

Interactive comment on “Variability of organic and elemental carbon, water soluble organic carbon, and isotopes in Hong Kong” by K. F. Ho et al.

K. F. Ho et al.

Received and published: 6 September 2006

To respond to the reviewer's specific comments: In the experimental section 2.4 Stable carbon isotope analyses, one would like to see some numbers on the overall precession and reproducibility of the set up used.

Some numbers about precession and reproducibility were added as suggested.

In section 4.2 Variability of WSOC the authors discuss the seasonal variations of WSOC at the three measurements sites with a particular emphasis on the HT station, representing the regional scale situation. It seems to me that the difference of WSOC from summer ($1 \mu\text{g m}^{-3}$) to winter ($2.8 \mu\text{g m}^{-3}$) is not particularly surprising when taking into account that this station features a roughly 4 times higher total OC

concentration in winter ($5.5 \mu\text{g m}^{-3}$) than in summer ($1.5 \mu\text{g m}^{-3}$). The numbers in table 3 also clearly show a higher WSOC fraction in summer in the HT station than winter. This should be more clearly stated in the discussion of this section. The sentence: “The WSOC/OC ratios are higher in winter than in summer at the urban sites” is not supported by the numbers given in table 3 (WSOC/OC of 51.3% in HT winter samples and 69% in HT summer samples).

I agree with the comments of the reviewer and show clearly a higher WSOC fraction in summer in the HT station than winter. Moreover, the sentences: “The WSOC/OC ratios are higher in winter than in summer at the urban sites” was revised as “The WSOC/OC ratios are higher in winter than in summer at PU and KT”.

In section 4.3 the authors show data on the d^{13}C measured in the OC and EC fractions of the samples. While I do agree that the difference in the observed d^{13}C in OC for the HT station from the two urban stations is striking, I would not necessarily expect SOA of biogenic origin to have the same d^{13}C as bulk plant materials. The authors could discuss in more detail what the effect of a high biogenic SOA fraction on the WSOC fraction in their samples would be.

This point is very good and very important. However, it is hard to discuss the effect in the current manuscript due to the fewer samples and the simple measurements. It may not have statistic significance for the effect because this is a very complex topic. The isotopic signature should be measured for WSOC and some species of SOA. We have applied a project to investigate the effect based on long term observation and large amounts of samples in Pearl River Delta Region.

In the summary the authors conclude that their analysis provides a “rapid and cost effective technique for conducting source determinations”. While I do agree with the general usefulness of the approach, I would disagree with filter sampling and analysis being rapid. C The sentence was changed from “rapid and cost effective technique for conducting source determinations” to “feasible and cost effective technique for con-

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ducting source determinations”.

Technical corrections: The insert in figure 3 explains the full black circles as “HKW” instead of “HTW”. The figure 3 was revised as suggested by the reviewer.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4579, 2006.

ACPD

6, S2923–S2925, 2006

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