation Coefficient calculations and comparisons

1. A very good attempt was made in this section to derive quantitative values for comparison of PC and EC by their Attenuation Coefficient. However, there is a potential source of an error, which may in fact be quite large, making all the comparisons very misleading. The authors should make some effort to address this in order to see if the error is minimal and can be neglected, or large but correctable.

Not too long ago in the development of spectroscopy, even the best of instruments could have errors due to stray light. But with the crude optical design of the current Thermal/Optical OCEC instruments, this stray light can be a large fraction of the signal reaching the detector, especially at lower Transmissions levels. Unless the authors make a measurement of this Stray Light signal, and, if significant, make corrections for it within the calculations for k, all quantitative values, especially at lower Transmissions, could have large errors. This would result in nearly all comparisons and interpretations being wrong.

2. The authors explicitly state on page 13743 that a temperature of 550 C in He is not sufficient to evolve all OC, thus

"overestimating the EC concentration". However, they use a temperature program that goes only to 580 C (IMPROVE-A). Are they implying that this temperature corrects this problem? If not, then it is certainly a potential source of error in calculations of Attenuation Coefficients later during the He/Oxygen phase. The authors must address this.

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